THE FUNDAMENTAL TASKS OF SYSTEMS SCIENCE

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ABSTRACT

Systems science in its modern form originated in the 1930s, and expanded its claims until the 1970s. In the 1960s, under the term systems analysis, it was a compulsory tool in the US Federal Administration, and cherished in some administrations of other countries, like Western Germany. The present contribution aims at dressing inventories of the claims made by systems scientists, confronting these claims with critical literature, and drawing conclusions as to the feasability of the different claims.

The analysis comes to the following conclusion:

Systems science ought to set its priorities in those domains where it can most serve scientific progress and societal benefits. Both of these aims can be fulfilled, if it concentrates its efforts on networking the specialised sciences. Here the systems concept offers a set of conceptual and methodological instruments; there is a demand for this in science and society; and there is a prevalent recognition in science and the public at large that systems science is suited for that and has sufficiently demonstrated its capacity.

Contributing to this main task of networking the specialized social and natural sciences is the task of finding integrative concepts. Beyond the systems concept and the feed-back concept, the concept of information, and their derivatives, a number of concepts have come to the fore, e.g. self-organization, hierarchy, evolution. Also epistemology and model theory must be included. All these concepts are applicable to analysis and synthesis beyond the specialized sciences, but theses concepts, and the systems concept itself, are also part of the latter, in the sense that they are used there and cannot be claimed as being specific to systems science.

Keywords: epistemology, integrative concepts, networking the specialised sciences, world views

FORMULATION OF THE PROBLEM

In 1996, the German epistemologist Klaus Mueller made a voluminous critical analysis of systems science, which he still calls ”systems theory”, maintaining Bertalanffy’s terminology. He argues that systems science is still in a stage of underdevelopment on epistemological grounds. That it competes with the paradigm of the exact sciences, without having a consistant epistemological basis. That it uses analogies as a major method, without taking into account the purely heuristic function of them. That it lacks the unity of the exact sciences, being subdivided into different streams that use different terminology. He points out that even the meaning of the systems concept is not the same for all systems scientists, some using it as describing reality (”The world contains different kinds of systems, cf. organisms or cities”), for others, the systems concept refers to the sphere of the observer (”For many practical purposes, it is convenient for human obervers or actors to describe certain phenomena, cf. organisms or cities, as systems”).

On the other hand, Mueller is startled by the attraction that systems science holds for the scientific specialists as well as for the laymen. ”Terminology from systems theory has become a customary element of scientific discourse. ...As late as the 1960s, systems theory was regarded as a revolutionary perspective, but in the course of time the general systems theory has become a legitimiser that influences the consciousness of numerous scientists - thus entering into competition with analytical epistemology,” (Mueller, 1996, p. 1). ”As one of the great concepts of science next to positivism and dialectic philosophy, general systems theory claims to be a formal and material unifying science, which transcends positivism ...” (p. 10).

Proponents of systems science have to take these arguments serious.

Does systems science really cancel traditional scientific concepts; does it make them obsolete? Or does the systems concept occupy a legitimate, maybe even indispensable position in the overall system of the modern sciences **without** invalidating other concepts ?

In this contribution, the aims of systems science as declared by the founding fathers in North America and by their followers in West-Germany in the 1970s, are confronted with the literature that is critical of the systems concept. The points of criticism are gathered and compared with the aims formulated by the proponents of systems science.

This allows for an evaluation of the function and status of systems science as an independent discipline. This also yields answers to the question: Which aims and instruments can today be advocated for systems science on the basis of past and present experiences, in the context of epistemology and the canon of disciplines?

HISTORICAL ASPECTS

One point of departure for this analysis is the progressing **specialisation** of the disciplines in the context of a **division** of the disciplines into three groups (Kulla, 1979, p. 24, referring to Stachowiak) with different methodological emphases:

- **Natural sciences**: empirical and inductive,
- **Formal sciences**: axiomatic and deductive,
- **Humanities**: hermeneutic.

These emphases do not, however, prevent a given discipline from using other methods for a given task, although normally to a lesser degree.

Important results of research on the history of science and the history of technology, which is based on science (cp. Wussing, 1983; Mason, 1974; Klemm, 1986; Jetter, 1992) are:

In Antiquity, all three of the above-mentioned methodological concepts were developed simultaneously. In the Middle Ages, when the hermeneutic direction prevailed, the empirical approach and undogmatic use of epistemology were suppressed (”science as the maiden of theology”). As a reaction to that, from about 1500 A.D. onward and under new social conditions, the empirical approach was reactivated and developed with enormous theoretical and practical success, connected to the increasing specialisation, referred to already.

The rapid development of science and science-based technology has continued to the present day. It is linked to the rise of a specific form of society which is based on science, called the democratic industrial society or simply ”modern society”. This type of society is spread into more and more regions of the world, accompanied by a ”population explosion”.

The science system and democratic industrial society need integrative concepts to link the results of the specialised disciplines and to find solutions for the steering problems which become increasingly complex. Systems theory, systems analysis and systems science are the result of these efforts (from around 1930 onward) to overcome the specialisation of the disciplines theoretically and/or in practice. Prominent names in this context are Bertalanffy and Wiener.

