

Outline for a Morphology of Modelling Methods

Contribution to a General Theory of Modelling

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Abstract: The purpose of this article is to classify and compare – in essence to *model* – a number of different types of modelling methods employed within Operations Research and the Management Sciences (OR/MS). The classification of these methods is based on a selected number of generally recognised modelling properties. On the basis of this meta-model, requirements for the successful application of different modelling methods – for the study of given systems or objects of scientific enquiry – can be examined. The method employed for this meta-modelling task is General Morphological Analysis (GMA). The problem of a General Theory of Modelling (GTM) is also discussed.

Keywords: general theory of modelling; morphological analysis; modelling methods; OR methods

1. Introduction: Why is there no general theory of modelling?

Why is there no *general theory of modelling*? By this I do not simply mean a logico-mathematical theory or a formal theory of *representation*. I mean an overall framework which treats of the epistemological, methodological and operational aspects of scientific modelling as a whole – from physics and biology to design theory and social planning models. No such framework seems to exist. There is no systematic (operational) classification of modelling methods; no commonly agreed upon terminology for different types of models and their properties; and no heuristic procedure or diagnostic for testing the applicability of different modelling methods for different modelling tasks. And although there are more than 100 acronyms that employ the letters GTM (including Global Traffic Manager, Gross Trading Margin and the chat marker “Giggling To Myself”), nowhere can I find GTM denoting a General Theory of Modelling.

Forty years ago, as a student first of physics and then social anthropology, I would have given plenty (maybe even cut my hair) for even a simple *typology of modelling methods*. What a lot of time and confusion it would have saved had even a half-decent modelling typology existed. (A role model would have been McKinney’s (1966) “Typology of Typologies”.)

Suppose there *were* a GTM. What would such a theory involve? Here is my take on it, at least as concerns *scientific* modelling (to be sure, a qualification which is not totally clear).

1. It would provide a *real definition* of the concept of a model, i.e. an *operational* definition that tells us something about how models are developed and how they operate. This, in contrast to merely *nominal definitions* such as “a model is an abstract representation of the thing being modelled”.
2. This definitional framework should be as general as possible, in order to contain the principle different types and forms of modelling methods employed in science.

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3. The operational definition should either include, or make it possible to abstract, a set of properties (or parameters) which indicate how models do their work, and by which different modelling methods can be classified, compared, exemplified and otherwise scrutinized.
4. It would provide a framework for identifying the *requirements for the successful application* of different modelling methods for different modelling tasks and for different objects of scientific enquiry. It should be possible to develop a heuristic procedure – a *diagnostic* – to test the applicability of a given modelling method by relating the (methodological) limits of its properties to the demands of the properties of the object being modelled.
5. It should give us a basis for treating models and modelling as a two-way street – as a *filiouque*, so to speak: not simply as “means of representation”, but also as *epistemic tools* in themselves; as instruments for hypothesis and theory generation.
6. Finally, as a corollary to all of the above, it would provide a common *modelling language and terminology*. Indeed, one of the most obvious indications that there is no “real” general theory of models and modelling is the simple fact that there is no generally agreed upon terminology concerning what different types of models there are, their different *physiologies* (so to speak) and their *means* of representation.

Of course, all of this is a pipedream. No such general theory exists. Or, does it?

By their very nature, *models* and *modelling* are subjects of study in virtually every area of science: from mathematics, physics, engineering and computer science, to psychology, sociology and the philosophy of science. I am not going to get bogged down trying to present a survey of the recent history of modelling methods. This would involve a major scholarly undertaking in itself. The following are only *examples* from areas in which I have been involved as a modeller.

If one were to look for a GTM, one might expect to find something of the sort within the framework of Operations Research and Management Science (OR/MS) – as this field of combined research and practice has been especially preoccupied with the issues and problems of modelling techniques for decision support. Rivett’s “Principles of Model Building” (1972) is an example. Laing (1981) put forward a “Classification of Models” based on the work of Rivett, Ackoff (1979) and others, in which he categorises models along a spectrum from the purely physical to the purely mathematical. However, these and other efforts during the 1960-1980 period were directed more towards a modelling *phenomenology*, and Laing never claimed that he was attempting to formulate a general theory.

The development of fast, small computers, advanced graphic interfaces and new high-level programming languages created a veritable paradise for mathematical modelling. In this context, Ziegler (1984) and Ziegler et. al. (2000) offer up what they call a general theory of *Modelling and Simulation* (M&S – which some maliciously enjoy calling S&M). However, while this represents a huge area of study in OR/MS, it is certainly not the only area, and maybe not even the most relevant area any longer. In any event, it is interesting to note that most references to any type of *general* modelling theory concern “mathematical modelling and simulation”. This is still the dominant feature of OR/MS and of “systems analysis” – at least in the U.S. (The fact that Horst Rittel (1972) and Russell Ackoff (1979) delivered devastating critiques of “systems analysis” and traditional “hard” OR methods for social/organisational planning problems seems to have had little impact.)

On the other side of the coin is what are called Problem Structuring Methods (PSM), developed mainly in the U.K. during the 1980’s and put on the map by, among others, Jonathan Rosenhead (1989). PSM has always prescribed a wider set of modelling concepts which have been referred to (*inter alia*) as *soft systems methodologies*. (Peter Checkland’s claim to this term for his particular set of techniques represents only one example.) A more general theory of *conceptual modelling* has also been put forward within the framework of PSM (see e.g. Kotiadis & Robinson, 2008). But, again, these do not constitute an overall framework for a GTM.

Then there is Herbert Stachowiak and his work *Allgemeine Modelltheorie* (1973). Stachowiak is the German language model-guru. He presents an elaborate theory of modelling where the concept of “model” is essentially equated with the *functional mapping of attributes* from the so-called *original* (or target) onto attributes of a *model*. Stachowiak is clearly influenced by cybernetics and the first wave of information theory developed in the 1950’s and 1960’s. His approach is structural and primarily representational (although from the “non-statement view”).

Unfortunately, Stachowiak’s works have not been published in English. It is, of course, preposterous to demand that everything be published in English before being recognised as valuable. However, my skills in German being mediocre at best, and having only read second-hand references to Stachowiak, I am not prepared to make judgements one way or the other. If he has, in fact, formulated a viable GTM, or even put forward an important contribution to such, then his work will last and hopefully, eventually, be published in English.

There are already some indications that Stachowiak has become a source of inspiration for more contemporary theories of modelling. These have emerged since the 1990’s in the areas of information science (IS), software engineering and engineering design theory. Here the intent is to go beyond mathematically based causal modelling into what has been termed *multi-formalism* modelling, *multi-paradigm* modelling and a (more or less) general theory of *conceptual modelling*. Examples have been put forward by e.g. Vangheluwe (2000) and Thalheim (2010).

