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CHAPTER 13

OVER BUDGET, OVER TIME, OVER AND OVER AGAIN

MANAGING MAJOR PROJECTS

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Characteristics of major projects

Ex post studies of the Channel Tunnel between France and the UK—the longest underwater rail tunnel in Europe—make shocking reading. Construction cost overrun was 80 percent in real terms using the final business case as baseline, overrun on financing costs was 140 percent, and the demand shortfall was 50 percent (Flyvbjerg, Bruzelius, and Rothengatter 2003). The actual net present value to the British economy is negative, at –17.8 billion dollars, as is the internal rate of return on the project, at –14.45 percent, leading to the inevitable conclusion that "the British Economy would have been better off had the Tunnel never been constructed" (Anguera 2006: 291).

If the Channel Tunnel were just an isolated instance of what Hall (1980) has aptly called "great planning disasters," we need not worry much. However, statistical analyses document that the tunnel is not the outlier it might seem at first sight, it's business as usual (Flyvbjerg, Holm, and Buhl 2004, 2005). In recent surveys of major projects, nine out of ten had cost overrun, cost overruns of 50 to 100 percent were common, and overruns above 100 percent were not uncommon. On the demand and benefit side, estimates were typically wrong by 20 percent to 70 percent compared with actual developments (Altshuler and Luberoff 2003; Flyvbjerg, Bruzelius,

and Rothengatter 2003: 18–19; Morris and Hough 1987; Priemus, Flyvbjerg, and van Wee 2008).

Major projects and programmes generally have the following characteristics. (A major project is here defined as a project costing a hundred million dollars or more; a major programme as a suite of projects costing a billion dollars and up. Most of the chapter's conclusions apply equally to major projects and major programs. However, for ease of writing and reading, "major project" is the main term used in the text.)

- Such projects are inherently risky due to long planning horizons and complex interfaces.
- Decision-making, planning, and management are typically multi-actor processes with conflicting interests.
- Technology and designs are often non-standard.
- Often there is overcommitment to a certain project concept at an early stage, resulting in "lock-in" or "capture," leaving alternatives analysis weak or absent, and leading to escalated commitment in later stages.
- Due to the large sums of money involved, principal–agent problems are common.
- The project scope or ambition level will typically change significantly over time.
- Statistical evidence shows that such complexity and unplanned events are often unaccounted for, leaving budget and time contingencies sorely inadequate.
- As a consequence, misinformation about costs, schedules, benefits, and risks is the norm throughout project development and decision-making.
- The result is cost overruns and benefit shortfalls that undermine project viability during project implementation.

This is not to say that projects do not exist for which costs and/or benefits were on or better than the budget. The Bilbao Guggenheim Museum is an example of that rare breed of major project which is built on time, with costs on budget, and revenues higher than expected (Flyvbjerg 2005). But it is far easier to produce long lists of major projects that have failed in terms of cost overruns and benefit shortfalls than it is to produce lists of projects that have succeeded. To illustrate, as part of ongoing research on success in major project management the author and his associates are trying to establish a sample large enough to allow statistically valid answers. But so far they have failed. Why? Because success is so rare in major project management that at present it can be studied only as small-N research.

The characteristics of major projects listed above are deeply problematic, because they produce failure upon failure. Most of the time this impacts people mainly in terms of financial losses, which is bad enough for taxpayers and other investors who fund major projects. But worse, particular groups, who are often already

disadvantaged, are sometimes forced to carry a disproportionate share of negative environmental and social impacts from projects that do not even deliver the promised benefits.

In what follows, the deeper causes of cost overruns and benefit shortfalls are uncovered. In addition, possible solutions to the problems are described.

Causes and root causes of underperformance

It is useful to distinguish between "causes" and "root causes" in explaining cost overruns, benefit shortfalls, and delays in major projects. Conventionally, the following are listed as causes of project underperformance in the literature and in practice: project complexity, scope changes, technological uncertainty, demand uncertainty, unexpected geological features, and negative plurality (i.e. opposing stakeholder voices) (Flyvbjerg, Bruzelius, and Rothengatter 2003; Miller and Lessard 2000; Morris and Pinto 2004). No doubt, all of these factors at one time or another contribute to cost overruns and benefit shortfalls, but it may be argued that they are not the real, or root, cause. The root cause of underperformance is the fact that project planners tend to systematically underestimate or even ignore risks of complexity, scope changes, etc. during project development and decision-making (Flyvbjerg, Garbuio, and Lovallo 2009). Such ignorance or underestimation of risks is often called optimism, and if we accept this terminology the root cause of underperformance is optimism, whereas complexity, scope, technology, etc. are simply specific issues about which planners have been optimistic and through which optimism therefore manifests itself. Similarly, it may be argued that escalated commitment and lock in, which are also often listed as causes of underperformance, are not root causes (Staw and Ross 1978). These phenomena are so common in major projects that the risk of their occurrence should clearly be considered in sound project preparation. But, again, such risks are typically ignored or underestimated and that is the root cause of underperformance.

Below, the focus will be on root causes of underperformance and not on conventional causes. This means that a substantial part of the conventional literature is left out. Not because this literature is unimportant, but because the chapter has a different focus and is attempting to understand better what the deeper causes of underperformance are.

At the most basic level, the underlying causes of project underperformance may be grouped into three categories, each of which will be considered in turn: (1) bad luck or error; (2) optimism bias; and (3) strategic misrepresentation (Flyvbjerg,

Garbuio, and Lovallo 2009). Bad luck, or the unfortunate resolution of one of the major project uncertainties mentioned above, is the explanation typically given by management for a poor outcome (Ascher 1979; Clapham and Schwenk 1991; Ford 1985; Morris and Hough 1987). The problem with such explanations is that they do not hold up in the face of statistical tests. Explanations that account for underperformance in terms of bad luck or error have been able to survive for decades only because data on project performance has generally been of low quality, i.e. data has been disaggregated and inconsistent, because it came from small-N samples that did not allow rigorous statistical analyses. Once higher-quality data was established that could be consistently compared across projects in numbers high enough to establish statistical significance, explanations in terms of bad luck or error collapsed. Such explanations simply do not fit the data (Flyvbjerg, Holm, and Buhl 2002, 2005).

