Systems engineering and modeling: some epistemological remarks

Fabio Roda

LIX, École Polytechnique, 91128 Palaiseau, France.
roda@lix.polytechnique.fr

Abstract. In this paper we provide some epistemological and historical remarks that concern systems engineering and modeling.

1 Introduction: Philosophy of Engineering

In this paper we provide some epistemological and historical remarks that concern systems engineering and modeling. This is consistent with the idea that science and co-science (i.e. philosophy of science) can cooperate fruitfully. However, we think that it is confusing to mix them vaguely and that we have to separate real applications and the consideration about their philosophical implications. Thus, using a terminology that philosophers love, this work belongs to the meta-level.

2 Philosophy of Engineering

The match between philosophy and engineering is quite unusual and merits further clarifications. The basic issue in the philosophy of science can be introduced as follows: scientists study the world, philosophers of science study how they do that (and sometimes they also study scientists themselves).
Borrowing from Lipton [1],

> "*I am a philosopher of science: what do I do? Here is the short version: astronomers study the galaxies; I study the Astronomers.*"

There has been a certain disregard by philosophers of science towards technology, which they consider a straightforward application of pure sciences.

> *"The method and the theories of science can be applied either to increasing our knowledge of the external and the internal reality or to enhancing our welfare and power. If the goal is purely cognitive, pure science is obtained; if primarily practical, applied science. Thus, whereas cytology is a branch of pure science, cancer research is one of applied research."* [2]

This idea, i.e. "technology is applied science", hides the fact that technology and engineering disciplines have some features needing a special epistemological investigation. Recently, a new branch of epistemology called *philosophy of engineering* has attracted increasing interest: it is concerned with the clarification of the epistemological role of technology and engineering within science and human knowledge. To continue the analogy introduced above, engineers study how to design systems, philosophers of engineering study how they do that. The crucial difference between engineers and scientists is that the former *decide* how to manufacture or produce working artifacts and systems, while the latter *analyze* nature and formulate theories to explain how natural systems work. Decision-making naturally brings engineers to think about objectives much more than natural scientists need to do. This profiles a different kind of rationality. On the one hand, we might believe that models are simplified versions of reality that exists independently from our ideas about it, and that the task of science is to describe this reality. On the other hand we might think that models do not describe reality, but they actually create it (indeed most of the modern epistemology tells us that all observation is theory-laden, for example see Hanson [3] on this point and, more philosophically, think of Kant and his *Copernican revolution*). Likewise, we might feel a need to simply explain systems, as opposed to endowing them with aims. If truth is not discovered, but it is invented, the hierarchy between science and technology is inverted.

> *"Despite the more than two millennia that separate Aristotle's thinking from ours, Aristotle's conception [sets] the agenda for almost all subsequent thinking about explanation. [. . .] The rivalry had been between those who thought that all causal explanation must proceed in terms of efficient causation and those who (following closely on Aristotle's footsteps) thought that there is room (and need for) teleological explanation (that is, for explanation that cites final causes). [. . .] Aristotle saw goals and purposes in nature, mechanical philosophers either excised all purpose from nature (Hobbes, Hume) or placed it firmly in the hands of God (Descartes)".* [4]

This debate fails to have a clear outcome within epistemology, but if the target system is artificial rather than natural, then it must have a goal, and the issue becomes clearer. What we might call the "pure problem" of scientists is "is it true?", while that of engineers might either be "does it work?", or, perhaps more appropriately, "does it do what the stakeholders want?" It is clear that there is a relationship between being able to verify a statement and making a choice. However, decision making and systems design have some features that make them a special case from an epistemological point of view.