Bertalanffy’s General Systems Theory (in the sense of: a general theory of systems) was ascribed to basic research. Wiener‘s concept of cybernetics was developed in the USA in the context of ”big science” and was also applied there (construction of the atomic bomb, the ”peaceful use of nuclear energy”, space projects, landing on the moon). The positive experiences made in these domains led, in the United States in the 1960s, to an expansion of the application of ”systems analysis” into education and social security. ”Systems analysis” under the name of the Planning-Programming-Budgeting-System (PPBS) was made compulsory in 1965 under the democratic president Johnson for financial planning and application for public grants in the context of the federal bureaucracy. Under the republican president Nixon, PPBS was abolished in 1971 following massive practical problems in its application. Furthermore, it was regarded by conservative social groups in the United States as a fiend of centralisation (as a negative concept) and as being at least adjacent to communism.

In West Germany, too, there were approaches in the governmental sphere to a broad planning based on systems concepts (in the Office of the Federal Chancellor under Minister Ehmke after 1969; and in the state of Rheinland-Pfalz under the then state prime minister and later federal chancellor Helmut Kohl). These approaches failed, mainly because of resistance from the specialised ministries and because of the unrealistically comprehensive objectives.

In the Eastern bloc, cybernetics was regarded at the beginning of the 1960s as a means to a more efficient planning process and was supported accordingly, but fell into disrepute with the party ideologists by the end of the decade and was repressed even in the world of research.

Until the end of the 1970s, systems research was a focal area of public support for research in Western industrialised countries. The far-reaching expectations that had been raised by advocates of the systems concept are exemplified by the list of tasks drawn up by Zahn to justify priority promotion by the Volkswagen Foundation.

Table 1: Overview of the tasks of systems research (from: Zahn, 1972, 42)

Theory-oriented	**Methodology-oriented**	**Application-oriented**
Development of methodology to formalise verbal statements for different phenomena of reality (general mathematical systems theory): - Scrutinising basic epistemological problems; - Establishment of general homeological system laws for the structure and the behaviour of dynamic complex systems; - The further development or adaptation of existing theories; - Development of a formal theory of social systems; - Development of formal concepts to represent management hierarchies, especially to deal with problems of communication and coordination.	The further development of methods to represent, analyse, design and control dynamical nonlinear systems: - Further development and adaptation of methods of system recognition (para-meter and system estimations) and of system optimisation; - Development of methods of selection, integration, subdivision, aggregation and coordination of system elements; - Development of methods to validate structures and dynamics of complex system models; - Linking of methods of systems research with methods of mathematical statistics, econometry and sociometry; - Development of models to identify objectives; - Further development of problem-oriented computer software.	Analysis of biological, ecological, economic and social systems with the aim of early recognition of problems for society and of finding ways to their solution and/or their containment: - Indication of consequences of demographic, economic and technological developmental paths; - Indication of alternatives for action (laws and programmes) in social systems; - Obtaining foundations for decisions for long-term planning and for control of decisions in social systems; - Analysis of the adaptability of value systems.

Renowned results of systems science were, for example, the world models by D. and D. Meadows et al. (1972) and by Mesarovitch and Pestel (1974). Unfortunately, the prognostic power of these models was greatly overestimated. More long term practical consequences resulted from ”Global 2000”, a comprehensive interdisciplinary analysis of important world problems that transcend single disciplines. It led to practical programs of action, like the Brundlandt Report and the Agenda 21.

At the end of the 1970s, exaggerated objectives and methodological difficulties to realize them led to a certain disenchantment (compare below Section 4.1.5.). Nevertheless, the further expansion of the basic concepts of systems thinking in science, politics, administration and the public was not held up by these set-backs. This was, among other reasons, due to the fact that by an expansion of the focal concepts of systems science, illustrated by conceps like autopoiesis, entropy, complexity, chaos, catastrophy theory, limitations of the 60s and 70s were overcome (Bailey, 1992: ”new systems theory”).

In this context, it seems to be even more urgent, regarding aims and methods of systems science, to distinguish on the one hand those concepts that are compatible with established scientific approaches and helpful for the scientific enterprise in general and for society at large, from, on the other hand, errors and exaggerations. An overview with this aim will be attempted in the following sections.

PRIORITIES OF SYSTEMS SCIENCE

In the course of ist 60 to 70 years of history, different authors have set aims and methods of systems science in different directions and with different scopes.

Priorities regarding the aims and methods of systems science were:

(1) Use of the **concepts of system and model** for intra- and interdisciplinary phenomena.

(2) Search for **structural similarities and integrative concepts that transcend disciplines** (conclusions on the basis of analogies, especially using organismic and functionalist ideas; ”cross-level hypotheses”; certain mathematical equations that describe phenomena in different disciplines), with **central interdisciplinary concepts** like: open systems, hierarchy of systems, homeostasis, autopoiesis, control, feedback, equilibrium, self-organisation, emergence, networks.

(3) On the basis of 1 + 2, the construction of a **universal scientific language**, and even the transdisciplinary reconstruction of the complete scientific system.

(4) Emphasis on **”holistic” thinking**, with some going as far as the Gaia concept considering the whole biosphere as a single organism.

(5) **Quantification and mathematical modelling**, linked in the USA in the 1960s to quantified utility analysis (compare above the concept of PPBS in Section 3).

