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Conflicting Notions of a Project: The Battle Between Albert O. Hirschman and Bent Flyvbjerg

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Abstract

The field of project management has erected an impressive edifice of knowledge that apparently hinders us in learning anything from experience except what we already know. We will use the recent controversy between Hirschman and Flyvbjerg to trace this academic imposition to a narrow notion of a project and to find inspiration in a radically different notion for opening the field to new types of issues and lessons.

Keywords

Hiding Hand, Providential Ignorance, practical judgment, project notions, project success

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Introduction

The winner's curse is a celebrated existential paradox; also, in the world of projects, what we strived for may not be what we want when we get it because our sacrifices proved to outweigh the benefits (Thaler, 1992). Perhaps, there is also a risk of the "winner's curse" for project management research; the winning paradigm may have won its dominant position by sacrificing some of its intellectual curiosity. It seems that even when we try to learn from experience, we tend to confirm what we already know. When we try to kill bad ideas with sound methodology, often we reinvigorate the bad ideas. When we reach out to new disciplines or ideas, we tend to reduce them to fit our presumptions. When we invite academic debate, we end up petrifying it. What if, when we try to rethink the field of project studies, we risk killing the thinking?

In this essay, we will illustrate the risk of the winner's curse by reflecting on the reception of Albert O. Hirschman's thinking in the field of project management. The least of the problem is perhaps that his ideas have been ignored, by and large. A few scholars have acknowledged his ideas and have portrayed him as "an early rethinker of PM [project management]" (Ika & Söderlund, 2016), while other scholars boldly rejected his ideas as being both wrong and deceptive (Flyvbjerg, 2016a; Flyvbjerg & Sunstein, 2016). It seems that Hirschman is an odd bedfellow for project management.

The following reflections aim to learn from the ill fate of Hirschman's work. We do not assume that Hirschman has already done the rethinking because we are the ones who are challenged to do so. The point is not to accept or reject his ideas and concepts but to take inspiration from them. It is the presumption of the following reflections that Hirschman is offering a refreshing new view on central issues in project management that may allow us to imagine an alternative to the awkward body of

knowledge in which the field is currently entrapped, philosophically, theoretically, and practically.

Let us begin by reviewing the remarkable academic controversy between Hirschman and Flyvbjerg to show how a conventional paradigm in project management defends its dominance to its detriment. This controversy may give us a hint about what we are up against if we want to rethink project management.

We situate the controversy between Hirschman and Flyvbjerg at a safe distance from the battlefields on which scholars currently fight fiercely over data, methodologies, and even the proper rendering of Hirschman's ideas and intentions (Flyvbjerg, 2016a, 2018; Flyvbjerg et al., 2018; Flyvbjerg et al., 2019; Flyvbjerg et al., 2002; Ika, 2018; Lepenies, 2018; Love, Ika, & Ahiaga-Dagbui, 2019; Love, Sing, Ika, & Newton, 2019; Room, 2018). We maintain our distance from these fights by insisting that, in our case, the controversy stems from conflicting notions of a project, what it is, and what it should be. In our view, it is unlikely that rethinking starts with more data and better methodologies; it probably starts with a critical reflection on what it is we are thinking about.

Hirschman Versus Flyvbjerg

In 1967, Hirschman published *Development Projects Observed*. One of the aims of this book was to challenge the World Bank's notion of projects and their appraisal. He proposed to understand projects also in terms of their side effects and developmental by-products, suggesting a different rationale for project success than simple compliance with original business cases and cost-benefit analyses (Hirschman, 2014). However, his ideas and observations failed to convince the World Bank, never changed its practice, and never caught on in the rest of the field of project management (Alacevich, 2014).

With few exceptions (e.g., Ika & Söderlund, 2016), Hirschman's seemingly unimpactful ideas about projects were left to rest in peace until now, 50 years later, when Hirschman's once-upon-a-time heresy has suddenly been subject to academic fire and fury. Having amassed much empirical evidence on the underperformance of large projects, Flyvbjerg (2016b) felt urged to wage war not only on Hirschman's ideas but also on his intellectual persona and legacy. From his vantage point, he saw Hirschman peddling wrong views, misrepresenting his findings, and misleading his readers.

It is perhaps no big wonder that Hirschman and Flyvbjerg would come to look upon reality in conflicting ways. After all, Hirschman was a "planner who really didn't believe in planning" (Gladwell, 2013), realizing that some drivers of change and development went under the planners' radar, while Flyvbjerg is a planner who strongly believes in the type of planning that aims to "get projects right from the outset" (2017b, p. 13). Hirschman and Flyvbjerg could also be claimed to differ in their primary focus. The former was impressed with the muddling-through character of projects (Lindblom, 1959) and searched for ways to appraise their achievements retrospectively, while the latter focuses on the premises of prospective capital investment decisions and aims to make them more realistic. Likewise, Hirschman inclined toward a broader notion of rationality, in the sense of changing the world in a desirable direction, while Flyvbjerg inclines toward a thinner notion of rationality, in the sense of making consistent choices (Elster, 2016).