> *"In engineering the ultimate purpose of modeling is to realize reliable artifacts or technical processes. This contrasts substantially with the natural sciences where, conceptually at least, the aim underlying the modeling activities is to gain knowledge for knowledge's sake."* [5]

Epistemologists, in the first half of the '900, usually made reference to natural sciences as chemistry, biology and, most of all, physics (probably due to its resounding success). In this case, the observer is in front of a system that is given and he/she has to describe and understand it. However, in engineering the system is actually *built* by the observer (or one of his/her fellow humans). Thus, the *demarcation criterion* of natural science may be not perfectly suitable. Systems designers still have to do verifications and observations (as natural scientists) but most of all they have to make choices. They are interested in the truth of statements as much as in the effectiveness of choices. From the point of view of systems design, good models are the ones that help to split properly the domain of possible choices in good and bad ones. Some epistemologists underline the problem-solving aspect of science, for example Laudan.

> *"Science is essentially a problem-solving activity. [. . .] The approach taken here is not meant to imply that science is "nothing but" a problem-solving activity. Science has a wide variety of aims [. . .] My approach, however, contends that a view of science as a problem-solving system holds out more hope of capturing what is most characteristic about science than any alternative framework has."* [6]

Considering science as problem-solving corresponds to a change of perspective since we are more interested in getting local solutions rather than global theories. In particular, Khun suggested Operations Research as a good example of the problem-solving approach to science.

> *"For Kuhn, science is problem-solving rather than truth-seeking activity . . . . And what would be a more striking example of problem-solving than OR! . . . As a problem-solving activity OR is oriented towards practice: it tries to use the methods of science to find optimal solutions to problems concerned with alternative courses of actions. As the solutions are its primary aim, it is clear in which sense OR is not a truth-seeking activity: it is not a knowledge-seeking enterprise."*[7]

Philosophy of engineering focus of these special aspects of applied science. We adopt the same perspective. It has been said that *"Philosophy of science is about as useful to scientists as ornithology is to birds"* [1], namely that it is not very useful in practice, but we try to show that some epistemological issues arise anyhow. In our opinion, they require a consideration. At least, epistemology is useful for an external analysis of the scientific method. A scientific analysis of the scientic method would be self-referential.

Nevertheless, no-one is better placed than an scientist or an engineer to understand and analyze his or her own way of working. With the words of Schlick,

> *"A philosopher, therefore, who knew nothing except philosophy would be a knife without blade and handle. Nowadays a professor of philosophy very often is a man who is not able to make anything clearer, that means he does not really philosophize at all, he just talks about philosophy or writes a book about it. This will be impossible in the future. The result of philosophizing will be that no more book will be written about philosophy, but all books will be written in a philosophical manner."* [8]

2.1 Epistemic vs non-epistemic values

McMullin [9] introduces a distinction between *epistemic* and *non-epistemic* values, that is relevant in epistemology. He proposes that a value is epistemic if it helps to "*promote*

[1] Richard P. Feynman

the truth-like character of science". Otherwise, it is non-epistemic. Dorato [10] confirms that we can use the term *epistemic* for values "*regarded as capable of furthering our knowledge"* and *non-epistemic* to refer essentially to values that are ideological, economical, political, ethical, environmental, esthetic or religious. Non-epistemic values can influence science, indirectly. They influence, for example, the choice of the destination of economic endorsement of research projects. Nevertheless, there is a strong agreement, in the scientific community, on the idea that non-epistemic values have no role in determining scientific truth. Non-epistemic values influence the use of the results of pure science, but are never (or hardly never) integrated in the content of scientific theories.

However, for engineering and technology disciplines the role of non-epistemic values appears to be less clear. Safety, equity and economical sustainability are examples of non-epistemic values (since they do not produce knowledge) that have an important role in the decision making process concerning real systems. Engineers, who have to choose between two or more alternative models, in some cases, have to consider non-epistemic values, and integrate them in their models. It is the case of the systems we have considered in this work.

This leads us to think about the way a model can gain a justification when it does not rely upon pure epistemic values. In fact, a first possibility is the experimental approach. We "try and observe". This is not unusual. A second possibility is the collaborative methodology. Another one deals with ethical values. This is not totally common. Thus, in the next sections, we present the tradition that is behind each one of these approaches. However, preliminarily, we present some remarks about the concept of *model*.

