Chapter 2
The Importance of Being Atheoretical: Management as Engineering

Camilo Olaya

Abstract Engineering is the "discipline of the particular" par excellence. Engineers develop heuristic knowledge to build action-oriented solutions for specific situations. This type of knowledge is concrete, contingent, goal-oriented, particular, temporal, contextual, uncertain, value-laden, and task-specific, and as such it challenges the traditional ideals of scientific knowledge, which is typically assumed to be abstract, unconditional, disinterested, universal, timeless, utopian, certain, value-neutral, and theory-bound. A large part of social-systems engineering produces knowledge through models, with no a priori theories about human action, e.g., there is no *homo oeconomicus*. For instance, system-dynamics models capture decision rules that define processes driven by actors in concrete situations. Such an epistemology shows a valuable lack of concern for empirically-sourced (induced) knowledge. Non-inductive engineering knowledge is generated neither from "generalizable" data nor from "general laws" for social systems, but rather from the ability to design in operational terms. This knowledge grows through trial-and-error. This chapter demarcates these epistemological aspects to show how and why a model-based science denotes an engineering attitude that improves action and change in specific settings. This stance is a consistent way of facing the contingency of systems that are formed by free, innovative actors and, furthermore, of developing a science of management.

Keywords Model-based management • Philosophy of engineering • Science • Evolutionary epistemology • Social systems

> *[The military helicopter's bay door rolls open to reveal a handful of ordinary-looking people already waiting inside. They all wear bewildered expressions. It seems they've all*

C. Olaya

S. Grösser and R. Zeier (eds.), *Systemic Management for Intelligent Organizations*, DOI 10.1007/978-3-642-29244-6_2, © Springer-Verlag Berlin Heidelberg 2012

gotten the same treatment. Helen steps inside. A young man offers Helen his hand.]
Hi. I'm Yusef.
Helen.
Helen, do you have any idea why this is happening to us?
No.
Well, think. What do we have in common?
What do you do for a living, Yusef?
I'm a nuclear physicist.
I'm an astronomer.
Geologist.
I'm an astrobiologist.
All right. So here we're all scientists.
No, not me. I'm an engineer.
From the Film "The Day the Earth Stood Still"

I am an engineer, too. Seemingly, engineers are not scientists. The truth is that we, as engineers, are not usually interested in building theories. We like to solve problems. Theories are general, whereas problems are specific. Scientists look for answers to general questions and explain phenomena, whereas engineers devise solutions, frequently using models. However, I think this reliance on models is precisely why there is a *science* of engineering. This chapter outlines such a clarification, i.e., engineers are scientists, though of a distinct type. Moreover, this chapter suggests that an engineering epistemology is a natural stance for the development of a science of management.

Why is this clarification needed? There is a common belief that engineers are not scientists, or that they do not produce scientific knowledge; rather, "they apply it." These ideas are held not only in science fiction movies. Several engineers I know, including professors and "scientific engineers," seem to believe this idea as well; engineers themselves do not tend to reflect on the *type* of knowledge that they produce, or how they accomplish such production. Moreover, philosophers and historians of science have traditionally neglected the study of engineering (paradoxically, technology has received far more attention). A proper examination of whether engineering produces knowledge in its own right is habitually absent in academic practice. Engineers seem to be seen as problem-solving, tool-using technicians incapable of producing new knowledge: knowledge-users, not knowledge-makers. However, a certain theory of knowledge in fact informs engineering practice, making it highly likely that the world of science is probably missing the diverse contributions made by an engineering *epistemology*.

My application to do doctoral studies in Economics exemplifies this situation. The documents were initially rejected because of my background. I still have a copy of the painful e-mail:

> Dear Mr. Olaya, we are sorry to inform you that with your academic background you have to pass additional examinations. Your degree is a technical degree that is the reason why you have to pass additional examinations. We offer Doctoral degrees in Economics, Social Sciences and Law. For your future we wish you all the best.

Professor Markus Schwaninger, whom we are honoring in this book, had previously agreed to be my thesis supervisor. He very kindly wrote a letter to the Office of Admissions, clarifying that my master studies in Industrial Engineering—perhaps the most "social" of all engineering disciplines—was sufficient as a social *science* background. His letter was effective, and I was admitted, as the only engineer in a 50-student doctoral cohort full of economists and other social *scientists*. Four years of doctoral studies can be traumatic for such a black sheep. It very soon became clear that all my classmates were concerned about the correct application of research methods for building theories, finding research gaps, developing research questions, collecting data, manipulating data, and analyzing data. Data and theory-building in particular were their main sources of stress. What new theory would they eventually propose in their theses? In the meantime, I was thinking in terms of models: imagining models, building models, running computer models, and analyzing the results of these models. In a doctoral seminar on research methodology, I was the only student who presented computer simulation and modeling as the *method* for dealing with my research question. Everyone else had either a "qualitative approach" or a "quantitative approach;" no one had anything that even resembled a model for a specific situation. I began to think that maybe I was in the wrong place.

Fortunately, one of the few people who seemed to understand what I wanted to do was again my supervisor, Professor Schwaninger, because of his deep appreciation of the value of models for gaining understanding, generating new knowledge, tackling problems, and managing social systems. For the past few years he has led a research program on Model-Based Management (Schwaninger 2009, 2010), which invites those who run organizations to consider that better management is management based on models. Even so, however, there is still relatively little research attention given to the use of models for enhancing managerial effectiveness, and therefore there is also a minority understanding of this matter—except perhaps by a fortunate band of honorary black sheep,

In this chapter, I will highlight the meaning and the significance of Professor Schwaninger's invitation. Model-Based Management makes use of distinctive elements of engineering knowledge, which turns out to represent a wide-ranging spectrum of possibilities for developing management as both an effective practice and a broad science. The argument runs as follows. Professor Schwaninger's definition of a model as "an abstract, conceptual system by which a *concrete* system is represented" (2010, p. 1420, emphasis added) is a good starting point. Here, a model stands for a specific system in a concrete place at a specific time. Likewise, the starting point of any engineering task is also a specific situation, usually a problem to solve at a given time. This knowledge usually grows through the Popperian schema of "trial-and-error," that is, model-aided trials are generated for every new situation, with only the successful ones (solutions or effective *designs*) surviving. By contrast, the science pursued by the management discipline is preoccupied is assumed to be one that deals with situations that call for handling by *theories* that are constructed from individual cases or data, via induction from the particular to the general. Apparently, the application of such theories suggests courses of action for managers. This chapter suggests, on the contrary, that instead of relying on the application of theories, or on the suggestions arising from them, managers can do better by developing and using *models*. By approaching each unique problem by building a model for it, one matches more closely the recognition of that *contingent* and *contextual* complexity which managers routinely face in dealing with social systems. In the long run, then, seen as a trial-and-error accumulation of knowledge, management practice can develop a truly Popperian science in the same way that engineers do.

2.1 The Science of Management

At the turn of the twentieth century, management, as a discipline and a unified practice, was still largely undefined (Crainer 2003). Two engineers changed that scenario. The French mining engineer Henri Fayol formulated a top-down perspective of five functions for management: planning, organizing, coordination, command, and control, along with his famous 14 principles of management,[1] which were, perhaps, the first general, theory-like suggestions for managing social systems. He proposed a distinct and enduring (Fells 2000) managerial philosophy that recognized, perhaps for the first time, the universality of management and its identification as a discipline in its own right (Crainer 2003). Frederick Winslow Taylor was also an engineer, specifically a mechanical engineer. He was also, perhaps, the first *scientific* engineer. In 1911, he published his magnum opus in which, inspired by President Roosevelt's call for national efficiency, he developed what he called "scientific management," which "fundamentally consists of certain broad general principles, a certain philosophy, which can be applied in many ways...a science for each element of a man's work" (Taylor 1911, pp. 20, 27).

Let us jump forward 100 years. Management still has the ambition of creating order (Brunsson 2008), a sort of antidote to chaos (Harding 2003) *based on the production and utilization of scientific knowledge*, as illustrated by the following standard MBA-textbook definition:

> Management is the process of designing and maintaining an environment in which individuals, working together in groups, accomplish efficiently selected aims... Management applies to any kind of organization.... This framework has been used and tested for many years. Although there are different ways of organizing managerial knowledge, most textbook authors today have adopted this or a similar framework even after experimenting at times with alternative ways of structuring knowledge.... *Managers can work better by using the organized knowledge about management. It is this knowledge that constitutes a*

---

[1] Henri Fayol's 14 principles of management are as follows: Division of work, authority, discipline, unity of command, unity of direction, subordination of individual interests to the general interest, remuneration, centralization, scalar chain, order, equity, stability of tenure of personnel, initiative, and *esprit de corps* (Parker and Ritson 2005; Pryor and Taneja 2010).

