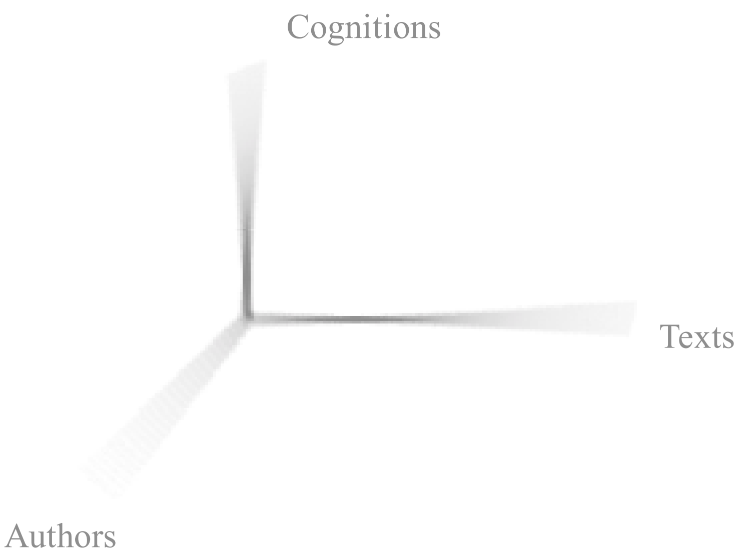
The Challenge of Scientometrics

The Development, Measurement, and Self-Organization of Scientific Communications



Loet Leydesdorff

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The Development, Measurement, and Self-Organization
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PREFACE

This study is the result of several years of collaboration as a scientometrician with philosophers, historians, and sociologists of science. It goes without saying that I am grateful to my colleagues in the Department of Science & Technology Dynamics of the University of Amsterdam for their relentless criticism and scepticism about scientometrics. In 1987, I had the opportunity to organize a workshop on the relations between qualitative theories in science and technology studies and the use of scientometric methods under the aegis of the European Association of Studies in Science and Technology (EASST), and to help edit a special issue of *Scientometrics* devoted to this subject (see: Leydesdorff *et al.* 1989). The program of study in this book is largely based on the research agenda that was formulated during this workshop.

Among the many colleagues, with whom I have discussed issues relevant to this study, I am particularly grateful to *Susan Cozzens* for several years of collaboration, and to *Michel Callon* and *Jean-Pierre Courtial* for discussions about the co-word methodology. In 1990, I spent some time as their guest at the Centre de Sociologie de l'Innovation of the École Nationale Supérieure des Mines in Paris. In the Dutch context, I wish to mention my colleagues *Gertrud Blauwhof*, *Peter Van den Besselaar* (Department of Social Science Informatics), *Wouter Van Rossum* (Groningen State University), and *Arie Rip* (Twente University) for discussions of scientometric methods and their theoretical interpretation. Finally, I am indebted to *Gene Moore* for correcting my English, but I remain responsible for any mistakes in the text.

Amsterdam, February 1995

Preface to the second edition

This second edition is not substantially different from the first one published by DSWO Press (Leiden University) in 1995. The text has been thoroughly revised, updated, and improved as necessary. I am grateful to Manfred Bonitz for spotting a number of errors and typos in the first edition. Among other things, I extended Chapter Ten with a new section about the implications of path-dependent transitions for firm behaviour and institutional agency (Blauwhof 1995; Leydesdorff and Van den Besselaar 1998).

The first edition has in the meantime been translated into Japanese by Yuko Fujigaki, Takayuki Hayashi, Hideyuki Hirakawa, Junichiro Makino, Masahi Shirabe, and Hiroyuki Tomizawa under the title *Saientometorikus no chôsen: kagaku-gijyutsu-joho no jiko-soshiki-ka* (Tokyo: Tamagawa University Press, 2001). The discussions with my Japanese colleagues were particularly intensive during the preparation of a special issue of *Scientometrics* on the 'Theory of Citations,' (Vol. 34, No. 1; see Leydesdorff 1998). In this context, I would also like to thank my colleague Paul Wouters for his contribution to what he has called *The Citation Culture* (Wouters 1999; Leydesdorff and Wouters 1999).

A further elaboration of my theory is available in *A Sociological Theory of Communications: The Self-Organization of the Knowledge-Based Society* (Universal Publishers, at http://www.upublish.com/books/leydesdorff.htm, 2001). The two books can be considered complementary in terms of providing theory and methods for the investigation of the knowledge base in processes of scientific communication and codification.

Loet Leydesdorff

Amsterdam,
February 2001

Chapter 1

Scientometrics and Science Studies

The tension between qualitative theorizing and quantitative methods is pervasive in the social sciences, and poses a constant challenge to empirical research. But in science studies as an interdisciplinary specialty, there are additional reasons why a more reflexive consciousness of the differences among the relevant disciplines is necessary.

First, the intellectual distance between contributions from the humanities, such as ‘history of ideas’ and philosophy, at one end of the spectrum of relevant disciplines, and from ‘scientometrics’ at the other end, is even more dramatic than in most social sciences; while an awareness of the differences in *methods* is particularly important because of the central position of the ‘philosophy of science’ in the constitution of the specialty. Second, in the past few decades, science studies has developed into an interdisciplinary specialty with its own journals, scholarly societies, and university departments. The consequent professional identity and ideology require a degree of integration of the insights from the various relevant disciplines, and the development of relatively independent and recognizable norms and standards in relation to neighboring disciplinary structures.

The span between disciplines which vary as much in terms of methods, standards and discursive styles as laboratory studies, intellectual history or scientometric indicators, is usually too large for the practice of empirical research. Within the framework of a single research project it will often prove inefficient or impractical to raise methodological questions concerning useful results from other disciplinary backgrounds. For example, historians who want to describe an intellectual lineage, and who may use patterns of citations or any other indicators to illustrate their arguments, are not usually interested in the possibility of clustering the same data with slightly different choices of methods into different structures that could shed further light on the object of study.

Decisions about provisional boundaries and methods are legitimate at the level of a project, or even at the level of an institutional program. However, intellectual exchanges at the level of the interdisciplinary specialty make a 'deconstruction' of the implied assumptions inevitable: what in one context appears as the practical assumptions of research may show a lack of sophistication and neglect of available knowledge when viewed from the perspective of another discipline. Without a common frame of reference, such discussions may easily disintegrate into priority disputes among participants from different programs and disciplines.

The commonality in the frame of reference in science studies has been formed mainly by a common interest in the subject matter, i.e., the development of the sciences. Theoretical integration has lagged behind because of the noted diversity among contributions from relevant disciplines. Efforts to integrate have taken the form of encyclopedic work, in which selections are made on the basis of pragmatic criteria, and collective efforts to produce handbooks, yearbooks, etc. (e.g., Spiegel-Rösing and De Solla Price 1977; Knorr-Cetina and Mulkay 1983; Van Raan 1988; Jasanoff *et al.* 1994).

Thus, it seems that interdisciplinary science studies are facing a dilemma. Theoretical justification is to be found in the various disciplinary backgrounds, while in debates within the specialty, these backgrounds can only function as legitimations for a particular approach. As soon as the approach is questioned, the discussion moves to a more philosophical level. But when one focuses on the results, the analyst seems to have no clear standards to evaluate them without provisionally accepting the approach. Capitalization on what the various contributions can teach us about the dynamics of science cannot be pursued systematically. This seems not a contingent choice: we actually lack methods for integration beyond the level of encyclopedic gathering. Indeed, the state of the art of science studies is 'pre-paradigmatic:' it is an interdisciplinary area integrated only at the level of its subject matter, and an applicational area for various contributing disciplines.

1.1 The Challenge of Scientometrics

As noted, the commonality in the intellectual enterprise of science studies is found in the commonality of the objects under study. Therefore, it seems appropriate to begin our search for a common framework at this end: what are the legitimate theoretical objects for science studies? What are the dimensions in which to phrase questions about them? How do we demarcate these questions from others which are not primary science-studies questions, although they may be of some relevance for the study of the sciences? These are epistemological questions concerning what should be considered as 'the world of science' as distinguished from other realms that can be studied in modern society.

The strength of the scientometrics program is its positive definition of science as an area of inquiry. The scientometric approach has often been reproached for its 'objective' pretensions (e.g., Edge 1979; Chubin and Restivo 1983). In my opinion, these pretensions are articulated with respect to particular methods and results, and one should not on this basis refute the challenge of scientometrics at the epistemological level, that is the claim that scientific developments are amenable to measurement.[1] I shall argue in this study that a multi-dimensional scheme like the one depicted in *Figure 1.1* can be used to describe this 'world of science.'

Along the three dimensions and their corresponding units of analysis, one can distinguish studies at various levels of

[1] The very possibility of defining science positively, and of making it subsequently an object of scientific investigations, is sometimes denied in the more reflexive tradition in science studies (e.g., Woolgar 1988).

aggregation.[2] For example, words are organized in texts, scientific articles in journals, journals belong to archives; scientists compose research groups, research groups belong to scientific communities; knowledge claims are based on theories, theories are embedded in disciplines. (One may wish to add more dimensions than the three indicated here.) The scheme suggests differences also in the nature of the dynamic processes along and between the axes (see, e.g., Holzner *et al.* 1987).

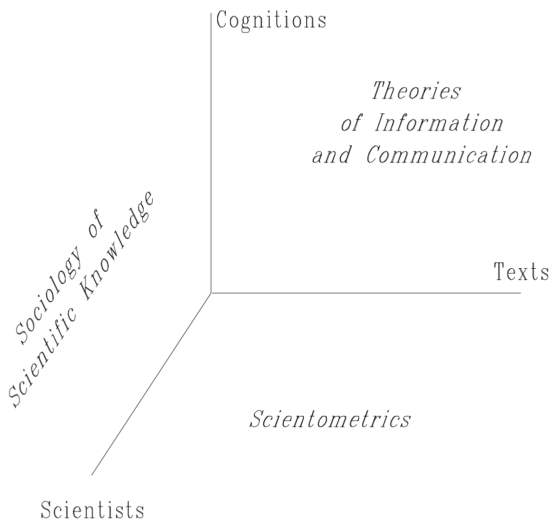

Figure 1.1
The study of the sciences as a multidimensional problem

In addition to a scheme which describes the types of objects and thus demarcates questions which we shall recognize as relevant to theorizing about the sciences, one is in need of a 'language' in which to study the phenomena within such a scheme. This 'language' should provide us with the methodological apparatus for describing this world coherently, despite the heterogeneity of the

[2] See for a similar categorization: Borgman 1989.

phenomena. Furthermore, the language should enable us to capture the core processes in scientific developments, and also guide us in the further choice of methods.

The pervasiveness of 'words' in science has previously led me and others to focus on words and co-words in such a comprehensive effort (Callon *et al.* 1986; Leydesdorff 1989b; Callon *et al.* 1993). In this study, I argue that 'information' is the more fundamental concept (cf. Mandelbrot 1968). The systematic processing of information in order to reduce uncertainty about the environment is the core process in scientific developments that the scientometrician attempts to map.

1.2 WORDS, CO-WORDS, INFORMATION, ENTROPY, SELF-ORGANIZATION

I proceed in two major steps: after a critical examination of qualitative and quantitative perspectives on science studies in Part One, a list of criteria can be composed for the methods which are needed for the development of science studies as an integrated enterprise. In Part Two, I shall show that information theory can comply with the listed criteria. By using this method, central problems in science studies will be addressed, both on the qualitative side (e.g., the significance of a reconstruction) and on the quantitative side (e.g., the prediction of science indicators).

'Information' (Shannon 1948) is yet content-free, which means that its content can still be defined at each level of aggregation, and in relation to the dimensions examined in a particular research design. Additionally, information as a measure is non-parametric, which means that we do not have to make any *a priori* assumptions concerning measurement scales or other mathematical idealizations (Krippendorff 1986). Furthermore, in its current formalization (Theil 1972), information theory is directly derived from probability theory, and, therefore, it is possible to relate results systematically to those of many other forms of social science statistics, and also to import results from the Bayesian philosophy of science. Finally, since all formulas in information theory are composed of simple summations, the use of these measures is highly appropriate with respect to the decomposition and/or aggregation.

The study of the sciences is so complex since the communication processes under study are multi-layered. Both the data and the (latent) structures in the data are in flux. Furthermore, the data can be considered as appreciations by scientists who are able to revise their interpretations reflexively. But changes in the data cannot be distinguished systematically from changes in the relevant dimensions–or more generally, changes at different levels–unless algorithmic methods of data analysis are used. Information calculus enables us to combine the multi-variate analysis of complex data structures (e.g., networks) with time-series analysis in a single design.

In Part Three, the study of possible irreversibilities in networks will lead me to the second major step in this study, namely to second-order systems theory, the theory of self-organization, and eventually to the specification of a mathematical sociology of scientific knowledge as a process of codification of scientific communication. It will be shown that the delineation of a complex unit of analysis from its contexts is a prerequisite for the prediction of the future behavior of the system(s) under study. This delineation of contexts remains necessarily hypothetical: structural developments are latent, and therefore, they can only be declared on the basis of an uncertain reconstruction. The analyst observes the interactions or 'instantiations' (Giddens 1979) of complex dynamic systems; the observations enable us to update our expectations.

1.3 ORGANIZATION OF THE STUDY

The scientometric conceptualization of science as a multi-dimensional construct that is susceptible to measurement is embedded in a philosophy of science. This philosophical position needs justification. The general organization of this study in different parts reflects this need of, on the one hand, justification and demarcation, and on the other, of methodological analysis and perspectives for empirical research.

Part One contains the theoretical justification of the program of the study. The multi-dimensional scheme is developed and assessed in relation to philosophical and sociological perspectives. In the next chapter, I analyze some major programs in the sociology of science, and show how methodological issues emerge as soon as sociologists do not limit their domain to the institutional dimensions of the scientific enterprise, but develop a sociology of scientific knowledge as well. I argue that in the sociology of scientific knowledge, important methodological problems have been reflected upon, but not yet been sufficiently clarified. Authors have coped with these problems by making strongly programmatic assumptions (e.g., Bloor 1976; Callon *et al.* 1983; Latour 1987a).

In my opinion, more rigorous distinctions among analytical dimensions, and between static and dynamic questions are needed. For example, the 'socio-cognitive' (inter-)action has become central in the new sociology of scientific knowledge.[3] The analyst, however, should disentangle the question of how the social and the cognitive dimensions co-vary in 'socio-cognitive' (inter-)action at any given moment in time, and the dynamic question of how action shapes and reproduces structure at a next moment. Both questions can be made subject to specification, and then be combined. The specific limitations introduced by programmatic assumptions with respect to the relations between these questions can also be specified.

One observes socio-cognitive interactions, but what these interactions mean can only be specified if one has hypothesized contexts in which these interactions can be provided with a meaning. The specification of the potentially different meanings of the interactions for a social context, a field of science, and/or other (e.g., subsequent) interactions, requires that one distinguish between a social variation, a cognitive variation, and a socio-cognitive co-variation or interaction term. The various effects cannot be expected to coincide, and thus, asymmetry prevails. Since sociologists of

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[3] See, for example, Pinch (1982), at p. 17: "Within this interpretation 'paradigm' is taken to be a term which emphasizes the combined *socio-cognitive* nature of scientific activity."

scientific knowledge have programmatically argued in favor of symmetry in explaining the effects of 'socio-cognitive interaction,' this analytical conclusion may have far-reaching consequences.

Among other things, *Figure 1.1* (above) has provided us with a scheme that implies the declaration of an *analytically* independent cognitive dimension. This assumption, however, has consequences for research designs and the interpretation of results. How can a cognitive unit of analysis (e.g., a theory) be delineated in empirical research? In Chapter Three, I analyze some of the major traditions in modern philosophy of science by focusing on the methodological question of what is being explained about science in terms of what. On the one hand, the cognitive content of scientific knowledge is made central in philosophies of science that rooted in or react to logical positivism (notably, critical rationalism). In this context, the cognitive dimension is made the essential 'why' of everything else in the scientific enterprise. Cognitive developments function both as *explanans* for what is happening in science in all other dimensions, and as a normative criterion for distinguishing between what is in need of an explanation as a contribution to science and what is not.

For example, when Lakatos (1970) discussed the choices made by Niels Bohr when he developed the model for the atom as a research program, he explained Bohr's choices with hindsight in terms of what we know to have become the accepted model of the atom. The behavior of the scientist as an actor–in terms of choices with respect to lines of research–was explained in terms of the cognitive development of physics. However, the behavioral aspects are circumstantial in Lakatos' philosophy of science, and therefore, the philosopher's aim is not to explain these aspects, but to use the historical examples as only an illustration of the reconstruction in the cognitive dimension. Similarly, an illustration in the realm of the relations among scientific texts would have been possible, and notably equivalent in terms of its methodological status. Both the behavior of actors and the texts may serve as circumstantial evidence for ongoing theoretical developments.

In the other main tradition of the philosophy of science–the (neo-)conventionalist one–there is no such methodological equivalence between the content of texts and the behavior of actors.

They have a different status; here, language is the medium in which science develops, and therefore texts and discourse have a privileged position in the explanatory scheme. Science is, according to Quine (1962), a fabric of fact and theory, and correspondingly, any logical gap between theory and observation is reduced to matters of warp and weft, that is to matters of degree within language, and not of kind (Hesse 1980). The Popperian asymmetry between basic statements grounded on conventions versus theories located in World Three (or, analogously, Carnap's distinction between observational and theoretical statements) is now explicitly denied. In the neo-conventionalist tradition, one cannot even talk about explaining cognitions in terms of discourse, since the distinction between cognitions and language is now problematic. The relations among language and community are the remaining focus of interest.

The denial of the possibility of a separation between the cognitive and the linguistic dimension in the conventionalist tradition of the philosophy of science may seem attractive from the point of view of designing empirical research projects. The reduction of complexity, which is then possible, has been empirically fruitful: the question of what constitutes cognitive structure, i.e., its epistemological or even ontological status, can be neutralized as beyond the scope of empirical research, and therefore relatively irrelevant. Among others, adherents of the 'sociology of translation' or the actor-network approach have built heavily on these philosophical positions (e.g., Law and Lodge 1984).

I shall argue at two levels *against* a sociological reduction of the multi-dimensional problem to only the two dimensions of the literary manifestations of the sciences and the perceptions by local actors or groups. In Chapters Two and Three my argument is formulated at the theoretical level, and in Chapters Four and Five it will be shown empirically why one runs into problems if one focuses exclusively on observables like words, their co-occurrences or human actions. By using the full texts of eighteen scientific articles in a limited domain of biochemistry it can be shown that variation among word distributions is a result of various types of variation–conceptual variation, semantic variation, etc.–which have to be distinguished (Chapter Five). This distinction among independent sources of variation reintroduces the analysis of latent factors as a problem. In addition to their identification at each moment in time, one can raise questions about how the various factors change over time, in relation to change in the observable data. As we shall see, the declaration of change in latent factors requires the use of an algorithmic *calculus*.

The discussion of the various programs will make it possible (in Chapter Six) to list the requirements for a useful methodology of science studies. In addition to more technical requirements, methods should, for example, allow for the use of qualitative data and dynamic analysis, and not be restricted to contributions from specific theoretical perspectives in advance. I argue that there is scope for the development of such methods: probabilistic reasoning, because of its extensions in such a wide range of relevant disciplines as, e.g., information theory, statistical decomposition analysis, loglinear modelling, and Bayesian statistics, offers a perspective to develop a single comprehensive framework in which contributions from a great variety of disciplinary perspectives on science can be absorbed.

In Part Two of the study, I demonstrate the strength of using the relatively simple statistics of information theory to study some major problems of science studies. In Chapters Seven and Eight a static and a dynamic analysis of relations among the eighteen texts used in the study of word-distributions (from Chapter Five) is pursued, using information theory. However technical these studies may seem, their implications for empirical science and technology studies are substantive. As soon as the phenomena to be studied can be specified in empirical terms, the proposed methods can be applied to address issues such as (i) how much each unit (case or variable) accounts for the variation, (ii) the effects of aggregation and disaggregation, and (iii) in the dynamic model, questions concerning reconstructions.

The exploitation of these advantages will lead me from Chapter Nine onwards to reflections concerning the distinction between complexes of data which develop as systems, and those which do not. Chapter Ten focuses on how to study systems not in terms of relations but in terms of operations. Irreversibilities in network structures ('path-dependencies' and 'emergence') are discussed in probabilistic terms. Theoretically, this enables us to operationalize concepts in the actor-network approach (cf. Callon *et al.* 1986; Chapters Ten and Eleven), and to add the time-dimension to the 'structural theory of action' (cf. Burt 1982; Chapter Twelve).

In Part Three, the notion of developing systems is addressed more explicitly. In Chapter Eleven, the impact of EC science policies on the transnational publication system in Western Europe is analyzed empirically: did a European system emerge in addition to the various national research systems? This research question reintroduces the multi-variate perspective in the dynamic analysis: if an actor-network is not one system, but a composite of separate systems (actors) in a network with potentially different operations, how then are we able to study the interactions between these (e.g., national) systems?

The assumption that the actors at the nodes can operate with relative independence from the operation of the network is a familiar model in parallel and distributed computing: each processor performs its own operations while the network runs a different program (e.g., Rumelhart *et al.* 1986; Bertsekas and Tsitsiklis 1989). This model is also used in artificial intelligence as a solution to the problem of a local update in the light of specific information (Pearl 1988). The methodological toolbox which we have created in Part Two provides us with an operationalization of a notion of structure as contingent, but in its distributed operation only dependent upon other contingencies insofar as the latter operate. Otherwise, self-referentiality (operationalizable in terms of auto-covariation) prevails. In Chapter Twelve, this program for empirical science studies is delineated from the Bayesian program in the philosophy of science, and from the use of knowledge representations in artificial intelligence.

Luhmann (1984) elaborated a model for society as a communication system. The social system is no longer understood as an aggregate of human beings, but as the system of links which is added to and contingent upon the nodes (i.e., individuals) that perform their own operations. Luhmann's (1990) sociology of science and Shannon's (1948) mathematical theory of communication share a common background in modern biology and non-equilibrium thermodynamics (e.g., Prigogine and Stengers 1979/1984; Maturana and Varela 1980; cf. Swenson 1989). However, they are not just another application of the principles of thermodynamics: they reveal with hindsight that the study of the sciences at the meta-level is itself part of the development of the sciences, and therefore can be linked with current developments in methodologies, i.e. the study of complex systems that are not in equilibrium. In a final chapter, entitled 'The Possibility of a Mathematical Sociology of Scientific Communications,' I specify these conclusions of the study in relation to other traditions in science studies.[4]

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[4] See for the elaboration at the level of the social system: *A Sociological Theory of Communication: The Self-Organization of the Knowledge-Based Society.* Universal Publishers, at http://www.upublish.com/books/leydesdorff.htm, 2001.

PART I

THEORETICAL PERSPECTIVES ON SCIENTOMETRICS

Chapter 2

Scientometrics and the Sociology of Science

Until around 1970, questions about the growth and dynamics of scientific knowledge belonged to the realm of philosophy. The central issue in the philosophy of science was the *validity* of knowledge (the 'context of justification'). The philosophical reconstruction, however, was analytically to be distinguished from questions about how that knowledge was being produced (the 'context of discovery'). The latter realm was believed to belong to the domain of the social sciences.

The link between the philosophical issue of the growth of scientific knowledge and the sociological quest for explanations of variance in observable distributions was in large part established by historians like *Price* and *Kuhn*, who were able to see the substantive developments in the wider contextual perspective of the institutional growth of the scientific enterprise. Price (1965) emphasized the relations between knowledge growth and document sets; Kuhn (1962) highlighted the relations between authors working within paradigms and the growth of knowledge.

Studies concerning the relations between document sets and groups of authors constitute a natural extension of the set of questions accessible to the multi-dimensional scheme which was introduced in the previous chapter (e.g., Crane 1969), although these questions may be less obvious from the perspective of writing the intellectual history of science. This extension, however, provided a bridge between bibliometric approaches and sociological theorizing in science studies (e.g., Griffith and Mullins 1972; Cole and Cole 1973). In particular, following the proposal by Small and

Griffith (1974; cf. Marshakova 1973) that co-citations be used as representations of how authors perceive their relevant environment, important research efforts were undertaken to combine bibliometric data with sociometric data in order to explain developments in the sciences (e.g., Mullins *et al.* 1977; Studer and Chubin 1980).

One factor underlying the subsequent development of scientometrics has been the rapid growth of information science in the 1970s. The systematic utilization of the *Science Citation Index* in the biennial *Science Indicators Reports* of the U.S. National Science Board and in the official R&D indicator series of countries like Australia and Canada increasingly raised methodological issues, such as the statistical characteristics of bibliometric data-sets (e.g., Narin 1976; Martin and Irvine 1983, Moed *et al.* 1985). The growth in the use of databases derived from the *SCI* and other scientific abstracting services has opened up a market for quantitative science-policy studies. Reliable statistics about science, involving time-series data, the extrapolation of trends, or 'picking the winners' (Irvine and Martin 1984), have become major objectives in the field (see, e.g., Van Raan 1988).

Growing acceptance of bibliometric data by scientists and science policy-makers, however, does not in itself provide a sufficient intellectual guarantee as to their meaning and importance. The use of ever more complex scientometric techniques in larger and larger databases may lead to a crisis of interpretation since, despite their potential policy relevance, we still have a theoretically underdeveloped understanding of what this bibliometric data actually means (e.g., Edge 1979; McRoberts and McRoberts 1987). In addition to questions about the theoretical significance of the indicators, the limitations to the use of various databases and indicators have been noted in many places (e.g., Woolgar 1991). The continuous call for a theory of citation in quantitative science studies[5] can itself be considered as indicative of the urgency to explore more systematically the relations between the use of scientometric methods and qualitative approaches.

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[5] See for a literature overview: Leydesdorff and Amsterdamska (1990). For the discussion of 'a theory of citation,' see Leydesdorff (1998).

2.1 UNITS OF ANALYSIS, LEVELS OF AGGREGATION, AND DIMENSIONS

With respect to the question of how to relate qualitative theory to scientometric methods the main problem now seems to be that we have, on the one hand, a set of indicators, techniques, and databases concerning the sciences, and, on the other, sociological theorizing which is not easily specifiable into models which can be operationalized and tested with the help of scientometric data and techniques. The scientometric models, however, as soon as they are tested outside the domains for which they were originally developed, seem to break down when confronted with the more complex interactions within the scientific system. In addition to its multi-dimensionality and its reflexivity, the scientific enterprise is organized at various levels of aggregation (e.g., Spiegel-Rösing 1973, pp. 106-131; Küppers *et al.* 1979; Studer and Chubin 1980, pp. 269f.), and under certain conditions it seems even possible at some of these levels to convert resources and results into one another (Latour and Woolgar 1979).

In order to address these complexities in empirical research, there are mainly two strategies: either one elaborates the differences or one reduces complexity drastically by equating units of analysis and by collapsing levels of aggregation. The latter approach is taken by what has been called the 'sociology of translation': in actor-networks the structural differences among social, cognitive, and natural units are explicitly denied, and it is postulated that each node of the network can again be composed out of another (and similar) network (Callon *et al.* 1983 and 1986). I return to this approach more extensively below.

In the terminology of quantitative studies it is more common to distinguish analytically among dimensions, units of analysis, and levels of aggregation. For example, performance measurements usually take an organizational unit of analysis (e.g., a research facility or a research group) as a point of departure in the analysis of science, while others (e.g., in social studies of science) tend to conceptualize science in terms of cognitive units of analysis or specific discourses (Collins 1985a). In bibliometric studies, document sets can be distinguished as yet another unit of analysis.

In my opinion, each unit of analysis in science studies is a composite of, among other things, cognitions, texts, and scientists.[6] These building blocks are different in their nature: one can not reduce scientists to their cognitions or a text to its author(s), nor can a cognition be equated with the language in which it is expressed. Observable units of analysis, however, are complex; complex units may be taken as objects in quite different types of theories, since the aggregation and organization of the composite may refer to different systems and their developments over time. Volumes of journals are, for example, specific aggregations of documents while journals are also social institutions.

Things can be grouped differently, and therefore grouping can be considered as a variable. Aggregation and/or organization imply the application of one grouping rule or another. The specification of this aggregating variable clarifies with hindsight that what has been grouped is also analyzable in terms of a distribution. Aggregation and organization is the result of an interaction between the grouped and the grouping dimensions. For example, if cognitions are organized into texts, or texts are organized in cognitive terms (e.g., research questions or scientific theories), the composite result can always be analyzed in terms of the grouped *and* the grouping variables. Complex phenomena should therefore *not* be analyzed only as aggregates of lower-level entities, but in terms of analytical dimensions and their interactions. The higher-level system contains more variance than the sum of the composing units; notably, the interaction between what is being grouped and how it is grouped has been added.

[6] Analysis in other dimensions (e.g., in terms of economic relations or policy relevance) is by no means excluded. However, while the relevance of the social, the cognitive and the textual dimension can be considered as given with the delineation of this object world, i.e. the sciences, other dimensions have to be introduced on the basis of additional theoretical considerations and/or specific research questions (e.g., in technology studies).

In summary, to the extent that we introduce cognitions, texts and actors as 'heterogeneous' building blocks which can be included in the networks, each composite is analyzable with reference to these dimensions. In the above example of volumes of (textual) journals the grouping variable(s) can be specified in cognitive and/or in social terms. At each moment in time, there may be co-variation among the various dimensions, and in the dynamic analysis each co-variation leads to an interaction term.[7]

Along the different dimensions one can distinguish studies at various levels of aggregation. Composite units of analysis of relevance to science studies, however, can usually be specified with reference to the three noted dimensions. Most scientometric studies will choose (groups of) authors and/or (sets of) documents as units of analysis, since studies which specify cognitive units of analysis require access to scientific development from a philosophical, historical, or another appreciative point of view. In the linguistically-oriented branch of the philosophy of science, scientific knowledge is increasingly conceptualized in terms of networks in language, which are considered accessible to empirical research using techniques from the information sciences (Hesse 1980).

Let us focus first on the two main units of analysis in current scientometric analysis: authors and groups of authors–classified using social and/or cognitive criteria–and documents and sets of documents. If we indicate configurations among authors or institutes, we are using scientometric data as input for an essentially *sociometric* analysis. One may make comparisons among authors, institutes, communities, etc., and analyze the underlying structure with the aid of various techniques of social network analysis (e.g., Freeman 1978; Burt 1982). The indicator–let us take citation as the prime example–must then be assessed in terms of what it might mean in terms of social network relations. Consequently, from such

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[7] It is another question whether, how, and to what extent we are able only to describe or also to measure in terms of these dimensions. Note that description requires categories, and is thus equivalent to measurement with variables at the nominal scale.

a study one may draw inferences about such typical sociological issues as performance, hierarchies, and group and elite structures (cf. Burt 1983).

When scientometric indicators are used in a bibliometric framework, other questions become central: for example, whether it is possible to ‘map’ the sciences and their developments. These questions link scientometric efforts to theoretical aspects of information retrieval, data representation, and, eventually, linguistics. Note how the perspective changes when we use citations–or, for example, a shared title-word–as an indicator of a relation between two texts, between the authors of those texts, or even between the citing author and the cited text (citation context analysis), or the cited author and the citing text (Leydesdorff and Amsterdamska 1990).

The sociometric tradition is not confined to the study of authors and their publications. Other social science methods (e.g., surveys, behavioral data) may add to the data which can be obtained by using scientometric techniques. On the one hand, behavioral (e.g., sociometric) data other than those gained from archival literature can be more important when we want to study how scientists actually produce science. The bibliometric analysis may, on the other hand, reveal regularities and patterns in scientific communication which are not consciously available to the actors involved–and therefore should not be asked of them–yet structure their behavior.

Moreover, in order to study the *dynamics* of the various structures, time must also be introduced as a dimension: what can count as a historical event has to be specified. For example, compare, on the one hand, the use of single publications as ‘units of event’ (cf. Holmes 1984) for the reconstruction of the intellectual development of an author with, on the other hand, a progressive problems shift setting a research agenda at the level of a scientific field (Lakatos 1970). The frequencies of these events are expected to be different.

2.2 THE SOCIOLOGY OF SCIENTIFIC KNOWLEDGE (SSK)

It has been a central message of SSK that the cognitive is the social and vice versa, and that everything can be analyzed in terms of discourse. The main argument for this has been that the various dimensions cannot be distinguished 'in socio-cognitive (inter-)action' which shapes the social *and* the cognitive at the same time.[8] Therefore, the analysis should not be pursued in terms of dimensions like 'cognitive' versus 'social' or 'internal' versus 'external' (Callon *et al.* 1983). In short, SSK has not accepted any *ex ante* disciplinary division of labor in terms of the subject matter of the scientific enterprise. In comparison with older traditions in the sociology of science, this was a major step forward (cf. King 1971): all units of analysis in science studies are considered to be both cognitive and social. This led to empirically richer descriptions of the world of science than those which could be given by the traditional approaches in sociology and philosophy.

Consequently, the distinction among different meanings of key concepts–'interpretative flexibility'–is widely accepted among scholars in SSK and related traditions. One can no longer get away with a description of, for example, a specialty only in terms of the organizational variables of a scientific community (cf. Crane 1969 and 1972; Whitley 1984). Nor is it sufficient to operationalize a specialty purely epistemologically as a set of theoretical questions linked to relations among observations, arguments and inferences (cf. Hesse 1980); nor can it be sufficiently described as only a body of literature or a communication structure. As with all the major concepts in science studies, it is necessary to place 'specialty' in the perspective of social structure, cognitive structure, scientific communication and literature.

However, the attribution of different meanings to the same concept has not been sufficiently understood in SSK. The same event (action or state of affairs) may have different meanings because it can potentially be situated differently in relation to different relevant contexts. As noted, one major consequence of this

[8] See for a review of SSK, e.g., Collins 1983a.

distinction is the potential absence of a one-to-one relation between the social and the cognitive significance of the same event. In my opinion, the contingency of the relationship between the 'social' and the 'cognitive' contexts of 'socio-cognitive interactions' provides us with an excellent starting point for empirical sociological research.

The assumption of the *analytical* independence of the cognitive dimension does not imply a return to older traditions in the philosophy and sociology of science. Traditionally, the two dimensions have been conceptualized as two separate domains–to be pictured in a spatial metaphor as parallel planes and to be studied by distinctive scholarly traditions, i.e., philosophy and sociology. However, analytical dimensions are orthogonal; they unfold a multi-dimensional construct in which it is possible to specify meanings for the very concepts of science studies (see *Figure 1.1*). The model then allows, for example, for terms like 'interaction' and dynamic 'feedbacks' among dimensions. In the remainder of this chapter, I shall show the fruitfulness of the distinction among dimensions in relation to unresolved methodological problems of empirical research in the recent sociology of scientific knowledge (SSK).

2.2.1 The delineation of the unit of analysis

In his seminal study of SSK, Bloor (1976, at p. 2) formulated that "knowledge for the sociologist is whatever men take to be knowledge." Consequently, the 'strong program' introduced the principle of symmetry of explanation: the same explanation should be used to explain both true and false knowledge since scientific knowledge could no longer be defined as 'true belief.' In discussing the example of mathematics, Bloor (1982) argued that even rules of logical inference derive their truth from social negotiation and human belief.

From this perspective, the unit of analysis has shifted unambiguously from scientific knowledge to (wo)men who *believe* in such knowledge. With reference to Durkheim's (1912) analysis of forms of religious life, Bloor (1976, at pp. 40ff.) consequentially denied the very possibility of distinguishing between the social and the cognitive with any analytical validity (cf. Bloor 1983 and 1984). There is no objective knowledge, but only subjective or communal belief. The various strands in the new sociology of knowledge share this epistemological assumption; they differ according to the type of sociological categories which are proposed as most appropriate for the explanation of these beliefs.

For example, micro-constructivists and ethno-methodologists (e.g., Latour and Woolgar 1979) focus on the social locus where processes of de- and reconstruction take place in science, that is, the laboratory bench. Others (e.g., Whitley 1984) propose to analyze social processes in science at the level of scientific communities. In my opinion, the sociological reduction of science to (wo)men and their social institutions can be heuristically fruitful, but it eventually leads to confusion (cf. Slezak 1989; Henderson 1990; Collins and Yearley 1992).

Is it possible to delineate a unit of analysis of science studies in social terms? Let me use the concept 'research program' as an example in order to show how ambiguities in the operationalization of this concept in empirical research relate to unnecessarily programmatic choices in demarcating the unit of analysis. In the philosophy of science, 'research programs' have been considered the basic building blocks for the rational reconstruction of science (Lakatos 1970). In this tradition 'research programs' are only defined as cognitive units of analysis. Whenever Lakatos discusses the attributes of these 'research programs'–e.g., their progressive development or their degeneration–the discussion remains exclusively at the level of the cognitive field. Therefore, Lakatos' 'research programs' are not confined to socially identifiable units of analysis, like research facilities, sets of journals, etc. Moreover, the social locus of a research program may change historically, for example, when the center of activities in a research program moves from one country to another, or when the further development of the research program requires other institutional provisions.

In a social science design, one cannot accept this exclusiveness of the cognitive perspective. Science is shaped in departments, in laboratories, in research facilities. These social units have their research projects and programs for socially contingent reasons. Although these institutional research programs are related, in one way or another, to the development of the discipline, or to the research program in a Lakatosian sense (that is, at the level of the scientific field), they are not identical with it. An institutional research program is analytically distinguishable from a research program at the level of the relevant field.

As long as knowledge production is analyzed exclusively in terms of individual scientists' practices, the analysis can move back and forth from the scientist's institutional role to his/her intellectual position at the field level, as manifest from his/her oeuvre. The scientist under study is then the system of reference. But as soon as one moves beyond this micro-level of the individual actor, a grouping variable is necessarily implied in the aggregation. The variation in the aggregate is the sum of the variation in the units plus the 'in-between group' variation. (The 'in-between group' variance contains the mutual information between the grouping and the grouped dimensions; see Chapter Nine.) Thus, we may aggregate the set of cognitive rules and contents in a social institution using social demarcations to something which we may call 'the cognitive structure' of that social unit,[9] but the result will usually not be the same as the position of that social unit in the cognitive structure at the level of the field. In the latter case, one applies another grouping rule.

2.2.2 Whitley's (1984) solution

In his study precisely entitled *The Intellectual and Social Organization of the Sciences*, Whitley (1984) proposed to study the problem of the noted transition from field to social institution as a multi-level problem, by defining the scientific field in terms of scientific communities. Scientists are organized in reputationally

[9] Note that 'cognitive structure' is commonly defined in this way in studies in the tradition of social network analysis. See, for example: Krackhardt (1987).

controlled work organizations, which are distinguishable from their local organizations, and which then constitute the field. Indeed, the scientific community and localized work organizations are both social units of analysis, and can therefore be related within a sociological framework (Shinn 1982).[10]

The noted problems of delineation return as soon as one tries to operationalize Whitley's variables. Both the dependent variables (i.e., reputational structures) and the independent variables (i.e., organizational characteristics like task uncertainties and mutual dependencies) are affected by ambiguity in their definitions.[11] In my opinion, the question of how these variables relate to the 'cognitive context' is pervasive throughout the study. The ambiguities refer to the concepts, and not only to their operationalization.

Let us take, for example, the problem of delineating 'chemistry' and 'chemical engineering' as disciplines. Informed people would not deny that these two disciplines are strongly related. In some universities they are organized within the same faculties, chemical engineers may publish in chemistry journals, etc. However, there are also important differences, for example, with respect to such factors as relations with industry, the scale of experiments, the use of mathematics, etc. A description which aims to explain the intellectual organization of the sciences should enable us to account for these differences as well as for the similarities.

Using Whitley's scheme, 'chemistry' and 'chemical engineering' would have to be positioned rather differently, since considered as cultural work there are important differences in terms

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[10] See for an operationalization of the scientific community also: Crane (1969) and (1972).

[11] The definition of the dependent or independent status of variables can be found, for example, at p. 11 of Whitley (1984): "This means that the outcomes of research tasks are inherently different and uncertain and the level of task uncertainty in the production system as a whole is greater than in most other work organizations. This in turn *leads* to a particular structure for organizing and controlling research which I term the reputational system." (italics added, L.)

of the strategic and functional aspects of their organization. For example, if one wants to practice chemical engineering, one needs access to industrial processes and one cannot make do with test tubes alone; and this will have important implications for the types of institutional organization researchers have to engage in, both at the lab site and in the institutions of the scientific community, since the attainment of success and therefore reputations is dependent on this relation (Shinn 1982; Van Steijn 1990). However, despite these different arrangements, knowledge contributions and scientific theories may (or may not) be shared with other branches of chemistry. The independent (organizational) variables may be largely circumstantial in terms of the development of the knowledge system.