3 Models

The root of the term *model* can be traced back to the Latin term *modus* which in turn would derive from the Indo-European root "med-". Its meaning is measure [11]. Modus has two diminutives *modellus* and *modulus* which we find in different contexts linked to engineering related disciplines. The roman architect Vitruvius uses *modulus* to mean architectural standard, which is a surprisingly modern use of the term. Tertullianus uses *modulus* to indicate basis for a marble sculpture. In the period which spans from the Roman Empire to the Middle Ages, terms derived from *modulus* spread across Europe and we detect the terms *modle*, *mole* and *moule*, which came into English as *mould*. Modern English also introduced directly the term *module* from Latin. During the italian renaissance *modelo* and *modello* are employed by important architects, such as Brunelleschi, who uses it while building the cupola of the dome of Firenze, and Alberti:

> *"Be sure to have a complete Model of the Whole, by which examine every minute Part of your future Structure eight, nine, ten Times over, and again, after different Intermissions of Times"*. [12]

From the Italian *modello* derives the French *modèle* and the English *model* and *modell*. Shakespeare uses model both with reference to buildings, thus in the architectural sense, and in a more general sense as "kind of behavior" and Bacon indicates with *modulus* a mental copy of the real world, which is quite close to the modern use. Nowadays these terms are intensively diffused. For example, during the decade 1990-1999 there have been 17,000 publications including them in the title.

The remarkable point is that along centuries there is an interesting feature which characterizes models: they appear to be tools which help to design artifacts. Models are visions of a target system constructed respecting constraints drawn from its environment, which help the system designer/architect to conceive it. In engineering disciplines, modeling is first of all an activity that is close to design. The designing of systems and services requires both analytical and synthetic processes, because designers invent and create new *artificial* systems to fulfill a need. This is different from describing and understanding a given *natural* system. From this point of view modeling assumes a meaning which is much more practical with reference to other scientific disciplines as natural sciences, formal logic and mathematics. Modeling is a set of activities, tools, heuristics (in the broad sense of the term), capabilities which lead a designer to build system-answer to a problem-question which he/she is confronted to.

3.1 Model validation

> *"The mathematical models that are used in OR are representations of the system under study. These models may be imperfect and idealized, but still the quality of the solutions that they yield crucially depends upon their closeness to reality in the relevant respectes."* [7]

Engineers separate the "judgment" of a system into two distinct phases, *verification* and *validation*. The verification process guarantees that the system has been realized correctly, respecting all the specifications documented during the phase of requirements engineering. The validation process ensures that the system functions as expected. Notice that, from an end-user perspective, a system which performs perfectly a wrong task is not a good outcome. This issue is very important in systems design.

> "*Simply put, the Product Verification Process answers the critical question - Was the end product realized right? The Product Validation Process addresses the equally critical question - Was the right end product realized?*" [13]

This issue concerns also the method of OR, which typically includes two phases: in the first phase a problem is formalized into a model; in the second phase efficient techniques are searched in order to solve the model. Model verification deals with questions about the capacity of providing correct solutions with a limited amount of computational resources and time. We refer to this issue as the problem of *efficiency*. Model validation assesses that the model really addresses the right problem. We refer to this issue as the problem of *effectiveness*. For example, a model for the shortest path problem and a fast and correct algorithm that finds its solutions would not be a good answer for someone who is looking for paths that go through the "top *n*" interesting cities starting from Milan and arriving in Paris. It would be efficient but not effective. In OR several important problems are already accurately identified and classified, therefore the focus is most of all on the capacity of solving them, i.e. efficiency. The problem of efficiency is well defined. Computational complexity theory deals with it and provides a stable framework.

However, engineers and system designers are often puzzled by the problem of writing the right model. In systems design effectiveness is a major issue.