(6) Analysis of and finding solutions for central **world problems** (like population explosion, hunger, destruction of the environment, exhaustion of natural resources, war), frequently in connection with mathematical models of single countries and of **world models** (Meadows; Mesarovic and Pestel; Global 2000). This also implies the detailed inclusion of socioeconomics.

(7) **Supplementing functional concepts** by catastrophy theory and **chaos theory** in the 1980s.

(8) Following from 4 and 6, the development of an increasingly **integrative scientific world view,** including concepts of

- the structure of the biosphere,
- the development and status of the species Homo sapiens in it (theory of evolution and evolutionary epistemology),
- the human systems and the possibilities of their development (Jischa, 1993; Agenda 21),
- the status of persons (i.e. ourselves) in this context (cf., e.g. Kriz, 1997).

World views in this sense are not normally included into the realm of systems science, but the main ideas of systems science are present within them. In the ”Leo Apostel” Center at the Vrije Universiteit Brussel, research on such world views is part of the current interdisciplinary research programme (Aerts et al., 1994; 1995).

CRITICISM OF SYSTEMS SCIENCE

Some Critical Authors

Criticism of systems science and related concepts like systems theory and cybernetics is very manifold, and the authors start out from very different ideological positions. The starting point for criticism was especially the triumphal march of systems analysis in the USA in the 1950s and 1960s, as well as the promotion of systems research in West Germany in the 1970s.

In the following, some especially remarkable publications are described which emanated from both the USA and Germany. (The particular problem of cybernetics and systems science in the German Democratic Republic as a country run on state socialist grounds until 1989 cannot be considered here.)

Ida Hoos (1972)

Hoos (1972, 2nd edition 1974), political scientist at San Francisco State University, especially criticised the interplay of politics and science in the context of the Planning-

Programming-Budgeting System (PPBS). This method was subsumed under systems analysis, and it comprises very thorough budgeting of public projects. The focus is a cost-benefit analysis that tries to express all variables in monetary terms. This method, as already mentioned in Section 2, was introduced in 1965 in the US administration, but was abolished in 1971 because of the many problems it had created.

Hoos intends to give a critical investigation of the state-of-the-art of systems analysis (p. 7). She concentrates on the exaggerated promises of so-called systems research institutes in their proposals to obtain public funds for their studies (p. 7) and compares these promises with the results. To give her arguments a broad foundation, she investigates the epistemological basis of systems analysis (Chapter 2) and its applications in different domains of American politics and the administration (Chapters 3 to 6), in private industry (Chapter 7) and in future studies (Chapter 8). Mercilessly she reveals the inefficiencies of the so-called ”system-analytic planning” and the problems to realise these plans in the domains of waste management, supersonic aviation, education, health, and management information systems. In the final section of the book, a chapter on ”Systems analysis in a social perspective” (p. 241) she summarises her criticism and especially warns about:

- asking the wrong questions,
- reducing the analysis to quantifiable variables,
- inadequate quantifications,
- symbolic rather than practical solutions,
- delegating responsibility to external experts,
- trusting one single method,
- too high a financial input for the studies.

Despite the severe criticism of the state-of-the-art, as she found it, she nevertheless perceives a definite function for systems concepts, as she points out in the penultimate paragraph of her book: ”This is not to say that systematic approaches do not have a contribution to make to the understanding of social process and improvement of the the social condition. ... The systems approach, if it is ever to become conceptually sound, must be a genuine multi-disciplined endeavor, in which contributions from the pertinent fields of knowledge are meaningfully synthesized, and not merely homogenized into a synthetic and symbolic language” (p. 247).

A few pages previously, she had stated with a certain surprise: ”The very durability and resilience of the systems approach is a factor worthy of note in a review of its phenomenology,” (p. 241).

Robert Lilienfeld (1978)
Lilienfeld argues against systems analysis from the point of view of social philosophy. He includes Hoos` arguments, but proceeds in a much more fundamental way. He distinguished three kinds of systems science literature (pp. 1-2):

- articles and studies of a highly technical nature,

- essays and articles of a missionary nature addressed to a broad readership,
- books and anthologies giving introductions to systems analysis for a broad readership.

Lilienfeld concentrates his criticism on the latter two kinds of literature.

His book is divided into three parts:

(1) The disciplinary origins of systems theory,
(2) The societal claims of the systems thinkers,
(3) Systems theory as an ideology.

The book contains a lot of material and interesting information, but it is characterised by a fundamentally negative attitude towards systems theory.

In the first part, the origins of systems theory are described using the following sources
(1) general systems theory by Bertalanffy,
(2) cybernetics,
(3) theories of information and communication, artificial intelligence,
(4) operations research and systems analysis,
(5) economics: Linear programming, input-output theory, games theory, decision theory.

An example can illustrate how the unrealistic promises of systems theoreticians are interpreted as the basic weakness of the theory: In the chapter concerning operations research two case studies are described showing how systems science research did not fulfil the expectations raised. Under the community renewal programme created by an act of Congress in 1959, simulation studies for the cities of San Francisco and Pittsburgh were made at the price of more than 1 million US-dollars each. Their aim was to evaluate alternative developmental paths; ”...the results were disastrous,” (p. 129).