In other words, *there is no necessary relation between how disciplines are cognitively organized and how they function as work organizations.* Of course, there may be an empirical relation (Shinn 1988). But even if organizational forms and cognitive contents co-vary, one is not allowed to infer any causality from the relations between the two. On the basis of the correct (epistemological) assumption that the sciences are socially and historically constituted, Whitley (1984) has mistakenly drawn the methodological conclusion that the intellectual organization of the sciences can be analyzed sufficiently in terms of the fine-structure of their social and historical organization.[12] However, one should clearly distinguish between how the variation in the knowledge content of science is partially determined, and how it is also partially conditioned by its social history. As noted, *the social and the intellectual organization of the sciences determine each other in socio-cognitive interactions, but they only condition ('enable and*

[12] Whitley (1988, pp. 52f.) summarized his epistemological and methodological assumptions in a German publication, as follows: "a) Die grundlegende Vorstellung zu dieser Problematik is die These, dass wissenschaftliche Erkenntnisse von - unter verschiedenen Bedingungen unterschiedlich organisierten - Menschen produziert werden, (...)", and then: "c) Die soziale Organisation der Wissensproduktion und -validierung *bestimmt* die Forschungsstrategieen und -resultate." (italics added, L.)

constrain') each other in the remaining variations (i.e., given the interaction).

Consequently, Whitley's (1984) study has led to confusions such as that scientific work was supposedly controlled by rewards and reputations, as if these were not proxies for contributions to the solutions of scientific problems. Proxies or indicators should not be equated or confused with the parameters of the system(s) under study.

2.3 DISCOURSE ANALYSIS

In addition to the risk of reducing science to only its associated social processes, and consequently of talking no longer about science and its progress, but only about scientists and their beliefs, reputations, and interests, the program of the new sociology of scientific knowledge became entangled in another methodological problem, when its adherents began to pursue empirical research. In a programmatic article, Mulkay *et al.* (1983) argued in favour of the *methodological* priority of discourse analysis on the basis of flaws in earlier work in science studies.

Mulkay and his colleagues argued that the initial assumption of 'symmetry' in the strong program, i.e., symmetry in explaining true and erroneous beliefs, is untenable when one studies the sciences empirically as forms of discourse. Scientists apply at least two repertoires, one of which is contingent and the other which these authors call 'empiricist.' Significantly, beliefs taken to be correct are expressed more in the 'empiricist' repertoire, for example, in formal scientific literature; while scientists tend to use more contingent repertoires in order to account for allegedly incorrect beliefs:

> Criteria are presented as constituting a clear-cut, impersonal, unavoidable constraint on the choice of correct theories; whilst the same criteria are much more likely to be depicted as socially contingent and malleable when they are cited in connection with incorrect theories. (*Ibid.*, p. 198.)

Why do scientists *asymmetrically* attribute certain types of beliefs and the corresponding verbal behavior to certain arguments and not to others? Why do they act as if they believe in science? External to the framework of discourse analysis, the conclusion seems rather obvious: once one reintroduces 'asymmetry' between beliefs which are believed to be correct and those held to be erroneous, the important next question is whether the fact that a belief is held to be correct, may be a useful indicator for the correctness of this belief. However, the question of the warrant for this belief has been the central question in the philosophy of science, and therefore one either has ultimately to accept the *not exclusively* social character of the issue, or to adopt a (hyper-)reflexive position in which in the end there is nothing but belief, and therefore no such thing as objective science (Woolgar 1988).

Actually, Mulkay *et al.* (1983, at p. 198) noted that scientists involved in controversies will tend to raise the proportion of contingent elements in their discourse, while 'the discourse forthcoming from less lively fields has proved to be comparatively intractable to sociological investigation.' These authors, however, did not infer from this variation the possibility of another (e.g., cognitive) factor which might help to explain it.

2.4 THE SOCIOLOGY OF TRANSLATION

A radical answer to the declaration of the various levels of aggregation in the various dimensions as a problem for science (and technology) studies has been proposed by the predominantly French school of the 'sociology of translation.' In a programmatic article Callon *et al.* (1983) focused on the problem of the heterogeneity of units of analysis in the sociology of science and technology. These authors (*ibid*., p. 193) argued that distinctions such as internal versus external, or cognitive versus social, can and should be overcome by using the notion of 'translation:'

> Translation stands for all the mechanisms and strategies through which an actor–whoever he may be–identifies other actors or elements and places them in relation to one another. Each actor builds a universe around him which is a complex and changing network of varied elements that he tries to link together and make dependent upon himself.

Note that actors may tie into their actor-network 'actors or elements,' i.e., they may also use non-social units as elements in order to stabilize their networks. Since "scientific observation is an activity in which social and cognitive factors are so intertwined that it is impossible to distinguish between them" (*ibid.*, p. 193), the relation between social and non-social 'actants' is said to be mutual and symmetrical. The distinction between 'social' and 'cognitive' disappears when everything and everyone is transformed into an 'actor-network.'

For example, in a study of the introduction of scientific principles of breeding in fishery, Callon (1985) argued that the actor-network consists of the oceanologists who try to transform fishing into 'aquaculture,' the science of oceanology which imposes a problem-formulation, the fishermen who defend their interests, and the scallops who breed and swim into the networks (or not). The thesis in the sociology of translation is not that the cognitive or natural constraints on the situation might be analyzed *as if* they acted upon the situation–that is, as a heuristic device–but that every unit should be analyzed in substantively similar terms. The 'actor network' is proposed as the single unit of analysis, into which all other units of analysis in all relevant dimensions are homogenized by definition.

Thus, the 'heterogeneity' is not addressed in terms of analytically different dimensions, but in terms of an assumed coincidence and congruity within the subject matter. The congruence of the different dimensions in the empirical category of an 'actor-network' is guaranteed on *a priori* grounds. Since one knows *a priori* that the relations in the actor network are mutual and symmetrical, nothing can be explained; the sole purpose of the analysis is to tell a story (Latour 1987a; cf. Collins and Yearley 1992). In other words, the 'actor network' is not only an *empirical* category; it is also an answer to the methodological problem of 'heterogeneity,' and therefore, a *methodological* category. The program thus reinforces itself.

Texts, and more particularly words, occupy a central position in this homogenized world of heterogeneous entities. In the sociology of translation, texts are 'inscriptions' which may function as carriers of symbolic relations to other actors. Since they may influence the situation as such, they can also be considered as 'actants' which then add to the actor-network. Additionally, the texts are conceptualized as a reflection of a specific actor-network. Therefore, as Law (1986, at p. 81) formulated:

> The words of a scientific paper may thus be seen as a *network of problematisations* which stand for an actor-network, (...)

The reflection in this so-called 'semiotic' network of texts is assumed to be perfect, since all relations are declared as symmetrical on *a priori* grounds. The dynamics among the words should then provide the analyst with access to the dynamics of the actor-network; one is supposedly allowed to infer from the one to the other, and vice versa. For example, 'successful words' in terms of the co-word maps can grow to become macro-actors if they manage to subsume many more words, in a hierarchical structure. The scientific field is at the same time the macro-actor who coincides and is congruent with this representation.

In addition to this *a priori* congruence among the various dimensions in the sciences, the various levels of aggregation are considered methodologically equivalent. Actor-networks are nested: some actors behave as macro-actors on behalf of groups (Callon and Latour 1981); some words represent underlying clusters of words at higher levels of aggregation, and are therefore macro-terms (Callon *et al.* 1983 and 1986). However, the macro-actor is itself again an actor-network, as is each actor in the lower level network. Actor-networks are conceptually equivalent, and hence their decomposition and/or aggregation is considered unproblematic. All grouping rules and constraints are themselves part of the network. The very notion of a contextual variable is at odds with this networks conception: everything is in the network on *a priori* grounds.

2.5 NETWORKS OF ACTORS / NETWORKS OF WORDS

Historically, the claim that one might use co-words as operationalizations of actor-networks emerged only gradually in the 'sociology of translation.' Law and Williams (1982, at p. 554), for example, noted that "(i)t is a mistake of much discourse analysis to ascribe power to the words themselves. It is rather *people* who operate with, and alter, these networks."[13] Later, the texts were seen as the medium *par excellence* through which the specificity of the actor-networks in the science system, i.e., networks consisting of problem formulations, can be studied. The claim was voiced that the study of words and their co-occurrences could give us access to a 'cognitive' (Rip and Courtial 1984) or 'qualitative' (Callon *et al.* 1986) scientometrics, that is, a form of network analysis which adds the cognitive dimension to the social network analysis. The textual structure was then conceptualized as the medium in which scientists realize the problem transformations. The symmetry between actor-networks and co-word networks additionally justified the direct policy relevance of the computerized devices of co-word analysis like LEXIMAPPE and CANDIDE (see, e.g., Callon *et al.* 1989; cf. Leydesdorff 1992a).

Several scholars in the tradition of social studies of science have criticized the 'sociology of translation' for dropping such important distinctions as social/cognitive, social/natural, cognitive/natural, by equating all these elements as part of the actor-network (e.g., Amsterdamska 1990; Fujimura 1991; Collins and Yearley 1992). Hitherto, the debate has not been fruitful. Since as noted the concept of the actor-network in the sociology of translation has both a methodological and an empirical status, critique voiced at the substantive level cannot affect the methodological core of the program.

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[13] See Callon (1985) for a typical study in the sociology of translation, which did not yet use co-word maps.

However useful the programmatic unity of the 'sociology of translation' may be to those who work within this framework, I think that its effects on analytical clarity have sometimes been dramatic. Every useful concept can be reintroduced into this program, and thereafter redefined from its perspective. For example, the 'density' and 'centrality' of words in co-word maps mean something different in Courtial (1989) from the same concepts in the social network analysis tradition (e.g., Freeman 1978; Burt 1982), allegedly since these dimensions are now defined not only technically but also strategically in relation to actor-networks. The problem of interpretation, and eventual validation, is by definition equated with the sociological question of whether the resulting pictures can be made meaningful, and whether they can be made useful in the translation process or for its description.

People, however, may find pictures meaningful and useful which are based on the *wrong* type of statistics (cf. Whittaker 1989; Leydesdorff 1992a). One has to provide the scientometric legitimation in terms of the more rigorous terminology of quantitative studies. However, in this confrontation a legitimation gap arises in the actor-network program: bibliometric and sociometric data are expected to be differently distributed, and to exhibit different dynamic patterns. On *a priori* grounds, one would not even expect symmetry and coincidence to prevail: the social system of science encompasses more than knowledge production as reflected in texts, and the organization of texts. Relations within and among texts also obey rules other than those of language users (notably, those of language itself). The dynamics of texts (in terms of co-occurrences of words) may coincide with the dynamics of social action or with the dynamics of problem formulations, but they do not necessarily have to. When authors working within this tradition claim that the congruence and coincidence of the actor-network with the co-word network is precisely what constitutes science, the reasoning is circular: first, one has to show empirically that the dynamics in the two dimensions are congruent. Bold assumptions at the epistemological level do not solve the problems of empirical research.

2.6 CONCLUSIONS

The serious problems which were signalled in the sociology of scientific knowledge were dissolved in the sociology of translation by abandoning the explanatory purposes of the previous program. I have argued that both in the sociology of translation and in discourse analysis the problems of asymmetry and multi-dimensionality return as soon as the issue of the validity of the inferences is raised with reference to empirical explanations.

In the post-modern tradition, these problems have been made the subject of subjective reflexivity (e.g., Woolgar 1988). In my opinion, the increased attention to reflexivity in the sociology of scientific knowledge should be understood as a response to the failure to solve certain methodological problems of empirical research concerning the analysis of reflexive discourses.

Indeed, the sciences have been socially constructed. But this is a meta-theoretical insight: it is true by definition. In any empirical design, however, the socio-cognitive edifice of science is only *partially* reconstructed by socio-cognitive interactions during the period under study.

The formulation of this empirical problem returns us to the classical issue of socio-cognitive differentiation over time (Guice 1994). A post-modern explanation, however, should differ from grand theory in that it remains constructivist and empirical. Thus, socio-cognitive differentiation can no longer be considered as an ontological given; if differentiation has occurred historically, it must be explained.

On the one hand, what is represented is not immutable like an eternal truth: the *a posteriori* contexts are not necessarily a reproduction of the *a priori* social and/or cognitive dimensions of the socio-cognitive interactions. (The axes may have been rotated by the interaction.) On the other hand, one expects the emerging system to select those socio-cognitive interactions which contribute to its reproduction as a differentiated system, for evolutionary reasons (cf. Simon 1969). In other words, the knowledge claims produced in the local network are selected (or rewarded) in the global network. The (sub-)systems interact *and* are dynamically sorted by the interactions (Simon 1973; Luhmann 1990).

Thus, the dynamic analysis provides us with an additional argument for specifying a cognitive context that is analytically independent. The actors involved in the interaction have some room to manoeuvre by using what has been called 'interpretative flexibility' (Mulkay *et al.* 1983). But how flexible can the interpretation be? When does such twisting become a transgression? One is simply not allowed to infer from the possibility of historical change the non-existence of cognitive standards. Indeed, reflexive analysts may with hindsight draw conclusions other than those of the scientists who generated these insights. But one can draw such conclusions only on the basis of the acknowledgement of a cognitive dimension that is analytically independent.

The sociological perspective has taught us–with a wealth of historical evidence–that science and its progress are heavily dependent on material and social conditions, and therefore that scientific results may vary in relation to these conditions. However, the 'epistemological' claim of 'the social construction of knowledge' made by authors in the new sociology of scientific knowledge is at best valid as a heuristic device in empirical research. Alternatively, one might propose as an analogous heuristic that we study the social organization of science in terms of its cognitive constitution, e.g., explaining the development of research groups in terms of theory developments. Although the two designs can be considered as 'symmetrical,' substantive results are expected to be completely different, since the cognitive variation and the social variation coincide only in the socio-cognitive co-variation (cf. Slezak 1989).

The specification of the uncertainty in and the organization of the knowledge contents in empirical terms is preliminary to the study of the interactions of this variation with the variations in institutional settings or discourses. Thus, in addition to the challenge to integrate the dynamics governing the relations among scientists, texts and cognitions into a comprehensive framework, we are in need of a conceptualization of, and perhaps a semantics for, the cognitive dimension of the scientific enterprise. Are we able to specify independent operationalizations in the cognitive dimension which allow for the study of interactions and mutual contingencies without *a priori* reduction of the complexities of these relations?

Chapter 3

The Intellectual Organization of the Sciences

If one accepts the conclusion of the post-modern sociology of scientific knowledge that truth can change historically and with context, should scientific truth itself be considered perhaps as a variation containing an uncertainty, and not an identity? How can one understand variation in the cognitive dimension in relation to notions that have been handed down to us by philosophers who have analyzed the cognitive structure of science?

The analysis of variance is not a familiar concept in the philosophy of science. Cognitive units of analysis seem specifiable in *empirical* research only in terms of criteria and rules which specify whether or not a scientist, a research group, a paragraph, or a citation is relevant to the development of a given knowledge system. What, then, might such rules mean in terms which are eventually amenable to empirical analysis? Are these the grouping rules in the aggregation, which we encountered in the previous chapter? And if so, why? How can one reformulate this philosophical question in terms of a sociological design which could be amenable to empirical research?

Many social scientists will readily agree with the quality of the distinction between social units of analysis, like human agency, and textual units of analysis, like sets of documents. In a research design one either attributes articles to authors or authors to articles, according to the type of question one wishes to raise. How should one attribute observable articles and authors to changing theoretical positions, problem formulations, substantive research programs, or scientific specialties? Can one attribute, for example, a text and its author to a more complex unit of analysis, of which they themselves are a part? The composite would then have to be considered as a superstructure.

Pinch (1985) introduced the concept of 'evidential contexts' for this superstructure. Various aggregation rules are possible, and therefore the delineation of 'evidential contexts' in cognitive terms poses a problem for the empirical researcher. For example, as noted in Chapter Two, the aggregate of individual cognitions in a social institution to something which one can then attribute as a 'cognitive structure' to this social unit, is not the same as the position of this social unit within the cognitive structure at the level of the scientific field. The system of reference is different in each case. Correspondingly, the dynamics may also be different.

Thus, the cognitive unit of analysis in science studies cannot be retrieved unambiguously in terms of observables. The *specific arrangement* of the texts and authors constitutes the cognitive unit in such a case. This arrangement is based on a 'virtual' organizing principle which can be hypothesized, but only observed by implication. In other words, the grouping rule implied in the specification of the empirical domain can be made reflexive, and can then be formulated as an expectation. With reference to cognitive structure the observables can then be considered as 'instantiations' (Giddens 1979).

The notion of such virtual, that is, not directly observable, organizing principles is nothing new to sociology: it lies at the origin of, for example, Weber's 'Idealtypes,' and has figured also in the sociological study of values in processes of institutionalization (e.g., Parsons 1951). However, in modern sociology of science, the 'opening of the black box' of cognitive structure in science had deliberately been put on the agenda so that these functionalist approaches would become inadequate (see, e.g., Barnes 1969; Whitley 1972; Collins 1983b). The hypothesized 'organizing principles' not only have functions, but they are themselves in turn to be understood as historically contingent, and therefore analyzable with sociological methods. Additionally, the functions themselves are dependent on the nature of the various sciences, and therefore potentially specific to each of them.

What access can empirical research gain into 'cognitive structures' which have been so defined? How can this notion of variation and change in latent dimensions be understood and operationalized so that one can, for example, systematically study its co-variation with other variations (e.g., in language)?

3.1 KNOWLEDGE AND LANGUAGE IN THE (NEO-)CONVENTIONALIST TRADITION

A plea for the analytical independence of the cognitive dimension seems to move in the direction of realist positions in the philosophy of science, like critical rationalism and neo-positivism. However, the problem with these positions is that they eventually imply a notion of 'cognitive structure' as having an ontological status (for example, in Popper's World Three). In sociological terms, such a rationalistic reification of science in terms of results would lead us back to Merton's functionalism, and that is not what we intended (see above). In addition to independence, the notions of variation and change should be kept firmly in the perspective.

An obvious candidate for a philosophy of science which stresses these elements is the conventionalist tradition which bases itself on the *Quine-Duhem thesis*. In this tradition (Hesse 1974 and 1980), science is conceptualized as a network structure. Not only do these networks change continuously, but also what counts as a knot and what as a link may change together with the perspective. General principles like 'coherence' and 'consistency' reinforce structures provisionally among the many possibilities. However, cognitive structure is in a continuous flux, while new knowledge flows in and other knowledge elements become obsolete.

This philosophy of science denies a 'correspondence' relation of language to the world: the understanding of the world is considered *relational within* language and theory. Consequently, the qualitative demarcation problem between the sciences and other interpretations of the world is denied, and the analytical independence of the cognitive dimension is again tendentially at risk.

On the one hand, Hesse's networks are to be found empirically in language, but as philosophical networks they are considered relational to theory at the same time. Therefore, they cannot be reduced to language only. A 'knot' in Hesse's networks has a different epistemological status from that of a (verbal) 'link.' On the other hand, the possibility of a distinction between the theoretical and the linguistic fabric of the sciences is explicitly denied on *a priori* grounds. What has been a 'link' at one moment in time, may have to be considered as a 'knot' at another moment. Furthermore, the demarcation between 'belief in truth' and 'truth' is also problematic, since we do not have scientific access to reality other than through conventions.[14]

What does it mean that the world is "relational within language and theory," if the two concepts are related on *a priori* grounds? In my opinion, a consistent reading of this philosophy of science implies that the world can be understood as an event in the interaction between language and theory. Thus, language and theory co-vary ('relate') dynamically; this co-variation is considered as an event analogous to the 'socio-cognitive interaction' in the previous chapter. However, as noted, the notion of co-variation implies possibly remaining variations in either dimension. Correspondingly, scientific statements can be expected to have both a semantic and a theoretical meaning. From an empirical–as opposed to a philosophical–perspective, however, these two meanings do not have to coincide.

3.2 THE EPISTEMOLOGICAL PRIORITY OF SCIENTIFIC METHODS

Hesse's neo-conventionalist position in the philosophy of science is antithetical to the tradition of 'critical rationalism:' conventionalism tends to deny the objectivity of scientific

[14] The application of the network concept of Hesse in co-word analysis can therefore be legitimated with references to passages in Hesse's work (cf. Law and Lodge 1984). I return to the issue in Chapter Four.

knowledge, which critical rationalism sees as crucial; and the latter tends to deny the variability of cognitive structure, which the former sees as essential. These two traditions stand at opposite ends of the spectrum which modern philosophy of science has to offer. How then, can one maintain that (i) one should conceptualize cognition as a qualitatively different, independent dimension, and that (ii) one can speak meaningfully about variation in this dimension as well? Such a position seems at odds with both these philosophies of science.

In an article on philosophy of science, Gellner (1985) has argued that precisely these two contradictory philosophies–Gellner took *Popper* and *Quine* as their most outspoken representatives–share a common assumption, namely that they "naively and complacently assume that a basically sound intellectual tradition is our ever-present birthright." However, as Gellner (1985, p. 18) argued:

> The facts of the matter are different, and rather sadder, alas, than either Quine or Popper realizes. All is not at all well with most cognitive traditions. Most are stagnant. The Cosmic Exile, or subjectivist empiricism, which both these thinkers spurn, was probably essential for the establishment, or at any rate the philosophical ratification, of that healthy, cumulative cognitive tradition which both of them take far too much for granted. It is *not* our birthright. Its emergence was a miracle.

Although not our birthright, modern science is our cultural heritage: its constitution has been socially achieved, and its history is accordingly linked to the history of Western philosophy. At whatever point in this history one starts the discussion–with the ancient Greeks, Galileo and Descartes, Kant, Marx, or Popper–the central questions in Western philosophy have always been: how can we know what we know? how can we find out about the truth of our thoughts? and how should we proceed to become more knowledgeable? Conceptually, these are epistemological and methodological questions, and they have been reflected philosophically.

The modern sciences are based on the epistemological assumption that truth is not a given, but that it can be discovered by discursive reconstruction. Nature is not 'out there,' shouting 'no' to our experiments; on the contrary, one is sometimes able to improve one's understanding by careful experimentation given a previous understanding (Kant 1787). Data are not a given, but always problematic, if only because of the possibility of error in measurement.

The questions of the quality of the data and the significance of the observations ask for methodological warrants of the theoretical inference. Thus, scientific methods are among the independent factors in the social construction of the modern sciences. Only since the early decades of this century, when logical positivism claimed a privileged status for scientific knowledge, have issues of demarcation and of scientific method gradually become the subject of a separate specialty, i.e., the philosophy of science.

The differentiation between a) the study of scientific methods as the primary mode of reflection on what the sciences are about, and b) the practices of science–where these same issues are embedded in questions concerning the quality of substantive results–has obscured the perception of fundamental relations between scientific developments and the historical development of society and culture. The methodological reflection has tendentially lost substantive content, while the positive sciences have parcelled out their need for philosophical reflection.

Because of this division of labor, it has become more difficult nowadays to grasp the intrinsic relations between scientific debate and social developments. If we try nevertheless to express these relations philosophically, we seem to be able to do so only too generally, that is, without precise substance, or else too trivially, that is, without philosophical sophistication. The development of the philosophy of science into a separate specialty has to some extent alienated questions of scientific method from the substantive development of the sciences (cf. Feyerabend 1975).

Scientific discourses, however, are *at the same time* debates about the methods appropriate to warrant the evidence adduced. Scientific methods are embedded in scientific practices; they are not 'meta,' but 'epi' to the sciences. The development of the sciences, of their relations with society, and the reflection on both are all processes in flux with relations among one another. We need to consider all these elements in order to understand the sciences as contingent developments without the *a priori* reification of any of them.

3.3 SCIENTIFIC METHOD AS A FUNCTION OF SCIENTIFIC DISCOURSE

What is this scientific method if it has no ontological warrant outside the epistemic networks that we discursively construct? Although philosophers of science may disagree about the epistemological status of scientific method, their philosophies can be analyzed without exception in terms of rules and/or criteria which define such important things as the theoretical object of a science–what is to be explained in terms of what–or what should count as evidence in order for an argument to be true. For example, a positivist will stress that only 'verifiable facts' should go into the premises of a science, while a conventionalist may claim that scientists strive primarily for coherence and consistency in the body of scientific knowledge. This procedural definition of scientific method, however, is still too abstract for the constitutive meaning of it to be grasped.

Standards and norms are not externally given, but functionally and reflexively developed in scientific communication. Their selective application to an argument determines whether this argument will be considered as true. In defining its object in terms of rules and criteria, scientific discourse develops a reflexive code as a second criterion of selection of the subject matter in addition to substantive ones. The balance between substantive and reflexive selections in discourses is an evolutionary achievement, which can sometimes be provisionally stabilized (Leydesdorff 1993a).

The provisionally stabilized paradigms and/or other elements of cognitive structure are expected to develop, and thus to exhibit an evolutionary 'life-cycle.' (Consequently, scientific discourses can be considered as systems of translation). What one observes at any moment in time, can then be considered as a consequence of the operation on the representation (*ex post*), or as a condition of the next operation (*ex ante*). This *virtual* operation of cognitive structure can correspondingly be specified only as an expectation.

Specification of the expected operations provides us with a frame of reference for the analysis of observable instances. Additionally, it makes us reflexive about the hypothetical character of the grouping rules (perhaps implicitly) used in the delineation of the relevant domains.

3.4 THE SOCIAL SCIENCES

The discussion above is based on the philosophy of the natural sciences. In the social sciences, questions of methods are even more pervasive than in the natural sciences, since any idealization seems to imply a loss of perspective on the richness and complexity of the historical process. However, this makes idealization no less important than in the natural sciences. As noted, without grouping rules the empirical researcher is not able to delineate the unit of analysis. The additional complexity in the social sciences mainly casts doubt on the possibility of using idealizations across historical periods.[15]

Let me rely on a major philosopher of the social sciences in order to introduce this issue. According to *Max Weber*, idealizations in the social sciences–actually *Weber* speaks of the cultural sciences–are historical constructs, which may again lose their relevance, although not necessarily their validity:

> Once cultural sciences have defined their problems and the methods with which they will address them, they will always tend to consider the elaboration of these studies as an objective

[15] In later chapters, we shall introduce the distinction between the static and dynamic analysis of complex systems. The idealization can be made at each moment in time, but remains historically contingent.

> in itself, and they will no longer actively question the value of knowing the single data in terms of ultimate goals. Often, they will even not remain reflexive with respect to the anchorage of their studies to ideas of values. And it should be like that. However, at one moment or another, the color will change: the meaning of the perspective which was used without reflection, will become unsecure; the road seems now to lead into zones of twilight. The light of the important problems of the culture has gone beyond. At such moments, the sciences have to provide themselves with the means of changing position and of changing their methodological apparatus, in order reflexively to grasp the higher grounds of reasoning from which to look down on the stream of history. Science follows the constellations which make it a meaningful enterprise.[16] (Weber [1904] ^{3}1968, p. 214.)

This reflexive awareness of the historical contingency of idealization has ever since (and also for other reasons) increasingly been generalized to the natural sciences as well, as has become most obvious in Kuhn's (1962) notion of 'paradigm'. However, the root of this reflexivity concerning the discursive constructedness of theoretical systems stems from the philosophy of the social sciences. In science studies, the value of this epistemological insight for studying the natural sciences has been discussed extensively, but

[16] "Alle kulturwissenschaftliche Arbeit in einer Zeit der Spezialisierung wird, nachdem sie durch bestimmte Problemstellungen einmal auf einen bestimmten Stoff hin ausgerichtet ist und sich ihre methodischen Prinzipien geschaffen hat, die Bearbeitung dieses Stoffes als Selbstzweck betrachten, ohne den Erkenntniswert der einzelnen Tatsachen stets bewusst and den letzten Wertideen zu kontrollieren, ja ohne sich ihrer Verankerung an diesen Wertideen überhaupt bewusst zu bleiben. Und es ist gut so. Aber irgendwann wechselt die Farbe: die Bedeutung der unreflektiert verwerteten Gesichtspunkt wird unsicher, der Weg verliert sich in der Dämmerung. Das Licht der grossen Kulturprobleme ist weiter gezogen. Dann rüstet sich auch die Wissenschaft, ihren Standort und ihren Begriffsapparat zu wechseln und aus der Höhe des Gedankens auf den Strom des Geschehens zu blicken. Sie zieht jenen Gestirnen nach, welche allen ihrer Arbeit Sinn und Richtung zu weisen vermögen (...)."

it remains itself a notion fundamental to the very possibility of social sciences.

Actually, in view of the current uncertainty among post-modern sociologists about the need for an analytical framework (e.g., Gouldner 1970; Phillips 1973a), it should be noted that the founding fathers of some of the social sciences were very much aware of the need to ground their discipline in an elaborated analytical framework. For example, *Marx* opened *Das Kapital I*, Chapter One by declaring his scientific program as the further analysis of goods and their relations,[17] and by specifying the (dialectical) methods which he was going to apply. *Freud* claimed on page one of the *Traumdeutung* that a new realm could be made the subject of fruitful investigations, if one applied his psycho-analytic procedures to dreams as a realm which could then be made the subject of a science.[18] Very much in the same spirit, *Parsons* stated in the preface to his *The Social System* (1951), that "(t)he present volume is an attempt to bring together, in systematic and generalized form, the main outlines of a conceptual scheme for the analysis of the structure and processes of social systems."

Whatever we may think of these bodies of theory today, at certain moments these social scientists contributed to the constitution of their fields of science by specifying these axiomatic idealizations. Some social scientists will argue that the attempt to

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[17] "Der Reichtum der Gesellschaften, in welchen kapitalistische Produktionsweise herrscht, erscheint als eine 'ungeheure Warensammlung', die einzelne Ware als seine Elementarform. Unsere Untersuchung beginnt daher mit der Analyse der Ware." (Marx 1867, p.1.)

[18] "Auf den folgenden Blättern werde ich den Nachweis erbringen, dass es eine psychologische Technik gibt, welche gestattet, Träume zu deuten, und dass bei Anwendung dieses Verfahrens jeder Traum sich als ein sinnvolles psychisches Gebilde herausstellt, welches an angebbarer Stelle in das seelische Treiben des Wachens einzureihen ist. Ich werde ferner versuchen, die Vorgänge klarzulegen, von denen die Fremdartigkeit und Unkenntlichkeit des Traumes herrührt, und aus ihnen einen Rückschluss auf die Natur der psychischen Kräfte ziehen, aus deren Zusammen- oder Gegeneinanderwirken der Traum hervorgeht." (Freud 1900, p.1.)

build such grand schemes is nowadays self-defeating. However, the correct assumption of the contingency of all (scientific) knowledge does not warrant the conclusion that there can be no scientific methods. It only specifies that methods have epistemological functions and should not be reified, and it specifies as a criterion for methods in the social sciences notably that they should preferably be able to account also for the historical contingency of such knowledge.

3.5 FROM DISCIPLINARY AXIOMATA TO ORGANIZED SCIENCES

The sciences are parts of a scientific culture; they may share some axiomatic premises and oppose each other in certain other respects. The variety of disciplinary structures thus generated should not be characterized in terms of peaceful coexistence. Here, the conceptualization of cultural phenomena as elaborated in *Max Weber*'s theory of science (*Wissenschaftslehre*, ^{3}1968) is again most useful.

According to *Weber*, cultural phenomena are by definition related to values; values are incompatible among each other ('völlig unaustragbar'), and in historical reality social actors have to fight a continuous struggle for their ideas inspired by values. Analysts of social reality may synthesize 'idealtypes' from it as the mental constructs in terms of which one tries to organize one's understanding of what goes on in the social world. Since idealtypes differ among each other in combining various values, in the course of history other combinations may become more important in an ongoing collision of values in historical social life.[19] The idealtypes are therefore contingent to the analysts' historical conditions. For Weber (e.g., [1904] ^{3}1968, at p. 203) 'idealtypes' have mainly heuristic value.

[19] In contrast to relativism, the values do not coexist in Weber's conceptualization but set the stage for the continuous struggle which is the object of the social sciences (see, e.g., Weber [1917] ^{3}1968, at p. 508).

We can learn from this vision by using an analogy: the idealizations of the sciences function like the values in Weber's analysis, and consequently, the intellectual organization of the disciplines may teach us as analysts about the idealtypes as virtual embodiments of the combinations among them which have actually become valuable to society and the scientists living in it at a certain moment in time.[20] The intellectual organization of the sciences takes place in social reality, but not exclusively as intellectual organization. In Weberian terms we can conceive of the intellectual organization of a particular science as an 'Idealtype:' it is a mental construct with the help of which we–the analysts–can try to understand more complex scientific practices. Therefore, it is methodologically a variable, which is attributed to an organization as a *social* unit of analysis (e.g., Whitley, 1984).

Given the multi-dimensional model used in this study, one is able to attribute also an intellectual organization to a discipline or a specialty as a *cognitive* unit of analysis. As noted, this is an analytically different concept. For example, the central theories in a specialty may be more controversial in one case than in another. This theoretical uncertainty may systematically influence such variables as task uncertainties and mutual dependencies at the level of organization of the scientific community, as, for example, analyzed in the noted study by Whitley (1984). Thirdly, if one wishes to define disciplines in terms of the journals which constitute a literary archive, intellectual organization can be defined in terms of texts.

Perhaps, there are a few limiting cases in which one is able to study the sciences in a framework with fewer dimensions than these three. One may deliberately confine oneself to explanations in only one dimension, for example, in terms of only social agency. Or one may raise questions which are simple enough so that one could make do with a lower order of dimensionality, for example, about the age distribution of a research facility for purposes of institutional management. However, such a reduction in

[20] Actually, Weber himself writes about the different sciences in these terms. See: Weber [1904] [3]1968, at pp. 185f.

dimensionality will lead to a very partial and provisional understanding of the complex phenomena in which one is interested, and further questions can the be raised which point to the inadequacy of the reduction implied in the conceptualization.

3.5.1 Multi-, inter-, and trans-disciplinarity

In contextual analysis the complexity of various dimensions and various levels can be reduced by zooming into a particular relation for the purpose of pursuing empirical research. However, as soon as one wants to broaden the scope again, for example, in order to contribute to more theoretical debates, the problems of multi-dimensionality and of the various levels of aggregation at which the sciences are organized return. In science studies, one cannot deny the complexity of the subject matter: the systems under study are internally reflexive, and therefore a positivistic model of studying scientists and their texts as billiard balls will eventually break down.

Because of the analytical independence of the textual dimension, scientometric indicators provide policy-makers with the sense of an objective leverage for judging performance in the other dimensions. Citation analysis has been of particular interest to science studies and for the evaluation of the performance of groups or individuals because of the possibility of indicating links between the social and the cognitive dimensions of science or between 'impact' and 'quality,' by using textual means (cf. Leydesdorff and Amsterdamska 1990). Analogously, policy makers or policy analysts may sometimes be able to bracket out a discussion of the analytic complexity of the subject of intellectual organization provisionally by using programmatic catchwords like 'inter-disciplinarity,' 'multidisciplinarity' or even 'cross-' or 'trans-disciplinarity;' but in science studies, one should be cautious of the unreflexive importation of such categories.

For example, one may wish to distinguish between *multi-disciplinarity* and *inter-disciplinarity* in terms of definitions. The label *multi-disciplinary* can then be used as an indication that one is studying, or wishes to study, the same subject(s) from the perspective of various disciplines, in addition to each other. From each of these perspectives the other perspectives are provisionally contextualized as far as necessary in order to allow for quality control within the respective disciplinary frameworks. For example, in the case of science studies, a philosopher would leave the sociological questions involved to the sociologists, and vice versa. Therefore, multi-disciplinary studies in general reflect the full richness and sophistication of each of the contributing disciplines, but they often lack the integration which is usually intended with the label *inter-disciplinary*.

In interdisciplinary research one emphasizes integration, and therefore, the research focuses on areas of overlap between the various disciplinary perspectives. For example, one can elaborate on a concept which has a more precise meaning in various disciplinary perspectives, like 'paradigm' in the case of science studies. Often, in concrete research, a trade-off has then to be made between keeping the full range of scholarly traditions as available in the more established disciplines in a multi-disciplinary cooperation, and capitalizing on possibilities to cross-reference among more established disciplines at the expense of some analytical rigor and scholarly precision.

Consequently, in both these programmatic models one has to sacrifice room for explanation on *a priori* grounds: in the case of multi-disciplinarity, one focuses on complementarity, and in the case of inter-disciplinarity on overlap and integration. The holistic appeal of interdisciplinarity squeezes theoretical domains together with an appeal to one single object of analysis; the divisive approach of multi-disciplinarity may not fill the holes, since the disciplinary borderlines tend to be reified.

The conceptualization of the complexity of science studies as a *multi-dimensional* problem makes it possible, in principle, to avoid such programmatic choices, and to develop a framework in which one can maintain the complementarities and study the objects also as composed products of the various dimensions, since we have distinguished between disciplinary frameworks as cognitive forms of organization on the one hand, and intellectual organizations as 'idealtypes' in social analysis on the other.

In the multi-dimensional scheme, disciplinary perspectives in the cognitive sense no longer legitimate the reification of disciplinary perspectives in the intellectual organization. From this perspective, intellectual organization in social reality may or may not coincide with disciplinary delineations in the cognitive sense. Or, to give an example with policy relevance, if an interdisciplinary enterprise lacks an axiomatic framework in the cognitive dimension, quality control can, therefore, not easily be operationalized in this dimension. A science policy analyst may wish to discuss how to organize for quality control or, in other words, whether one should evaluate the program under study also in terms of whether it has contributed to the emergence of an analytic framework, in the epistemological sense.

In summary, the assumption of the 'multi-disciplinary' approach was based on the imposition of disciplinary boundaries in the cognitive dimension on the intellectual organization. However, the latter functions as an 'idealtype' in the social dimension, and merits specific articulation. On the other hand, the 'interdisciplinary' approach basically confounded the complexity of the scientific idealization with the complexity of 'real world problems.' In this case, an evaluation scheme could add to the questions of 'intellectual organization' cognitive criteria–e.g., paradigm and theory development, standardization of methods, specification of researchable questions. Again, the organization of the relevant scientific literature can be added as a third dimension. We turn to this dimension in the next chapter.

3.5.2 The analytical character of dimensions

Dimensions are different, and therefore they merit separate attention, although not necessarily in each individual research project. The reader may wonder why I proposed to propose these three dimensions and not others. This is not a question of principle but a provisional assumption for reasons of parsimony. The three dimensions enable me to show that whenever texts, scientists, and cognitions are bound together in a complex (or 'heterogeneous') network there are always grouped and grouping variables, and therefore interactive effects. Additionally, these interactive effects can be reinforced by developments over time.

In technology studies, however, one may wish to take technological artifacts as a separate dimension, and in some cases one may wish to argue similarly in favor of instruments as a relevant dimension in science studies. Furthermore, within each dimension one may distinguish multi-dimensionality again, as, for example, in the case of co-occurrences of citations ('co-citations') and co-occurrences of words ('co-words') among document sets as scientometric indicators of intellectual organization. These extensions only add to my principal argument that one has to distinguish dimensions before one can meaningfully study their interaction terms (cf. Leydesdorff 1989b).

Dimensions should not be equated with contexts or systems. In this stage of the study, the model allows for the creation and disappearance of systemic stability in either of the dimensions *or among them.* We don't need to make any *a priori* assumptions concerning the empirical questions of whether such contexts or systems have been stabilized over time. Thus, we are able to study empirical questions like whether the development can in certain stages be considered as self-referential or better be described with a model that accounts for goal-referentiality (Hanneman 1988). In this respect, the analysis pursued in this study will be more data-oriented and less reifying than the systems theoretical one. In a second-order systems theory (Part Three), the emergence of systems and self-organization remains a contingent possibility. In the final chapter (Chapter Thirteen), I shall specify, among other things, empirical conditions for the testing of the *hypothesis* of self-organization.

3.6 CONCLUSIONS

The discussion in this chapter was focused by distinguishing the two extremes in the philosophy of science in relation to our subject matter, namely, critical rationalism and (neo-)conventionalism. The former, Popperian view contains the idea of independence for cognitive structure but does not easily allow for variation; the latter, conventionalist approach–which follows mainly the Quine-Duhem thesis–allows for variation, but does not distinguish scientific cognition analytically as independently operationalizable. We are now able to specify more precisely how to understand this opposition.

In critical rationalism cognitive structure is positioned in World Three. Such a conceptualization also uses the metaphor of the opening of a new space. However, World Three is not a world of contingencies, but a Paradise to which only the 'right' cognitions are admitted through a process of asymmetric purification. 'Objective' knowledge has then a different status ontologically, and not in terms of functionality for the research process. As is well known, the model of physics is implicitly or explicitly used by these philosophers as the ideal against which to test the development of science in social reality.

Using a Weberian terminology, I have called this approach an idealtypical one: an idealtype is a mental construct which may be obtained by idealization from social reality. In this philosophy of science, 'physics' is reified as a model for science; the theoretical organization of physics is not understood reflexively, but used as a yardstick to fight for certain values and axiomata. One type of intellectual organization is confounded with the structure of science in order to maintain a normative position toward science.

That physics is just one type of intellectual organization becomes empirically obvious as soon as we compare it with the intellectual organization of, for example, the medical sciences, natural history, or even some parts of experimental physics in contexts of application. As soon as we allow for differentiation and division of labor, it is impossible over any stretch of time to assess precisely what is relevant to the cognitive core and what is not. Developments take place across a research front, and not along a single pathway. Therefore, we have to allow in our conceptual apparatus for variation not only among the disciplines but also within them. A major question then, becomes the question of how to describe such variation.