Before reflecting further on the implications of this controversy between Hirschman and Flyvbjerg, let us focus on the central point of contention, the principle of the Hiding Hand.

The Principle of the Hiding Hand

When evoking the controversy, Flyvbjerg targeted one of Hirschman's most central ideas, the principle of the Hiding Hand, and the accompanying notion of Providential Ignorance. Here follows a short, somewhat idiosyncratic account of these ideas.

Projects of any kind and size face inherent and genuine uncertainty because they project action and outcomes into a future that cannot be known in advance, only forecasted. Forecasts imply "a large component of judgment, intuition, and educated guesswork" (Kahneman & Tversky, 1979, p. 33). Consequently, all projects are destined to face challenges and difficulties once they get started, simply because the forecasts rendered the subsequently experienced reality incorrectly or incompletely. However, since we initially thought and talked about projects as already accomplished actions (Schutz, 1973), our forecasts may have taken on the genuine nature of foreknowledge, which may further have fostered a feeling of being in control of the future by means of our intentions and decisions. Time is conflated into discrete moments of before and after, and one gets the sense that change will be implanted in the post-project reality simply by a priori choice and design (O'Driscoll & Rizzo, 1996/1985). When outcomes deviate from anticipations, which they repeatedly do, it continues to come as a surprise and to create a sense of betrayal. The Hiding Hand is Hirschman's metaphor for our ability to neglect such consistent experiences and, therefore, to remain able to be surprised when history repeats itself. Alternatively, we might have learned from such experience that, in practice, the future is impossible to predict, let alone to choose, but we have not. We might have come to see future outcomes as a case of macrobehavior (Schelling, 1978), that is, the aggregate and cumulative outcome of multiple, parallel, and independent processes extended in real

time, but we continue to consider them a product of our own, sometimes bad, intentions and choices.

The Hiding Hand helps us to maintain trust in our ability to foresee and choose the future. It does so by enabling us to ignore all the reasons why this cannot be the case: “The Hiding Hand does its work essentially through ignorance of ignorance, of uncertainties, and of difficulties” (Hirschman, 2014, p. 27). Note that the Hiding Hand does not hide the specific challenges and difficulties that will arise in the future because nobody knows (or can know) such things ahead of time—there is nothing to hide. It is the fact that we do not know, and cannot know, the future that is hidden, implying that we believe we need to know less than we do to project our acts into the future rationally. The Hiding Hand hides our ignorance about the future, saving us the worry that we must act purposely without knowing the exact consequences of our actions—but, of course, not saving us from the inevitable troubles in the future when stumbling into unforeseen situations.

Thus, the Hiding Hand lures us into committing to a project on the false premise that we know enough about the future to plan and to ensure the realization of our intended goals. Such false premises imbue all projects with a dubious start, which seems likely to translate into the significant underperformance that Flyvbjerg (2011) has uncovered. It appears that the Hiding Hand predestines projects to failure and subsequent regrets. Had the sponsors known the uncertainties and difficulties that the Hiding Hand hid, they might well have decided against starting the project in the first place.

However, Hirschman added a new dimension to the conventional narrative described above. In his studies of significant development projects, he searched for and

found surprising and salient impacts that had neither been planned nor openly pursued. Such side effects materialized, for example, when projects suddenly faced unexpected obstacles that forced them to invent new solutions or workarounds. They also materialized from the projects' gearing into the wider world, where they left traces potentially aiding some wider developmental purpose or that possibly established new premises for the projects' progress. If we retrospectively come to see such side effects as valuable, we may also come to appreciate the falsely premised (and, in many other respects, disappointing) projects for having occasioned something valuable, unintentionally and sometimes unobtrusively. Hirschman dubbed this phenomenon Providential Ignorance, suggesting that projects started on false premises may strike it lucky and accomplish something good. Therefore, occasionally, they should be appreciated rather than regretted retrospectively. In this vein, Hirschman:

made the pitch for the "centrality of side effects." This was an amplified earlier recommendation to look out for unexpected rewards from projects. But the gist was, more or less, the same: widen the lens when evaluating projects and look out for those unplanned and hard-to-quantify dividends. (Adelman, 2013, pp. 401–402)

While occasionally providential, ignorance is not, of course, the cause of positive side effects. Also, ignorance is not the cause of projects getting started. Projects start because somebody chooses to pursue a purpose or goal that is considered important and worthwhile. In a complex and unpredictable reality, there is an inherent risk that the project will not fulfill that purpose, at least not at a reasonable cost. Ignorance of such risks makes it easier to muster commitment to the designed project with the fulfillment of the purpose as the projected and promised outcome.