First, if underperformance were truly caused by bad luck and error, we would expect a relatively unbiased distribution of errors in performance around zero. In fact, the data show with very high statistical significance that the distribution of error is exceedingly biased with a mean statistically different from zero.

Second, if bad luck or error were main explanations of underperformance, we would expect an improvement in performance over time, since in a professional setting errors and their sources would be recognized and addressed through the refinement of data, methods, etc., much like in weather forecasting or medical science. Substantial resources have in fact been spent over several decades on improving data and methods in major project management, including in cost and benefit forecasting. Still the evidence shows that this has not led to improved performance in terms of lower cost overruns and benefit shortfalls. Bad luck or error, therefore, do not appear to explain the data. It is not so-called estimation "errors" or their causes that need explaining. It is the fact that, deliberately or not, in the vast majority of projects, risks of scope changes, high complexity, unexpected geological features, etc. are systematically underestimated during project preparation, resulting in underestimated costs and overestimated benefits.

We may agree with proponents of conventional explanations that it is, for example, impossible to predict for the individual project exactly *which* scope change, complexity, or geological problem will materialize and make costs soar. But we must maintain that it is possible to predict the risk, based on experience from previous projects, *that* some such problems will haunt a project and how this will affect costs. We must also maintain that such risk can and should be accounted for in forecasts of costs, but typically is not. Moreover, major projects are prone to what Taleb (2007) calls "black swans," i.e. extreme events with low probability and high impact, but forecasts and risk assessments rarely reflect this. For explanations in terms of bad luck or error to be credible, they would have to explain why forecasts of performance are so consistent in ignoring cost and benefit risks, including in the extreme version of black swans.

For the above reasons, explanations of underperformance in terms of bad luck or error must today be considered falsified, despite their long historical reign. We need to look elsewhere for valid explanations of underperformance. We need to look at explanations in terms of optimism bias and strategic misrepresentation.

Optimism bias

Explanations of project underperformance in terms of optimism bias and strategic misrepresentation both see the high failure rates for projects as a consequence of flawed decision-making (Flyvbjerg, Garbuio, and Lovallo 2009). According to the first explanation—optimism bias—the flaw consists in managers falling victim to what psychologists call the planning fallacy (Buehler, Griffin, and Ross 1994). In its grip, managers make decisions based on delusional optimism rather than on a rational weighting of gains, losses, and probabilities. They overestimate benefits and underestimate costs and time. They involuntarily spin scenarios of success and overlook the potential for mistakes and miscalculations. As a result, managers pursue initiatives that are unlikely to come in on budget or on time, or to ever deliver the expected returns. These biases are often the result of the inside view in planning: decision-makers have a strong tendency to consider problems as unique and thus focus on the particulars of the case at hand when generating solutions (Kahneman and Lovallo 1993). Adopting an outside view of the problem has been shown to mitigate delusion. It is applied by ignoring the specific details of the project at hand and uses a broad reference class of similar projects to forecast outcomes for the current project, as we will see below.

When in the grip of the inside view, managers focus tightly on the case at hand, by considering the plan and the obstacles to its completion, by constructing scenarios of future progress, and by extrapolating current trends (Kahneman and Tversky 1979b; Lovallo and Kahneman 2003). In other words, by using typical bottom-up decision-making techniques, they think about a problem by bringing to bear all they know about it, with special attention to its unique details. The inside view facilitates two cognitive delusions, namely the planning fallacy and anchoring.

When forecasting the outcomes of risky projects, managers often fall victim to the planning fallacy. Psychologists have defined it as the tendency to underestimate task completion times and costs, even knowing that the vast majority of similar tasks have run late or gone over budget (Lovallo and Kahneman 2003). It is a well-established bias in the experimental literature. In one set of experiments, Buehler, Griffin, and Ross (1994) assessed the accuracy of psychology students' estimates of completion times for their year-long honors thesis project.

In the experiments, the students' "realistic" predictions were overly optimistic: 70 percent took longer than the predicted time, even though the question was asked toward the end of the year. On average, students took fifty-five days to complete their thesis, which was twenty-two days longer than predicted, i.e. a time overrun of 67 percent. Similar results have been found with various types of subjects and for a wide variety of tasks such as holiday shopping, filing taxes, and other routine chores (Buehler, Griffin, and MacDonald 1997; Newby-Clark, McGregor, and Zanna 2002).

These findings are not limited to experiments. Cost and time overruns are well documented in the provision of large-scale infrastructure projects (Flyvbjerg, Holm, and Buhl 2002; Mott MacDonald 2002; National Audit Office 2003, 2005). In business, executives and entrepreneurs seem to be highly susceptible to optimism. Studies that compared the actual outcomes of capital investment projects, mergers and acquisitions, and market entries with managers' original expectations for those ventures show a strong tendency towards over-optimism (Malmendier and Tate 2003). An analysis of start-up ventures in a wide range of industries found that more than 80 percent failed to achieve their market-share target (Dune, Roberts, and Samuelson 1988).

Anchoring and adjustment is another consequence of the inside view in thinking that leads to optimistic forecasts (Tversky and Kahneman 1974). Anchoring on plans is one of the most robust biases of judgment. The first number that is considered as a possible answer to a question serves as an "anchor." Even when people know that the anchor is too high or too low, their adjustments away from it are almost always insufficient.