In the other major approach in the philosophy of science, the conventionalist tradition, the central thesis states that any theory can be maintained in science as long as those who adhere to it manage to keep it coherent and free of contradictions. Science is here reduced essentially to a language game (Quine 1953; Hesse 1980). I emphasized that in the conventionalist tradition the demarcation of science from other belief systems tends to disappear. The seamless integration of this philosophy of science into the actor-network approach is possible in principle, since the two approaches share a focus on empirical concreteness: cognitive structure is not only operationalized, but sometimes explicitly equated with textual (word) structure (see, e.g., Law and Lodge 1984). The representation, however, is insufficiently distinguished from the representing system (e.g., language). While the notion of variation is deeply accepted into the theoretical structure of this philosophy of science, the notion of scientific method as a discursive selector of special cultural value to us has tendentially disappeared.

In addition to these two mainstreams of philosophy of science, many others can be distinguished. However, I argued that these two extremes in the debate provide us with the scope of relevance for this study. On the one hand, there is the notion of cognitive structure, founded in the analytical idealizations of the various sciences and critical of the factual practices of scientists. On the other hand, these virtual principles form the cognitive horizon to which practicing scientists are expected to relate themselves.

The dimensions should not be reified; they have the status of theoretically informed hypotheses. The observable events are a result of the interactions between the various dimensions. The model allows us to specify different attributes to (complex) units of analysis in the multi-dimensional space, and to analyze the phenomena in terms of their interactions. For example, the analytical distinction between disciplinarity in the cognitive dimension and intellectual organization in the social dimension enabled us to clarify concepts and problems like multi- and interdisciplinarity.

The historical and contentual reconstruction may tell us of (important or minor) problem shifts within the development of a specialty–whether or not we are able to quantify these with scales as in formal content analysis is another issue–while we can study the same developments in terms of the scientific community/communities involved, or the bodies of literature, or, for example, the in- and outflow of citations. The problem of integrating results from these different perspectives in science studies will eventually boil down to the search for methodologies which enable us to combine results from these different forms of analysis, that is, to capture reflexively the uncertainty in the different representations.[21] In a later chapter, I shall argue that the problems posed by the latter question can be solved by using information theory: analysis in one dimension *informs* us about what to expect in others, and analysis in several dimensions tells us more than analysis in any one of them. Thus, we shall return to the issue of how to analyze 'heterogenous networks,' but reflexively.

[21] In the post-modern tradition, the 'intertextuality' among the different accounts can be declared, but not operationalized systematically.

Chapter 4

The Methodological Priority of Textual Data

In this chapter I focus on the third axis of the multidimensional scheme, i.e. the textual sedimentation of scientific discourses. Various authors (e.g., Hesse 1980; Callon *et al.* 1983; Law and Lodge 1984) have suggested that relations among texts can provide us with a network model of scientific developments.

From a methodological perspective, the choice of texts as units of analysis seems promising for an integrative effort, since scientific articles function in scientific developments at the level of knowledge growth, and also contain most of the relevant attributions in terms of the organization of research, such as authors, institutional addresses, journals, etc. Moreover, the linguistic approach, in principle, combines the force of qualitative understanding, with the option to use information processing and management techniques to handle large databases. If one could succeed in developing a model for the development of the sciences in the textual dimension, one might be able to extend this model to the composite case in a next step.

4.1 THE NETWORK MODEL OF SCIENTIFIC THEORIES

How should one study scientific texts in terms of networks? The concept of a network appeals to sophisticated methodologies of network analysis. Various analytical conditions, however, have to be met, if one wants to apply techniques from social network analysis to textual data. First, the differences between social networks and networks of texts should be further specified, since differences in theoretical meaning may have consequences for the operationalization (cf. Burt 1983; Courtial 1989).

In social network analysis, networks have been put forward mainly for studying the mediation between structure and action (e.g., Burt 1982). In the philosophy of science, however, Hesse (1980), following Quine (1953), has specified the 'network model' of scientific theories. The network is defined by Hesse (1980, at p. 86) as an essentially linguistic expression of the continuous integration of observation and theorizing in the sciences.

In contrast to the empiricist and rationalist traditions which focus on the syntactic logic of theorizing, the network is knitted by 'words'–predicates, names of entities–which have to be understood and used in positions relative to each other (*ibid.*, pp. 64f). The distinction between theoretical and observational descriptions is considered not one of kind but rather of degree; the pragmatic and nonformal use of predicates can be observed empirically with respect to the question of co-occurrence or co-absence (*ibid.*, p. 103). As Hesse (1980, at p. 92) put it:

> If, however, the claim that [the predicate] is used of theoretical entities in a different sense implies only that charged elementary particles are different kinds of entities from charged pith balls, this claim can easily be admitted and can be expressed by saying that the predicate co-occurs and is co-absent with different predicates in the two cases. The fact that use of the predicate has different lawlike implications in relatively theoretical contexts from those in observation contexts is better represented in the network model than in most other accounts of theories, for it has already been noticed that in this model the conditions of correct application of a predicate depend partly on the other predicates with which it is observed to occur.

In other words, the sciences can be studied as knowledge systems empirically by studying the use of language without *a priori* assumptions about the difference between 'theoretical' and 'observational' references for textual elements. Thus, the envisaged networks can be defined as structures of science which are retrievable in terms of co-occurrences and co-absences in scientific texts.

These nonformal networks of co-occurrences and co-absences are also different from 'semantic' and 'semiotic' networks. 'Semantic' networks, which are used for formalized computer representations of knowledge (e.g., Findler 1979), are based on parsing of syntactic structures of language. 'Semiotic' networks share with the networks discussed above that they give priority to the analysis of textual networks in terms of co-occurrences and co-absences of words (Law and Lodge 1984). In the semiotic tradition in science studies as elaborated in the sociology of translation, however, the basic question is about actors (or more generally, actants) who build networks in order to create a power base enabling them to enforce translation. 'Co-words' can provisionally stabilize positions in an ongoing fight for power (Callon and Law 1982; Latour 1987a); therefore, the study of co-word patterns informs us about strategic options and choices (cf. Courtial 1989). However, the co-words themselves are actants in these translations, and accordingly, they are no longer considered only as indicators. Thus, the search is for 'co-word'-patterns with respect to their strategic positions in scientific debate *among* texts and/or authors, and not primarily with respect to their function within arguments or within theory.

In summary, Hesse's (1980) 'network model' is about relations among cognitive units of analysis; she pleads for the study of these relations as observed in the co-occurrence and co-absence of terms in texts, hypothesizing that these will *indicate* predicates and theoretical entities. Her approach to the structure of science is lexigraphical.

4.2 TEXTS AND CO-OCCURRENCES OF WORDS

The use of words to reconstruct the sciences (e.g., in terms of scientometric mappings) seems attractive from an information science point of view: words are meaningful, obviously present at all levels of analysis and in all databases, and the study of the dynamics of their frequencies and distributions is relatively easy with the help of the computer. In contrast to citations, words can be followed also in policy documents and conference papers (e.g., Callon and Courtial 1989; Kranakis and Leydesdorff 1989). Moreover, lexigraphical change in science has been associated qualitatively with paradigm development in historical studies of science (e.g., Kuhn 1984).

The obvious advantage of lexicon over syntax in empirical research is that character-strings can be analyzed directly from the text without much intervention by the analyst. The reconstruction of the textual organization of scientific texts at the level of words and co-words might make it possible in the longer term to integrate perspectives from information retrieval, with its strong tradition of using words in queries, and from artificial intelligence. The eventual objective in this stage is the possibility to reconstruct scientific developments from scientific texts using lexicon, and without using subjective interpretations (cf. Langley *et al.* 1987).

However, it is precisely in relation to information science that the weak spots of the lexical approach have become manifest: term-term associations are local in an unrestricted (natural language) environment (Lesk 1969; Salton 1970; Salton and McGill 1983), and word distributions in texts are known to be stochastical (Ijiri and Simon 1977; Chen 1985). Although words and their co-occurrences may be useful indicators of the intellectual organization of the sciences under specific conditions, for these two reasons the co-word model has to be further specified.

First, in the matrix of textual units as cases (e.g., documents, sentences) versus words as variables, two structures can analytically be distinguished: the word structure and the document structure. In principle, these two dimensions are conceptually independent (since orthogonal). The document structure is defined by its boundaries, and should therefore be assessed in terms of its relations to the environment; the word structure reflects the internal intellectual organization in terms of the codification of word usage in the set.

Because of the locality of term-term associations, the analysis of a *document structure* in terms of word occurrences is more likely to succeed in restricted document sets than in a natural language environment. A restricted document set is a document set which has been selected from a wider database by a specifiable criterion. For example, if one is interested in the intellectual organization of an institutional research program, one way to proceed may be to search for all documents related to that program in terms of authors and corporate addresses.

Second, the extent to which word usage is being codified in the specific (restricted) subject area will determine whether a *word structure* can be found in terms of the underlying documents. Although the two conditions (restrictedness and codification) are conceptually independent, in practice the condition of codification will often presuppose the condition of restrictedness. Hence, the revealing of document structures may be the more general application of using co-occurrences and co-absences in word distributions. Whether a meaningful word structure can also be found within this restricted set remains to be shown empirically.

4.2.1 Levels of aggregation in scientific texts

In addition to the analysis of the function of words and their co-occurrences in 'actor-networks,' the proponents of the sociology of translation have introduced the concept of the sciences as nested networks: some words ('macro-terms') are more important than others, since at a higher level of aggregation they may be used as representations of underlying clusters (Callon *et al.* 1983, p. 212; Callon *et al.* 1986, pp. 103-23)–analogous to the way in which actors may behave on behalf of groups in constituting a state apparatus (Callon and Latour 1981). In the actor-network model, however, the various levels of aggregation are considered equivalent: each actor-network may be composed of other actor-networks which may interact and be compared with other actor-networks at other levels of aggregation. Analogously, co-words have the same semiotic value in different contexts.[22] On the basis of

[22] The distinction between textual elements as 'actants' (messages) and social elements as 'actors' (senders or receivers), which is sometimes used

this semiotic assumption, the co-word instrument is considered as an objective–that is, context-insensitive–instrument for mapping the translations in and among networks.

Indicators, however, are context dependent: they have no meaning out of context. While the semiotic assumption may have been a fruitful heuristic from the perspective of theorizing about how power is negotiated and constituted in societies–since the heterogeneous entities are handled as if they were equivalent, they can all be 'enrolled' in the same way–nevertheless, in analysis of texts *per se*, the level of aggregation does make a contextual difference. The sciences are nested structures: each higher level of organization exerts control over the lower levels, at least as contextual variables (see *Figure 4.1*).

For example, what makes an article a theoretical contribution can only be decided within the wider context of the specialty; what makes a proposition an argument is inherently constrained by theory; and what makes a word-combination a lawlike concept is, among other things, dependent on the argument. 'Words' can at best be indicative of theoretical entities and predicates; their relations may indicate lawlike concepts at the level of a statement (Hesse 1980), but whether the co-word instrument has this same function at other levels of aggregation remains an empirical question.

Higher-order structures of intellectual organization, such as theories, specialties and disciplines, are unlikely to be represented by sets of words only (although they have to be expressed in language, as we have been taught by the pragmatic perspective). At the highest level of aggregation in scientific literature, one finds scientific journals and books.

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in semiotic analysis, is deliberately abandoned in the sociology of translation. For example, in note 2 to the programmatic introduction to Callon *et al.* (1986), the editors state: "Our conception of 'actor' is broader than that which is normal in Anglo-Saxon sociology."

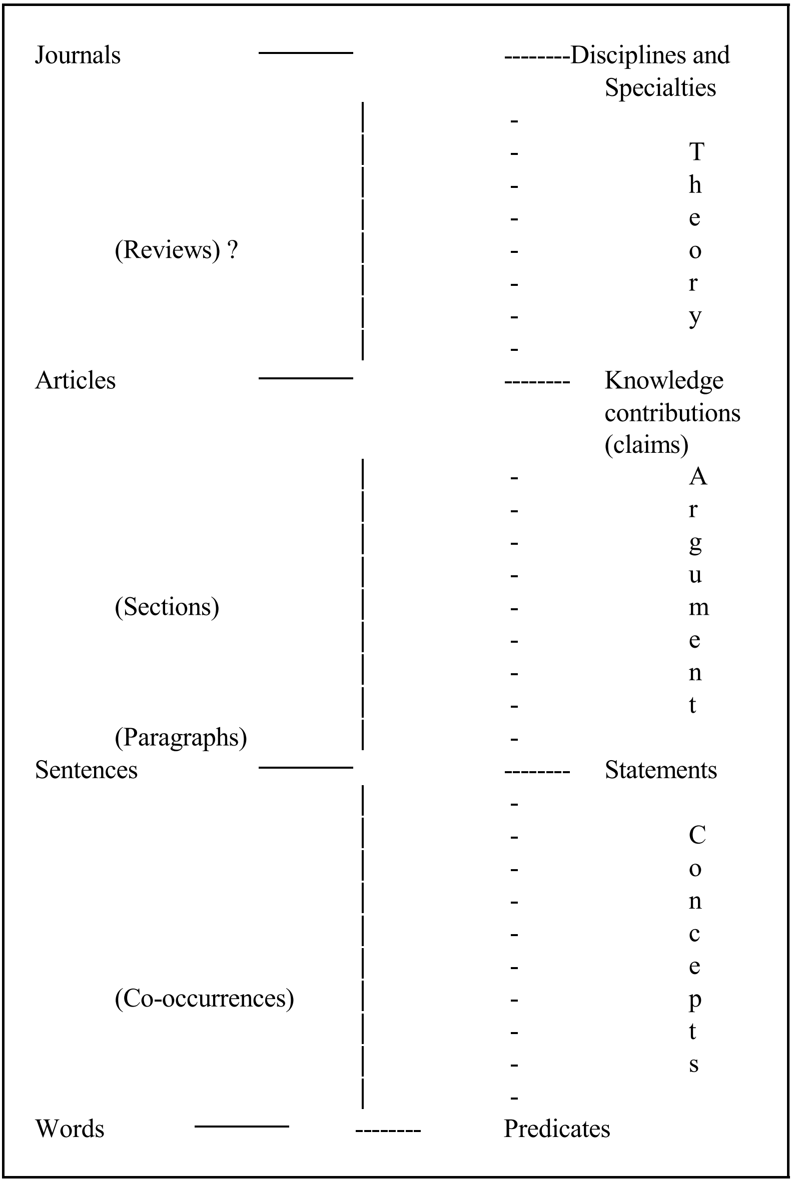

Figure 4.1
Aggregation and organization of textual units

As the (aggregated) journal level may be considered indicative of specialty and disciplinary developments,[23] the (aggregated) article level (reviews, notes, etc.) is presumably indicative of theoretical developments within a journal structure (cf. Bastide *et al.* 1989). In addition to dynamics between review articles, research papers and technical notes–all categories with distributions at the specialty level both among journals and within some of the journals–we may assume that there is theoretical debate among papers within a given journal cluster belonging to a specialty. Thus, various dynamics of translation are simultaneously ongoing.

New knowledge claims are the basic units of these processes of translation. One expects a significant article to organize a knowledge claim, and thereby to contribute to theory development. The new knowledge claim is expressed in an argument. In order to construct the argument, theoretical entities, predicates, methodological rules, and warrants from earlier research (such as citations) have to be arranged in an analytic order. Sections of the article organize the various elements involved: after having laid out the problem in its relevant contexts, the author has to specify the methods to be used, present the experimental data, draw conclusions, and discuss their implications for further theoretical development.

The argument is woven nonformally into this logical order: sentence after sentence, paragraph after paragraph (Amsterdamska and Leydesdorff 1989). The question to address now is whether one can trace the (latent) structures by using the co-word analysis instrument? Is it possible to analyze the structures of science from the bottom up by aggregation and by using lexicon alone?

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[23] Scientific journals can be clearly clustered in terms of disciplinary and specialty structure, using various indicators (e.g., Carpenter and Narin 1973; Narin 1976; Leydesdorff 1986; Tijssen *et al.* 1987; Leydesdorff and Cozzens 1993). See also Chapter Nine.

4.2.2 Units of analysis

In a network analysis model one has to specify what counts as nodes and what as links. Since in Hesse's terminology co-occurrences of words form the links (the 'knitting') in the network, the question becomes what can be considered as the nodes (the 'knots').

Hesse (1980, p. 87) defined 'knots' primarily at the epistemological level; therefore, the 'knots' would not belong to the text. 'Knots' in a particular science and at a particular time relate the network to reality; others mainly function to make theories consistent and coherent. Hesse also emphasized that when science develops, what counts as a knot in one context may function as a link in another. Since the links are defined as "predicates and their lawlike relations," this suggests also that the nodes in the epistemic networks should be representable as (relations among) words. This is consistent with Hesse's general insistence on the point that knowledge has meaning only in relation to language, and not as a correspondence relation between language and the world (cf. Hesse 1988).

Thus, the co-word methodology–i.e., taking words as units of analysis and their co-occurrences as attributes–could be legitimated with reference to Hesse's philosophy of science (Law and Lodge 1984; cf. Collins 1985b). Co-word analysts create a (symmetrical) 'co-word matrix' showing how many times two words occur together in a given document set. Although this procedure is consistent with the sociographic metaphor of words as actors ('actants') that maintain diadic relations, the use of this symmetrical matrix has several methodological drawbacks. In the asymmetrical matrix of cases (documents, sentences, sections, etc.) as units of analysis versus occurrences of relevant words as variables, co-words represent a special category of those cases which have two words in common.[24]

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[24] Using matrix algebra, a symmetrical co-word matrix can be generated from an asymmetrical words/documents matrix $\mathbf{P}$ by multiplication with its transpose $\mathbf{P}^\mathbf{T}$ (Engelsman and Van Raan, 1991).

In addition to the fact that this original matrix (that is, documents versus words) allows us to take the full scope of all multivariate relations among words into account, the relations among cases can be studied in terms of words present or absent in them. For example, using the matrix of sentences as cases and word-occurrences as the variables, one can study both word-structures and among-sentence structures such as paragraphs, sections, etc. One can then analyze also the effects of aggregation and disaggregation among the documents in relation to issues concerning the composition of document sets.[25]

Two sentences seem to me to be the smallest meaningful units in a text that can share a word, a co-occurrence of two words, etc. Sentences are composed of words, and paragraphs are composed from sentences, sections from paragraphs, etc., so that we can conceptualize the aggregation process as a repetitive and hence routinizable process: words add up to sentences, sentences to paragraphs, etc. At each higher level, we can create a matrix of cases (i.e., sentences, paragraphs, sections, texts, etc.) versus the words occurring or absent in the cases, on the basis of the matrix of sentences versus words by straightforward aggregation (*Figure 4.2*). The crucial question now becomes whether new structural properties of the matrix emerge at higher levels of aggregation, and if so, how these properties can be interpreted in relation to the assumptions which are exhibited in *Figure 4.1*.

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[25] The significance of a co-occurrence can be tested only against the expected value of co-occurrences with respect to distributions of words given the document set. Without this specification, the strength of the association–sometimes measured with the Jaccard index, the cosine or the Pearson correlation–can no longer be considered as a measure of the probability of co-occurrence, but only as a summary statistic of the matrix indicating the relative weights of the different co-occurrences (Rip and Courtial 1984; Zaal 1988).

Basic matrix of sentences versus words

	Word A	B	C	D	…	...	Z		
Sentence 1	1	0	0	2	0	0	1	)	
Sentence 2	1	1	0	0	1	0	0	)	Paragraph 1
Sentence 3	0	0	0	1	0	1	1	)	
.....		.	.	.	.	.	.		
.....		.	.	.	.	.	.		

Paragraph 1 is in this example the sum of the rows, representing sentences 1 to 3

Aggregated matrix of paragraphs versus words

	Word A	B	C	D	...	...	Z		
Paragraph 1	2	1	0	3	1	3	0	)	
Paragraph 2	.	.	.	.	.	.	.	)	Section 1
.....		.	.	.	.	.	.	)	

Further aggregated matrices of sections, articles, journals, etc. versus words

	Word A	B	C	D	...	...	Z
Section 1		.	.	.	.	.	.
etc.		.	.	.	.	.	.
Article 1		.	.	.	.	.	.
etc.		.	.	.	.	.	.
Volume 1	.	.	.	.	.	.	.
etc.	.	.	.	.	.	.	.

Figure 4.2
Aggregation of textual units

4.3 FULL TEXT ANALYSIS OF A SINGLE DOCUMENT IN TERMS OF WORDS

The most restricted document set is *one* document. It is a plausible assumption that in one and the same scientific article, author(s) will try to prevent variation in the meanings of words, and hence word usage can be expected to be as 'codified' as possible.[26] Within an article words can be attributed to sentences, paragraphs, and sections as document structures, which also maintain aggregative relations with one another. If we attribute words to units at these various levels of aggregation, do we find structure? If we do, what does this structure reveal in cognitive terms?

I explored these questions with the help of an article by H. J. Sips, A. K. Groen and J. M. Tager entitled 'Plasma-Membrane Transport of Alanine is Rate-Limiting for its Metabolism in Rat-Liver Parenchymal Cells,' published in the October 1980 issue of *FEBS-Letters* (Vol. 119, pp. 271-274).[27] Remember that in this stage, we are mainly interested in developing a 'co-word model' for the retrieval of 'epistemic networks' within a text. In a next stage (Chapter Five), the resulting model (see below) will be tested in a set of texts.

The article under study has the format of a normal research report. Its main argument is that the possible connection between the transport of amino acid across the membrane and its subsequent metabolism in the liver cell, which until then had received little attention, is a straightforward case in which the former is rate-limiting for the latter in the case of alanine. One importance of this

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[26] In general, this present exploration will be limited to written communication in the form of scientific articles, since we may assume that in the rational repertoire of this specific form of discourse the socially contingent production of the article is suppressed as far as possible (cf. Gilbert and Mulkay 1984).

[27] I chose this text, since it has been thoroughly analyzed in several studies, among other things for the purpose of citation analysis. See: Leydesdorff (1989b); Amsterdamska and Leydesdorff (1989); Leydesdorff and Amsterdamska (1990).

claim is that it makes the biochemical transport mechanisms across the membrane relevant for those researchers who focus mainly on metabolism within liver cells, which has been an area of medical interest.

The text contains 1832 words, of which 508 are unique, organized into 59 sentences.[28] Additionally, the article contains two figures and one table (with legends), an acknowledgement, and 20 footnotes. For our analysis, we used only the text of the 57 sentences which constitute the argument (leaving out also the sentences of the title and the acknowledgement). This part of the text contains 412 unique words. The sentences are organized in 15 paragraphs, and three sections with subheadings 'Introduction,' 'Materials and methods,' 'Results and discussion.' Close reading reveals that the last four paragraphs of the paper is a discussion section, which should be distinguished from the presentation of empirical results in a third section. (The authors, however, did not insert a subheading.) *Table 4.1* summarizes the organization of the text.

	Nr of Paragraphs per Section	Nr of Sentences per Paragraph
Introduction	3	8
Methods and Materials	3	11
Results	5	23
Conclusion/Discussion	4	15
Total	15	57

Table 4.1
The organization of the text in sections, paragraphs and sentences

[28] The number of 1832 words is based on a mechanical word count. If one excludes numbers and measures, only about 1700 words can be counted, of which 496 are unique.

With the exception of adverbs directly derived from an adjective, all adverbs, numbers, and pronouns were excluded from the analysis. Synonyms, words with the same root, and various conjugations of verbs were stored in a dictionary which was used during the analysis. In case of doubt, two words were counted as having separate meanings. Sometimes this required careful decisions: for example, in this text 'metabolic' was counted as if it meant the same as 'metabolism,' while 'metabolite' was counted separately.

Words which occur only once can be regarded as noise, since they can only indicate difference and no similarity.[29] Ninety-three meaningful words (or more precisely groups of equivalent words) occurred more than once in the argument. They are listed in *Table 4.2*; these words are included in the analysis. As expected, their distribution is skewed, ranging from alanine, which occurs 58 times, to 25 words which occur only twice. In total, these 93 words occur 527 times in this text.

ABSENCE	
ACCUMULATION	
ACID	
ACT	
AGREE	AGREEMENT
ALANINE	
AMINO	
AMINOOXYACETATE	
ANALOGOU	
ATTENTION	
BATCH	
BUFFER	
CARRIED	
CATABOLISM	CATABOLIC
CELL	
CENTRIFUGE	CENTRIFUGED, CENTRIFUGATION, CENTRIFUGING
CHAMBER	
CHANGE	
COLUMN	
CONCENTRATION	

[29] Moreover, the reduction of numbers of words involved is convenient in order to limit the amount of computation.

CONCLUDE	CONCLUSION
CONDITION	
CONSTANT	
CONSUMPTION	
DETERMINED	
DRY	
EFFECT	
ENZYME	ENZYMIC
EQUILIBRIUM	EQUILIBRATE
EXPERIMENT	EXPERIMENTATION
EXTERNAL	
EXTRACELLULAR	
FASTED	
FED	
FIG	
GLUTAMATE	
INCREASE	INCREASED, INCREASING
INCUBATION	INCUBATED, INCUBATING
INDICATE	INDICATED, INDICATIVE
INFLUENT	
INHIBITED	
INHIBITOR	
INTEREST	
INTRACELLULAR	INTRA- (and extracellular)
ISOLATED	
KINETIC	
LEAD	LED
LIMITING	
LIVER	
LOW	
LOWER	
MEASURE	MEASUREMENT, MEASURED, MEASURING
MEDIUM	
MEMBRANE	
METABOLISM	METABOLIC, METABOLIZED
METABOLITE	
METHOD	
MG	
MIN	
ML	
MM	MMM
MOL	
OBSERVED	OBSERVATION
OIL	
OXOGLUTARATE	
PARENCHYMAL	
PERFORMED	
PERIFUSATE	

PERIFUSED	(PERIFUSION)
PHYSIOLOGICAL	PHYSIOLOGICALLY
PLASMA	
PRESENCE PRESENT	
PYRUVATE	
RAT	
RATE	
RESULT	RESULTING
SAME	
SAMPLE	
SHOW	SHOWN, SHOWED
SILICONE	
SITUATION	
STATE	
STEP	
SUBSEQUENT	SUBSEQUENTLY
SUSPENSION	
SYSTEM	
TABLE	
TRANSAMINASE	
TRANSPORT	
UPTAKE	
USE	USED, USING
VOLUME	
WT	

Table 4.2
93 Words and 33 synonyms used for the full text analysis of ***Sips et al. (1980)***

Thus, the basic matrix for the further analysis is that of 57 sentences versus 93 words.[30] The other relevant matrices can be composed as aggregations within this matrix: for example, the aggregate of the first eight sentences (rows) constitutes the introductory section as one case (see *Figure 4.2* above). Therefore, one can generate from this matrix other matrices which make it possible to study the document and word structures at the level of the full text, in each paragraph and section, or among paragraphs and among sections.

[30] PERIFUSION is bracketed in *Table 4.2*, since it was left out of the analysis by mistake.

Each matrix can be subjected to various types of multi-variate analysis. In this study, I used factor analysis for studying the matrices in terms of word and document structures.[31] For the graphic representation, dendograms were produced by using CLUSTAN 2A (Everitt 1974; Wishart 1978). Discriminant analysis was used to test whether sentences belong to sections, etc., probabilistically in terms of the 93 words involved.[32]

4.3.1 Word structures at different levels of aggregation

Four relevant levels of aggregations are distinguished: the sentence, the paragraph, the section, and the full text. Words are ordered into sentences, sentences into paragraphs, etc. At each higher level one can check for the emergence of new structural properties.

The first finding is that the factor analysis of the 93 words attributed as variables to *sections* as cases leads to a sharp and highly meaningful three-factor solution. This structure cannot be found at lower levels of aggregation. Dendograms (from cluster analysis) enable us to display the results visually: in *Figure 4.3*, there is a complete and sharp distinction between three groups of words.[33]

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[31] All matrices have been factor-analyzed both orthogonally and obliquely (cf. Kim 1975). The discussion is based on the orthogonal solutions, since the oblique solutions did not add to the understanding.

[32] Since section numbers, paragraph numbers and sentence numbers co-vary at the ordinal level, it makes no sense to use more complex MANOVA designs (Norusis 1986, pp. 103f.).

[33] Since dendograms are based on cluster analysis, one has to choose a similarity criterion and a clustering algorithm. In order to create dendograms which give a discrete and fair representation of the principal component structure, one may choose the Pearson correlation as a similarity criterion and Ward's method as a clustering algorithm (see, e.g., Tryon and Bailey 1970, at p. 118; Leydesdorff 1987; Leydesdorff and Zaal 1988). However, Ward's method is defined only for Euclidean distance matrices.

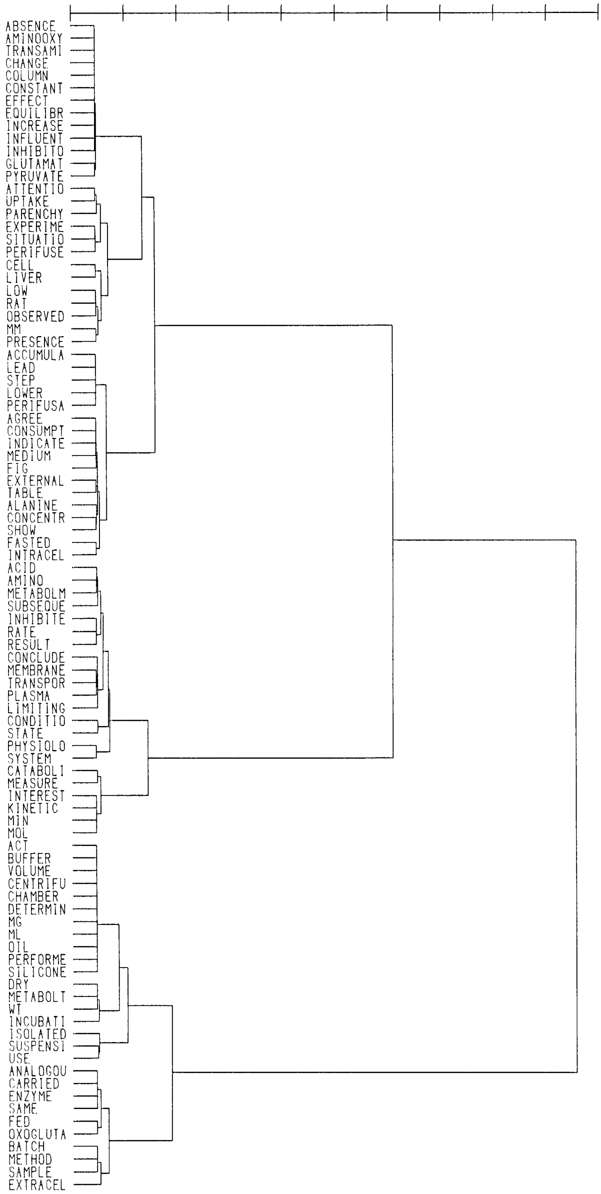

Figure 4.3 *Clusters of words over sections*

From the top to the bottom we can see a cluster with words related to the observations being reported in the paper, a second cluster with words indicating the theoretical thrust of the paper, and a third more distant cluster consisting of words related to methods.[34]

The distinctions are precise; for example, in the first instance, one wonders why 'Mol' and 'min'–which stands for minute–are listed among the theoretical terms. However, one should realize that what is actually being observed using a servometer is an 'increase' on the plot, and not the number of 'Mol' per 'minute' being transported. Only when one starts interpreting the graph in terms of a kinetic theory, is one able–using this theory as a premise–to calculate the rate of Mols per minute from the observation. As one can see, 'increase' is indeed among the observational terms.

In some cases the separation between the three types of words is contingent to the text: for example, 'intracellular' is included in *Figure 4.3* among the observational terms, while 'extracellular' is classified among the methodological ones. In the text, 'intracellular' is most often associated with 'intracellular alanine concentrations,' while 'extracellular' is also associated with 'extracellular medium.' Both words also have substantial factor loadings on the theoretical factor. Many words have more than one substantial factor loading, and therefore the actual picture is sometimes more complex than the graphic representation can show.[35]

---

[34] In itself, the fact that the variance can be explained by three factors is an analytical consequence of having only four cases, and hence three dimensions (see, e.g., Bray and Maxwell 1985). However, this does not explain why the structure is clear and meaningful.

[35] In principal component and factor analysis, in contrast to clustering algorithms, the assignment of cases or variables to groups is not necessarily unique. Moreover, the principal component analysis, which the cluster analysis and the factor analysis have as a common initial base, is used only as a means of achieving economy of representation, while the objective of the factor analysis is to explain correlations among variables (see, e.g., Kim and Müller 1978, pp. 16f.).

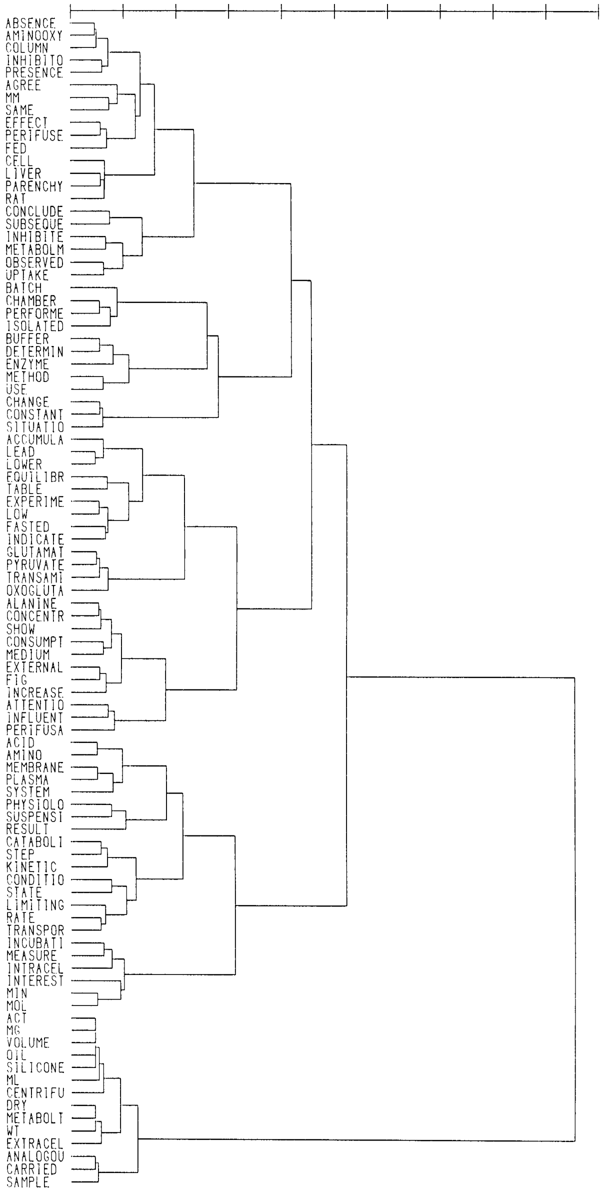

Figure 4.4 *Clusters of words over paragraphs*

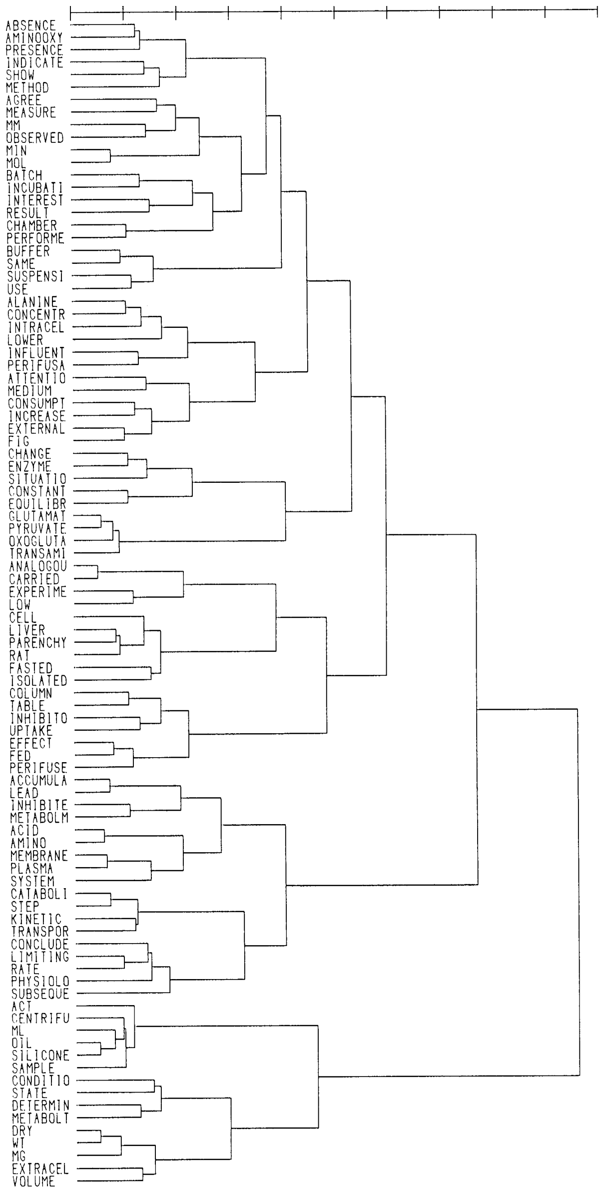

Figure 4.5 *Clusters of words over sentences*

Figures 4.4 and 4.5 exhibit the cluster structure when paragraphs and sentences are taken as units of analysis, respectively. At these lower levels of aggregation only the cluster with methods-words remains more or less distinct; the lower the level of aggregation, the more the relations among observational terms and theoretical terms become important. At the level of paragraphs, one is able to find a second cluster with exclusively theoretical terms when going down along the tree in *Figure 4.4*; but this is no longer the case at the level of sentences (*Figure 4.5*).

In summary, the strong interrelations between observational and theoretical terms which is suggested in the philosophical literature reviewed above, is notably the case at lower levels of aggregation, i.e., in individual sentences, representing statements or parts of the argument. At higher levels of aggregation, i.e., those related to the structure of the knowledge claim embodied in the article and the theoretical contribution it intends to make, it was possible to distinguish clearly between observational and theoretical terms. The specific position of methodological terms is less sensitive to the aggregations.

4.3.2 The attribution of sentences to sections

Although at the level of sentences we did not find a clear word structure in the former analysis, the relations of the sentences among each other, in terms of the words being used in them, are distinct. This is not surprising; after all, sentences, paragraphs, and sections have positions in the argument which are specific. However, it is remarkable that we do not need a syntactic or semantic analysis to make the relevant distinctions; one can reconstruct the order with the help of only lexicographical tools.

```
                       ALL-GROUPS SCATTERPLOT - * INDICATES A GROUP CENTROID
                                 CANONICAL DISCRIMINANT FUNCTION 1
        OUT     -24.0     -16.0      -8.0        .0       8.0      16.0      24.0        OUT
          X---------+---------+---------+---------+---------+---------+---------+---------X
     OUT X                                                                                X
         I                                                                                I
         I                                                                                I
         I                                                                                I
         I                                                                                I
         I                                                                                I
    24.0 +                                                                                +
C        I                                                                                I
A        I                                                                                I
N        I                                                                                I
O        I                                                                                I
N        I                                                                                I
I   16.0 +                                                                                +
C        I                                                                                I
A        I                                                                                I
L        I                                                                                I
         I      2 2*                                                                      I
D        I        2                                                                       I
I    8.0 +            2                                                                   +
S        I                                                                                I
C        I                                                                                I
R        I                                                                                I
I        I                                                                   3333         I
M        I                                                                   3*3          I
I     .0 +                                             11                    3 3          +
N        I                                            1*                   3              I
A        I                                             1                                  I
N        I                                                                                I
T        I                                                                                I
         I              4     4                                                           I
F   -8.0 +                 4*4                                                            +
U        I                4 444                                                           I
N        I                                                                                I
C        I                                                                                I
T        I                                                                                I
I        I                                                                                I
O  -16.0 +                                                                                +
N        I                                                                                I
         I                                                                                I
2        I                                                                                I
         I                                                                                I
         I                                                                                I
   -24.0 +                                                                                +
         I                                                                                I
         I                                                                                I
         I                                                                                I
         I                                                                                I
         I                                                                                I
     OUT X                                                                                X
          X---------+---------+---------+---------+---------+---------+---------+---------X
        OUT     -24.0     -16.0      -8.0        .0       8.0      16.0      24.0        OUT
```

Figure 4.6
Classification of sentences into sections using words as discriminating variables

```
                        ALL-GROUPS SCATTERPLOT - * INDICATES A GROUP CENTROID
                                  CANONICAL DISCRIMINANT FUNCTION 1
        OUT      -6.0      -4.0      -2.0        .0       2.0       4.0       6.0        OUT
          X---------+---------+---------+---------+---------+---------+---------+---------X
     OUT X                                                                                X
         I                                                                                I
         I                                                                                I
         I                                                                                I
         I                                                                                I
         I                                                                                I
     6.0 +                                                                                +
C        I                                                                                I
A        I                                                                                I
N        I                                                                                I
O        I                                                                                I
N        I                                                                                I
I    4.0 +                                                                                +
C        I                                                                                I
A        I                                                                                I
L        I                                 1                                              I
         I                                                                                I
D        I                                            3                                   I
I    2.0 +                                           *                                    +
S        I                                      3                                         I
C        I                                                                                I
R        I                                   *                                            I
I        I                                     1        3                                 I
M        I                                                                                I
I     .0 +                                                                                +
N        I                                                                                I
A        I                               2   1                                            I
N        I                                                                                I
T        I                        *                                                       I
         I                     2                            4                             I
F   -2.0 +                                        4         *       4                     +
U        I                                                                                I
N        I                                                    4                           I
C        I                                                                                I
T        I                                                                                I
I        I                                                                                I
O   -4.0 +                                                                                +
N        I                                                                                I
         I                                                                                I
2        I                                                                                I
         I                                                                                I
         I                                                                                I
    -6.0 +                                                                                +
         I                                                                                I
         I                                                                                I
         I                                                                                I
         I                                                                                I
         I                                                                                I
     OUT X                                                                                X
          X---------+---------+---------+---------+---------+---------+---------+---------X
        OUT      -6.0      -4.0      -2.0        .0       2.0       4.0       6.0        OUT
```

Figure 4.7
Classification of paragraphs into sections using words as discriminating variables

The clearest illustration of this result is exhibited in *Figure 4.6*. It indicates that the sentences in the various sections cluster together if they are discriminated in terms of the words included. The sentences from the 'introduction'-section occupy an intermediate position–near the origin–between the sentences from the methodological and conclusion sections on the one hand, and those from the results section on the other.[36] This corresponds to our intuitive understanding of the relations among the sections. Not only are the word distributions strongly structured among the sections, but the sentences as well are precisely recognizable (by the computer) as belonging to various sections, and the sections are different in terms of sentences and word distributions.[37]

At the level of paragraphs, it is still possible to achieve a 100% correct classification of the sentences into 15 paragraphs. However, the graphic representation suggests that the mutual positions of the sentences within paragraphs are not clustered so discretely as they are in the sections. The attribution of paragraphs to sections, finally, leads also to a hundred percent correct classification, but to a less pronounced picture than that of the attribution of sentences to sections (see *Figure 4.7*; in comparison to *Figure 4.6*).