”In both cities planning officials developed healthy scepticism regarding the reliability of consultants who, it was felt, pursued their own interests and enthusiasms, turning in reports of little use to officials and of little relevance to the problems originally posed, and who were able to decamp without bearing responsibility for their contributions or suggestions,” (p. 131).

Lilienfeld cites G. Brewer, a systems scientist from the RAND-Corporation, who concludes from these examples: ”If the experience is even partially representative, the long-term prospects for the integration of the computer into the urban decision process are dismal indeed,” (p. 131).

In the second part of Lilienfeld’s book, the interdisciplinary concepts of systems thinkers are described and criticised. Here Lilienfeld concentrates on the publications by Laszlo (pp. 160-179). Laszlo clearly transcends the empirical domain and makes normative statements. Lilienfeld implies that the normative statements are supposed to convince because they are formulated in system-oriented terminology.

In the chapter on systems thinking in the social sciences, Lilienfeld underlines that systems thinkers do not present any new empirical results, but create new terminology: ”Thus the vocabulary of systems science, as developed by Easton and Deutsch, appears as a vast and elaborate detour by means of which we arrive at precisely the concepts that are the heritage of political science and sociology ...”
(p. 221).

In the third part the author describes, among other topics, his view of the relations of systems theory to the intellectuals and of their ideas and demands, which he considers mostly inadequate (p. 261 to the end of the text on p. 280). Thus he states that the organismic view of systems theory yields the basis for an ever-increasing centralisation of society (pp. 263, 278). Under the title ”The Scientist – King” (p. 279), Lilienfeld insinuates that ”the proponents of reason, of philosophy, and of science, have asked the powers of the world to surrender their control of the world to them. With their tools of reason, conceptualisation, and science they have assumed they could solve all problems. Plato was perhaps the first of these ...” (ibidem).

Czayka (1974)
With an overview of the book by Czayka, a professor of business administration, we now touch on the German discussion about systems science, which lacks the ideological pungency that is characteristic for US-American publications.

The book ”Systems Science – A critical discussion with examples from economics and business” is a sober inventory of a specialist.

After explaining different basic concepts, he cites typical programmatic statements of renowned systems scientists and generalises from that,

”that the common objective of systems science is a *reintegration* of Science, at present subdivided into the disciplines,” (p. 62).

Czayka points to the analytic epistemology (ibidem), ”by which a substantial contribution was delivered,” (together with annotation 88: ”As to the unity of objectives and methods of explanations of the empirical sciences compare especially Popper,” The Objectives of Empirical Science, 1964). He claims without documenting it further that ”this contribution is hardly recognised by systems scientists,” (p. 63) and he concludes from that:

Systems scientists ”preserve this problematic with an inadequate pioneer attitude with the consequence that their epistemological concepts must at least be called extremely superficial,” (ibidem).

He distinguishes different schools in systems science:

- the ”theoreticians of similarity” (p. 64) and
- the holistic and multi-disciplinary approach (p. 66).

The discovery of similarities refers to classes of equivalents of existing empirical theories from different disciplines with regard to their logico-mathematical structure. In this domain, Czayka sees a task for systems science, which has not yet been taken up by other disciplines, except for cybernetics, which is specialised on models of control (p. 64).

In considering the holistic and multi-disciplinary approach, Czayka ascertains the logical impossibility to research certain phenomena from all possible aspects.

He nevertheless recognises that the delineation of scientific problems could be done differently to the way in which it is presently done by the existing disciplines. He discerns the possibility to create multi-disciplines for the solution of certain practical problems, i.e. empirical sciences with a theoretical and a technological branch (pp. 69-70).

As to the application of systems science in other disciplines, Czayka envisages three directions:

- the ”systems science terminology in the strict sense,” which he considers helpful for didactics and interdisciplinary communication,

- the ”systems science system typology”,

- ”certain second-hand formal methods” (all items: p. 91), that means derived from mathematics and logics (ibidem, annotation 151).

Applying systems science to his own discipline, economics and business, Czayka discerns two approaches:

- The first applies formal cybernetic models ”in a superficial interpretation” to economic phenomena and depicts them in the form of box charts (p. 92). He considers the usefulness of this approach to be rather limited.

- The second concept interprets, on the basis of cybernetics, ”economic theories in the context of economic policies and business administration and by that with an institutional background.” This, according to Czayka, implies positive didactic, methodological and heuristic effects (p. 93).

The author concludes: ”We cannot, however, expect of any application of systems science concepts in economics or in any other empirical science, a direct contribution to the empirical validation or falsification of existing or newly conceptualised theories and with this to the expansion of empirical knowledge,” (ibidem).

Kappel and Schwarz (1981)
The book ”Systems research 1970-1980 – Developments in the Federal Republic of Germany” (1981) contains the view of two systems scientists who were asked to evaluate a decade of systems science research and its funding by the Volkswagenwerk Foundation.

The authors start out from the research programme that was put forward in 1972 by Zahn, which had served as basis for funding systems research by the Volkswagenwerk Foundation (cf. above Section 3).

They conclude: ”The discussion of important streams of research from the 1970s shows that obviously there is still very substantial theoretical and empirical work to be done in order to come closer to the intentions and visions of the founding phase. The experiences have also demonstrated that work does not progress continuously as some had imagined in the past,” (p. 82).