Thus, projects are predestined to run, sooner or later, into dead ends, on which occasions those being held accountable for the project will be forced to improvise, that is, to think and act creatively by doing things that were neither planned nor preconceived but that, hopefully, will prove appropriate and useful. While far from guaranteeing the ultimate success in any traditional sense, such improvisation in the face of unforeseen situations may cause something new and valuable to be discovered and achieved. Since no planned situation will ever be planned in enough detail, all projects will repeatedly depend on some measure of improvisation and creativity to get ahead and reach completion. If success were considered dependent on such uncontrollable and unspecifiable future action, the whole venture might easily appear too risky. Thus, the projects would not likely be started. Therefore, they would also not run into situations that force somebody to improvise and to create new dividends, an aspect that Hirschman advised us to look out for. In this sense, we would have lost the opportunity for discovering and demonstrating new types of possibilities, values, and meanings, but also for developing capabilities to act constructively in uncharted situations.

Again, none of this suggests that projects start because of ignorance; it is also not suggested that learning from experience should enable us to count on and plan for such side effects in the future. Logically, if side effects were planned and intended, they would no longer be side effects (Elster, 2016).

In preparation for the discussion below, we will highlight the following points drawn from our reflections on Hirschman's ideas:

1. Ignorance about the future is an existential condition for projects, not a planning fallacy. Projects will necessarily be started on false premises

because, in committing to do them, we presume to know enough about the future to act rationally and purposively. Thus, the alternative to falsely premised projects is not validly premised projects but no projects.

2. Committing to invalidly premised projects will serve us better than committing to no projects. This is true in general, even if we cannot know if it is true in any particular case. We cannot know how and why our commitment to invalidly premised projects did, in fact, benefit us until after the project has been completed. All projects must rely on the ability to improvise when the envisioned next step is unspecified, unfeasible, or unreasonable. All achievements hinge on such an ability. Without improvisation, few projects would reach a stage where it would make sense to evaluate their achievements.
3. While the “start digging” syndrome is cynical and disdainful (Flyvbjerg, 2016a), there may exist a need for a simplified and unreflective form of decision making that enables people to commit to projects on insufficient and narrow premises—and, thus, for some form of Hiding Hand to make us disregard our lack of knowledge about and control over the future. With luck, we may *eventually* come to encounter achievements that are due not to the design of the project but to efforts and happenstance during the implementation of the project. How such achievements count in the appraisal of the project is subject to practical judgment in particular cases. When success is no longer a simple measure of the distance between promise and achievement, project success becomes ambiguous and negotiable.
4. Inherently, projects entail risks, and ignorance of uncertainties and difficulties in the future makes people take more significant risks than they intend. No

experiential learning can take away the fundamental unpredictability of the future, also because circumstances and contingencies are, to a large extent, situation and project specific. Hirschman contended that people may learn to accept higher risks, suggesting that somehow future projects may be started with less ignorance of the innate ignorance. His ideas have been criticized by some for being elusive (Schön, 1994) and (improperly) construed by others as suggesting a reduction of the risk (Flyvbjerg & Sunstein, 2016).

While the field of project management aims to learn how to plan and select projects rationally, Hirschman aimed to learn how to accept the innate risk of projects without shunning the responsibility for their performance and accomplishments. It seems that a rethinking of project management also implies a rethinking of what we can learn from experience.

On this backdrop, let us now present a more conventional view on the reality of projects, that is, Flyvbjerg's view, that seemingly annuls most of Hirschman's ideas and observations.

Slapping the Hiding Hand

In Flyvbjerg's view, the principle of the Hiding Hand is wrong, misleading, and irresponsible. Ignorance is bad, not providential. Hirschman's rehabilitation of it is immoral because it legitimizes irrational and undemocratic decisions primed by project peddlers. In short, it "leads to starting projects that should not have been started."

(Flyvbjerg, 2016a, p. 176) Flyvbjerg considers Hirschman's deliberate search for a silver lining to unsuccessful projects unscholarly and methodologically unsound. In effect, celebrating side effects is an ex post facto rationalization of poor decision making and project planning, Flyvbjerg seems to claim. He shares the perspective of Lovallo and Kahneman (2003) when ascribing the predominance of failures in his study

of large projects to flawed decision making caused by delusional optimism and deliberate deception (Flyvbjerg, 2003).