In the context of planning for major projects there is always a plan, which is very likely to serve as an anchor. Furthermore, the plan that is developed is almost always seen as a "realistic" best or most likely case, developed according to what the World Bank (1994: ii. 22) calls the "EGAP principle," i.e. the assumption that Everything Goes According to Plan. Executives know that events may develop beyond the best or most likely case so they generally attempt to capture unforeseen costs by building in a contingency fund that is proportional to the size of the project. However, when compared with actual cost overruns, such adjustments are clearly and significantly inadequate (Flyvbjerg, Bruzelius, and Rothengatter 2003). Furthermore, the initial estimate serves as an anchor for later stage estimates, which therefore insufficiently adjust to the reality of the project's performance.

The power of these heuristics and biases is well illustrated in a field study where the Rand Corporation examined forty-four chemical pioneer process plants, owned by 3M, Du Pont, and Texaco, among others. Actual construction costs were over twice as large as the initial estimates (Merrow, Phillips, and Meyers 1981). Furthermore, at every subsequent stage of the process, managers underestimated the cost of completing the construction of the plants. Finally, even a year after

start-up about half of the plants (twenty-one) produced at less than 75 percent of their design capacity, with a quarter of the plants producing at less than 50 percent of their design capacity. Many of the plants in this latter category had their performance expectations permanently lowered.

Interestingly, however, when you ask forecasters about causes of inaccuracies in actual forecasts, they do not mention optimism bias as a main cause, whereas they will talk at length about scope changes, complexity, geology, and other unforeseen circumstances (Flyvbjerg, Holm, and Buhl 2005: 138–40). This may of course be because optimism bias is hard-wired and unconscious and thus not reflected by forecasters. After all, there is a large body of experimental evidence for the existence of optimism bias, referred to above. But the experimental data is mainly from simple, non-professional settings. This is a problem for psychological explanations, because it remains an open question whether such explanations are as general as they are presented to be, and thus to what extent they apply beyond the simple settings of the experiments from which the explanations were derived.

Optimism bias would be an important and credible explanation of underestimated costs and overestimated benefits in major project forecasting if estimates were produced by inexperienced forecasters, i.e. persons who were estimating costs and benefits for the first or second time and who were thus unknowing about the realities of major project development and were not drawing on the knowledge and skills of more experienced colleagues. Such situations may exist and may explain individual cases of inaccuracy. But given the fact that in modern society it is a defining characteristic of professional expertise that it is constantly tested—through scientific analysis, critical assessment, and peer review—in order to root out bias and error, it seems unlikely that a whole profession of forecasting experts would continue to innocently make the same mistakes decade after decade instead of learning from their actions. Learning would result in the reduction, if not elimination, of optimism bias, which would then result in estimates becoming more accurate over time.

But existing data clearly shows that this has not happened. Flyvbjerg, Holm, and Buhl (2002) show that cost underestimation in large transport infrastructure projects has been constant for seventy years. The profession of cost forecasters would indeed have to be an optimistic—and non-professional—group to keep their optimism bias throughout the seventy-year period of the study, and not learn that they were deceiving themselves and others by underestimating costs. This would account for the data, but is not a credible explanation. Therefore, on the basis of the data, one is led to reject optimism bias as a primary and singular cause of cost underestimation and benefit overestimation. Optimism bias may be part of the explanation of underperformance but does not appear to be the whole explanation.

Strategic misrepresentation

The second explanatory model for project underperformance—strategic misrepresentation—accounts for flawed planning and decision-making in terms of political pressures and agency issues. Agency issues are covered in detail in Flyvbjerg, Garbuio, and Lovallo (2009). In what follows, the focus is therefore on explanations of project underperformance in terms of political and organizational pressures. Strategic misrepresentation is the second root cause of project underperformance; optimism bias was the first.

Whereas the first explanation is psychological, the second is political. According to this model, politicians, planners, or project champions deliberately and strategically overestimate benefits and underestimate costs in order to increase the likelihood that their projects, and not their competition's, gain approval and funding. This explanatory model has been set forth by Flyvbjerg, Holm, and Buhl (2002, 2005) and Wachs (1989, 1990). According to the model, actors purposely spin scenarios of success and gloss over the potential for failure. This results in managers promoting ventures that are unlikely to come in on budget or on time, or to deliver the promised benefits.

Strategic misrepresentation can be traced to political and organizational pressures, for instance competition for scarce funds or jockeying for position, and it is rational in this sense. If we now define a lie in the conventional fashion as making a statement intended to deceive others (Bok 1979: 14; Cliffe, Ramsey, and Bartlett 2000: 3), we see that deliberate misrepresentation of costs and benefits is lying, and we arrive at one of the most basic explanations of lying that exists: Lying pays off, or at least agents believe it does. Where there is political pressure there is misrepresentation and lying, according to this explanation. However, misrepresentation, lying, and failure can be moderated by measures that enhance transparency, provide accountability, and align incentives.

Explanations of underperformance in terms of strategic misrepresentation account well for the systematic underestimation of costs and overestimation of benefits found in the data. A strategic estimate of costs would be low, resulting in cost overrun, whereas a strategic estimate of benefits would be high, resulting in benefit shortfalls. A key question for explanations in terms of strategic misrepresentation is whether estimates of costs and benefits are intentionally biased to serve the interests of promoters in getting projects started. This question raises the difficult issue of lying. Questions of lying are notoriously hard to answer, because per definition a lie consists in making a statement intended to deceive others, and in order to establish whether lying has taken place, one must therefore know the intentions of actors. For legal, economic, moral, and other reasons, if promoters and managers have intentionally cooked estimates of costs and benefits to get a project started, they are unlikely to formally tell researchers or others that this is the case, because this could lead to sanctions. Despite such problems, two studies exist that succeeded in getting forecasters and managers to talk about strategic misrepresentation (Flyvbjerg and Cowi 2004; Wachs 1990).

