Systems Research Vol. 5, No. 2, pp 171–177, 1988.
Printed in Great Britain.

0731-7239/88 $3.00 + 0.00
Pergamon Press plc
© 1988 International Federation for Systems Research

Correspondence

Theory Versus Model: a Comment on 'Toward a Consistent Terminology for Management Theory Building' by William Acar

M. JOSEPH SIRGY

Virginia Polytechnic Institute and State University, Blacksburg, VA 24061, U.S.A.

(*Received* 14 *September* 1987)

I READ Acar's article in *Systems Research* [4 (1987), 119–125] with great interest since I share Acar's need for a more standard way of using research terminology, terminology such as methodology, paradigm, approach, framework, method, and theory. I do applaud Acar's effort in trying to sort out and provide some standard meaning to these terms commonly used in scientific research. I do share his views in making distinctions between methodology and method among paradigm, approach, and framework, between method and technique, and between method and procedure. However, the distinction between theory and model seems fraught with ambiguity and bias. Acar's use of the term model is strongly biased by how a few management scientists and operation researchers use the term, as in computer models. He seems to imply that models can be construed as indirect form of inquiry, while theory is a different 'animal' altogether. A theory comprises of hypotheses, theorems, laws, etc. I agree that scientists use these two terms quite interchangeably and often inappropriately. However, I should point out that the inappropriate usage of the terms is not strictly due to the fact that models and theories are very different animals, but mainly because different scientists tend to have different interests. I have noted over the years that basic scientists (e.g. physicists, chemists, biologists, psychologist, sociologists) use the term '*theory*' much more often than the term 'model', whereas applied scientists (e.g. mechanical engineers, computer engineers, electrical engineers, management scientists, marketing scientists) use the term '*model*' much more often than the term 'theory'. And both sets of scientists use the terms 'theory' and 'model' to mean basically the same thing. They use a 'model' or a 'theory' to mean *explanation*, *prediction*, and/or *control* of some phenomenon. In other words, scientists (basic and applied) construct theories and models to explain, predict, and/or control some aspect of reality. Basic scientists are traditionally interested more in explanation and possibly prediction, but least interested in control. Applied scientists, on the other hand, are most interested in prediction and control, but quite often least interested in explanation. Engineers and business scientists tell us over and over again that they are interested in the things that work. They are pragmatic, and do not hesitate to adopt 'models' that work (work being defined ultimately by prediction and control). *Explanation* to engineers and business managers is 'extra'. They can do without it. Prediction and especially control of the phenomenon under study are most important.

If we accept the possibility that basic scientists use the term 'theory' while applied scientists use the term 'model' (which is simply based on my own personal observation having worked both as an experimental/social psychologist and a marketing researcher—basic and applied social/behavioral science) then we may infer the following distinction between model and theory. A *model* is a conceptual tool that postulates a set of relationships between/among concepts for the main purpose of controlling a given phenomenon from which prediction serves the controlling function. A *theory* on the other hand is a conceptual tool that postulates a set of relationships between/among concepts for the main purpose of explaining a given phenomenon from which prediction falls out of the explanatory function (see Table 1).

Table 1. Distinction between 'theory' and 'model'

	Theory	Model
Use by scientists	Most commonly used by basic scientists	Most commonly used by applied scientists
Purpose and goal	*Explanation*, followed by prediction, and control, respectively	*Control*, followed by prediction, and explanation, respectively

Theory Versus Model: Further Comments on 'Toward a Consistent Terminology for Management Theory Building' and Rejoinder to Sirgy

WILLIAM ACAR

Graduate School of Management, Kent State University, Kent, OH 44242, U.S.A.