Flyvbjerg enlists behavioral science as a whole in his war on Hirschman's heresy. He declares that a "Kuhnian paradigm shift" has occurred in the field of project management (Flyvbjerg et al., 2018: 183; Flyvbjerg, 2017a)—a shift that Hirschman (like many others) missed. While some have considered Hirschman an early student of cognitive biases, Flyvbjerg (2016a) diagnoses him as a victim of such biases. Repeatedly, he aligns Hirschman with the most unscrupulous politicians, like Mayor Willie Brown, known for his blunt "start digging" mantra. The Hiding Hand principle is claimed to serve as a legitimization of such opportunism. Like many others (e.g., Love & Ahiaga-Dagbui, 2018), Hirschman overlooked the "root causes" of project underperformance:

The root cause of cost overrun, according to behavioral science, is the well-documented fact that planners and managers keep underestimating scope changes and complexity in project after project ... It is not scope changes, complexity, etc. in themselves that are the main problem; it is how human beings misconceive and underestimate these phenomena, through overconfidence bias, the planning fallacy, etc. (Flyvbjerg et al., 2018, p. 183)

In a sense, Flyvbjerg's root cause (ignorance of cognitive biases) resonates well with the notion of ignorance of ignorance, but he rejects Hirschman's tolerance for such ignorance on empirical and methodological grounds. What follows is our shorthand version of his indictment of Hirschman and his heretical ideas (Flyvbjerg 2016a, p. 185):

1. Actively looking for unplanned successes, Hirschman overlooked or hid the predominance of failures.

2. A few case studies provide no proof that, as a general law, projects will typically compensate for a false start by discovering or inventing recovering action further downstream.
3. Playing with ideas concerning the centrality of side effects and silver linings is a poor substitute for methodical testing of empirical propositions. The aim to provoke (Hirschman, 1995, p. 129) is not a sound academic agenda.
4. Learning from Hirschman would undermine progress by legitimizing his openly admitted effort to give cognitive biases a positive connotation, as in his expression “a bias for hope” (Hirschman, 2001, p. 102). Being the root cause of underperformance, biases of all kinds should be debiased, not legitimized.

The ideas and findings that Flyvbjerg attributes to Hirschman are commonly disowned in the literature: “We now know that the Hiding Hand principle is overly optimistic about the downstream innovative capacity of megaprojects to solve the problems overlooked by upstream planners.” (Davies, 2017, p. 37)

Flyvbjerg’s takeaway from Hirschman’s work turns out to be little more than a reaffirmation of his position, namely that the projects that failed to deliver according to plan should not have been started and since they were started regardless, that the decision to start them must have been the unfortunate result of incorrect and manipulated forecasts of costs and benefits. Hirschman diverts efforts to improve project performance when he rationalizes achievements by pointing to side effects and hard-to-quantify dividends. The remedy to the alarmingly poor performance of projects is better informed decision making.

Facts and Their Implications

In another context, Flyvbjerg acknowledges “the richness and originality of Hirschman’s work and that he was a leading 20th-century intellectual and economist, well worth reading today” (2018, p. 383). If so, how is it possible for Hirschman to misconceive reality so thoroughly that his entire work on project management deserves bold rejection? Why are there no lessons or inspiration to be drawn from the richness and originality of Hirschman’s work on development projects?

It may come as a surprise that Hirschman and Flyvbjerg do not disagree much about the empirical facts. Even Hirschman considers many of the propositions that Flyvbjerg attributes to him as “silly” (Hirschman 2014, p. 10). If we did not know better, we would think that the following quote came from Flyvbjerg:

Exaggeration of prospective benefits is at least as common a device to elicit action as underestimation of costs. This error, specially when combined with an underestimate of costs, has of course often led to disaster—history abounds with examples, from bankruptcies and white elephants to lost or ruinously won wars. (Hirschman 2014, p. 22)

If not the facts, what is the bone of contention? Arguably, it is the implications we should draw from these facts which, as we shall argue below, depend on the notion of a project to which one subscribes. When introducing the notion of root causes, Flyvbjerg acknowledges both the fact of ignorance about the future and the possibility of an initial ignorance of such ignorance as an empirical phenomenon. He only objects to Hirschman’s suggestion that such ignorance of ignorance may be an acceptable premise for project planning. The issue is the neglect of the common experience that unknowable contingencies are impending, and he aims to bring such experience to the attention of decision makers by proposing, for example, an “outside view” (Lovaglio &

Kahneman, 2003) and “reference class forecasting” (Flyvbjerg et al., 2018). In this manner, biased premises for decision making are debiased, promising more successful projects from the outset.