> "- *What is a valid model? - has been one of the least discussed topics in the OR literature. [. . .] Thinking about model construction and model validation is basically to raise the issue of different ways of producing knowledge and deciding about the acceptability of the knowledge thus produced"*.[14]

The problem of effectiveness encompasses several approaches and has blurred boundaries. Validation tests can be based on comparing model predictions to real world results. However this kind of validation is not always possible because repeated tests can be expensive, time-consuming or simply impossible. Thus, alternatively, models can be validated using historical events and inter-subjective arguments. In our opinion, the problem of model validation in OR can not be separated from general issues about the approach to scientific knowledge. We believe that philosophy of science and in particular philosophy of engineering are good frameworks for the problem of effectiveness. A few authors share this opinion with us.

> *"Whether Operational Researchers are aware of it or not does not make any difference: to take an option in the debate on model validation in OR is, explicitly or not, to actualize epistemological choices"*. [15]

4 Experimental approach to model validation

Modern science is empirical. Experimentation has a role in science which can not be underestimated. According to R.P. Feynman:

> *"The principle of science, the definition, almost, is the following: The test of all knowledge is experiment. Experiment is the sole judge of scientific truth"* [16]

Nevertheless, in this section we provide some arguments to remind that the debates that emerged in contemporary epistemology show that the role of experimentation is (sometimes) considered as troublesome. There are a bright and a dark side of the coin. We start from the bright side.

First of all, experiments are used to produce a *confirmation*, as they can give us strong arguments to trust a hypothesis. Secondly they can favor the *discovery* of new theories showing new unknown phenomena which call for an explication. As representatives of these two uses of experiment, we can cite, among others, G. Galileo and F. Bacon. Both of them championed a more empirical attitude in natural philosophy and both of them supported a new vision of knowledge based on observations that had to be performed without prejudice or preconception. However, we consider Galileo to exhibit an example of the use of experimentation to confirm a theory and Bacon as an example of use of experimentation to favour the discovery of new theories.

Observations can endorse a theory. With the telescope, Galileo discovered the four large moons of Jupiter, which, since they do not orbit Earth, provide an argument against the Ptolemaic theory that fixed it at the center of the universe. In this case, facts obtained trough experimental work (repeated observation) confirm a theory (Copernican system).

Observations can foster new, general ideas, as explained by Bacon. In fact, Bacon was a convinced inductivist. His *Novum Organum* (1620) can be considered as the first modern work on inductive logic. In particular, it analyses the methods that can be used to produce theoretical inductive inferences, namely from particular to general, which had been relegated to a minor role during the previous centuries.

> *"The syllogism consists of propositions, propositions consist of words, and words are tokens for notions. Hence if the notions themselves (this is the basis of the matter) are confused and abstracted from things without care, there is nothing sound in what is built on them. The only hope is true induction."*

More recently, the more radical defense of empiricism is reasserted by the logical empiricists of Vienna Circle[2]: who stated, in their *Manifesto*, that true knowledge is totally empirical because the scientific enterprise is characterized

> *"essentially by two features. First it is empiricist and positivist: there is knowledge only from experience [...] Second, the scientific world-conception is marked by the application of a certain method, namely logical analysis."*

One of their most famous thesis is the *verification criterion of meaning*: the meaning of a proposition consists in its method of verification, and a proposition which cannot be verified is meaningless. Thus, the role of experimental verification is even stronger than in the vision of Galileo and Bacon, since it is at the basis of meaning.

We now take a look at the dark side of the experimentation coin. Duhem [17] proposes that it is not possible to test experimentally a single hypothesis because complex theories includes many hypotheses and it is really hard to establish which statements are contradicted by a test (systems engineers would call this a traceability problem). Moreover, an observation that refutes a model can be compatible with many other ones. For example, the observation of Galileo was consistent with both the models proposed by Copernicus and the one proposed by Tycho Brahe. This position is known, nowadays, as Duhem-Thesis[3].