Also, from the early 1990’s come the notions of *domain ontologies* and *domain modelling*. In this context, a domain ontology is a description of the concepts and relationships that can exist for the purpose of *enabling knowledge sharing and knowledge-reuse among “agents” in information systems*. More specifically it represents “... the objects, concepts, and other entities that are *assumed to exist* in some area of interest and *the relationships that hold among them*” (Genesereth & Nilsson, 1987, my emphasis). The IS concept of a domain ontology was put forward in the 1980’s by John McCarthy (1986) of Stanford University, and was more or less established in the early 1990’s by Thomas Gruber (1995) – whose mantra has become: “*an ontology is a specification of a conceptualization*”.

As usual, these few examples have been taken from Anglo-Saxon and European (in this case, German) research. I am sure that there have been developments towards a general modelling theory taking place elsewhere. Be that as it may, I think that I can safely state that none of the above cited sources qualifies as being called a General Theory of Modelling (GTM), at least not in the sense that I have outlined at the beginning of this paper. *If this be the case*, then the obvious question is: Why? With *models* and *modelling* playing such a central role in all of science, why has there not been a more concerted, “scientific” effort to develop a program for a General Theory of Modelling?

Two obvious thoughts come to mind. Firstly, maybe no GTM is even possible, due to the very nature of the concepts of a “model” and “modelling”. Maybe no stable theoretical framework can be created to treat these concepts in both a (logically) *consistent* and (epistemologically) *complete* manner (*analogous* – and I mean *only* analogous – to the Gödel problem of the un-decidability of certain mathematical proofs). Although this is certainly a valid issue, I do not think that it is a practical one for what is being discussed here. We are not treating the idea of a GTM as a water-tight mathematical proof, but as a framework for a practical set of guidelines that will help us to better communicate about models and the modelling process, and by which we can carry out a critical inquiry into the *requirements for valid application*.

This brings us to the second possibility: that competing “schools” of basic scientific thought (e.g. the generic “realist-nominalist” dichotomy or the *Naturwissenschaft-Geisteswissenschaft* dichotomy) have made it difficult to agree upon a common basis for such a theory, and that those partial theories that do exist are fragmented among a number of scientific disciplines and epistemological outlooks.

In this context, one might expect that the natural sciences, and especially mathematical physics, would already have in place a domain specific GTM – this in contrast to the more “messy” *Geisteswissenschaft*. But this does not seem to be the case.

In 1994, Joseph Agassi presented a conference paper titled: "Why there is no theory of models" (Agassi, 1995). A student of Karl Popper, Agassi has been concerned with the history and philosophy of science and its relation to politics and ideology. In the cited paper, he is mainly concerned with the relationship between scientific models in physics and natural science, and fundamental assumptions about the natural world. But it is fully applicable to the theory of modelling in general – including models for social planning. Agassi sees our aversion to “metaphysics” as one of the main culprits:

“The aversion to metaphysics led to reluctance to analyze the concept [of *model* – TR]. It is the concept of generalized initial conditions, which is still unclear. It cries for examination, especially in view of the dilemma of the *doctrine of emergence* which is now increasingly popular. First we have to give up the hostility to metaphysics and to acknowledge that at its best metaphysics is a powerful *scientific heuristic* and so it often acts as an ally to scientific research.” (Agassi, 1995, Abstract. My emphasis).

"Metaphysics", in this context, means the *epistemological-ontological assumptions* that we make about the fundamental nature of the objects, relationships and processes that we are attempting to model. Agassi is saying that that our inherited aversion to dealing with this issue has led us to treat models and modelling as though these were free of such issues, naively believing that we can make the “metaphysical” issue go away if we choose not to think about it ("I feign no hypotheses").

This echoes the warning that Henri Poincaré gave us 100 years ago: The denial of ontological assumptions does not mean that we have rid ourselves of them, but that we are left with

“... dangerous hypotheses ... which are tacit and unconscious. Since we make them without knowing it, we are powerless to abandon them” (Poincaré, 1913).

While I certainly agree with all of this (I wrote my doctoral thesis on the subject 35 years ago), I wonder if it is the best way to approach the problem of a GTM. For, as Agassi himself has shown in much of his own writing, the history of science is the history of *metaphysical politics*, where it is often policy perception and ideology that define ontological assumptions, not the other way around.

This does not mean that the ontological issue should simply be dropped. However, we can make progress in the area of a GTM without first having to tackle the “metaphysical” issue for science as a whole. Instead, we can treat this problem as a *methodological* (and heuristic) one. Hopefully, the discussion of such methodological issues would eventually lead to a discussion about ontological assumptions, including the possibility of the expansion of present ontological categories.

Importantly, we have to look at scientific models as *epistemic tools* and *theory generation instruments* in themselves. This issue is reflected in the discussion over whether, or to what extent, models should be thought of as being primarily *representational*, or as being *epistemic artefacts* with a life of their own (see e.g. Knuuttila, 2005, 2011). There is, of course, no contradiction here; these are fully complementary roles. However, it would be a mistake to consider these as “just two different roles”. The epistemic approach subsumes the representational role, as it is concerned with the *epistemic means of representation* – a topic that will be at the very core of any GTM.

So much by way a background. This paper does not presume to present a program for the development of a GTM. It presents only the bare-bones morphology for a class of modelling methods utilised in OR/MS. As such, it is incomplete. It is published at this time because of some uncertainty as to if, or when, a more complete morphology will be forthcoming. Also, I do not believe a single person is equipped to carry out carry out such a study. It cries for collaboration.

2. What is a scientific model?

As discussed above, the general notion of a *scientific model* is so broad and all-encompassing that any attempt to give it both a *complete* and (*logically*) *consistent* definition may be futile. *For the purposes of this paper*, I instead put forward what I consider to be two *necessary criteria* for something to be a scientific model (at least as this is usually employed in OS/MS), and then present a number of *properties* that emerge out of these criteria. The two criteria are:

- A. A scientific model must contain two or more mental constructs that can serve as *variables* i.e. *dimensions* which (at least potentially) can support a range of values or states (e.g. variables such as age, gender, product type, operational range).
- B. One must be able to establish *relationships* either between the variable entities *as such*, or between the values of the *value ranges* within the variables (e.g. causal, probabilistic, logical, etc.)

Although this in itself is a very general and abstract notion of the concept of “model” – and is certainly not the only one that can be formulated – I feel that it suffices to illuminate the modelling principals discussed in this outline.