(*Received* 4 *November* 1987)

I AM pleased to see that my effort toward building a research terminology for the management sciences, as expressed in my recent article [1], is generating a healthy dialogue on the subject. I am also gratified by Sirgy's response, and his acceptance of the distinctions I have attempted to establish between methodology and method, among paradigm, approach and framework, and among method, technique and procedure. The scheme I propose appears reasonable, and attempts to balance two of the most important requirements of a definitional system manifest in methodological treatises such as Ackoff's [2]. I interpret these requirements as expressing the need for balancing consistency with traditional or 'core' meanings on the one hand, and definitional clarity and usefulness to the definer on the other. It is for this reason that both Kuhn [4] and I appear to be temporarily willing to live with the anomaly of the word paradigm possessing both a global sociological meaning (as in the 'Western paradigm', which is a short encapsulation of the Mediterranean and European historical emphasis on progress through science), and a more restricted methodological meaning (such as in the 'OR/MS paradigm' which is a part of the scientific methodology).

However, there are good reasons as well as precedents for distinguishing technique, method and methodology. Moreover, there is no clearly unique fit of the notions of theory and model at some specified level of the hierarchy of the concepts of methodology, paradigm, approach, framework, method and technique. I argue that the notions of model and theory are not part and parcel of the methodological hierarchy, but that each term brings in an additional perspective: in traditional usage, theorizing is contrasted with using tools on a black-box basis, and modelling is the device of analysing a situation by means of a symbolic representation in contrast to directly.

I welcome Sirgy's letter because it reopens the neglected issue of the dynamics of modelling and theory building. Although there is no one-to-one correspondence between the notions of model (or theory) and any one of the levels of the methodological hierarchy, it is however the case in managerial research that one finds models at lower levels than theories. In the natural and social sciences, some researchers and educators still use the term theory in its traditional meaning to denote a mere hypothesis. On the whole, the term has gradually shifted to a set of hypotheses and has now come to denote a much larger conceptual construct, one which includes hypotheses, models as well as hard results—propositions or theorems. For instance, in his treatise on the scientific methodology, Ackoff [2, p. 22] sets out to clarify the notions of theory and law by defining a 'deductive system' as comprising: a set of undefined and defined concepts, a set of assumptions and transformation rules, a set of *deduced* (emphasis mine) theorems, and instances of application of them. He connects with traditional usage by proposing that:

> The assumptions constitute the theory, the theorems constitute laws, and the instances of the theorems are the facts.

In contemporary scientific usage, theory denotes the entire deductive system (as in 'probability theory' or in 'the theory of evolution') and not just its basic hypotheses or axiomatic foundations. I would then follow Ackoff's system in the following definition.

Theory: A deductive system comprising:

(i) A set of defined and undefined concepts denoted as the 'symbolism' or 'language' of the theory.
(ii) An assumptional or axiomatic foundation denoted as the 'postulates' or 'hypotheses' on which the theory rests.
(iii) A set of implicit or explicit formation or transformation rules called the 'logic' of the theory.
(iv) A set of deduced propositions called the 'theorems', 'laws' or 'results' of the theory.
(v) A set of instances of applications of the results or theorems known as the 'facts', 'consequences' or 'implications' of the theory.

In view of our progressive understanding, due to the evolution of physics, of the relativity of knowledge, the words theorem, proposition and results have been gradually replacing the word law in scientific usage; similarly, the word fact is gradually giving way to the terms consequences and implications. We now perceive very clearly what the Singerian philosophical school has forecast [3, 5]: the relativity of our knowledge base and the increased use we make of models. Both basic and applied research make an extensive use of the modelling activity.

It becomes essential for us to understand the concepts of model and theory, and it behoves us to capture their relationship. Sirgy is right to point out that scientists ought not to continue using these terms interchangeably. He perceptively attributes the overlap in usage to a conceptual overlap; and is again correct in pointing out that basic scientists tend to use the term theory more frequently than the word model, while the converse holds true for applied scientists, engineers and operational researchers.

The question is to discover why. Sirgy notes that both basic and applied scientists attempt to explain and control reality, but with a difference: he views basic scientists as more interested in explanation and possibly prediction but less interested in control, while he deems applied scientists to be focused mainly on prediction and control. He then offers contrasted definitions of model and theory correctly stating that:

(a) in both model and theory, we postulate a set of relationships among concepts;
(b) however, in a model prediction serves the controlling function while, in a theory, prediction falls out of the explanatory function.