Flyvbjerg and Cowi (2004) interviewed managers, public officials, planners, and consultants who had been involved in the development of large UK transportation infrastructure projects. In sum, their study shows that strong interests and strong incentives exist at the project approval stage to present projects as favorably as possible, that is, with benefits emphasized and costs and risks de-emphasized. Local authorities, local developers and landowners, local labor unions, local politicians, local officials, local MPs, and consultants all stand to benefit from a project that looks favorable on paper and they have little incentive to actively avoid bias in estimates of benefits, costs, and risks. National bodies, like certain parts of the Department for Transport and the Treasury who fund and oversee projects, may have an interest in more realistic appraisals, but until recently they have had little success in achieving such realism, although the situation may be changing with the initiatives to curb bias set out in HM Treasury (2003) and UK Department for Transport (2006).

Wachs (1986, 1990) found similar results for transit planning in the USA, also based on interviews that teased out the intentions of actors in order to establish whether lying took place or not. Taken together, the UK and US studies both account well for existing data on cost underestimation and benefit overestimation. Both studies falsify the notion that in situations with high political and organizational pressure the underestimation of costs and overestimation of benefits is caused by non-intentional error or optimism bias. Both studies support the view that in such situations promoters and forecasters intentionally use the following formula in order to secure approval and funding for their projects:

Underestimated costs + Overestimated benefits = Project approval

Using this formula results in an inverted Darwinism, i.e. the "survival of the unfittest." It is not the best projects that get implemented, but the projects that are artificially and misleadingly made to look best on paper. And such projects are the projects with the largest cost underestimates and benefit overestimates, other things being equal. But these are the worst, or "unfittest," projects in the sense that they are the very projects that will encounter most problems during implementation in terms of the largest cost overruns, benefit shortfalls, and risks of non-viability. They have been designed like that.

Explanatory power of optimism bias vs. strategic misrepresentation

We saw above how political and organizational pressure may influence and bias the outcome of the business case in major project management. Explanations of outcome in terms of optimism bias have their relative merit in situations where political and organizational pressures are absent or low, whereas such explanations hold

less power in situations where political pressures are high. Conversely, explanations in terms of strategic misrepresentation have their relative merit where political and organizational pressures are high—this being the situation for most major projects—while they become immaterial when such pressures are not present.

Thus, rather than compete, the two types of explanation complement each other: one is strong where the other is weak, and both explanations are necessary to understand the phenomenon at hand—the pervasiveness of bias in major project management. It has been a problem until recently that optimism bias was presented as a global model, i.e. it was seen by its proponents as explaining all or most bias in human decision-making (Kahneman and Lovallo 2003). With the findings on strategic misrepresentation presented above, this view can no longer be upheld; it has been falsified in the Popperian manner. This does not mean that explanations in terms of optimism bias have no value, needless to say. It just means they are not as global as first assumed, which is a perfectly normal development for new theories as they are tried out in more and more areas. We need to combine optimism with strategic misrepresentation to get a fuller picture of what transpires in decision-making, and especially when we want to understand situations that are more complex—like major projects—than the simple experimental situations from which optimism models were developed. We also need to combine the two types of explanation when contemplating how to cure problems of bias and misrepresentation in decision-making.

Taking the outside view

When considering what project managers can do to root out bias in decisions on major projects, we need to distinguish between two fundamentally different situations: (1) project managers consider it important to get estimates of costs, benefits, and risks right, and (2) project managers do not consider it important to get estimates right, because optimistic estimates are seen as a necessary means to getting projects started. The first situation is the easier one to deal with and here better methodology will go a long way in improving project management. The second situation is more difficult, and more common for political projects as we saw above. Here changed incentives are essential in order to reward honesty and punish deception, where today's incentives often do the exact opposite.

Thus two main measures of reform are (1) better forecasting methods, and (2) improved incentive structures, with the latter being the more important, because political problems cannot be solved by technical means. Better forecasting methods are covered in this section, better incentives in the next.

If project managers genuinely consider it important to get forecasts right, it is recommended they use a new forecasting method called "reference class forecasting"

to reduce inaccuracy and bias. This method was originally developed to compensate for the type of cognitive bias in human forecasting that Princeton psychologist Daniel Kahneman found in his Nobel prize-winning work on bias and uncertainty in decision-making (Kahneman 1994; Kahneman and Tversky 1979a). Reference class forecasting has proven more accurate than conventional forecasting. It was used in project management in practice for the first time in 2004 (Flyvbjerg and Cowi 2004), in 2005 the method was officially endorsed by the American Planning Association (2005), and since then it has been used by governments and private companies in the UK, the Netherlands, Denmark, Switzerland, Australia, and South Africa, among others.

For reasons of space, here only an outline of the method is presented, based mainly on Lovallo and Kahneman (2003) and Flyvbjerg (2006). Reference class forecasting consists in taking a so-called "outside view" on the particular project being forecasted. The outside view is established on the basis of information from a class of similar projects. The outside view does not try to forecast the specific uncertain events that will affect the particular project, but instead places the project in a statistical distribution of outcomes from this class of reference projects. Reference class forecasting requires the following three steps for the individual project:

1. Identification of a relevant reference class of past projects. The class must be broad enough to be statistically meaningful but narrow enough to be truly comparable with the specific project.
2. Establishing a probability distribution for the selected reference class. This requires access to credible, empirical data for a sufficient number of projects within the reference class to make statistically meaningful conclusions.
3. Compare the specific project with the reference class distribution, in order to establish the most likely outcome for the specific project.

Figure 13.1 shows what reference class forecasting does in statisticians' language. First, reference class forecasting regresses the best guess of the conventional forecast—here the project promoters' forecast, indicated by the dashed curve—toward the average of the reference class. The distribution of outcomes in the reference class is indicated by the dotted curve. Second, reference class forecasting expands the estimate of interval in the conventional forecast to the interval of the reference class.

With an example from major project management, planners in a city preparing to build a new subway would, first, establish a reference class of comparable projects. Through analyses the planners would establish that the projects included in the reference class were indeed comparable.