A second difficulty concerns the trustworthiness of what we are used to consider *objective facts*. Starting from the platonic *allegory of the cave* up to now, several philosophers have warned about the possibility that facts could be illusory. Many times in the history of philosophy evidence has been called into question. However, in this case, the target is not knowledge in general, it is the exactly the scientific method which is questioned. In the context of modern science a common reference, from this point of view, is the work of Hanson, as mentioned above. Hanson believes that there is not unconditioned observation of facts and, moreover, there is not a neutral language to

[2] The Vienna Circle was an association of philosophers centered at the University of Vienna in 1922. Among its members there were Moritz Schlick, Rudolf Carnap, Richard von Mises, Otto Neurath, Herbert Feigl.

[3] We remark the often the terms Duhem-Thesis and *Duhem-Quine Thesis* are used as equivalent, but, in reality they refer to quite different thesis.

express them. Observational terms are “full of theory”. Thus the idea that theories are confronted to pure facts is wrong, in his opinion.

> *“There is a sense, then, in which seeing is a ‘theory-laden’ undertaking. Observation of x is shaped by prior knowledge of x. Another influence on observations rests in the language or notation used to express what we know, and without which there would be little we could recognize as knowledge.”* [3]

What we observe is influenced, from the beginning, by our system of reference, our opinions, our background knowledge and, in general, our theory.

A third difficulty is explained by Hempel. He proposed the so-called paradox of confirmation, which he explains through the example of the ravens. We normally admit that the observation of a black raven confirms the hypothesis that “all ravens are black”. On the other hand, a white raven is a clear counterexample. However if we also admit (and in general we do) the *equivalence condition*, then we get strange results. The *equivalence condition* states that if two hypothesis are logically equivalent, then certain evidence that confirms the first one confirms also the second (equivalent) one. A logical equivalent of “all ravens are black” is “all non-black objects are non-ravens”. This last is confirmed by a non-black non-raven, e.g. a white tie. It follows that a white tie also confirms “all ravens are black”. This is logically correct, but it sounds strange.

We know that Popper proposes a fundamental improvement to the verification principle of Vienna Circle. He believes that inductive inferences have no justification, since no matter how many singular facts you have observed, you are never sure that a different singular phenomenon could occur, making your general conclusion wrong. Thus verification is, in practice, not feasible. He introduces a different criterion to defend the possibility of empirical justification of a theory. A theory has to divide the world into two distinct classes of phenomena: the ones that are compatible with it and the ones that contradict it. Thus, we should not look for facts that confirm a theory, but for the ones that could make it false. The longest a theory resists to these assaults, the better. It is trusted, or, using his terminology, *corroborated*. This is a considerable progress with reference to the positions of Vienna Circle. Problems caused by induction are reduced.

Nevertheless, according to his opponents, the falsification method proposed by Popper does not escape to the issues of theories underdetermination. During the sixties, authors like Kuhn and Lakatos promoted the idea that science progresses through many different ways, making our comprehension of its method more encompassing. Their focus was no more on one single theory against facts. Scientific research started to be considered as a complex system that comprehends many heterogeneous elements. The terms *paradigm* proposed by Kuhn and *research program* proposed by Lakatos gained a remarkable success and entered the terminology of philosophy of science, becoming quite common. In particular (following [18, 19]) there are 4 types of basic research programs: *descriptive*, *explanatory*, *design*, *explicative*. Descriptive research programs aim “simply” to describe of a set of phenomena, while explanatory programs try to provide an explanation and a framework to predict similar phenomena. These first two types concern empirical sciences. Design research programs deal with the realization of artifacts that fulfill certain previously chosen needs. This type concerns engineering and related disciplines. Explicative research programs are meant to provide precise, possibly formal explication of interesting, but unclear concepts. This last type regards mathematics and analytic philosophy. Thus, there are at least four different approaches to science, and not all of them are purely based on experimentation. The “lesson” of these philosophers of science is that we should consider the method of science simply as “what scientists do”, without limitations. Feyerabend, most of all, strongly endorses this point of view.

From our point of view, we notice that, actually, system designers and decision makers (sometime) have to make choices that can not be based on experimental evidence. Therefore, in the following sections, we consider different possible approaches.

5 Collaborative approach to model validation

In this section we trace historical and conceptual roots of this kind of method, namely the search of truth (only) through an open discussion.