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[36] The intermediate position of the introduction can also be shown by the following table of the factor scores:

	Factor 1 (observational)	Factor 2 (methodological)	Factor 3 (theoretical)
Introduction	-.744023	-.902473	-.939134
Methods & materials	-.550255	**1.391648**	-.102801
Results	**1.455252**	.009590	-.363522
Conclusions	-.161004	-.498766	**1.405457**

In later chapters, we shall see that word usage in the introductory section has mainly a function in the relations with the field level. In the next chapter, I shall use pronounced differences in factor scores among the other sections for the factor designation.

[37] With hindsight, these results legitimate our decision to distinguish the discussion section from the results section.

In summary, the 'discriminating power' of words as variables for the grouping of sentences into paragraphs or paragraphs into sections is in both cases smaller than that of words for the grouping of sentences into sections. These results correspond to the conclusion about sections mentioned above as having a central position in the *structure* of a scientific article.

4.4 CONCLUSIONS

In this chapter, the notion of 'epistemic networks' was first defined in relation to Hesse's philosophy of science. Can epistemic networks be operationalized in terms of word occurrences and word distributions? In the one article here under study, we found two main structural properties:

1. At section level a three-factor word structure emerged, of which the factors could be clearly designated as 'theoretical,' 'observational,' and 'methodological.'

2. Word usage within sentences and within paragraphs is significant for the position of sentences in paragraphs, of sentences in sections, and of paragraphs in sections, respectively. The assignment of sentences to sections is most pronounced.[38]

The two results are related, but the one is not a logical consequence of the other. The factor structure predicts that the centroids of groups of sentences in sections (in terms of words as variables) are significantly different, but not that the clouds around the centroids are also significantly discrete. The latter was shown only with the discriminant analysis.

[38] This assignment seems rather insensitive to minor errors. As usual, it took several runs before all errors were removed from the analysis, but the pronounced structure emerged in the first 'quick and dirty' runs.

At the level of sections we found a pronounced three-factor solution among word distributions, since, for example, theoretical terms tend to co-occur and to be co-absent with methodological and observational terms from section to section. The clearest case is the methods section, in which we found the highest frequencies (and hence co-occurrences) of the methodological words involved. However, the retrieved distinction between words with theoretical and observational meaning was unexpected given the emphasis on nonformal word usage in neo-conventionalist literature.

At lower levels of aggregation than the section, word structures are more confused. The overall impression is that one can find order, depending on the aperture one is using to analyze. These results suggest that the section level might be the most fruitful level of analysis for comparisons among texts. In particular, it could be most rewarding to make comparisons among the methods sections of scientific articles.[39]

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[39] The mentioning and codification of names of instruments in the methods sections makes it feasible to relate this analysis to sociological analysis of instrumentation, on the one hand, and to economic and policy analysis of the diffusion of new instruments, on the other (Susan Cozzens and Frank Wamelink, *personal communications*).

Chapter 5

Full Text Analysis of Scientific Articles

Since both the themes and the words, and also other potentially relevant variables (the author, the journal, the laboratory) can be expected to change between texts, we are in need of a model for distinctions among different sources of variation if we wish to study a *document set*. The results from the previous chapter, indeed, have provided us with such a model, notably a discriminant analysis model. In this chapter, this model will be tested in a restricted set of documents.

The model can be written as in *Figure 5.1*. Three factors (F_1, F_2, and F_3)–which have been designated as 'theoretical,' 'methodological,' and 'observational' in the previous chapter–are hypothesized as latent variables which structure the relations among words.[40] However, since we found that the factors emerge only if we analyze at the level of the sections, we may introduce the sections (S_1, S_2, S_3, and S_4) as independent dummy variables.[41] The letters U, V, etc., represent disturbance or error terms, that is, relations among words which are not explainable in terms of the three factorial terms.

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[40] Since there are no disturbance terms associated with the factors, this is a case of the so-called MIMIC model in the class of models for discriminant analysis (see, e.g., Jöreskög and Goldberger 1975).

[41] If the factors were to be conceptualized as latent variables with multiple causes in addition to the section structure, there would be disturbance terms associated with the factors, and the LISREL approach should be used (cf. Bray and Maxwell 1985, pp. 61 ff.).

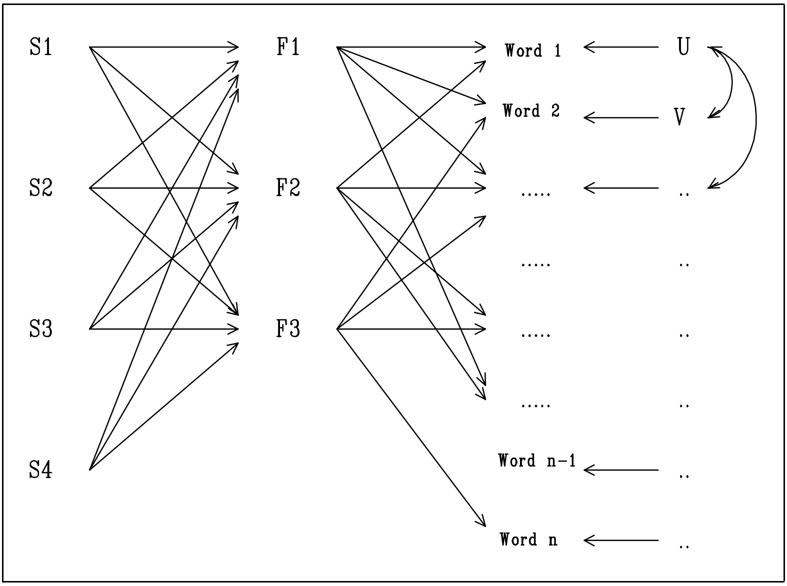

Figure 5.1
Path analytical model for the organization of words in sections

Note that the notion of a word occurrences as a frequency is different from words as nominal variables. Hitherto, we used words as nominal variables, which can be either attached or not to units of analysis; when conceptualized in this model, words have values on underlying dimensions. Therefore, we can now compare text with text, despite the fact that the words may be different. For example, we can compare the five words among texts with highest loadings on the factor 'methods,' etc.[42]

Given the overall objective of the reconstruction of scientific developments in terms of machine-readable texts, the objective in

[42] The factor loadings are by definition the (partial) correlation coefficients between the respective factors and variables. Therefore, the square of the factor loading (r^2) is the proportion of variance in the variable explained by the variance in the factor, and vice versa (since the formula for r is symmetrical).

this stage of the study is to create lists of words for each text with decreasing 'theoretical,' 'methodological,' and 'observational' values. In artificial intelligence lists constitute the databases from which the clauses operate. Thus, the creation of lists which can be designated dimensionally might solve part of the problem of how to engineer the knowledge without being oneself an active participant in the knowledge production and control process under study.

5.1 SAMPLE CHOICE

At the time of this research (1988) the journal *Biochemistry* of the American Chemical Society (ACS) was the only major biochemistry journal available full text on-line (for the period after January 1, 1982). As noted, the research in the Department of Biochemistry at the University of Amsterdam was analyzed in several previous studies. Four distinct and yet closely related lines of research were being pursued in the group of Professor Karel van Dam, including one concerned with 'membrane transport in rat liver cells and vesicles,' to which the *Sips* paper analyzed in Chapter Four belongs. Among these four research lines, the research of the group involved in the study of 'regulation of binding of membranes in *Dictyostelium discoideum*' was assessed as most relevant for the journal *Biochemistry*.

D. discoideum is a slime mold which functions as a model system for important biochemical processes in developmental biology. Under conditions of starvation individual cells of this species are able to notice one another's presence by the secretion of 'chemo-attractants.' Subsequently, the individual cells aggregate, form a multicellular organism, and start a well-defined developmental cycle, resulting in two differentiated cell types in a specific spatial arrangement. The question is: how do cells manage to send and receive these mutual signals in a controlled manner in order to coordinate biological action?

There is a steady amount of work being done on this problem worldwide, which results in a regular production of articles, on the order of magnitude of one hundred per year. Publication patterns are scattered: *Developmental Biology*, *Cell Differentiation*, and *Molecular and Cellular Biology* are among the more important journals in the area. The journal *Biochemistry* is not a central publication outlet, although there are regular publications concerning the more biochemical issues involved in *D. discoideum*, including the chemical nature of the chemo-attractants.

Among the articles of the Amsterdam Laboratory, the article by R.L. Bernstein, C. Rossier, R. van Driel, M. Brunner and G. Gerisch, 'Folate Deaminase and Cyclic-AMP Phosphodiesterase in Dictyostelium Discoideum–Their Regulation by Extracellular Cyclic-AMP and Folic Acid' (*Cell Differentiation* 10, 1981, 79-86), has been the most frequently cited representative of this line of research (Amsterdamska and Leydesdorff 1989). Therefore, it has been taken as the starting-point in the present inquiry. The third author, Dr. Roel van Driel, is senior scientist at the Amsterdam laboratory; he acted as an advisor to the study reported here.

Using the on-line installation of *Biochemistry*, 17 articles were found in the period 1982-1988[43] subsumed under *Dictyostelium*, in 15 of which this word also appeared in the title. (The two others used *Discoidin I* in the title.) Together with the noted article by Bernstein *et al.*, this provides us with a sample of 18 articles. In order to check whether this sample provides a sufficiently complete representation of the subject area in this journal, I used several strategies:

1. One of the chemo-attractants for *Dictyostelium discoideum* is cyclic-AMP. Therefore, the study of 'cAMP protein kinase,' which is a separate area of study, is a closely related subject. Additionally, I used 'cAMP' as a title word, and I also searched *Biochemistry* on-line for 'Dictyostelium' as a free text word. This led to 5, resp. 27 additional titles. Two of the latter were the noted articles with *Discoidin I* in the title.

2. In a questionnaire distributed to all authors who cited the *Bernstein et al. 1981*-article, we asked the respondents to identify the central members of this scientific community

[43] All searches were performed on December 8, 1988.

(Leydesdorff and Amsterdamska 1990). All citations to these authors in articles published in *Biochemistry* were downloaded using the *Science Citation Index*. This produced 16 relevant titles for the period 1982-1988 of which two already belonged to the original sample.

3. All citations within the sample to earlier articles in *Biochemistry* were retrieved. This led to a few potentially relevant papers.

The results of these searches and the original sample were then extensively discussed with Dr. Roel Van Driel, the noted co-author of the original article, and also a co-author of one of the articles found in these searches (*Janssens et al. 1986*). Van Driel did not consider all the articles in our sample, which was based on 'Dictyostelium' as a search term, to be central to his area of investigation. In his view none of the articles found in the additional searches were as important to the subject area as were the ones included in the original sample.

Obviously, the additional searches had drawn a wider circle than the one centered exclusively on 'Dictyostelium:' some of the other articles had been noticed thanks to the abstract services, but Van Driel had not read more than the abstracts in those cases. Relevance increased when 'cAMP' was combined with the study of 'protein kinase', but still the contributions were never considered as central to the core of the field of *D. discoideum* as were the articles in the sample based on this term as a keyword.

From the interviews, I concluded that the original sample of 18 articles (listed in *Table 5.1*) provided me with a sufficiently complete domain. Additionally, this core set of articles was more likely to be restricted than the wider set. The full texts of 17 articles were downloaded from *Biochemistry* and stored on disk. The original article was additionally typed into a file.

Figure 5.2 summarizes Van Driel's assessment of the internal organization of the sample.[44] He mentioned nine articles as central to his research interests. Among these were the two articles which

[44] The same scheme will be used also in Chapters Seven and Eight.

he co-authored. These nine articles were: *Bernstein et al. 1981; Rutherford et al. 1983; Rutherford et al. 1984; De Gunzburg et al. 1984; Marshak et al. 1984; Janssens et al. 1986; Van Haastert 1987; Shiozawa et al. 1987*; and *Mutzel et al. 1988*. These groups were also well known socially to Van Driel.

Bernstein, R. L., C. Rossier, R. Van Driel, M. Brunner, and G. Gerisch, "Folate Deaminase and Cyclic AMP Phosphodiesterase in Dictyostelium Discoideum: Their Regulation by Extracellular Cyclic AMP and Folic Acid," *Cell Differentiation* (1981), 10, 79-86.

Olsen G. J., and M. L. Sogin, "Nucleotide Sequence of Dictyostelium discoideum 5.8S Ribosomal Ribonucleic Acid: Evolutionary and Secondary Structural Implications," *Biochemistry* (1982), 21(10), 2335-2343.

Weinert, T., P. Cappuccinelli, and G. Wiche, "Potent Microtubule Inhibitor Protein from Dictyostelium discoideum," *Biochemistry* (1982), 21(4), 782-789.

Rutherford, C. L., and S. S. Brown, "Purification and Properties of a Cyclic-AMP Phosphodiesterase That Is Active in Only One Cell Type during the Multicellular Development of Dictyostelium discoideum," *Biochemistry* (1983), 22(5), 1251-1258.

McCarroll, R., G. J. Olsen, Y. D. Stahl, C. R. Woese, and M. L. Sogin, "Nucleotide Sequence of the Dictyostelium discoideum Small-Subunit Ribosomal Ribonucleic Acid Inferred from the Gene Sequence: Evolutionary Implications", *Biochemistry* (1983), 22(25), 5858-5868.

Rutherford, C. L., R. L. Vaughan, M. J. Cloutier, D. K. Ferris, and D. A. Brickley, "Chromatographic Behavior of Cyclic AMP Dependent Protein Kinase and Its Subunits from Dictyostelium discoideum", *Biochemistry* (1984), 23(20), 4611-4617.

De Gunzburg, J., D. Part, N. Guiso, and M. Veron, "An Unusual Adenosine 3',5'-Phosphate Dependent Protein Kinase from Dictyostelium discoideum," *Biochemistry* (1984), 23(17), 3805-3812.

Marshak, D. R., M. Clarke, D. M. Roberts, and D. M. Watterson, "Structural and Functional Properties of Calmodulin from the Eukaryotic Microorganism Dictyostelium discoideum," *Biochemistry* (1984), 23(13), 2891-2899.

Takiya, S., K. Takahashi, M. Iwabuchi, and Y. Suzuki, "Structural and Functional Properties of Calmodulin from the Eukaryotic Microorganism Dictyostelium discoideum," *Biochemistry* (1985), 24(4), 1040-1047.

Bisson, R., G. Schiavo, and E. Papini, "Cytochrome c Oxidase from the Slime Mold Dictyostelium discoideum: Purification and Characterization," *Biochemistry* (1985), 24(26), 7845-7852.

Janssens, P. M. W., J. C. Arents, P. J. M. van Haastert, and R. van Driel, "Forms of the Chemotactic Adenosine 3',5'-Cyclic Phosphate Receptor in Isolated Dictyostelium discoideum Membranes and Interconversions Induced by Guanine Nucleotides," *Biochemistry* (1986), 25(6), 1314-1320.

Van Haastert, P. J. M., "Kinetics and Concentration Dependency of cAMP-Induced Desensitization of a Subpopulation of Surface cAMP Receptors in Dictyostelium discoideum," *Biochemistry* (1987), 26(23), 7518-7523.

Shiozawa, J. A., M. M. Jelenska, and B. C. Jacobson, "Topography of the Dictyostelium discoideum Plasma Membrane: Analysis of Membrane Asymmetry and Intermolecular Disulfide Bonds," *Biochemistry* (1987), 26(15), 4884-4892.

Kohnken, R. E., and E. A. Berger, "Affinity Labeling of the Carbohydrate Binding Site of the Lectin Discoidin I Using a Photoactivatable Radioiodinated Monosaccharide," *Biochemistry* (1987), 26(26), 8727-8735.

Kohnken, R. E., and E. A. Berger, "Assay and Characterization of Carbohydrate Binding by the Lectin Discoidin I Immobilized on Nitrocellulose," *Biochemistry* (1987), 26(13), 3949-3957.

Mutzel, R., M.-N. Simon, M.-L. Lacombe, and M. Veron, "Expression and Properties of the Regulatory Subunit of Dictyostelium cAMP-Dependent Protein Kinase Encoded by .lambda.gt11 cDNA Clones," *Biochemistry* (1988), 27(1), 481-486.

Klein, G., J.-B. Martin, and M. Sartre, "Methylenediphosphonate, a Metabolic Poison in Dictyostelium discoideum. 31P NMR Evidence for Accumulation of Adenosine 5'-(.beta.,.gamma.-Methylenetriphosphate) and Diadenosine 5',5'''-P1,P4-(P2,P3-Methylenetetraphosphate)," *Biochemistry* (1988), 27(6), 1897-1901.

Klein, G., D. A. Cotter, J.-B. Martin, M. Bof, and M. Sartre, "Germination of Dictyostelium discoideum Spores. A 31P NMR Analysis," *Biochemistry* (1988), 27(21), 8199-8203.

Table 5.1
Bibliography of the sample
(18 biochemistry articles on ***Dictyostelium Discoideum)****.*

Two articles from a unit in Denver (Colorado) should be considered as outstanding contributions from a perspective of developmental biology on *D. discoideum*; *Olsen et al. 1982* could even count as a key reference to this type of work. The *McCarroll et al. 1983* article is from this same group. One of the other articles (*Takiya et al. 1985*), which was unknown to Van Driel, dealt also with nucleotide-sequences and could hence be considered as part of this subgroup.

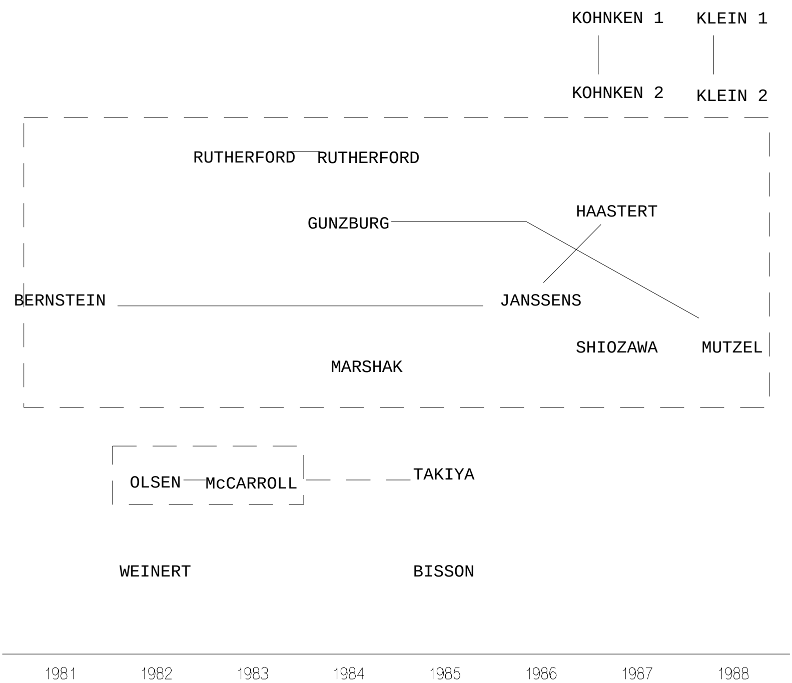

Figure 5.2
Schematic representation of the specialty of biochemistry of ***Dictyostelium discoideum****, operationalized as 18 articles between 1981 and 1988*

One of the authors of the article by *Weinert et al. 1982* was known to Van Driel, but he nevertheless felt that he would not read more than the abstract, and he thought it unlikely that he would ever cite it. The same relation of interest in the abstract “but no citation relation” holds for the two articles by *Kohnken and Berger 1986* about ‘Discoidin I,’ although in this case Van Driel knew one of the authors personally. The article by *Bisson et al. 1985* would have been read beyond the abstract, for locally contingent reasons, since another part of the Amsterdam laboratory is working on ‘cytochrome c-oxidase,’ and hence an article on *D. discoideum* and cytochrome c-oxidase would have been noticed from two sides. Two articles by *Klein et al. 1988* were assessed as more oriented toward questions of metabolism, and hence considered as a sideline.

5.2 PROCESSING

For the analysis, I used again only the body of the argument of the articles. Acknowledgements, footnotes, captions, and subheadings were excluded. All words contained in the remaining full sentences were counted and organized in a database in terms of the sections in which they occurred. Words occurring in section headings were excluded from the analysis.

With the exception of adverbs derived directly from adjectives, all adverbs, numbers, pronouns, conjugations of 'to be,' 'to have,' 'may,' 'will,' 'shall,' 'can,' etc., (copulas and modal verbs) were excluded. Singulars and plurals were equated; comparatives and superlatives were replaced by the basic forms of the adjectives. Conjugations of verbs were equated only when the present indicative form would not lead to ambiguity with a corresponding noun. All one-letter abbreviations were excluded from the analysis. In the case of highly specialized nouns and adjectives, the two were equated (for example, 'electrophoresis' = 'electrophoretic'). In order to limit computation, only words which occurred three or more times in one of the articles were included. The net result was a total of 1287 words, which occurred 28,422 times in the document set.

This processing leads to a matrix of four sections versus the number of words in the analysis of each of the articles. As in the previous chapter, these matrices are factor analyzed with the words as variables, and cluster analyzed with the words as the cases. The cluster analysis has primarily been used to check for the existence of smaller clusters of words which could have been overlooked in the factor analysis, since the number of eigenvectors was limited to three (*n-1* degrees of freedom), given that the number of cases (sections) is only four.

5.3 RESULTS

Factors were designated using the factor-scores of the four sections in the following order: the factor with highest value (as a latent variable) on the methods-section was indicated as 'methodological,' the one with highest loading on the results-section as 'observational,' and the one with highest loading on the discussion-section as 'theoretical.'[45] If necessary, signs were adjusted to the designation.[46] A typical example of a table of factor scores (i.e., for *Janssens et al.* 1986) is given in *Table 5.2*.[47]

	Factor 1 (observational)	Factor 2 (theoretical)	Factor 3 (methodol.)
Introduction	-.69373	-.94153	.93929
Methods and Materials	-.41174	-.30620	**-1.40951**
Results	**1.48498**	-.16397	.13393
Conclusion	-.37950	**1.41170**	.33629

Table 5.2
Factor scores for ***Janssens et al. 1986***

As a criterion for the attribution of a factor to a certain function in the article, I decided that apart from the sign, the factor score for a given section should be more than ten times as large as the other factor scores. Thus, if the factor score for the methods section was 1.0, all the other sections should have a score < 0.1. In that case, this factor was designated as the methodological one. By using this criterion, in 17 of the 18 articles factors could be unambiguously designated in terms of the three hypothesized categories, and

45 Since principal components extraction is used in originating the factors, factor scores are not estimates but exact (*SPSS-PC+ Manual*, Advanced Statistics, B-61).

46 If for example, the factor solution led to a factor score for the methods section with a negative sign but an absolute value > 1, in contrast to the other sections having only positive factor scores on that factor, the sign attribution was taken as a consequence of factor extraction and rotation, and all the signs of factor loadings for that factor were changed.

47 See also Chapter Four, footnote 35, at p. 81.

consequently, factor loadings could be sorted so that comparisons among texts of words and dimensions became possible.

In one case, the *Van Haastert 1987* article, the cluster of methodological words–which can be shown to exist using cluster analysis–is less important in terms of eigenvalues than the fine-structure of the various parts of the wordset which indicates the theoretical argument of the paper. The reason for this is that the methods section is very small in comparison to the other sections; the article has an extraordinarily long introduction in which the author reviews various theoretical positions in the specialty. Obviously, the long summarizing introduction uses a vocabulary which differs more from the theoretical repertoire used in the discussion section than either of these differs from the methods section in terms of eigenvalues.

Note also that this is the only article in the set which is single-authored.[48] The *Van Haastert* article was excluded from further analysis in terms of words and their distributions. (However, the article will be included in Chapters Seven and Eight, when this same set is studied with information measures.)

The listings of words with attributed factor loadings can be compared for the three dimensions among the remaining 17 texts, and among dimensions for the same text. We can consider the factors as latent variables for each of the articles, and raise the

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[48] The exceptional character of this article, in terms of organization within this set, led me to some considerations regarding normalization. Of course, one can easily normalize the four sections in terms of their respective length (in terms of words, sentences of paragraphs) or just normalize the final matrix in terms of the margin totals of the four sections involved. However, by doing so–and I actually did this in a few cases to study the effects–one loses a piece of information, namely, the relative importance of the three dimensions for the various articles, which can be expressed in terms of the eigenvalues for the factors. Moreover, one has no *a priori* guarantee that normalization will upgrade the undervalued section to such a degree that the eigenvalue will become one of the three most important of the matrix. Actually, in the case of the deviant *Van Haastert*-article, the results of the factor scores after normalization were more pronounced than before, but the order of the factors was not yet changed.

question of whether there is significant commonality within each of the dimensions (theory, methods, observations) among the articles. For example, while not every article will report on ‘centrifugation,’ the ones which do are expected to do so in the dimension of methods mainly, and not at variance over dimensions. Of course, some other words may change positions, but we would expect this to happen against a stable background.

Empirically, however, this was not the case! For example, among the 95 words in our sample which begin with the letter ‘A,’ 39 (i.e., 41%) load on factors representing different dimensions in at least one article. All kinds of combinations occur, also among the three dimensions. Not only the more trivial words, but also central terms exhibit such variation among factor loadings for different articles. Examples of such words include not only, ‘ability,’ ‘absence,’ ‘absorbance,’ ‘acetate,’ ‘acid,’ ‘activate,’ etc., but also ‘aggregation’ (typical for *D. discoideum*!) ‘amoeba,’ ‘association,’ ‘AMP’ and ‘ATP.’

Explorative correlation and factor analysis of the matrix of factor loadings (3 x 17= 51 variables) teaches us that correlations are highest among factor loadings representing the methodological dimension. In this dimension, there is the highest correspondence among the articles, followed by the theoretical dimension in the second place, and the observational dimension in the third. This is in accordance with what one would expect on the basis of the results in Chapter Four: methods are best defined and codified; one will more easily disagree about theories and therewith about the terms to use. Empirical results are almost by definition of various kinds, since the different articles do not study the same aspects of *D. discoideum*.

In summary: although many words occur in various sections of the text, using the distribution of words over the sections of *each individual article* I found a clear pattern in the noted three dimensions. However, *at the level of the set* this structure is no longer obvious: a substantial number of these words will tend to hold one position in terms of these three dimensions in one text, and another one in another text. The main conclusion is that the dimensions cannot provide a stable background of word patterns which can be used to indicate change in terms of (co-occurrences of) words. Words not only change position in terms of the dimensional scheme of 'theory,' 'methods,' and 'observational results,' but they obviously also change in meaning from one text to another. The codification of meaning which could be shown to be identifiable in one text breaks down if we generalize among more texts, even within one narrowly defined subject area.

These results accord with Hesse's (1980) thesis about the sciences as fluid networks. What may be a useful term for a theoretical concept in one context, may be used much more as an observational term in the context of another article. Not only the nodes and the links of the network change, but what counts as a node and what counts as a link may differ among theoretical perspectives, and also change over time.

Since these notions of Hesse were operationalized in terms of textual elements, most notably words representing theoretical entities and predicates, we may conclude from the above results that such changes occur also in the very micro-structure of a scientific specialty, that is, at the level of comparisons among articles in a restricted document set. As a consequence, we cannot distinguish how much of the observable variation is dependent on change in terms of the changing positions of individual words against a more stable background vocabulary ('links'), or on change in the vocabulary itself, that is, in the way it attaches to the description of reality ('knots').

5.4 CONCLUSIONS

At the set level we find change both in terms of how words are used and in terms of what words stand for conceptually. In other words, we can no longer distinguish at the level of the document set between change in intrinsic meaning and meaning in use, between change in word usage or change in the denotation of concepts. Within each individual article, word usage is more codified, and thus it seems always possible to distinguish between words with a major theoretical, methodological, or observational meaning within the context of a given text.

The purpose of the analysis, however, was to create an analytical framework in which it would be possible to compare among articles as events in a dynamic analysis. To this end, I applied methods of multi-variate analysis to each article as one instance of observation, in order to reconstruct 'scientific development' in terms of a series of events which one would then like to compare in terms of specified dimensions. However, the conclusion has to be that the *various articles are not comparable in terms of the words*.

These results show that the philosophical notions of Mary Hesse are applicable to the empirical investigation of the sciences as linguistic structures. The network is fluent both in its development over time and in terms of variations in perspectives at each moment. Although the 'knots' and the 'links' in the networks of Hesse have a different epistemological status, they may change position in terms of their functions in texts. Paradoxically, the conclusion from the previous chapter, that theoretical and observational words can be distinguished in terms of co-word patterns at the level of a single article (knowledge claim), is no longer valid at the level of a document set (representing the discourse).

Note that the document set under study was highly restricted; yet the set was not codified in terms of word usage. The distribution of word usage at the set level contains a considerable 'intertextual' interaction effect or, more technically formulated, in-between group variance as an effect of the aggregation.

5.5 CONSEQUENCES FOR BUILDING ARTIFICIAL INTELLIGENCE USING LEXICON

The subsumption of phenomenologically similar words or other textual signals under keywords assumes stability in the meanings of the indicated concepts. One major implication of the considerations in this chapter for the generation of artificial intelligence from (co-)word relations among scientific texts is that the assumption of the conceptual stability of terms over texts is more problematic than has often been assumed in declarative knowledge engineering, thesaurus construction, and indexing.[49]

The fluidity of epistemic networks in which nodes and links may change positions is pervasive, and may destabilize any knowledge representation on the basis of co-occurrences of words. The usual distinction between the data stored in a knowledge base, for example, the archived literature, and the inference engine is problematic in science, since with the choice of a theoretical perspective not only the relative weights of various pieces of data may change, but also the 'rules of the game' can be affected. Sciences are not haymaking machines which collect facts according to standardized procedures, but developing conceptual apparatuses. Accordingly, (co-)words are embedded in changing contexts. A declarative knowledge representation based on analysis of textual (co-)occurrences is *not* able to account for the dynamics of the sciences at various levels of aggregation.

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[49] A third aspect to the so-called 'indexer effect' can be specified on the basis of these results. In addition to (1) the previously signalled packing of the database in an index (Leydesdorff 1989b)–one creates a first (mostly intuitive) taxonomy and therefore any further clustering is by definition 'clustering of clusters'–and (2) the effect of the indexer not being a practicing scientist herself (Healey *et al.* 1986), (3) the selection of documents creates an additional effect at the level of the aggregated document set: phenomenologically similar words may or may not have different meanings at different moments in time and from different theoretical perspectives.

Chapter 6

From Words and Co-Words to Information and Probabilistic Entropy

In the previous chapters scientific texts were analyzed in terms of word distributions, and the question was raised of what these frequency distributions teach us at different levels of aggregation, and also how they relate at different moments of observation. We found both stability and change. It became clear that what is changing is not analyzable simply as a result of 'differences in the data' between two measurement points, since both the categories and the values of the variables are in flux. Not only do word distributions change, but also the meanings of words may change.

The two types of change refer to different theories. Already in 1955, Bar-Hillel hinted at the possibility of an *information calculus* enabling us to understand the statistical interpretation of word occurrences and their meaning in a single research design. In this chapter, I discuss the relation between this abstract notion of an information calculus and the empirical occurrences of words in texts.

6.1 WHAT IS INDICATED BY THE INDICATORS?

The statistical interpretation informs us in a very basic sense about the text as a system of signals defined at the (co-)word level. One could also have studied the system of signals at the character level (cf. Shannon 1948), but in science studies substantive reasons have been specified for looking at the aggregation process and the dynamics of networks in terms of word occurrences (Hesse 1980; see also Chapter Four).

In his noted study, Bar-Hillel (1955) argued that the smallest unit of meaning in a text is not the word but the sentence. He stated that "(h)alf sentences often do not have meaning." Therefore, he proposed to look at *words in sentences*. Words can have different meanings in sentences because of their different positions, argumentative functions, etc. Sentences, however, are part of the document structure. In this context, the unit of observation may still be the word, but the unit of analysis (and thus, the hypothesized system under study) is different. The occurrence of a word in a sentence is no longer considered the instance of that word as a nominal variable, but the instance of another category which first must be specified in terms of a theory about words in sentences. The latter theory refers to structure in meaning among language users, while in the co-word analysis model one has been primarily interested in word occurrences in textual data.

The scheme by which an author like Bar-Hillel rates word occurrences would be different from that of the co-word analyst: two different words may be instances of the same variable in a scheme which assesses word occurrences in terms of meaning (e.g., synonyms), and the same word may be rated on different variables in two different instances (e.g., because of its position in the sentence). In general, researchers using different theories can be expected to generate from the same data two (or more) different relative frequency distributions. The two measurements inform us in other dimensions about the subject under study.

Methodologically, this is the crucial step: the theoretical assumptions may be completely different, but in each case the result can be expressed as a relative frequency distribution. Both analysts are able to ascertain whether something, the relevance of which can be specified in terms of their respective theories, is the case or not; or has occurred or not; or is to be expected or not. The results of their empirical studies can be compared as formal representations. Formal representations can always be rewritten as a (potentially multivariate) probability distribution.

In summary, theories guide us in collecting the data, and in providing us with an interpretation of the results. Although theories can be mutually incommensurate, the results of the measurement are not necessarily incomparable. On the contrary, otherwise incompatible theories do not have to contradict one another in terms of the measurement; one expects a mutual information or co-variance[50] between the results when one studies the same subject in different dimensions.

6.2 THEORIES AND METHODS IN SCIENCE STUDIES

In quantitative studies of science, the need for theoretical specification in terms of structural units of science and in terms of various dimensions has often been recognized (e.g., Small *et al.* 1985; Mullins *et al.* 1988), but hardly ever have these units been made subject to substantive theorizing. The emphasis has been on the organization of data, the various methods of multi-variate analysis, and on graphic representations of the results ('mappings'). The intrinsic relations between research questions and the choice of various parameters in statistical methods, e.g. similarity criteria and clustering algorithms, is often not discussed, and thereby a vision of methods as only a kind of magical toolbox tends to be reinforced.

All relative frequency distributions, however, are the results of (sometimes implicit) theoretical assumptions concerning the subject of study. Each reconstruction contains theoretical assumptions, although these assumptions may have been used without further reflections. For example, a co-word mapping can be considered as a representation of a field of science. However, the field of science that is represented in the representation is something other than the representation itself. Various representations are possible. In the case of scientometric mapping, similarity criteria and clustering algorithms span a parameter space of possible representations.

One has to assume the existence of a field of study before one can appreciate the representation in a mapping. Thus, we return to the question of the bridging of the gap between qualitative theorizing in science studies and the use of scientometric methods.

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[50] Although differently defined the mutual information and the co-variance are both measures of the uncertainty in the co-variation.

In my opinion, further integration can be brought about only reflexively: methodologists should not consider their methods as theoretically content-free without further justification, and theoreticians should not consider methods and methodology as toolboxes which remain external to the theoretical enterprise. Methods contain the core of reflexivity concerning the quality of inferences, and therefore the reliability of theoretical statements.

Methodological reflection adds to the substantive understanding of how the specific contributions are positioned with respect to the subject of study: how much and which part of the variation can be explained from each of the available perspectives? The alternative of a programmatic choice of one theoretical perspective or another begs the question of interdisciplinarity. While theorizing guides us in generating research questions and in appreciating the results, methodology focuses on issues of validity.

6.3 METHODOLOGICAL REQUIREMENTS IN SCIENCE STUDIES

The above considerations allow us to list the methodological requirements for developing science studies as a more integrated enterprise. 'Second order' methods should allow us to compare not only results with expectations given one model or another, but also among different representations. As noted, methods are usually developed for specific theoretical purposes. However, all the theoretical models remain informed conjectures. Is it possible to develop methods which are yet content-free with reference to the (possibly 'incommensurate') reconstructions under study? Can the heuristic function of theory in data collection and its appreciative function in the interpretation of empirical results be uncoupled reflexively from the formal data analysis in between?

Additionally, such content-free methods should enable us to vary over levels of aggregation, measurement scales, and relevant variables. As noted, the specification of other relevant variables may imply the attribution of the same data to other possible units of analysis or the addition of other data with reference to the same unit of analysis. In the latter case, methods should allow for the specification of the *increment* in the information thus obtained. In the case of other units of analysis, methods should enable us to perform secondary analysis by using previous data collections and data analysis. Only if this latter requirement is warranted can one build on the data generated in the many case studies performed for other (e.g., policy) reasons.[51]

In summary, we are now able to specify the following requirements for methods in science studies:

1. Methods should make it possible actively to import data and results (e.g., descriptions, facts, trendlines) from other types of studies. One might call this *the requirement of secondary analysis.* Data analysis should support the translation among the various paradigms which are used in science study.

2. Methods should allow for variation in the types of theories and methods which use the same or similar data. They should therefore be permissive with respect to the research process, and not prescriptive in any strong sense.[52] This might be called the *requirement of multiple paradigms*.

In addition to these two requirements, we can also specify:

3. *the requirement of aggregation and decomposition.* Methods should allow us to control for the relations among levels of aggregation.

This latter requirement, however, holds not only when we move among levels of aggregation in one dimension. In empirical science studies, the researcher may wish to import, for example, information about developments in literary structures at the field

[51] This is an urgent question for scientometrics, since data collection is often too expensive for fundamental research, while contract money is often available for indicator work.

[52] Of course, if one wishes to use a particular method one should use it correctly.

level (e.g., journal structures) into a design which focuses on processes at the level of social (e.g., institutional) organization. The units of analysis at the different levels of aggregation are then heterogeneous. Thus, this leads to a fourth requirement which is a composite of the above requirements, i.e.:

4. *the requirement of 'heterogeneous nesting'* (Callon *et al.* 1983).

Furthermore, we do not require measurement to be more precise than nominal, since we wish to allow for historical and explorative research. Whereas description is a minimal requirement, more precise measurement is often possible.[53] For example, in an aggregate one may be able to specify not only whether something was the case, but also how often it was so. The number of nominal instances can then be counted at the interval level, etc.

One would like to be able to use any further information that can be achieved by more accurate measurement. Therefore, in addition to the above specified requirements, we may now specify as a fifth criterion for more integrative methods in science studies a permissive requirement with respect to the measurement technique:

5. Methods should allow for variation in the measurement scale of observations, but save any additional information from better measurement. This *requirement of neutrality in terms of the measurement scale* asks technically for a *non-parametric method*.

Actually, the use of non-parametric statistics is also convenient for the import of scientometric data since the distribution of this data is often skewed. Most multi-variate statistics, however, is based

[53] In my opinion, only contributions which involve at least the lowest measurement scale, that is, description in nominal categories, can be considered as part of *empirical* science studies. Note that philosophical contributions can be relevant for empirical science studies by using this criterion.

on assumptions concerning the shape (e.g., normality) of the distributions, and therefore one can additionally specify the following requirements for the methods that we look for:

6. *the requirement of multi-variate statistics*, i.e., methods for science studies should allow us to develop non-parametric equivalents of clustering algorithms, etc., on datasets which one can also compose and disaggregate. Higher-level results should be interpretable in terms of lower-level results, and *vice versa*.

