IDENTIFICATION OF MATRICES IN SCIENCE AND ENGINEERING

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SUMMARY. Engineering science is a scientific discipline that from the point of view of epistemology and the philosophy of science has been somewhat neglected. When engineering science was under philosophical scrutiny it often just involved the question of whether engineering is a spin-off of pure and applied science and their methods.

We, however, hold that engineering is a science governed by its own epistemology, methodology and ontology. This point is systematically argued by comparing the different sciences with respect to a particular set of characterization criteria.

Key words: engineering vs. basic sciences, epistemology of engineering, technical matrices in science and engineering, the methodology of engineering, the ontological/methodological/epistemological differences between engineering and basic sciences

1. INTRODUCTION

There is a need for an epistemology of sciences which, such as engineering science, are occupied with producing knowledge aiming at the solution of technical problems having a practical impact. When engineering science has been the object of epistemological and philosophical investigations, most often two aspects of the relation to natural science have been dealt with. The one is, whether engineering science solely is applied science, or whether it is a unique field of its own, scientific of not. The other, to the former related issue, concerns the dependence of engineering science upon the natural sciences. I.e., are engineering sciences simply utilizing the results and methods of natural sciences, or are engineering sciences to a large extent governed by a production of their own knowledge, results and methods independently of the natural sciences?

To the former issue, consider initially the difference between pure science and engineering science in terms of the modal moods adopted by practitioners of the two fields. According to Simon [Simon 69], pure sci-

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ence is concerned with 'analysis' while engineering is concerned with 'synthesis'. The modal mood of a pure scientist is largely descriptive, while the mood of engineering is generally prescriptive. The adoption of either one of the modalities is of course closely related to the goal of scientific inquiry. According to Bunge [Bunge 66], insofar the goal is purely cognitive, pure science is obtained, while applied science is the result of the goal being largely practical. Gutting [Gutting 84] correctly points out that either way, the goal is cognitive and additionally:

> This view has two main elements: (1) Technology is (like pure science) a cognitive enterprise, producing its own distinct body of knowledge about the world. (2) Technology is also (unlike pure science) a *practical* enterprise, concerned with the most immediately pressing needs of the society in which it exists. In sum, technology is neither just applied science nor just a set of techniques (of no cognitive significance) for implementing goals.[1]

Perhaps Gutting's second statement is a bit simple. True, engineering does concern the needs of the society, but that is probably only one aspect among a host of others. Apart from Bunge and Cutting, Skolimowski [Skolimowski 66], deSolla Price [Price 65], and later Layton [Layton 88] and Vincenti [Vincenti 82], have provided contributions to the dispute as to whether engineering is a spin-off of pure and applied science in terms of its epistemological status, application of methods and methodological prescriptions. However, the consensus nowadays seems to be that engineering science, while sharing some grounds with pure and applied science, is an enterprise of its own creating its own body of knowledge and featuring its own methods and methodology.

A similar but somewhat differentiated view is embraced here. So far, the arguments provided to show the discrepancy between pure and engineering science, may be characterized by either one of the following two virtues: general epistemological and philosophical arguments justifying the differences, or arguments based on detailed studies of particular fields of pure/applied vs. engineering science. These contributions, while very valuable, are not too systematic.

This paper provides a systematic survey of the following *research profiles*: (1) pure science, (2) applied science, (3) engineering science. Each research profile admits a certain set of research objects, epistemic and ontological assumptions, methods, values etc. We have adequately profited from introducing concepts from the Kuhnian realm of thought, in particular the notion of a *paradigm*, though as will become apparent, not without some non-trivial modifications to Kuhn's original ideas. If the different research profiles may be more or less adequately described with respect to certain characterization criteria, then the similarities and differences will drop out of this very classification.

Consider the following table illustrating the research profiles:

Pure Science	Applied Science	Engineering Science
Hydrodynamics	Hydraulics	Theory of urban waste water systems

None of the research profiles are deducible from any of the other (for instance, hydraulics was practised long before hydrodynamics and to some extent gave rise to hydrodynamics), even though they may of course benefit from each other.

Hydrodynamics or fluid mechanics may be considered to be the pure science dealing with the action of fluids at rest or in motion, and attempts to provide formal mathematical descriptions and theories of fluids' behavior. Then hydraulics is the application of fluid mechanics to devices involving liquids, usually water or oil. Briefly, hydraulics deals with such general problems as the flow of fluids through pipes or in open channels.

Given, among other things, some theory of hydrodynamics and hydraulics an engineer may undertake the task of first theorizing over some specific structure or object to be constructed and subsequently actually design everything from storage dams, pumps, water turbines, flowmeters, jets, nozzles to valves, urban waste water systems etc. Clearly however, the use of theory is highly eclectical in engineering science. Below, an example from the design of a computer program for computing optimal solutions for drainage rests on a number of different theoretical insights ranging from fundamental equations in hydraulics over meteorology to statistics and the theory of computation, etc.

To the engineer, constructing the actual waste water system, it is only of derived interest, if at any interest at all, what sort of, say, epistemical and ontological assumptions the pure science has to make in order to make his formal models of fluids' behavior work. As briefly indicated above, the engineer's approach is not generally descriptive, her task is not to explore fundamentals, but to construct structures and objects of practical importance.

These reflections already provide a hint: There seems to be a difference in the epistemical and ontological assumptions working from pure science towards engineering partially based on a difference in cognitive values governing the respective enterprises. Hence two characterization criteria have been sketched: 'Epistemical and Ontological Assumptions' and 'Values'. These characterization criteria are not sufficient for demarcation, but necessary. Other necessary and hopefully jointly sufficient characterization criteria for demarcation will be provided below.

In general it turns out that the more one moves towards the right hand side of the above table, the more fuzzy the demarcation tends to be. Again, engineering is a heterogeneous enterprise comprising knowledge from a variety of fields which implies a certain looseness in the general characterization. For example, while engineering in the above table includes both the theory and the design of urban waste water systems, one may even propose to add two additional *design* profiles of say, theory-based engineering work and empirical engineering work the latter based on purely empirical methods and investigations and which only includes the actual construction of urban waste water systems.

Theory-based Engineering Work	Empirical Engineering Work
Design of urban waste water systems	

Such design profiles would draw on knowledge from hydrodynamics, but also from fields not related to hydrodynamics at all; for instance concrete technology in the physical construction of the actual plant, economics etc. The empirical engineering profile perhaps constitutes the greatest part of actual engineering activity, but an accurate description of such a design profile is hard to provide in the current framework and will not be dealt with here.