There are approaches to the scientific knowledge that skip most of the issues about the capacity of science of catching the ultimate truth about reality. For example, *instrumentalism*.

> *“Instrumentalism can be formulated as the thesis that scientific theories, the theories of the so-called “pure” sciences, are nothing but computational rules (or inference rules)”.* [20]

Ontological[4] problems about the effective existence of an immutable “being”, that has to be described by a conclusive explanation, are totally left out. Instrumentalism does not focus on the distinction between truthfulness and falseness of scientific theories. On the contrary it considers, by choice, “only” their practical utility. Important representatives of this approach are, among others, E. Mach, H. Poincarè, P. Duhem, E. Le Roy. For example, Poincarè proposes that we can consider the axioms of the geometry as simple *conventions*. Similarily, Le Roy thinks that science has a pure instrumental value and that scientific laws are only convenient synthesis of sets of facts. The position of Duhem is more variegated, but not very different.

> *“A physical theory is not an explanation. It is a system of mathematical propositions which can be derived from a small number of principles that serve to precisely depict a coherent group of experimental laws in a both simple and complete way”.* [21]

The “second”[5] Wittgenstein (see.[22]) believes that a general formal study of the language is not viable. No theory can provide general rules that are valid in all cases. On the contrary, we can establish only local norms since human language is elaborated in local contexts. He thinks that these norms emerge from behaviors and cultures based on what he calls *language games*, i.e. specific sets of linguistic rules. A perfect language does not exist and in particular there is not a perfect scientific language. Moreover, in his opinion, this reflects the absence of a common underlying structure, namely the

[4] Ontology is the branch of metaphysics that studies the nature of existence or being as such

[5] We remark that the “second” Wittgenstein is almost different from the “first” one, whose positions are represented most of all by the *Tractatus logico-philosophicus*.

absence of a common logic. We should drop the idea that there is one single “Logic” at the basis of human rationality and accept the fact that we act and think according to particular *practices* which are functional to particular aims and can not be generalized.

Instrumentalism, conventionalism and the “second” Wittgenstein open the door to the entrance in the field of philosophy of science of elements that, in the first decades of the 20^{th} century, had been kept out. Social components are introduced as a fundamental part of scientific knowledge. The separation between external and internal components of scientific enterprise starts weakening, so that context and content begin running into one and knowledge is no more *justified true belief*, but, more weakly, *locally accepted belief*. Physics loses its supremacy as model of all scientific disciplines, and the nineteenth-century idea, renewed by the project of unity of science of Vienna Circle, that all branches of science could be reduced to mathematical explanation, is replaced by a more encompassing approach that admits final causes, interpretations, narrative explications. From the point of view of these authors, the study of nature is similar to the study of social institutions, myths, political groups. In other words, these epistemologists think that knowledge is only a social construction, namely that truth does not exist in itself and it is only agreed consensus (often, of experts). This current of thought suggests that what we consider true is composed by simple beliefs that someone, who has the power, prestige or status to do it, has legitimated.

Bloor and Barnes and other researchers of the University Edinburgh funded in the ’60 the *Strong program of sociology of knowledge* (*Strong Program*, for short) endorsing these ideas. This stream of research fits in with the tradition of sociology of science of Merton (cf. [23]) but has stronger objectives. Traditional sociology of science wants to explain the influence of social factors on the process that leads to a discovery, but does not believe that they influence also its content. We could say that it focuses more on scientists than on scientific theories. Basically, the contribution of sociology is considered useful to explain scientific failures. Correct theories do not need sociological explanations. Wrong ones can be object of a sociological analysis. On the contrary the *Strong Program* states that truth is a social product, thus all statements, even correct ones, have a sociological justification. For example, Bloor thinks that the psychologist approach to mathematics proposed by J.S. Mill still had full plausibility. Mill thinks that to understand mathematics is equivalent to understand the psychological processes that are carried out by mathematicians. Frege contrasted this idea, asking for an objective substrate of mathematics. Starting from Frege’s objections, Bloor states that this substrate is provided by the inter-subjective layer of psychological processes, namely the social one. Mathematics, from this point of view, becomes essentially a social practice.

