

# 1. Introduction: scope of the book

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## 1.1 INTRODUCTION

This book aims to enlarge understanding of the decision-making on mega-projects and to suggest recommendations for a more effective, efficient and democratic approach. This is not the first book published on this theme. But this is certainly a unique book, presenting an up-to-date and differentiated overview of the state of the art, based on experiences and visions of authors from Europe and North America.

Traditionally, it has been the job of the government to develop, finance and – often – to manage major investment projects, which we have bundled together in this book under the blanket definition of ‘mega-project’. There are many successful mega-projects, most of which have taken some time to bear fruit – both directly and indirectly. However, there are also many potential problems, which could turn mega-projects into what Peter Hall labels ‘planning disasters’ (Hall, 1980). These problems include low transport performances, adverse environmental effects (landscape erosion, noise pollution, toxic emissions etc.), underestimated investment costs and disappointing returns.

In this book, authors from different scientific disciplines address various aspects of decision-making in mega-projects, such as management characteristics and cost–benefit analysis, planning and decision-making, and innovation, competition and institutions. Many cases are drawn from different parts of the world, both best and worst practices.

The subject matter is varied and highly differentiated, but certain questions crop up time and again. For example, how do we deal with protracted preparation processes, how do we tackle risks and uncertainties, and how can we best divide the risks and responsibilities among the private and public players in the different phases of the process?

The next section elaborates the scope of the book. The notion of mega-project will be explained. We will then present a brief review of recent publications on common pitfalls in decision-making processes on mega-projects

(Section 1.3). Section 1.4 presents the content of the book, divided into three parts: I Management characteristics and cost–benefit analysis; II Planning and decision-making; and III. Innovation, competition and institutions.

## 1.2 SCOPE OF THE BOOK

Frisk's contribution in Chapter 12 identifies the following general characteristics of mega-projects, referred to as the 6 Cs:

1. colossal in size and scope;
2. captivating because of size, engineering achievements and aesthetic design;
3. costly: costs are often underestimated;
4. controversial: funding, mitigation packages, impacts on third parties;
5. complex: risk and uncertainty in terms of design, funding and construction;
6. control issues: who are the key decision-makers, funding, operation etc.

Mega-projects are often technological *tours de force* with an innovative and, not infrequently, an experimental character. They sometimes reflect the cutting edge of modern technology (Frick refers to the notion of 'the technological sublime' in Chapter 12), sometimes the initiators overreach themselves, and the problems and deficiencies become embedded in the project.

There is not only a question of technological complexity, but also of social complexity, as De Bruijn and Leijten argue in Chapter 2. There are often concerns about public support, and the rationality and consistency of political decisions. A question consistently raised is where private decision-making, private funding and private risk-taking should be preferred and where public decision-making is necessary, for example in order to safeguard public values, to have risks borne publicly, or to come to the aid with public finance. Until recently in the transport infrastructure sector, public decision-making, public finance and public risks were involved exclusively, which usually resulted in a weak orientation to the market and serious cost underestimates. The approach has shifted progressively towards arrangements between public and private institutions, in which the public institutions are required to safeguard public values, and private institutions usually ensure a better market orientation, more dynamism, and flexibility. There is a constant quest for the optimum balance between competition and collaboration, and a certain control of the transaction costs, which may be excessively high in innovative arrangements (Van de Velde and Ten Heuvelhof, Chapter 13).

We have opted to solicit contributions from the circle of independent, academic experts from Europe and North America. It was tempting also to request contributions from the banking world (such as the World Bank) or from the world of developers and contractors. The expertise of financiers and practitioners is included in the scientific contributions in this book, but our preference is for this expertise to come from independent, academic sources.

It was likewise tempting to request contributions from authors who derive their insights from mega-projects in countries such as China, India and Korea, where the biggest known mega-projects are currently being planned and executed. However, we chose evidence-based contributions, embedded in the modern context of democratic governments and market-oriented private institutions. In some contributions (Chapter 7 by Flyvbjerg; Chapter 8 by Miller and Lessard), mega-projects in other continents are included, but in general there is insufficient empirical information available on mega-projects outside Europe and North America, and in many cases the context is still too specific and too traditional, as in China, where until recently the socialist regime dominated decision-making, and where the concept of private property was scarcely understood. All this is now changing rapidly. Globalisation is introducing an increasing number of Western institutions, companies and experiences into Asia, Africa and Latin America. We think that the experiences compiled in this book are relevant for the entire modern developed world, and to some extent also for the developing countries that have now embarked on a spectacular economic transition. None the less, in the course of time there will be enough data for a book compiling experiences with mega-projects outside Europe and North America. Whereas now many scientists and practitioners in Asia, Africa, Australia and South America are able to benefit from the findings and lessons in our book, soon scientists and practitioners in Europe and North America will likewise be learning lessons from the decision-making on mega-projects in other parts of the world.