In a survey, the following conclusions are drawn among others (bold characters by E.U.):

- ”... the **interdisciplinary perspective has left the most visible traces**. The work discussed here has again demonstrated how necessary this approach is and how it can be used with great gains. ... It cannot be doubted that even the present state of systems research lays a common basis for communication and cooperation of scientists of different disciplines,” (ibidem).

- ”With the **operationalisation or modelling of higher types of systems** (especially within cybernetic systems theory) the progress in the work discussed here has been very limited. ...The expectation of the great, all-encompassing solution has proved to be an **illusion,**” (p. 83),

- ”... Systems theory may run the risk of arriving at **propositions of little stringency and minor empirical relevance,**” (ibidem). The reasons for that could be:

  - the development of abstract theoretical systems without feedback from empirical studies,

  - ”the careless effort to develop very large and multi-dimensional models, which is bound to lead to theoretical and methodical simplifications ...” (ibidem).

- ”... systems research cannot forgo **disciplinary experience**, and for finding adequate theories and methods a thorough examination of disciplinary theory is absolutely necessary; ...” (ibidem).

- System researchers ought to ”try to cope with some of the ‚great **challenges‘ of the founding phase,**” (p. 87).

As a final conclusion the authors state: ”Even referring to the projects discussed in this report, one has to ask in many cases where the specific systems-theoretical aspect is expressed, and whether simply all those projects are subsumed under the concept of systems research which use mathematical methods to develop quantitative models. This would be a deplorable undermining the initial concepts,” (p. 89).

Klaus Mueller, 1996
A criticism of systems theory in the sense of systems science, which is very rich in material and multi-facetted, was presented by Klaus Mueller, a Berlin social scientist and epistemologist, mentioned already in the introduction to this article.

”As one of the great scientific concepts next to positivism and dialectic philosophy, systems theory claims to be a formal and also material unifying science, transcending positivism, ... For this it relies on its own traditions from which their specialities result:

- organismic heuristics,
- scientific conclusions by analogy,
- confidence in mathematics as a means of synthesis, and
- fundamental ideas of order that include nature and society, reality and consciousness,” (p. 10, format of enumeration introduced by E.U.).

The aim of the analysis is to ”clarify the scope of the systems-theoretical approach.” For that ”I (= K.M.) chose the form of a historical representation that starts with the philosophical prehistory of the modern systems concept and gradually leads to the current systems theory. ... Against this background the question can be asked whether since the 1980s a new systems theory is in the process of development,” (pp. 9-10).

”An epistemological reconstruction seems to be necessary, as the general systems theory is not in the systematized condition that the name promises and because a number of problems arise from this that have so far been insufficiently illuminated. ... In other words, the research programme of systems theory does not dispose of a description that would be comparable to Ernest Nagel‘s *The Structure of Science* or Carl G. Hempel‘s *Aspects of Scientific Explanation,”* (pp. 3-4).

Again and again Mueller underlines the independence of systems science, and sees it as a rival to other scientific concepts:

”Characteristic for the general systems theory was the claim to link their basic concepts and methods to form a model of explanation and of understanding theory that corresponded to a uniquely **idiosyncratic** conception of epistemology and reality. And these epistemological and science-theoretical principles were the reason for this rivalry with analytical science theory,” (p. 219, bold characters by E.U.).

With this view that systems science ought to replace other concepts, Mueller set very high standards for systems theory. However, in contrast he also expects systems science to choose to be either a logical/deductive or an empirical/analytical science. If systems theory were to comply with this last demand, it would loose its comprehensive character and with that the possibility to realise its own comprehensive and integrative approach. In so far, there is an imminent contradiction in Mueller‘s fundamental view of the objectives of systems science. This is visible in many special problem areas raised by Mueller and which he declares to be insufficiently grounded in scientific theory.

As already mentioned, Mueller bases his criticism on a very extensive description of the genesis of his topic:

- the meaning of the systems concept from Antiquity to the Weimar Republic,
- the relevance of the New Sciences, information theory, cybernetics and operations research, which came into being parallel to systems theory,
- and all that in the context of new demands of society on Science.

On this basis Mueller concentrates on the description and criticism of Bertalanffy’s general systems theory, which he calls the Bertalanffy-Programme. This theory is regarded by Mueller as representative for systems theory. After a very incisive description in three chapters he comes to the criticism on which this overview will concentrate:

”Both the epistemological criticism (of the analytical science theory against systems theory – E.U.) and the internal differences within systems theory, point to the concept of theory in the Bertalanffy-Programme: to a method of forming concepts that oscillates between materialising universal concepts of mathematics and logifying empirical conditions,” (p. 245). From this starting point the criticism is subdivided into eight sections:

> (1) The dilemma of a universalist systems concept:
>
> ”If the relevance of the concept of a system is guaranteed by definition, then the positivist suspicion would be confirmed that general systems theory is nothing more than a system of analytically true propositions: that it is one huge tautology,” (pp. 246-47). This is an exaggerated formulation of the dilemma between generality and specificity which was also described by the systems scientist Boulding. What is the optimal degree of generality?
>
> (2) Theory and mathematics: ambivalence of the concept of theory in systems science:
>
> Mueller refers to the antithesis between the logical and the empirical validity of a mathematical system (p. 250). In analytical science, the empirical validity is proved by experiments that can lead to a falsification of the theory in reality.
>
> This problem is then transposed to systems theory. ”The distinction between logical and empirical interpretation of a system of axioms raises the question which differentiates analytical logic of science and general systems theory ... . If the axiom systems of ‚pure logic‘ or mathematics do not *per se* relate to reality,

according to which criteria ought the application of mathematics in reality to be judged?
General systems theory evades this question because it wants to be both a formal and a material science at the same time" (p. 251). Mueller imputes that systems theory tries to make the examination of the criteria of validity superfluous with the help of the concept of isomorphism (isomorphism between theory, thinking and reality).