> *science. . . .the organized knowledge underlying the practice may be referred to as a science*" (Koontz and Weihrich 2010, pp. 2–3, 8, emphases added).

Taylor and Fayol, the founding fathers, would be pleased by this claim of Taylor's. There seems to be a science of management, a universal, organized, structured body of accumulated, theoretical, and teachable knowledge that can be applied to management work. The self-proclaimed status of science rests on various epistemic elements that I abbreviate under the expression "science by observation."[2] I will summarize these elements below.

2.1.1 The Epistemology of Management

In the second half of the twentieth century, the science of management took the same direction taken by most contemporary social sciences, namely, toward an incorrigible empiricism anchored in observation as the *source* of knowledge. In this view, the generation of theories is driven by empirical data and observation.[3] With firmly established observations, an *inductive* mechanism develops general statements. For instance, the influential "grounded theory" method develops theories from systematically obtained data: "Generating a theory from data means that most hypotheses and concepts not only come from the data, but are systematically worked out in relation to the data" (Glaser and Strauss 1967, p. 6). Likewise, in case-study research, the analysis of case-based data is the source from which theoretical propositions are developed, i.e., "the interest here is. . .theory generation from case study evidence" (Eisenhardt 1989, pp. 535–536). This can happen via pattern-matching or by establishing causal links to explain a phenomenon, e.g., "You may begin by taking the data you have collected for a single case and attempting to see whether they converge over a logical sequence of events (chronologically) that appears to explain your case's outcomes" (Yin 1998, p. 252). Induction is also the standard method in ethnography (van Maanen 1983), field research (Snow and Thomas 1994), and quantitative methods: "Theories. . .usually have been developed through *induction*, a process through which observations are made (possibly casually at first), data are collected, general patterns are recognized

---

[2] "Positivism" or "idealism" are more accurate words, though they have been widely misused in management research literature; see Blackmore's (1979) clarification.

[3] Qualitative-based researchers collect data to interpret, understand, construct statements, and build theories: "Qualitative research involves the studied use and collection of a variety of empirical materials—case study; personal experience; introspection; life story; interview; artifacts; cultural texts and productions; observational, historical, interactional, and visual texts—that describe routine and problematic moments and meanings in individuals' lives" (Denzin and Lincoln 2000, p. 3). As for quantitative research, Black (1999) also stresses the following in his well-known text: "*Empirical* indicates that the information, knowledge and understanding are gathered through experience and data collection. . .At the foundation of the process of trying to understand events and their causes are observations" (pp. 3, 4, 6).

and relationships are proposed" (Black 1999, p. 8, emphasis original). In a comprehensive review of leading management journals regarding the research methodologies actually employed by scholars, Scandura and Williams (2000) summarize the process of theory-building: "Theory often involves an inductive process…A generalization that starts from the data points that observations produce" (p. 1250).

A process of induction aims, by definition, to generalize and to predict. It is expected that theories should apply to new instances, i.e., cases in different settings and places, in both the past and future. Attached to an inductive mode of thinking is the aim of generalizing from particular instances to generalities. The term *theory* denotes this aim, as a theory is expected to hold on a more general basis beyond what is observed. In fact, generalization and prediction are typical elements of good theory-building.[4]

These theories are stated as causal explanations, so as to answer *why* questions and therewith acquire the essence of a theory (Sutton and Staw 1995). Carlile et al. (2003) assert: "A theory is a statement of what causes what and why" (p. 5). The goal of these theories is to give general accounts of *observed* regularities, according to what the researchers have observed and confirmed. These accounts usually adopt a law-like formulation (Vallentyne 1988), an aspiration strongly defended in social science by several authors who argue in favor of the same type of laws allegedly used in physics, which is taken as example of 'good' science. For example, Kincaid (1988) claims that the restricted generalizations of social science are examples of "good" and "respectable" science because they are empirically based, causal, restricted generalizations which follow a confirmatory and inductive process.[5]

How can knowledge acquired through observation be trusted? In other words, why do researchers trust the theories based on them? The conjunction between the goals of generalization and prediction on one side and inductive processes on the other leads to *confirmation* as a final requisite of theory building, i.e., *under what conditions is a theoretical hypothesis confirmed by a piece of evidence?* (Edidin 1988).

---

[4] Wacker (1998), in his research guidelines for theory-building, stresses that as long as a theory can provide answers to questions like *Could a specific event occur?*, *Should a specific event occur?*, or *Would a specific event occur?,* then we have a theory: "Good theory-building research's purpose is to build an integrated body of knowledge to be applied to many instances by explaining who, what, when, where, how and why certain phenomena will occur" (p. 371).

[5] Almost any issue of the *Academy of Management Journal* illustrates this bias that fabricates induced-from-data, and general (though restricted), law-like, causal, theoretic propositions. The following are examples: (1) "Executives who either scrutinize the interest of potential partners or target strong direct ties are likely to form new interorganizational ties more efficiently" (Hallen and Eisenhardt 2012, p. 50); (2) "Cognitive team diversity positively relates to individual team member creativity" (Shin et al. 2012, p. 200); and (3) "Market commonality, resource similarity, and their interaction are related in the same direction with both the likelihood of foothold attack and foothold withdrawal" (Upson et al. 2012, p. 104). Usually the research questions are biased toward law-like causality, such as the following: (1) What are the determinants of power? (Finkelstein 1992); (2) What are the factors for successful inter-partner learning? (Hamel 1991); and (3) What are the determinants of absorptive capacity? (van den Bosch et al. 1999).

In this case, collected data constitute the basis for having *positive* knowledge. For example, with regard to qualitative research, the principle of "theoretical saturation" serves as a criterion for when to stop adding new cases: "Theoretical saturation is simply the point at which incremental learning is minimal because the researchers are observing phenomena seen before" (Eisenhardt 1989, p. 545); similarly, replication of multiple cases can produce "corrobatory evidence" (Yin 1998). Within quantitative methods, this premise operates no differently: "Theories are the basis of research studies and can be thought of as formal statements of explanations of events, expressed in such a way as to allow for their investigations, confirmation and verification" (Black 1999, p. 8). Hence, theories are *valid* as long as they are confirmed by different and future observations. The influence of the positivism of the Vienna Circle prevails in this domain by defining the possibility of scientific statements in observational or experimental verification (Ray 2000).

Finally, the search for confirmation is nothing less than the search for *justification* of knowledge. In the present case, intellectual authority inheres in sense experience. Justification philosophy, understood as the search for epistemic authorities, has been the dominant style of Western philosophy—supporting the customary view of knowledge as *justified true belief*—as one that looks for "well-grounded" (positive) knowledge. Justificationism is rooted in the question, *When is it rational to accept a particular theory?* The expected answer is: *It is rational when it has been verified or probabilified to a sufficient degree* (Radnitzky 1987). This position supports most of current Western thinking regarding what science should be. Given a justificationist logic, it is rational to accept only those positions that have been justified according to rational authority, which in this case is sense experience.

2.1.2 Summary

The habitual epistemology of the science of management assumes that knowledge should be justified, and thus establishes that an empirical basis must be the source of knowledge; accordingly, the epistemic authority is sense experience. Hence, this knowledge is approached and generated via observations—a passive stance in which the environment imprints, or instructs, the researcher—and, later on, confirmed with further, repeated observations that allow for generalization (induction) with allegedly valid, predictive, law-like, causal statements called "theories." Table 2.1 lists these essential elements.