We remark that, among others, Popper was absolutely opposed to this approach and he believed that sociology and psychology cannot be used to ground science.

> *“...to me the idea of turning for enlightenment concerning the aims of science, and its possible progress, to sociology or to psychology ... is surprising and disappointing. In fact, compared with physics, sociology and psychology are riddled with fashions and uncontrolled dogmas ... This is why I regard the idea of turning to sociology or psychology as surprising.”* [24]

However, independently from the question of establishing which one of these opposed approaches to knowledge is correct (which is not our task) we can retain that there is an approach to scientific knowledge that tells us that a decision can be legitimately supported by a deal stipulated by all the people in charge of the choice.

Coming back to the point of view of our work, we can observe that collaborative decision making has its own tradition and, thus, indirectly, a kind of legitimation. We do not believe that this is the best method, neither that this is the only method, as strong program sociologists tell us. Nevertheless, in practice, when no other options are available, or empirical evidence is missing, decisions are taken by means of stakeholders' agreement. We concur that this is not inadmissible. In practice, it happens, quite often. In our experience, this is not unusual in projects management and systems design.

6 Ethical approach to model validation

In this section, we look in literature for relationships between ethics and science (OR and management sciences in particular).

Churchman [25] warns about the possible immorality of OR which, in his opinion, could not respect the Kant's moral law "*make only those decisions which treat humanity as an end, never as a means only*" since, in some occasions, OR treats people only as means, in order to achieve an optimum. Nevertheless, the relationships between ethics and OR are recurrent. Wenstøp [26] offers us a comprehensive overview of the last four decades, indicating the work of Boulding [27] as a divide. Boulding proposes OR as an instrument for ethics due to its capability of optimizing consequences of a decision and maximizing utility, which is the goal of some kinds of moral approaches, for example utilitarianism.

Ackoff observes that OR should take care of the interest of the stakeholders (an idea that is consistent with the approach we have adopted in this work).

> *"Decisions should be made by consensus of all who are directly affected by the decisions, the stakeholders."* [28]

Wallace's edited book, *Ethics in Modeling* [29], covers several arguments related to the role of ethics in design disciplines and endorses an attentive care for stakeholders and ethical issues. Brans [30, 31] indicates Multi Criteria Decision Analysis as the OR tool that can "*take the interests of the stakeholders and nature into account, and calls for a multifaceted concept of ethics, consisting of respect, multi criteria management and happiness"* [26]. Gallo [32] underlines that the research should care about both the consequences of a decision and the respect of fundamental principles. He identifies the two that should ground OR. The *responsability* principle, based on the though of Jonas [33], and the *sharing and cooperation* principle. Brans and Gallo [34] provide another historical account of the relationships between OR and ethics, indicating Churchman as one of the main initiators of this "match". They observe that:

> "*Unlike natural sciences, OR/MS*[6] *[...] has as its object not natural reality but rather a man-made reality, the reality of man-machine complex systems [...] Hardly any area in OR/MS can be considered far enough from the real world to escape from ethical considerations*".

[6] Operations Research / Management Science (OR/MS)

Mingers [35] analyses the relationships between OR and *Discourse ethics* (DE), a moral framework developed by Habermas [36, 37]. According to Mingers, this theory fits well with the science of decision-making. Habermas thinks that we can, through the analysis of communicative structures, identify the conditions for the acceptability of a valid argument and that these conditions are common to a valid moral theory.

> *"How then should we apply DE to OR? [...] DE does not put itself forward as a panacea but it does provide a processual template against which proposals and decisions can be tested for ethical legitimacy, and, if followed, should lead to actions that are better in the long run for both organizations and civil society as a whole."* [35]

Le Menestrel and Van Wassenhove focus on the trade-off between

> "*scientific legitimacy of OR models (ethics outside OR models) and the integration of ethics within models (ethics within OR models)"* [38].