Note, that in earlier studies (Ritchey, 2008, 2011) I put forward a more restricted set of criteria for the notion of a (scientific) model, in which variables were *required* to have specified value ranges, and that the connections between variables were defined on the basis of the connections between their respective value ranges. However, this criterion has the effect of excluding a number of important descriptive modelling techniques (such as classical “influence diagrams” and analytic hierarchies) since the variables (or nodes) of these “models” can be treated as black boxes, i.e. they are not internally specified and allow for no variability. However, for the purposes of this study, this requirement is relaxed in order to explore and compare a wider variety of “modelling types”. In the end, however, what is, and what is not, to be considered a scientific model is a matter of convention, as long as we make are rules clear and we apply them consistently.

On the basis the criteria given in A and B, we abstract the following five properties:

P1. Specification: Are the variables of the model (1) (*internally*) *specified*, i.e. contain a well defined range of ordered or non-ordered values or states; or are the variables (2) *unspecified* and treated as *black boxes*?

P2. Directionality: Are the connections between the variables (1) *directed* or (2) *non-directed* (*symmetrical*)?

P3. Quantification: Are the relationships of connectivity between the variables (1) *quantified* or (2) *non-quantified*?

P4. Cyclic relationships: Does the model allow for (1) *cyclic connectivity* (closed loops, circular feedback/recursion) between the variables, or is the model (2) *acyclic*.

P5. Type of connectivity: What is the nature of the connective relationships between variables? For instance, are they (1) *mathematical/functional*, (2) *probabilistic*, (3) *quasi-causal* (e.g. imply *influence only*), or (4) *non-causal* (e.g. logical, normative)?

Why have I chosen just these five properties? There are certainly other modelling properties that can be abstracted. First of all, because we have to start somewhere. But also, because these are some of the simplest operational properties that we can identify (note that P2, P3 and P4 are basic parameters in [mathematical] graph theory). A more complete morphology could certainly treat a wider range of modelling properties, including the distinctions between e.g. continuous/discrete (value ranges); deterministic/stochastic/agonistic (relationships); open/closed (modelling contexts); hierarchical/non-

hierarchical (variable structure); and whether or not self-reference, whole-part interactions or so-called *strong-coupling*, is accounted for.

The addition of these or other properties would not necessarily increase the number of possible modelling types drastically. This is because these additional properties are not necessarily mutually orthogonal to the original five, or among themselves. They represent partially overlapping concepts. However, the need for *orthogonality* is not an issue here. We are examining a number of acknowledged modelling properties, and if these properties contain overlapping concepts, then this should be seen as part of the enquiry. Indeed, one of the advantages of General Morphological Analysis is that it liberates us from “enslavement to orthogonality”.

3. A morphology of modelling types*

The five selected modelling properties make up the parameters of the morphological field in Figure 1. This field contains 64 (2 x 2 x 2 x 2 x 4) “cases” or *configurations*, each represented by one condition selected under each of the parameters (as in Figure 2, representing a modelling type which includes Bayesian Network models). As stated above, these properties do not necessarily represent *orthogonal relationships*, as some of them are partially overlapping and contain properties that are logically contradictory. A *cross-consistency assessment* (CCA) of these properties will allow us to identify and weed out such contradictions, thus removing from the problem space any “configurations” which contain incompatible properties.

Specification (of variables)	Directionality (of relationships between variables)	Quantification (of relationships between variables)	Cyclic relationships (between variables)	Type of connectivity (of the relationships between variables):
Specified value range	Directed	Quantified	Cyclic	Causal: mathematical-functional
Non-specified value range ("Black boxes")	Not directed	Non-quantified	Acyclic	Causal: probabilistic
				Quasi-causal (influence)
				Non-causal (E.g. logical/normative)

Figure 1: Morphological field consisting of five modelling properties

* For a detailed description of General Morphological Analysis see: <http://www.swemorph.com/pdf/gma.pdf>.

Specification (of variables)	Directionality (of relationships between variables)	Quantification (of relationships between variables)	Cyclic relationships (between variables)	Type of connectivity (of the relationships between variables):
Specified value range	Directed	Quantified	Cyclic	Causal: mathematical-functional
Non-specified value range ("Black boxes")	Not directed	Non-quantified	Acyclic	Causal: probabilistic
				Quasi-causal (influence)
				Non-causal (E.g. logical/normative)

Figure 2. The properties of a Bayesian Network model.

Out of necessity, I carried out this CCA *solo*. This is not the way it should be. Such an assessment should be done in a group setting with modelling specialists representing different competencies and traditions, and where dialogue can be established. In fact, the whole enterprise of specifying modelling properties, and “typing” the models, should be done in such a setting, in order to bring about an adequate discussion. This is basic to the morphological method.

Be this as it may, the (solo-) assessed CCA is shown in Figure 3. I chose to be extremely tolerant at this point, and only dismissed those combinations of conditions which represent blatant (logical) contradictions – i.e. “contradictions in terms” (marked by “X”). I allowed all the rest to survive – even those pairs of conditions that I couldn’t get my head around or which seemed, at least at first glance, to be “strange”. Thus we are following the anti-totalitarian principle: everything that is not expressly forbidden is allowed. It will be interesting to see if the next – Leibnizian – step is realized: “everything that is not impossible is actual”.

The deleted pairs of conditions concern the following:

Models employing mathematical functions as their connection properties must, by definition 1) have internally specified variables, 2) be quantified and 3) have (functional) direction. The 4th contradiction concerns the fact that models based on probabilistic connections are – by definition – quantified.

These four pair-wise exclusions reduce the problem space from 64 possible modelling types to 42 (a larger number of possible modelling combinations than I had originally expected). Thus, beyond these four analytical contradictions (unless I have missed something – which certainly wouldn’t surprise me), everything else is, at least, possible. One might suspect that some of these possible theoretical modelling types would be of marginal utility or just plain weird. However, as we all know, “weird” things can sometimes lead to interesting discoveries.

All of this can, and should, be questioned (for instance, the number and choice of the five “modelling properties” put forward here). The dream scenario would be to bring half-a-dozen (open-minded, curious) modelling specialists – all with general modelling interests but different modelling-area specialities – into a facilitated GMA workshop in order to build this “upper domain ontology” concerning modelling methods. If someone, or some organisation, is willing to sponsor/finance such an effort, I will donate my time and effort to organise and run such a workshop.

		Intern		Conne		Model		Cyclic	
		Variables internally specified	Variables non-specified	Directed	Not directed	Model is quantified	Model is non-quantified	Cyclic	Acyclic
Connections between the variables:	Directed	-	-						
	Not directed	-	-						
Model quantification	Model is quantified	-	-	-	-				
	Model is non-quantified	-	-	-	-				
Cyclic feedback/ recursion	Cyclic	-	-	-	-	-	-		
	Acyclic	-	-	-	-	-	-		
Mode of connectedness between variables	Causal: math-funct.	-	X	-	X	-	X	-	-
	Causal: probabilistic	-	-	-	-	-	X	-	-
	Quasi-causal (influence)	-	-	-	-	-	-	-	-
	Non-causal	-	-	-	-	-	-	-	-

Figure 3: The Cross-Consistency Assessment of the field for Figure 1.