Furthermore, one is interested not only in these complex data structures at each moment in time, but also in their development over time. Therefore, in addition to providing us with a full equivalent of 'multi-variate analysis,' methods should provide us with possibilities for studying time series of data, to make predictions, and to reconstruct. This leads to the formulation of two further requirements:

7. *the requirement of dynamic analysis*, i.e., methods should allow us not only to analyze (multi-variate) data in slices at each moment in time, but systematically to account for change in the various dimensions, and in relation to overall development.

8. *the requirement of reconstruction*, i.e., methods should enable us not only to analyze dynamically and multi-variately, but also to investigate irreversible transitions in time-series of (potentially multi-variate) data (cf. Arthur 1988). Note that the formulation of this requirement is in itself neutral with respect to the question of whether one analyzes historical descriptions or multivariate data sets.

Finally, with respect to the data we may formulate one additional criterion which pertains to the specificity of the domain of science studies:

9. *the requirement of virtually no systems limitations on the number of variables*, since methods should allow us to study complex phenomena and/or large communities and archives, that is, many variables, at both aggregated and decomposed levels.

Methods should preferably not only meet one or a few of these criteria, but make it possible for the analyst to integrate results from studies in which only a subset of them are needed. Therefore, methods should in principle comply with all these requirements. In other words: one type of analysis should be systematically relatable to another in terms of *specifiable* transformations.

In this respect, Shannon's (1948) classical information theory can be of help. The expected information content of a distribution is, among other things, non-parametrical, content-free, and definable in statical and dynamical measures. Theil (1972) has elaborated Shannon's formulas for the multi-level and the multi-variate case (see also: Krippendorff 1986). At this stage, however, my claim is not that information theory provides us with *the* method to be used in order to comply with all the specified criteria, but rather that information calculus is *a* useful method for this purpose. In the following chapters, this method is applied to a large set of problems in science studies in order to show the fruitfulness of the approach.

6.4 SUMMARY AND CONCLUSIONS

Bridging of the gap between qualitative theorizing and the use of scientometric methods is only one among a set of requirements for the further integration of science studies. In the preceding chapters, I elaborated the question of integration in science studies as a methodological issue. By using the example of the measurement of 'meaning' in terms of word occurrences in the semantic tradition and the measurement of word distributions in the semiotic tradition, I specified in this chapter how to relate the different meanings of data in science studies.

A set of criteria could be derived for methods in science studies which aim at integration, despite the noted differences at the theoretical and the methodological levels. By further reflection on some methodological issues, and issues in relation to the type of data involved, additional criteria for this purpose could be specified.

In information theory, information is formally equated to probabilistic entropy. The use of these measures as an integrative device in scientometrics relates to theories about dissipation in potentially self-organizing systems (e.g., Smolensky 1986; Swenson 1989; Leydesdorff 1994b). Scientometric indicators provide us with a rich domain in terms of complex and longitudinal data for testing hypotheses concerning the dynamics of cultural evolution (cf. Van Raan 1991). However, the elaboration of this relation with theories of entropical systems will be postponed to the final chapters of this study. I focus now first on the use of information theory as a methodology for data analysis.

PART II

METHODOLOGICAL STUDIES

USING INFORMATION THEORY

Chapter 7

The Static Model

In the following two chapters, I shall show that the expected information content of distributions provides us with a straightforward means to develop a static and a dynamic model for the development of the sciences. In this chapter, I analyze how knowledge about one indicator (variable) can reduce our uncertainty in the prediction of other indicators, and how relations across various levels of aggregation can be assessed. In the second study, I address the problem of the use of indicators and the relations among them for reconstructions over time. In later chapters, I shall turn to the issue of predictions.

The model studies are based on word occurrences in the same eighteen articles as in Chapter Five. As above, the occurrences of words will be used as nominal variables. By using the sequence number of the articles and the organization of each article in sections, the models can be generalized for the multi-variate case. Thus, any indicator or variable can be assessed in terms of its validity in relation to other indicators and its value for predictions.

7.1 THE INFORMATION MEASURE

If we define h as the information content of the message that an event occurred–e.g., that a variable had a certain value–then the expected information content of the distribution of a variable can be written:

$$H = \Sigma_i \; p_i \, h_i \tag{7.1}$$

in which h_i is the expected information of the event when the variable occurs with this value, and p_i the likelihood for that event to occur. Using Shannon's function for information[54] ($h_i = - {}^2\log p_i$), we may write:

$$H = - \; \Sigma_i \, p_i \, {}^2\log p_i \tag{7.2}$$

(The use of the binary base allows us to express the information in bits.) In the case of frequencies and relative frequencies, p_i can be replaced with f_i/n, and we derive:

$$H = {}^2\log n - (1/n) \, \Sigma_i \, f_i \, {}^2\log f_i \tag{7.3}$$

Figure 7.1 from Attneave (1959) exhibits the prediction relations among two variables:

$$H(x|y) = H(x,y) - H(y) \tag{7.4}$$

$$T(x,y) = H(x) - H(x|y) \tag{7.5}$$

$H(x|y)$ is the uncertainty (in bits of information) in the prediction of x if we know y; $T(x,y)$ is the 'mutual information' of x and y–also sometimes called the 'transmission'–i.e., the *reduction* of the uncertainty in the prediction of x if y is known. Hence, $T(x,y)/H(x)$ is the proportion of reduced uncertainty in the prediction of x if y is known, and $H(x|y)/H(x)$ is the proportion of uncertainty which has remained. $T(x,y)/H(x)$ is also a measure for the association between x and y, in one sense comparable to a chi-square, but asymmetrical in x and y.

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[54] The information *h* in bits received by a message in terms of the probability *p* that prevailed prior to the arrival of the message is equal to $-{}^2\log p$ (Shannon 1948; Theil 1972).

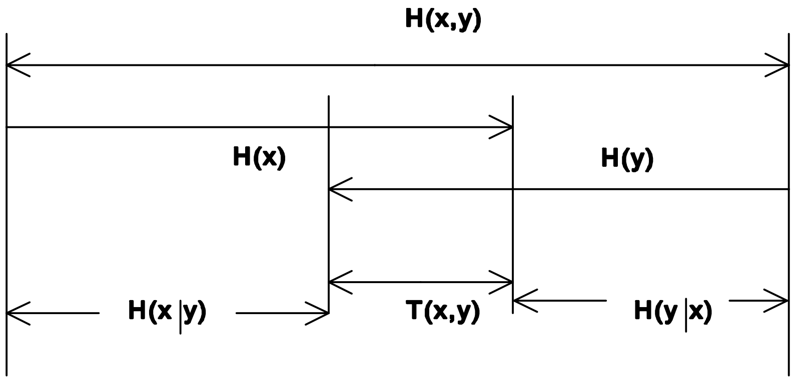

Figure 7.1
Relations of expected information contents, mutual information, and conditional entropies between two variables ***x*** *and* ***y****. (Source: Attnaeve 1959.)*

Elaborating on these measures, I shall address in the following chapters a number of questions central to science studies. My general point is that science studies, as an interdisciplinary field with a variety of epistemological and methodological standards, can benefit greatly from these measures: they pose virtually no constraints on measurement requirements (except at the nominal level), nor on the number of variables (since no mainframe capacity or complex statistical packages are needed), and they are extremely flexible with respect to the level of aggregation, since they are composed from additions (Σs).

As soon as one is able to specify notions about science, scientific progress, etc., in terms of things which can, for example, be observed in or attributed to scientific texts, a wide range of research designs becomes available. Whether focusing on words, finance in dollars, citations, logical inferences, problem shifts, or the results of content analysis, we are then able (1) to evaluate relations among variables at various levels of aggregation (this chapter), (2) to reconstruct scientific developments from the mere counting of occurrences of the specified nominal variables, and (3) to make predictions about the probability of their occurrence in new events.

In other words, the method is content-free with respect to the theoretical operationalization of qualitative questions (why this variable?) or–in the case of the building of an expert system–the specification of decision criteria (why use this indicator?) and the technicalities of the counting machinery: word occurrences are easier to count than, for example, logical inferences. This method is a data-analytical tool which informs us about the information contained in the data. Furthermore, the additivity of the information measure enables us to use new empirical data in order to improve on what has already been counted (measured) in the past, e.g., the data archived in the *Science Citation Index* and other such databases.

7.2 SAMPLE CHOICE

As noted, this model study is based on word occurrences in the same eighteen articles as in Chapter Five. As described there, the net result of the processing was a total of 1287 words, which occurred 28,422 times in the document set. Each of the articles contains four sections ('Introduction,' 'Methods and Materials,' 'Results,' and 'Discussion'). (See *Table 5.1* for the full bibliography.)

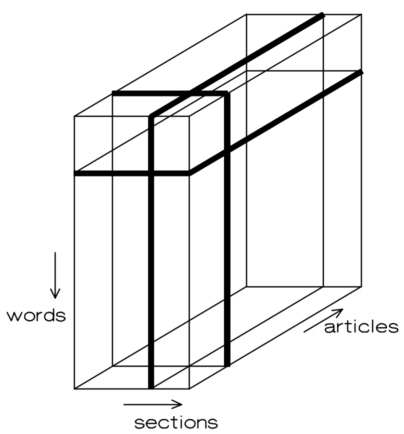

Figure 7.2
Schematic representation of relations among 18 articles with 4 sections, containing 28,422 occurrences of 1287 words.

This sample can be represented as a cube of variation in a three-dimensional space of words, sections, and articles (see *Figure 7.2*) containing on the order of 10^5 cells (i.e., 18 articles x 4 sections x appr. 1300 words). One expects the overwhelming majority of these cells to be empty.

7.3 RESULTS

I shall use the (s) of *s*ections, the (a) of *a*rticles, and the (w) of *w*ords as respective subscripts to the expected information content *H* associated with these dimensions. Note that unless otherwise indicated, the relative frequency distributions of the variables are defined in relation to the total number of 28,422 word occurrences for the whole set. Hence, H(w) should be interpreted as word distributions at field level, that is, as *the repertoire of the specialty*. H(w) should not be confused with word distributions within articles or sections. The latter are indicated with additional subscripts.

7.3.1 Relations between articles and the set

The set of articles can be considered as an operationalization of the specialty of *Dictyostelium discoideum*, and each of the articles as a contribution to this field, with an implicit notion of temporal sequence from 1981 to 1988 (see *Figure 5.2* above). We can thus calculate the extent to which the word distributions in the articles may be predicted from word distributions throughout the field, and vice versa. In the process of model specification of scientific developments, such knowledge may be useful for specifying the direction of the dependency of the two levels.

The matrix of margin totals over sections in the y-z plane of *Figure 7.2* contains total frequencies of occurrences for each word in each of the texts. The maximal information content of this matrix of 18 articles versus 1287 words can be expressed as:

$$H(\text{max}) = {}^{2}\log 18 + {}^{2}\log 1287 = 4.17 + 10.33 = 14.50 \text{ bits}$$

Actually, we find:

$$\begin{aligned} H(\text{words,articles}) &= H(\text{articles}) + H(\text{words}|\text{articles}) \\ &= (4.10 + 6.85) \text{ bits} \end{aligned}$$

or equivalently:

$$
\begin{aligned}
H(\text{words,articles}) &= H(w) + H(a|w) \\
&= 9.07 + 1.87 \qquad = 10.94 \text{ bits}
\end{aligned}
$$

This is only 75.4% of the maximal information content.

From the different values of these entropies, we may now calculate (using formulas 7.4 and 7.5 above) the reductions of uncertainty in the prediction of word occurrences in the articles, given prior knowledge of the word distributions over the whole set, and vice versa. The former reduction is:

$$\frac{T(a,w)}{H(a)} = \frac{H(a) - H(a|w)}{H(a)} = \frac{4.10 - 1.87}{4.10} = 54.4\%$$

The latter is:

$$\frac{T(a,w)}{H(w)} = \frac{H(w) - H(w|a)}{H(w)} = \frac{9.07 - 6.85}{9.07} = 24.4\%$$

Therefore, one may conclude that word frequency distributions in the articles are more than twice as predictable from word frequency distributions at the level of the aggregate, than is the case vice versa. Were we to accept word occurrences as the medium of transmission between scientific articles and scientific specialties, these results would count directly against the 'micro-constructivist' hypothesis in science studies, which requires the structure in the aggregate to be explained in terms of individual acts, and not vice versa.

7.3.2 Dimensions of the transmission

A further question with respect to the relations between the specialty and article level arises, when we take into account the fine structures of the articles in sections. What are the functions of the various sections with respect to the coupling mechanisms between articles and field? Is the transmission brought about by words in the 'Methods and Materials' section different from that of the 'Discussion' or 'Results' sections? Answers to these questions can inform us further about the warp and the woof of the fabric of a given specialty: which dimensions are determined more by the field level, and which are more open to deconstruction and reconstruction in individual contributions?

In terms of the cube in *Figure 7.2*, the calculation is analogous to the previous one, but using now each of the slices along the y-z plane corresponding to the respective sections instead of the margin totals of frequencies across the sections. The results of the calculations are summarized in *Table 7.1*.

	$H_{articles}$	H_{words}	$H_{a\|w}$	$H_{w\|a}$	N
Intro	4.06	8.21	1.38	5.52	2,568
Methods	3.90	8.31	1.84	6.25	6,095
Results	4.05	8.78	1.63	6.36	12,448
Discussion	3.96	8.74	1.45	6.22	7,311
Average	4.00	8.61	1.60	6.22	
Full text	4.10	9.07	1.87	6.85	28,422

Table 7.1
Entropies in bits of Information, disaggregated at the section level.

The entropies of the sections are lower in both dimensions than the entropies of the full texts, as expected, since in addition to the entropies within groups, there should be some entropy between sections.[55] The latter, however, are small, both at article and at field level:

[55] $H = H_0 + \Sigma_s Q_s H_s$, in which Q_s is the weighing of the contribution of the entropy of each of the sections, and H_0 is the between-sections entropy.

$$\frac{H(a) - (Ha|s)}{H(a)} = \frac{4.10 - 4.00}{4.10} = 2.4\ \%$$

$$\frac{H(w) - H(w|s)}{H(w)} = \frac{9.07 - 8.61}{9.07} = 5.0\ \%$$

However, transmission between the word distribution at field level and distribution at article level (T(w,a|s)) has increased by 7.6% from 2.23 bits to 2.40 bits, when we take the average of the section structure into account.

What does this mean? First, the low values of the between-sections entropies support the conclusion from Chapter Four that the section structure in articles is highly informative: the word distributions in each of the sections contain almost as much (unweighted) information in relation to the field as do the articles as a whole. Among the articles, at the field level, there is somewhat more exchange between sections, leading to a conditional entropy twice as large for the field in comparison with the articles (5.0% versus 2.4%).

Further specification of the coupling between field and articles in the four dimensions of sections reveals that the introductory section functions as the strongest 'transmitter' (or 'mutual informer') between these two levels. (Remember that we found in *Figure 4.6* an intermediate position of sentences in the introductory section by using discriminant analysis.) If we know the distribution of the words in the introductory sections at the level of the field, the prediction of the uncertainty of the use of words in the introductions is reduced by 66.0%, compared with only 52.8% for the methods sections (see *Table 7.2*). In other words, coupling between article and field in terms of word-occurrences is weakest in the methods sections: this sample is integrated less in terms of methods than in terms of theorizing, if we use word occurrences as the measure.

	Articles | words	Words | articles
Introduction	66.0	32.8
Methods	52.8	24.8
Results	59.8	27.6
Discussion	63.4	28.8
All text	54.4	24.4

Table 7.2
Percentage Reduction of Uncertainty in the Prediction, differentiated by section.

From this table, we may further conclude that the transmission as a percentage of total variance in each of the sections is higher in both directions than for the word distributions over the texts. Part of the transmission is section-specific. However, with the exception of the introduction, this effect is much larger from the field level to the article level than vice versa. More than the other sections, the introduction section of each article also contributes actively to the repertoire at the field level.

The results correspond to our intuitive understanding of the relations between the field level and the article level. In the introductory sections, and to a lesser degree in the final discussion, one expects authors to make a deliberate attempt to legitimate their work in terms of current problems in the specialty, and hence to provide a stronger coupling between articles and field. This effect is mutual in the sense, that the authors both orient themselves with regard to the field, and try also to show by their intentional choice of words that they are doing so.

In the 'Methods & Materials' sections one expects much more codification from the field or specialty level, i.e., one expects the papers' vocabularies to be derived more from the specialties' vocabularies. However, this does not need to be the vocabulary of the field in the narrow sense of the subject area of the specialty. Our focus in sampling was on the subject matter *Dictyostelium discoideum* independently of the use of various biochemical methods, and this may lead to the lower transmission in this dimension. However, in general, we may conclude, in the light of our previous considerations about the different dimensional characters of sections, that the dynamics of word pattern relations are to a large extent specific for sections. In the next chapter, I return to these different functions in terms of their respective dynamics.

7.4 GENERALIZATION FOR THREE DIMENSIONS

In the previous section, I repeated the calculations for each of the dimensions and then compared the average within-group entropy with the overall entropy. A more direct approach to this problem would be to generalize it to three dimensions, as represented by the cube in *Figure 7.2*. Although the results remain essentially the same, the extension becomes particularly fruitful in terms of clarity of interpretation if we want to extend the analysis to more than three dimensions, for example, in the case of including still more levels of aggregation.

The three dimensions considered hitherto are the repertoire of the words (w) at the field level, which contains 1287 words (n = 1,....., 1287); the 4 sections (s) of each of the articles; and the 18 articles (a). I will write Q_{wsa} for the number of occurrences of word w in section s of article a, measured as a fraction of the total number of word occurrences (N = 28,422).

One can, for example, detemine such things as how much uncertainty there is, in general, regarding the occurrence of a randomly selected word at the field level, and by how much this uncertainty is reduced by information about how frequently it occurs in the four sections and/or in the 18 articles. Alternatively, one may compute how much the uncertainty about whether a given word occurs in an article is reduced by information about the distribution of the words over the articles, etc.

One measure of the uncertainty about the relations among articles that prevails before any knowledge is available about which words are at stake is the (one-dimensional) marginal entropy of the articles in terms of total numbers of words per article:

$$H(a) = - \Sigma^{18}_{a=1} Q_{..a} \log Q_{..a}$$

(The dots are conventional for margin totals.)

Since there are 18 possibilities, this entropy may assume values between 0 and a maximum of $^2\log 18 = 4.17$. Its actual value can be computed as:

$$H(a) = 4.10$$

This is the upper limit for the uncertainty of the distribution of all words over the articles. Obviously, this distribution is almost random: 4.10 is 98.3% of 4.17.[56]

When we know which words of the field repertoire occur, and in which section, we can reduce this uncertainty, notably with:

- $H(a|w)$ if we know distributions of words over the field;
- $H(a|s)$ if we know how the texts are organized in sections;
- $H(a|w,s)$ if we know both these distributions.

Instead of calculating the conditional entropies of each dimension given a second dimension, and then–as in the previous section–averaging over the third, we can also now calculate the joint contribution of two given distributions to the uncertainty in the third dimension directly, using the formula (Theil 1972, pp. 157f.):

$$H(x|y,z) = H(x,y,z) - H(y,z)$$

[56] The same procedure can be followed in the other two dimensions: the maximal entropy for the words at field level is $^2\log 1287 = 10.33$ bits, and for the sections $^2\log 4 = 2$ bits. From the marginal distributions, entropies of 9.07 (= 87.8%) and 1.83 (=91.6%), respectively, can be calculated. Therefore we may conclude that the reduction of uncertainty by only marginal distributions is largest in the dimension of the field, i.e., when we know only the words of the repertoire, followed by the dimension of the sections, but that the distribution of word occurrences over the articles–regardless of which words are involved–is almost random.

	H(sections)	H(articles)	H(words)
	1.81	4.10	9.07
given:			
sections		4.00	8.61
articles	1.71		6.85
words	1.36	1.87	
a,w	1.09		
s,w		1.61	
a,s			6.23

Table 7.3
Entropies and Conditional Entropies for Three Dimensions (in bits of information)

Table 7.3 gives the values of H for all relevant distributions in bits; *Table 7.4* the various transmissions which can be calculated from the former table as percentages. In interpreting these tables, one should keep firmly in mind that in the *w*-dimension we are considering the 1287 words at the field level, and hence the repertoire as a variable of the field.

	T(sections)	T(articles)	T(words)
given:			
sections		2.4	5.1
articles	5.5		24.5
words	24.9	54.4	
a,w	39.8		
s,w		60.7	
a,s			31.3

Table 7.4
Transmission in Percentages among Three Dimensions

Note firstly that a combined knowledge of word distribution over the field ('the repertoire') and of the distribution of occurrences over the sections ('the organization') reduces the uncertainty in the prediction of the word occurrence in articles by 60.7% (see *Table 7.4*). On the other hand, knowledge of the distribution of word frequencies both in the articles and over the sections reduces uncertainty in the prediction of the word distribution over the field by only 31.3%–which is almost half the former value. In general, knowledge from the field level is more predictive for the distribution in the other two dimensions than vice versa.

Secondly, as noted earlier, coupling between sections on the one side, and articles and the field on the other, is almost absent in this direction (2.4% and 5.1%, respectively). Yet the dependency of the distributions at section level on the distributions at field level does not vanish (24.9%), indicating the extent to which the field bypasses the article level in determining the sectional organization of the articles. This confirms again that the organization of words into sections indeed constitutes a separate dimension.

Much more can be said about the interpretation of the figures in *Tables 7.3* and *7.4*. For example, one is also able to calculate *partial entropy reductions*, e.g., $\{H(a|s) - H(a|s,w)\}$–this happens to be (4.00 - 1.61 =) 2.39 bits–as the incremental reduction of the uncertainty in the prediction of the word distribution in articles due to knowledge about word distribution at the field level, given that the distribution over the sections was already known. (Since the entropy reduction is additive in character, the transmission in percentages by partial entropy reductions may also be derived directly by subtraction and addition.)

What do we gain after we have analyzed articles in terms of word distributions, having taken sectional divisions into account, by the inclusion of the next higher level of aggregation (the set of articles or 'the field') into the analysis? Obviously, quite a bit: 2.39 bits is 58.3% of the 4.10 bits of the $H(a)$; hence, by doing so we have reduced the uncertainty in the prediction of the word occurrences in the articles by more than half. The substantive implication is that it makes little sense to compare word occurrences among two texts without also taking the context of the set into consideration.

7.5 THE AGGREGATION PROBLEM

Let us now address the question of the relations between levels of analysis by comparing the articles with the set of articles, in terms of distributions of word frequencies over sections. In other words, we will decompose the 'repertoire' at the field level in terms of the contribution of each of the articles as a subgroup, and we will then compare the results with the results of another analysis in which we also take the intermediate level of the sectional division into account.

Recall that H(words)–'the repertoire' or the distribution of word occurrences at the higher level of aggregation–was 9.07 bits. Each of the 18 articles contributes a proportional share to this entropy:[57]

$$H(\text{words}) = H_0 + \Sigma_{i=1}^{18} (n_i / N)\, H(w)_i$$

For each of the 18 articles, we can calculate the univariate distribution of words $H(w)_i$, and by using the number of word occurrences in each article over the number of total word occurrences (28,422), we may calculate a weighted average value for $H(w)_i$ using $\Sigma_{i=1}^{18}$ n(article) / N as weighing factors. H_0 is then the part of the repertoire of the field which cannot be decomposed into the contributions of the various articles; it is the 'in-between articles' part of the repertoire.

However, we can also study each of the articles in terms of its own contribution to the repertoire. In our case, H(words) is already known to be 9.07 bits, and H(words|articles) was computed above

[57] A weighted average is appropriate since the entropy is additive (Theil 1972, p. 18.)

(*Table 7.3*) as 6.85 bits; hence the 'in-between articles' part of the vocabulary constitutes 2.22 bits, or 24.5% of the vocabulary at field level. The precise composition of the contributions of each of the 18 articles to the 'vocabulary' can be calculated using:

$$\frac{n_i \cdot H(w)_i}{N \cdot H(w)} \cdot 100\%$$

$Article_i$	$H(w)_i$	% Contribution
1. (BE81)	5.85	2.0 *
2. (OL82)	6.89	3.9
3. (WE82)	6.91	4.0
4. (RU83)	7.16	5.6
5. (MC83)	7.23	4.6
6. (RU84)	6.79	4.2
7. (GU84)	6.92	5.6
8. (MA84)	7.22	5.9
9. (TA85)	6.16	4.2
10. (BI85)	7.54	5.3
11. (JA86)	6.47	3.9
12. (HA87)	5.94	2.9
13. (SH87)	6.96	5.6
14. (KO187)	7.29	6.5
15. (KO287)	6.86	5.0
16. (MU88)	6.69	3.0
17. (KL188)	6.24	1.9 *
18. (KL288)	6.36	1.7 *
Average	6.85	75.5
	Average	4.2
	St.dev.	1.4

Table 7.5
Contribution of the repertoire in each of the articles to the repertoire of the specialty

This leads to *Table 7.5*.

It is clear that the asterisked first text (*Bernstein et al. 1981*) and the last two texts (*Klein et al. 1988a and 1988b*) contribute significantly less than average to the vocabulary of the field,[58] which may be an effect either of their relative brevity (see the discussion in Chapter Five) or of their excentric positions (see *Figure 5.2*), or of interaction between these two factors.

Entropy at the aggregated level is always higher than average entropy at the lower level, unless all units at the lower level (articles) happen to have exactly the same entropy, that is, in this case word distributions. The difference ($H(w)_0$) between the higher-level H(words) and the lower-level average is a simple measure of the specificity of the vocabulary that prevails in the articles of the set (Theil 1972, p. 66).

Note that the weighted average ($\Sigma\ Q_i * H(w)_i$) of the contribution of all articles to the reduction of uncertainty at the field level is equal to the conditional entropy H(w|a) at field level, given the distributions in the articles in the case that the lower-level units are the composing elements of the higher-level unit;[59] and also that $H(w)_0$, the 'in-between articles' contribution to the entropy, is equal to H(w) - H(w|a), or to the transmission between the field level and the article level in terms of word occurrences.

The next step is to extend this analysis to the bivariate comparison of texts in terms of words and sections. The average H(w|a,s) was 6.22 bits (*Table 7.3*), against an overall value for H(w|s) of 8.61 bits. The partial entropy reduction is 72%, as against

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[58] Since one may expect this contribution to be distributed normally, it is legitimate to calculate a standard deviation in this case.

[59] $\Sigma_i\ Q_i * H_i = \Sigma_i\ (n_i / N) * (\Sigma_j\ q_j \log 1/q_j)$

$$= \Sigma_i\ \Sigma_j\ (n_i / N) * (f_{ij}/n_i \log n_i/f_{ij})$$

$$= \Sigma_i\ \Sigma_j\ \{(n_i \log n_i) - f_{ij} \log f_{ij})\}/ N$$

$$= \Sigma_i\ \Sigma_j\ \{(f_{i.} \log f_{i.} - f_{ij}\log f_{ij})\}/ N$$

This result is identical to the conditional entropy, as can be seen by combining formulas 7.3 and 7.4 above.

75.% in the former case. Therefore, the average contribution of the articles to the field repertoire is slightly decreased if we take the sectional distributions into account. Our conclusion is again that the sectional structure is on the average more field-dependent than article-dependent; its inclusion therefore depresses the average dependency of the field on the articles.

7.6 WHICH WORDS?

An important next question, also in relation to information retrieval, is the question of how much certain words contribute to the entropy, whether or not they are specified into the various sectional dimensions of the articles.

Above, H(a) was calculated as 4.10 bits. This is the distribution of the margin totals of word occurrences over the articles, irrespective of which particular word is involved, or where it occurs in the article. Each of the 1287 words involved contributes a proportional share to this entropy:

$$H(a) = H(a)_0 + \sum_{i=1}^{1287} (n_i / N)\, H(a)_i$$

As we saw above, the right hand term of this equation is in this case also equal to $H(a|w) = 1.18$ bits, and $H(a)_0$ is equal to the transmission of 2.23 bits. Therefore, we may note that the 'in-between words' contribution to the total entropy is larger than the sum of the entropy contributions of the words themselves. The nature of this 'in-between words' contribution to the entropy merits a separate study because of its importance for searching with combinations of words in information retrieval.

Table 6

Relative contribution of words at field level to the reduction of uncertainty in the prediction of word occurrences in articles; overall and section specific.

% of H(a|w)	
Figure	3.44
Bind	3.43
Protein	2.96
Cell	2.71
c-AMP	2.35
Mm	2.16
Activity	1.90
Show	1.65
pH	1.48
Site	1.43
Sequence	1.34
Subunit	1.33
Use	1.25
Dictyosteli	1.11
Degree	1.08
Concentr	1.08
Minute	1.06
Describ	1.06
Discoid	1.04
Ml	1.03
Ezyme	1.02
Contain	1.01

% H(a|w,s=intro)	
Cell	10.29
c-AMP	5.22
Protein	5.07
Bind	4.10
Discoideum	3.83
Dictyostelium	3.12
Sequence	3.05
Subunit	2.52
Site	2.52
Activity	2.16
Eukaryote	1.58
Enzyme	1.53
Structure	1.51
Form	1.30
Nucleotide	1.26
Show	1.05

% H(a|w,s=methods)	
Ml	4.44
pH	4.01
Minute	3.70
Cell	3.31
Mu	3.27
Degree	3.17
Buffer	3.15
Protein	2.75
Describe	2.46
Gel	2.45
Contain	2.42
Use	2.07
Time	1.81
c-AMP	1.37
Acid	1.35
Activity	1.30
Bind	1.27
Concentration	1.07

% H(a|w,s=results)	
Bind	4.43
Show	3.06
c-AMP	2.97
Activity	2.74
Protein	2.60
Subunit	1.79
Site	1.41
Cell	1.34
Table	1.32
Concentration	1.19
Sequence	1.13
Enzyme	1.07
Peak	1.04
Label	1.03
Experiment	1.03
Use	1.03

% H(a|w,s=discussion)	
Protein	4.64
Site	2.83
Cell	2.70
Sequence	2.46
c-AMP	2.18
Dictyostelium	2.11
Discoideum	2.02
Subunit	1.89
Show	1.53
Activity	1.48
Figure	1.41
Use	1.36
Enzyme	1.31
High	1.20
Concentration	1.18
Structure	1.14
Nucleotide	1.09
Acid	1.09
Data	1.04

Here, we focus rather on the independent contributions of words to the reduction of uncertainty in the predictions of word occurrence in the articles. Words do not contribute equally to this reduction of uncertainty. *Table 7.6* gives these contributions for the words that contribute more than 1% to H(a|w), for both the general repertoire of the field (operationalized as the 18 articles), and for the conditional cases of various sections, respectively. *Thus, we have been able to achieve the objective of the study reported in Chapter Five, that is, to rank words in terms of sectional dimensions.*

Let me stipulate that in this analysis we used *all* the words in our sample which occurred at least three times in a text and were not trivial, while of course one may wish to limit the analysis to only a certain set of words, for example those which have been previously selected as keywords. It will be clear by now that the additivity of the entropy measure allows us to calculate the reduction of uncertainty in the prediction as a percentage of H(a|w), and hence to assess quantitatively how reliably the subset may function as an indicator for the total set (cf. Salton and McGill 1983, pp. 63 ff.).

7.7 CONCLUSIONS

The purpose of the above application of statistical decomposition analysis to five problems is mainly to sensitize the reader to the wide range of questions in science and technology studies, and in other (e.g., sociological) studies as well, which can be addressed by this method. Actually, the method provides us with a tool for developing models by which to test almost all of the currently most relevant empirical questions of science and technology studies without posing *a priori* methodological constraints.

First, the requirement of nominality is the weakest condition for measurement: the researcher has only to be able to discern whether what s/he is looking for is the case or not. Secondly, with respect to the number of variables, we do not need sophisticated computer software with inherent systems limitations, since the *log*-

function is directly available in most database-management systems. Thirdly, the aggregation problem boils down to a further extension of the dimensionality of the problem. This approach makes the multi-level problem less acute in most research designs.

In addition to these methodological advantages, the model is insensitive to the theoretical background of the research questions we wish to pose: for the testing of a hypothesis or the evaluation of an indicator, it does not matter whether we generate the specification of the variables and their relations from inductive reasoning, with bold conjectures, or–as in the case of indicators–for pragmatic and utilitarian purposes. Precisely in interdisciplinary fields such as science studies, where there is little consensus at the epistemological or methodological levels, this method provides a means of introducing more rigor into the comparison of results and the testing of theories.

The analysis was pursued here using word occurrences as an example of a nominal variable, that is, a variable which can be counted in a text. I extended the analysis to the section as a second nominal variable, and to the articles as a third. However, a similar study could be done using other variables, for example citation counts. Analogously to the design used here, we might ask: how much do citation counts in each of the texts inform us about the distributions of these counts at the field level, and vice versa, and to what extent does the field perspective improve our prediction at the article level? Or, how are citation distributions related to word distributions, and do these relations among indicators vary across fields? In summary, such mutual information values offer an instrument for validation studies of indicators (e.g., Leydesdorff 1992a).

Chapter 8

Modeling the Dynamics of Scientific Developments

From the perspective of studying the dynamics of science and technology, we are interested not so much in relations among variables or indicators, but in the prediction of an event, given comparable events about which we already have knowledge. The quality of the prediction can be measured by the expected information value of the message, which converts the *a priori* probabilities of the events stored in the knowledge base into *a posteriori* probabilities, including the new event. This measure enables us to test hypotheses concerning the reconstruction of scientific developments.

In the static analysis the sequence number was used as one among other variables. Questions were then raised about the extent to which one variable co-varies with another. In the dynamic model, we change the perspective. Here, we are not interested in the question of whether variables co-vary with sequence numbers on a time axis, but in the comparison of events. The central question is not about the co-variation, but about whether the values of variables in earlier instances can provide us with *a priori* probabilities about future events. If an event actually occurred, we can compare the actual values with the predicted ones, and thereby calculate how much information the message, which accounts for the difference between the actual values and the predicted ones, is expected to give us.

The possibility of predicting in terms of specified variables with hindsight, and of comparing the predictions with what actually happened in history, quantitatively addresses the problem of how to distinguish between historical order and systematic order in reconstructive research designs. For example, if historians or sociologists of science claim that the intellectual history which they write highlights a significant lineage, *ipso momento* that they should be able as historiographers to articulate the significance of that lineage in terms of things which can be observed in or attributed to texts, we would be in a position to test their accounts against the variance in the relevant domain(s). Alternatively, if one claims following Lakatos that science can be rationally reconstructed using progressive problem shifts as an indicator, and if a philosopher were able to specify what constitutes the occurrence of such a problem shift, one could use such occurrences as a nominal variable with a certain distribution at any moment in time in the specified domain(s), compare this distribution with those in later events, and raise the question of the differences between the systematic order and the historical order.

A third perspective is the possibility of aggregating texts to representations of fields, and subsequently to assess the expected information change brought about by the addition of a new text to this archive. One can compare not only two texts, but also one text, notably a new text, in relation to the information stored in one or more archives, operationalized as sets of texts. From there it seems only one step toward building artificial intelligence from scientific texts.

8.1 METHODS

The research reported in this chapter is based on the same word occurrences found in the eighteen articles used in the previous chapter. Since each of the articles can act as an (*a posteriori*) receiver of information and as an (*a priori*) sender of information, $18 \cdot (18 - 1) = 306$ transmissions are possible.

Whenever necessary, to limit complexity and computation, I focus the analysis on the case of *Janssens et al. 1986* at the receiving end, and on *Bernstein et al. 1981* at the sending end of the transmission. As the reader may remember, these two articles were both co-authored by Van Driel, who was our original informant in the selection of the sample (see Chapter Five).

The relative frequencies of word occurrences available in the sending text(s) can now be regarded as the *a priori* probabilities, and the same values in the receiving text as the *a posteriori* probabilities. The expected information value (i) of the message which turned the *a priori* probability (p) into the *a posteriori* probability (q) can be derived straightforwardly from the information measure h as used in the previous chapter. If event E ultimately occurs:

$$i(q) \qquad = h(p) - h(q) = \log (q / p) \tag{8.1}$$

Theil (1972, pp. 56 ff.) explained this as follows:[60]

> (W)e proceed *under the condition that E (event) ultimately does occur*. The starting point is the prior probability p, the endpoint is the certainty that E occurred, and between these two points two alternative routes will be considered: one in which (...) a message is received that transforms p into q followed by a second message that transforms q into 1 (certainty), and a second route in which p is directly transformed into 1 with no intermediate step. Since the initial situations (probability p) and the eventual situation (certainty) are the same for the two routes, we shall require that they have the same total amount of information; (...) The information provided by the second (direct) route consists of the single value $h(p) = -\log p$. The total

[60] Alternatively, the relation between the dynamic perspective and the static perspective can be described in terms of the information content of the message which turns the maximal entropy of a distribution ($\log n$) into a distribution with expected information content H. This would be the case if the prior probabilities were all equal, since then each p_i would be equal to $1/n$, and hence, in this case:

$$\begin{aligned} I(q{:}p) \qquad &= \Sigma_i\, q_i \log (q_i/(1/n)) = \log n - \Sigma_i\, q_i \log q_i \\ &= H(\max) - H(q_i) \end{aligned}$$

information provided by the first route is equal to the information of the message that transforms p into q, plus $h(q)$.

The probability that the event will ultimately occur is only q, and hence:

$$I(q{:}p) = \Sigma_i\, q_i \cdot i(q)$$

$$I(q{:}p) = \Sigma_i\, q_i \cdot {}^2\log (q_i/p_i) \qquad (8.2)$$

The expected information value I is an inverse measure of the quality of the prediction.[61] In a perfect prediction we would need no additional information, and therefore I would vanish (log(p/p) = 0).[62] In the case of change, it can be proven that I is always positive (Theil 1972, pp. 59f.). In general, a message of change provides us with uncertainty or information. But without any *a priori* probability of the expected information (p= 0), an event ('emergence,' 'discovery') would come as a total surprise, so that I becomes infinite. (I return to the case of 'emergence' in a later section.)

Because of the property of additivity in entropy, we may generalize to the case where we have more than one *a priori* probability distribution, as in our example of a set of texts. The probabilities from the 'later' instance–independently of whether 'later' refers here to 'later in time' or, for example, 'later in the sense of after aggregation' or otherwise–may be regarded as revisions p_i' of the prediction; and with hindsight, we can calculate whether these revisions made the forecast more accurate ('information improvement') or worsened the prediction:

$$\begin{aligned} I(q{:}p) - I(q{:}p') &= \Sigma_i\, q_i \log (q_i / p_i) - \Sigma_i\, q_i \log (q_i / p_{i'}') \\ &= \Sigma_i\, q_i \log (p_i')/ p_i) \qquad (8.3) \end{aligned}$$

[61] If two is used as the base for the logarithm, all values for I are again in bits of information.

[62] In other words, any event is a perfect prediction of itself.

Note that while each *I* is positive, the revision of the production does not have to be positive. Furthermore, for bivariate and multivariate cases, we can deduce now the corresponding formulas like:

$$I(q_{ij} : p_{ij}) = \Sigma_i \Sigma_j q_{ij} \log (q_{ij} / p_{ij}) \qquad (8.4)$$

$$I(q_{ij} : p_{ij}) - I(q_{ij} : p'_{ij}) = \Sigma_i \Sigma_j q_{ij} \log (p'_{ij} / p_{ij}) \qquad (8.5)$$

8.2 CHANGES IN DISTRIBUTIONS OF WORD OCCURRENCES AMONG TEXTS

Let us first apply the model to the univariate case of word occurrences in each of the eightteen texts of the subfield operationalized in the previous chapter. In this case, we compare the expected information values of the *a posteriori* probabilities of word occurrences in each of the texts (rows in *Table 8.1*) with the word occurrences in all other texts (columns) as *a priori* probabilities; the difference is expressed in terms of bits of expected information of the respective in-between messages. The comparison in this analysis is based on those words which the respective texts have in common. (As noted, we return to the case of emerging word occurrences in a later section.)

If we focus on *Janssens et al. 1986* (the italicized row in *Table 8.1*) we see that the expected information value in comparison with *Bernstein et al. 1981* is much larger than any of the expected information values from the other comparisons. However, we knew that these two articles were closely related, and since high expected information value is consistent with significant differences between *a priori* and *a posteriori* relative frequencies, this seems a counter-intuitive result.