Many of the cases in this book are related to major transport infrastructure projects. This mega-project category is highly relevant, and an abundance of research has been undertaken to gather empirical evidence on them. But there is more to mega-projects. This is stated explicitly in Chapter 8 (Miller and Lessard) and Chapter 9 (Samset), which deal with a broader set of large engineering projects, including nuclear plants, offshore constructions, water treatment plants, military weapons systems, ICT systems and complex real-estate projects (e.g. hospitals, offices, shopping malls and urban centres). The scope of this book is certainly wider than large transport infrastructure projects. Where general phenomena, concepts, findings and lessons are involved, we always have the broader set

of mega-projects in mind, which extends beyond the infrastructure projects: technological complexity, social complexity, cost overruns, strategic behaviour, contested information: all these phenomena apply not only to transport infrastructure projects, but also to mega-projects in a broader sense.

### 1.3 COMMON PITFALLS IN DECISION-MAKING ON MEGA-PROJECTS

#### **Introduction**

Various recent studies on mega-projects have identified certain characteristics that are typical of decision-making processes for those projects. In this section we shall discuss the findings of researchers who each studied a large number of mega-projects: the American researchers Altshuler and Luberoff (2003), and Miller and Lessard (2001), followed by the European researchers Flyvbjerg, Bruzelius and Rothengatter (2003). Then we examine the approach of Short and Kopp (2005) from the Joint OECD/ECMT Transport Research Centre. Finally, the conclusions of the Dutch Parliamentary Committee on Infrastructure Projects (Tijdelijke Commissie Infrastructuurprojecten; TCI, 2004) are summarised.

#### **Mega-Projects According to Altshuler and Luberoff (2003)**

Altshuler and Luberoff (2003: 6–7), who investigated three types of mega-projects – highways, airports and rail transit systems – search for a broader approach spanning four dimensions. First, the authors integrate their findings with leading theories on urban politics and empirical research by others on urban renewal. Second, they address national patterns. Although the authors make substantial use of case studies, they take them from multiple sources to illustrate broad themes and intersperse them with discussions on national developments. Third, Altshuler and Luberoff examine the situation from an intergovernmental perspective. Most urban mega-projects in the latter half of the twentieth century were undertaken with substantial federal funding and within contours of opportunity defined by federal programmes. The authors delineate the multilevel dynamics of these cases, highlighting the roles of federal, state and local players. Finally, they trace developments over half a century, long enough for considerable evolution to have occurred.

Altshuler and Luberoff (2003: 219–47) identify a number of common patterns:

1. Urban mega-projects ceased to be routine after 1970. Implementation depended far more on the case-by-case initiative and style of the advocates.
2. Mega-project support coalitions were, with rare exceptions, spearheaded by business enterprises with immediate interests at stake. The exceptions were led by environmental groups promoting mass transit projects.
3. Mega-project ideas frequently originated in the public sector and were then ‘sold’ to prospective constituencies. Even when the initial impetus came from private groups, energetic and skilful public-sector leadership was still required in most cases to widen the base of public support, mollify critics, secure resources at higher levels of government, and generally manage conflict. The authors dub this kind of leadership ‘public entrepreneurship’.
4. However broad their support coalitions, mega-project proposals were rarely implemented if they imposed substantial costs on neighbourhoods or the natural environment. Altshuler and Luberoff call this the ‘do-no-harm’ paradigm.
5. But even the most sensitively planned mega-projects generated some negative impacts, so it became widely accepted that these should be ‘mitigated’ as far as possible. The mitigation norm frequently became a major source of leverage for groups with agendas that went beyond damage limitation.
6. Though urban mega-projects were often founded mainly by the federal government, they almost invariably originated locally, where they also drew their main constituency of support with little or no regard for national objectives. Altshuler and Luberoff refer to this as ‘bottom-up federalism’.
7. The whole point of mega-project finance was to avoid increases in broad-based local taxes – particularly if levied on host-city residents alone – and, more specifically, property and income taxes. Alternative sources of funding included local taxes designed mainly for visitors, state and regional sales taxes, and sometimes lottery revenues. Altshuler and Luberoff describe this as ‘locally painless project financing’.
8. Finally, the costs of mega-projects rose spectacularly in 1970–2000 and surpassed official estimates by a considerable margin at the time of authorisation. The causes of this development seem to lie in the realm of politics rather than in engineering or accounting.