What we should do differently in the future is not so much a matter of facts as a matter of practical (or value) judgment. According to Dewey, these judgments also imply “a judgment of what and how to judge—of the weight to be assigned to various factors in the determination of judgment” (1915, p. 517). Hypothetically, Hirschman and Flyvbjerg might agree that many projects have constructive side effects and still differ in their appreciation of this fact. Flyvbjerg would consider them a planning failure and therefore call for more and better planning; Hirschman would see them as unexpected opportunities and seek ways to take advantage of them. Flyvbjerg would define ignorance as a problem and seek to reduce it by sourcing more and better knowledge; Hirschman would consider ignorance a fundamental human condition and seek ways of escaping the looming fate of foolishness or inaction. Flyvbjerg would be concerned with the risk of starting projects that should not have been started; Hirschman would be concerned with the risk of missed opportunities, that is, projects that should have been started but were not. Flyvbjerg would be committed to banning unrealistic planning and undemocratic decision making, thereby making projects more beneficial prospectively; Hirschman would be committed to searching for examples of the ways agency, improvisation, and serendipity have made projects valuable in retrospect, which, in Flyvbjerg’s view, is an invitation to ex post facto rationalizations of wrong decisions.

Conflicting Notions of a Project

On several occasions, we have referred to underlying notions of a project as the key to understanding the controversy between Hirschman and Flyvbjerg. Such notions cannot be tested empirically against data and evidence because it is the notion that determines how we judge the data and where we search for evidence. Therefore, choosing between such notions is difficult, as we shall see below.

Projects as Leaps into a Designed Future

The implications that Flyvbjerg draws from his data and experience make good sense when a project is considered a consciously designed and planned *leap into a designed and desired future*. It is the pre-existing design that allows us to see if subsequent events fit and, if not, to take corrective action. Thus, there is no denial of the fact that the implementation of projects may be as riddled with contingencies and difficulties, as Hirschman suggested. However, ideally, such contingencies and difficulties should have no impact on the realization of the projected outcome. The specific route a project takes toward its consummation may be erratic and oblique, but the destination is initially given by human choice and purpose. The common distinction between *project success* and *project management success* (Ika, 2009) seems to codify the ambition to make performance in terms of outcomes *independent of*, that is, causally insulated from, the travail and turmoil of performance in terms of process. By implication, the primary task of project management is to ensure that the project process remains *inconsequential* for the project achievements, as originally designed and decided. In turn, project governance will keep an eye on how the project managers handle this task:

the core purpose of project governance is to evaluate and shape the development of the project throughout its life cycle in such a way that its outcomes remain

safe, strategically aligned and beneficial to the stakeholders, as agreed at the time of approval. (Cicmil & Braddon, 2012, p. 222)

Ideally, from approval to completion, nothing of substantive significance for the project's achievements should occur. In spite of accumulated knowledge on how to manage projects to such an effect, experience tells us that the task of making the implementation process inconsequential is not a trivial one. "Over budget, over time, over and over again" is Flyvbjerg's (2011) short summary of the performance record of major projects. A high frequency of project fiasco is true in most sectors of society (Cicmil & Hodgson, 2006). When projects fail to deliver as promised, project management must have failed to make the implementation process inconsequential. While project managers are commonly held accountable for such failures, Flyvbjerg points out that they may also have been given an impossible task. As mentioned above, the root cause of underperformance is biased and manipulated decisions that send projects off on a disastrous course—a death march (Yourdon, 2004)—in the sense that their ill fate is already sealed by the design. Less unrealistic estimates and forecasts would give project managers a fairer chance of surviving and reaching their destination by providing more slack resources in the fight against contingencies and difficulties. The poor track record of projects is no reason to renounce the ideal. The challenge is to bring practice closer to the ideal, *not* vice versa.

By honoring the ideal of making projects right from the outset, project management aligns itself with the central values and tenets of modern society. Decisions should be made rationally and governed by a logic of consequences (March, 1994). When projects are decided rationally, they become the collective embodiment of purposive human action. This constructs an image of projects as the most legitimate organizational form, and its popularity is testified by the proclaimed 'projectification'

of firms and society (Lundin & Söderholm, 1998; Midler, 1995), promising a welcome escape from mindless bureaucracy and unscrupulous politics.

Projects as Pursuit

Hirschman's notion of projects is different, if mainly implicit. It derives from his general approach to the social world, focusing on "the unique rather than the general, the unexpected rather than the expected and the possible rather than the probable" (2013, p. 22). Every project represents ongoing experimentation. There is no leap into a preconceived future since the future is accomplished in numerous small steps, that is, in moments of action. The projected future may set the project in motion and may well guide and inform action, serving as a "supra-act" (Ryle, 2000b). However, it will not reduce the need for agency in the face of contingencies, difficulties, and uncertainties. Presumably, such situations present actors with alternative routes, and the chosen routes will determine which destination the project may and will reach. Without such consequential agency, the project would come to a premature end.