4. Examples modelling types

As a first test of this (meta-) model, we can plug into it a number of more or less well established or recognized modelling types. As far as I know, there is no generally agreed upon nomenclature for all of these modelling schemes. Some are clear enough – for instance Bayesian Networks – whereas others may be contested. Still others may denote rather vague categories of models such as “influence diagrams”. The names that I chosen here are not prescriptive. If better names are available, bring them on. (One of the main purposes – and results – of morphological modelling is to develop such a common “domain terminology”.)

The eight modelling methods presented (with their graphical representations) are:

1. **Morphological models** (The meta-model contains itself as a specific case.)
2. **System Dynamic Models**
3. **Bayesian Network models**
4. **Weighted influence diagram**
5. **Un-weighted influence diagram**
6. **Non-directed, non-quantified graph**
7. **Probabilistic Decision Tree**
8. **Un-weighted Analytic Hierarchy**

The remaining 34 modelling types are listed in Table 1, along with their formal properties.

4.1. Morphological models

Specification of values ranges	Directional connections	Quantification of connections	Cyclic connections	Type of connectivity
Yes	No	No	Yes	Non-causal

Variable specification	Connections between variables	Model quantification	Cyclic feedback	Nature of the connections between variables
Variables are internally specified	Directed	Model is quantified	Cyclic	Causal: mathematical-functional
Variables not internally specified ("black boxes")	Not directed	Model is non-quantified	Acyclic	Causal: probabilistic
				Quasi-causal (influence)
				Non-causal (E.g. logical/normative)

Figure 4.1: A morphological model showing the properties of morphological models.

4.2 System Dynamic Models

Specification of values ranges	Directional connections	Quantification of connections	Cyclic connections	Type of connectivity
Yes	Yes	Yes	Yes	Math/functional

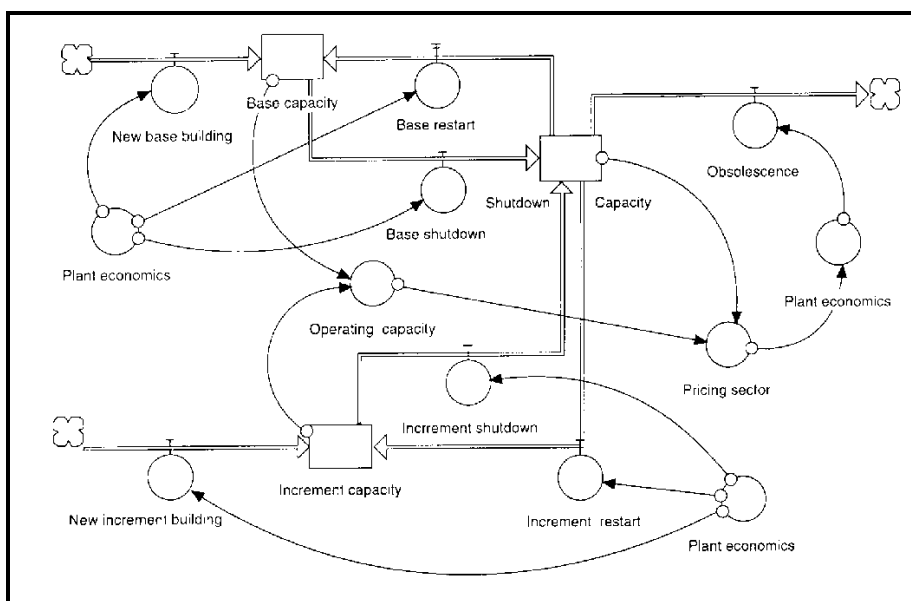


Figure 4.2: Mathematical/functional model with the causal, cyclical variable relationships (as depicted in software PowerSim™).

4.3 Bayesian network models

Specification of values ranges	Directional connections	Quantification of connections	Cyclic connections	Type of connectivity
Yes	Yes	Yes	No	Probabilistic

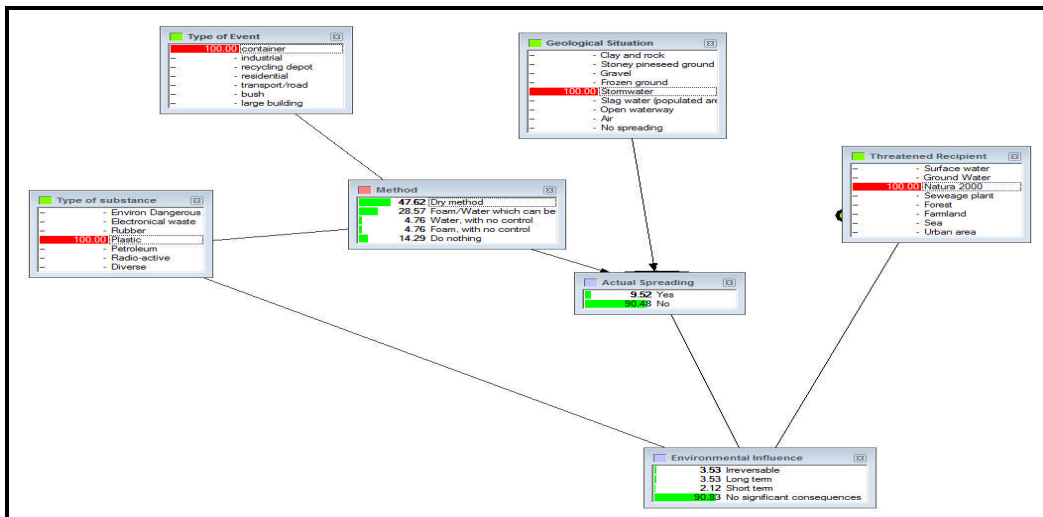


Figure 4.3: Bayesian network model (as depicted in software by Hugin™.)