2. FROM PARADIGM TO TECHNICAL MATRIX

Initially consider the following case taken from the engineering science. The example stems from concrete technology and involves the choice of design parameters while building rigid frame warehouses for the U.S. Air Force in the 1950's. The choice of design parameters resulted in the subsequent collapse of portions of the warehouses. The problems encountered originate from the fact that the fundamental behavior of concrete members in transferring shear load, which is a very common design feature, is not fully understood. This is again illustrated by the fact that the problems involved in determining the shear capacity of reinforced concrete members are not readily soluble by neither theory nor experimental testing. Due to these underdetermined aspects different schools or codes have evolved; the U.S. code dictates that the concrete will participate with the web steel in carrying the shear; most European codes on the other hand conclude that the full shear strictly be carried by the web once the stress exceeds certain values, while finally the German school additionally requires, that whereever the specified shear capacity is exceeded, the web steel must be carried to the center of the span. Despite these differences, the schools nevertheless share certain hypotheses among other things that an increase in shear capacity of a reinforced member equals an increase in the amount of web reinforcement. Extensive studies and past track records however additionally suggest that principal stresses caused by a combination of shear and flexural stress may play a significant role in the load capacity of the members. It has furthermore now been demonstratively concluded that the individual members should be designed to handle the axial forces occurring in constructions. None of these additional research parameters and currently confirmed hypotheses were included in the design and construction of the actual bays which collapsed. In particular, the field inspectors concluded that the collapse was due to general weakness in the buildings caused by cracking which again was the result of three questionable assumptions made by the engineers behind the construction: (1) the columns were hinged at the base, (2) the expansion joint would not develop more than 25% of the vertical load as a traction force, and finally (3) the hypothesis concerning the elastic behavior of the frames can be expected to predict the precise distribution of internal forces in long haunched concrete members.

One may view the failure as an instance of global underdetermination. The empirical evidence is insufficient to settle the truth value of the construction hypothesis or theory. And, since, the theory is underdetermined, the choice of design parameters may also be so. Neither school took into account the two additional discoveries that the principal stresses caused by a combination of shear and flexural stress may play a significant role in the load capacity of the members nor that the individual members should be designed to handle the axial forces occurring in constructions.

The classical and most successful philosophical answer to the skeptic insisting on the insurmountability of underdetermination is to delimit the set of circumstances (or possible worlds) over which the scientist has to succeed in order to attain knowledge or obtain results. Less abstractly, each school dictates a set of circumstances under which their methods are to succeed. More specifically, the three assumptions (1)–(3) above account for some of these background assumptions over which the engineers are to succeed. Somewhat crudely, (1) *if* the columns were fully hinged at their lower end, (2) *if* expansion joint will not develop more than 25% of the vertical load as a traction force, and finally (3) *if* the hypothesis concerning the elastic behavior of the frames can be expected to predict the precise distribution of internal forces in long haunched concrete members, then the designers will succeed in building the bays. Even though the engineers succeeded in building the warehouses, some of the epistemic assumptions were wrong: With respect to (1) considerations of partial restraint of the column ends would have accentuated the calculated axial tensile forces in the frame. As for (2), tests later indicated that the bearings for the expansion joint can develop high values of friction. To the end (3), the elastic behavior of the frames cannot be expected to predict the precise distribution of internal forces in long haunched concrete members. Approximate analyses performed subsequently indicate that redistribution of momentum initiated by volumetric effects can give higher flexural stresses in the vicinity of the failure than originally assumed. But updating the background knowledge in this way was impossible at the time due to general global underdetermination of the theory which again was responsible for the three different schools, none of which could have dealt with the problem, since:

> It would appear that design according to other existing codes, American or European, would not require web reinforcement that would have prevented the warehouse failure.[2]

Therefore justifing the introduction of certain components from the Kuhnian realm is twofold: The Kuhnian paradigm (1) aids in restricting the background knowledge, and: (2) induces some of the criteria enabling the distinct characterizations of the various research profiles.

While presenting the notion of paradigms, Kuhn maintained that a scientific discipline is a complex system consisting of symbolic expressions, ontological assumptions, epistemic assumptions, values, concrete exemplars held together by a relatively strong set of group obligations. These symbolic expressions, ontological assumptions etc. serve, among many other purposes, the purpose of separating one paradigm from another even though there, according to Kuhn, is much more to a paradigm than what explicitly may be stated and described. Hence the possible detection of differences in the characterization criteria is a necessary but not sufficient condition for isolation and identification of paradigms. However, all the characterization criteria are assumed to be jointly sufficient for the identification of distinct paradigms even though they by no means are exhaustive.

Our notion of a *technical matrix* is motivated by the Kuhnian thought of a paradigm. Actually Kuhn used the locution ‘disciplinary matrix’ and our technical matrix is a modification of Kuhn’s nomenclature since Kuhn originally never intended to stretch the locution to be applicable to other sciences than the pure.

We hold that when a scientist or scientific community is working within a particular jointly accepted:

1. *procedures and methods for delimiting a set of (research) objects,*
2. *epistemic and ontological (metaphysical) assumptions,*
3. *theoretical structure,*
4. *experimental structure (and experimental techniques),*
5. *methods,*
6. *values,*
7. *exemplars and research competence.*

the scientist is said to be working within a specific technical matrix. Jointly, these elements constitute the necessary and sufficient conditions for the characterization of a research profile.

The technical matrix sets the cognitive standards and authority for legitimate work within the science it governs so the practitioners working within the matrix practise what Kuhn calls *normal science*. In the period of normal science the scientists will articulate the paradigm in their ongoing attempt to account and accomodate the behavior of some relevant aspects of the real world as revealed through the results of experimentation. Even though the scientists are uncritical and exhibit consensus with respect to the fundamentals that constitute the matrix, a matrix will always be sufficiently imprecise and unarticulated leaving a great deal of puzzle-solving to be done – both theoretically and experimentally. The existence of a paradigm which can support normal scientific activity is one of the maintained characteristics yielding a demarcation criteria for sciences vs. non-sciences.

Taking the Newtonian matrix as an example, theoretical puzzles incolved devising mathematical techniques for dealing with the motion of a planet subject to more than one attractive force, and developing assumptions suitable for applying Newtons laws to fluid mechanics. Conversely, experimental puzzles involved the improvement of the accuracy of telescopic observations and the development of experimental techniques capable of yielding reliable measurements of the gravitational constants.

Puzzle-solving in engineering and applied science resembles puzzle-solving in pure science, though with some qualification. Consider the example from the introduction. In hydrodynamics puzzles related to basic ontological issues as for instance: (i) investigating the possibility of deriving the Navier-Stokes equation from basic statistical mechanics; (ii) considering whether there exist solutions to the Navier-Stokes equation under very general boundary conditions. On the other hand, puzzles in hydraulics related to the numerical difficulties arising when attempting to solve the highly complex Navier-Stokes equation for fluids in definite geometrical structures. Finally in engineering, take as an example the development of the computer program MOUSE for computing the optimal solutions for drainage.[3] The program is based on fundamental equations in hydraulics, but also on other fundamental equations from other disciplines. Making the equations computationally feasible constituted a puzzle. Designing the actual program involved other puzzles. Many parameters and correlations (for example between drainage and precipitation in different types of soil) are sought by different methods, theoretical and experimental. When the program was developed and tested to give the first practical results, a considerable further development consisted in refinements, optimizations, applications and sub-applications to a wide range of different situations and conditions. All done in the attempt to provide increased usuability, optimization, accuracy and applicability.

Puzzle-solving in engineering science is of two types. Like in pure science in establishing understanding of specific problems, providing parameters etc. Here the aim is practical usefulness but often not related directly to methods for solving specific practical problems. Secondly, it involves first the development of methods for solving some problem and second includes assessment of the particular developed methods' (numerical) usability, optimization, stability etc.