Projects that are conceived as processes of pursuit and discovery must necessarily unfold in "disorderly and circuitous ways" (Offe, 2014, p. 591). Giving side effects and unplanned and hard-to-quantify dividends much emphasis, Hirschman rejected the pre-existing project design as the ultimate criterion for what fits and what counts. What is possible and appropriate to do is determined by the specific situation at hand, not the situation designed and expected. Situated judgment is required, and since such judgment is not preordained, each moment holds some significance for the fate of the project. The original design and promises are not (at least not necessarily) ignored but necessarily made subject to such situated judgment. Considering all the other matters of concern at some specific moment, how much weight these prior goals and plans should be given is part and parcel of the judgment of what to do next.

Hirschman's inspiration for seeing projects more in terms of pursuit than accomplishment should probably be sought far from the fields of economics and management, where conventional project management finds its inspiration. Being a "lifelong admirer of the Russian's spiritualist prose" (Adelman, 2013, p. 24), Hirschman recognized contingencies as not only a fact of life but also as a driver of history. In the words of Morson (2013, p. 68), the future is not simply the "past to be." All contingencies imply a fork in the road that requires small but consequential choices. In retrospect, such a multiplicity of choices will have ruined every preconception of being in control of events, of the future being decidable. Rather, since each moment holds contingency, "no moment has pastness until it is past" (Morson, 2013, p. 68).

If project implementation is such a muddle, Hirschman must find a role for project management that is more sophisticated (and demanding) than policing the integrity of the original design. Learning from Tolstoy, what is needed to manage projects (and conduct war) is to attend *wisely to what is happening*, relying more on the presence of mind than on plans, and learning from a multiplicity of experience without reducing it to simple lessons and prescripts (Morson, 2013, p. 39–40). One of the challenges to management is the fact that every moment or event is enacted by real actors with "heterogeneous and often conflicting needs [that cannot] be reduced so as to 'optimize' demands and tactics" (Offe & Wiesenhal, 1980: 75). Were managers to treat participants as mere "factors of production" – as they appear in the plans and designs – moments of contingency, difficulty and uncertainty would likely be turned against them. Taking inspiration from Offe and Wiesenhal (1980), we suggest that it is necessary to resolve these moments of potential conflict when things cannot happen according to plan by some form of collective deliberation, also implying that the resolution will simultaneously express and define the interests of the participants in the

particular moment—and simultaneously express and redefine the identity and destiny of the project. For project managers, the challenge is to recognize (or construct) the forks in the road that the beaten track lures us into ignoring and to exercise the necessary pragmatic judgment in choosing between alternative routes, hoping to escape the predestined ill fate of projects managed according to conventional project management wisdom.

In conclusion, this notion of a project would seem consistent with a Deweyan projects-as-practice perspective (Buchan & Simpson, 2020), which in Hirschman's case shows in his:

deep and persistent aversion to “teleology and historical laws” that allow for strong predictions. Everything is possible, hopes may fail, and all depends on the right agency, which in turn depends on situational contexts, both favorable and unfavorable. (Offe, 2014, p. 585)

There is a complete reversal of roles for project design and project management. Hirschman directed attention to the critical role of project management in understanding project achievements. Project management is the never-ending task of exercising the right agency, attending wisely to what is happening, and of drawing on accumulated experience in a casuistic manner—in “the root sense of the word” (Morson, 2013, p. 40). Thus, Hirschman was looking for project managers to strive to contribute constructively, but from a weak position, as actual outcomes are beyond the control of everyone, including the project management. Consequently, the eventual outcome should never be misconstrued as an easy indicator of the quality of the effort (Ryle, 2000a).

Knowing and Learning

Projects understood as leaps into a predesigned future and as processes of pursuit and discovery direct our attention to different aspects of reality and to pursuing different ideals of successfulness. Being incommensurable, they represent an impossible choice that we might feel forced to make. However, the spirit of our reflections is to seek ways of making them supplementary, not competing, notions. Where in the academic edifice of Flyvbjerg and project management could we find (or make) room for the insight and wisdom that Hirschman offers? Let us first examine the walls of the edifice a little closer.

The stronghold of Flyvbjerg and project management is the alignment with the idea of purposive human action. Unquestionably, we should aim to do only the things that will benefit us in the future. When rationally choosing to start projects, we should know what it requires to take us safely to the designated future. However, as Loasby (2000, p. 4) has cleverly suggested, the only way we can know such things is “by setting bounds to what we seek to know, and ignoring ... what lies beyond.” What we ignore because it lies beyond what we seek to know is the practices of pursuit and discovery that Hirschman chose to address. Acknowledging such concerns might risk undermining the very foundation of the body of knowledge in project management.