Second, if the planners were concerned, for example, with getting construction cost estimates right, they would then establish the distribution of outcomes for the reference class regarding the accuracy of construction cost forecasts. Figure 13.2 shows what this distribution looks like for a reference class relevant to building

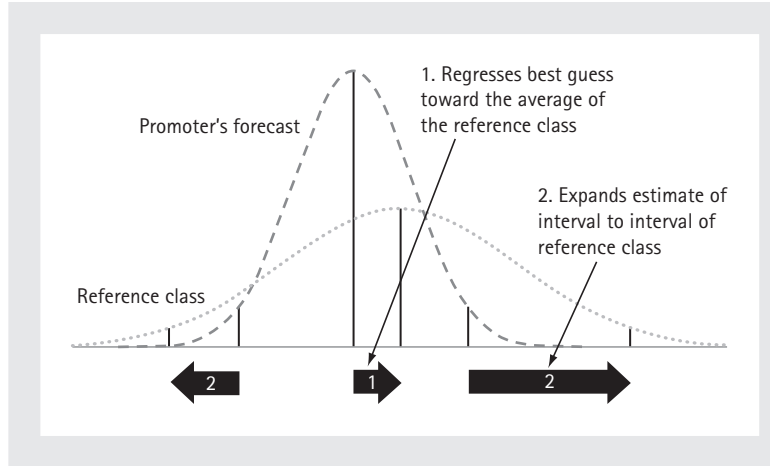

Fig. 13.1 What reference class forecasting does, in statisticians' language

subways in the UK, developed by Flyvbjerg and Cowi (2004: 23) for the UK Department for Transport.

Third, the planners would compare their subway project to the reference class distribution. This would make it clear to the planners that unless they have reason to believe they are substantially better forecasters and planners than their colleagues who did the forecasts and planning for projects in the reference class, they are likely to grossly underestimate construction costs. It is part of reference class forecasting to establish whether evidence for such reasons exists, and if it does (uncommon) to adjust the forecast for this, and if it doesn't (common) to make sure that assumed risks in the forecast are similar to actual risks in the reference class.

Finally, planners would then use this knowledge to adjust their forecasts for more realism. Figure 13.3 shows what such adjustments are for the UK situation and these adjustments are actually used by the UK Department for Transport in the manner described here to cost proposed rail projects. More specifically, Figure 13.3 shows that for a forecast of construction costs for a rail project, which has been planned in the manner that such projects are usually planned, i.e. like the projects in the reference class, this forecast would have to be adjusted upwards by 40 percent, if investors were willing to accept a risk of cost overrun of 50 percent. If investors were willing to accept a risk of overrun of only 10 percent, the uplift would have to be 68 percent. For a rail project initially estimated at, say, £4 billion, the uplifts for the 50 and 10 percent levels of risk of cost overrun would be £1.6 billion and £2.7 billion, respectively.

The capital cost of the proposed Edinburgh Tram Line 2 was estimated like this. An initial cost estimate of £320 million made by planners was adjusted for optimism bias and acceptable risk, using the probability distribution in Figure 13.2. This

resulted in a new cost estimate of £400 million, including contingencies to insure against cost overruns at the 80 percent level, i.e. with a 20 percent risk of overrun. If the Scottish Parliament, who were underwriting the investment, were willing to accept a risk of overrun of 50 percent, then the cost estimate including contingencies could be lowered to £357 million. Insurance is expensive, here as elsewhere, and the marginal cost of insurance against cost overruns increases as the level of acceptable risk decreases, as seen in Figure 13.3.

The contrast between inside and outside views has been confirmed by systematic research (Gilovich, Griffin, and Kahneman 2002). The research shows that when people are asked simple questions requiring them to take an outside view, their forecasts become significantly more accurate. However, most individuals and organizations are inclined to adopt the inside view in planning major initiatives.

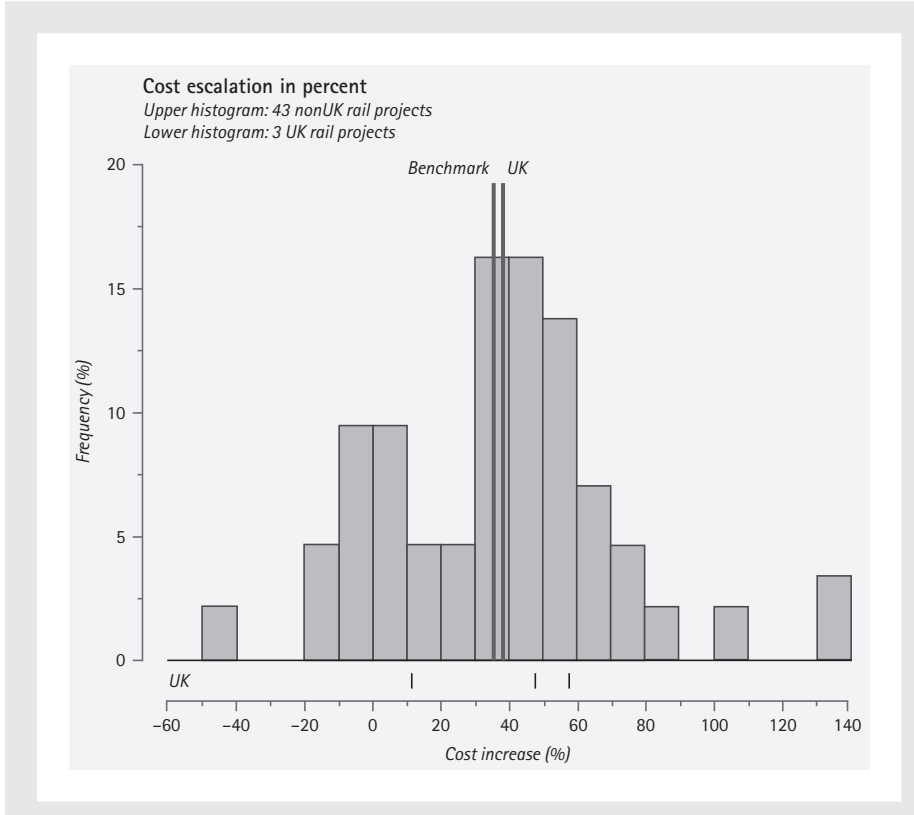

Fig. 13.2 Inaccuracy of construction cost forecasts for rail projects in reference class.