Kuhn's early account of scientific development included anomalies, crisis and scientific revolutions. However Kuhn later modified his account. In particular, Kuhn modified the thesis of radical incommensurability between paradigms in later writings. Even for pure theoretical science, as basic physics, it is questionable whether the development always takes the form of revolutions. What remains however is the typically *mono-paradigmatic* nature of the pure sciences. This is where a paradigm and Kuhn's understanding of scientific development differs from the one adopted here pertaining to engineering science. Engineering science seems to be somewhat more lenient in its developmental behavior and paradigmatic nature. First, engineering's development is often of a more evolutionary nature and second *poly-paradigmatic*. As in the example of concrete technology it is not too uncommon for different schools, and in other cases, even matrixes, to co-exist in engineering sciences. Actually in the concrete example above Anderson finally proposes to essentially merge the three different schools. Hence the use of matrixes rather than paradigms. Apart from the poly-paradigmatic nature, the dynamics and development of an engineering matrix is substantially externalistic and may stem from either: (1) new theoretical discoveries either adopted from pure science or the engineering science itself; (2) practical challenges while constructing new artifacts (like bridges), and (3) possibilities linked to new tools (like powerful computational abilities).

Matrixes may present anomalies just as paradigms, say in terms of 'functional' failures of some technological system otherwise developed in accordance with the prescriptions set forth by the normal science. Or, 'presumptive' anomalies derived from the underlying science which anticipates the failure of the conventional system in some future circumstance or proposes a radically new system. However, such anomalies may not, even though recalcitrant enough to cause the collapse of, say, rigid frame warehouses, result in anything revolutionary. They may lead to extensive revisions of the methods but often just to minor revisions of the safety factors: of the limits within which the use of the methods is considered safe. Also, different schools, within the very same overall matrix, may converge on a new path, drawing inspiration from the different established bodies of engineering knowledge.

In sum, the conjecture to be spelled out is that engineering may be characterized by the Kuhnian model insofar as it exhibits (1) the matrix as a cognitive authority and a framework for future technological work, (2) invokes the sort of scientific community concensus required for the paradigmatic thought, (3) but engineering science doesn't very often carry the strong incommensurability of different matrixes.

3. INGREDIENTS OF A TECHNICAL MATRIX

Consider the following diagram of scientific practice (Figure 1).

3.1. *Delimitation of Objects*

Scientists in general do not study natural phenomena in their full complexity, but delimit the object to be scrutinized from their original environment. The scientist must defer to a piecemeal identification of the research object by selecting a proper set of parameters which both serves the purpose of circumscribing the object and dictates the 'relevant' aspects under which the object is to be investigated. An interesting consequence of this process is the fact that by delimitation, the scientist may have to idealize and transform the original object of interest to such a degree of artificiality that it no longer has any real counterpart. Afterwards validating the results obtained from the research and model object become a non-trivial task since, given the state of the artefact, it is hard to guarantee that the world *always* will behave in ways consistent with the scientist's background knowledge.

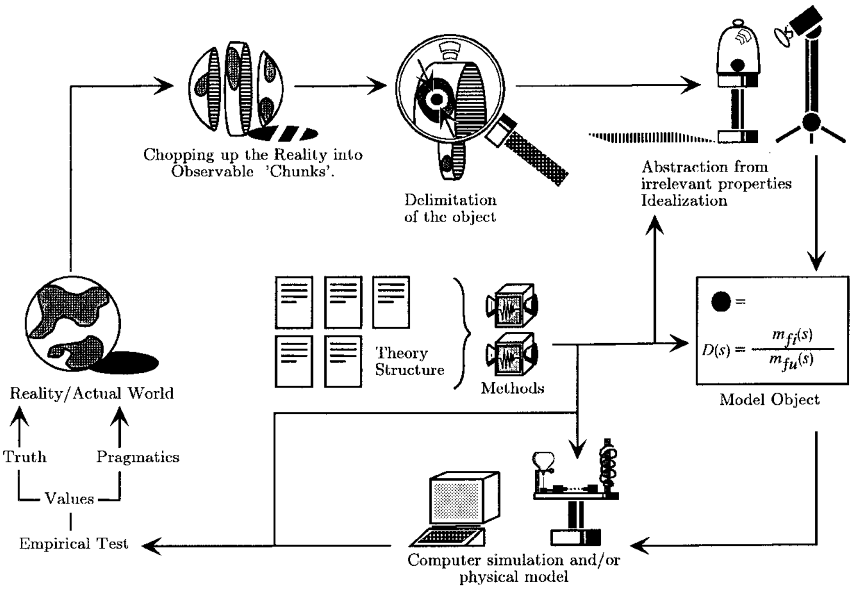

Figure 1. The structure of scientific inquiry.

Before engaging in any such discussion, the process of preparing a research object will briefly be reviewed.[5]

3.2. *Research Object*

Preparing the initial research object may be conceived as a three step process:

1. *Isolation.* The part of reality to be studied is isolated from its environment.
2. *Abstraction.* A *generic* object is constituted by abstracting away from all other properties than the ones to be studied experimentally and theoretically.
3. *Idealization.* All inhomogeneities and imperfections which characterize the actual object are eliminated.

The outcome is an isolated research object, a complex artefact which is understood to be an instance of a perfect generic object controllable according to choice of parameters (hypotheses).

3.3. *Model Object*

A research object is an entity that can be viewed as an instance of a *a model object* which in turn is a conceptual representation of the research object. Model objects are abstract conceptual and typically formal objects and are often considered to constitute the proper objects of scientific and engineering inquiry.

Consider for instance the field of spark ignition engine control systems. Conventional electronic engine control systems typically suffer from poor transient air/fuel ratio control accuracy [Hendricks *et al*. 92]. This is due to a number of reasons, one of them being that the conventional systems fail to compensate for the non-linear dynamics of the fuel film. Usually, certain physical compensators are used to deal with this issue which of course should be non-linear. However, most algorithms consider them to be linear for ease of computation. This is only possible under the simplifying assumption that the fuel film model is linear (when it in fact is non-linear due to the fact that the parameters of the model strongly depend upon the engine states and input) an ‘ideal linear compensator’ (this compensator could be “•”; in Figure 1) can be expressed as the transfer function *D*(s):

$$\text{“}\bullet\text{”} = D(s) = \frac{m_{fi}(s)}{m_{fu}(s)}$$

where s is the Laplace operator, m_{fi} is the injected fuel mass flow, and m_{fu} is the commanded fuel mass flow. This is exactly to consider the compensator as a model object. It does not exist in the real world as a transfer function, but of course as some physical entity, but as a model object the equation is satisfactory under the simplifying epistemic assumption that the fuel film model is linear for small input and state perturbations which again limits the set of circumstances under which the compensator may work properly. The engineer has to succeed under all circumstances in which the fuel film model is linear, but this unfortunately implies poor transient air/fuel ratio control accuracy, since the fuel film model in fact is non-linear.

3.4. *Epistemic and Ontological Assumptions*

During inquiry, a scientist typically and tacitly requires two sorts of fundamental assumptions. Epistemic assumptions comprise axioms of two kinds:

1. axioms describing the conditions under which knowledge may be obtained, and
2. axioms of successful epistemological convergence (verification, refutation, decision or discovery) to knowledge (for example theories).