### **Mega-Projects According to Miller and Lessard (2001)**

In Chapter 8, Roger Miller and Donald Lessard present an overview of the IMEC study, an assessment of 60 mega-projects (IMEC = International

Program in the Management of Engineering and Construction). These projects include 15 hydroelectric dams, 17 thermal and nuclear power plants, 6 urban transport facilities, 10 civil infrastructure investments, 4 oil platforms and 8 technology initiatives. Per project, seven to eight participants—sponsors, bankers, contractors, regulators, lawyers, analysts and others were interviewed. Particular emphasis was placed on front-end development decisions, but execution and initial ramp-up to operation were also studied.

The goal of the IMEC study was to understand the changes that were occurring (increasing financial, political and social complexity), and to identify the practices that, in the experience of executives involved in projects, really made a difference. The study reflects the collective experience from Europe, North and South America, and Asia. The study involved systemic and strategic perspectives, and focused on themes such as coping with uncertainty through risk analysis, institution-shaping and strategies.

Mega-projects or large engineering projects (LEPs) are presented as high-stakes games characterised by substantial irreversible commitments, skewed reward structures when they are successful, and high probabilities of failure. Their dynamics also change over time. The journey from initial conception to ramp-up and revenue generation takes ten years, on average. While the ‘front end’ of a project – project definition, concept selection and planning – typically involves less than one-third of the total elapsed time and expense, it has a disproportionate impact on outcomes, as most shaping actions occur during this phase. During the ramp-up period, the reality of market estimates and the true worth of the project are revealed. Sponsors may find that actual conditions are very different from expectations, but only a few adaptations are possible. Once built, most projects have little flexibility in use beyond the original intended purpose. Managing risks is thus a real issue.

Successful projects are not selected but shaped. Successful sponsors appear to start with project ideas that have the potential to become viable. These sponsors then embark on shaping efforts to influence risk drivers ranging from project-related issues to broader governance. The seeds of success or failure of individual projects are thus planted early and nurtured over the course of the shaping period as choices are made. Successful sponsors, however, do not escalate commitments, and they abandon quickly when they recognise that projects have little possibility of becoming viable.

Two other key concepts related to risk that emerge from the study are governability – the creation of relationships that allow a project to be reconstituted and proceed even after major changes in project drivers and the resulting payoffs to the various parties involved – and turbulence – the tendency for risks to compound dramatically once things begin going off track.

Miller and Lessard argue that projects are dynamic, iterative and often chaotic systems. Project-management architectures must reflect this. While projects tend to resemble a spiral more than the classic waterfall, even this metaphor may be too orderly. Projects are better viewed as evolutionary and path-dependent systems composed of episodes displaying different dynamics.

These findings apply equally, albeit in somewhat different ways, to the three distinct classes of risk (in terms of their causes) encountered in most projects: those emanating from the dynamics of the project itself (technical and operational risks); those associated with the markets with which the project interacts (market risks); and those related to the political, social and economic setting of the project (institutional/social risks).

Chapter 8 informs the reader in more depth about the findings of the IMEC study.

### **Mega-Projects According to Flyvbjerg, Bruzelius and Rothengatter (2003)**

Flyvbjerg et al. (2003) base their analysis on a unique database comprising 258 large infrastructure projects spread over many years in all continents and involving a total investment of €90 billion. Despite the geographical and temporal spread and the wide differences in the characteristics of the projects, Flyvbjerg et al. (2003) identified some common features.

The most important finding is that in nine out of ten cases the costs of mega-projects are underestimated. Cost overruns are in particular a problem in the development of rail infrastructure. On the other hand, the demand for transport, and hence the actual performance of transport, are invariably overestimated. This conclusion confirms the findings of previous studies by Wachs (1989; 1990) and Pickrell (1989; 1992). The actual costs for rail projects are, on average, 45 per cent higher than the projected costs. The differences are so great and so consistent that Flyvbjerg et al. (2003) rule out the likelihood of coincidence. The World Bank refers to this phenomenon as 'appraisal optimism' (Short and Kopp, 2005: 366); Flyvbjerg et al. (2003) call it 'misinformation' (TCI, 2004: 41).

Misinformation undermines Parliament's ability to exercise democratic control. Flyvbjerg et al. stress that the effects of misinformation are not confined to the political arena; for example:

- misinformation destabilises the decision-making on a project. It is bound to emerge sooner or later that the information was incorrect;
- setbacks disrupt the process; for example, new research may be needed and the political and market players might start getting edgy;

- incorrect information can also lead directly to squandering of taxpayers' money.

If the actual costs had been known beforehand, the project could have been abandoned, and other projects with a higher societal yield per invested euro could have been considered. As Flyvbjerg et al. say: 'The wrong projects are being chosen and implemented.'

The dividing line between misinformation and prevarication is wafer-thin. According to Flyvbjerg et al. (2003), misinformation is essentially the wilful and deliberate telling of untruths – which is tantamount to lying.