"It is precisely this concept which was unacceptable for science theory... 'In so far as a calculus is applied to reality, it loses the character of a *logical* calculus and becomes a descriptive theory *which may be empirically refutable*; in so far as it is treated as irrefutable, i.e. as a system of *logically true* formula, rather than a descriptive scientific theory, it is not applied to reality,'" (pp. 251-52 – the citation within the citation originates from Popper, 1946. p. 210).

(3) Theory and experience: Hempel vs. Bertalanffy

This section is a continuation of the previously mentioned antithesis between purely mathematical and empirical validity applied to the technique of simulation.

(1) Functional explanations

In this section, Mueller points out imprecisions of the concept of function which, in his opinion, is central to systems science (the mathematical concept of function as opposed to the biological and social use of it). He makes the connection to the concepts of functional imperative, self-regulation, and functional equivalent. He emphasises that the substitution of causal relations by functional statements and the ideological use of so-called systems imperatives is not permitted, alleging that systems scientists do not always respect these limitations.

(2) Cross-level hypotheses and emergence

Mueller points out difficulties with the empirical foundation of cross-level hypotheses and with the application of the concept of emergence (pp. 259-261). He claims an inconsistency between the concepts of homomorphy and emergence (p. 261).

(3) "Well-behaved systems": Limits of systems analysis

Mueller states that the instruments of systems science (comparative statistics, difference and differential equations, analysis of threshold values) are not equally suitable for researching all phenomena of reality. "Less ‚mechanistic' phenomena like, for example, evolutionary, neurobiological and social processes are interpreted as complications; more complex phenomena like fluctuations, structure breaks and systems with several state-variable areas are interpreted as deviations from the natural state of things," (p. 265).

(4) Internal contradictions within the Bertalanffy-Programme

Mueller points out that different authors within systems science use different basic concepts and disagree as to their validity. Central in this respect are the following concepts:

- Organismic heuristics vs. general theory of mechanisms (pp. 266-67),
- holism vs. reductionism (”*Mikrofundierung”*) (p. 268),
- analogies (p. 269).

Citations by Bertalanffy that link the systems theory to the rules of verification and falsification of the analytic science theory (ibidem), are classified as ”belated” and as a ”concession”.

(8) Symbolic realism and the epistemological motivation of the classical systems theory

Mueller points out references of both systems theory and analytical science theory to the concept of symbolism, which was used as an ordering principle and a point of crystallisation for ideas of the unity of science and as a constructivist understanding of reality (pp. 270-76).

Whilst the following chapter on ”social science heuristics of classical systems theory” is irrelevant here, the chapter on ”Crisis and decline of the general systems theory” is oriented towards the developments in the world of politics, as described by authors mentioned previously in this text. Mueller considers the zenith of Bertalanffy's systems theory as the first half of the 1960s (p. 311), mainly caused by promises of application in social technology and administration.

For the second half of the 1960s Mueller diagnoses a slump. The reasons for that ”were ... not so much epistemological objections, but social developments in the real world," (p. 312):

* the Vietnam War,
* public poverty vs. private affluence,
* the oil crisis that could not be cured by Keynesian economics,
* the student movement as an attack against the traditional university system (ibidem).

Rational public budgeting and modern social policy suffered badly in bureaucracy (p. 315). Obstacles were the scepticism of conservative critics concerning the welfare state (p. 316) about planning, and on the other hand a change of mood of the general consciousness, not attributable to particular political groups ..., ”that led to the gloomy studies by Dennis Meadows, Jay Forrester, Michajlo Mesarovic and others about the destructive consequences of unlimited growth. ... The ecological costs of economic growth and the escalating interstate potential violence led to a boom of catastrophe discourses that often descended into the synthetic pseudo-religions of a ‘New Age,’” (p. 317).

In the final chapter ”On the future of systems theory”, the description of the new concepts autopoesis, theory of catastrophe and chaos theory is especially relevant. ”Under the impression of these new concepts the vocabulary of systems science was altered in a significant way. The categories of the determined system, the homeostatic equilibrium and of adaption (sic) to pre-existing environmental conditions gave way to attentiveness to instabilities, catastrophic changes and a ‚self-organisation‘ that is removed from external influences,” (p. 325).

In unison with his fundamental criticism of systems science, Mueller sees the ”perspectives of systems theory” (pp. 357-359) as mainly negative. However, he mentions ”its momentum towards an interdisciplinary conception of science that is up to the present state of theory formation and its problems,” (p. 359). He believes that the absorption of these impulses depend on a (self-)critical analysis of the history of systems theory.