(2) will be dealt with in greater detail in the below section on methods since it largely is a methodological issue.

The other sort of fundamental assumption consists of the ontological assumptions. Ontological assumptions describe the fundamentally existing components of the world which the scientist is prepared to accept. Pertaining to these assumptions one may either adopt a fundamental or an instrumental view. Pure scientists typically adopt the former fundamental view of their posited components, otherwise the whole enterprise of pure science seems to vanish.[6]

Engineers, on the other hand, as a rule adopt the latter instrumental view. For instance, in modelling a human shin-bone (tibia) [Thomsen 90] with a highly complex geometry and structure the engineering scientist may choose to consider his instrumental ontology not to include the human shin-bone as such but rather a so-called Timoschenko bar which is a mathematically very feasible object. The human shin-bone is for ease of modelling and computation viewed as included in the 'dynamic and structural properties of a rectilinear, twisted, non-uniform Timoschenko bar which was made up of two linearly elastic and transversally isotropic compounds and one flexible compound' [7] (*i.e.* the model object). This finite element model of the human tibia proved to be quite accurate with certain boundary conditions and is nowadays taken to constitute one of the main engineering understandings of the human shin-bone. However, the shin-bone of course still consists of bone, marrow, a complex curvature and geometry, so the finite element model may only instrumentally be considered to be an ontological object.

In the pragmatic universe, which is of professional interest to engineers, problems concerning metaphysical assumptions are of little, and in any case, of derived interest. Developments and debates about metaphysics governing the scientific objects are almost exclusively of interest to pure and perhaps applied science. For engineering scientists metaphysical assumptions and problems contribute to a 'pre-understanding' shared with many other professionals and professional groups. For pure science however, debates over metaphysics are debates over truth and is therefore clearly an important concern, if not *the* concern. Much of pure scientific work goes to circumscribe the set of possible ways the world could be, *i.e* circumscribing background knowledge. The difference in view of the ontology is closely related to their (pure and engineering science) respective cognitive goals, an issue to be treated below.

It is possible to describe in more detail the structure of the axioms in 1. Suppose a scientist is investigating certain features of the gravitational force on Earth, then he often tacitly assumes that he only has to consider ways the world could be, in which the gravitational force exists and the worlds considered *ceteris paribus* exhibit the same other features as planet

Earth. The application of scientific law and theory to a given actual situation is usually hedged with the proviso that for the laws, predictions or dictates to hold, all other unspecified features of the world are normal. This qualifying *ceteris paribus* clause circumscribes the background knowledge and at the same time forces the scientist to assume the possibility of success beyond what is immediately observable. For instance 'In all rigid frame buildings in the U.S., the concrete participates with the web in steel in carrying the shear' is *ceteris paribus* true of all rigid frame constructions including the collapsed bays in front of the field inspectors *insofar* the building contains concrete, a steel web, and an original engineer who wasn't European nor German'. The law or theory is true unless some extraordinary circumstance holds. So the field inspectors may have to leap beyond available evidence.

Ceteris paribus clauses (default clauses) are typical in science. The exigencies of science, whether, pure, applied or engineering require leaping to conclusions and going beyond the available evidence. An interesting result of formal logic is the non-monotonic nature of *ceteris paribus* clauses. Say that a set of epistemic assumptions is monotonic if upon adding assumptions, derived theoretically or experimentally, the same consequences are still derivable. On the other hand, a set of epistemic assumptions is non-monotonic if upon encountering new evidence, some of the original theorems are no longer derivable, by contradicting and blocking some important fundamental axioms used in the derivation. Suppose the scientist discovers that he fails to succeed in some circumstance that *ceteris paribus* should be within his range. Of course, the evidence received for the failure could be wrong. But it may also be a flaw in the epistemic assumptions. Then he may be forced to revise his epistemic assumptions; unfortunately deductive logic alone does not dictate any help in fulfilling this task. There is a multiplicity of ways in which the scientist can revise his epistemic assumptions in order to accommodate the new discovery, all of which are consistent extensions, contractions or revisions, but none of which deductive logic can unambiguously settle. Hence, which extension or revision of the epistemic assumptions is the right one? Formal studies to this end furnish different replies. The typical and most popular solution is to assume some epistemic assumptions' epistemic entrencement or superiority over others. In other words, in the scientists (or community's) web of beliefs, the ones closer to the core of the web are less pruned to be the victim of revision.

The axiomatic superiority of some epistemic assumptions has its own explanation. It stems from their ontological significance which is partially illustrated both in the theory structure and exemplars/research competence, both themes to be dealt with below.

3.5. *Theory Structure*

If knowledge attainment is relative to background knowledge, assessing and discovering theories and hypotheses, making predictions etc. are all epistemic constructs; only if one obtains success in all state descriptions simpliciter may one speak of theories, hypotheses and predictions as being genuinely ontological constructs. Of course, since the background knowledge is a proper subset of the set of all possible state descriptions, some of the ontological features are present in the background assumptions adopted, but not all of them. This is were the danger of failure lies as illustrated by the example from concrete technology. However, considering theories to be epistemic constructs does not imply turning the entire ontology into one. In the world exists whatever exists, but the scientist's circumscription of relevant research parameters and subsequent theoretical descriptions of these parameters are epistemic constructs.

Consider the field of analytical mechanics. The laws and theories of analytical mechanics may be described by the following structure. The top-level of the structure includes the abstract symbolic laws like energy conservation. Below, one encounters the so-called Hamilton equations which are designed to express properties of classical mechanical model objects. Further down this latter more concrete laws like Newton's law of gravitation and Coulomb's of force between particles emerge. But when an engineering scientist applies analytical mechanics to study, say, strength in various materials like steel and concrete, he may for fundamentals profitably parasite on higher-level laws and theories but these unfortunately do not directly reveal how for instance the process of rupture in building blocks develop. Hence, additional laws and generalization may have to be developed based on both empirical and theoretical considerations which cannot be deduced from basic physics. A comprehensive study of these additional laws may be found in [Jakobsen and Pedersen 96]. One of the upshots of this investigation is that engineering tends to have a highly eclectic use of basic theory and adopts this basic theory to particular problems.

The point to be pursued is that the system of laws within a scientific theory is hierarchically organized in accordance with some measure on epistemic entrencement or superiority. Consider the laws of energy conservation again. These laws may be labelled *principles* since they furnish very general descriptions of all kinds of systems and can be considered to be universal constraints which all kinds of physical systems must satisfy.

Suppose, these principles of energy conservation ceased to hold. Such a situation may provoke heavy revisions in the theoretical structure, because *ceteris paribus*, nothing any longer remains equal. Such potential revisions are clearly undesirable because any contraction, extension, or revision of the background knowledge is now consistent (Quine-Duhem taught us that), since there is nothing besides the evidence to be consistent with.

On the other hand, Newton's and Coulomb's laws are usually not considered to be principles. They are less general and designed to hold for certain regions of the world. These laws may be revisable even though they by no means are simple empirical generalizations. The fact that both electrical and mechanical forces operate inversely with the square of the distance is extremely well-corroborated both theoretically and experimentally. So any revision of these laws would require a substantial reorganization of the theoretical structure, but not any possible replacement goes. *Ceteris paribus*, the principles of energy conservation would still have to hold so these principles delimit the set of possible consistent extensions, contractions and revisions.