4.4 Weighted influence diagrams (aka Vester diagram; weighted, directed graph)

Specification of values ranges	Directional connections	Quantification of connections	Cyclic connections	Type of connectivity
No	Yes	Yes	Yes	Quasi-causal

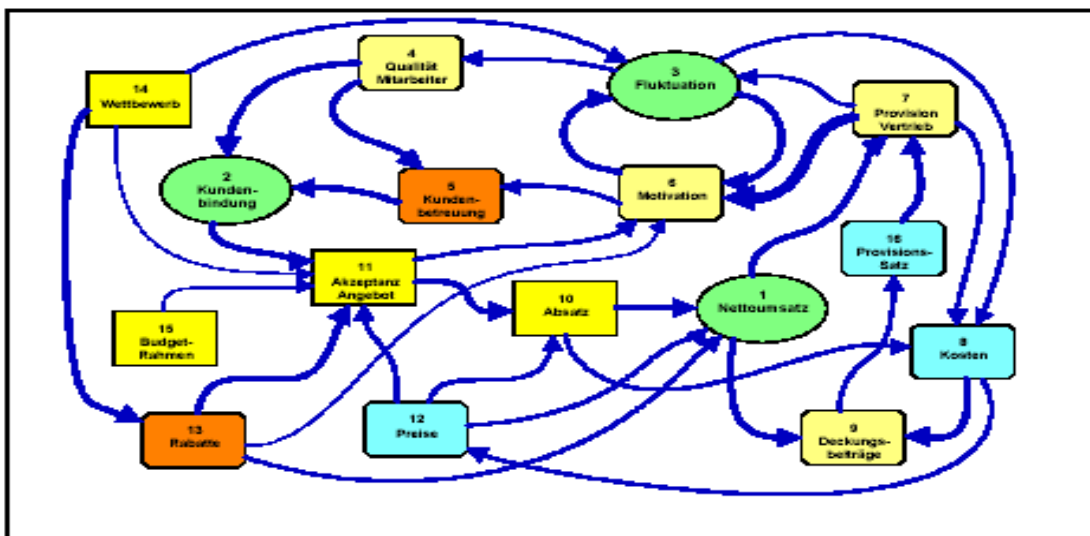


Figure 4.4: Weighted quasi-causal graph with cyclic structure (as depicted in software Gamma™.) Note: the blue connecting arrows are weighted.

4.5 Non-quantified influence diagram (Non-quantified, directed graph)

Specification of values ranges	Directional connections	Quantification of connections	Cyclic connections	Type of connectivity
No	Yes	No (+/-)	Yes	Quasi-causal

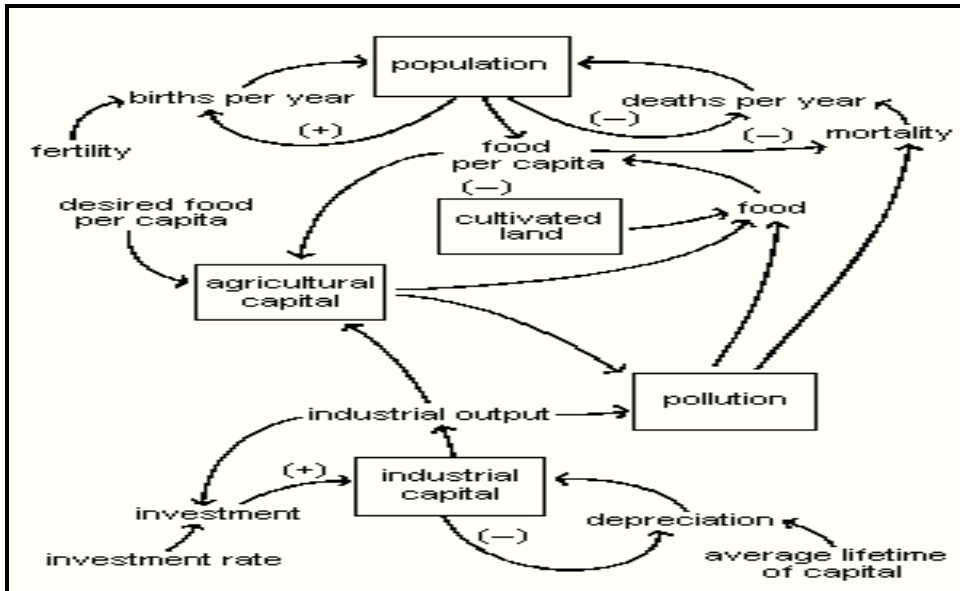


Figure 4.5: Classical, non-quantified "influence diagram" (positive and negative influence depicted)

4.6 Non-directed (symmetric), cyclic, non-quantified graph

Specification of values ranges	Directional connections	Quantification of connections	Cyclic connections	Type of connectivity
No	No	No	Yes	Quasi-causal

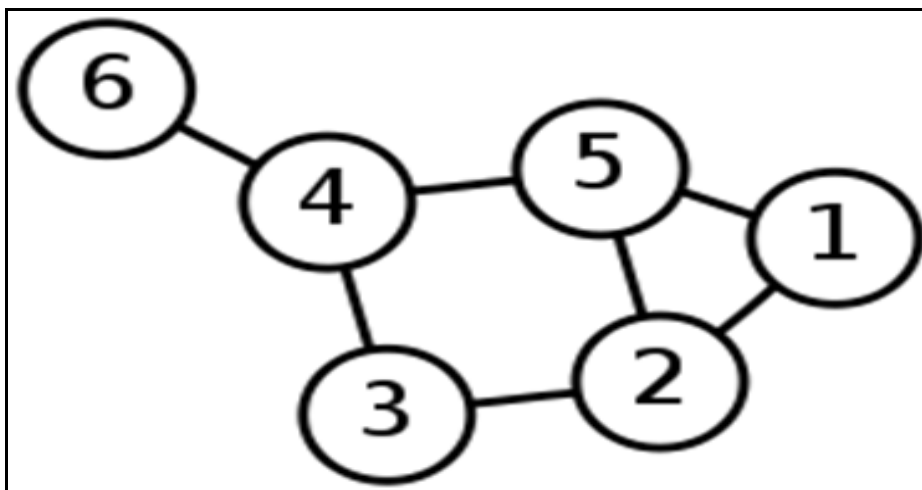


Figure 4.6: Non-quantified, undirected (symmetric) graph (aka "police diagram")

4.7 Probabilistic Decision Tree

Specification of values ranges	Directional connections	Quantification of connections	Cyclic connections	Type of connectivity
No	Yes	Yes	No	Probabilistic

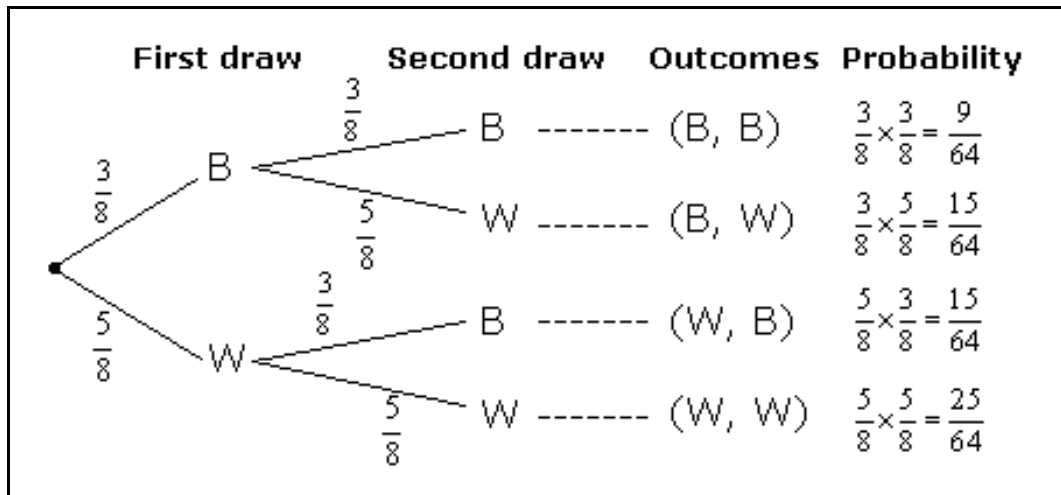


Figure 4.7. Probabilistic decision tree

4.8 Un-weighted Analytic Hierarchy

Specification of values ranges	Directional connections	Quantification of connections	Cyclic connections	Type of connectivity
No	Yes	No	No	Non-causal