Note: Average cost increase is indicated for non-UK and UK projects, separately. Constant prices.

Source: Flyvbjerg and Cowi (2004).

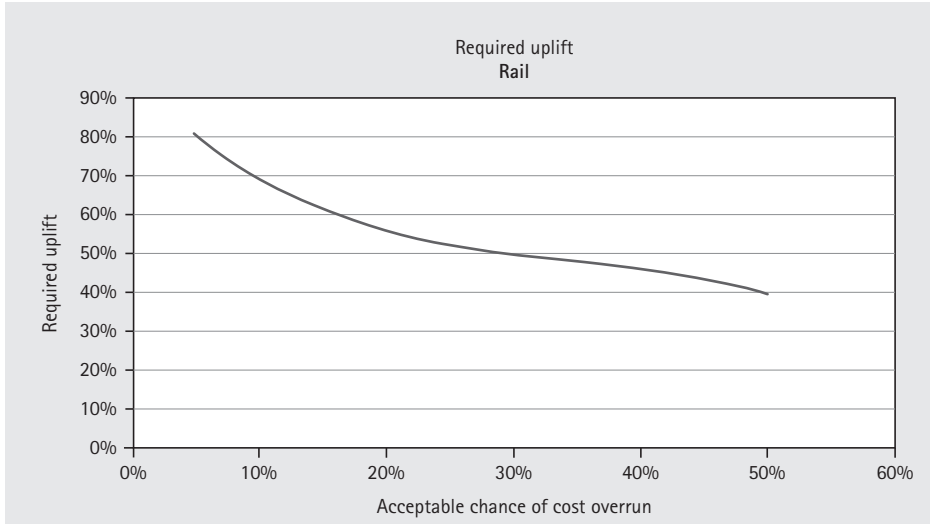

Fig. 13.3 Required adjustments to cost estimates for UK rail projects as function of the maximum acceptable level of risk for cost overrun.

Note: Constant prices.

Source: Flyvbjerg and Cowi (2004).

This is the conventional and intuitive approach. The traditional way to think about a complex project is to focus on the project itself and its details, to bring to bear what one knows about it, paying special attention to its unique or unusual features, trying to predict the events that will influence its future. The thought of going out and gathering simple statistics about related cases seldom enters a manager's mind. This is the case in general, according to Lovallo and Kahneman (2003: 61–2). And it is certainly the case for cost and benefit forecasting in large infrastructure projects. Despite the many forecasts the author and his associates have reviewed, before the Edinburgh Tram forecast, which is based on their research, they had not come across a single genuine reference class forecast of costs and benefits. Neither had Daniel Kahneman, who first conceived the idea of the reference class forecast.

While understandable, managers' preference for the inside view over the outside view is unfortunate. When both forecasting methods are applied with equal skill, the outside view is much more likely to produce a realistic estimate. That is because it bypasses cognitive and political biases such as optimism bias and strategic misrepresentation and cuts directly to outcomes. In the outside view managers and forecasters are not required to make scenarios, imagine events, or gauge their own and others' levels of ability and control, so they cannot get all these things wrong. Surely the outside view, being based on historical precedent, may fail to predict

extreme outcomes, that is, those that lie outside all historical precedents. But for most projects, the outside view will produce more accurate results. In contrast, a focus on inside details is the road to inaccuracy.

The comparative advantage of the outside view is most pronounced for non-routine projects, understood as projects that managers in a certain locale have never attempted before—like building an urban rail system in a city for the first time, or launching a completely new product to the market. It is in the planning of such new efforts that the biases toward optimism and strategic misrepresentation are likely to be largest. To be sure, choosing the right reference class of comparative past projects becomes more difficult when managers are forecasting initiatives for which precedents are not easily found, for instance the introduction of new and unfamiliar technologies. However, many major projects are both non-routine locally and use well-known technologies. Such projects are, therefore, particularly likely to benefit from the outside view and reference class forecasting.

Reference class forecasting is useful as a point of departure for "predict and prevent" strategies in forecasting (as opposed to "predict and provide," Owens 1995). A reference class forecast will show managers and clients where their project is heading if it performs like the projects in the reference class, which is the common situation. But such an outcome may be unacceptable to those involved. In this case, the immediate task becomes one, not only of providing contingencies for delays, cost overruns, and benefit shortfalls, but of devising a strategy that prevents these from occurring, i.e. a strategy to beat performance in the reference class, something that is difficult but not impossible to do. Pitsis et al. (2003) describe how it was done through the strategy of "future perfect thinking" for a major tunneling project that formed part of the infrastructure for the Sydney 2000 Olympics. Tunnels have a poor performance record in terms of delays and cost overruns (Flyvbjerg, Holm, and Buhl 2002), but the Sydney tunnel was built on time and almost on budget, according to Pitsis et al.

Improved incentives and accountability

In the present section we consider the situation where project managers and other influential actors do not find it important to get forecasts right and where managers, therefore, do not help to clarify and mitigate risks but, instead, generate and exacerbate them. Here project managers are part of the problem, not the solution. This situation may need some explication, because it possibly sounds to many like an unlikely state of affairs. After all, it may be agreed that project managers ought to be interested in being accurate and unbiased in their work. It is even stated in the Project Management Institute's Code of Ethics and Professional Conduct

(2006: 4, 5) that project managers should "provide accurate information in a timely manner" and they must "not engage in or condone behavior that is designed to deceive others." But there is a dark side to project management, which is remarkably underexplored in the literature (Flyvbjerg 1996).