3.6. *Methods and Methodology*

Methodology is the study of the methods by which science arrives at its posited truths. Attainments of knowledge and results may come about in many ways. One way is to formulate some hypothesis and wait for evidence, or design experiments, to either verify, refute or decide the hypothesis in question for some criterion of convergence. Call verification, refutation, decision for *success criteria*. Inductive logics and confirmation theory adopt this approach. Such methods, complying with these ideas, will be referred to as *assessment* methods.

Another type of assessment method is experimental parameter variation (EPV). Rutherford's conjecture that in the center of an atom exists a highly concentrated electrical charge was systematically investigated by Geiger and Marsden in 1913 by observing the scattering of a stream of α-particles while encountering the atomic nuclei. Geiger and Marsden systematically varied a number of parameters including the deflection under which the number of scattered particles was counted, the thickness of the metal foil constituting the scattering target, the atomic weight of the foil etc. All done in an attempt to either verify, refute or decide a theoretical conjecture. Schematically, rather than inputting a single evidence stream, the scientist chooses a set of distinct 'tunable' evidence streams, and waits for the method to tell her to what extent the hypothesis is true when the parameters are either individually tuned or a set of them tuned in conjunction. By this procedure the scientist may fine-tune, refine or constrain an otherwise

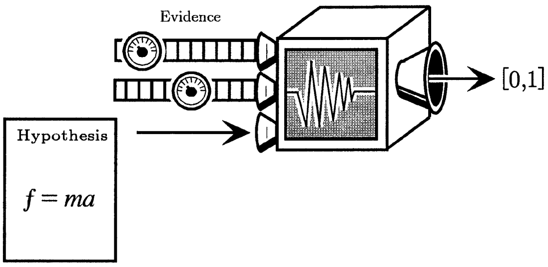

Figure 2. Experimental Parameter Variation (EPV) Methods. EPV-methods take 'tunable' evidence streams and hypotheses as inputs and map into the closed unit interval. The closed unit interval as the range is to specify the extent to which the hypotheses are true, once the evidence streams are tuned.

fundamentally true theory.

Experimental parameter variation methods serve at least two purposes. For one, they aid in the refinement or modification of a given theory or theoretical model. According to Vincenti [Vincenti 90], pure science applies EPV-methods in this way as illustrated by the Rutherford example. Engineering sciences on the other hand applies them somewhat differently:

> Experimental parameter variation is used in engineering (and only in engineering) to produce the data needed to by-pass the absence of a useful quantitative theory; that is, to get on with the engineering job when no accurate or convenient theoretical knowledge is available. This is perhaps the most important statement about the role of parameter variation in engineering.[8]

This gives way to the second purpose that EPV-methods serve. EPV-methods in engineering are intended to provide useful design data rather than assessing given hypotheses within the bound of the evidence tuning. In this respect, EPV-methods are more akin a kind of discovery algorithm than an assessment procedure, However, before discussing EPV-methods for engineering it may be instructive to look at the classical philosophical reservations toward discovery algorithms.

Indeed, prominent epistemologists and philosophers of science have argued that assessment methods are the real object of research for science and the philosophy of science. This lead Reichenbach to formulate the now classical distinction between the *context of justification* and the *context of discovery*. Whether a formulated hypothesis or theory is verified or refuted by the evidence is strictly a logical matter which can be settled 'out of court' in a *reliable* fashion. Conversely, no guidelines or logical prescriptions can be given for the discovery of new hypotheses from evidence. The discovery of new hypotheses is like art, aided by intuition, sudden insights and perhaps mystical revelations – *unreliable* in sum. This line of thought is also emphasized by Hempel's later speak of the *logic* of justification

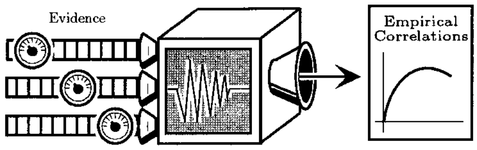

Figure 3. EPV-methods in engineering. Experimental parameter variation in engineering is different from EPV in pure science. While pure science attempts to assess a certain hypothesis or assess the boundary conditions within which the hypothesis is acceptable or unacceptable, EPV in engineering attempts, by varying parameters, to find empirical correlations adequate for design.

while keeping Reichenbach's context of discovery. Reichenbach's idea was embraced from philosophers like Carnap, Hempel and Popper to physicists like Einstein. Discovering new hypotheses from the evidence should, if studied at all, be studied by empirical psychology, sociology and history. It is however noticeable that Kuhn never accepted this distinction himself. It may even be formally shown, that the stipulation that there is no such logic of discovery simply is an inaccurate dichotomy from a certain point of view.[9]

Take an engineering discovery. In 1923 Frank Whittle filed a patent describing a scheme for a gas turbine for the jet propulsion of an aircraft. The main idea was to generate the propulsive jet of air within the turbine itself rather than to use the engine to drive a propeller. The innovation was not a divine revelation. Rather, the theoretical scheme he proposed resulted from a careful collection of empirical evidence combined with proper theory of mathematics, physics and aerodynamics. In particular, by purely scientific arguments based on physics and applied mathematics he demonstrated the need of shaping the turbine blades according to correct aerodynamic principles. We may say, that the theory of gas turbines accredited to Whittle is an instance of discovery based on evidence and auxiliary well-established theory.

Returning now to EPV-methods in engineering one may identify them as a kind of discovery processes.[10] The engineer is in need of adequate design data rather than assessment of theory. Insofar as the engineer does not have convenient or adequate theoretical knowledge for either some particular construction or a batch at his disposal, he may have to systematically vary the important parameters in such a way that they provide empirical correlations eventually furnishing acceptable design data.

Empirical correlations are much weaker constructs than theories. Emperical correlations may be either particular or general. While establishing Hook's law scientists plotted stress curves for different metals under different conditions and eventually ended up with a correlation curve describing

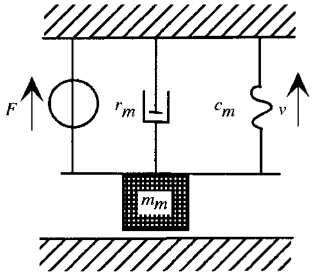

Figure 4. The mechanical System.

regularities sufficient for a generalization. However, one would be hard pressed to call this a theory. Returning to the example from concrete technology for which the design parameters were based on EPV, the collapse of the warehouses initiated a thorough fundamental investigation of structural strength and behavior. 13 major beam specimens were tested with respect to failure under flexural loading and with respect to flexure and axial tension given a 1/3-scale model of the second frame span of the original warehouses. This experimental parameter variation finally resulted in a new empirical correlation in the American building code such that 'steel strapping applied as external web reinforcement with a prestress exceeding 30.000 psi to frames should be an adequate remedial measure'.[11] In general, empirical correlations describe either special cases, empirical regularities, or optimization conditions for designs.

Finally, an interesting methodological issue tagged onto engineering and EPV is the extensive use of both analogies and lumping. In general, lumping denotes the application of many different (even from different fields of science) sub-models and -systems to describe some complex system. Perhaps, this methodological characteristic is best illustrated by the operationalized version of lumping in terms of *lumped-parameter models*. Lumped-parameter-models are characterized by breaking complex systems into sub-systems each of which describes some aspect of the overall original complex system. Once the parameters have been identified one may often gain from making analogies between the given system and another model system which is well-understood and nearly ideal in terms of behavior.