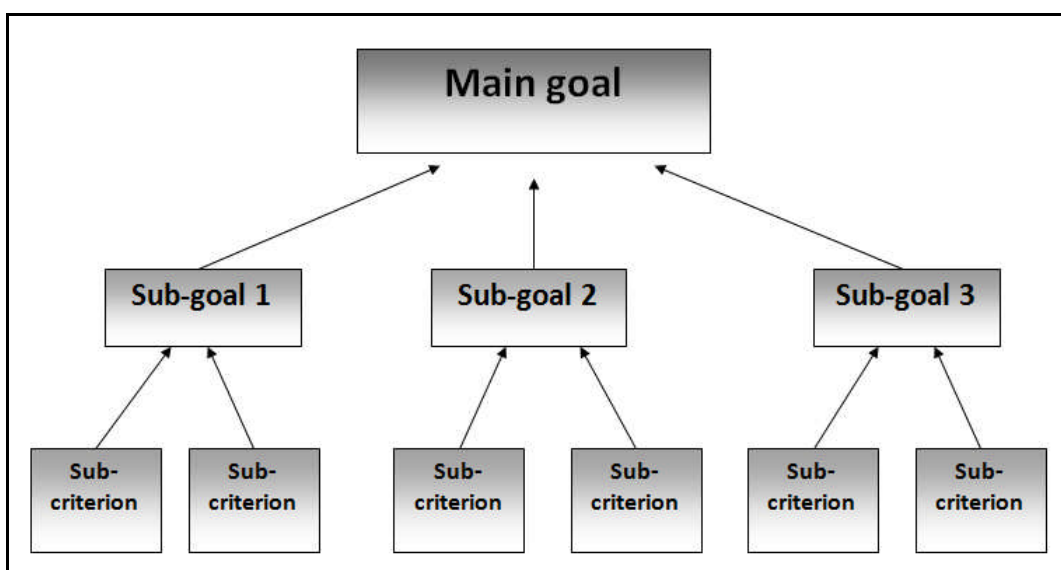


Figure 4.8 Un-weighted Analytic Hierarchy

5. Table of modelling types

#	Properties (modelling Parameter) – > Modelling types	Internal specification of variables	Directionality of relationships	Quantification of relationships	Cyclic?	Type of connectivity
1	Morphological models (GMA)	yes	no	no	yes	Non-causal
2	System Dynamic Modelling (SDM)	yes	yes	yes	yes	Mathematical
3	Bayesian Network modelling (BNM)	yes	yes	yes	no	Probabilistic
4	Weighted influence diagrams	no	yes	yes	yes	Quasi-causal
5	Non-quantified influence diagrams	no	yes	no	yes	Quasi-causal
6	Un-directed, non-quantified graph	no	no	no	yes	Non-causal
7	Probabilistic Tree Diagram	no	yes	yes	no	Probabilistic
8	Un-Weighted Analytical Hierarchy	no	yes	no	no	Non-causal
9		yes	yes	yes	no	Mathematical
10		no	yes	yes	no	Quasi-causal
11		yes	yes	yes	yes	Quasi-causal
12		yes	yes	no	yes	Quasi-causal
13		no	no	no	yes	Quasi-causal
14		yes	yes	yes	no	Quasi-causal
15		no	yes	no	no	Quasi-causal
16		yes	yes	no	yes	Quasi-causal
17		no	yes	no	yes	Quasi-causal
18		no	no	yes	yes	Quasi-causal
19		yes	no	no	yes	Quasi-causal
20		yes	yes	no	no	Quasi-causal
21		yes	no	yes	yes	Quasi-causal
22		no	no	yes	no	Quasi-causal
23		no	no	no	no	Quasi-causal
24		yes	no	no	no	Quasi-causal
25		yes	no	yes	no	Quasi-causal
26		yes	yes	yes	yes	Non-causal
27		no	yes	yes	yes	Non-causal
28		yes	no	yes	yes	Non-causal
29		no	no	yes	yes	Non-causal
30		yes	yes	yes	no	Non-causal
31		no	yes	yes	no	Non-causal
32		yes	yes	no	no	Non-causal
33		yes	no	no	no	Non-causal
34		no	no	no	no	Non-causal
35		yes	no	yes	no	Non-causal
36		no	no	yes	no	Non-causal
37		yes	yes	yes	yes	Probabilistic
38		no	yes	yes	yes	Probabilistic
39		no	no	yes	yes	Probabilistic
40		yes	no	yes	no	Probabilistic
41		no	no	yes	no	Probabilistic
42		yes	no	yes	yes	Probabilistic

Table 1. 42 modelling types classified by way of 5 properties. Modelling types 9-42 are sorted by “type of connectivity”.

6. Notes on the requirements for the successful application of different modeling methods

Means of representation

All models must have a means of representing the things that they are modelling – represented here by what I have called *operational properties*. The specification of a set of such properties is a two edged sword: On the one hand, it provides a framework for describing and classifying (i.e. “morphing”) variations in modelling types; on the other hand, it represents *constraints* placed on the model *and* the associated demands it places on the quality of the empirical information available concerning the modelling *object*. [Note: What modelling theory refers to as the “object”, the “original” or the “target” of the modelling process is not, of course, some “thing in itself” (the mythical *ding an sich*). These “objects” are themselves “models”: they are created out of our perceptions, experience and “under-standings” (i.e. hypo-theses) about the world. A model is a model of a model.]

The extent to which a specific modelling method *meets the requirements for its successful application vis à vis* a specific modelling object depends on the nature of the empirical information available concerning the modelling object, and how this “matches” the properties of the modelling method. If you think that this statement is a tautological no-brainer, then there is a lot of modeling activity going on with no brains.

There are two ways to think of this “mismatch” between modelling method and modelling object.