The central challenge when defining decision-making processes for mega-projects is to create incentives that deliver more reliable information – particularly in the early stages. This may be achieved through second opinions, hearings, workshops and independent experts. Flyvbjerg et al. (2003) maintain that cost–benefit prognoses should be left to the organisations that will actually suffer the consequences of any inaccuracies. Accordingly, they also argue that private parties should be allowed to participate in the preparations for mega-projects provided they bear at least 30 per cent of the total investment risk.

The conclusions of Flyvbjerg et al. (2003) do not bode well for political decision-making. They imply that governments and parliaments base their decisions on (deliberately fabricated) incorrect information. The costs are underestimated and the benefits overestimated: there are too many mega-projects with far lower returns than predicted, and nowhere near enough economic benefits. Moreover, huge differences exist between projects: while some are prepared on the basis of the grossly exaggerated predictions of the initiators, others are prepared on the basis of (more or less) accurate information. So it makes no sense to argue that the impacts of the mistakes cancel each other out.

### **Mega-Projects According to Short and Kopp (2005)**

Short and Kopp (2005: 362–3) have assembled data that show long-run trends (1975–2000) in the ratio of investment in different modes of transport. In Western European countries the percentage invested in road transport is declining slightly while the percentage that goes to rail transport is on the increase. Conversely, in Central and Eastern European countries investment in rail transport is declining, standing at around 37 per cent in 2000. Short and Kopp (2005: 363) write: '[T]he fact that the rail market share has been constantly declining and that its share of investment is increasing in Western European countries is certainly worth noting.' It looks as if the renaissance of rail investment in Western Europe

is tied in with the popularity of the high-speed railway and light rail (ECMT, 1994).

Short and Kopp (2005: 363) have misgivings about the recent rise in investment in rail infrastructure in Western Europe. Considering the sharp fall in the railway's market share in the modal split (lower than 10 per cent in many West European countries), the low rates of return on rail investment, the relatively large sums needed to make an impact, and the high maintenance consequences of rail investments (in Germany it is estimated that every €100 invested in rail means annual maintenance costs of €40), Short and Kopp (2005: 363) ask 'whether we can afford this'. Rail investment and user charge in rail transport without accompanying policy measures are likely to be costly and ineffective (Affuso et al., 2003).

Short and Kopp (2005: 363) refer to Bonnafous (2003), who points out that the costs per kilometre of high-speed railway lines are far higher than in the past. In fact, they have almost doubled in real prices, from €5.03 million for the first lines to over €9.91 million for more recent ones. Though the first high-speed rail projects in Japan and France (Bonnafous and Crozet, 1997) were successful, recent ones show more dubious results.

Short and Kopp (2005: 363) compiled a set of proposals for improving planning and decision-making in transport infrastructure mega-projects:

- Greater efforts should be made to explain the planning methods to a broad expert audience. Secrecy about forecasting methods, modelling assumptions, model selection criteria and, in particular, the determination of planning objectives, can make people suspicious of the planning outcomes.
- Quality checks on the planning outcomes, similar to reviews by scientific journals, could help to improve the reputation of planning agencies.
- Even if the quality of a planning process is beyond reproach, it is not always certain that the outcomes will be directly translated into political decisions and then implemented. If this is due to defects in the planning, information should be relayed back to improve the planning process in general and move it forward to an interactive planning-policy learning process.
- The rejection of planning outcomes should be justified. The reasons for political non-acceptance of planning outcomes should be backed by a broad audience.

In general, Short and Kopp (2005) observe a lack of transparency in decision-making on transport infrastructure projects on a national and European scale. The methodological underpinning of decision-making on

these projects also left much to be desired. Short and Kopp (2005: 364) even say that no use is made of traffic forecasts or economic analyses. Neither data nor costs are publicly available. There is also a dire shortage of reliable and competent *ex-post* evaluations (Quinet, 2000; Rothengatter, 2000). As European decision-making is sometimes geared exclusively to projects worth at least €500 million, a bias has emerged towards mega-projects. Short and Kopp (2005: 364) write: '[T]hese international processes did not make enough use of economics, became over-politicised and biased towards mega-projects, and had no close links with financing or implementation.'

Unfortunately, the findings with regard to decision-making in international projects apply likewise to decision-making in national transport infrastructure projects. There is a pressing need at both levels for better data, better economic appraisal and more transparency (Mackie and Preston, 1998).

In tram and light-rail projects the costs also tend to be underestimated and the benefits overestimated (Pickrell, 1989). Even so, Short and Kopp (2005: 365) concede that, at the end of the day, sensible decisions are still taken: 'Some cities, for example, Strasbourg, Nantes and Grenoble believe that their visions of accessibility and liveability have been achieved and that their light rail systems have enhanced them. Other cases are much less convincing – Sheffield in the UK is one example and there are several more in the US.'