Table 2.1 Pursued elements in the epistemology of management science

Purpose	Development of theories
Theories	Law-like causal statements
Explanation	Causality
Source of knowledge	Observation
Knowledge	Justified true belief
Method	Induction, generalization
Validity	Empirical confirmation
Goal	Prediction

2.2 The Science of Engineering

Engineering is the "discipline of the particular" par excellence, that is, practical wisdom coupled to action. However, engineering as distinct from "technology" as such has been dismissed by intellectuals, philosophers, historians of science, and engineers themselves as a worthwhile and authentic epistemic enterprise on its own terms (Goldman 2004; Miller 2009; Mitcham 1998; Van de Poel and Goldberg 2010). Even so, there has been a complementary need in the past century to recognize engineering knowledge as distinctive and intrinsic to engineering, different from traditional concepts of scientific knowledge. Yet the idea that engineering *is* "applied science" implies that what makes an engineer an engineer, and what an engineer delivers, is (applied) scientific knowledge instead of a different type of knowledge, that is, *engineering* knowledge (Davis 2010).

This section demarcates engineering from science. The very opportunity for a contribution to management science by engineering develops from the significant opposition that engineering knowledge represents to the elements of management science shown in Table 2.1. Dichotomies can be dangerous, but they can also be very instructive: The power of opposition delivers argumentation (Macagno and Walton 2010), facilitates cognitive processes (Krishen and Homer 2011), and serves to imagine extremes so as to better anticipate the spectrum of possibilities (Godin 1999), as in Heraclitus's dictum "from the strain of binding opposites comes harmony" (Heraclitus, ca. 500BC, p. 31). Thus, although there is a risk of missing the shades of gray because of the apparent naiveté of simplification, this section takes that risk because it aims at exploring both the defining differences (not commonalities) and the *intrinsic* elements of engineering knowledge.

2.2.1 The Epistemology of Engineering

Perhaps the first step to take in that direction is to dismiss the traditional (Layton 1974) and misleading (Goldman 2004; Hansson 2007; McCarthy 2010; Pitt 2010; Van de Poel 2010) belief that engineering is an "applied science." The characteristic that is generally accepted as essential to engineering is *design* (Pitt 2011; Van de Poel 2010). Thus, it should suffice to say that design means a *creative* rather than merely an *applicative* (or *reproductive*) enterprise (Doridot 2008). Moreover, design goes beyond nature (Auyang 2009), and thus is unmistakably distinct from natural philosophy. Instead of referring to nature, design refers to human artifice: it is the attribute of a human being who adapts means to a preconceived end (Layton 1974). In fact, design-rich, science-independent engineering is easy to appreciate (Pitt 2011): For instance, consider the Mayan pyramids and the Inca road system.

Design cannot stay at home in theory, for it is a contextual and intensely particular process (Goldman 2004). Engineers relate directly to practical problems: their "know-how" is constructed contingently and in specific contexts (McCarthy 2010). Although the practice of engineering can aspire to special types of generalizable knowledge via, e.g., abstraction or idealization (de Vries 2010), engineering already must start with less far-reaching idealizations than natural science, because the practical approach in engineering requires that designs *work* in real life; e.g., the effects of friction or air resistance cannot be dismissed (Hansson 2007). These functional considerations set engineering knowledge apart (Auyang 2009). Such a practical approach delivers practical knowledge; engineers know *what to do* in non-ideal situations that require the identification or development of a corresponding tool or application, and this "know-how" is where the nature of engineering knowledge resides (McCarthy 2010).

Engineering itself is also a culture or distinctive way of doing things (Davis 2009; Godfrey and Parker 2010), and a type of knowledge shared by researchers, design teams, and whole corporations (McCarthy 2010). The philosopher Sven Hansson (2007) establishes six defining characteristics that, in combination, distinguish engineering science from traditional sciences:

- Study objects are constructed by humans (rather than being objects from nature).
- Design is an essential component. Objects are not only studied but also constructed by engineering scientists.
- The categories for classifying objects are usually specified according to functional rather than physical characteristics; e.g., to determine whether an object is a screwdriver requires determining whether it indeed drives screws.
- Engineers operate in value-laden contexts that influence concepts and designs; e.g., "user-friendly," "risk," "better," etc.
- Engineering knowledge is harder to generalize than natural science knowledge because of real-world restrictions and complexity that cannot be disregarded.
- Exact mathematical precision and analytical solutions are not required if a sufficiently close approximation is available.

In addition to these points, Doridot (2008) demarcates the elements of a Normal Engineering Science (in the Kuhnian sense), from which I want to highlight the following: (1) the creation of intentionally determined artifacts by experimental *methods* that in turn become more fundamental than (and not derived from) *theory*, which in turn brings in (2) a *pragmatic* concept of truth.

Indeed, engineers do not favor a priori starting points: first, they consider the issue; then, they determine what to do. This aim-oriented approach represents a third way that holds its own between the so-called objectivist and subjectivist

Table 2.2 Typical science versus engineering-based reasoning, based on Goldman (2004)

Typical science	Engineering
Sufficient reason/necessity	*Insufficient reason/contingency*
Theory	Practice
Know-that	Know-how
Abstract	Concrete
Theory-bound	Task-specific
Justified knowledge	Unjustified knowledge
Unconditional, necessary	Contingent
Understanding, contemplation	Action
Disinterested	Goals, purpose
Truth	Effective, satisfying
Universal	Particular
Commonness, normality	Uniqueness, heterogeneity
Prediction	Anticipation
Timeless	Temporal, historical
Absolute	Relative
Utopian, context-free	Contextual
Value-neutral	Value-laden, purpose, consequences
Certain (known probabilities)	Uncertain (unknown probabilities)

philosophies (Doridot 2008). This third way also runs between the halves of the traditional discovery vs. invention dichotomy, along a middle path that is committed to *designing* the world (Floridi 2011). A problem-oriented way of life, such as the one that engineers follow, means dealing with new situations that are different from previous situations, and with new and different problems for different clients in new and different settings. This *modus vivendi* explains why the methods of engineering are *heuristic*, in the sense that they are *unjustified*, fallible, context-defined, and problem-oriented. Moreover, this heuristic knowledge deals with authentic novelty because, unlike probabilistic risk analysis, engineering practices (e.g., safety factors, multiple safety barriers, etc.) work under conditions of genuine uncertainty with *unknown* probabilities (Hansson 2009). Perhaps more importantly, engineering knowledge requires the exercise of the engineer's judgment. Unlike "pure knowledge," judgment is an epistemic and contingent relation between the judge and what he has in front of him (Davis 2009); this marks another point of departure from scientific theories which, on the contrary, are explicitly value-neutral (Goldman 2004). Hence, engineering knowledge provides change and solutions—or assists in doing so—given the available resources, to poorly understood and uncertain situations in a rich, multi-variant space of technical, ethical, aesthetic, and humanistic criteria (Koen 2010).

A general division can be demarcated under the previous depiction. Goldman (2004) traces the historical opposition between a form of reasoning based on what he calls *sufficient reason* (necessity) and a form of reasoning based on *insufficient reason* (contingency). Such a distinction serves as the starting point for showing an antagonism between typical science and engineering, summarized in Table 2.2.

The ideal of *sufficient reason* finds its best example in mathematics, where it is paradigmatic of the most admired Western values that also depict the typical ideal pursued by science, namely, theory-bound universal knowledge. The customary epistemological elements of management science discussed in the previous section (Table 2.1) are a good example. The engineering way of doing things works under the opposite and undervalued principles that favor contingent solutions. The sufficient-reason paradigm has been favored since Plato, who endorsed a dichotomy between *episteme* and *techne,* to the present day, which signifies the divorce of reason from action, as well as the prevailing priority of theory over practice in our current academic culture, and therefore of, thinking over making and doing, and correspondingly of representations as copies over representations as models (Floridi 2011).

2.2.2 Trial and Error

Gaining engineering knowledge, under the premise of *insufficient reason*, does not mean reinventing the wheel. Engineering knowledge grows; every design *is* knowledge, and such knowledge adapts over time, which may explain its success. Designs evolve over time because the problems they solve change, demanding adaptive changes in designs. Knowledge-making producers of artifacts (as opposed to mere knowledge-users or information-imprinted agents) use a trial-and-error approach along with a long process of accumulation (Floridi 2011; Ziman 2000). The seminal works on engineering knowledge carried out by Walter Vincenti, engineering professor at Stanford University, illustrate this process. He shows how engineering design follows a task-oriented, Darwinian process of variation and selection, that is, “trial and error” (Vincenti 2000). The development of Edison’s lighting system is an example of an unjustified, blind innovation to the well-accepted gas lighting system (Vincenti 1995). Edison grew 6,000 vegetables during his search for a workable filament for the incandescent lamp; literally one thing after another was tried until one of them worked (Vincenti 1979). In fact, direct testing has always been a major engineering approach, possibly because of complexity that does not allow for a mathematical solution (Hansson 2007). Vincenti also gives a full, detailed description of the process of trial and error in the innovation of flush rivets in American airplanes (1984) and retractable airplane landing gear (1994).