One has to agree with these properties. However, my sense is that they are *incidental* to the process. They are due to the fact that operational researchers deal with specific situations and the here-and-now; in this sense, OR/MS modelling aims primarily at prediction and control. However, basic scientists such as physicists also use models. To them, models allow us to grasp reality by analysing mathematical, therefore analysable structures. This is a general feature: the aim of symbolic modelling is to give us a better grasp of reality by allowing us to represent it by means of more readily understood or more analysable artifacts. This requirement is more crucial than sheer realism, and is all the more crucial when the analyst envisions some generalization down the road. Paradoxically enough, what is a model but an embryonic theory? And what is a theory but a generalized full-term model? We have here some interesting dynamics.

For the sake of the clarity of exposition, I left those dynamics out of my first terminology paper. I simply reinforced the 'core' view that modelling is antithetical to direct inquiry, and that theorizing is antithetical to black-box application. This allows us a clear starting point, but leaves out the issues of the possibility of 'direct' inquiry and that of the dynamics of model and theory. The first has been dealt with by the philosophy of science [2, 3]; the second is complex and is partially but not fully captured by Sirgy's typology. Definitions of *model* abound. Simply focusing on the mapping out of reality will not suffice for our present purpose. I like to think of a model as a situation-specific set of graphical or symbolic relationships. In a sense, a model has relationships, but may be embryonic as far as the other criteria which delineate a theory:

- its language is generally borrowed and is seldom tailor-made;
- its underlying assumptions may not yet have fully surfaced;
- its underlying logic may still be in the making;
- its propositions are still being explored and probably have not reached theorem status yet;
- not all its implications have yet been understood.

Because a model may be theoretically or methodologically incomplete along *each* of these dimensions, it initially differs a great deal from a full-fledged theory. But, in time, if these methodological requirements become increasingly more satisfied, the model tends closer and closer to full theory status. These dynamics, being contingent upon the purposes of the analysts, do not always take place, and the evolution of models toward theories does not always occur.

It is hoped that this correspondence will clarify these issues to the temporary satisfaction of systems researchers. It would even be better if, in the words of T. A. Cowan, it would contribute to generating a helpful dialogue on issues for which there cannot be a last word; the systems approach requires a dialectic of the various stakeholder communities [3].

REFERENCES

1. W. Acar, Toward a consistent terminology for management theory building. *Syst. Res.* 4 (1987), 119–125.
2. R. L. Ackoff, *Scientific Method.* John Wiley, New York (1962).
3. C. W. Churchman, *The Design of Inquiring Systems.* Basic Books, New York (1971).
4. T. S. Kuhn, *The Structure of Scientific Revolutions*, 2nd edn. University of Chicago Press, Chicago, IL (1970).
5. E. A. Singer (Edited by C. W. Churchman), *Experience and Reflection.* University of Pennsylvania Press, Philadelphia, PA (1959).

Two Hierarchical Dimensions of Research Concepts: Rejoinder to Acar's Response

M. JOSEPH SIRGY

(*Received* 23 *December* 1987)

ACAR'S response to my comment on his article 'Toward a Consistent Terminology for Management Theory Building' was well-received. Acar, in the original article, argued that models and theories may not be part of the methodological hierarchy; that theorizing is the converse of using tools on a black-box, and that modeling involves the use of symbolic representation to describe a situation or a phenomenon. I pointed out that models and theories denote the same thing, i.e. explain, predict, and/or control a given phenomenon. I also argued that basic scientists tend to use the term 'theory' more frequently than 'model', while the converse holds true for applied scientists. I attributed this phenomenon to the possibility that basic scientists are more interested in explanation (and possibly prediction) than control, whereas applied scientists are interested in prediction and control more so than explanation.