Consider for instance the suspension of a wheel of a car. The mechanical system describing the suspension is shown in Figure 4.

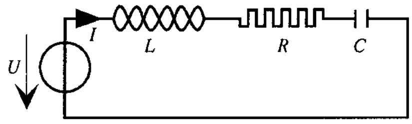

Figure 5. The Electrical Circuit.

The mechanical impedance Z_m of the actual suspension is given by the equation:

$$Z_m = \frac{F}{v} = j\omega m_m + r_m + \frac{1}{j\omega c_m}$$

where F is force, v is velocity, $j = \sqrt{-1}$, ω is the angular frequency, and its parameters are r_m for loss resistance and c_m is compliance (*i.e.* the reciprocal spring constant). The dynamic properties of the system may be described by the constant coefficient integro-differential equation:

$$F = m_m \frac{\mathrm{d}v}{\mathrm{d}t} + r_m v + \frac{1}{c_m} \int_0^t v \mathrm{d}t$$

Now, consider a series LRC electrical circuit (Figure 5). The components of such a circuit are nearly ideal in terms of dynamic behavior. Thanks to current technological knowledge and skills, nearly ideal resistors, capacitors, inductances can easily be made which are accurate down to a few tenths of a percent.

The equations describing the electrical circuit are easily solvable, well-understood and provide close to a perfect fit with the evidence implying nearly a perfect identification of the parameters. The electrical impedance Z_e of the circuit is given by the equation:

$$Ze = \frac{U}{I} = j\omega L + R + \frac{1}{j\omega C}$$

where U is the voltage drop, I is the current, L is the inductance and C is the capacitance. Additionally U is given by the following integro-differential equation:

$$U = L\frac{\mathrm{d}I}{\mathrm{d}t} + RI + \frac{1}{C} \int_0^t I \mathrm{d}t$$

There is an astonishing formal similarity between the former and the latter sets of equations. In fact they have the same form even though the parameters are different. However different, the set of equations describing the electrical circuit is identical to the set of equations describing the mechanical system and one may identify the electrical counterparts to the mechanical components [Rasmussen 80]. For instance, instead of mass one has inductance, instead of loss resistance one has resistance, instead of compliance one has capacitance. In general:

Mechanical System	SI-unit	Electrical Circuit	SI-unit
Force – F	N	Voltage drop – U	V (Volt)
Velocity – v	m/s	Current – I	A (Ampere)
Mech. Impendance – Z_m	Ns/m	Impendance – Z_e	Ω (Ohm)
Mass – m_m	kg	Inductance – L	H (Henry)
Loss Resistance – r_m	Ns/m	Resistance – R	Ω (Ohm)
Compliance – c_m	m/N	Capacitance – C	F (Farad)

The operationalized version of lumping in terms of lumped parameter models illustrates both points. First, the model of the car suspension is a lumped-parameter model consisting of a number of individual parameters all together comprising the overall complex. Second, the identification and understanding of the mechanical system and its parameters may be based on its analogy to the well-known electrical circuit model or vice versa. The two models are actually isomorphic.

Lumping is probably a very representative aspect of the engineering endeavour. An engineering scientist chooses to break a complex system into manageable sub-systems which are individually easier describable. He attempts to construct an overall picture of the complex system by fitting together the different sub-systems. But there is no guarantee that the real world is breaking its complexes (if it breaks its complexes up at all) up into exactly the systems chosen by the scientist nor that the systems are strictly with each other compatible. Analogies may make up for some of the latter difficulty, even though reasoning by analogy by no means is reasoning by deduction or reliable induction. But the former may still be resistent. However, lumping is not necessarily connected with the aim of getting to the truth of the matter, whatever the truth about the complex system may be, but rather connected with the aim of furnishing the *ultimate practical explanation and justification*. Hence, if the lumping works within acceptable statistical boundaries of accuracy, then that is as good as it is going to get for all practical purposes. So lumping typically signifies the existence of a different cognitive goal than truth, namely practical usability, which is of course as it should be for engineering (cf. further below).[12]

It is also noticeable that the use of lumping and EPV is less pronounced in the pure sciences because the aim of inquiry is truth rather than practical explanations; of course there are borderline cases like solid state physics which does indeed use lumping. However, even in the unlikely event that the world breaks some complex system into the same sub-systems as the one advocated by the pure scientist, subsequently fitting the sub-systems to the overall system require analogous reasoning which is doubtfully a very reliable inference engine.

But just because analogous reasoning isn't a very reliable inference engine does not entail that it finds no application in the pure sciences, especially during the process of discovery. Take Maxwell's equations for instance. Even though all four equations were not all Maxwell's inventions[13], his putting them into a coherent system was. Curiously enough, two of the equations were established by making analogies to fluid mechanics. In later publications on electro-dynamics when the theory was well-established by itself, Maxwell explicitly dropped the analogy to fluid mechanics, since it was no longer needed for the preliminary justification of the theory; preliminary justification, since the theory ultimately required relativistic principles.

3.7. *Values*

In general, a notion of *correctness* will be considered to be a fixed relation between, the world, the hypothesis and the method the scientist applies. This is essentially due to the fact that the scientist applies certain theoretical and/or experimental methods to formulate hypotheses correct or incorrect of the aspect of the world under consideration. Correctness could for instance be theoretical truth, but could in principle also denote other cognitive goals of say empirical adequacy, simplicity, unification, consistency, practical usability etc.

In the philosophy of science there are often disputes pertaining to what should count as cognitive goals or values and what should count as methodological recommendations for 'rational' scientific inquiry. These two things are not to be equivocated among. Some pure scientist may have a cognitive preference for truth as a value but may apply a method that gives simple hypotheses. But simplicity as a blessing by itself may still be a bane since it may not be sufficient to secure truth if truth depends on something other than the simplicity of the formulated hypothesis. So other standards may be imposed as well. However no matter how many other auxiliary standards for justification the scientist chooses to entertain, if the cognitive goal is truth, the scientist has to (epistemologically) ensure that the standards by which he decides to justify are essentially truth-conducive principles.

Consider the following view of the engineering sciences:

One might think of engineering sciences as intrinsically objective but operating within a value-laden context. The engineering sciences were created to serve certain generalized goals. Specific values appear in the design context, where people make decisions about building or not building artifacts having particular attributes and certain anticipated negative side effects in order to achieve a specific goal deemed to be beneficial.[14]

An engineer, as opposed to a scientist working in pure science, may hence be satisfied with some pragmatic notion of practical usability as the ultimate cognitive value rather than theoretical truth. Then practical usability replaces truth as the relation of correctness. Operationalized, probably the most important methodological recommendations (or Values according to Kuhn, see below) in engineering include safety and simplicity. Take safety first. For instance, the theory for designing air-craft propellers in the 1920's was poor. Especially, the theory couldn't deal adequately with the secondary effects of viscosity and compressibility yielding poor correspondence between theory and experimental results. To remedy this, even in the 1940's designers were instructed to, whenever proper information was available, to select propellers based on experimental parametric data (obtained by EPV), rather than improve the theory. It was considered more practical and safer too.[15]

Simplicity in engineering has a different ring to it than in the pure sciences. Simplicity in engineering is related to the task of making a certain model and adhered methods less time consuming. A typical sequence of steps is illustrated in the formerly mentioned project where the dynamic properties of a human shin-bone are investigated [Thomson 90]. In the initial steps a rather complicated and theoretically correct model of the human shin-bone, a Timoschenko bar, is applied. In subsequent steps simpler models are introduced and it is concluded, that for all practical purposes it is safe to use the much simpler Bernodly-Euler bar. The application of the Timoschenko bar thus validates the use of the Bernodly-Euler bar without selling out on too much of the reliability of the model.