Short and Kopp (2005: 366) write: 'It is clear that an evaluation framework for light rail needs to address broader aims: better accessibility to cities and particular groups, more attractive cities, revitalised city districts and more users of other modes to reduce congestion and pollution.' The question is how all these effects can be credibly determined in advance.

Short and Kopp (2005: 366) draw six general conclusions on investment in and planning of transport infrastructure projects:

1. Policy and research need good data. The broad information that is currently available allows some analysis but it is still insufficient and in need of fundamental improvement. Often, project data are not collected or made available, and *ex-post* monitoring of projects and policies needs to be systematically introduced and strengthened.
2. Even at this level, important questions can be raised about appropriate levels of investment and how the investments are allocated to the various modes. The search for answers will involve more in-depth analyses and may prove a rich topic for research.
3. National investment planning methods are flawed in several respects, the most serious being lack of transparency, not differences in appraisal methods.

4. International planning is growing in importance but it risks inheriting all the flaws of national planning and some new ones as well. But that does not alter the fact that there are areas where infrastructure has to be planned on an international scale. We therefore need better analyses and a clearer understanding of where international planning might apply and how it could work effectively.
5. Project appraisal is still inconsistent and weak. Strategic appraisal is in its infancy. *Ex-ante* appraisal is often biased and *ex-post* analysis rarely takes place.
6. Research into planning and decision-making processes could, given their ever-increasing complexity and duration, be of great value to society.

### **Mega-Projects According to the Duivesteijn Commission (2004)**

The experience of the Dutch Duivesteijn Commission (TCI, 2004) confirms the findings of Short and Kopp (2005).

In 2004 the Dutch Parliamentary Commission on Infrastructure Projects published its official report (TCI, 2004). The conclusions of the Duivesteijn Commission are based on the decision-making process for two major infrastructure projects: the dedicated High Speed Rail Link South (Amsterdam–Belgian border – HSL-South) and the dedicated Freight Railway Link, connecting Rotterdam and the Ruhr Area (Betuwe Line). The TCI (2004) also turned its attention to recent international analyses of decision-making in large infrastructure projects. The central concern of the TCI study (2004) is the role of the Dutch Parliament.

In the Netherlands, empirical evidence drawn from the two cases mentioned bears out the findings of Altshuler and Luberoff (2003): the Port Authority of Rotterdam, the Dutch Railways and the ECT Terminal in Rotterdam strongly backed the Betuwe Line (no. 2). Public entrepreneurship was observable in both the HSL-South and the Betuwe Line (no. 3). The do-no-harm approach was adopted by municipalities and action groups in both cases. This led to a number of extra tunnels and other plans for mitigating negative impacts, incurring substantial cost overruns. With the exception of the province of Gelderland, no local or regional authority made a financial contribution (no. 4). Mitigation of negative impacts played an important role in the Betuwe Line in particular, but HSL-South also generated some interesting and costly mitigation programmes, including the Green Heart Tunnel (no. 5). ‘Bottom-up federalism’ was observed in HSL-South (Amsterdam–Schiphol) and even more so in the Betuwe Line (Port Authority of Rotterdam) (no. 6). ‘Locally painless project financing’ occurred in both cases (no. 7). Finally, it was the huge cost overruns in both cases that prompted the TCI to launch a parliamentary inquiry

(no. 8). In comparing the USA and the Netherlands we have to keep in mind that the finance of local projects in the Netherlands is much more dependent on the national public budget than in the USA.

The TCI (2004: 15) observed that mega-projects are often contested during preparation and implementation. They are characterised by dynamism and complexity, as is reflected in systematic budget overruns. Large projects are one-off events in public administration and therefore require an individualised approach.

The frequent budget overruns point to a financial complexity which is intertwined with the immense technological and social complexity of mega-projects. The decision-making on complex projects takes place in a policy arena of interdependent parties (TCI, 2004: 18). Teisman (1998) writes: 'The players are stuck with each other. Mutual dependence creates relationships. The policy field evolves into a network of interdependent ties. . . . Initiatives come under fire in systems founded on checks and balances – which could lead to better policy proposals.'

Any number of obstacles can crop up and obstruct political management and monitoring of the decision-making process. A huge problem is that the Lower House is not involved in the decision-making procedure in the initiation phase of mega-projects and, in effect, acquiesces. In the later stages it seems to be primarily committed to pushing through spatial adjustments.

The government has set its course and is holding fast; it is subject to a process of entrapment (Brockner and Rubin, 1985). The government has shown that it is hardly capable of learning lessons. Hence the government is also to blame for the many overrun budgets.

There was no overall appraisal at the start of the decision-making process. There was no scope for weighing up the alternatives. Sometimes the decision-making was prematurely included in the text of a coalition agreement. However, since the start of the Betuwe Line and the HSL-South much has improved by introducing the OEEI method in calculating *ex-ante* costs and benefits (Eijgenraam et al., 1999).