Acar seems to agree with my treatment of the terms theory versus model. He correctly points out that a model is an 'embryonic theory', and a theory is a 'generalized full-term model'. He advances a standardized use of the term theory using Ackoff's definition of theory and further distinguishes model from theory using a set of criteria. I do applaud this further treatment of the terms theory versus model in an effort to standardize research terminology. His reponse to my comment has further provoked additional thoughts that I would like to communicate in this comment article. Acar's response reinforced the notion that research concepts can be hierarchically arranged into *two* distinct hierarchies, a *methodological hierarchy* and a *substantive hierarchy*. The substantive hierarchy of research concepts refers to research concepts dealing with the explanation, prediction, and/or control of *natural* phenomena. The methodological hierarchy, on the other hand, deals with the development of research concepts involving *methods* that may be employed to investigate natural phenomena. Acar's original article dealt mostly with the methodological hierarchy, not with the substantive hierarchy. A graphical display of the two hierarchies is shown in Fig. 1.

I envision both substantive and methodological hierarchies as involving paradigms (superordinate level), theories (first-order subordinate level), and models (second-order subordinate level). This type of hierarchy can be viewed as varying from abstract to concrete concepts. Using Acar's new definition of theory and model, models therefore can be viewed as highly concrete (operational) theories. They are 'quasi' theories in which 'its propositions are still being explored and have not probably reached theorem status yet', 'its underlying assumptions may not yet have fully surfaced', etc. Models *mature* into theories having 'a set of defined and undefined concepts', 'postulates on which the theory rests', 'transformation rules', 'a set of deduced propositions', and a 'set of instances of applications'.

Theories *mature* into paradigms. For example, in psychology what used to be learning 'theory', cognitive 'theory', phenomenological 'theory', psychoanalytic 'theory', and humanistic 'theory' are *not* theories in the precise sense of Acoff's definition of theories. They involve a cluster of concepts theories, and models that are tied together with a set underlying of assumptions about what constitutes reality and the methods used to discover that reality.

Hence, along the abstract–concrete hierarchy of substantive research concepts (as well as the meth-

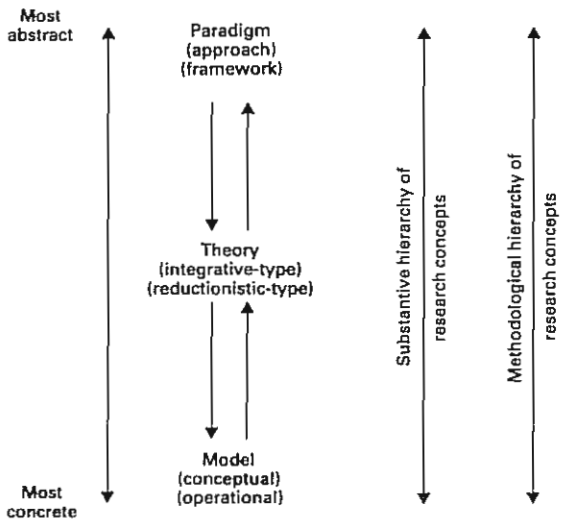

Fig. 1. Substantive and methodological hierarchies of research concepts.

odological hierarchy), scientists conduct research within certain theoretical paradigms involving one or more specific theories, from which specific models are formulated and tested. Within systems science, we can identify substantive paradigms and methodological paradigms. Examples of substantive paradigms include

- Open systems 'theory'
- Living systems 'theory'
- Systems ecology
- Functionalism
- Cybernetics and control 'theory'
- Psychocybernetics
- Sociocybernetics
- Field 'theory'
- Conflict 'theory'.

These are paradigms (cluster of theories, extensive program of research involving related concepts, principles, etc.). One can trace Parson's functionalism as a theory to its full exposition as a research paradigm in sociology and systems science.

With respect to the methodological hierarchy, we also have paradigms. Examples of methodological paradigms within systems science include

- Cost–benefit analysis
- Critical path methods
- Expert systems
- Fuzzy set 'theory'
- Game 'theory'
- System dynamics
- Input–output analysis
- Linear programming techniques
- Network 'theory'
- Non-linear modeling
- Optimization 'theory'
- Queueing 'theory'
- Reconstructability 'theory'.

Within a specific paradigm, researchers use the terms 'approach' and 'framework' interchangeably. And here Acar's definitions of approach and framework fit well in the context of both hierarchies (substantative and methodological).