Kuhn himself also acknowledges a similar point. In discussing choice of theories he argues that the justificational standards for choice (according to Kuhn standards including simplicity, accuracy, broad scope, fruitfulness) of theories rather are cognitive values than methodological recommendations:

I am suggesting, that the criteria of choice with which I began function not as rules, which determine choice, but as values, which influence them.[16]

This also explains why the engineering scientist does not have to provide an epistemological argument securing the truth-conducive nature of the adopted principles for justification. Only if the principles were intended to be *truth-tracking*, would such an argument be necessary.

3.8. *Exemplars and Research Competence*

Examplars play a paramount role in shaping the general research competence of the individual scientist. Assuming familiarity with Kuhn's general notion of exemplars and their role in the pure sciences we will initially restrict attention to a relatively brief account of the exemplars common to engineering science, and subsequently describe what research competence amounts to. This is due to the fact that the differences in the exemplars, apart from their obvious differences, are probably less significant than the research competence they are respectively intended to shape.

Time an again engineering students are confronted with a wide range of theoretical and technical problems solved in rather similar ways. The students see a uniform way of analyzing problems and identifying factors which are sufficient to constitute the main factors in a causal or stochastic model. Additionally they witness the same assumptions being made and the same type of factors or parameters being neglected. They meet in hydraulics, control theory, mechanics and various other fields the same procedure for interpreting problems and setting up differential equations and integral functions or making Laplace transformations or regression analysis. Apart from being familiar with a certain way of using a simplified and limited part of mathematics, they meet a uniform framework for interpreting and modelling nature as a controllable mechanism. Repeatedly the students experience linearization, idealizations and approximations leading to acceptable results. In far most cases the stability and reliability of such procedures and methods are confirmed. Partly, of course, because the problems presented to students are chosen with the aim of confirming the theory demonstrated.

In engineering science these exemplars seem to have the form of repeated demonstrations of such procedures on rather commonplace problems in different areas. Continuous exposure to exemplars during the period of study and training eventually provide the engineering student with some outcomes:

1. *Circumscription of background knowledge*. Hence, a certain understanding of nature and the role of technical problem-solving. This understanding defines and circumscribes the limitations of the engineering scientific universe. It states the degree of understanding of regularities of nature which is necessary for manipulating it.
2. *Recognition of the nature of the paradigmatics*. For an engineering scientist this, among other things, amounts to a conviction about the possibilities and stability of using simplified natural science and other sciences for technical-problem-solving. This component includes a confidence in such simplifications and analogies which are considered

reasonable. Applying methods from natural science: if, say, linearizations and approximations are reasonable, stability of the solution is ensured. Assuming perfect elasticity in a material by a method of calculation is an approximation, but deviations due to approximations will not exceed what is permissible, when the method is used within certain limits.

3. *Preferences concerning methology*. The exemplars relate to certain types of delimitation of problems, identification of decisive factors, simplification of theory, quantitative methods, lumping, and types of justification. In spite of the generality of such preferences, groups of engineers also hold preferences for different methodological approaches. In building statics for example some groups of (especially Danish) engineers base the development of methods for calculation on assumptions of ideal plasticity claiming the superiority regarding simple and reliable methods. In a field like geo-technics, different groups of engineers hold preferences for scale experiments in artificial gravity fields – for conventional theoretical opposed to more practical empirical methods and for stochastic methods.
4. *Preferences concerning epistemology and the aim of inquiry*. Since the aim of inquiry is largely practical, the auxiliary epistemic assumptions made for satisfying the goal or contructing an adequate model are not up for debate, unless they have a practical impact. Hence maximal levels of abstraction are allowable in the epistemic assumptions, but then they only seldomly serve a practical purpose. Instead optimal epistemic assumptions for all practical purposes are entertained.

A similar, but in many cases opposite, account may be provided for the pure sciences's use of exemplars. These outcomes of exposure to exemplars look suspiciously familiar. In essence, they are the components delimiting a technical matrix, but they also constitute the research competence. So the ideal research competence amounts to the complete recognition of the intrinsic characteristics of ones own scientific field, whether one is a pure scientist or an engineer. Formulated differently, the research competence individuates the technical matrix within which the scientist is operating, much in the same way as linguistic competence individuates the language a user speaks.

The engineering student becomes an ideal engineering scientist once the research competence is complete. Of course, complete research competence is a limit concept as much as an ideal engineer or ideal pure scientist is. But this is just to say that scientific development as such converges in the limit to ideal research competence relative to a specific technical matrix. To close the loop of this investigation, it remains only to be said, that scientists, both pure and engineers, are far from the ideal research competence. But this is exactly the scientific challenge, otherwise the scientists would be omiscient relative to a matrix, they would know the entire world of the matrix all at once, there would be no such things as underdetermination and there would be little, if anything, to inquire about. None of which were found to be the case.

4. ENGINEERING VS. PURE SCIENCE: MESSIANIC OR MYOPIC INQUIRY

The below table summarizes some of the conclusions reached so far.

	Pure Science	Applied Science	Engineering Science
Examples	Hydrodynamics	Hydraulics	Theory of urban waste water systems
Delimitation of Objects	Idealized, isolated objects. Causal mechanisms		Physical (real) entities and artefacts in environments created by man. Intentionally determined
Epistemic and Ontological Assumptions	Essential	Less essential	Adopted from pure science
Theory Structure	Hierarchical structure of nomological systems Mainly mono-paradigmatic		Theory adopted to problems. Polyparadigmatic. Ecclectical use of theory
Methods	Derived from theory		Methods are more fundamental than theory
Values	Explicit justification. Truth is important		Implicit justification. Effiency and practical usefulness. Pragmatic concept of truth
Exemplars	Building Research Competence		

One may observe a fluctuation of the objectives moving from pure science towards engineering science. While the objective for pure science is a (at least) theoretically true representation of the actual world, the objective for engineering science is an optimal degree of theoretical correctness (typically limited by time and resources) combined with pragmatic considerations of practical usability. However, practical usability does not imply truth, nor does theoretical truth imply practical usability. The latter direction is obvious while the former perhaps is in need of an example. Consider the modern engineering methods for the calculation of structural elements of concrete and reinforced concrete. Two different and to some extent competing perspectives are adoptable: Either one many consider concrete as behaving as an elastic material up to a certain load which leads to rupture. Or, one may adopt the plasticity view of concrete for which concrete is assumed to be rigid-plastic for some specified boundary conditions such that no deformations occur for stresses up to a certain point called the yield point. When dimensioning a structural element one can, for a given load, calculate the necessary conditions for keeping some deformation inside acceptable and permissible limits. These calculations are profitably based on the assumption of perfect elasticity. On the other hand, while assuming perfect plasticity it is possible to develop a general theory of rupture which fits experimental evidence reasonably well. Either one of them is practically feasible and have positive merits depending on application, but none of the views are strictly speaking true. Hence, engineers usually adopt a combined view, assuming plasticity and elasticity wherever and whenever they respectively prove to be over the other superior.