Setting bounds to what we seek to know might further explain why we seem to have exhausted the possibilities for learning from experience. We know how to ensure the success of projects by proper design, planning, and management; otherwise, we would give up the idea of purposive human action. When repeatedly experiencing projects that fail, we infer that the practice must have violated the established knowledge—otherwise, they would have succeeded! Following Loasby’s reasoning, to

know how to design and plan projects, we must necessarily ignore the complexities and difficulties of bringing projects to success—the same complexities and difficulties that we retrospectively scorn practice for having underestimated or mishandled. We protect our knowledge about how to rationally manage projects by framing experience in such a way that the recurrent failures do not change what we know. “Constant failure is a queer name for progress,” says Dewey (1915, p. 522). Thus, we may have entrapped ourselves in an awkward body of knowledge that allows us to learn only the things we know already. Academically, this could be considered the winner’s curse but it could also be appreciated as a safeguard against heresy. Thus, Flyvbjerg can safely add to the body of knowledge by arguing that reference class forecasting is the solution to the root problem of underperformance but only because future underperformances will tell us that a stupid or unscrupulous underestimation must have occurred months or even years before. First, making attributions based on performance and then treating these attributions as the cause of the performance (Rosenzweig, 2014) make us unable to learn new things from experience. Since ignorance (or inattention) is, according to Loasby (quoted above), constitutive of the kind of knowledge that is required for us to imagine human action to be rational and purposive, the Hiding Hand may be gloved in new procedures for making decisions but still empowers people to start projects on the wrong premise that they know enough to control the future.

It is this academic edifice of project management that we want to open to learning, with inspiration from Hirschman. We do not suggest that he has an alternative edifice that should replace the current one. Hirschman’s aversion to knowledge in the form of historical laws and his pitch for casuistry would seem to allow him to collect stories with little operational usefulness, holding no “immediately applicable,

‘practical’ lesson” (Hirschman, 1995, p. 129). Without regrets, he prioritizes staying open to learning new aspects of reality over the building of a body of knowledge.

Conclusions

To reiterate, in project management, we seem to learn the same lessons from experience over and over again: that projects end “over budget, over time, over and over again,” that project design and plans are biased and that the decision to start the project is commonly manipulated by strategically miscalculated costs and benefits (Flyvbjerg, 2011). By way of conclusion, we ask if we might possibly learn something more?

For example, might we learn what makes projects successful. If by axiom, we treat the right forecasts as the way to success, there is nothing much we can learn, except more ways that a project can fail. However, following Hirschman, we might think of success criteria as partly endogenously established, thus functioning as a premise for and as an outcome of the project process. Sometimes, projects achieve things that render the formal criteria irrelevant at the end. Budget overruns may possibly even serve as an indicator of worth in the valuation of such achievements (Kreiner, 2017; Kreiner, in press). Not that they necessarily will, but that they possibly can, and that they occasionally will send such a signal. Rather than presuming we already know what success is and the criteria upon which to measure it, we might possibly pose the question and nuance the understanding of this central aspect of projects by discovering multiple and contingent ways for projects to succeed (Kreiner, 2014).

The notion of side effects suggests that there are concerns and interests besides the project makers’ that will, for one or the other reason, come to count. It has been suggested that “the project’s object—[e.g.] the infrastructure object—will continue to

mobilise new actors and concerns. The concerns around the object ... formulate and reformulate decisions on how to manage [the project]" (Revellino & Mouritsen, 2017, p. 298). In moments of contingency, project managers will have to negotiate a way forward and make judgments about the relative weight of multiple concerns to reach a judgment of what to do next. To understand how such consequential, practical judgments are made, we need to identify with the project managers and understand the dilemmas faced in practice. This would also imply that we need to construe of the project, not in the abstract terms of the project design, but with regard to the concrete, also material, terms of specific moments calling for inquiry and action (Buchan & Simpson, 2020).

Furthermore, it is easy to imagine that the project manager might act in the best interests of the project (and ultimately the client) by compromising the promises that premised the decision to engage in the project, simply because any other judgment might result in even bigger sacrifices, including the potential discontinuation of the project. Thus, the prevalence of fiascoes based on the traditional criteria may indicate that the probability of discovering, along the way, more salient values, concerns, and interests than keeping the original promises is very high. Therefore, an apparent project fiasco might also be investigated as a potential case of successful project management. Ultimately, not any overrun or shortage will come to be seen as a success, but, possibly, if we actively look for reasons to consider them successful, many of them would be valued constructively, as Hirschman suggested.