The main obstacles to management and monitoring in the implementation phase are the project organisers, project control, risk management, contracting, and the public–private partnership. In all these areas the ambitions were found to be too high and the achievements too low, partly because of a lack of professionalism in the public sector.

The findings of the TCI (2004) bear out the conclusion of Short and Kopp (2005: 366), who call the role of government into question: '[I]n many cases, its role as protector of the public interest has become subordinate to its role as promoter of projects. Achieving the right balance requires an urgent redefinition of the job of Transport Minister . . . '.

## 1.4 CONTENT OF THE BOOK

The book is structured in three parts, which relate to the three components of the subtitle of the book: I Management Characteristics and Cost–Benefit Analysis; II Planning and Decision-Making; and III Innovation, Competition and Institutions.

### **Part I Management Characteristics and Cost–Benefit Analysis**

Part I starts with Chapter 2, ‘Management characteristics of mega-projects’ (Hans de Bruijn and Martijn Leijten). In this contribution the authors discuss the most common pitfalls for managers of mega-projects and ways to avoid them. Projects may be unmanageable (in terms of time and money) as a result of a challenging design or a complex social system, or impoverished as a result of a safe design to prevent this unmanageability. In addition, this chapter focuses on the characteristics of the technical and social complexity, and how projects can be managed to avoid these pitfalls. This leads to the central question whether the manager should be mainly involved with the substance of his project or rather with the process that should lead to its completion.

Chapter 3 is on the ‘*Ex-ante* evaluation of mega-projects: methodological issues and cost–benefit analysis’ (Bert van Wee and Lóránt Tavasszy). This chapter discusses methodological issues from the cost–benefit analysis (CBA) perspective. Several of the issues, however, are also relevant for other evaluation frameworks such as multi-criteria analysis (MCA). The issues include both the more technical/methodological issues as well as modelling issues.

‘Cost–benefit analysis and the wider economic benefits from mega-projects’ is the topic of Chapter 4 (Roger Vickerman). Wider benefits, going beyond the direct benefits to the users of transport infrastructure, are frequently claimed as the basis for justifying projects that have only marginal rates of return based on user benefits. This chapter reviews the basis for such claims, referring to empirical evidence from European, Dutch and UK studies. It assesses the way in which such evidence can be used to refine the appraisal process for mega-projects. There is a particular emphasis on the value of the new economic geography, especially the impacts on the labour market. The chapter argues that there is no simple rule of thumb that can be applied to such projects, and that the data requirements, although demanding, are feasible for mega-projects.

Chapter 5, written by Hans de Bruijn and Martijn Leijten, is on ‘Mega-projects and contested information’. Good information is key to good decision-making on mega-projects. Decision-making is information-sensitive

and empirical research shows that, in many cases, a lack of information has resulted in poor decision-making. This chapter deals with three issues related to mega-projects:

1. The concept of contested knowledge will be introduced. The stronger the different interests of the main actors are, the stronger the incentives will be to make information more contested and devalue it.
2. If the contested character of information is denied, what are the implications for decision-making? Denying the contested character of information will make decision-making a free fight. This is a paradoxical conclusion: if information is contested and actors look for objective information, the role of information will be devalued rather than strengthened.
3. A number of strategies will be introduced to cope with the contested character of information. Their essence is not finding objective information but negotiating on what the right information for correct decision-making might be. The result of these strategies is negotiated knowledge rather than objective knowledge.

## **Part II Planning and Decision-Making**

Chapter 6, by Hugo Priemus, tries to explain 'How to improve the early stages of decision-making on mega-projects'. The author argues that the decision-making process on mega-projects is mostly at its weakest in the early stages.

Very often a solution is presented without a valid analysis of the problems. In addition, feasible alternatives are not put forward, because lobby groups work hard mobilising support for the 'superior' solution. Alternatives are only later suggested by others than the promoters, and are often whittled down to nothing.

The systems analysis methodology is presented in this chapter and strongly recommended: at an early stage alternatives are generated, ranked according to the *ex-ante* calculations of costs and benefits, and finally selected. This methodology is certainly not new among scientists, but in practice governments and other promoters of mega-projects seldom use this well-known approach, let alone more advanced techniques such as actor modelling, simulation and gaming, which could very well be combined with systems analysis.

'Public planning of mega-projects: overestimation of demand and underestimation of costs' is the theme of Chapter 7 by Bent Flyvbjerg. This chapter presents evidence that forecasters generally do a poor job of estimating travel demand and construction costs for new transportation infrastructure. For

travel demand, in nine out of ten rail projects passenger forecasts are over-estimated; actual ridership is on average 51 per cent less than that forecasted. In 50 per cent of road projects the difference between actual and forecasted traffic is more than  $\pm 20$  per cent; for 25 per cent of roads the difference is greater than  $\pm 40$  per cent. For construction costs, nine out of ten projects have underestimated costs and cost overruns.