All of the engineers involved in Vincenti’s cases created effective designs with direct guidance from neither physical or theoretical “first principles” nor any data from empirical, “validated” knowledge. As Pirtle (2010) shows, these engineers were guided by the use of conceptual models—in such cases, mental conceptions guide the search of how designs should look and work..Vincenti (2000) himself underscores that the variation-selection (trial-and-error) process is what guarantees that engineering knowledge “works” in the real world under real constraints. Such a process helps to understand why engineering knowledge seems to explain the world more accurately than traditional scientific knowledge.

The higher epistemic and efficiency constraints faced by engineers (Pirtle 2010) generate a more secure and trustworthy type of knowledge (Pitt 2011) that does not need epistemic authority. That is to say, an engineering-based epistemology need not concern itself with epistemic justifications. The usual Western established notion of knowledge as "*justified* true belief" means nothing in a pragmatic approach in which knowledge is *unjustified*. In the words of Pitt: "If it solves our problem, then does it matter if we fail to have a philosophical justification for using it? To adopt this attitude is to reject the primary approach to philosophical analysis of science of the major part of the twentieth century, logical positivism, and to embrace pragmatism" (2011, p. 173).

A process of blind variation and natural selection is perhaps the only non-positivist method of growth of knowledge in which the requisite of justification is dismissed all the while engineering knowledge grows: "What works is what counts." The term *blind* denotes the fact that the generation of trials is not conditioned by either observation or previous results. Concern regarding the origin of such trials does not exist, i.e., it is irrelevant; trials do not necessarily have to be a priori supported by anything, including theories or data. Variations can be freely generated with the help of any procedure, sourced merely from reason or guesswork, or guided by previous expectations (either "theoretic" or not) (Stein and Lipton 1989), guided with the help either of computers or simply by imagination and instinct. Hence, this type of knowledge is not sourced exclusively through observations (or any other indirect mechanism of representing the world). Therefore, the engineer is far more active than the "ideal" scientist because s/he is not "imprinted" by observations; the engineer actively runs blind trials. Although it is an inefficient process, it also has the virtue of *effectiveness*: *it solves the problem*. Blind trials often hit the target. Such an inefficient process applies Ackoff's dictum: "It is better to do the right thing wrong than the wrong thing right" (Ackoff 2001, p. 345).

The trial-and-error process in the growth of knowledge is the same process indicated by the epistemology of Karl Popper (1963, 1968, 1972): An evolutionary growth of unjustified knowledge that becomes the natural home for engineering knowledge. Popper's epistemology is a problem-solving oriented schema of knowledge growth that follows the method of trial-and-error, that is, variation and selection. Blind variations are generated, selected, and maintained (or eliminated) through evolutionary cycles, and instances of fit are achieved by selection among an abundant generation of possibilities. Given these processes, an evolution in the direction of better fit to the selective systems becomes inevitable (Campbell 1965). Here, the knowledge process follows the logic of natural selection; the increments of knowledge involve not only the development of species but also other epistemic activities, such as thought and science:

> The growth of our knowledge is the result of a process closely resembling what Darwin called 'natural selection'; that is, *the natural selection of hypotheses*: our knowledge consists, at every moment, of those hypotheses which have shown their (comparative) fitness by surviving so far in their struggle for existence; a competitive struggle which eliminates those hypotheses which are unfit. This interpretation may be applied to animal knowledge, pre-scientific knowledge, and to scientific knowledge. What is peculiar to scientific knowledge is this: that the struggle for existence is made harder by the conscious

> and systematic criticism of our theories… This statement of the situation is meant to describe how knowledge really grows. It is not meant metaphorically…We try to solve our problems, and to obtain, by a process of elimination, something approaching adequacy in our tentative solutions (Popper 1972, p. 261, emphases original)

Perhaps the best summary of this idea is the phrase “knowledge by trial-and-error.” This epistemology has been widely neglected—or, in the best cases, misunderstood—in the dominant philosophical traditions (Bartley 1987), which evidently include the traditions that have shaped management research.

2.2.3 The Challenge of Engineering Knowledge

To summarize, the central epistemic elements that underpin the science of management—*induction*, *validity by confirmation*, and the *justification of knowledge*—become irrelevant in the trial-and-error process of growth in engineering knowledge, which instead grows by selection or elimination, not by confirmation. Engineering knowledge is conjectural. “Conjecture” in this context means that there is no positive or confirmed “valid” knowledge. We try to refute our conjectures and not to confirm them, which was Popper’s answer to Hume; as long as we do not succeed, our knowledge remains unchallenged, although uncertain.

A non-justificationist epistemology defies the grounding of mainstream management science presented in the previous section (Table 2.1), and, in addition, it also defies the most popular conceptions of science. The academic community of management science, being informed by those scientific epistemologies, seeks positive knowledge built on justified, general theories which in turn are based on confirmed observations. This section has shown that engineering knowledge challenges such assumptions: Unjustified, task-specific trials are tested in contingent, truly uncertain, and ethically demanding situations in a process that ends up accumulating successful, evolving, problem-solving designs.

A challenge represents opportunities. On the one hand, knowledge does not have to be positive, verified, or confirmed; knowledge does not have to be based on observations either, and observations do not have to be generalized. The accumulation of scientific knowledge can grow through trial-and-error, as Popper already has argued, and as engineering already shows. On the other hand, the trials posed by engineers are habitually *model*-aided trials. The next section shows Professor Schwaninger’s Model-Based Management to be a form of engineering knowledge, a task-specific activity that aims to produce effective, transforming designs for the particular and complex environment that managers face, namely, social systems.

2.3 The Engineering of Social Systems

Engineers use models to guide understanding, engage with the world, explain events, and design systems. In regard to human systems, which are the same systems with which managers deal, engineering faces more challenging and perhaps more promising scenarios than, for instance, the engineering of mere physical devices. This section uses system dynamics (SD) modeling as an example.

2.3.1 Models of Social Systems

System-dynamics models are a particular type of model that helps generate knowledge from an endogenous perspective (Sterman 2000a). Upon first review, it would seem that SD models promote generalization through idealization by removing elements that are part of the modeled system and thereafter representing the system based on properties or "working principles" that would govern such a system, e.g., thermodynamics in physical systems (Pirtle 2010). Some social sciences follow a similar stance; for instance, consider the branches of economics that assume the working principles of *homo oeconomicus* as a starting point. However, SD models differ from such approaches in not being theory-bound but rather task-specific. David Lane (2001) has already explored this concept, showing that SD models are not assumed to work under invariant universal laws, nor do they seek to deliver theories of human behavior or individual action. A SD model is a theory for a specific situation or, more accurately, it is a small theoretical statement about a particular situation. These models or "micro" theories are essentially structure-based and not content-based explanations, i.e., they are not defined by the properties of objects or entities but rather by the ways in which actors, processes and activities are arranged and organized in a particular setting. A SD model is a functional abstraction, and as such, it is at home in the intrinsically functional tradition of engineering knowledge (Auyang 2009). These models help to build *dynamic hypotheses*, which are mechanisms with explanatory power (Olaya 2004, 2005). These hypotheses are developed for each specific problem or setting: They explain contingent, specific, problematic behaviors in terms of the structure of the corresponding system.[6]

AU3

A SD model is essentially a model of decision rules employed by actors. A large part of the craft of building this type of model is the ability to study specific

---

[6] Nevertheless, we can also establish general classes of models, e.g., "generic structures," which are theories of structures (feedback loops, levels, rate equations, etc.) that are linked with corresponding dynamic behaviors (Lane and Smart 1996) which can fuel processes of conceptualization, model construction, and generation of trials. This fact marks an intersection with typical scientific knowledge that aims to enhance understanding, either within a domain of application or across different domains, by transferring structures across them. In general, models can help to build theories that transcend concrete situations (Schwaninger and Groesser 2008).

decision-making processes and to reliably represent them in decision rules, under different eventual, contingent scenarios, so as to "produce" the different decisions that such rules generate (Sterman 2000a). This method requires studying the concrete system that will be modeled or, more specifically, the decision rules that the actors in the particular system actually use. In 1956, Jay Forrester (2003) highlighted, as a defining characteristic of this type of model, the study of decision criteria—what he referred to as "guiding policies"—that must not be defined as depending on historical and exogenous data but rather on "motivations, hopes, objectives and optimism of the people involved" (p. 341). It is hard to overestimate the power of modeling, because it implies that a social system is not assumed to function in a way that can be described with a priori laws or theories of human behavior. Moreover, modeling also means that these decision rules serve as starting points, instead of "observed regularities" or data. These implications make sense if we assume that the decisions that agents make (the results of applying decision rules), which constitute typical observable data, change over time, usually according to different environmental conditions. Hence, decision-making agents generate observable *irregularities*. Contingent engineering knowledge can address systems assumed to be driven by such an agency—systems in which agents can *act*. I illustrate these ideas in the next section.