1) **When certain fundamental properties of the modelling object *outpace* (so to speak) the properties of a chosen modelling method.**

When there is evidence that there are *fundamental properties* that can be attributed to the modelling object that will not fit into – is not represented in – the modelling method, then any attempt to accommodate the model by ignoring these properties will result not only in short-changing the “object”: it guts its very foundation. Of course, we all know that models are “simplifications”, and represent “reality” only imperfectly. But this is not the point of contention. Contention comes into play when such a simplification discounts some *basic principle governing the existence of the object*. This is a *fatal simplification* – the point of Poincaré’s argument about ontological assumptions. In this case, we need to expand the methodological domain of the modelling method to accommodate the “ontological domain” of the modelling object. The principle of “emergent qualities” (which Agassi mentioned) is a point in case.

2) **When the properties of a chosen modelling method *out-pace* the quality of the information available concerning the modelling object.**

In this case, any attempted completion of this information by qualitatively “stretching” it to accommodate the model, or by imposing *ad hoc* teleologies, will give results that risk being completely arbitrary.

Let us begin with point (2). This was the basis of Bernhard Riemann’s critique of Herman Helmholtz’ theory of the *mechanism of the ear*.

Analysis and synthesis

Mathematician Bernhard Riemann (1826-1866) wrote his last (unfinished) work – *The Mechanism of the Ear* – during the spring of 1866 when he was 39 years old, and dying of tuberculosis. The unfinished manuscript was published by his friends and colleagues Ernst Weber and Gustav Fechner shortly after Riemann’s death.

The Mechanism of the Ear was written in response to the publication of Hermann Helmholtz' "On the Sensations of Tone as a Physiological Basis for the Theory of Music" (1863). Helmholtz was a leading proponent of the synthetic approach to physiology. Essentially, this approach is based upon the assumption that the study of biology can be reduced to methods based entirely upon classical (Newtonian) mechanics. On this foundation, Helmholtz developed a theory to describe the mechanism of the human ear based careful anatomical studies and by applying the laws of acoustics as they were understood at the time. He then attempted to apply this theory to the principles of musical composition, harmony and counterpoint.

There is much in Helmholtz' study of the physiology of the ear which has been of lasting value, especially his careful *anatomical investigations*. However, the *theory* he formulated concerning the mechanism of the ear – and its premature application to the theory of musical composition – was seriously flawed.

Why, of all people, would Bernhard Riemann – the greatest mathematician of his time (some say of any time) – devote *his* time to the enterprise of developing an alternative theory concerning the mechanism of the ear? It concerned an issue of method. In the introduction "On the Method which is Applicable to the Study of the Physiology of the Finer Sense Organs", we see that Riemann is using this specific "case study" in order to discuss the wider applications of the analytic and the synthetic methods for scientific investigation in general. In the first 13 paragraphs of this introduction, Riemann presents an astonishing, exquisitely concise *analysis of the analytic and synthetic methods*. For a detailed presentation, see Ritchey (1991).

There are two ways of defining *analysis* and *synthesis* as basic scientific methods of approach: as a *compositional concept* and as a *procedural concept*. (We are not concerned here with the everyday use of the term "analysis" simply as a connotation for "investigate" or "study".) Although the procedural concept is the more fundamental and operational, it is, unfortunately, the compositional concept that is most widely touted. As a compositional concept, *analysis* is defined as the procedure by which we break down an intellectual or substantial whole into parts or components. *Synthesis* is defined as the opposite procedure: to combine separate elements or components in order to form a coherent whole.

Careless interpretation of these definitions has sometimes led to bizarre ideas – for instance, that synthesis is "good" because it creates wholes, whereas analysis is "bad" because it reduces wholes to alienated parts. According to this view, the analytic method is regarded as belonging to reductionist tradition in science, while synthesis is seen as leading the "new way" to a holistic perspective. An example of such silliness is to be found in Ackoff's *Redesigning the Future* (1974):

"Machine Age thinking was analytical and based on the doctrines of reductionism and mechanism". ... Analytical thinking is a natural complement to the doctrine of reductionism. It is the mental process by which anything to be examined, hence understood, is broken down into parts." (p. 8f. – my emphasis.)

Ackoff meant well: he was trying to get the message across that mechanistic, reductionist approaches to the *study of social systems and social planning problems* represent bad (social) science. However, quite aside from the fact that it is the *synthetic method*, not the analytic method, which is methodologically associated with a reductionist approach to scientific inquiry, such a (mis-) interpretation completely misses the point of the relationship between these two methods.

Analysis and synthesis, as scientific methods, always go hand in hand; they complement one another. Every synthesis is built upon the results of a preceding analysis, and every analysis requires a subsequent synthesis in order to verify and correct its results. In this context, to regard one method as being inherently better than the other is meaningless – tantamount to asking whether it is better to breathe in or to breathe out.

The problem with this “compositional misrepresentation” dissolves when we look at the classical, procedural notion of analysis and synthesis. Riemann clears up the compositional-procedural interplay at the beginning of his introduction (here he is referring to the “organ” as a “system” to be explained):

“... there are two possible ways of gaining knowledge about the organ's [i.e. a system's] functions. Either we can proceed from its construction, and from there seek to determine the laws of the mutual interaction of its parts as well as its response to external stimuli; or we can begin with what the organ accomplishes and then attempt to account for this. By the first route we infer *effects from given causes*, whereas by the second route we seek *causes of given effects*. Following Newton and Herbart, we can call the first route *synthetic*, and the second *analytic*.

Riemann uses the classical, procedural definition of the concepts of analysis and synthesis, which is expressed in terms of *cause* and *effect*. We can easily see how the cause-effect relationship is translated into the compositional relationship. A system's internal processes – i.e. the interactions between its components – are regarded as the *cause* of what the system *as a whole, or a unit*, performs. What the system performs is thus the *effect*. From these very relationships we can immediately recognize the requirements for the application of the analytic and synthetic methods.

The synthetic approach -- i.e. *to infer effects on the basis of given causes* -- is therefore appropriate when the laws and principles governing a system's internal processes are known, but when we lack a detailed picture of how the system behaves (or will behave) as a whole.

The analytical approach -- *drawing conclusions about causes on the basis of effects* -- is appropriate when a system's overall behavior is known, but when we do not have clear or certain knowledge about the system's internal processes or the principles governing these.

System levels

Thus this procedural definition defines two relative “system levels” (and I now take the liberty of using the term “system” as reference to a modeling “object”): one from the point of view of the system's internal components and interactions (its physiology, so to speak); and one from the point of view of its behavior as a whole, i.e. what it accomplishes, its purpose or goal (depending on what type of system we are talking about). When we have (relatively) “clear and certain knowledge” about one of these system levels there is no problem: we choose the appropriate method of attack.