	BE81	OL82	WE82	RU83	MC83	RU84	GU84	MA84	TA85	BI85	JA86	HA87	SH87	KO187	MU88	KL188	KO287	KL288
BE81	0.0	1.0	0.5	1.3	1.5	1.9	0.9	1.1	0.5	1.4	1.7	1.1	0.8	1.3	1.1	0.9	0.5	1.2
OL82	1.0	0.0	0.6	1.1	0.4	1.1	0.9	0.7	0.9	0.8	0.8	0.8	1.0	1.1	0.7	0.8	1.1	0.9
WE82	0.4	0.6	0.0	0.4	0.8	0.4	0.7	0.5	0.5	0.7	1.1	0.7	0.7	0.8	0.5	0.4	0.6	0.3
RU83	1.0	0.9	0.4	0.0	1.0	0.5	0.8	0.9	0.8	0.7	1.2	1.2	0.9	0.8	0.7	0.7	0.3	0.5
MC83	1.2	0.4	0.5	1.0	0.0	0.7	0.9	1.0	0.7	0.8	0.6	0.7	1.1	1.0	0.6	0.8	0.8	0.5
RU84	1.6	0.9	0.4	0.5	0.8	0.0	0.5	0.7	0.7	0.4	0.7	0.7	0.9	0.5	0.6	0.7	0.6	0.4
GU84	1.1	0.8	0.5	0.7	0.9	0.5	0.0	0.8	1.3	0.5	0.7	0.6	0.8	0.5	0.5	0.5	0.3	0.8
MA84	0.9	0.7	0.6	1.1	1.1	0.8	0.9	0.0	0.5	0.7	0.8	0.9	0.7	0.9	0.9	0.6	0.6	0.4
TA85	0.6	0.7	0.4	0.6	0.6	0.6	2.1	0.6	0.0	0.6	0.2	0.4	0.7	0.6	0.4	1.5	0.6	0.4
BI85	1.2	0.7	0.6	0.5	0.9	0.4	0.6	0.7	0.5	0.0	0.7	0.8	0.8	0.6	0.7	0.7	0.5	0.4
JA86	***1.6***	***0.6***	***0.8***	***0.9***	***0.5***	***0.5***	***0.5***	***0.9***	***0.3***	***0.7***	***0.0***	***0.7***	***0.9***	***0.4***	***0.4***	***0.4***	***0.3***	***0.4***
HA87	1.4	0.9	0.6	1.0	0.8	0.5	0.5	1.0	0.5	0.8	0.6	0.0	1.3	0.5	0.5	0.7	0.3	0.5
SH87	0.8	0.8	0.7	0.7	1.2	0.8	0.6	0.8	0.6	0.7	0.8	1.2	0.0	0.8	1.0	0.8	0.3	0.7
KO187	1.4	0.9	0.8	0.8	1.0	0.4	0.5	1.0	0.5	0.7	0.3	0.4	0.8	0.0	0.6	0.5	0.6	0.4
KO287	1.7	0.8	0.6	0.8	0.6	0.5	0.5	1.2	0.4	0.8	0.4	0.4	1.4	0.4	0.0	0.6	0.3	0.4
MU88	1.0	0.7	0.3	0.7	0.9	0.6	0.5	0.6	1.0	0.5	0.5	0.8	1.0	0.5	0.6	0.0	0.4	0.2
KL188	0.6	1.1	0.5	0.3	0.9	0.7	0.3	0.7	0.5	0.6	0.3	0.3	0.4	0.6	0.4	0.4	0.0	0.4
KL288	1.1	0.6	0.3	0.5	0.5	0.4	1.2	0.4	0.4	0.4	0.4	0.5	0.7	0.5	0.4	0.2	0.3	0.0

Table 8.1

Expected information values in bits from the comparison of each of the articles (rows) with other articles in the set (columns)

Remember that *Bernstein et al. 1981* (BE81) is the only article of the set which is not from *Biochemistry* but from *Cell Differentiation*. If we compare this article with all the other articles both over the row and the column in terms of the expected information values of the messages based on changes of relative word frequencies, we see that *Bernstein et al. 1981* is deviant overall. Among other differences, it is much shorter than the other articles.

Thus, it becomes clear that relative word frequencies may differ for entirely different reasons, such as, for example, variation in background, 'distance' between the articles in space, or 'distance' in time, that is, words which have changed their meaning in time. Without further theorizing, we seem unable to specify the differences among these sources of variation. However, as soon as we make certain hypotheses, we can test them.

For example, we can test the suggestion above that the relatively large 'distances' of *Bernstein et al. 1981* is caused by the difference in repertoires among journals.[63] If the reason for the deviance in *Bernstein et al.*'s vocabulary were based mainly on the differences between the two journals, we would expect the *information improvement* when we distinguish among the sections to reveal closer relations between this article and *Janssens et al. 1986*, since word usage within the sections would presumably be more similar between these two when we control for the size effect in the other variable.

To calculate the information improvement, we have to extend the uni-variate analysis to the bi-variate by comparing the matrices of sections versus words for each of the articles on the basis of formulas 8.4 and 8.5 (given above). Unless all word occurrences were to be divided equally among the sections, we gain information by looking at more details, and therefore we can expect the information improvement to be positive. The increase will be minimal if the sectional division of word occurrences is more the

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[63] In reaction to a draft of the manuscript Van Driel commented that "word usage, indeed, is a bit dependent upon the journal to which one wants to submit the article."

same among events after we have accounted for the overall distributions.

	I_i	I_{ij}	$(I_{ij} - I_i)/I_i$ as %
BE81	1.59	1.96	23.1
OL82	0.61	1.12	82.9
WE82	0.84	1.35	59.7
RU83	0.90	1.35	50.1
MC83	0.54	0.84	55.7
RU84	0.54	1.17	116.3
GU84	0.53	0.90	69.1
MA84	0.93	1.44	55.2
TA85	0.30	0.70	133.6
BI85	0.65	1.16	76.9
JA86	****	****	***.*
HA87	0.74	1.15	55.2
SH87	0.86	1.25	45.7
KO187	0.38	0.79	108.6
KO287	0.44	0.87	98.8
MU88	0.43	0.83	96.4
KL188	0.28	0.94	230.3
KL288	0.40	0.80	101.6

Table 8.2
Comparison of uni-variate and bi-variate expected information values for ***Janssens et al. 1986*** *as the* ***a posteriori*** *event*

The last column of *Table 8.2* shows the gain of expected information as a percentage of the original expected information (for *Janssens et al. 1986* as a receiver only). Indeed, this value is by far the lowest for *Bernstein et al. 1981*.

The second column of *Table 8.2* is based on a comparison of the two-dimensional arrays p_{ij} with q_{ij}. In the previous chapter, we would have indicated this as I(words,sections), and within that framework we could call the information improvement (I_{ij} - I_i) the conditional information expectation of the sections given the information expectation based on the overall word-distribution. However, the simple calculation rules for relations about entropies, conditional entropies, and joint entropies, as developed in the previous article, no longer hold for the expected information values in the dynamic model.

If *i* indicates words, and *j* sections, we can express the univariate I(words) as follows:

$$\mathrm{I(words)} = \Sigma_{1}^{i} q_{i.} \log (q_{i.}/p_{i.}) \tag{8.6}$$

and the bi-variate distribution as follows:

$$\mathrm{I(w,s)} = \Sigma_{j=1}^{4} \Sigma_{1}^{i} q_{ij} \log (q_{ij}/p_{ij}) \tag{8.7}$$

To establish the relationship between the *I(words)* and *I(w,s)*, we write the latter as:

$$\mathrm{I(w,s)} = \Sigma_{1}^{i} q_{i.} \Sigma_{j=1}^{4} q_{ij}/q_{i.} [\log (q_{ij}/q_{i.})/(p_{ij}/p_{i.}) + \log(q_{i.}/p_{i.})]$$

$$= \mathrm{I(w)} + \Sigma_{1}^{i} q_{i.} \Sigma_{j=1}^{4} q_{ij}/q_{i.} \log(q_{ij}/q_{i.})/(p_{ij}/p_{i.}) \tag{8.8}$$

Hence, the information improvement is:

$$I(q_{ij}:p_{ij}) - I(q_i:p_i) = \Sigma_{1}^{i} q_{i.} \Sigma_{j=1}^{4} q_{ij}/q_{i.} \log(q_{ij}/q_{i.})/(p_{ij}/p_{i.})$$

Note that a different decomposition is possible in which we could compare the two-dimensional information expectation with the one-dimensional (*.j*) information expectation of words over sections. In general, any *n*-dimensional information expectation can be decomposed into its *n* dimensions, and into all possible combinations of them: in *n · (n - 1)* ways.

Interpretation of these multi-variate information expectations is more difficult than in the case of the decomposition of static entropy values. For example, in the above case (distributions among *words* and *sections* in *Bernstein et al. 1981* as predictors for these distributions in *Janssens et al. 1986*; first row in *Table 8.2*), we may say that the information improvement of (1.96 - 1.57 =) 0.37 bits is a weighted average of the expected information values of uni-variate word distributions over sections for the 33 words which the two texts have in common. Therefore, each of the relevant words contributes to this improvement something on the order of 10 millibits. However, these contributions can be calculated for each word or group of words more precisely, by the disaggregation of the right-hand term of formula (8.8). I will not pursue this more detailed analysis here, but instead focus on the relations among the various articles at the level of the scientific field.

In summary, the results of the dynamic and the static analysis will be quite different. The static model is useful when one wishes to evaluate the quality of the indicators. Once the latter have been chosen in one combination or another, that is, once a decision rule is specified, one can use the dynamic model to calculate the relevance of events. *The question of the validity of (static) indicators is of a different kind than the question of their quality as (dynamic) predictors about events.*

What was done here for words and sections is generalizable to all (nominal) variables. We can straightforwardly compute what co-citation analysis adds to citation analysis, and vice versa, if we want to use them for impact assessment, since impact can be defined in terms of the relation between (set of) articles as events. In the static model, we can compute the transmission between two variables within a certain domain, that is, the extent to which knowledge of the one reduces our uncertainty in the prediction of the other. In the dynamic model, we can calculate how much our ability to predict the distributions of these variables in a new event can be improved when we add the one measure to the other.

8.3 THE PROBLEM OF EMERGENCE

Given the formulas for the expected information value I ($= \Sigma$ q log(q/p)), we are confronted with a division by zero in the case of the emergence of a 'new' occurrence in the *a posteriori* text. The meaning of this in terms of information theory is that the appearance of something which was predicted with certainty *not* to occur (p = 0) comes as a total surprise, so that this message has infinite expected information value.[64]

Whether a particular word occurs in a given cell or not may be of great information value, but in empirical research it cannot be infinitely informative. The zero would in this case indicate not that we are certain that there will never be an occurrence of this word, but only that $0 \leq p < 1/n$, since by definition, one is unable to measure probabilities lower than $1/n$ in a distribution of nominal variables.

There are several solutions to this problem of a signal below the threshold of $1/n$. We might, for example, raise the value of a zero to one in this stage only.[65] Alternatively, we could say that we want the transition from 0 to 1 to be as informative as the transition from 1 to 2. In this case, one should replace the zeroes in the *a priori* distributions with 0.5. However, the arbitrariness of these solutions seems unsatisfactory. The problem has first to be solved conceptually.

In empirical research, 'emergence' is relative to a context. While 'emergence' means that we cannot compare the *a posteriori* probability of the emerged event with its *a priori* probability, we can still compare the relations between sets of events of which the emerging one is a specific case. The number of events, however, should in this context be taken not as n but as n-*1*. For example, if a word does not occur in one section of the *a priori* distribution, we should not decompose this distribution into 4 probabilities, but only into 3. In the *a posteriori* distribution we have four cells, but we can only compare three of them with *a priori* values. (Without correction our $\Sigma\, q$ would not add up to unity in the *a posteriori* case, and we would lose the additional information from the message contained in the fourth comparison.) Thus, we should not decompose the respective distributions further than into three

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[64] In the static case, by convention (*0 log 0)* is equal to zero, which is the value of the limit for $p \rightarrow 0$.

[65] See, for example: "If any cells of the matrix are vacant, unity may be inserted to replace the blank for this stage only" (Price 1981, at p. 57).

subgroups, and leave one group of two events not decomposed. In this case we can straightforwardly compute with these three probabilities.

The next question is, which cell should we keep together with the empty cell in the *a priori* distribution? If we take the cell with the lowest value in the *a priori* case with a value larger than zero, we minimize the risk that the information contained in the 'emergence' of the one cell may be compensated within the subgroup of two merged cells by a strong decrease in the other.

Using such 'partial decomposition' we can compute an additional (underestimated) expected information value for 'emergence' by subtraction: this *I* minus the *I* for the precise change in probabilities relating to the subgroup of only (three) *a priori* not-empty cells. However, this is only the additional expected information produced by taking the comparison with the empty cell into account *in the context of the distribution*. As against replacing zeros with arbitrary values between zero and one, with partial decomposition we do not intervene in the distribution, but merely refrain from calculating what cannot be calculated or interpreted meaningfully: 'emergence' only makes sense in the context of a distribution; in itself it remains a complete surprise.

Table 8.3 shows the various I_{ij} of *Janssens et al. 1986* as an *a posteriori* event in relation to the other texts of the sample, using both the method of partial decomposition proposed here, and the substitution of cell values of zero with 1.0 and 0.5, respectively. A comparison of the third with the second column shows that partial decomposition is more informative than the arbitrary introduction of unity as a threshold. In the latter case, the prediction from *Bernstein et al. 1981* even deteriorates (as a consequence of the lower total number of word occurrences *N* in the shorter *Bernstein* article by comparison with the others, and of the a relatively higher frequency of zeros in cells of not-empty distributions of the words included in the analysis). We may also conclude that in this analysis, replacement of the zeros with 0.5 in the two-dimensional array does not differ much from partial decomposition. However, the method of partial decomposition is not sample-dependent, and is therefore more general.

	I(w)	I(w,s) method used here	n = 0 replaced with n=1	n=0.5	
	(1)	(2)	(3)	(4)	(col.4-col.2)
BE81	1.59	1.96	1.57	1.86	-0.10
OL82	0.61	1.12	0.90	1.20	+0.08
WE82	0.84	1.35	1.23	1.36	+0.01
RU83	0.90	1.35	1.29	1.41	+0.06
MC83	0.54	0.84	0.77	1.07	+0.23
RU84	0.54	1.17	1.10	1.29	+0.12
GU84	0.53	0.90	0.80	0.90	0.00
MA84	0.93	1.44	1.36	1.65	+0.21
TA85	0.30	0.70	0.65	0.82	+0.12
BI85	0.65	1.16	1.02	1.17	+0.01
JA86	****	****	****	****	**.**
HA87	0.74	1.15	0.98	1.16	+0.01
SH87	0.86	1.25	1.16	1.31	+0.06
KO187	0.38	0.79	0.73	0.92	+0.16
KO287	0.44	0.87	0.73	0.89	+0.02
MU88	0.43	0.83	0.76	0.89	+0.06
KL188	0.28	0.94	0.82	0.99	+0.05
KL288	0.40	0.80	0.64	0.86	+0.06

Table 8.3
Comparison of the effects of 'partial decomposition' and the replacement of zeros with values of 1.0 and 0.5

8.4 PUBLICATIONS AS EVENTS IN THE FIELD OF *DICTYOSTELIUM DISCOIDEUM*

Between each pair of articles (18 x 17) we can calculate uni-variate expected information values if we disregard the section structure, and bi-variate expected information values if we take the section structure into consideration. The latter can again be decomposed in terms of the four sections, and one can use the method of 'partial decomposition' to calculate expected information values for the 'emergence' of words in certain sections. However, these various values are based on different numbers of shared words for each of the instances, and are therefore incomparable.

How can we normalize? There are two obvious options:

1. Normalization can be achieved in terms of the events which are taken to be *a posteriori*, that is, to compare how much certain (groups of) words, e.g., those of the introduction section, contribute relative to the expected information value of the message associated with the *a posteriori* event.

2. When comparing among transitions (across articles), one should look at the amount of expected information per shared word: if more words are used in common, the opportunity to measure change in terms of word occurrences increases.

The first is a normalization in terms of one and the same *a posteriori* event, while the second makes it possible to compare expected information values among events.

8.4.1 Normalization in terms of an *a posteriori* event

In *Figure 8.1* word occurrences in the four sections of *Janssens et al. 1986* are normalized as percentages of contribution to the overall expected information of this article as an *a posteriori* event, in relation to various other articles and to the average of the set (which has above been considered as an operationalization of the field).

The graph shows a general pattern: to the extent that these texts have words in common, the distributions of word occurrences are particularly similar in the introduction and the methods sections. (However, as is already known from the static model in the previous chapter, the field dependency is different for word occurrences in these two sections.) In the dimensions of the other two sections as well, the various articles make rather similar predictions about word distributions with respect to *Janssens et al. 1986.* Note that the results section is always more informative than the discussion in this respect.

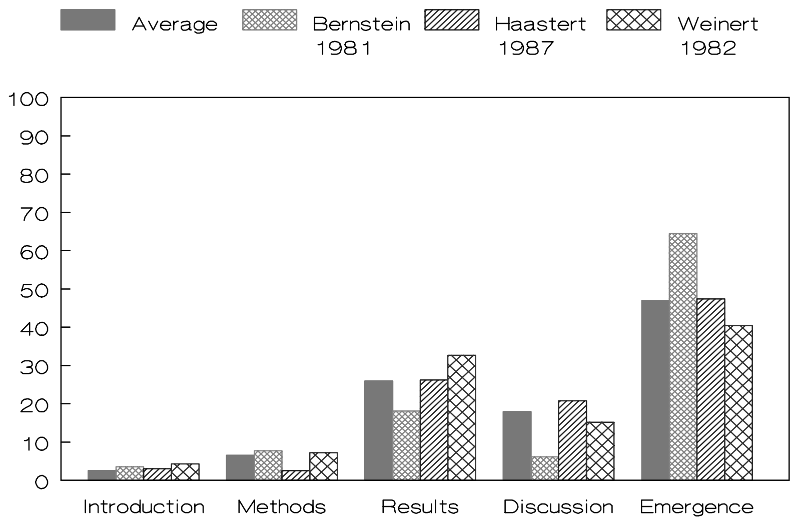

Figure 8.1
*Percentage contribution of expected information values by word occurrences and by 'emergence' to the bi-variate expected information of **Janssens et al. 1986** as the **a posteriori** event*

In addition to the average, *Haastert 1987* is included in the graph as an *a priori event*, since this text was noted above as a deviation from the average in the methods section (see Chapter Five); *Weinert et al. 1982* as an example of a text described by our informant as dealing with a somewhat different subject (so that we would expect a larger information value in the dimension of the results, since this section usually focuses on subject matter); and *Bernstein et al. 1981*, which was identified above as a shorter article. The deviant pattern of the latter as a sender in the category 'emergence' illustrates the points made in the previous sections about zeros in cells: *Janssens et al. 1986* used the words it shared with *Bernstein et al. 1981* in sections other than those in which they were used exclusively in the more concise text of the latter. In all other respects, these two texts were shown above to be more similar in terms of word usage in the sections than any other two texts in the sample.

8.4.2 Normalization across texts

In an expert system, and in science policy applications, we are usually interested in how a new event can be related to various (aggregates of) archived texts. The interesting normalization is therefore not primarily in terms of contributions to the new event, but in terms of the relevance of the new event for existing relations among previous events. How does the new text link up to the literature, and what is its impact on the network of previously existing relations?

	JANSSENS et al. 1986			HAASTERT 1987		
	I_{ij} in bits	nr of words	I/word (mbits)	I_{ij} in bits	nr of words	(mbits)
BE81	1.9624	33	59.5	1.7009	27	63.0
OL82	1.1175	40	27.9	1.5715	29	54.2
WE82	1.3456	49	27.5	1.3325	48	27.8
RU83	1.3516	55	24.6	1.5196	47	32.3
MC83	0.8369	32	26.2	1.2841	23	55.8
RU84	1.1711	44	26.6	0.9749	35	27.9
GU84	0.9005	67	13.4	0.9438	52	18.1
MA84	1.4403	42	34.3	1.3783	34	40.5
TA85	0.7042	29	24.3	1.0394	19	54.7
BI85	1.1565	58	19.9	1.3797	38	36.3
JA86	******	**	****	0.9203	60	15.3
HA87	1.1455	60	19.1	******	**	****
SH87	1.2485	51	24.5	1.7309	34	50.9
KO187	0.7918	56	14.1	1.0519	44	23.9
KO287	0.8740	50	17.5	1.1164	40	27.9
MU88	0.8349	40	20.9	1.2167	32	38.0
KL188	0.9400	22	42.7	0.7610	25	30.4
KL288	0.7962	27	29.5	0.9760	21	46.5

Table 8.4
Expected information values, number of shared words and information/word for ***Janssens et al. 1986*** *and* ***Haastert 1987****, respectively*

Again, we use only word occurrences as the nominal variables, and we define the best predictor of a text as that text which has the most similar word distribution. As noted, normalization per shared word is necessary if we want to compare among more than two texts, since the mere fact that two texts share more words would lead to a higher chance to show differences in relative frequencies.[66] *Table 8.4* shows the information expectation values for the cases of *Janssens et al. 1986* and *Haastert 1987* taken as events, and compares these events in terms of word occurrences with all the other texts. The values are expressed in terms of millibits/shared word.

The figures in this table become interesting if we compare predictions among each other, and they raise the question of whether some of the texts can be seen as revisions of other predictions of *Janssens et al. 1986*. Do some revisions boost the signal of the originally sending text? In this case, to pursue the metaphor, the receiver would no longer have to listen to the original sender, since that signal would be overpowered by the auxiliary station. If we try to reconstruct how the development in the communication took place, the arrow from the original transmitter should be replaced by arrows between original transmitter, auxiliary transmitter, and receiver, since the 'distance' associated with the original arrow is greater than the sum of the 'distances' between the three points.

In terms of expected information values, we have to evaluate an inequality among three texts (or aggregates of texts): is the normalized revision of the prediction plus the normalized prediction of the revision (by the originally *a priori* text) larger or smaller than the normalized prediction from the direct relation between the *a posteriori* and the *a priori* text (*Figure 8.2*)?

[66] This normalization is sensitive, particularly with low frequencies, to small deviances; and therefore we should carefully examine what we are doing when we deal with small values, for example, as a consequence of disaggregation. These problems are a consequence of the magnitude of errors when using averages in small samples only.

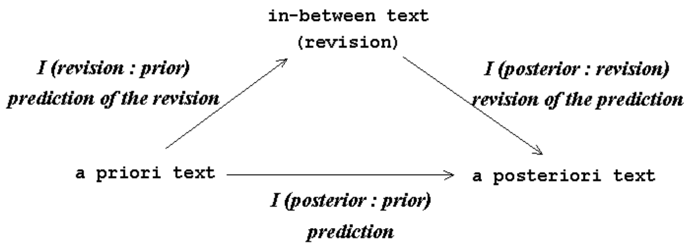

Figure 8.2
Prediction and possible revision of prediction

In the form of an equation, one has to ask whether:

$$I(posterior:revision)/word + I(revision:prior)/word < I(posterior:prior)/word$$

In contrast to a normal revision, a revision will be defined as a *critical revision* if the prediction is not only improved by the revision, but the pathway through the revision is more efficient for average information transmission than the direct path. In other words, the pathway is *critical* in terms of the specified variables.[67]

Figure 8.3 provides a graphic representation of all critical revisions in the case of *Janssens et al. 1986* as the posterior event. *Figure 8.4* does the same for *Haastert 1987* as the posterior event in relation to the 17 other texts. Remember in reading these graphs that each point is connected directly to the *a posteriori* event by a prediction, and that we are here focusing only on 'triangulations.' The absence of a line means that the prior text is directly received as a sender, while one could extend the metaphor to say that the critically revised texts are like senders overpowered by the revisions. Bold arrows represent the most important critical

[67] In the sociology of translation (Callon *et al.* 1986), one speaks also of 'obligatory passage points.'

revisions, fine arrows those of secondary importance, and the dotted arrows minor ones.

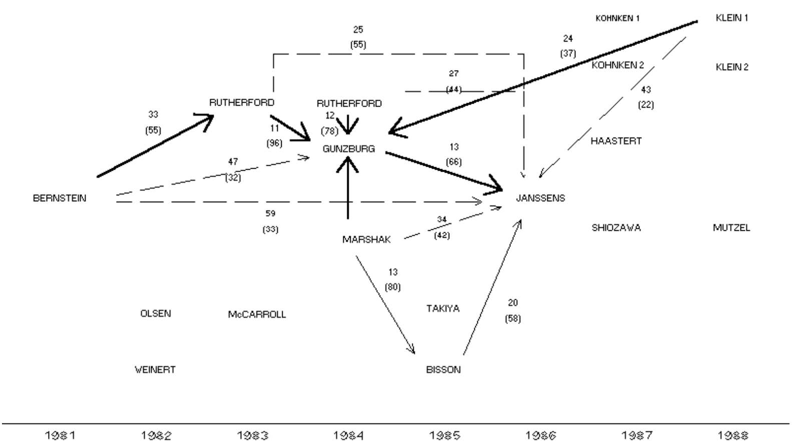

Figure 8.3
Pathways of critical revisions of predictions of word occurrences in ***Janssens et al. 1986*** *as an* ***a posteriori*** *event*

The picture differs remarkably in the two cases, given the fact that the two articles are co-authored (by Van Haastert), and appeared in the same journal only one year apart. (In *Figure 8.3*, I pencilled in the values of both the expected information value per word in millibits and the number of shared words between brackets for both the revisions and the original values in order to enable the reader to follow the developments in more detail.) In the former case (*Janssens 1986*) the *Gunzburg 1984* text was the best predictor. It is particularly interesting that there was a gradual improvement of the prediction by *Bernstein 1981* through *Rutherford 1983* and *Gunzburg 1984*. The figure suggests, therefore, the existence of a pathway.[68]

[68] Other texts were also ‘channeled’ through *Gunzburg 1984*, including the first text by *Klein 1988*, which is historically much later, but obviously more related to *Janssens 1986*–in this case more as a ‘sideline’ through

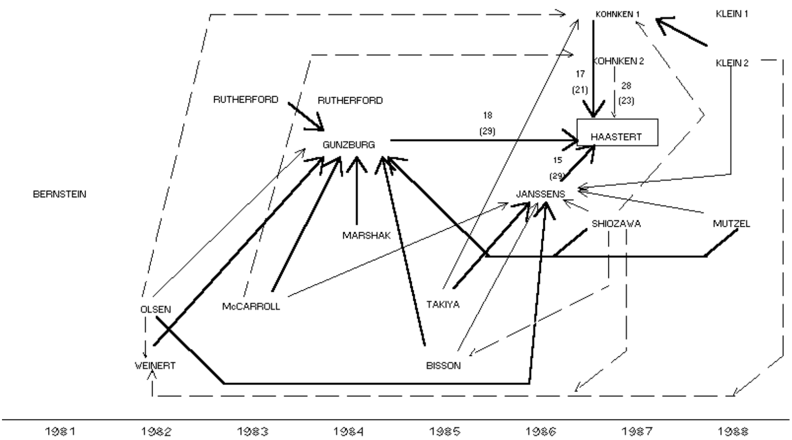

Figure 8.4
Pathways of critical revisions of predictions of word occurrences in ***Haastert 1987*** *as an* ***a posteriori*** *event*

The picture is quite different if *Haastert 1987* is taken as the *a posteriori* event (*Figure 8.4*). *Haastert 1987* is best predicted by three texts, which in many cases improve the predictions of other texts to such an extent that as transmitters these texts disappear behind the former three signals. The picture is one of a core surrounding the new event, and much less that of a pathway. Of the three sources, the co-authored text of *Janssens 1986* is obviously the best predictor, followed by *Gunzburg 1984*, which again has more relations to other texts than *Janssens 1986*; additionally, the articles by *Kohnken et al. 1987* seem of importance to the vocabulary in *Haastert 1987.*

Note that in contrast to the previous picture, there are in this case no arrows from *Gunzburg 1984* to *Janssens 1986*, nor from *Bernstein 1981* to either of these. This reminds us that the view of

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Gunzburg 1984 than directly.

the field is dynamic, i.e., related to the event as the receiver of signals. While for *Janssens 1986* the signal from *Bernstein 1981* was obscured to such an extent that it was replaced by *Gunzburg 1984*, this is not the case for *Haastert 1987*. Although the word occurrences in *Haastert 1987* are best predicted by those in *Janssens 1986* and *Gunzburg 1984*, *Haastert 1987* was still receiving a direct–i.e., not critically revised–signal from *Bernstein 1981* on his 'vocabulizer.' However, it was the weakest signal in the field, as we can see from the value for *I*/word, which reaches a maximum of 63 mbits/word for this case in the right-hand column of *Table 8.4*.

8.4.3 Critical revisions in the sectional dimensions

The next step is to differentiate over the four sections, as an example of extension to more than one dimension. *Figures 8.5 to 8.8* show the results of this computation for *Janssens et al. 1986* as an event. Since many more arrows now emerge as important improvements of the predictions, I limited the presentation to only the two best improvements (bold and fine arrows, respectively). In order to be cautious given the small size of the sample (see footnote 6 above), only a dashed arrow is drawn when fewer than 10 shared words were involved.

Figure 8.5 shows the critical revisions among the introductory sections in relation to the introduction section of *Janssens et al. 1986* as the *a posteriori* event. We see that in this case some of the historically more proximate articles function as the best revisions. In our opinion, this can be taken to mean that *Janssens et al. 1986* follows the trend which we know more generally from the static model, namely that the field is most actively deconstructed and reconstructed in the introductory section, or alternatively, one might say that the author in this section particularly links his contribution with the current research front.

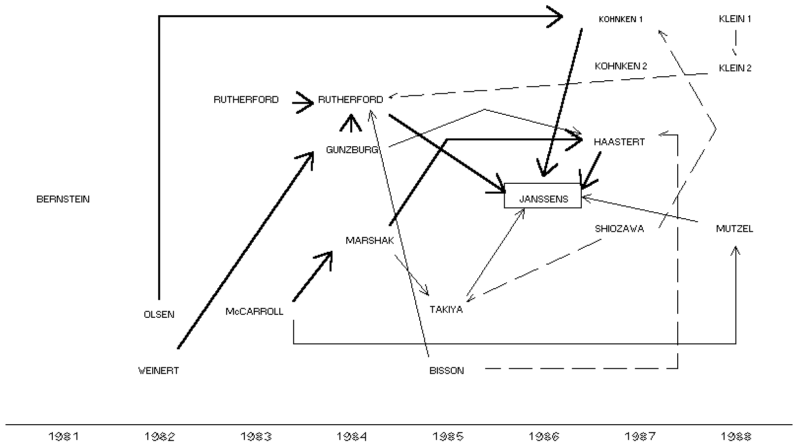

Figure 8.5
Pathways of critical revisions of predictions of word occurrences in the ***introduction*** *section of* ***Janssens et al 1986*** *as an* ***a posteriori*** *event*

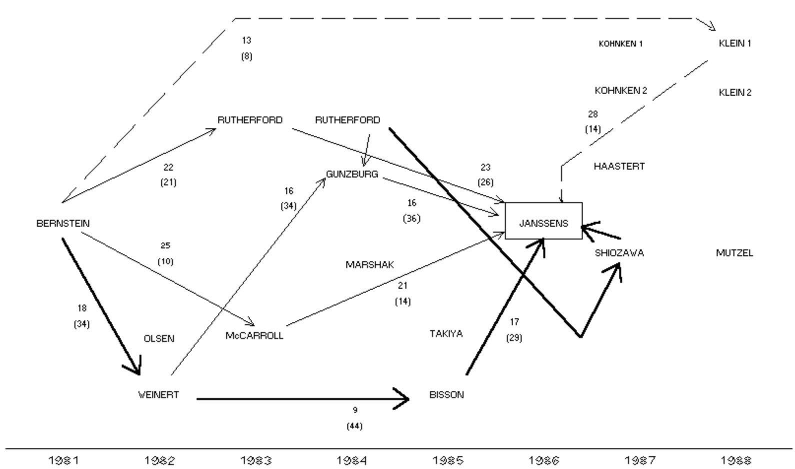

Figure 8.6
Pathways of critical revisions of predictions of word occurrences in the ***methods*** *section of* ***Janssens et al. 1986*** *as an* ***a posteriori*** *event*

Figure 8.6 shows a very different pattern. Of the 19 critical revisions of predictions of the methods section of *Janssens 1986*, 12 are critical in relation to the methods section of *Bernstein 1981* as the *a priori* case. The pattern is one of flow through the system. In our opinion, the interpretation of this should be that particularly along this dimension, *Janssens et al. 1986* has changed ('learned'?) since *Bernstein et al. 1981*: new methods, concepts about methods, or words in use for methods have been proposed by some of the other authors, and the authors of the *a posteriori* event have taken these changes in the field thoroughly into account. This confirms the earlier impression of codification taking place more visibly in the method sections.

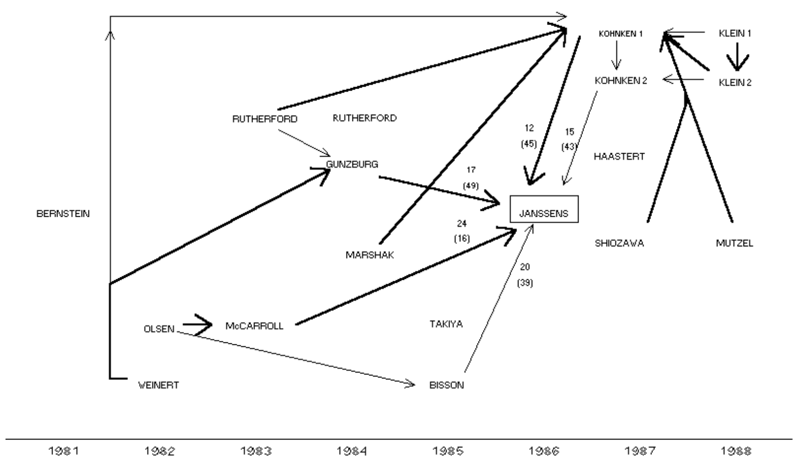

Figure 8.7
Pathways of critical revisions of predictions of word occurrences in the ***results*** *section of* ***Janssens et al. 1986*** *as an* ***a posteriori*** *event*

The picture for the results section (*Figure 8.7*) reveals the relation with the *Kohnken et al. 1987* group, which we noted already when discussing *Haastert 1987* as an event. However, this relation was not mentioned in the interviews with our informant.[69] Finally, in the discussion sections, we find fewer critical revisions (*Figure 8.8*). In our opinion, this indicates that in this more theoretical dimension *Janssens et al. 1986* argues more directly with the other positions possible in the field. The more remote position of *Rutherford 1984* in comparison to *Rutherford 1983* and *Gunzburg 1984* was also noticeable in some of the other pictures. The relation with the *Olsen/McCarroll*-group and with the *Klein 1988*-group also seem more 'distanced' now, since critically revised. (Remember that a revision has a better chance to be a critical revision if the original text is further 'distanced' as a sender from the receiver, that is, if the expected information value of the message necessary for the transmission is greater.)

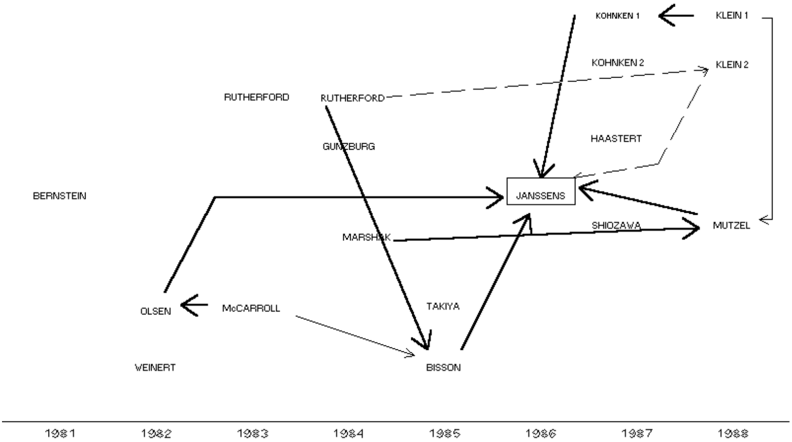

Figure 8.8
Pathways of critical revisions of predictions of word occurrences in the ***discussion*** *section of* ***Janssens et al. 1986*** *as an* ***a posteriori*** *event*

[69] Since in co-authored articles it is common practice that each author is responsible for particular parts, it may well be that this is the part which was brought in by *Van Haastert* as one of the co-authors of *Janssens et al. 1986*, and not by *Van Driel*.

Note that the overall pattern in *Figure 8.3* is not congruent with any of the patterns at the sectional level in *Figures 8.5 to 8.8*. This result from the dynamic model confirms our conclusion from previous chapters that the sections are structural dimensions of the articles. Furthermore, the differences between the pictures emphasize once again the point that different nominal variables may lead to completely different outcomes. Hence, different indicators are expected to lead to different assessments of events: as noted above, the extent to which indicators 'partially converge' is a problem to be dealt with in the static model. The use of combinations of indicators enriches the dynamic picture, since with higher dimensionality the information improvement increases.

8.5 TOWARDS THE GENERATION OF EXPERT SYSTEMS FROM SCIENTIFIC TEXTS

If, for example, we consider a journal as a set of existing texts which contain probabilistic information with predictive value about those texts which will be included in it, submitted to it, or (alternatively) rejected by its editor(s), we can conceive of this information as a set of *a priori* probabilities about a new text, which upon its arrival becomes an event with *a posteriori* probabilities. However, to express the information expectation, we have to specify those dimensions (and levels of aggregation) which we choose to regard as important. For example, should the expected information content of the new article be operationalized in terms of shared words with the already published articles, or citations, or co-citations?

By applying the static methods described in the previous chapter, one can calculate the relations among the variables within the archive in terms of mutual information, i.e., assess their quality as indicators in this domain for whatever one wishes to indicate. Once the variables have been chosen, one may calculate the expected information values associated with the occurrence of new events in relation to each of the cases and/or in relation to the aggregate, in each dimension using the methods specified in this chapter. Because of the additivity of the information concept, one can compute various indicators by simple addition and subtraction of the various expected information values or also compute their interaction in terms of bi- or multi-variate equations. The analysis can be extended to nominal variables which are attributed to the texts but not contained in them. If, for example, the results of the refereeing process are expressed in terms of scores on a scale, one may wish to specify this measurement as a relevant dimension.

Furthermore, the specification of variables and their relations is equivalent to the specification of decision rules in relation to knowledge bases in artificial intelligence. Decision rules can be specified as functions of the nominal variables; for example, by weighing comparisons of co-occurrences of shared words as decision criterion 1 (d_1) and co-occurrences of citations as decision criterion 2 (d_2), one can specify the following decision rule:

$$D = a * d_1 + b * d_2 + c * d_1 * d_2$$

In this formula *a, b* and *c* function as weighing parameters, which remain to be chosen.

Choices with respect to variables and their relations determine the outcomes, and therefore at this point appreciative theorizing has to step in. In principle, a computer system storing this data could act as an expert system to provide the expected information values (both in bits and in percentages of the reduction of the uncertainty of a prediction) in each of the specified dimensions, and trace significant differences quantitatively back to earlier work in the archive, also decomposed into relevant dimensions. If one were then to compare the results of these assessments with independent expert opinions or with independent statistical information about actual occurrences, the choice of the weighing parameters for the various dimensions could be adjusted dynamically, and the system could begin to learn.

Note that the flexibility of the model also allows for refinement of the database with hindsight, i.e., for the further specification and attribution of variables in subcategories, without distorting the previously stored information. The later application of different decision rules to the system does not change the earlier data. The decision rules are user-oriented: either pragmatical for policy purposes, or sophisticated for theoretical purposes.

However user-dependent the decision rules may be, this dependency does not necessarily apply to the substantive knowledge base. The major difference in this reconstruction from other designs for expert systems is that the knowledge engineering was already accomplished by the scientists themselves, when they published their articles. This feature constitutes a major advantage over expert systems about the sciences grounded in knowledge that was extracted by interviewing or other forms of social reconstruction.

Chapter 9

The Static and Dynamic Analysis of Network Data

In this chapter, the results of the static and dynamic analysis in the previous chapters will be generalized. I shall show that one can use measures derived from information theory–also known as statistical decomposition analysis (Theil 1972)–as *one* conceptual framework in order to study the most common problems of multivariate analysis, both in a static and in a dynamic mode.

In addition to the integration of results from these two perspectives, statistical decomposition analysis allows for a precise study of the effects of aggregation and disaggregation. Despite the fact that the methodology requires the variables to be measured only at the nominal scale, it preserves any additional information contained in more refined measurement (Krippendorff 1986).

Since the various models are derived within one framework, the results of the multi-variate analysis and the time-series analysis can be made relevant for one another. Additionally, using the static model, one can create an exact dendogram, and determine the precise number of clusters. The algorithm is generalizable to clique analysis.

Using the dynamic model, developments can be revealed which were not suggested by the comparison of results of various forms of multi-variate analysis for each year separately. The question of using these methods in a research design about structure/action relations will be discussed.

As data, I used the matrix of aggregated citations among thirteen major chemistry journals as a typical set of network data (see *Table 9.1.*) [70] Aggregated journal-journal citations are often considered as a high-level sociometric structure in scientometric studies in order to create 'maps of science' (Price 1965). This type of matrices has been thoroughly analyzed in recent decades using various forms of multi-variate analysis (Carpenter and Narin 1973; Narin 1976, at pp. 185-90; Doreian and Fararo 1985; Leydesdorff 1986 and 1987; Doreian 1986; Tijssen *et al.* 1987; Tijssen 1992).