On the dark side, project managers and planners "lie with numbers," as Wachs (1989) has aptly put it. They are busy not with getting forecasts and business cases right and following the PMI Code of Ethics but with getting projects funded and built. And accurate forecasts are often not an effective means for achieving this objective. Indeed, accurate forecasts may be counterproductive, whereas biased forecasts may be effective in competing for funds and securing the go-ahead for a project. "The most effective planner," says Wachs (1989: 477), "is sometimes the one who can cloak advocacy in the guise of scientific or technical rationality." Such advocacy would stand in direct opposition to PMI's ruling that project managers should "make decisions and take actions based on the best interests of society" (Project Management Institute 2006: 2).

Nevertheless, seemingly rational forecasts that underestimate costs and overestimate benefits have long been an established formula for project approval as we saw above. Forecasting is here mainly another kind of rent-seeking behavior, resulting in a make-believe world of misrepresentation which makes it extremely difficult to decide which projects deserve undertaking and which do not. The consequence is that too many projects proceed that should not, and that many projects don't proceed that probably should, had they not lost out to projects with "better" misrepresentation (Flyvbjerg, Holm, and Buhl 2002).

In this situation, the question is not so much what project managers can do to reduce inaccuracy and risk in forecasting, but what others can do to impose on project managers the checks and balances that would give managers the incentive to stop producing biased forecasts and begin to work according to their Code of Ethics. The challenge is to change the power relations that govern forecasting and project development. Better forecasting techniques and appeals to ethics won't do here; institutional and organizational change with a focus on transparency and accountability is necessary.

As argued in Flyvbjerg, Bruzelius, and Rothengatter (2003), two basic types of accountability define liberal democracies: (1) public sector accountability through transparency and public control, and (2) private sector accountability via competition and markets. Both types of accountability may be effective tools to curb misrepresentation in project management and to promote a culture which acknowledges and deals effectively with risk, especially where large amounts of taxpayers' money are at stake and for projects with significant social and environmental impacts, as are common for major projects. In order to achieve accountability through *transparency and public control*, the following would be required as practices embedded in the relevant institutions (the full argument for the measures may be found in Flyvbjerg, Bruzelius, and Rothengatter 2003: chapters 9–11):

- National-level government should not offer discretionary grants to local agencies for the sole purpose of building a specific type of project (aka "categorical grants"). Such grants create perverse incentives. Instead, national government should simply offer "block grants" to local governments, and let local political officials spend the funds however they choose to, but make sure that every dollar they spend on one type of project reduces their ability to fund another.
- Forecasts and business cases should be made subject to independent peer review, for instance by national audit offices.
- Forecasts should be benchmarked against comparable forecasts, for instance using reference class forecasting as described in the previous section.
- For publicly funded projects, forecasts, peer reviews, and benchmarkings should be made available for public scrutiny, including by the media.
- Public hearings, citizen juries, and the like should be organized to allow stakeholders and civil society to voice criticism and support of forecasts.
- Scientific and professional conferences should be organized where forecasters would present and defend their forecasts in the face of colleagues' scrutiny and criticism.
- Projects with inflated benefit–cost ratios should be reconsidered and stopped if recalculated costs and benefits do not warrant implementation. Projects with realistic estimates of benefits and costs should be rewarded.
- Professional and occasionally even criminal penalties should be enforced for managers and forecasters who consistently and foreseeably produce deceptive forecasts (Garett and Wachs 1996).

When the author first began suggesting, in lectures for project managers and forecasters, that deception and criminal penalties may be concepts relevant to our profession, he would get headshakes, sighs, and the occasional boo. Enron and Iraq changed that, almost overnight. Today people listen and the literature has become replete with books and articles that hammer out the links between lying, forecasting, and management. For instance, a recent book popularizing optimism bias, the planning fallacy, and strategic misrepresentation bluntly states: "Anyone who causes harm by forecasting should be treated as either a fool or a liar. Some forecasters cause more damage to society than criminals" (Taleb 2007: 163). Law-making has followed suit, most prominently with the 2002 Sarbanes–Oxley Act, which stipulates up to twenty years in prison for a knowingly false forecast intended to impede, obstruct, or influence the proper administration of affairs. There is little doubt that penalties like this influence behavior. The point is that malpractice in project management should be taken as seriously as it is in other professions, e.g. medicine and law. Failing to do this amounts to not taking the profession of project management seriously.

In order to achieve accountability in forecasting via *competition and market control*, the following would be required, again as practices that are both embedded in and enforced by the relevant institutions:

- The decision to go ahead with a major project should, where at all possible, be made contingent on the willingness of private financiers to participate without a sovereign guarantee for at least one third of the total capital needs. (A sovereign guarantee is a guarantee where government takes on the risk of paying back a loan, even if the loan was obtained in the private lending market. The lower limit of a one-third share of private risk capital for such capital to effectively influence accountability is based on practical experience (Flyvbjerg, Bruzelius, and Rothengatter 2003: 120–3).) This should be required whether projects pass the market test or not, that is, whether projects are subsidized or not or provided for social justice reasons or not.
- Forecasters and their organizations must share financial responsibility for covering cost overruns and benefit shortfalls resulting from misrepresentation and bias in forecasting.
- The participation of risk capital would not mean that government reduces control of major projects. On the contrary, it means that government can more effectively play the role it should be playing, namely as the ordinary citizen's guarantor for ensuring concerns about safety, environment, risk, and a proper use of public funds.

Whether projects are public, private, or public–private, they should be vested in one and only one project organization with a strong governance framework. The project organization may be a company or not, public or private, or a mixture. What is important is that this organization enforces accountability vis-à-vis contractors, operators, etc., and that, in turn, the directors of the organization are held accountable for any cost overruns, benefits shortfall, faulty designs, unmitigated risks, etc. that may occur during project planning, implementation, and operations.