At the *theory* level of either substantive or methodological hierarchy, we may note the distinction between integrative (or unifying) theories versus reductionistic-type theories. Integrative theories are theories designed to unify a variety of theories, models, concepts, relationships, etc. Many systems scientists who are interested in *general* system theory work closely with this type of theories. Examples of integrative theories include Miller's living system theory, Berrien's general system theory, Ackoff/Emery's purposeful system theory, etc. A recent example of an integrative theory involves my own work in psychocybernetics [1, 2]. Also, for those who are interested in a comprehensive discussion on the subject of constructing *general* system theories, please see [3].

Reductionistic-type theories, on the other hand, are theories that deal with a given phenomenon and have a few theoretical propositions. Again, we may construe that integrative-type theories are more abstract, while reductionistic-type theories are less abstract along the abstractness–concreteness hierarchy.

Models can be further broken down into at least two categories along the abstractness–concreteness dimensions. These are conceptual models (more abstract and less concrete) and operational models (less abstract and more concrete). Conceptual models are models that depict relationships between or among constructs. Relationships shown in a conceptual model are usually referred to as 'conceptual hypotheses', as opposed to 'operational hypotheses'. Operational hypotheses serve to 'operationalize' the conceptual hypotheses, i.e. show how the conceptual relationships specified in the model will translate in the context of the construct's measures.

In sum, Acar's response to my comment has spurred further ideas adding to Acar's ideas with respect to showing how research concepts are hierarchically interrelated. I am sure that Acar will join me in hoping that his article and our comments may spur additional thinking on this very important topic.

REFERENCES

1. M. J. Sirgy, *Self-Congruity: Toward a Theory of Personality and Cybernetics*. Praeger, New York (1986).
2. M. J. Sirgy, Toward a general systems theory of social behavior: a psychocybernetic perspective. *Syst. Res.* 4 (1987), 92–110.
3. M. J. Sirgy, Strategies for Developing General System Theories. *Behav. Sci.* 33 (1988), 25–37.

The Substantive and Methodological Hierarchies: Rejoinder to Sirgy's Responses

WILLIAM ACAR

(*Received* 22 *January* 1988)

I FIND it stimulating and rewarding to read Sirgy's second response to my effort toward establishing a hierarchy of research concepts [1]. His vigorous and creative response provides new food for thought and opens up new avenues for research.

Not only does Sirgy agree with my adaptation of Ackoff's concept of 'theory' [1, 2], he seems also in agreement with my notion of the dynamics of models and theories. Agreeing that full-fledged theories often start as concrete models grounded in the specifics of a particular situation, and they develop along the five dimensions which characterize a theory, he goes further and extends the scheme forward to embrace the dynamics of theory itself. He thus suggests that, in a similar vein, theories themselves probably expand, coalesce and mature into paradigms. He gives examples of paradigms. He then moves on to provocatively suggest that there might actually be not one but *two* hierarchies: a *substantive* hierarchy dealing with the explanation and control of natural phenomena, and a *methodological* hierarchy dealing with the development of methods for investigating or describing the natural phenomena. This is a perceptive encapsulation of my article. He goes on to suggest that each of these hierarchies contains three levels of theory:

—models at the most operational and most concrete level;
—theories, of the reductionistic or integrative type, at the intermediate level;
—frameworks, approaches and/or paradigms at the upper, most abstract level—which he groups together although he appears to agree with my detailed definitions.

Sirgy is correctly reminding us that bottom-level techniques and theories tend to be situationally grounded while upper-level frameworks are perforce fairly abstract in character. But he does not appear to stress distinguishing among the notions of 'framework', 'approach' and 'paradigm'. Furthermore, his examples of paradigms present both methodological and substantive difficulties and require more careful scrutiny. For example, in Sirgy's own acknowledgement, linear programming and critical path computations are *techniques* (or at most methods); they should not in my view be placed on the same footing as optimization theory, which is the suprasystem containing *them* as well as network theory and queueing theory. Besides, optimization theory is not a separate paradigm but, together with computer modelling, is a subset of the OR/MS paradigm as described in [1]. There is a need to separate out such methodological elements from each other, *all the while* capturing their relationships. Calling them all paradigms certainly glorifies them all, but does little to help us understand the structure of their set. My analysis of the methodological perspective afforded us by Ackoff's treatise [2] led me to the hierarchy depicted in Fig. 2 of [1].