2.3.2 An Example: Operational Thinking Versus Induction

There are several scenarios in which engineers generate knowledge through modeling; for instance, consider the recognition of feedback structures, the pervasive inertia of accumulations, the role of nonlinearities, and the impact of information delays on the behavior of systems. This section identifies the notion of *operational thinking* as one of these possibilities; a more detailed version was developed in a previous work (Olaya 2012).

The first section showed that the most common way to "know" the world uses observations (data) to generate knowledge in order to generalize (for induction) and build predictive, general theories about the world. As an example, Mkhabela (2004) develops a typical time-series approach for a dairy farm in South Africa, in which the equation that defines milk production is as follows:

$$\gamma_t = \beta_0 + \beta_i t + \beta_s A + \varepsilon_t$$

Equation 2.1: Calculation of milk production with γ_t: milk production (liters) for time period t. s = 1 (autumn), 2 (winter), 3 (spring), 4 (summer). A = 1 if s = 2, 3 or 4, or A = 0 if s = 1. β_i : the appropriate regression coefficients. ε_t: random error.

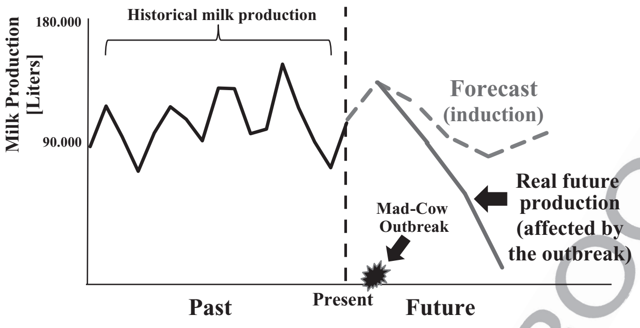

Fig. 2.1 Knowledge based on data misses unexpected events (Olaya 2012)

This time-series equation (Eq. 2.1) uses historical milk data from 33 observations for the period 1990–1998 to forecast milk production. This equation is an abstract generalization from past data regarding milk production. As human beings, we tend to believe that if conditions are similar, then events will repeat themselves. This process of generalization in space and time based on observation (i.e., induction) illustrates the “commonsensical” method of science, i.e., using data as a source of knowledge to generalize and predict.

However, there is a problem with such an approach. Figure 2.1 shows a timeline in which the “past” section displays the historical, observed data for milk production and the vertical dotted line represents the present. Now, let us suppose that there will be a first-ever mad cow outbreak. A time-series based forecast (for example, based on Eq. 2.1) is unable to capture such a contingent event even though its goal is to “forecast.” Unobserved events are excluded from inductive knowledge. Observations are used to “understand” the world by hypothesizing what can, may, or will happen, but the world has to be uniform for induction to work; otherwise, all innovations, including outbreaks, become “black swans.”[7]

Hume (1740) already showed that induction is an untenable position for generating knowledge: “There can be no demonstrative arguments to prove, that those instances, of which we have had no experience, resemble those, of which we have had experience” (p. 62). However, we none the less develop some sciences anchored in these assumptions, so that future events will resemble past events and organizations will resemble each other. These assumptions are needed in order to have science based on observation (Table 2.1). This is a “common sense” philosophy that prevails in what the social sciences, including management science, seek as their epistemological stance.[8]

---

[7] In fact, one forecaster of the dairy industry states: “Forecasting the dairy markets has almost become a fool’s errand, because of the frequency with which ‘black swan events’ turn our outlooks upside down. There is no ‘normal’ anymore” (Levitt 2011, p. 34).

[8] This situation is somewhat ironic, because the most influential scientists of modern times (e.g., Newton, Darwin, and Einstein) were non-justificationists: Newtonian mechanics, the evolutionary theory of Darwin, and the theory of relativity were not induced from particular cases or “data.” As Popper (1974, p. 171) stated, “induction is a myth,” a very popular one in the social sciences.

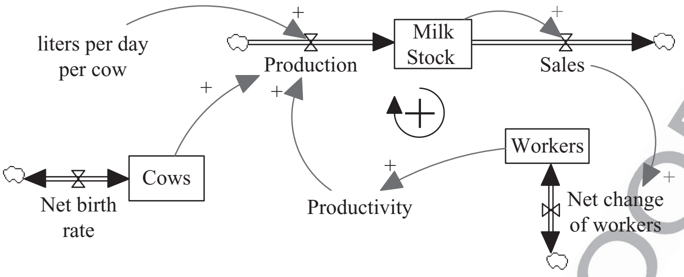

Fig. 2.2 Operational model for milk production on a particular farm

Engineering involves a different manner of engaging with the world. The next example is inspired by an idea from Barry Richmond (1993). Figure 2.2 shows a stock-and-flow model of a dairy farm.

The values of the variables in Fig. 2.2, the equations that define them, and the configuration and arrangement of the system as displayed in that model are specific to this particular farm. For example, *liters per day per cow* refers to the type of cows that this farm uses, and the *Productivity* multiplier is specific for the workers that this farm employs and the way in which these workers (and no others) affect the production of milk on this farm according to their particular skills, available technology, mode of working, historical accumulated knowledge, etc. Let us focus on a possible equation for *Production*:

$$Production_t = liters\,per\,day\,per\,cow \times Cows \times \mathrm{Pr}\,oductivity$$

Equation 2.2: Formulation of production

Equation 2.2 establishes that the daily production of milk equals the number of cows (at that point in time) multiplied by the amount of milk that each cow produces per day; this amount is also affected by the *Productivity* multiplier, which in turn depends on the number of workers available (Fig. 2.2). Equation 2.2 is not a *law* of milk production, nor is it a *theory* of milk production. This equation for *Production* is a decision rule for this particular case; that is, it defines how the actors in this system act and decide, according to the modeler. It does not necessarily work for other farms, not even for the very next neighboring farm, because the impact of the number of workers on productivity for other farms is most likely different (affected, e.g., by lazy workers, better milking techniques, etc.). The engineering professor Barry Richmond called this type of thinking "operational thinking," which refers to thinking in terms of *how things really work*, as opposed to, for instance, how they theoretically work, or how they usually work (Richmond 1993). Such an attitude is a trademark of engineering thinking; in this case, Equation 2.2 captures how operations and decisions (milk production) are actually produced as a function of resources, the use of materials, and information. Contrast this latter equation with the time-series equation (Eq. 2.1),

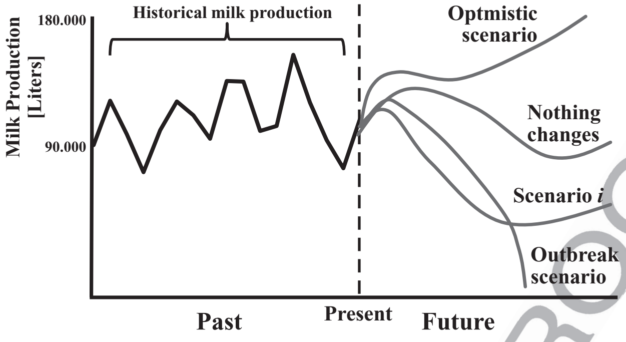

Fig. 2.3 Exploration of scenarios. Operational knowledge shows how the systems works (according to the operation of the system, not according to the data) under diverse, unknown future conditions (Olaya 2012)

which is a non-operational equation for milk production that relies on *data* instead of cows, workers, decision rules, delays, and so on.