Journal title	*Variable name*	*Sequence number*
Chemical Physics	ChemPhys	1
Chemical Physics Letters	ChemPhLt	2
Inorganic Chemistry	InorgCh	3
J. of the American Chemical Society	JACS	4
J. of Chemical Physics	JChemPh	5
J. of the Chemical Society- Dalton T	JChemSc	6
J. of Organic Chemistry	JOrgChem	7
J. of Organometallic Chemistry	JOrgMetC	8
J. of Physical Chemistry	JPhChUS	9
Molecular Physics	MolPhys	10
Physical Review A	PhysRevA	11
Tetrahedron	Tetrahe	12
Tetrahedron Letters	Trahelt	13

Table 9.1
Journals used for the construction of a journal-journal citation network

[70] The 13 journals comprise a set of journals heavily linked to the *Journal of the American Chemical Society (JACS)* and the *Journal of Chemical Physics* as the two central chemistry journals in the *JCR*-journal set for 1984. The list was generated in the context of another research project (Cozzens and Leydesdorff 1993; Leydesdorff and Cozzens 1993).

	1	2	3	4	5	6	7	8	9	10	11	12	13 (citing)
1	984	724	51	189	1136	-	-	-	459	142	74	-	-
2	963	2387	206	810	2816	31	40	-	1660	331	250	53	-
3	35	157	5480	1912	138	1242	111	1228	319	14	-	28	29
4	344	1102	4873	15521	1185	1214	6952	2448	3240	126	-	3045	3694
5	2732	4622	715	2240	15069	166	157	163	5199	1575	1134	117	30
6	-	-	946	452	-	1443	28	830	52	-	-	-	26
7	-	29	157	2264	-	62	5024	484	74	-	-	1617	2259
8	-	32	713	958	-	641	307	3765	-	-	-	106	211
9	257	845	511	1208	1538	87	191	45	4315	122	41	51	56
10	330	455	84	220	1195	-	-	-	395	1082	113	26	-
11	162	327	-	-	1115	-	-	-	170	183	3977	-	-
12	13	29	49	831	-	24	891	131	49	-	-	806	724
13	-	32	84	1918	-	37	2802	548	61	-	-	1819	3385
(cited)													

Table 9.2
Original datamatrix for 1984

The data is readily available from the *Journal Citation Reports* of the *Science Citation Index.*[71] The matrix of aggregated journal-journal citations among these 13 journals for 1984 is given in *Table 9.2*. In the static analysis, I focus on this data only, and then include the corresponding data for other years in order to develop the dynamic model.

9.1 "CITING" AND "CITED" AS VARIABLES IN A STATIC DESIGN

By using the static measures *H*, expected information contents for citing and cited patterns can be computed for the journal-journal citation matrix, and the results can be evaluated in terms of their mutual information. (The subsequent decomposition of the matrix in terms of its constituents will be discussed in a later section.) For the 1984 matrix, one finds the following values:

$$
\begin{aligned}
H(\text{citing,cited}) &= 5.667 \text{ bits} \\
H(\text{citing}) &= 3.457 \\
H(\text{cited}) &= 3.173
\end{aligned}
$$

and therefore:

$$
\begin{aligned}
H(\text{citing}|\text{cited}) &= 2.493 \\
H(\text{cited}|\text{citing}) &= 2.209
\end{aligned}
$$

$$
T(\text{citing,cited}) = 0.964
$$

[71] In the printed edition, the tails of the citation distributions are summed under 'all others.' As a rule of thumb, values equal to or lower than five are omitted (Garfield 1972.) In addition to these thresholds, there are statistical and systematic errors caused by the lack of standardization in the spelling of journal names (Rice *et al.* 1989). However, as these authors note, for the type of journals which we will use here (with large numbers of total citations included in the *JCR*), these effects are insignificant.

$$\frac{H(\text{citing}) - H(\text{citing}|\text{cited})}{H(\text{citing})} = 27.9\ \%$$

$$\frac{H(\text{cited}) - H(\text{cited}|\text{citing})}{H(\text{cited})} = 30.4\ \%$$

This means that the citing pattern is a 10% better predictor of the cited pattern than vice versa (cf. Leydesdorff 1993c; see also Chapter Twelve). The mutual information between 'cited' and 'citing' is only of the order of a 30% mutual reduction of the uncertainty in the prediction. One might have expected the two patterns to determine one another to a much larger degree, but there are obvious groupings in the data. Each subset contains high mutual information within it, but there is hardly any interaction between the groups.

This result therefore raises the question of whether we can also infer the grouping of the journals using statistical decomposition analysis. Of course, this brings back the major questions that have led to the development of various forms of multivariate (e.g., cluster) analysis. First, what will we regard as more or less similar, i.e., what will be the similarity criterion? Second, are we to use agglomerative clustering techniques or divisive ones? Should we focus on diadic relations between cases which exhibit strong graphs or on patterns of relations?

9.2 CLUSTER ANALYSIS

9.2.1 Agglomerative clustering

A simple agglomerative clustering algorithm would merge as a cluster those two distributions (rows, columns, etc.) which are most identical, that is, mutually have the lowest I associated with the message which converts the one distribution into the other, and subsequently to iterate the procedure. In each step this algorithm seeks the strongest graphs.[72]

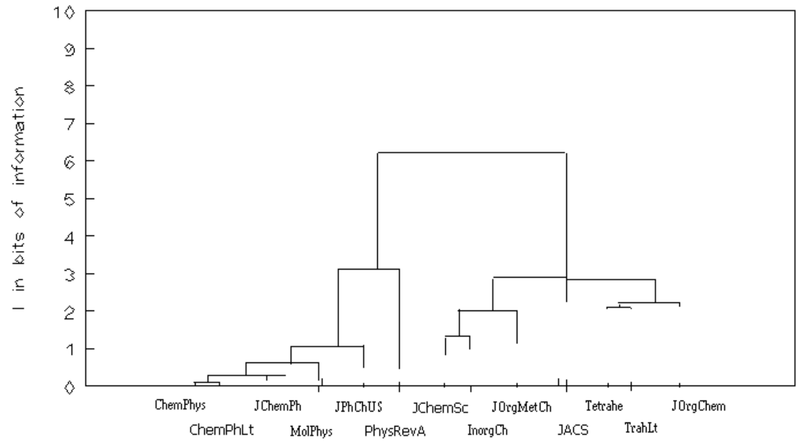

Figure 9.1
Agglomerative clustering of the citing patterns of 13 chemistry journals in 1984

Figure 9.1 shows the results, in the form of a dendogram, for the citing patterns of these 13 chemistry journals in 1984. Since the initial step does not imply the construction of a symmetrical (dis)similarity matrix, the leaves of the dendogram do not have to be equal. The vertical distances meaningfully represent the expected information values of the clustering in each case.

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72 However, I_{ij} will in general be unequal to I_{ji}, and therefore, it may occur (as actually happens here with *JACS* in relation to the 'organic' and 'inorganic chemistry' clusters) that despite the fact that two cases form a strong graph (in the sense of mutually having the lowest I associated with transformation into each other), a third case can combine with one of the two values with an in-between value for I. If that happens, we merge this third case in the same step of the clustering procedure.

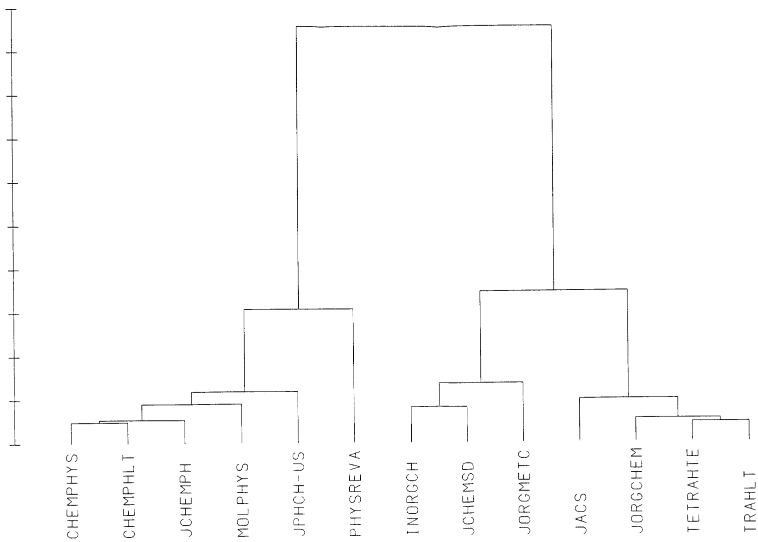

Figure 9.2
Cluster analysis of citation patterns for 13 chemistry journals; Ward's mode of analysis, Pearson correlation coefficients

If we compare the results with, for example, a dendogram in *Figure 9.2* which is based on using Ward's mode of analysis for clustering the Pearson correlation as a similarity measure,[73] we notice that the qualitative order is the same for the 'physical chemistry' cluster. However, the complexity of the position of *JACS* is visible with extreme precision in *Figure 9.1*, while it is not in *Figure 9.2*. In the former picture, *JACS* is only marginally more linked to the 'organic chemistry' cluster than to the 'inorganic chemistry' group.

[73] Cluster analysis is well-known for its proliferation of options, caused by the possibilities of choosing among similarity criteria and clustering algorithms. The results can be very different, accordingly. I usually found the best graphic representation of factor analytic results by using this combination of Ward's mode of analysis with a Pearson correlation matrix, although for formal reasons this combination is not allowed (cf. Leydesdorff and Zaal 1988).

9.2.2 Divisive clustering

It is *impossible* to decide on the basis of the agglomerative procedure how many groups should be distinguished, since the agglomerative steps are formally equivalent. However, one would like to know whether *JACS* is to be counted as part of the 'organic chemistry' group or whether it should rather be handled as a separate case. Were we to choose the latter option, then what about *Physical Review A*, since this journal bifurcates from the 'physical chemistry' cluster at distance even larger (in terms of bits) than the distance of *JACS* from the 'organic chemistry' cluster core? Could we not find a more strict criterion for division into groups using the rules of statistical decomposition for divisive clustering into groups directly?

The problem of how to choose the number of clusters, factors, groups, dimensions, etc. is a pervasive one in multivariate analysis. If there are no *a priori* theoretical reasons–as is usually the case in exploratory uses of the techniques–such decisions tend to remain somewhat arbitrary. In factor analysis, methods such as visual inspection of the scree plot or a cut-off at certain eigenvalues are common practice. In cluster analysis and multi-dimensional scaling, decisions based upon visual inspection of the results are common.[74]

Statistical decomposition analysis offers a straightforward answer to this problem, since disaggregation is highly formalizable. As noted in Chapter Seven, disaggregation of a set in *g* groups can be described with the following formula:

$$H = H_0 + \Sigma_g P_g H_g$$

in which *H* is the expected information content (probabilistic entropy) of the aggregated distribution, and P_g the probability of each of the groups which as a subset has an uncertainty equal to the

[74] In bibliometrics, Small and Sweeney (1985) have proposed 'variable level clustering,' that is, in essence the adaptation of the clustering level to the density of the cluster involved; the search for a formal criterion is thus replaced by a procedural one.

respective H_g's. The 'in between group entropy' H_0 is a measure of the specificity that prevails at the level of the subsets, and thus it should be possible to use it as a measure for the quality of clustering.

The right-hand term of the above equation is equal to the entropy of a variable (*n*) under the condition of a grouping variable (*m*): $H(n|m)$. The left-hand term, H_0, is therefore, equal to $H(n) - H(n|m)$, which is the uncertainty in *n* that is *not* attributable to the uncertainty within the groups, or in other words the *transmission* (*mutual information*) of the grouping variable *m* to *n*. The larger this transmission, the more reduction of uncertainty there will be among the groups, and therefore the better the groups will be in terms of the homogeneity of their distributions. However, by definition:

$$H(n|m) = H(n,m) - H(m)$$

and therefore:

$$H_0 = H(n) + H(m) - H(n,m)$$

This means that the increase of H_0 if we add another group (cluster, factor, etc.) is composed of a part that is dependent only on the grouping variable ($H(m)$), and a part which is dependent on the interaction between the variables *n* and *m* ($H(n,m)$). The question thus becomes: for which value of *m* does the function $\{H(m) - H(n,m)\}$, and consequently H_0, reach a maximum? Although this problem may be addressed using differential calculus ($d\{H(m) - H(n,m)\}/ dm = 0$), let me address the problem here with a more intuitive approach.

If we divide one group into two subgroups *i* and *j*, using $H_{ij} = H_0 + P_i H_i + P_j H_j$, the aggregated H_{ij} may be larger than both H_i and H_j, or larger than one of them and smaller than the other. (The two groups cannot be both larger than H_{ij}, since the 'in between group' H_0 is necessarily larger than or equal to zero.) The case of $H_i < H_{ij} < H_j$ corresponds to the removal of the more than average heterogeneous case(s) into a separate subgroup: therefore, this new subgroup has a higher uncertainty, and the remaining subgroup becomes more homogeneous than the original group. This is always possible, but it is not yet clustering, which entails the notion of reducing uncertainty in *both* subgroups. Therefore, we may define 'divisive clustering' as the case where both new subgroups have a lower expected information content than the undivided group.

Note that the above justification of the division is based on the right-hand term of the formula for disaggregation only ($\Sigma_g P_g H_g$). The value of the left-hand term (H_0) is sensitive both to the number of groups–since each further division adds to H_0 unless the two groups have similar H_gs–and to the quality of the attribution of cases to groups given a certain number of groups. However, the two questions–(1) concerning the number of groups, and (2) concerning the attribution of cases to groups–can be studied independently, given the two terms in the equation noted above.

The possible number of attributions of n cases to m groups ($m < n$) increases so rapidly with the number of cases and the number of groups that systematic comparison of all possible combinations can imply heavy computation. In practice, this type of repetitive approach to the data, which is characteristic of information theory (Krippendorff 1986), can be programmed in DO WHILE-loops.

First, we investigate whether the setting apart of any of the cases leads to two subgroups, both of which have lower H_gs than the overall H. If so, we begin with the one which leads to the highest H_0, and systematically evaluate whether the addition of other cases to this one subgroup leads to a further increase of H_0, etc. Once we have investigated all the possibilities and decided upon the best division into two subgroups, the analysis can be repeated for the two subgroups respectively.

After normalization of H_0 in terms of the grand sum of the matrix, a dendogram can be constructed, which is exact both in terms of the vertical distances between the nodes and in terms of where to draw the line above which further division leads to subgroups that are not both lower in their entropy than their respective aggregates. This level in the graph corresponds to a maximum for H_0.

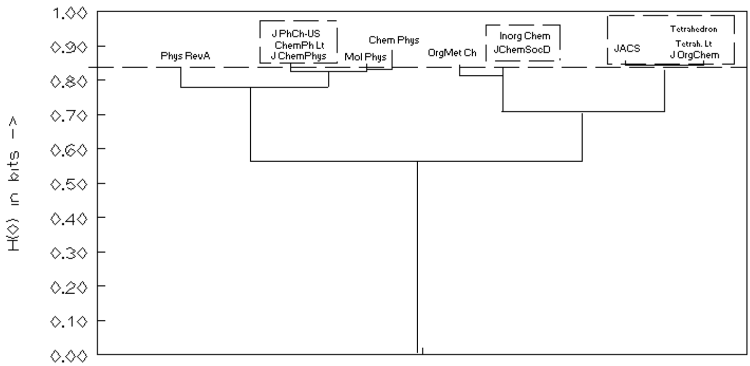

Figure 9.3
Divisive clustering of CITING patterns of 13 chemistry journals

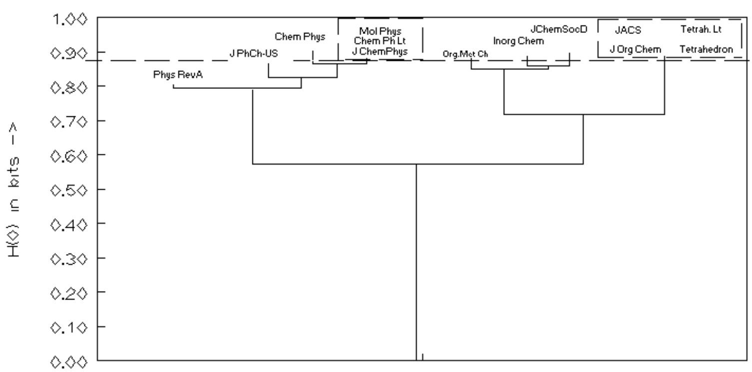

Figure 9.4
Divisive clustering of CITED Patterns of 13 chemistry journals

Figures 9.3 and 9.4 show the dendograms for citing and cited patterns, respectively. The dotted line represents the level above which further division becomes counterproductive (i.e., would lead to a decrease of total H_0). From these figures, we may conclude that there is a relevant subdivision of the cluster which we considered as the one 'chemical physics' cluster above; however, this subdivision is somewhat different as 'cited' and 'citing.' Both 'cited' and 'citing,' the *J. of Organometallic Chemistry* has to be considered as an isolate, and 'cited' it seems that the whole cluster of 'inorganic chemistry' falls apart. However, along both axes, *JACS* firmly belongs now to the 'organic chemistry' cluster. (As was noted above, the major limitation of divisive clustering is that each case has to be attributed to one group only, and that details about inter-group positions, as shown by using agglomerative clustering and factor analysis, are not revealed.)

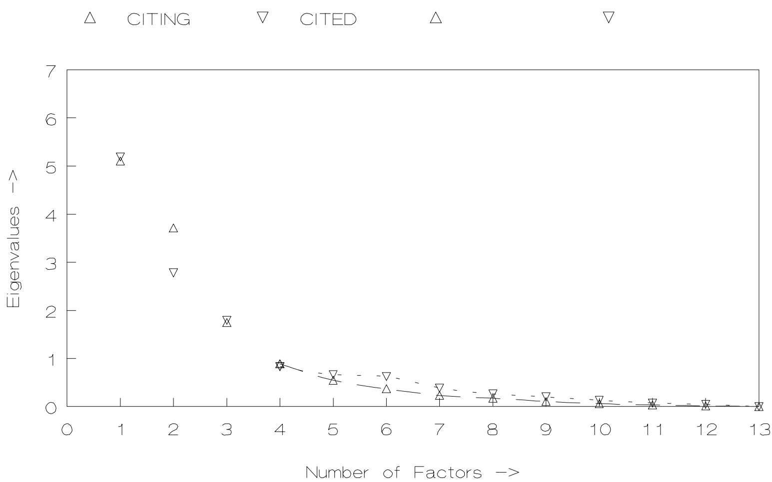

Figure 9.5
Screeplots eigenvalues citation pattern (citing and cited)

In summary, we may conclude that in terms of dividedness, the exact clustering of these two patterns leads us to distuinguish seven and eight subgroups, respectively. The 'cited' pattern is more divided than the 'citing' pattern. It is interesting to look back at the scree plots of the respective factor analyses along these two dimensions (*Figure 9.5*): with hindsight, we see more components in the cited patterns than in the citing patterns, but these differences are within the 'scree' of the graph, and would therefore be considered as irrelevant for the factor analysis.[75] In factor analysis, however, we aim primarily at the reduction of complexity in terms of the number of *relevant* factors, while in divisive cluster analysis we study the dividedness as such.

Factor	Eigenvalue	Pct of Var	Cum Pct
1	5.10338	39.3	39.3
2	3.71624	28.6	67.8
3	1.74990	13.5	81.3
4	.88657	6.8	88.1

Varimax converged in 5 iterations
Rotated Factor Matrix:

	FACTOR 1	FACTOR 2	FACTOR 3	FACTOR 4
CHEMPHLT	.97034	-.04210	-.05748	.04397
JCHEMPH	.95862	-.09472	-.07715	.10414
CHEMPHYS	.94609	-.12497	-.13481	.02694
JPHCHUS	.84504	.16857	.11524	-.02427
MOLPHYS	.83678	-.17293	-.19056	.05371
TETRAHE	-.08978	.97921	.12886	-.07722
JORGCHEM	-.08007	.95670	.16671	-.06099
TRAHLT	-.15185	.95405	.04416	-.09632
JACS	.19066	.78732	.52907	.01318
JCHEMSC	-.12914	.01697	.92257	-.09178
INORGCH	.04224	.24207	.87231	-.00175
JORGMETC	-.19436	.19302	.70483	-.14136
PHYSREVA	.09225	-.13880	-.16657	.96895

Factor designation:

'chem.phys' 'org.chem.' 'inorg.chem'

Table 9.3
Factor Analysis of Citing Pattern of 13 "Chemistry Journals"; Varimax Rotation - Kaiser Normalization; Four factors forced.

[75] The default factor analysis in SPSS generates a three factor solution.

If we force a four-factor solution in the citing patterns of this matrix, *Phys. Rev. A* loads significantly on this factor only (*Table 9.3*). *JACS* retains its interdisciplinary position. Subsequently, in the case of five factors the *J. of Organomet. Chem.* loads primarily on this factor. With more than four factors, factorial complexity and factor pattern correlations in the oblique solution also increase. In summary, we may conclude that *Physical Review A* constitutes a fourth element in the structure of the matrix along this axis. However, this was not made obviously visible by the factor analysis from SPSS or the two dendograms in the previous section.

9.2.3 Confirmational usage

In a confirmational design, we can investigate questions with respect to this matrix in more detail. For example, let us further investigate the 'interdisciplinary' position of *JACS* in the citing pattern of this matrix. We must therefore analyze in more detail the right leaf of the dendogram in *Figure 9.1*, which contains citing patterns for both 'inorganic' and 'organic chemistry' journals. Questions can be raised as to whether *JACS* should be considered as an isolate or as part of the 'organic chemistry' cluster; and so on.

one group			2.7088
	├ 'inorganic'		**2.5534**
	└ 'organic'		**2.4137**
		├ 'organic' –JACS	2.1101
		└ JACS	2.4304

Table 9.4
*Values of **H** associated with the right leaf of the dendogram in Figure 9.1*

Table 9.4 gives an overview of the associated values of *H* for these various options. It can be concluded that by the criterion of lower *H*'s for subgroups than for the aggregate, we should consider this set as two subgroups of journals only: the overall *H* (= 2.7088 bits) is larger than the value for the 'organic chemistry' group (2.4137 bits) or for 'inorganic chemistry' (2.5534 bits).

Further division of the 'organic chemistry' group into a separate group containing *JACS* and a group containing *J. Org. Chem.*, *Tetrahedron* and *Tetrahedron Letters* leads to an H for *JACS* of 2.4304, which is above the previously found value of the 'organic chemistry' group as a whole (2.4137). This means that the uncertainty has increased in this subgroup; therefore, in agreement with the above results from the explorative analysis, this hypothesis should be rejected.

Alternatively, the attribution of *JACS* to the 'inorganic chemistry' group, under the assumption of three subgroups, leads to only a slightly lower value for H_0 (0.7144 bits) than attribution to the 'organic chemistry' group (H_0 = 0.7170 bits). Therefore, *JACS* is more closely related to 'organic chemistry' than to 'inorganic chemistry' also in this analysis. However, the difference is marginal. This complex position of *JACS* between 'organic' and 'inorganic chemistry,' as evident from agglomerative clustering (*Figure 9.1*) and as factorial complexity from factor analysis (*Table 9.3*) was obscured in the results from divisive clustering, since each case then has to be attributed to a specific grouping.

9.3 GRAPHS AND CLIQUES

The above decomposition into clusters was based either on citing patterns or on cited patterns. In terms of the two-dimensional matrix H_{ij}, we grouped along one dimension only, that is, i or j, respectively. But in the case of a bivariate matrix we can also study H(citing,cited) and the effects of grouping in both dimensions in a single design.

Analogously to the above analysis in each of the dimensions, we can study the transmission $H(\text{citing}|a,\text{cited}|b)$, in which now both a and b are grouping variables. Grouping bivariate arrays in this way makes it possible to integrate relational approaches (from graph and clique analysis) with approaches based on structural equivalence (i.e., eigenvectors) into one conceptual framework.

Let me use the simple matrix depicted in *Figure 9.6* to explain this point. Principal component analysis, and therefore also factor

11	12	13	14
21	22	23	24
31	32	33	34
41	42	43	44

Figure 9.6
Illustrative, exemplary matrix

and cluster analysis, is based on grouping over rows *or* columns. However, were we for example to hypothesize that 1 and 2 form a clique, we would expect cells 11+ 12+ 21+ 22 to form a strong 'cluster' when compared with the sum of all the other cells. For both groups–the one supposedly forming a clique and the remainder–we can calculate an H_{ij}, and a P_{ij}, and after appropriate weighing an H_0 can be calculated (by subtraction from H_{ij} for the whole set).

Again, H_0 is a straightforward measure of the specificity of the subsets given the number of clusters (in this case two). However, the cluster may consist of any part of the matrix, and not necessarily a set of columns or rows: we can attribute each cell value to the hypothetical subset and to the grand sum respectively, and consequently calculate an H_0. For the same reasons as above, we are able to distinguish between the effects on H_0 of increasing the number of clusters ('cliques') and the effects of better grouping given a certain number of clusters.

Note that it is not necessary to determine only groups which are symmetrical with respect to the diagonal, since with these formulas we can vary along both dimensions independently. We may also decide not to include the diagonal elements themselves into the clusters, or to treat them as a separate group. Furthermore, the use of asymmetries in *i* and *j* leaves room also for the distinction between ‘weak’ and ‘strong’ graphs in the analysis. Strong cliques correspond to mutual diadic relations, and therefore they have to be operationalized in this framework in terms of the two variables *i* and *j* ranging over the same values only (i.e., $n < i < m$ AND $n < j < m$).

While the former analysis implied the notion of ‘structural equivalence’ as fundamental to the idea of factor and principal component analysis, this analysis addresses questions concerning relations in graphs, as they have been distinguished from the former analysis in social network analysis (Freeman 1978; Burt 1982).

With respect to the analysis of the 1984 matrix, I now limit the discussion to the question of whether the four major groups among the thirteen journals which we identified above in terms of structural equivalence also correspond to four cliques in this network. (However, in searching for four cliques in the matrix, we must allow for five groups, since the off-diagonal elements form a remainder-group.)

In terms of graph analysis, the ‘chemical physics’/ *Phys. Rev. A* -group separates into two cliques with the highest ‘within group’ densities, consisting of *Chem. Phys.*, *Chem. Phys. Lt.*, and *J. Chem. Phys.* on the one hand, and *Phys. Rev. A.*, *Mol. Phys.* and *J. Phys. Chem.*, on the other. The attribution with highest H_0 is boldfaced in *Table 9.5*. Note the differences from the results in the above analysis in terms of citing and cited patterns: as a clique, the group of three journals with ‘chemical physics’ in their title are one, while their citing and cited patterns are much more interactive with *Molecular Physics* and *J. Phys. Chem.* As a clique the latter two form a graph with *Phys. Rev. A*.

The values for H_0 in the *Table 9.5* are based on the initial assumption from the above analysis about the attribution of *JACS* to the ‘organic chemistry’ group of journals in the other part of the matrix. However, if in terms of clique analysis *JACS* is attributed to the ‘inorganic chemistry’ group, H_0 further increases by 0.0268 bits to 2.1352. Obviously in this analysis, the latter grouping is better than the inclusion of *JACS* as an ‘organic chemistry’ journal. As noted, we may vary the attribution over the two dimensions, and we may also group *JACS* asymmetrically, for its citing and cited patterns. (However, we have then to correct for the diagonal values.

Omission of the diagonal values can be argued for and against on substantive grounds, anyhow.)[76] It can be shown that the attribution of the *JACS* citing pattern to the 'inorganic' chemistry group, and of its cited pattern to 'organic' chemistry, leads to a further increase of H_0 of 0.0304 bits, as against a further increase of only 0.0255 bits for the attribution of cited to 'inorganic' and citing to 'organic chemistry.'[77]

		H_0
5 'chemical physics' journals	)	1.9982
Phys Rev A	)	
.... (various combinations with in-between values)		
Chem Phys + Chem Phys Lt + J Chem Phys	**)**	**2.1084**
Phys Rev A + Mol Phys + J Ph Ch-US	**)**	
Chem Phys Lt + J Chem Phys	)	2.1077
Phys Rev A + Mol Phys + J Ph Ch-US + Chem Phys	)	
Chem Phys + Chem Phys Lt	)	2.0358
Phys Rev A + Mol Phys + J Ph Ch-US + J Chem Phys	)	

Table 9.5
Clique analysis of the 'chemical physics' and Phys. Rev. A -group in terms of ***H_0*** *in bits of information*

[76] See for a discussion of the effect of diagonal values in the case of journal-journal citation matrices: Price 1981; Noma 1982; Todorov and Glänzel 1988.

[77] Tijssen *et al.* (1987) proposed the use of (quasi-)correspondence analysis for the study of asymmetry and the mapping of structure in a single picture. See Leydesdorff (1991c) for a further discussion of information theoretical indicators which measure asymmetry.

In summary, the grouping in four cliques shows *JACS* to be at an asymmetrical crossroads where the ‘inorganic’ and ‘organic chemistry’ groups meet. Additionally, a specialty group of journals which have ‘chemical physics’ as such in their title, is distinguished. Note that neither of these results is exhibited by applying clique (or ‘cohesion’) analysis using Burt’s (1987) program STRUCTURE (see *Figure 9.7*).

```
CONSERVATIVE CLUSTERING OF THE DISTANCES
(based upon the diameter or maximum algorithm)
-- high distances within clusters are
preserved

The criterion distance, D, is the largest
distance between any pair within a cluster.


        Values
         of D        0 0 0 0 0 0 1 0 1 1 0 0 1
    ------------     3 6 8 1 2 9 0 5 1 2 4 7 3
          .000       . . . . . . . . . . . XXX
          .000       . . . . . . . . . . XXXXX
          .000       . . . . . . . XXX . XXXXX
          .053       XXX . . . . . XXX . XXXXX
          .154       XXX . . XXX . XXX . XXXXX
          .275       XXX . . XXX . XXX XXXXXXX
          .358       XXXXX . XXX . XXX XXXXXXX
          .743       XXXXX XXXXX . XXX XXXXXXX
          .878       XXXXX XXXXXXX XXX XXXXXXX
          .959       XXXXX XXXXXXXXXXX XXXXXXX
          .995       XXXXX XXXXXXXXXXXXXXXXXXX
          .995       XXXXXXXXXXXXXXXXXXXXXXXXX
                     I J J C C J M J P T J J T
                     n C O h h P o C h e A O r
                     o h r e e h l h y t C r a
                     r e g m m C P e s r S g h
                     g m M P P h h m R a   C e
                     C S e h h U y P e h   h L
                     h c t y L S s h v e   e t
                         C s t       A     m
```

Figure 9.7
Cohesion Analysis of Citing Patterns Using STRUCTURE

9.4 THE DYNAMIC ANALYSIS

We now extend the analysis to similar matrices for 1981 and 1987, in addition to the one of 1984 used above. In a dynamic perspective we do not take 'time' or 'year' as another variate which co-varies with other variables. Instead, we compare among (sets of) events. The events are in this case the bi-variate probability distributions (that is, the matrices) for each of the three years, that is, 1981, 1984 and 1987. The following questions will be raised:

1. What is the overall pattern of change?
2. How can we decompose this overall pattern in terms of citing and cited patterns and in terms of cliques?
3. How can we best analyze these patterns? Is multivariate change or univariate change predominant? Is the structure changing, or only the composing elements?

9.4.1 Changing patterns

Table 9.6 exhibits the overall values for I_{ij} for the various years. Remember that this is a summary indicator of change in citing *and* cited patterns, since both these patterns are contained in each original matrix.

The unaggregated measure teaches us that the overall dynamics of the citation patterns between 1981 and 1984 were three times as large (54.3 mbits) as those between 1984 and 1987 (18.0 mbits). The values in the upper triangle are larger both in terms of cells and for its sum (Σ = 153.0 mbit) than for the lower triangle (Σ = 119.4), which makes it clear that overall we gain more information by comparing with the time axis than by comparing against the time axis.

The difference is due to the 1981-1984 period exclusively, and not to the 1984-1987 period: this result indicates a shift away from randomness in the matrix, that is, of more pronounced specification and differentiation[78] among the composing elements of the matrices during the 1981-1984 period, which came to a halt in later years.

a posteriori / *a priori*	1981	1984	1987
1981	****	54.3	81.7
1984	38.7	****	18.0
1987	62.7	18.0	****

Table 9.6
Expected information value for matrices of different years, in millibits

Where should we look for the sources of these changes? The difference between two corresponding cells f_{ij} in two matrices can be transformed into one ΔI_{ij} after normalization as relative frequencies, q_{ij} and p_{ij}. The overall I_{ij} between each two matrices is fully decomposable in terms of these Δs. As long as we normalize in terms of the grand sums of each of the matrices, the sums of any corresponding subsets can be compared straightforwardly. The 'information matrix' containing the Δs exhibits a transformation of the differences between the two matrices only.

An informative criterion for comparing subsets is the sign of a contribution to I_{ij}, that is, the sum of the Δs for a subset, to the overall I_{ij}. As noted in Chapter Eight, the *log* becomes negative if q is smaller than the corresponding p, and positive if q is larger than p; and therefore the sign of each of the cells, and of each subset, is a direct indicator of the relative increase or decrease of weight of that cell or that subset. (Note that I_{ij} for the whole group must be positive

[78] In the case of equiprobability in the *a priori* distribution, I becomes equal to the amount by which the entropy is reduced below the original (maximum) value by the *a posteriori* distribution (Theil 1972; see also Chapter Eight).

or zero (Theil 1972, at pp. 59f.).) This means that we can directly measure the dynamics among subsets using respective sigmas.

For example, if we want to compare the dynamics of citing patterns, we may sum the Δs over columns; if we want to compare cited patterns, we may sum the Δs over rows, and if we want to compare cliques as defined in the previous section, we may sum over respective rows and columns only, and compare the relative contributions to the overall I_{ij}. In addition, we may also analyze the difference between off-diagonal elements and diagonal elements for each subset, using the subtract of the two sigmas.

In summary, the matrix of values for ΔI which can be calculated between any two matrices representing the network elements can be used to answer a multitude of questions concerning the dynamics of clusters and graphs in various dimensions. Comparison of the three matrices under study here leads to the creation of three such matrices of Δ, notably one for the comparison of 1987 with 1984 data, one for the comparison of 1984 with 1981 data, and one for the comparison of 1987 with 1981 data.

9.4.2 The dynamics of relations among the thirteen journals

Let us now focus on the dynamics along the time axis, comparing 1981 with 1987 as *a priori* and *a posteriori* respectively. From *Table 9.6* (above) we know that the overall change over the period 1981-1987 is equivalent to a message of 81.7 millibits of information. The decomposition of these 81.7 mbits in terms of citing and cited patterns for each of the journals is given in *Table 9.7*.

The two values for each journal can also be used as coordinates on a map showing the dynamics of the citation patterns (*Figure 9.8*). Zero gain (or loss) in terms of expected information content in either dimension means that the pattern of that journal has been stable over the period under consideration; a positive value means a gain in relative contribution to the respective pattern, and a negative value a loss. Therefore, journals represented by points in the first quadrant gain in importance in both dimensions ('citing' and 'cited'), and journals represented by points in the third quadrant lose, relative to this journal set.

Note that these values are multi-variate and dynamic, in contrast to the 'impact factors' (Garfield 1979) and 'influence weights' (Narin 1976) of journals and other such indicators (Todorov and Glänzel 1988), which are time series points based on comparative static analysis, that is, computed for each year separately. In that case, the time-series of values of each indicator is plotted univariately against time, while we are here able to observe the dynamics multivariately, that is, including the interaction terms.

Citing		Cited	
J. Phys. Chem.-US	74.2 mbits	J. Phys. Chem.-US	47.1
J. Chem. Phys	47.0	Phys. Rev. A	38.1
Phys Rev A	30.9	Mol. Phys.	30.0
Tetrah. Lt	13.8	Chem. Phys. Lt.	15.0
Chem Phys Lt	9.7	Tetrah. Lt	11.1
J. Chem Soc. Dalton	-0.4	Chem. Phys.	6.1
Mol. Phys	-7.0	Tetrahedron	2.1
Inorg. Chem.	-8.0	J. Org. Chem.	1.4
J. Org. Chem.	-8.2	J. Chem. Phys.	1.4
Chem. Phys.	-11.2	J. Chem. Soc. Dalton	0.4
Tetrahedron	-12.1	Inorg. Chem.	-2.4
J. Org. Met.Chem.	-15.6	J. Org. Met. Chem.	-14.1
JACS	-31.4	JACS	-54.5
	(81.7)		(81.7)

Table 9.7
Decomposition of the expected information value of the change in journal-journal citation patterns for the period 1981-1987

Let me illustrate the difference between a time series of an indicator and this dynamic analysis by providing an example. One can compare the solutions for factor analysis and multidimensional scaling of two-dimensional arrays as if one were to cut the cube into slices, and then intrapolate the results. In this case, one finds stable patterns, particularly in the 'organic' and 'inorganic chemistry' parts of the map, and changes only in eigenvalues for factors. As an example, *Figure 9.9* shows the superposition of the multi-dimensional scaling solutions in two dimensions for 1981 (italicized) and 1987: the two pictures can be brought almost to coincide by rotation around the origin. However, from visual inspection of *Figure 9.8* it is obvious that the major effect in this matrix was the loss of relative contributions, both to the cited and the citing pattern, of *JACS* as a central journal. However, in *Figure 9.9* the position of *JACS* is almost completely stable!

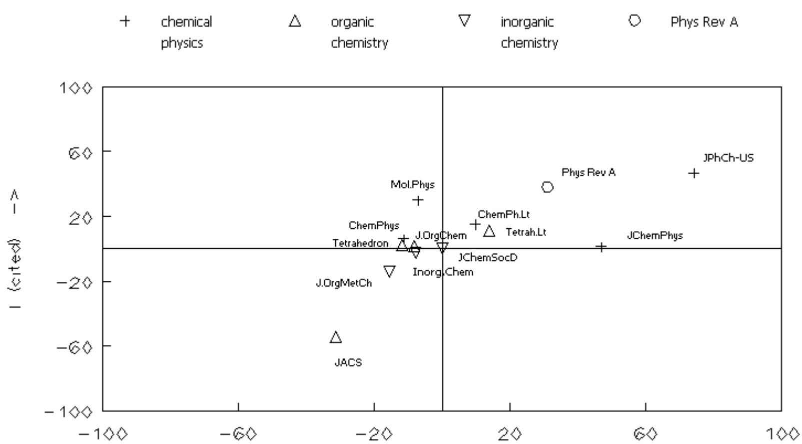

Figure 9.8
Dynamic analysis of citation patterns of 13 chemistry journals, 1981-1987.

Further analysis of the matrix of values for ΔIs (see above) teaches us that more specialized journals, particularly in the 'chemical physics' and 'physical chemistry' part of the set, gain weight in the data matrix at the expense of *JACS*. If we focus on the 'organic' and 'inorganic' subsets only–excluding *JACS*–then the 'inorganic' subgroup is 'losing' most in relation to this set. However, in terms of citing behavior, the expansion of citations in the 'chemical physics' and 'physics' part of the matrix is quantitatively more important than the relative increase of 'organic chemistry' in relation to only 'inorganic chemistry' and *JACS* in the other part of the matrix.

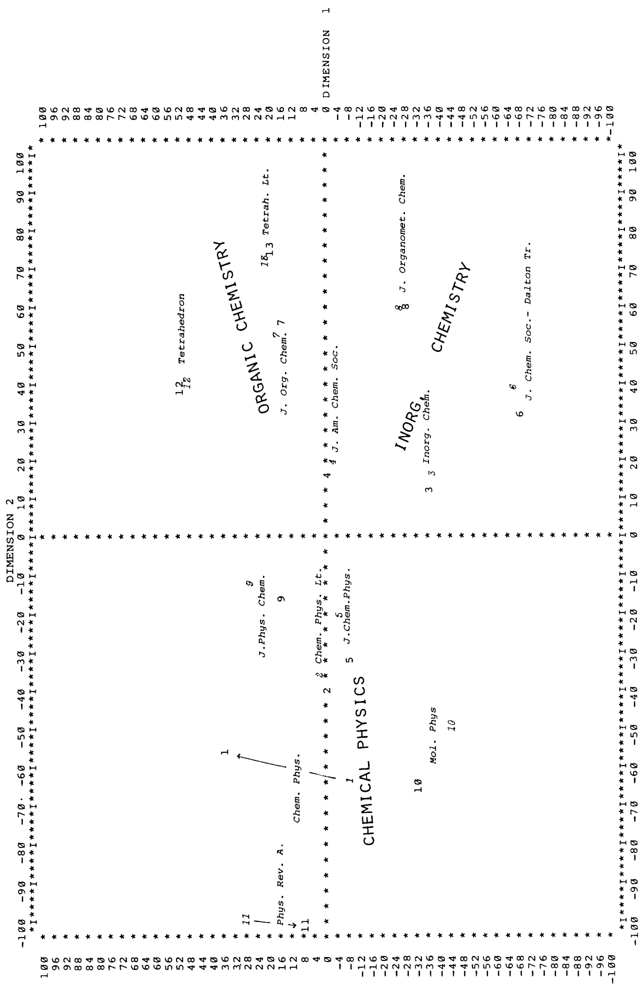

Figure 9.9
Multi-dimensional scaling (MINISSA) for 13 chemistry journals – 1981 and 1987 superimposed.

More detailed analysis of this part of the matrix, that is , the interface of chemistry with physics, teaches us that the journals with ‘chemical physics’ in their title have lost coherence among themselves (as a clique), and hence have also become more integrated into the group of other ‘physics’ journals. The gains in contribution of the latter group to the overall change, both in terms of being cited patterns and citing behavior, have been most important.