Summary Of The Aspects Of Criticism

As was clearly visible in Section 4.1, most of the basic aspects of systems science mentioned in Section 3 were points of departure for criticism:

Concepts of system and model:
These concepts are accused of being too general, too mathematical and too formal. Different definitions of a system are opposed to each other (Bertalanffy vs. Hall & Fagen). It is stated that no aspect of reality is excluded by this definition of a system. This leads to postulations for an unequivocal attribution of systems science to either the natural sciences **or** the formal sciences. The present attribution is accused of being unequivocal and as such needs clarification.

Structural similarities, integrative concepts:
It is said that this aim is legitimate, but it cannot be realised by empirical studies, or at least not to a sufficient degree. Conclusions on the basis of analogy could not be considered proofs. The use of organismic analogies and of the concept of functionalism is not always possible. Other interdisciplinary concepts (open systems, hierarchy of systems, homeostasis, autopoiesis, control, feedback, equilibrium, self-organisation, emergence, networks instead of causal chains) are regarded as exaggerated in importance and as not exclusively belonging to systems science. The abolition of thinking in causal terms was feared or at least sketched as a spectre.

Universal scientific language on the basis of systems science: Such an objective is regarded as impossible. The overwhelming majority of discipline-oriented scientists are made to feel insecure and challenged by such a claim.

Holistic thinking:
It is stressed by the critics that there is a difficulty in empirical thinking in this context. The Gaia concept is predominantly regarded as unscientific, because it could not unequivocally prove its innovative claim by empirical methods.

Quantification and mathematical modelling:
It is emphasised that not all areas of reality and especially of society can be treated sensibly with these instruments. The failures in using PPBS and the cost-benefit analysis are frequently pointed out. Here it has to be kept in mind that for the first time in the recent history of science, a very close cooperation of ”big government” and ”big science” was developed under the concept of systems analysis, in which billions of US $ were involved. The abuse connected to this new situation and especially the void promises contained in proposals for financing research projects were partially attributed to systems analysis as a scientific concept.

The concentration of systems science on mathematical modelling is also seen as a regression.

Critics also feared that empirical studies could tend to be replaced by simulation, leading to a loss of reality.

World models, the solution of major world problems:
Criticism referred to:

- frivolous data foundations,
- methodological limitations that are not sufficiently reflected upon,
- the creation of exaggerated expectations as to the possibilities of mathematical models,
- the propagation and misuse of belief in computers,
- disregard of economic dogma by attacking the dominant preference for economic growth,
- propagation of fears of catastrophe.

Chaos theory:
As was formerly criticised regarding confusions based on organismic analogies, now the one-sidedness of the generalisation of the chaos theory is criticised.

Integrative scientific world view:
The general concept of a world view that is vaguely discernible in the literature is not clearly attributed to the name of systems science by the public concerned. For that reason, according to the impression of the author, it is not included in the prevalent criticism against systems science. Part of the reason for that is also that interdisciplinary networking as a task for systems science is predominantly regarded as necessary.

Beyond the **aspects mentioned above,** further aspects are criticised:

- ”The inadequate capacity of the present systems theory ... to coordinate the **different streams within the approach**,
- the **marginalization of basic research in systems theory**‘” (Mueller, 1996, p. 357 – layout and enumeration inserted by E.U.).

Evaluation of Criticisms

Which **conclusion from these criticisms** were or have to be drawn by systems scientists ?
Are there, in the existing systems science programmes, unrealstic aims and inadequate methods ?

Systems concept:
The criticism of the excessive **generality of the systems concept** is **not convincing**, because the creation of analytical tools which are applicable as generally as possible is part of overall scientific methodology. The systems concept, according to the definition by Hall & Fagen (the definition of a particular system and its elements and limitations according to the specific necessity of the given problem) is as general as the notion of numbers. As in the case of numbers, the generality of the use of the systems concept can not be objected to on epistemological grounds: on the contrary, it is desired to be general.

The claim of the critics that **systems science ought to decide unequivocally to be either a formal or a natural science is also not cogent**: the importance of systems science has to be seen in its function as a network for the specialised disciplines on the basis of the systems concept (cf. also Kappel, Schwarz, 1981, p. 82). In this function systems science shares the methods of all specialised disciplines of both the empirical and the formal sciences, as well as the humanities. Depending on the context, the systems concept has to adapt to different demands; and the rules that are valid in the relative context then hold for the system-oriented works. This is expressed by Bertalanffy in the citation taken from Chapter 12, Section 6 of Mueller’s book: ”The evaluation of such models must follow the general rules for verification or falsification.”

Structural similarities, integrative concepts:
Pure analogies have moved somewhat to the background of systems science, because conclusions on that basis cannot be regarded as proof and the empirical foundings of far-reaching ”cross-level hypotheses” have encountered severe difficulties, especially when tried without cooperation with the specialised disciplines.

Rapoport (1981) enlarges on that taking the example of the organismic concept in systems theory: living systems from the single cell up to human society and the biosphere show structural similarities that are dealt with under concepts like metabolism, homeostasis, growth, information processes, control, propagation. He refers to representations of these ideas in matrix form that were presented by R. Gerard (S. 105) and by J.G. Miller (p. 106). ”...the theoretical fruitfulness of these two representations is dependent on the fruitfulness of the analogies suggested by them. ... The analogy is the heart of all generalisation, that means, of all induction,” (ibidem). The possible spectrum is estimated by the authors to be huge, ranging from the extreme of ”superstition” and ”sterile metaphoric models” on the one side to ”mathematical isomorphisms” on the other (p. 107). The test on the fruitfulness of such theories on the different layers is the task of the specialised disciplines (p. 108). The task of systems theory would be to reveal the general principles underlying these processes (ibidem – cf. also: Doucet, Sloep, 1993, p. 82).