Forecasting inaccuracy appears to be constant over time and space. Estimates of travel demand have not improved for 30 years, cost estimates and overruns not for 70 years. Inaccuracy exists across the 20 nations and five continents included in the study. Measures developed to improve this sorry state of affairs include improved governance structures with incentives that better reward valid estimates of demand, costs and risks, and punish deceptive estimates. Measures also include better forecasting methods, for example the use of 'reference class forecasting', based on theories of decision-making under uncertainty.

Chapter 8 is on 'Evolving strategy: risk management and the shaping of mega-projects' (Roger Miller and Donald Lessard). The authors argue in this chapter that the succession of shaping episodes that form the front-end process to cope with risks can be reinterpreted in terms of the real-options framework that is currently revolutionising academic treatments of project evaluation. In fact, as is often the case with cutting-edge practice, managers have been successful at creating value through the development and exercise of sequential options without explicitly framing the process in options terms. Academics have simply codified this practice in the form of a new conceptual framework.

The real-options framework is based on the same logic as that of financial options as developed by Black and Scholes (1974). It recognizes that the decisions that determine project cash flows are made sequentially over many episodes. The key insight of this approach is that uncertainty or volatility may actually increase the value of a project, as long as flexibility is preserved and resources are not irreversibly committed. As a result, the economic value of a project when it is still relatively unformed is often greater than the discounted present value of the expected future cash flows. Value is increased through the creation of options for subsequent sequential choices and exercising these options in a timely fashion. Thus sponsors seek projects that have the potential for large payoffs under particular institutional and technical circumstances. The study in this chapter illustrates the rich varieties of mechanisms through which these options are shaped and exercised over the life of the project – the real management that is integral to real options.

'How to overcome major weaknesses in mega-projects: the Norwegian approach' is the theme of Chapter 9, by Knut Samset. This chapter takes a

broad view on decisions made at different stages, up front and during implementation, of mega-projects – and their effects during the implementation and operational phases. The author discusses characteristics of the decision-making process and the basis for decisions during these phases. Some general requirements are outlined, and cases used to illustrate the points. The presentation focuses on basic generic principles, and only to some extent goes into further discussion of the complexities and restrictions that might apply when the principles are implemented. The quality assurance scheme applied by Norwegian authorities to improve up-front decision-making, management and the effect of major public investment projects is presented as one type of governance regime that might help overcome some of the problems observed. Current and potential effects and spin-offs of the regime are discussed.

Joop Koppenjan discusses ‘Public–private partnership and mega-projects’ in Chapter 10. This contribution concentrates in particular on the Private Finance Initiative (PFI) – as in arrangements in the transport and water sectors. The author discusses what public–private partnership (PPP) is about: definitions, motives and form. Then an overview is given of experiences with PPP. The central question is: does PPP live up to expectations? The author discusses a number of typical problems that occur in PPP processes which will have to be dealt with in order to make PPP schemes work.

Finally, a number of lessons are formulated regarding the conditions for successful PPP in mega-projects. The author argues that the current emphasis on PFI-like models in the world of infrastructure projects should be complemented with other experimental models: the variety of infrastructure projects calls for the development of a variety of PPP options, which makes the task for parties to learn to handle these options even more challenging.

### **Part III Innovation, Competition and Institutions**

Chapter 11 (Werner Rothengatter) is on ‘Innovations in the planning of mega-projects’. The focus is the aspects of new institutional arrangements and innovative assessment tools to improve on the performance of the planning process for mega-projects.

Wrong procurement is a major cause of public failure; the problems of high risk and long life of mega-projects deserve particular consideration in the procurement process. Innovations in planning approaches start at this point of departure and first suggest a different organisation structure. Important issues are the establishment of a project company under private law and the participation of private risk capital. Furthermore, the

integration of preferences of different stakeholder groups from the beginning is important to minimise conflicts in the procurement process. This can be supported by particular methodological approaches such as logic constraint programming. Finally, a dynamic assessment scheme is necessary, which includes the most important feedback loops between infrastructure use and the economy. One possibility is to apply system dynamics, which is illustrated by the example of the ASTRA model.

The basic message is that improved success of mega-projects is not so much a matter of better methods or more accurate calculations. Major progress can be achieved by changing the institutional environment so that the incentives of the stakeholders work in the direction of generating real economic benefits within the budget. Once the incentives are set right, the players will almost automatically be interested in using the best technologies and methods, as is suggested.