Such ideas may seem heretic and would seem to violate cultural norms about keeping one's promises. The design of the project has been conceived to imply the translation of the notoriously uncertain predictions and anticipations of achievements into specific, often contractually binding promises (Mouritsen & Kreiner, 2016).

Conventionally, a promise is supposedly only valid if the promisors are trusted to be able to keep their promises (Brandes, 2010). In terms of the project context with a poor track record, such trust seems plucked out of the air. However, we may see these promises as founded on contracts rather than interpersonal trust, making the promisors accountable and culpable if things should go wrong. Thus, in practice, the role of trust may be ambiguous. If we trust that we can contain the consequences of the betrayed promises, we need perhaps not wholly trust the promisor before moving ahead. The promise becomes an issue of negotiation between parties, none of whom are much concerned with the realism of the promise and who are possibly driven merely by opportunistic interests. Furthermore, it seems likely that such negotiations are not between equals. Project makers may often possess a monopsonist's power to extract potentially unreasonable promises from participants and suppliers of knowledge and economic resources to the project (Winch, 2013; Winch & Kreiner, 2011). Thus, the moral issue concerning projects and promises has two sides. First, there is the standard issue of project peddlers making false promises. Second, however, there is the issue of project makers exploiting their superior power to extract promises that the promisor is unable to keep. New issues would emerge if we allow the moral issues concerning promises to include both the making of unrealistic promises and the extraction of unfair ones.

Academically, we might support Flyvbjerg in scorning Hirschman for making a virtue of his bias for hope. Yet, such a bias may exemplify the mechanisms for creating a commitment to action in the face of risk and uncertainty. In Schelling's (2006, p. 1) conception, commitment implies "becoming committed to a course of action," "relinquishing some choice" (e.g., exit in the face of contingencies), and "surrendering some control over one's future behavior." When we accept that the achievement of the

project is not independent of the effort and agency during the project process, nobody would doubt the need for creating a commitment to the project beyond contractual obligations. The source of such commitment is an empirical question, but the design of the project, narrating its purpose and goal, would seem to offer an opportunity for nurturing commitment by placing the project in a grander scheme of things than can realistically be justified. Thus, creating commitment and thereby seeking to make projects successful works against Flyvbjerg's concern for the rationality of starting projects. Combining the two concerns, we might come to appreciate the notion of "action rationality" (Brunsson, 1985), which implies a simplified and less reflective form of decisions that goads people to action, causing us to realize that more elaborated decision procedures will not necessarily produce more valid decision premises in a notoriously uncertain and complex reality.

Opening the notion of a project to incorporate more concerns and interests might potentially also change the facts from which we try to learn. Conventionally, the costs that determine success and failure are the costs to the client only. Including the costs to all involved parties would change the measures of the project performance. We know that the distribution of costs is recurrently negotiated during implementation and often continued in legal litigation after completion. Such observations should add to our understanding of cost performance, causing us to consider it not merely a matter of forecasting but also a game of passing the buck.

Finally, if we refrain from presuming that success is the absence of failure, we might come to learn the multiple ways a project may succeed in practice. The context-specific valuation of projects and their achievements involves multiple judgments and interest-driven negotiations between stakeholders. Rather than defining the value and success of projects, project management might study the processes by which such

valuations are reached. We may still claim to know a great deal about projects, even if we allow ourselves to play with the implications that we can draw from such knowledge.

There is more life and strife in the edifice of project management than we have suggested in this essay, and Hirschman is also far from the only one chiseling cracks in its walls (e.g., Drouin et al., 2013; Geraldi & Söderlund, 2018; Hodgson & Cicmil, 2006; Lundin & Hällgreen, 2014; Lundin & Midler, 1998; Sergi et al., 2020). There are more nuances to both Flyvbjerg's and Hirschman's positions than we have covered. Thus, there will be ample opportunities for rejecting our ideas and killing the underlying thinking. However, next time we get surprised and morally offended by the poor performance of some major project, we should send Hirschman a thought, asking ourselves how we manage to get surprised and provoked when history repeats itself and conforms to our theory about it. And, next time we happen to review projects in action, we might strive to learn something new, for example, by contemplating the possibility that innocence (March, 1999), that is, not attending to the way we know projects normally are, is a wiser strategy than taking all imaginable precautions against what will generally, expectedly, and probably happen. It is well to know what an outside view might reveal, but we lose something essential about projects if we do not pursue them as unique, as experiments, thereby allowing ourselves to explore and to appreciate the unexpected and improbable, to discover new possibilities.

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