However, when we work with very complex objects of enquiry (e.g. social/organizational/political systems) we often neither have a clear nor complete conception of how such a system will behave as a whole, nor fully understand all the principles and (functional) interactions at work which cause that behavior. In such cases, we must ask the important question as to which method can be regarded as more suitable as the *primary method or chief point of departure*. Riemann's discussion of this issue in *The Mechanism of the Ear* relates importantly to how we use – and misuse – models and modelling.

We can approach this issue by considering two (ideal) reasons for building models: 1) to better *understand and explain existing systems* on the basis of how they work and what they do; and 2) to *conceptualise and design new systems* on the basis of what they should do and how best to do it. Of course, these two contrasting tasks correspond only *a priori* to real-world modeling. In practice the two tasks often overlap: i.e. we attempt to understand an existing system or process in order to find ways to improve it – to re-form or re-design it. But we can use these two idealised tasks to discuss the roles of the analytic and synthetic methods in developing models.

This is easy in the case of conceptualising, designing and developing something new. When we wish to design a new system (a plant, a process, an organisation, or a policy) we seldom start out by looking at its possible components and internal interactions. We begin, instead, by asking about its *pur-*

pose; its *goal*; what we what it to *accomplish*. On the basis of this, we *analyse* such an overreaching goal by “breaking it down” into sub-goals which we deem to be necessary requirements for fulfilling the overall goal. We then continue to break these criteria down into sub-criteria, and so forth. We stop this process when we reach the point where we are no longer defining criteria for fulfilling goal-requirements, but find ourselves starting to consider how these criteria can actually be implemented *compositionally* – i.e. the system’s actual physical or organisational construction. This latter question is the beginning of the synthesis phase.

What is being formulated in this process is called an *analytic hierarchy*, which is exactly what Thomas Saaty’s Analytical Hierarchy Process is about (AHP being is one of a number of related multi-criteria decision support methods).

If the analytic approach is most appropriate as the *chief point of departure* for the modelling/design of a new system, then one might imagine that the synthetic approach would best fill this role for explaining, modelling and simulating an already existing system. After all, such a system has an existing structure (composition) that one can examine in order to determine the laws of the mutual interaction of its parts, and thus to build a model that simulates its “physiology”. However, this depends entirely on the nature of the system that is the object of the modelling.

Helmholtz application of the synthetic approach – as the chief point of departure – to the study of the ear is a revealing example. Due to the limits of observation at the time, the empirical information he was able to collate did not suffice to explain some of the known psychophysical qualities of the ear (for instance the ear’s enormous sensitivity and the production of overtones). He was thus led to supplement this lack of empirical (compositional) evidence by formulating a number of *ad hoc* hypotheses in order to complete the explanation, and thus formulate his theory (or “model”).

In his examination of Helmholtz’ method, Riemann points out that the synthetic method *could* very well be used successfully with the finer sense organs – as it had successfully been used with regard to the physiology of the (larger) organs of locomotion – provided that the physical characteristics of the individual parts of the sense organ could indeed be determined with requisite precision or certainty. But this was not possible at the time – and is still a great challenge today.

To make a long story short, Riemann turned Helmholtz’ study on it head: he started from a top-down analytical approach in order to “re-conceptualise” the ear *on the basis of what it accomplishes*. From this perspective, he could then draw certain conclusions about what a theory of the mechanism of the ear *must account for*, and *how to test it*:

To test our explanation by reference to experience, we can in part draw upon what we have concluded about what the organ accomplishes, and in part upon what that explanation *presupposes as to the physical characteristics of the organ’s constituent parts*. (Riemann, 1953, p. 340. My emphasis)

He then points out that such conclusions can have universal scope, and can “give rise to advances in our knowledge of the laws of nature” -- as was the case, for example, with Euler’s efforts to account for achromatism of the eye.* Here we are using the *modeling process* in order to tell us – or suggest – something of “added value” about the “reality” we are attempting to model. This is an example of the *epistemic role* of modeling.

* In fact, it is only very recently that the principles that Riemann was asking about have been explained, completely supporting his methodological approach. Reichenbach & Hudspeth (2010, 2011) have finally found the mechanisms at work which can explain the ear’s enormous sensitivity – the fact that it has an amplification gain of 1:4000. The authors are now in the process of “synthesising” these principles in the form of a new type of amplifier.

Modelling self-referential systems

If the ear is difficult to model because of its microscopic-biological elusiveness, it is at least a (relatively) well-circumscribed, closed system that we can observe from the outside and which can be manipulated and tested in a (more or less) controlled manner. What about modelling *social systems*, which are open systems, of which we are (in many cases) a part, which cannot so easily be manipulated (in a controlled manner, that is), and which have a very special elusive quality that goes beyond any form of determined *mechanism*: i.e. the property of self-referential causality?

This clearly represents a case where a fundamental property of the modelling object would seem to “go beyond” the properties of any of the modelling methods presented in this study. The social/political/cognitive domain involves causative actions on society as a result of human reflexive self-reference (Bermúdez, 2000). Such “cognitive feedback” (not to be confused with how this term is sometimes used in psychology) affects interaction between macro and micro domains in the system, aka “whole-part” causality or “strong coupling”.

For modelling theory, one of the ways that this is expressed is in deliberate human actions which are motivated *by knowledge of the very model itself*. Since such (meta-) actions subsume the model, the model can never predict them. Any attempt to assimilate such meta-actions or meta-actors into a new, more inclusive model will simply result in a new, more “exclusive” level of possible meta-actions. This was one of the main points brought up by Rittel (1972) concerning the failure of what he called “first generation systems analysis” as applied to social planning problems. It was (and still is) a real problem concerning stock market instability, where sophisticated models and scheming humans try to “out-meta” each other.

The only modelling method presented here which *sometimes* attempts to (functionally) causally model social-cognitive behaviour is System Dynamic Modelling (SDM). It is, so to speak, at the top of the food chain in our morphology. SDM was originally developed, and is applicable to, Substance Flow Analysis (SFA) and associated processes, where the parametric relationships between the components of the system are relatively well-defined and stable, and where the system is treated as closed. As a predictive model, SDM is less applicable for open-systems and where the parametric relationships between variables are not known with any precision, or where they are changing rapidly in uncertain ways. Representing causation based on human motivations is not only out of the question for SDM, but lies outside our present modelling “metaphysics”.

All of this has been thrashed about earlier, at least since Horst Rittel (1972) put forward his epistemological critique of the use of “systems analysis” for the purpose of modelling social and policy driven systems. His uncut gem of a paper – with its concept of “wicked problems” – is still one of the most enlightening discourses available. In this context, it may be that a General Theory of Modelling will more likely be driven from a *policy modelling* and *design theoretical* perspective, than from the “hard school”. In any event, it will have to be approached from “outside” our present ontological framework.

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