Another example for the testing out of a concept regarded by many as integrative is given by Corning and Kline (1998) dealing with entropy. The conclusion of the first part of their analysis is: ”Of what relevance is entropy to an explanation of biological evolution ? It forms a constraint; it is not a cause. To repeat, horse manure does not explain a horse” (1998, 290). As a final conclusion, the authors call the reduction of biological evolution and the dynamics of living systems to the principals of thermodynamics or other purely physical concepts ”a theoretical cul-de-sac” (p. 476). They argue in favour of ”a more discriminating attitude toward the uses and limitations of thermodynamics and information theory in the life sciences” (p. 477).

The conclusion from that must be that interdisciplinary structural similarities do belong to the realm of systems science, but only in cooperation with the specialised disciplines. Whether or not, and if yes, in what way, general concepts, like emergence or entropy, yield useful statements in a circumscribed field of science, can only be decided by the responsible specialised science. By this token, these concepts are also part of the specialised sciences and thus not constitutive for systems science.

The universal language of science:
According to the author’s overview, this aim is no longer regarded as a task for systems science. In the inevitable and desired interdisciplinary communication more and more specialised scientists will favour an interdisciplinary precision of their terms, without systems science having to be active in this context by itself.

Holistic thinking:
At present, the concept of the top-down approach and the aim of including as many aspects as possible, can be regarded as a provisional explication of what is meant by holistic thinking. Despite all methodological difficulties, at least a tendency towards holistic thinking is necessary, also in the specialised disciplines. The difficulties, especially in the socioeconomic domain, are considerable. When analysing the results of holistic thinking, the limits have to be indicated very clearly, which unfortunately has not been the case in the past. More research into this aspect is necessary.

Quantification and mathematical modelling:
These methods are not constitutive for systems science. They have a **firm place** in the methodology of systems science **as well as** of many specialised disciplines. An inadequate quantification, as frequently met with in cost-benefit analyses, can not be attributed to systems science as such.

Dealing With World Problems And Designing World Models:

Some arguments of the critics were at least partially justified:
- There were wrong estimations of data, e.g. in the Meadows World Model.
- The conceptualisation and modelling of psychic processes was inadequate.
- The misinterpretation in the public of the prognostic power of mathematical computerised models was not sufficiently counteracted to.
- One reason for the catastrophy thinking derived especially from ”Limits to Growth” (Meadows et al., 1972) and Global 2000 (1981) was that the then prevailing concept of controlability of industrial societies by Keynsian methods was equally one-sided and at least exaggerated - as could be seen after the oil crises.

These exaggerations can be used as general arguments against the scientific soundness of the systems approach only if they are unseparably linked to the basic concept of systems science. This is not the case. The scientific treatment of many world problems is done in a much more practical way, especially by the Agenda 21 and the follow-up activities concerning sustainability. Mathematical world models are used with great precaution today; they serve for the description of much more narrowly circumscribed processes; a lot more specialised disciplinary experts are involved. In so far, the critique of the 70s was the cause for corrections in the methodology of systems science.

Chaos theory:
Systems science can not be blamed for the fact that the results of chaos theory have occasionally been presented in a too one-sided way and have been generalised too far in the direction of the general unpredictability of any system.

Integrative world view:
Approaches to create an integrative world view, as already mentioned in Section 5.2, are not (yet ?) part of general criticisms towards systems science.

Different streams of systems science:
The existence of different streams within systems science is not an exception in the general context of science. There have also been frequent conflicts about streams and approaches in other disciplines, and these still exist.

Neglect of basic research:
This criticism may be justified, but it is not a decisive argument against systems science in general and its epistemological grounds.

CONCLUSIONS FOR SYSTEMS SCIENCE

Systems science ought to set its priorities in those domains where it can most serve scientific progress and societal benefits. Both of these aims can be fulfilled, if it concentrates its efforts on **networking the specialised sciences**. Here the systems concept and other integrative concepts offer a set of conceptual and methodological instruments; there is a demand for this in science and society; and there is a prevalent recognition in science and in the public at large that systems science is suited for that and has sufficiently demonstrated its capacity.

Contributing to this main task of networking the specialized social and natural sciences is the task of finding more **integrative concepts**. Beyond the basic concepts of system, of feed-back, of information, of model, and their derivatives, a number of other concepts have come to the fore, e.g. self-organization, hierarchy, evolution, entropy. Also epistemology must be included. All these concepts are, to varying degrees, applicable to analysis and synthesis beyond the specialized sciences. But these concepts, and the systems concept itself, are also part of the latter, in the sense that they are legitimitely used there and **cannot be claimed as being the exclusive domain of systems science**.

When concentrating on the central function of networking the specialised sciences with the growing set of appropriate concepts, **systems science is not seriously questioned on epistemological grounds by the critical analyses** and evaluations included in this text.

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