Equation 2.2, though very simple, has other virtues; for instance, it allows us to capture hypothetical events that may never have been observed on the real farm. Such fictitious events can be simulated with a computer, and we usually call them "scenarios;" these explorations permit us to conjecture how and why the system "produces" its own behavior and what the system might be able to "produce" under different unknown circumstances (Fig. 2.3). For instance, the scenario of a mortal epidemic in which cows start to disappear, because of, for example, a mad cow outbreak, can be simulated by progressively decreasing the cow's *Net birth rate*. The quantity of cows will go to zero and, in such a case, the structure of the model (including the equation for *Production*) guarantees that production will be zero: no cows, no milk. This outcome is explained in terms of the arrangements, physical structure, relationships, and decision processes of the farm system. This type of knowledge of how and why the system behaves as it does, as a function of its own structure, its own decision rules employed by involved actors, its particular configuration and feedback loops, its specific material and information delays, its nonlinearities, etc., is captured in a *dynamic hypothesis*, i.e., an explanatory mechanism (Olaya 2004, 2005) of the system's behaviors in terms of its own structure. The scenarios in Fig. 2.3 are not "possible forecasts." Engineering knowledge changes the question, so instead of asking "what will happen?," it asks "how does it work?" so as to intervene and transform the system with robust policies that incorporate the way in which the particular system is organized and how its specific actors act.

2.3.3 Management as Engineering

A social system is a system of inter*acting* agents. Let us restrict the concept of a social system to human beings, with a purpose that is either formally established, e.g., a corporation, or perhaps blurry, multi-purpose and subject to many possible disputes, e.g., an urban transportation system. Each social system is complex, messy, and unique, with its own singular accumulated history, i.e., it is an evolved system; above all, it is created and realized by the very same people who form it with every decision that they make. To *manage* a social system is to manage people who are free decision-makers, whether as part of a dairy farm, a firm or a whole country. Their freedom requires us to reject the assumptions of uniformity and predictable futures forecasted from past actions or other social systems.

Figure 2.4 shows an operational model for a particular organization with a specific task: To diminish its high expenditure rate. The equations capture the particular decision-making processes of this specific organization; for instance, *Expenditure* is a function of the number of employees, salaries, inventory costs, production, and other costs. The challenge of a modeler is to reliably capture a function that describes the way in which expenditure is generated. Naturally, the formulation of such a decision rule requires the collection of a special type of "data" from, e.g., interviews with the respective decision-makers. However, the "data" for *Expenditure* that the model generates is not based on past expenditure but rather on the actual operation of expenditure, i.e., "how expenditure works" according to the system to which it belongs. In this case, the values through time of the variable *Expenditure* are not induced from its past values but rather are generated as the result of the operation of the whole model, which simulates, iteratively, every contingent decision. The behavior of *Expenditure* is therefore the outcome of combining decision rules, feedback loops, delays, nonlinearities, and so on. *Expenditure* is "produced by the system." This reasoning applies to all of the other variables; each one is formulated accordingly for this specific organization.

Additionally, a simulator allows for the exploration of scenarios and different, new policies, so as to understand how and why this specific arrangement of variables, values, and equations (that is, decision processes) for this particular organization "works." The implementation of these new policies represents new designs and redesigns for this system, based on the operational understanding of how this system works. However, the human mind is very limited in its capacity for examining the consequences of such redesigns (Norman 1983). Simulations serve as first testers, and survivor designs persist (remain); in this way, knowledge can evolve and accumulate over time. In the long run, computer modeling and simulation enhance and promote conceptual change, because they can be used to create task environments in which experiments can be made to examine the dynamic consequences of our assumptions (through their representation in a model). What are the expected results of the simulation? Did the results turn out as expected? Why did the results turn out the way they did? As experimentation continues, new questions surface and further trials are tested with the simulator; with possible

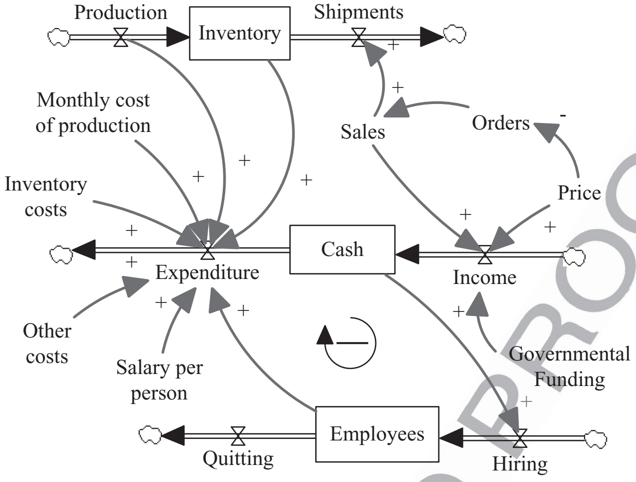

Fig. 2.4 A management model for a particular organization

modifications, fruitless trials are discarded and successful ones are retained. This process promotes knowledge and supports design and redesign in concrete settings and for specific purposes (Olaya 2009; Schaffernicht and Olaya 2012).

Figure 2.4 illustrates how engineering knowledge proceeds under the *insufficient reason principle* (Table 2.2). The model shows an action-oriented epistemology that seeks effectiveness according to specific management goals for this specific organization. Such a model uses neither theories of social action nor a priori assumptions. The knowledge that such an epistemology delivers is not theory-bound but rather task-specific. Nor does it use past data to generate results. Simulated data are generated using the actual functioning, arrangements, and operation of a system. New knowledge in the form of dynamic hypotheses—partial explanations of the behavior of *Expenditure* as a function of the structure of *this* system—can be generated through simulation. Such knowledge is specific, temporal, contextual, and pragmatic. This model-based approach, driven by operational thinking ("how things work," or more precisely, "how this thing—and no other—works"), introduces a contingent, task-oriented form of management. Moreover, a model constructed with this type of thinking, as opposed to a data-based model, allows for addressing "What if. . .?" questions that can be answered according to the decision rules of involved agents. The "if" captures contingency, i.e., "What happens if decision-makers choose this or that. . .?" Simulation helps us explore diverse scenarios, probable or improbable. This contingency recognizes the freedom of decision-makers who create and re-create a social system through decision processes.

A previous study (Lammoglia et al. 2010) proposes an option for implementing these ideas. Managerial efforts can be directed toward unrestricted processes of production of blind variation, that is, the production and iteration of models, which encourages the development of a modeling culture. For instance, Ellerman (2004) proposes the implementation of parallel processes of blind experimentation. Variation and exploration are improved by dividing modeling populations into subgroups with different probes under semi-isolated, selective pressure. The results from these subgroups are cross-communicated and compared in order to enhance the performance of the whole group, and the results of competing models, developed through semi-isolated stages, can be compared with concurrent models. This strategy differs from the traditional managerial principle of allocating resources only to the "best" or "optimal" model. This is a trial-and-error, model-supported process for producing new designs in the form of policies, actions, activities, etc. Hence, rather than expect applications or guidance from general theories, management can directly promote the growth of knowledge by establishing settings that (1) produce undirected and unrestricted model-based trials and (2) enact selective pressure to eliminate unsuccessful trials. Model-aided trials allow for the experimentation and exploration of diverse management scenarios. As long as the design works, then knowledge—in the form of conjectural, successful designs—remains unchallenged, although uncertain.

2.3.4 Summary

If what social systems do is driven by the decisions made by the corresponding actors and the way in which such decisions occur, e.g., the arrangement of actors, delays, the use of resources and information, and specific decision rules, then the design and redesign of these systems lead to the design of new arrangements, new configurations, and the promotion of new decision-making processes. Engineers succeed because the ability to design requires the combination of diverse elements into a working whole with the aim of achieving preconceived ends (Layton 1974). All of these tasks are, naturally, tasks for managers, and as long as managers design these social systems, they are indeed *engineering* those systems. Managers do not have to be forecasters to "manage," which still appears to be an aspiration of the science of management. Instead, managers can understand how and why their specific managed systems work, with the aim of promoting transformations accordingly and thereby generating the growth of knowledge within their own organizations.