This argument recalls the opposition of epistemic and non-epistemic values introduced previously. They identify three possible attitudes towards the relationships between OR and ethics. The first one corresponds to a sharp separation between them. It ensures objectivity of OR, but, in their opinion, is incomplete. The second one integrates ethics in OR. This approach is more complete, but has the flaw of accepting a certain amount of subjectivity. The third approach is based on a distinction between OR model and OR process. Ethics should be integrated with OR process, and not in the models. The OR process can operate as a connector between OR models and the real world and can include ethical matters without compromising the objectivity of OR models. Thus, they refer to this approach as *ethics beyond the model*.

> *"We present three methodological approaches to combine ethics with Operational Research. The first one is ethics outside OR models [...] The second approach is ethics within OR models [...] The third approach is ethics beyond OR models*"

7 Teleological approach to model validation

In this section we focus on the concepts of goals and objectives, which pervade systems engineering. In particular, we dare a possible (audacious) link. The concepts of goal and requirement, used in systems design, have their conceptual "ancestors" in the Aristotelian *final causes*.

For empiricists, the concept itself of teleological explanation of phenomena, namely the existence of purposes and objectives in nature for the sake of which things are done, is unadmissible. This would confer to nature something like a "free will", which is incompatible with the idea of nature as mechanism. However, Aristotle advanced aims as one of his famous four causes: *material*, *formal*, *efficient* and *final*.

> *"Aristotle was deeply committed to investigating and explaining natural phenomena, which is reflected all through the surviving treatises on natural philosophy [...] What unites the questions explored in these natural treatises,[...]*

is that they are predominantly questions asking for the purpose of things, or, as Aristotle puts it, questions asking for - that for the sake of which -. According to Aristotle's understanding of scientific knowledge, the answers to these specific why questions constitute teleological explanations [. . .]" [39]

Final causes (or *telos*) differ from other ones from many points of view. The most evident difference is that "normally" causes happen before effects while in teleological explanations are the effects which occur first. In a causal explanation a first event E_1 happens at time t_1 and a second one E_2 at time t_2. This is not a sufficient condition to state that E_1 *causes* E_2, but it is a necessary one. In teleological explanation this temporal sequence is inverted. The E_1 happens at time t_1 to serve the second one E_2 at time t_2, which is the cause.

"Whereas in a typical causal explanation the earlier-in-time cause explains the later-in-time effect, in teleological explanations, as traditionally understood, the later-in-time effect (that is, the aim or purpose for which something happened) explains the earlier-in-time cause (that is, why something happened). The typical locution of a teleological explanation is: this happened in order that that should occur." [40]

Bacon recommended a limited use of final causes:

"Bacon. . . quotes with approval the Aristotelien maxim - Vere scire est per causes scire - and the Aristotelien distinction of four causes, Materia, Forma, Efficiens, et Finis [but proposes . . .] his famous condemnation of final causes [. . .] He blames their use in Physics; he approves their use in Metaphysics". [41]

Nevertheless, this kind of causes was admitted by authors such as Leibniz and Kant (among others).

" Leibniz did admit teleological explanations alongside mechanical ones. Apart from the need of teleological explanations (in terms of God's purposes) in metaphysics, he argued that physical phenomena can be explained by mechanical as well as teleological principles. . . . Indeed, Leibniz wholeheartedly accepted the Aristotelian final causes alongside efficient causes". [24]

The question is if science should admit or refuse final causes. We propose a compromise solution. In our opinion, the answer is that, anyway, they are actually used in everyday activity by engineers, during systems design, but are hidden by the use of a different terminology. Of course we do not claim the "airplanes want to fly" or "ships want to swim". It would be an evident nonsense. However, stakeholders and systems have objectives, thus we simply suggest that the term "final causes" can have a (smooth) interpretation that is not incompatible with our *standard view* of science: the term "goal" is a (safe) synonym of the term "final cause". From this point of view, we might say (quite provocatively), that requirements engineering and operations research are applied philosophy.

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