Chapter 12, written by Karen Trapenberg Frick, is on ‘The cost of the technological sublime: daring ingenuity and the new San Francisco–Oakland Bay Bridge’. The ‘technological sublime’ refers to the repeated experiences of awe and wonder, often tinged with an element of terror, which people have had when confronted with particular natural sites, architectural forms and technological achievements. This chapter uses this concept of the sublime to contribute a new dimension to understanding the evolution of mega-project design and optimism bias. The case of the new San Francisco–Oakland Bay Bridge in Northern California is used to demonstrate how the technological sublime dramatically influenced bridge design, project outcomes, public debate and lack of accountability for its excessive cost overruns. The new Bay Bridge case raises several important additional dimensions that should be considered in policy analyses about mega-projects: the sublime, aesthetics and funding.

Chapter 13 (Didier van de Velde and Ernst ten Heuvelhof) is on ‘Provision and management of dedicated railway systems: how to arrange competition’. The main aim of using contracting under competition in the case of infrastructures was the introduction of additional incentives for budget control in infrastructure realisation and a better inclusion of trade-offs between building costs and maintenance costs in infrastructure design and operation. Further in-depth studies are required to identify the relative performance of these different arrangements.

The authors observe that, although the advantages may seem substantial from a theoretical perspective, innovative contracting is difficult to get off the ground. The main issue, identified as the vertical dimension in their framework of analysis, is whether production stages that are conceptually separable (such as infrastructure management and train operations) should be separated, or whether interdependencies between these or other stages

require integration to guarantee optimisation. These critical interface problems require specific attention when the use of competition is contemplated to realise one or several parts of a railway system. This is the fundamental issue of transaction-cost economics. One feature of the current reform practices in the railway sector is that they are, to a large extent, dictated by political or economic dogma rather than by optimal outsourcing decisions. Furthermore, many of the reforms go beyond simple outsourcing, as they introduce several non-hierarchically related initiative-takers along the various layers, adding to the complexity and requiring further coordination between these new actors.

Two questions present themselves in a horizontal sense: to what extent should the various activities (designing, building, operating, maintaining) and their financing be kept in one hand and how much room should private parties be given in their role as contractors? Successes appear to be scored mainly in effectiveness, better project control and innovation. Most problems occur in the areas of transaction costs, transparency, legitimacy and accountability. Many of the disadvantages might perhaps have been prevented if the process had been better organised.

Chapter 14 deals with a specific case: 'Rail infrastructure at major European hub airports: the role of institutional settings'. This contribution by Moshe Givoni and Piet Rietveld compares different approaches to intermodality. The authors argue that the development of rail networks around the world is directly linked to the development of cities. Large airports generate demand which is often even larger than that of city centres. Big airports are ideal places for developing railway services. Nevertheless, the connection of the world's largest airports to the rail network is a recent development.

The different degrees of rail development at airports which are big enough to justify investments in rail infrastructure are very context-specific. In terms of intermodal policies and more specifically rail services at airports, the cases of Schiphol (seamless intermodality) and Heathrow (limited intermodality) represent two extremes. The case of Charles de Gaulle (Paris) illustrates an intermediate position. These three cases are presented and compared. The different institutional settings contribute strongly to the explanation of the differences in outcomes.

The authors conclude that in countries with large airports and a developed rail network, airports should be a stop on the main rail lines. It is helpful to recognise large airports as cities. The role and function of these cities in a regional-economic perspective depend on their connection to the surface transport network. It is important not only to improve the interconnectivity between air networks and road networks, but also the interconnectivity between air networks and rail networks.

Chapter 15 draws institutional lessons across countries on making transport infrastructure policy. The author, Martin de Jong, argues that institutions and institutional systems are hard to change, but it is not impossible.

When congestion on the infrastructure networks or annual investment/maintenance costs are considered to have grown out of hand, or when citizens feel they should be more involved in the decision process, drawing lessons from policies in other countries can prove a helpful source of inspiration for institutional changes at home. This does not imply, however, that following good examples from elsewhere also results in policy successes at home. Political, legal, administrative and cultural practices differ among countries, and so do their economic, geographical and practical potential. In the end, each country can benefit immensely from experiences abroad, but policy actors will always have to take into account that a transplant must function in one's own institutional context, in one's own practical circumstances and in collaboration with other domestic policy actors. The chances that such a situation comes about is low when policy models are copied literally and without making the necessary amendments.

Four examples of promising policy transplants are presented. All four may prove invaluable sources of inspiration for policy entrepreneurs apt to provoke changes in their national systems for infrastructure decision-making, albeit in various directions. But in none of them can transfer be considered something automatic. It will require political and policy struggles among proponents and opponents, and in all cases intelligent thought and careful manoeuvring in negotiations will lead the transplants to deviate substantially from their examples.

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