2.4 Outlook

The old universalist vision of *general* management remains unchallenged. Fayol searched for universal principles to define the general activities that managers in any organization should perform. However, Taylor, unlike Fayol, had a bottom-up view grounded in the *task* idea; that is, he was a problem-solver: "Taylor's management principles are general principles in the sense that Taylor expected work in all kinds of organizations to be managed by managers. But in contrast to Fayol, Taylor expected the particular activities that managers were to perform to vary depending on the production and situation of the individual organization." (Brunsson 2008, p. 38). Taylor's view is a contingent orientation, more faithful to the "engineering spirit." However, in its quest to appear to be a science, management science has borrowed the positivist philosophy of physics; here I refer to Bartley's version of such a philosophy (1987). As a result, the search for well-grounded, justified knowledge based on confirmed observations (used to induce theories) became the ideal.

I share a concern with Allen (2001) regarding the resurgence of the view that there exists a direct route from observation to understanding in which "the data speak for themselves." The rather recent boom in "evidence-based" thinking exemplifies the elevation of data to the rank of supreme authority in knowledge creation; e.g., evidence-based economics would ask whether claims about economic quantities are justified by data, and whether claims about relations between economic quantities are justified by inference procedures (Reiss 2004). Several researchers in the social sciences ignore Hume's arguments against induction and read Bacon's *Novum Organum* too literally. This chapter proposes instead that management science has an opportunity to expand by questioning commonly held preconceptions about "what science should be." This opportunity almost certainly, in my view, entails a return by management science to its engineering origins. An attitude based on the generation of bold, model-aided *conjectures* (trials) and attempts to refute them (error-elimination), while at the same time discarding positive knowledge, is in direct contradiction to the persistent ideas of positivism. It represents a great challenge because it means that data-sourced theories are not absolutely necessary. Data becomes irrelevant as a source of knowledge in changing settings, and instead, the understanding of decision rules in a specific situation becomes necessary. Blind hypotheses and errors become welcome, too, because confirmation—as a way of advancing knowledge—is replaced by testing and elimination. Traditional preconceptions of "what something *scientific* should be" (Table 2.1) are far from what an evolutionary (adaptive) science, which is able to meet the challenges of a changing world, actually needs. However, engineering knowledge shows that *it works*. A manager, ultimately, is an engineer because s/he seeks to solve problems and to transform a system by redesigning it. Such a mental shift in the management field might require us to reconsider restrictive preconceptions about scientific knowledge and how it can be produced. Popper has already shown that the traditional method of engineers, trial and error, allows for the growth of knowledge. The recognition that a manager faces unique, contingent challenges that require the design and redesign of a social system is the only prerequisite for embracing an engineering stance.

In summary, let us consider one possible standard definition of engineering: *The practice of organizing the design, construction, and operation of any artifice which transforms the physical and social world around us to meet some recognized need* (Pitt 2011). Such a statement can define management as well, whenever such an artifice happens to be a social system. In fact, it matches the definition of management presented in the first section of this chapter. Social systems exemplify the quintessential contingent domain. This chapter has demonstrated that a model-based approach to the management of social systems is truly an engineering venture. Can a social system be engineered? As long as we assume that a social system is a complex arrangement of free decision-makers, whose unpredictable functioning can be modeled with powerful devices to understand how and why every unique system produces its own destiny, then, yes, social systems can be engineered. And perhaps this way of thinking provides the path for developing a science of management that aspires to match the non-uniform complexity of such systems.

Acknowledgements This chapter is an opportunity to express to Professor Schwaninger my gratitude for his inspiring example. I thank the editors for their invitation to contribute to this project and for their suggestions on my submitted draft. Further thanks go to two anonymous reviewers for thorough criticism. Finally, thanks go to Isaac Beltrán, César García, and Andrea García for reading, commenting on, and discussing earlier versions.

References

AU4

Ackoff RL (2001) OR: after the post mortem. Syst Dynam Rev 17(4):341–346
Allen JF (2001) Bioinformatics and discovery: induction beckons again. Bioessays 23(1):104–107
Auyang SY (2009) Knowledge in science and engineering. Synthese 168:319–331
Bartley WW III (1987) Philosophy of biology versus philosophy of physics. In: Radnitzky G, Bartley WW III (eds) Evolutionary epistemology, rationality, and the sociology of knowledge. Open Court, La Salle, pp 7–45
Black TR (1999) Doing quantitative research in the social sciences: an integrated approach to research design, measurement and statistics. Sage, London
Blackmore J (1979) On the inverted use of the terms ‘realism’ and ‘idealism’ among scientists and historians of science. Brit J Philos Sci 30(2):125–134
Brunsson KH (2008) Some effects of Fayolism. Int Stud Manage Organ 38(1):30
Campbell DT (1965) Variation and selective retention in socio-cultural evolution. In: Barringer HR, Blanksten GI, Mack RW (eds) Social change in developing areas: a reinterpretation of evolutionary theory. Schenkman, Cambridge, MA, pp 19–49
Carlile PR, Christensen CM, Sundahl D (2003) The cycles of theory building in management research. Working paper (Sloan School of Management), Boston
Crainer S (2003) One hundred years of management. Bus Strateg Revi 14(2):41–49
Davis M (2009) Defining engineering from Chicago to Shantou. Monist 92(3):325–338
Davis M (2010) Distinguishing architects from engineers: a pilot study in differences between engineers and other technologists. In: Van de Poel I, Goldberg DE (eds) Philosophy and engineering. An emerging agenda. Springer, Dordrecht, pp 15–30
de Vries MJ (2010) Engineering science as a “discipline of the particular”? Types of generalization in engineering sciences. In: Van de Poel I, Goldberg DE (eds) Philosophy and engineering. An emerging agenda. Springer, Dordrecht, pp 83–93
Denzin NK, Lincoln YS (2000) The discipline and practice of qualitative research. In: Denzin NK, Lincoln YS (eds) The handbook of qualitative research, 2nd edn. Sage, Thousand Oaks, pp 1–29
Doridot F (2008) Towards an ‘engineered epistemology’? Interdiscipl Sci Rev 33(3):254–262
Edidin A (1988) From relative confirmation to real confirmation. Philos Sci 55(2):265–271

Eisenhardt KM (1989) Building theories from case study research. Acad Manage Rev 14 (4):532–550

Ellerman DP (2004) Parallel experimentation and the problem of variation. Know Technol Policy 16(4):77–90

Fells MJ (2000) Fayols stands the test of time. J Manage Hist 6(8):345–360

Finkelstein S (1992) Power in top management teams: dimensions, measurement, and validation. Acad Manage J 35(3):505–538

Floridi L (2011) A defence of constructionism: philosophy as conceptual engineering. Metaphilosophy 42(3):282–304

Forrester JW (2003) Dynamic models of economic systems and industrial organizations. Syst Dynam Rev 19:331–345

Glaser B, Strauss A (1967) The discovery of grounded theory. In: Glaser B, Strauss A (eds) The discovery of grounded theory: strategies for qualitative research. Aldine De Gruyter, New York, pp 1–18

Godfrey E, Parker L (2010) Mapping the cultural landscape in engineering education. J Eng Educ 99(1):5–21

Godin B (1999) Argument from consequences and the urge to polarize. Argumentation 13:347–365

Goldman SL (2004) Why we need a philosophy of engineering: a work in progress. Interdisciplinary. Sci Rev 29(2):163–176

Hallen B, Eisenhardt KM (2012) Catalyzing strategies and efficient tie formation: how entrepreneurial firms obtain investment ties. Acad Manage J 55(1):35–70

Hamel G (1991) Competition for competence and interpartner learning within international strategic alliances. Strateg Manage J 12(Special Issue: Global Strategy):83–103

Hansson SO (2007) What is technological science? Stud Hist Philos Sci 38:523–527

Hansson SO (2009) From the casino to the jungle. Dealing with uncertainty in technological risk management. Synthese 168:423–432

Harding N (2003) The social construction of management. Routledge, London

Heraclitus (ca. 500BC) Fragments. (trans: Haxton B, 2001 ed). Penguin Books, New York

Hume D (1740) A treatise of human nature (2000 edn, Oxford philosophical texts). Oxford University Press, Oxford

Kincaid H (1988) Confirmation, complexity and social laws. PSA: Proceedings of the biennial meeting of the Philosophy of Science Association, vol 2. Symposia and invited papers, pp 299–307

Koen BV (2010) Quo vadis, humans? Engineering the survival of the human species. In: Van de Poel I, Goldberg DE (eds) Philosophy and engineering. An emerging agenda. Springer, Dordrecht, pp 313–341