The containing system of Fig. 2 is the Western Paradigm. Within it, I argue in the paper that there are two methodologies. the scientific methodology [2] and the anthropological methodology [e.g. 4]. Within a paradigm (in the usual sense used by scientists and by Sirgy) of the scientific methodology is the *a priori* analytical approach, which includes both the classical mathematical analysis in 'closed form' and the modelling/computer simulation approach. I have chosen the field of MS/OR as an example because its notions, being man-made, are relatively well understood. The next level within this theoretical hierarchy could be chosen to be the analytical closed-form derivation framework. It is a subset of the analytical approach; it excludes computer modelling and simulation but has its own subsets. Some of these are the mathematical programming methods and duality theory. One of them is the linear programming method. Within it, one may choose the tableau simplex technique or the revised simplex technique based on linear algebra, depending on one's degree of mathematical sophistication. One might even nowadays opt for Karmarkar's algorithm in some cases. This complex hierarchy is itself a simplification; clearly we have to posit more than three levels within each scientific paradigm.

Another problem with using Sirgy's proposal in its unadulterated form is that one might wonder whether the hierarchy does.actually pertain to the natural phenomena themselves, or merely our understanding or description of them. In other words, are we really talking about the substance

of things in science? While living systems theory appears to be addressing substantive issues, it is clear that open systems theory—the first one in Sirgy's list of 'substantive' paradigms—is actually a methodological approach applicable to a variety of contexts and contents. Moreover, models do not pertain to a single hierarchical level, but tend to span several levels, since most branches of modern science appear to rely on modelling far more than on any form of 'direct' inquiry [3].

Similarly, theories do not pertain to a single level. What we most often denote by 'theory' is not a particular level—since we may talk of 'Markoff theory' to denote a small segment of 'probability theory'—but a particular level of refinement or abstraction. A theory, as I argued previously, is a deductive system possessing fairly advanced features along five characteristic dimensions. Models are simply more rudimentary representations. The notations of 'model' and 'theory' are not intrinsic to the primary hierarchy of research concepts.

Does this mean that we should reject Sirgy's perceptive suggestion? Not at all. I would like to make use of it, but in a modified form. It is only with the notion of a substantive hierarchy totally distinct from the methodological one that I have some difficulty. I think Sirgy is right, but in the modified sense that there are *two* aspects of each approach to inquiry: a substantive issue (its 'content') and a methodological angle (its 'form'). Thus the hierarchy presented in [1] is at once a *theoretical* hierarchy as well as a *methodological* one. Since research does not proceed *in vacuo*, but uses the opportunity of a research setting, we may not have to cumber ourselves at this stage of two hierarchies. We should simply refer to the hierarchy elaborated in [1] as the theoretical–methodological, or simply the 'conceptual' hierarchy.

However, there is an aspect of Sirgy's proposal which ought to be of great interest to systems researchers. His view of a substantive hierarchy of systemic notions, models and theories might lead us to classify the field according to the *scope* of the various models, theories or approaches therein. Such a quest is in form and in spirit germane to the aims of our society, and leaves us with much food for thought.

Another aspect of Sirgy's thought of particular interest to me relates to his double-edged proposal to work on bringing further clarification to the concept of integrative and reductionistic theories, on the one hand, and to the notion of conceptual and operational models, on the other. This contribution is well received and deserves follow-up from systems researchers.

REFERENCES

1. W. Acar, Toward a consistent terminology for management theory building. *Syst. Res.* 4 (1987), 119–125.
2. R. L. Ackoff, *Scientific Method.* John Wiley, New York (1962).
3. C. W. Churchman and W. Ulrich, The status of the systems approach: Reply to Bryer. *Omega* 8 (1980), 277–279.
4. P. Diesing, *Patterns of Discovery in the Social Sciences.* Aldine–Atherton, Chicago, IL (1971).