In pure science one may crudely relate being, truth and reliability as counterparts to each other in the following way:

Ontology	**Epistemology**	**Methodology**
Being	Truth	Reliability

On the other hand we have the following distribution in the engineering sciences:

Ontology	**Epistemology**	**Methodology**
?	Practical usability	Simplicity

Reliability may be imposed as a methodological requirement on a method in the aim of finding the truth which explains why, for instance EPV, is rarely applied in the pure sciences.

Conversely, from a philosophical point of view, without some sort of epistemological argument to secure, say, the simplicity of the underlying reality, the approach suggested by a philosopher like Horwich [Horwich 90] implies an initial indifference to the question of whether following the proposed rules or embracing the methodological maxims has anything whatsoever to do with finding the truth, now or ever. For all that has been said, inquiry may proceed forever 'rationally' according to the dictates, and yet chase its tail for eternity, headed nowhere near truth. Such a perspective on inquiry may, philosophically, be called *categorical methodology*. We may clarify the point by invoking a distinction drawn by Levi. Levi [Levi 91] explicitly endorses what he calls the 'myopic' perspective on inquiry, *i.e.*, gaining truths and avoiding errors. He calls the alternative 'messianic'. However Levi's notion of 'myopia' must not be confused with categorical methodology. Levi's own approach is hypothetical, not categorical: it is based on epistemic utilities, which balance truth against content. Methodologists concerned with methodological recommendations, *i.e.*, simplicity, consilience, predictiveness etc. for holding a particular belief, have paid little or no attention to matters of reliability. Categorical methodologists might respond that the proposed norms are motivated not hypothetically, as means for finding the truth, but categorically, as ends in themselves. But an engineer can respond differently. He can agree to the fact that he may not be anywhere near truth, agree to the fact that truth and practical usability may conflict, and even agree to the fact that the applied methods are unreliable. But by so admitting, the engineering science does not have to become a discipline of messianic nature intrinsically as long as it does not deny that there is something called truth. What engineers do have to ensure is that, say, simplicity is tracking practical usuability and at the same time agree *that theoretical truth is not a Kuhnean value of theirs*.

NOTES

1 [Gutting 84], p. 63.
2 [Anderson 57], p. 68.
3 [DHI 96].
4 This conjecture seems, by and large, to be shared by [Gutting 84], [Wojick 79] and [Constant 1980].
5 For a comprehensive account of this process, see [Jakobsen and Pedersen 96].
6 Of course, pure scientists may during inquiry consider some of their components as instrumental means, but they must ultimately decide whether these means are part of their ontology.

[7] [Thomsen 90], p. 216.
[8] [Vincenti 90], pp. 161.
[9] See for instance [Kelly 95] and [Hendricks 96].
[10] Inference to the best explanation (IBE), as a type of discovery algorithm, may also be frequently encountered in engineering.
[11] [Elstner and Hognestad], p. 665.
[12] Finally, lumping typically requires a host of auxiliary (some tacit) assumptions for completion of the over-all fit of the sub-systems implying a rather complex test situation if the truth of these assumption is to be assessed rather than just their practical feasibility.
[13] One equation was Faraday's law of induction, another was Ampère's circuital law.
[14] [Layton 88], p. 92.
[15] [Vincenti 1990], pp. 155–156.
[16] [Kuhn 73], p. 336.

REFERENCES

Anderson, B. G. (1957), 'Rigid Frame Failures', *Journal of American Concrete Institute* **23**, 625–636.

Andreasen, B. S. (1989), *Anchorage of Robbed Reinforcing Bars*, Doctoral Thesis, The Technical University of Denmark.

Bunge, M. (1966), 'Technology and Applied Science', *Technology and Culture* **6**, 329–347.

Bucciarelli, L. (1994), *Designing Engineers*, Cambridge, The MIT Press.

Compton, A. H. (1921), 'Possible Magnetic Polarity of Free Electrons', *Philosophical Magazine* **41**, 279–281.

DHI, Danish Hydraulic Institute (1996), MOUSE (Computer Application), ed. 3.3. Hørsholm, Denmark.

Elstner, R. C. and Hognestad, E. (1957), 'Laboratory Investigation of Rigid Frame Failures, *Journal of American Concrete Institute* **23**, 637–668.

Gutting, G. (1984), 'Paradigms, Revolutions and Technology', in *The Nature of Technological Knowledge. Are Models of Scientific Change Relevant?* Laudan, R. (ed.), D. Reidel Publishing Company: 47–65.

Horwich, P. (1990), *Truth*, Cambridge: Blackwell.

Kuhn, T. (1970), *The Structure of Scientific Revolutions*, Chicago: University of Chicago Press.

Kuhn, T. (1973), 'Objectivity, Value Judgment and Theory Choice', Machette Lecture, Furman University.

Kelly, K. T. (1996) *The Logic of Reliable Inquiry*, Oxford: Oxford University Press.

Jakobsen, A. and Pedersen, S. A. (1996), 'Engineering Science and the Reality', forthcoming.

Layton, E. T. (1988), 'The Role of the Engineering Sciences', *History and Technology* **29**, 82–97.

Levi, I. (1991), *The Fixation of Belief and its Undoing*, Cambridge: Cambridge University Press.

Hendricks, E., Versterholm, T. and Sorenson, S. (1992), 'Non-linear, Closed Loop, SI Engine Control Observers', SAE Technical Paper, *SAE Annual Meeting*, Detroit, MI, U.S.A., February.

Van Fraassen, B. (1980), *The Scientific Image*, Oxford: Clarendon Press.

Hendricks, V. F. (1996), *Epistemology, Methodology and Reliability*, Doctoral Thesis, University of Copenhagen.

Peirce, C. S. (1958), *Charles S. Peirce: Selected Writings*, Ed. Weiner, P. New York: Dover Publications.

Price, D. de Solla, (1962), 'Is Technology Historically Independent of Science? A Study of Statistical Historiography', *Technology and Culture* **4**, 553–568.

Rasmussen, K. (1980), *Analogier mellem Mekaniske, Akustiske og Elektriske Systemer*, Polyteknisk forlag.

Simon, H. A. (1969), *The Sciences of the Artificial*, Cambrige: Mass.

Skolimowski, H. (1966), 'The Structure of Thinking in Technology', *Technology and Culture* **6**, 371–383.

Thomsen, J. J. (1990), 'Modelling Human Tibia Structural Vibrations', *Journal of Biomechanics* **23**, 3: 215–228.

Vincenti, W. G. (982), 'Control-Volume Analysis: A Difference in Thinking between Engineering and Physics', *Technology and Culture* **23**, 145–174.

Vincenti, W. G. (1990), *What Engineers Know and How They Know It.* Johns Hopkins University Press.

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