Koontz H, Weihrich H (2010) Essentials of management. An international perspective. McGraw Hill, New Delhi

Krishen AS, Homer PM (2011) Do opposites attract? Understanding opposition in promotion. J Bus Res. http://dx.doi.org/10.1016/j.jbusres.2011.08.002

Lammoglia NL, Olaya C, Villalobos J, Calderón JP, Valdivia JA, Zarama R (2010) Heuristic-based management (I): variation. Kybernetes 39(9–10):1513–1528

Lane D (2001) Rerum cognoscere causas: part I—how do the ideas of system dynamics relate to traditional social theories and the voluntarism/determinism debate? Syst Dynam Rev 17 (2):97–118

Lane D, Smart C (1996) Reinterpreting 'generic structure': evolution, application and limitations of a concept. Syst Dynam Rev 12(2):87–120

Layton ET (1974) Technology as knowledge. Technol Cult 15(1):31–41

Macagno F, Walton D (2010) Dichotomies and oppositions in legal argumentation. Ratio Juris 23 (2):229–257

McCarthy N (2010) A world of things not facts. In: Van de Poel I, Goldberg DE (eds) Philosophy and engineering. An emerging agenda. Springer, Dordrecht, pp 265–273

Miller G (2009) London calling philosophy and engineering: WPE 2008. Sci Eng Ethics 15:443–446

Mitcham C (1998) The importance of philosophy to engineering. Teorema 17(3):27–47

Mkhabela T (2004) Estimates of the increase in milk production due to the introduction of maize silage to a dairy farm in KwaZulu-Natal: a time series approach. Agrekon 43(4):484–491

Norman DA (1983) Some observations on mental models. In: Gentner D, Stevens AL (eds) Mental models. Laurence Erlbaum, Hillsdale, pp 7–14

Olaya C (2004) System dynamics explanations as mechanisms and some implications for theory building. In: Proceedings of the 22nd international conference of the system dynamics society. Keble College, University of Oxford, UK

Olaya C (2005) The significance of addressing system dynamics explanations. In: Proceedings of the 23rd international conference of the system dynamics society. Massachusetts Institute of Technology—System Dynamics Group, Boston

Olaya C (2009) System dynamics: philosophical background and underpinnings. In: Meyers RA (ed) Encyclopedia of complexity and system science. Springer, Heidelberg (pp forthcoming)

Olaya C (2012) Models that include cows: the significance of operational thinking. In: Proceedings of the 30th international conference of the system dynamics society. St. Gallen, Switzerland

Parker LD, Ritson PA (2005) Revisiting Fayol: anticipating contemporary management. Brit J Manage 16(2):175–194

Pirtle Z (2010) How the models of engineering tell the truth. In: Van de Poel I, Goldberg DE (eds) Philosophy and engineering. An emerging agenda. Springer, Dordrecht, pp 95–108

Pitt JC (2010) Philosophy, engineering, and the sciences. In: Van de Poel I, Goldberg DE (eds) Philosophy and engineering. An emerging agenda. Springer, Dordrecht, pp 75–82

Pitt JC (2011) Doing philosophy of technology. Springer, Dordrecht

Popper K (1963) Conjectures and refutations. The growth of scientific knowledge. Routledge and Kegan Paul, London

Popper K (1968) The logic of scientific discovery. Hutchinson, London

Popper K (1972) Objective knowledge. An evolutionary approach. Oxford University Press, Oxford

Popper K (1974) Unended quest. Routledge, London

Pryor MG, Taneja S (2010) Henri Fayol, practitioner and theoretician—revered and reviled. J Manage Hist 16(4):489–503

Radnitzky G (1987) In defense of self-applicable critical rationalism. In: Radnitzky G, Bartley WW (eds) Evolutionary epistemology, rationality, and the sociology of knowledge. Open Court, La Salle, pp 279–312

Ray C (2000) Logical positivism. In: Newton-Smith WH (ed) A companion to the philosophy of science. Blackwell, Malden, pp 243–251

Reiss J (2004) Evidence-based economics: issues and some preliminary answers. Analyse und Kritik 26(2):346–363

Richmond B (1993) Systems thinking: critical thinking skills for the 1990s and beyond. Syst Dynam Rev 9(2):113–133

Scandura TA, Williams EA (2000) Research methodology in management: current practices, trends, and implications for future research. Acad Manage J 43(6):1248–1264

Schaffernicht M, Olaya C (2012) Modeling as autonomous system: Varela meets Darwin. In: Proceedings of the 30th international conference of the system dynamics society. St. Gallen, Switzerland

Schwaninger M (2009) Model-based management (MBM): a research agenda. Institute of management discussion paper, University of St. Gallen

Schwaninger M (2010) Model-based management (MBM): a vital prerequisite for organizational viability. Kybernetes 39(9/10):1419–1428

Schwaninger M, Groesser S (2008) System dynamics as model-based theory building. Syst Res Behav Sci 25(4):447–465

Shin SJ, Kim T-Y, Lee J-Y, Bian L (2012) Cognitive team diversity and individual team member creativity: a cross-level interaction. Acad Manage J 55(1):197–212

Snow CC, Thomas JB (1994) Field research methods in strategic management: contributions to theory building and testing. J Manage Stud 31(4):457–480

Stein E, Lipton P (1989) Where guesses come from: evolutionary epistemology and the anomaly of guided variation. Biol Philos 4(1):33–56

Sterman J (2000) Business dynamics. Systems thinking and modeling for a complex world. McGraw-Hill, Boston

Sutton R, Staw B (1995) What theory is not. Adm Sci Q 40(4):371–384

Taylor FW (1911) The principles of scientific management, Charleston. Republished in 2008 by Forgotten Books

Upson JW, Ketchen DJ, Connelly BL, Ranft AL (2012) Competitor analysis and foothold moves. Acad Manage J 55(1):93–110

Vallentyne P (1988) Explicating lawhood. Philos Sci 55(4):598–613

Van de Poel I (2010) Philosophy and engineering: setting the stage. In: Van de Poel I, Goldberg DE (eds) Philosophy and engineering. An emerging agenda. Springer, Dordrecht, pp 1–11

Van de Poel I, Goldberg DE (2010) Philosophy and engineering. An emerging agenda. Springer, Dordrecht

van den Bosch FAJ, Volberda HW, de Boer M (1999) Coevolution of firm absorptive capacity and knowledge environment: organizational forms and combinative capabilities. Organ Sci 10 (5):551–568

van Maanen J (1983) The fact of fiction in organizational ethnography. In: van Maanen J (ed) Qualitative methodology. Sage, Beverly Hills, pp 37–55

Vincenti WG (1979) The air-propeller tests of W.F. Durand and E.P. Lesley: a case study in technological methodology. Technol Cult 20(4):712–751

Vincenti WG (1984) Technological knowledge without science: the innovation of flush riveting in American Airplanes, ca.1930–ca.1950. Technol Cult 25(3):540–576

Vincenti WG (1994) The retractable airplane landing gear and the Northrop "anomaly": variation-selection and the shaping of technology. Technol Cult 35(1):1–33

Vincenti WG (1995) The technical shaping of technology: real-world constraints and technical logic in Edison's electrical lighting system. Soc Stud Sci 25(3):553–574

Vincenti WG (2000) Real-world variation-selection in the evolution of technological form: historical examples. In: Ziman J (ed) Technological innovation as an evolutionary process. Cambridge University Press, Cambridge, pp 174–189

Wacker J (1998) A definition of theory: research guidelines for different theory-building research methods in operations management. J Oper Manage 16(4):361–385

Yin RK (1998) The abridged version of case study research. In: Bickman L, Rog DJ (eds) Handbook of applied social research. Sage, Thousand Oaks, pp 229–259

Ziman J (2000) Technological innovation as an evolutionary process. Cambridge University Press, Cambridge

Author Queries

Chapter No.: 2

Query Refs.	Details Required	Author's response
AU1	Please provide e-mail ID and complete affiliation details for the author.	See attached file
AU2	Please provide missing quotation mark in the sentence starting "It is this…".	Idem
AU3	Please provide details of Sterman (2000a) and Levitt (2011) in the referernce list.	Idem
AU4	Please cite Sterman (2000) in text.	Idem
AU5	Please provide conference location for Kincaid (1988).	Idem