If the institutions with responsibility for developing and building major projects would effectively implement, embed, and enforce such measures of accountability, then the misrepresentation in cost, benefit, and risk estimates, which is widespread today, might be mitigated. If this is not done, misrepresentation is likely to continue, and the allocation of funds for major projects is likely to keep on being wasteful, unethical, and sometimes even unlawful.

Glimmers of hope

Fortunately, signs of improvement have recently appeared. The tacit consensus that deception is an acceptable business model for major project development is under attack. At a 2009 White House Fiscal Responsibility Summit, President Obama openly identified "the costly overruns, the fraud and abuse, the endless excuses" in

public procurement for major projects as key problems (White House 2009). The *Washington Post* (February 24, 2009) rightly called this "a dramatic new form of discourse." Before Obama it was not *comme il faut* to talk about overruns, deception, and abuse in relation to major projects, although they were of epidemic proportions then as now, and the few who did so were ostracized. However, we cannot solve problems we cannot talk about. So talking is the first step.

A more material driver of improvement is the fact that the largest projects are now so big in relation to national economies that cost overruns, benefit shortfalls, and risks from even a single project may destabilize the finances of a whole country or region, as happened with the 2004 Olympics in Athens, where cost overruns were so large they negatively affected the credit rating of all of Greece. Similarly, when the new international airport in Hong Kong opened, computer glitches led to large revenue shortfalls that damaged Hong Kong's GNP (Flyvbjerg 2005). In the UK at the beginning of the century, cost underestimation and overrun was running rampant in so many projects in so many ministries that the reliability of national budgets suffered, leading the chancellor to order a Green Book on the problem and how to solve it (HM Treasury 2003). This move inspired other countries to follow suit. Law-makers and governments have begun to see that national fiscal distress and unreliable national budgets are too high a price to pay for the conventional way of managing major projects.

In addition, with private finance in major projects on the rise over the past fifteen to twenty years, capital funds and banks are increasingly gaining a say in the project development and management process. Private capital is no panacea for the ills in major project management, to be sure; in some cases private capital may even make things worse (Hodge and Greve 2009). But private investors place their own funds at risk, as opposed to governments who place the taxpayer's money at risk. Capital funds and banks can therefore be observed to not automatically accept at face value the forecasts of project managers and promoters. Banks typically bring in their own advisers to do independent forecasts, due diligence, and risk assessments, which is an important step in the right direction. The false assumption that one forecast or one business case (which is also a forecast) may contain the truth about a project is problematized. Instead project managers and promoters are getting used to the healthy fact that different stakeholders hold different forecasts and that forecasts are not only products of objective science and engineering but of negotiation. Why is this more healthy? Because it is more truthful about our ability to predict the future and about the risks involved.

Finally, democratic governance is generally getting stronger around the world. The Enron scandal and its successors have triggered new legislation and a war on corporate deception that is spilling over into government with the same objective: to curb financial waste and promote good governance. Although progress is slow, good governance is gaining a foothold even in major project management. The main drivers of reform come from outside the agencies and industries conventionally

involved in major project management, which is good because it increases the likelihood of success.

For example, in 2003 the Treasury of the United Kingdom required, for the first time, that all ministries develop and implement procedures for major projects that will curb what the Treasury calls—with true British civility—"optimism bias." Funding will be unavailable for projects that do not take into account this bias, and methods have been developed for how to do this (HM Treasury 2003; Flyvbjerg and Cowi 2004; UK Department for Transport 2006). In the Netherlands in 2004, the Parliamentary Committee on Infrastructure Projects for the first time conducted extensive public hearings to identify measures that will limit the misinformation about large infrastructure projects given to the Parliament, public, and media (Tijdelijke Commissie Infrastructuurprojecten 2004). In Boston, the government has sued to recoup funds from contractor overcharges for the Big Dig related to cost overruns. More countries and cities are likely to follow the lead of the UK, the Netherlands, and Boston in coming years; Switzerland and Denmark are already doing so (Swiss Association of Road and Transportation Experts 2006; Danish Ministry for Transport and Energy 2006, 2008).

It's too early to tell whether the measures being implemented will ultimately be successful. It seems unlikely, however, that the forces that have triggered the measures will be reversed, and it is those forces that reform-minded groups need to support and work with in order to curb deception and waste. This is the "tension-point" where convention meets reform, power balances change, and new things are happening.

Implications for research

If academic research is to contribute constructively and proactively to much-needed reform in major project management, we need to better understand:

- the trends that shape projects and project management, like those described above.
- strong theories of success and failure in major project management. Today too much theory in research on major project management is not intellectually robust having only weak links to leading research in economics, governance, planning, decision-making, environment, etc. Focusing on strong theory would help us bring the field forward academically. It would also allow us to develop better tools for preventing failure and replicating success.
- the importance of good data. Data on performance in major projects is generally of poor quality and is often idiosyncratic in the sense that it cannot be compared

systematically across projects and thus does not allow for statistical analyses and tests. This seriously sets back research, policy, and management. At present, the single most important thing we can do to heighten the academic level of research on major project management is to develop high-quality data that allows for systematic comparison across projects.

- the paradox that investing in and delivering major projects is a high-risk, stochastic activity, with high exposure to uncontrollable so-called "black swans"—much like investing in financial markets—but project managers and researchers widely ignore this state of affairs and thus underestimate the risks involved, still treating projects as if they exist largely in a deterministic Newtonian world of cause, effect, and control, despite all evidence to the contrary.

For someone embarking on a Ph.D. or similar research in major project management, taking these issues into account—and especially walking the extra mile for high-quality data and strong theory—would ensure a valuable contribution to the field and a comparative advantage over average research. This would benefit not only the researcher in question, but all of us, because it would raise the bar in a field that needs this to happen.

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