The major advantage of the approach of using the matrices of Δs for all cells is that we gain a more informative picture by direct summation of the contributions of any subset without the need for further assumptions.

9.5 REVISION OF THE PREDICTION

In addition to the study of relations between two matrices, we may raise the question of whether, and if so, to what extent the prediction of the posterior matrix from the prior matrix is improved or worsened by using in-between data to revise the prediction. The importance of this technique is also that it gives us the basis for a test of whether the in-between data are just to be regarded as a case between prior and posterior cases, or as containing additional information which merits separate analysis. This provides a particularly useful tool if one wants to reconstruct a line of actions in terms of irreversibilities and path-dependencies, as, for example, is often the case in science and technology studies.

In Chapter Eight, a *critical revision* was defined as the case where the revision is not only positive, but where the following inequality also holds:

$$I(q:p) - I(q:p') \quad > \quad I(p':p)$$

or:

$$I(q:p) \quad > \quad I(q:p') + I(p':p)$$

that is, in terms of expected information value, the pathway of the signal through the revision is more efficient than the direct transfer of the signal from the prior to the posterior event. In this case, one can consider the revision as an auxiliary transmitter which boosted the signal from the original sender. Consequently, there is no longer any need for the receiver to listen to the original sender.

If we apply this reasoning to the '1981' data as prior, and the '1987' data as posterior distributions, the '1984' data as a revision of the prediction *satisfies the inequality*. (By using *Table 9.6*: 81.7 is greater than 54.3 + 18.0; and also in the reverse direction: 62.7 > 38.7 + 18.0.) The in-between year '1984' thus boosts the signal from '1981' to '1987,' and vice versa. In other words, when using 1984-data in the prediction of 1987-data, the 1981-data are no longer relevant. This indicates the Markov property, which is well-known in systems theory: a system has the (first-order) Markov property if the future behavior of a *system* is not determined by its previous history, but by its present state.

Thus, the data provide us with an indication that the overall development of the data set is not just the sum of the development of its components.[79] What can be considered as a network of relations among lower-level units can also be considered as a system at a one-higher level. Here, we begin to envisage the relations between network analysis and system dynamic modelling.

9.6 FORECASTING

In addition to the dynamic analysis of network data, the use of *I* as a measure of dynamic development makes it possible to make best forecasts on the basis of a time-series of data, also in the multi-

[79] The revision of the prediction is also disaggregatable with respect to subsets of the matrix. In this journal-journal citation data, however, none of Δs *I* for rows and columns changed sign for the two periods of 1981-1984 and 1984-1987, respectively, but not in all such disaggregated cases were the in-between data for 1984 a *critical* revision of the prediction by the 1981 data in the sense of the inequality discussed above.

variate case. To arrive at an information theoretical equivalent of univariate time-series analysis–which we will then generalize to the multivariate case in the next step–we have to transform the time-series of data between the year *m* and the year *n* into a probability distribution using:

$$P_i = F_i / \Sigma_{i=m}^{n} F_i$$

We can visualize this distribution as follows:

year ->

P_m	P_{m+1}	P_{m+2}					P_{n-2}	P_{n-1}	P_n

In order to extend this series to the year *n*+1, we will compare the distribution in the former series (*m*, *m*+1, ..., *n*-1, *n*) with the distribution in the series (*m*+1, *m*+2, ..., *n*, *n*+1):

year ->

P_m	P_{m+1}	P_{m+2}					P_{n-2}	P_{n-1}	P_n	
	Q_{m+1}	Q_{m+2}					Q_{n-2}	Q_{n-1}	Q_n	Q_{n+1}

Remember that the best prediction is the one with the lowest *I*. Since the data for all years are given except for the year *n+1*, the best prediction for Q_{n+1} would be based on the addition of $\Delta I = 0$ to the Σ which constitutes the *I*:

$$\Delta I = Q_{n+1} \log (Q_{n+1} / P_n) = 0 \qquad (9.4)$$

For $Q_{n+1} > 0$, $\Delta I = 0$, only if:

$$\log (Q_{n+1} / P_n) = 0 \qquad (9.5)$$

or:

$$Q_{n+1} = P_n \tag{9.6}$$

From this equation one can derive the value (F) of the variable for the year $n + 1$ as a function of the value of that same indicator in the previous years of the series:[80]

$$F_{n+1} = \left\{ \frac{(\Sigma_m^n F_i) - F_m}{(\Sigma_m^n F_i) - F_n} \right\} * F_n \tag{9.7}$$

The coefficient is the sum of the time series minus the value for the first year of the series divided by the same sum minus the value of the last year.

The interpretation is simple: with no further information ($\Delta I = 0$), we may assume that the *distribution* of the time series data remains the same for the next year with the difference of one year only. Note that this assumption is much weaker than the assumption of linearity (or of a higher-order polynomial relationship) implied in regression analysis and time series analysis.[81]

---

[80] The probabilities of a distribution are defined in terms of frequencies F_i as follows:

$$P_i = F_i / \Sigma_{i=m}^n F_i$$

$$Q_i = F_i / \Sigma_{i=m+1}^{n+1} F_i$$

However, obviously:

$$\Sigma_{i=m+1}^{n+1} F_i = (\Sigma_{i=m}^n F_i) - F_m + F_{n+1}$$

Since $Q_{n+1} = P_n$:

$$\frac{F_{n+1}}{\Sigma_m^n F_i - F_m + F_{n+1}} = \frac{F_n}{\Sigma_m^n F_i}$$

From which we can calculate F_{n+1}, and then Q_{n+1} also follows.

[81] In Leydesdorff (1990d), the prediction based on the dynamic measure

Since the measurement is non-parametric, we are not required to make any further assumptions about the character of the trend beyond the assumption that without any additional information, we have no reason to expect change in the distribution over the years under study except for the noted advancement of one year only.

The extension of the univariate forecast to the multivariate one is straightforward. Following the arguments presented above, we can use the following figures for the multi-variate prediction:

year ->

$P_{1,m}$	$P_{1,m+1}$	$P_{1,m+2}$			$P_{1,n-1}$	$P_{1,n}$
$P_{2,m}$	$P_{2,m+1}$	$P_{2,m+2}$			$P_{2,n-1}$	$P_{2,n}$
$P_{j,m}$	$P_{j,m+1}$	$P_{j,m+2}$			$P_{j,n-1}$	$P_{j,n}$

$Q_{1,m+1}$	$Q_{1,m+2}$			$Q_{1,n-2}$	$Q_{1,n-1}$	$Q_{1,n}$	$Q_{1,n+1}$
$Q_{2,m+1}$	$Q_{2,m+2}$			$Q_{2,n-2}$	$Q_{2,n-1}$	$Q_{2,n}$	$Q_{2,n+1}$
$P_{j,m}$	$P_{j,m+1}$	$P_{j,m+2}$			$Q_{j,n-1}$	$Q_{j,n}$	$Q_{j,n+1}$

As above:

for $Q_{j,n+1} > 0$, $\Delta I = 0$, only if $Q_{j,n+1} = P_{j,n}$

and therefore:

$$\frac{F_{j,n+1}}{\Sigma_{i=m+1}^{n+1}\Sigma_j F_{ij}} = \frac{F_{jn}}{\Sigma_{i=m}^{n}\Sigma_j F_{ij}}$$

I is discussed in more detail, and in comparison to other available statistical techniques like for example, ARIMA.

However, obviously:

$$\Sigma_{i=m+1}^{n+1}\Sigma_j F_{ij} = \Sigma_j \{ \Sigma_{i=m}^{n} F_{ij} - F_{mj} + F_{n+1,j} \}$$

and therefore:

$$F_{j,n+1} = F_{jn} * \left\{ \frac{\Sigma_{i=m}^{n}\Sigma_j F_{ij} - \Sigma_j F_{mj} + \Sigma_j F_{n+1,j}}{\Sigma_{i=m}^{n}\Sigma_j F_{ij}} \right\}$$

$$F_{j,n+1} = F_{jn} * \left\{ \frac{\text{Grandsum}_{mn} - \text{Columnsum}_{m} + \text{Columnsum}_{n+1}}{\text{Grandsum}_{mn}} \right\}$$

Since the column sum for the year *n+1* is a normalization factor only,[82] the right-hand factor is a constant, and we may conclude that according to this reasoning the best prediction for next year's distribution would always be the current distribution (*n*). One may formulate this alternatively: as a *system* the dataset has no memory of the values of individual elements in previous states. (As noted, this is called the Markov property in systems theory. I return to the issue in the next chapter.)

However, we can now make *two* best forecasts: one on the basis of the values of individual elements of the system, and another for the dataset as a system, that is, on the basis of the last year's distribution. By comparing these forecasts with actual values, we are able to develop a basis for a test in order to distinguish whether or not the elements develop as coordinates in a system.

Since three matrices did not seem enough data for such a comparison, I used additionally the corresponding data for all the years in between 1981 and 1987. The results are summarized in *Table 9.8*. The columns represent the *a posteriori* distributions, the

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[82] The prediction of the value of this sum may, for example, be calculated on the basis of the time series for the column sums by using the methods specified above for the uni-variate case.

	82	83	84	85	86	87 (*a posteriori*)
81	**33.4**	115.3	96.8	16.0	16.1	23.3
82		**19.1**	187.2	15.6	17.1	24.3
83			**13.8**	32.4	20.7	28.0
84				**8.1**	29.9	30.3
85					**14.9**	44.5
86						**19.8**
(a priori)						

Table 9.8
Comparison of uni-variate and multi-variate predictions

rows the *a priori* ones. The expected information values are noted on the basis of uni-variate predictions.

The last figure in each column corresponds to the multi-variate prediction, since it is based on the last year only. It is clear that in none of the cases does the prediction on the basis of the uni-variate time series improve the prediction on the basis of the previous year only. Therefore, we may now conclude that the data does indeed change over time as a single system.[83]

9.7 SUMMARY AND CONCLUSIONS

By using information theoretical measures, I first addressed the question of measuring asymmetry in the matrix, and the respective contributions of rows and columns. Then I raised the question of whether the presence of structure in the matrix can be revealed using these methods. I showed that one can create, firstly, an exact dendogram in which the length of the leaves represents (in bits of

[83] However, from *Table 9.6* we know that the expected information value of the change between 1984 and 1987 is only 18.0 mbits, whereas we have a value of 19.8 mbits for the change between 1986 and 1987. Therefore, in this case only (and for 1987 data only), the data for a previous year would be a better predictor than those for the last year.

information) the asymmetrical mutual distances among the cases; and secondly, using divisive clustering one can determine the exact number of clusters (groups, factors, etc.) if the 'in-between group uncertainty' H_0 has a maximum value.

The analysis of the grouping, which up to this point had addressed one dimension of the matrix only, was subsequently generalized to any grouping in terms of the two dimensions of the matrix, including clique analysis as the special case in which one part of the matrix (e.g., including diadic relations) is analyzed in relation to other parts or to the remainder of it. The graph-analytic approach ('cohesion' or 'relational') and the factor-analytic ('structural equivalence' or 'positional') approach can be considered as special cases of a general algorithm for grouping in multi-variate arrays.

The next sections of the study addressed the dynamic analysis using the corresponding data for 1981 and 1987. The results give a view of the data that is completely different from the comparison of results of various forms of multi-variate analysis for each year separately. The measures of information theory make it possible to address issues of dynamic multivariate systems which cannot easily be analyzed in a single coherent theoretical framework using various, more common statistical tools.

9.8 RELEVANCE FOR SOCIAL NETWORK ANALYSIS

In his seminal study, Burt (1982, at p. 9) pictured his model as in *Figure 9.10*. Note that the arrow from 'action' is the only incoming one for 'structure;' thus, structure is to be explained in terms of (aggregates and patterns of) action. Obviously, with this model one can study only the relations between various aggregates of actions, and therefore in this theory, network analysis was a special case of multi-variate and multi-level analysis; but the model does not yet address the core questions concerning the *dynamics* of 'structure'/'action' contingencies.

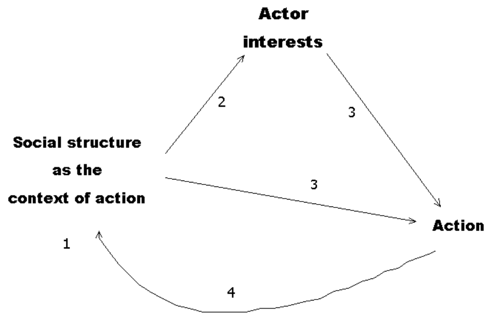

Figure 9.10
Components in a structural theory of action. (Source: Burt 1982, p. 9.)

While the loop in *Figure 9.10* suggests a dynamic feedback, in methodological terms this model is static: it is a loop, and not a spiral! If one extends the loop to a spiral with *time* as an additional dimension, the model takes the shape of *Figure 9.11*. However, this is a rather different model: now, structure has an additional incoming arrow from structure at a previous moment. In other words: structure is also self-referential.

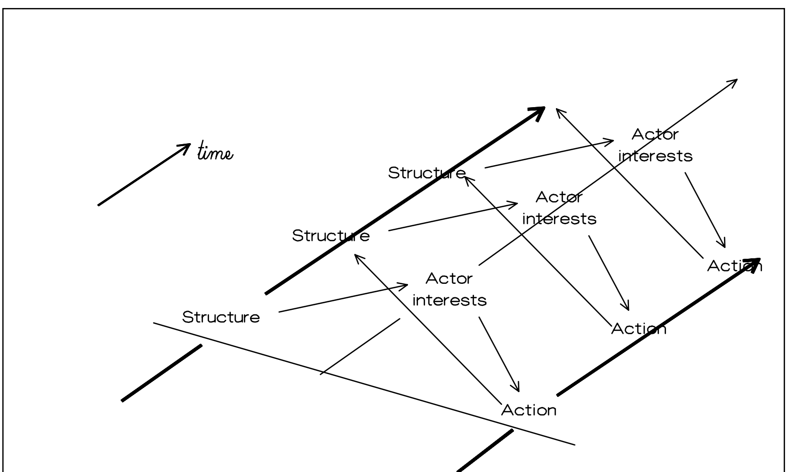

Figure 9.11
A dynamic extension of the structuralist model of Figure 9.10: Structure conditions action; action changes structure.

The problem of structure and action in sociology, therefore, is one step more complex than the choice of a positional or a relational approach in a static model. In either case, one still has to relate the results of the multivariate analysis to a dynamic perspective. *Figure 9.11* shows that the underdetermination of action by structure can be conceptualized as a static conditional probability distribution, while the reproduction and change of structure by action can never be a mere product of (static) aggregation, but must be the product of a dynamic interaction (cf. Giddens 1984).[84]

The constraints of structure upon action and the effects of action upon structure can be expressed as static and dynamic relations among conditional probability distributions. These models can be analyzed in one framework using the various methods from information theory. For example, with respect to the arrows in *Figure 9.11* we can address the following questions:

1. How much does knowledge about the structural conditions reduce our uncertainty of the distribution of actor behavior? The constraining and enabling function of structure (*s*)–for example, reputation–in relation to action (*a*)–for example, citations–at any moment in time, may be described as the static conditional relation between the (multi-variate) probability distribution of *a* and *s*, that is, $H(a) - H(a|s)$.[85]

2. Analogously, the effect of (aggregated) action on structure is a dynamic conditional probability relation, which can be expressed by formulas like:

$$I(s_{t=2} : s_{t=1}) - I(s_{(t=2)}|a_{(t=1)} : s_{(t=1)}|a_{(t=1)})$$

[84] One may wish to complicate the analysis by adding more arrows to the scheme, such as for example, action at t_1 having an effect on structure at t_3 without necessarily influencing the in-between structure at t_2, or action being guided by perceptions of previous structures as intermediating variables, etc.

[85] One may wish to include structure at previous moments into the analysis by using *s* at t= n-1 instead of *s* at t= n.

Despite its seeming complexity, this formula is easy to compute.

Note that every relation is again decomposable down to the level of the individual case, and that at each level we may apply directly the methodologies which were elaborated in previous sections of this chapter, for example, in order to identify clusters and cliques. Additionally, the explicit introduction of the time axis is not only methodologically fruitful but also theoretically meaningful. Structure is not just an aggregate of action;[86] as visible in *Figure 9.11*, the dynamic conceptualization urges us to reformulate their relations as the interaction between *two* self-referential loops (cf. Luhmann 1984; Leydesdorff 1993b). I return to this model in Chapter Twelve (see also Leydesdorff, 2001).

[86] Cf. Parsons (1950): "It is essential from the point of view of social science to treat the social system as a distinct and independent entity which must be studied and analyzed on its own level, not as a composite resultant of the actions of the component individuals alone."

Chapter 10

Irreversibilities in Science and Technology Networks

The French proverb 'Plus ça change, plusc'est la même chose' poses a problem to social scientists who are interested in the study of developments. Amidst the variability in the phenomena, there may be underlying stability in the structure of the system under study. The methodological question is whether one can distinguish changes of the system from changes that occur within the system. Obviously, this problem concerns what the researcher has specified as a system, that is, its elements, its boundaries and its operations; and the answer may also vary with the time perspective which one uses. However, change in what constitutes a system is of a different order from changes within the parameters set by these definitions.

In science studies one is often more interested in the more fundamental type of change, that is, in the dynamics of an assumed system rather than in the dynamics within this system. For example, in the reconstruction of scientific developments one may wish to focus on the 'emergence' of new paradigms (concepts, phenomena, research traditions, etc.) and on their sometimes discontinuous relations to older traditions. How have the new definitions of the situation arisen historically? Are the new developments to be analyzed in terms of the internal dynamics of the emerging complex, or rather in terms of relevant historical contexts?

Generalized to a methodological question, one may wish to ask whether one is able to specify criteria in order to decide whether change has to be attributed to the development of the system, to its contexts, or to their interaction at the boundaries of the system. How can we reconstruct developments, that is, changes over time, in a system which changes in a changing environment? Can one only achieve a qualitative, intuitive and historical understanding in the concrete case, or can we also develop more formal methods to study these questions?

10.1 METHODOLOGY

The model system which I develop here essentially addresses the question of how two strong groups of three 'actors' can be linked into one group, and how this link between the groups can further develop so that the transition into a one-group system becomes 'irreversible' (see *Figure 10.1*).

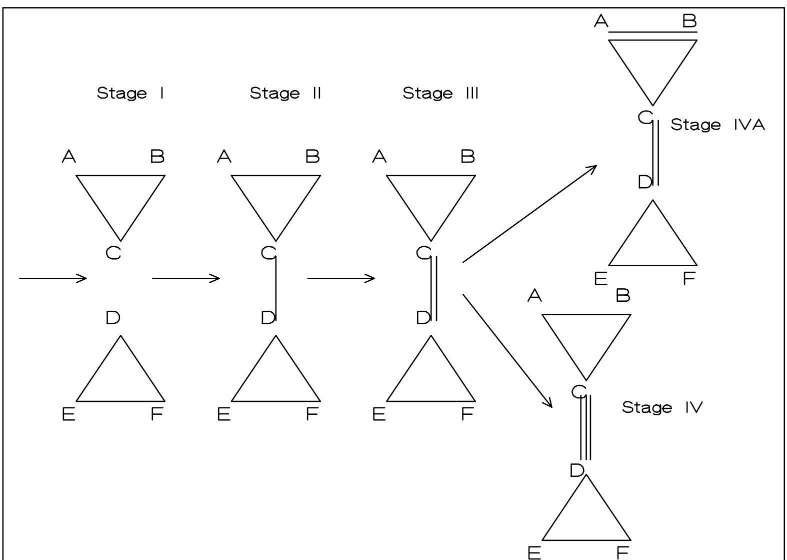

Figure 10.1
The model system

This question originated as an operationalization of a problem from the French 'actor-network' approach. Callon (1990) postulated such 'irreversibilities' in techno-economic networks. After having described the constitution of these networks, he presented the following problem:

> (O)ne problem remains to be resolved. How do the different actor-networks come together, while they had no reason *a priori* to be compatible among each other: B does not accept the definition given by A, C gives another identity to B ....? Starting from the possible divergencies, how can we explain the creation of an agreement, a compatibility, and how can we account for the latter's stability? The answer involves the process of convergence and irreversabilization in technical-economic networks.[87]

Callon (1990) focused mainly on the quality of the network relation and its operation. (The latter was also called 'translation.') However, since the question is stated as a problem in a model system, we may also make it subject to more formal considerations.

What are the chances for the emergence of a network among previously unrelated actors? Is there a probability that subsequently the network is more likely to be maintained by its operation than to fall apart again? Are there conditions under which network structures tend to become more pronounced? If an especially strong link is formed in a network such as the one between the two groups, what will be the effect of this process on the options for the further development of the network?

Let us assume that there are six actors (A, B, ... F) who may maintain relations with one another. Initially, each actor can have five

	A	B	C	D	E
B	1				
C	1	1			
D	1	1	1		
E	1	1	1	1	
F	1	1	1	1	1

Figure 10.2
The equi-probability triangle (Stage 0)

[87] *Ibid.*, translated from the French preprint, p. 14.

relations, and there are a total of 15 possible relations (6 x 5 / 2, i.e., the lower triangle of a 6 x 6 matrix). Let us also assume that in the initial state the 6 actors are divided into two groups, (A,B,C) and (D,E,F), who maintain relations within each group, but that there are no links between the two groups.

The baseline for the measurement of the links is the assumption that all relations are equally probable. If all relations are equally probable, the total uncertainty in the lower triangle (see *Figure 10.2*), which we shall henceforth use for our computations, is equal to $\log N = \log(15)$. The ${}^2\log(15) = 3.90689$ bits.[88]

Let us now introduce the two groups, that is, all the relations among A, B, and C, on the one side, and among D, E, and F on the other. In similar triangle format, we can write these relations as exhibited in *Figure 10.3*. The distinction between the two groups is obvious from the matrix. Of course, one can also draw this situation as a picture of two unrelated graphs (see *Figure 10.1*).

	A	B	C	D	E
B	2				
C	2	2			
D	1	1	1		
E	1	1	1	2	
F	1	1	1	2	2

Figure 10.3
Stage I; see Figure 10.1 for the graphic depiction

The uncertainty in this lower triangle has decreased in comparison with the previous situation. There is no longer equi-probability, since there is a probability of 6/ 21 for a linkage, now indicated by a '2', and a probability of 9/ 21 for a non-linkage similar to the previous situation.[89] The overall uncertainty in the triangle is:

[88] As in the other chapters, the two-base of the logarithm allows us to express all the information values in bits.

[89] I develop the problem here numerically, assuming that the links are at regular intervals, because the argument is then easier to follow than with

$$\begin{aligned} H &= -\Sigma_i\, p_i\, {}^2\log p_i \\ &= -9 \cdot \{(1/21) \cdot {}^2\log(1/21)\} \\ &\quad -6 \cdot \{(2/21) \cdot {}^2\log(2/21)\} \\ &= 3.82089 \text{ bits} \end{aligned}$$

The decrease in relation to the maximum entropy, i.e., the gain in information content, of this triangle is only: 3.90689 - 3.82089 = 0.086 bits, 86 millibits or 2.2%.[90]

We use Stage I as our starting configuration. From here on, we shall assume that the system develops by adding (or deleting) one link at a time. If we add one link at this stage, it may either change a '1' into a '2' or a '2' into a '3' within this network structure. Let us first focus on the possibility (indicated in *Figure 10.1*) of introducing one link between the two triads, by increasing the value for, say, the link between C and D to two. (The choice of C and D is arbitrary. Alternatively, we might have linked A and F: in terms

	A	B	C	D	E
B	2				
C	2	2			
D	1	1	2		
E	1	1	1	2	
F	1	1	1	2	2

Figure 10.4
Stage II

mathematical symbols. However, the problem can also be developed by using, for example, x as a measure for the initial equiprobability, and y_i for each increase.

[90] As noted in Chapter Eight, one can reach this same value by using the formula for I which expresses the expected information value of the message associated with the transformation of the previous distribution into the latter, or alternatively formulated, the expected information content of the *a posteriori* probability distribution, given the *a priori* probability distribution. The dynamic measure allows us in general to compare configurations without recurrence to a baseline of equiprobability.

of the probability distribution or the graph, this would have made no difference, since in both cases one previous '1' is merely changed into a posterior '2' and the two triangles become linked.)

The new configuration can be written as the lower triangle in *Figure 10.4*. Then, we are able to compute a value of *I* given either of the two above matrices as the *a priori* one. We may also say that we can evaluate this configuration as a new event in relation to the two previous stages. The respective values are: 23.79 mbits when compared with the latter configuration (Stage I), and 83.82 mbits when compared with the equiprobability triangle (Stage 0). Note that the distance to maximal entropy in terms of millibits of information has decreased (83.82 mbits instead of 86.00). By adding another two to the matrix, the probabilities between the occurrences of ones and twos have become more equal again.

However, if we compare the configuration in Stage II with the previous case of the two disconnected triads, the expected information value which can be associated with this last change is much lower (23.79 mbits only), since these two matrices are rather similar. When we compare the three situations, calling them Stages 0, I and II, respectively, we can draw a triangle (*Figure 10.5*), in which distances can be evaluated in terms of the amount of information associated with the transmission between them as senders (*a priori*) and receivers (*a posteriori*) of information.

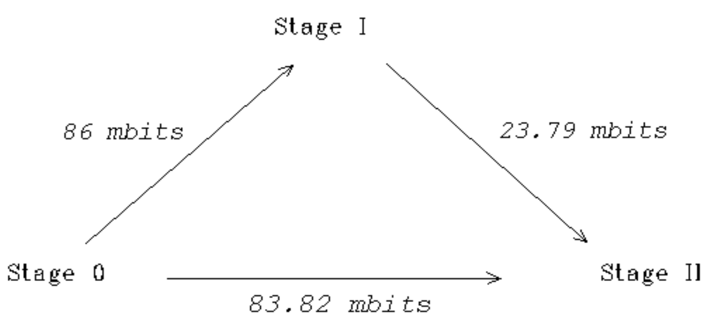

Figure 10.5
Expected information values associated to the transfer of messages to Stage II as an ***a posteriori*** *configuration*

Inspection of *Figure 10.5* teaches us that the prediction of the configuration at Stage II by the information content at Stage I is an improvement of the prediction in comparison to the prediction at Stage 0. (Remember that the value of the expected information content of the message (I) is an inverse measure of the quality of the prediction (Theil 1966).) However, the pathway of the signal through Stage I is inefficient in the transition from Stage 0 to Stage II. We return to this issue below, because it may provide us with an operationalization of 'path dependency' or what in the sociology of translation has been called 'obligatory passage points' (cf. Callon 1985).

From Stage I several other follow-up configurations could have arisen on the assumption that the system changes one link at a time. An additional link could have been absorbed by one of the two triangles of actors, so that one double link, a '3', would have been formed within this triangle. This change (into Stage IA) is only associated with 12.65 mbits of information. The information content of the message is an inverse measure of the prediction, and therefore the latter development is more likely than the development of the link between the two groups, which was evaluated above as requiring 23.79 mbits of information.

If we allow also for the *deletion* of a link, a third possibility would have been that one of the twos might have 'decayed' to a one again, so transforming one of the two triangles into a so-called 'weak' graph. (Let us assume that we have not allowed for the disappearance of the possibility of having a link between actors, as would be indicated by a zero in the corresponding cell.) This transformation (into Stage IB) is associated with 20.39 mbits of information.

Note that the *a priori* probabilities that the system at Stage I will develop into Stage IA or Stage IB are equal (i.e., 6/21). If the event occurs, the system, however, achieves a new state, and this state of affairs subsequently changes all the expectations for the system's future behavior. Subsequent transitions should therefore not be computed as independent chances (that is, by multiplication of *a priori* probabilities), but as conditioned by what has already occurred in the past. The information content of the message that this development has taken place is normalized *a posteriori*, i.e., with reference to the actual occurrence of the event(s). The network retains the information; it processes its history self-referentially. Were this not so, adding more links to the system would cause it to oscillate towards equi-probability.

For a quantitative evaluation of the probability of the various transitions (to be discussed in more detail below), we have to take into account that there are six twos and nine ones in Stage I, so that the chance that a one will change is larger than the chance that a two will change (see also Section 10.5 below). Apart from these chances, we can note that the transition to Stage II is associated with more information content (23.79 bits) than any of the other transitions available to Stage I. Thus, this transition is relatively unlikely to occur.

Given Stage II, one can investigate the addition of one further link to the effect that one two becomes a three at this stage. In terms of the probability distribution, it again does not matter which link we choose to increase, but let us take our newly formed link as an example. We arrive now at the following Stage III (*Figure 10.6*).

	A	B	C	D	E
B	2				
C	2	2			
D	1	1	3		
E	1	1	1	2	
F	1	1	1	2	2

Figure 10.6
Stage III

This more pronounced pattern is, of course, less random, and therefore, the expected information value associated with its distance to the equiprobability situation has increased to 111.80 mbits. However, we can now compute two more distances, notably between Stage III and Stage II, and between Stage III and Stage I. These distances are displayed in *Table 10.1*.

a priori / *a posteriori*	**0**	**I**	**II**
I	86.00		
II	83.82	23.79	
III	111.80	75.49	12.17

Table 10.1
Expected information values of messages among three stages

However, if we now evaluate the relations among Stages III, II and I in terms of Stage III as a receiver of a signal from the initial Stage I, which may be improved or worsened by the configuration at Stage II as an in-between stage, a new situation has occurred. Contrary to the geometrics of the triangle (*Figure 10.7*), the sum of two sides {(I -> II) + (II -> III)} is shorter than the one other side (I -> III), i.e., the shortest road for the information is to travel from I to III through II, or alternatively formulated: II acts as an auxiliary transmitter which boosts the signal from I to III.

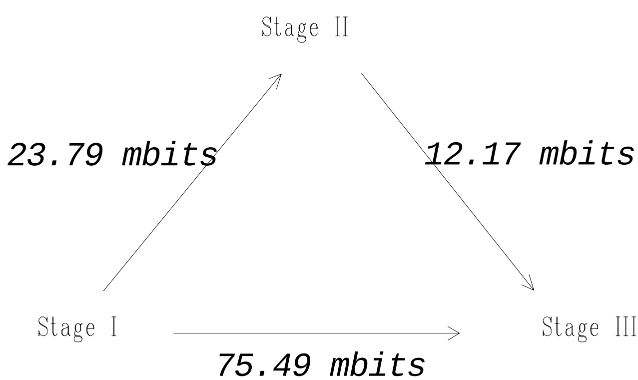

Figure 10.7
Expected information values associated to the transfer of messages to Stage III as an ***a posteriori*** *configuration*

Therefore, when we are at the receiving end (in Stage III), we need only to listen to the signal from the previous stage II; the earlier history of how that stage came into being is no longer of relevance, once we have arrived at III. The configuration of II has not only improved the prediction of Stage III by the configuration at Stage I, but it has also 'amplified' the signal.

This result can be given meaning in qualitative terms and in statistical terms. In qualitative terms it means that given this unit of change (the addition or deletion of one link at a time), the transition from Stage I to Stage III is *path dependent on* Stage II. An argument in terms of the sociology of translation can be made to consider Stage II as an 'obligatory passage point' (e.g., Callon 1985, at pp. 205 ff.).

In statistical terms, the phenomenon can be expressed as the Markov property: a system is credited with the (first order) Markov property if it contains all the information needed for its future behavior. This means that no information is obtained by knowing, beyond the state of the system at $t = t_1$, previous states of the system at $t < t_1$. In fact, the configuration of stage II has become the best predictor of stage III, irrespective of the previous stages of the system.

Therefore, already at the stage where we add one 'double link' to the network in this sequence, the configuration has started to behave as a system, and we may speak of 'path dependency.' Note that this implies a notion of 'irreversibility,' although the transition is of course still 'reversible' in the sense of the possibility of decay, if our theory–as we have assumed hitherto–also allows for the deletion of links.

Once we have reached Stage III, there are several possibilities for its further development. Among them, the system may develop to an even more pronounced system having one 'triple link,' it may develop 'two double links,' or it may decay to the previous Stage II. Let us call the further development of the one privileged link into a triple link stage IV, and the addition of one more double link stage IVa.

a priori / *a posteriori*	0	I	II	III	IV	V	IVA
0		85.43	85.87	111.01	144.74	182.07	133.41
I	86.00		19.50	55.77	97.41	140.97	61.46
II	83.82	23.79		10.95	34.62	64.25	19.17
III	111.80	75.94	12.17		7.27	24.17	10.53
IV	155.26	140.69	41.14	7.77		5.24	20.43
V	207.42	212.85	79.96	27.10	5.49		41.70
IVA	134.84	78.60	20.71	11.72	21.24	39.89	

Table 10.2
Expected information values of changes in the network

Table 10.2 provides us with an overview of all the associated expected information values for these developments back and forth, including the further development of Stage IV into Stage V, and vice versa. For example, the fourth column (Stage III as an *a priori* case) teaches us that the development of Stage III into Stage IV is more likely than the reversion of Stage III into Stage II. The latter step is associated with 41% more information than the former (10.95/ 7.77 = 1.41). The further development of a second double link is even more unlikely (11.72/ 7.77 = 1.51). Note that eventually an advanced state (e.g. Stage III) is more likely to develop from the decay of a higher state (e.g., Stage IV) than from the extension of a previous one (e.g., Stage II).

In summary: once the threshold of a configuration has been reached, the tendency in this network is to 'auto-amplify' upon further development to more pronounced stages (cf. Maruyama 1963). From then onwards, more dispersed configurations are more likely to develop as forms of decay from more pronounced situations than as a result of less structured configurations.

10.2 *MARKOV* CHAINS AND THE PROBLEM OF 'EMERGENCE'

The above results give rise to the question of why, at the transition from Stage II to Stage III, 'path dependency' becomes relevant. The comparison of transitions strongly suggests the treatment of this question in terms of Markov chain models. First-order Markov chains have been shown to be useful in the formal modelling of social and economic processes which involve change in a configuration.[91] Well-known examples include inter- and intragenerational mobility studies (Theil 1972).

		1	2	Stage II (*a posteriori*)
Stage I	1	8/9	1/9	
(*a priori*)	2	0	1	

Table 10.3
Transition matrix between Stage I and Stage II

		1	2	3	Stage III
Stage II	1	1	0	0	
	2	0	6/7	1/7	

Table 10.4
Transition matrix between Stage II and Stage III

		1	2	4	Stage IV
Stage III	1	1	0	0	
	2	0	1	0	
	3	0	0	1	

Table 10.5
Transition matrix between Stage III and Stage IV

[91] See for an introduction, e.g., Bradley and Meek (1986).

Let us consider the model system in terms of transition matrices. *Table 10.3* exhibits the transition matrix for the transition from Stage I to Stage II, *Table 10.4* for the transition from Stage II to Stage III, and *Table 10.5* for the one from Stage III to Stage IV. As Markov chain models, all these transition matrices are *irregular*, but as we shall see, for different reasons.[92]

The equilibrium probability (row) vector associated with the first transition as a first-order Markov chain process (*Table 10.3*) is [0 1].[93] This means that if we repeat this transition often enough, all the ones will be turned into twos at the end, since a two cannot turn into a one. Of course, the result is trivial, but it gives a feel for what a Markov chain model means.

If we now consider the next transition (*Table 10.4*): the strengthening of one of the links with a previous value of '2' to a value of '3.' This transition matrix is not a regular Markov chain, since after the transition there are three categories, while there were only two before it.[94] Therefore, one is not able to study this transition in terms of this probabilistic type of theorizing.

But why not? The answer to this question brings us to an important conclusion with respect to 'emergence.' After the emergence of a '3' *as a category* the world has changed irreversibly. In terms of information theory, a phenomenon has

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[92] In the *regular* case, a Markov chain model can be shown mathematically to have a limit value for the probability distribution when $t \rightarrow \infty$, that is, the eventual outcome of a Markov process is determined by the values of the components of the transition matrix and is independent of the starting state of the process. *Irregular* Markov-chains originate from models in which no transitions between subsets are allowed, or when the development is cyclic with a probability of one.

[93] Equilibrium is reached when further multiplication with the probability row vector does not lead to another result. Therefore, at equilibrium the following equation holds:

$$v = v * P$$

(in which v is the equilibrium row vector, and P the transition matrix).

[94] Mathematically, this leads to an impossible matrix multiplication.

emerged which had a prior probability of zero, and thus the expected information content of the message that this change has occurred is infinite! Emergence of a new category is a complete surprise for the system which existed previously.

In the previous exposé, however, we defined the addition and deletion of links as interval variables. Therefore, in addition to the infinite surprise of the '3' as an emerging category, we are able to derive another (dynamic and processual) *prior* probability of the emergence of a '3' (i.e., the transition from Stage II to Stage III) given the definition of the production rule for the operator in this system, i.e., the addition or deletion of one link at a time.[95] Whether we consider the three as one further complexity of the recursive operation of adding a one, or as the emergence of a new category, makes a tremendous difference![96] If one has theoretical reasons for defining the outcome of an iteration of the operation as a newly emerging state, including a new nominal designation of the emergent phenomenon (a '3'), the world has changed irreversibly.

Note that this threshold may have been reached by means of a so-called random walk (cf. Arthur 1988). If, again for theoretical reasons, this passage of a barrier is equated with a change in the

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[95] Given a number of states an alternative calculus can be found by treating the transitions between two stages in terms of thermodynamic reactions. The inverses of the expected information values (*Table 10.2*) provide us with likelihoods of the transitions, from which reaction constants can be derived for the following equilibrium:

$$\text{Stage II} + \text{Link} \underset{k_{-1}}{\overset{k_1}{\rightleftharpoons}} \text{Stage III}$$

From here, one can extend into non-equilibrium thermodynamics. However, I pursue a probabilistic line of reasoning in order to address the problem of scenario building for series of transitions in a later section.

[96] The values are here taken as categories, while in the previous section they were used as values of a variable to indicate the strength of a network-link. However, in terms of computation, the results are similar, since normalization using grandsums of the respective matrices leads to multiplication with a constant only.

structure of the *a posteriori*–i.e. receiving–system, thereafter the operator may be attributed a different probability. In this case, we get 'absorbing barriers' in Arthur's (1988) sense: the *a posteriori* system is 'irreversibly' different from the *a priori* one.

In other words, 'emergence' and 'irreversibility' are defined in terms of the structure of the system, while 'path dependency' is defined in terms of the process, that is, the iteration of the operation. A state of the system 'emerges' with a theoretically specifiable status, while in terms of the operation of the system this emergence may only appear as an unlikely, complex recombination. 'Irreversibility' is created if the 'emergence' provides reasons for redefining the relevant universe in terms of its constituents and operative processes.[97] As soon as a new category is added, all bets are off; the probabilities may have changed and therefore have to be recalculated.[98]

However, if we have theoretical reasons for considering the three as only 'more of the same,' the system has not changed structurally. In terms of systems dynamics, the addition and deletion of links are the operations through which the growth and decline of the system develops; while when we treat the various values as descriptions of different states of the system, we focus on structure

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[97] With hindsight, we may also assume a prior probability distribution for the threes in Stage III, independently of the question of whether they eventually occur or not. If we follow a reasoning like that in the previous section about the equiprobability of yet non-existent categories, we may add a third row vector to the transition matrix with equiprobability, and therefore: $[p_1 \;\; p_2 \;\; p_3] = [1/3 \quad 1/3 \quad 1/3]$. In that case, the equilibrium probability vector is $[1 \;\; 0 \;\; 0]$, and everything would in the long run decay to an equiprobability of ones only.

More generally, the equilibrium probability vector is equal to $[1 \;\; 0 \;\; 0]$ if $p_1 > 0$. However, in our model we also specified the process so that it did not allow for the transition of a three into a one, and thus $p_1 = 0$. Therefore, we can develop Markov chain models, and even compute a equilibrium probability vector in terms of p_2 and p_3 (which is now necessarily equal to $(1 - p_2)$).

[98] See for the problem of propagation: Pearl (1988).

instead of process. Processes can be path dependent, states can be emergent, and structures may have to be irreversibly redefined in the light of newly emerging categories.

Like all subsequent transitions, the next transition (*Table 10.5*) is associated with the identity matrix, since the only transition is from one unique value ('3') to one other unique value ('4'). This distribution is the limiting case for this transition directly: if we define the emergence of a four as a category, then this again constitutes an irreversible transition.

In summary, Markov transitions become irregular in the light of emergence. In science and technology studies, however, one is particularly interested in emerging phenomena! Therefore, it is important to distinguish the various developmental patterns. On the one hand, a new category can change the world 'irreversibly.' Thereafter, one has a different system with a potentially different relation to the relevant environment.[99] On the other hand, in terms of the operative process, the definition of relevant change makes it possible to analyze processes empirically in terms of 'path dependency,' that is, in terms of different states of the same system. In the case discussed here, the occurrence of path dependency in the series coincided with the stage where one three emerged. This coincidence, however, was not a necessary one, since the two concepts have been defined independently.

10.3 CONCLUSIONS

When we discussed multivariate forecasting using information theory in Chapter Nine, the main conclusion was that a data set could be considered as a representation of a *system* if it contained within itself a better prediction of the next observation than the sum of the best forecasts based on the past development of its elements (or subsets of elements). In this chapter, this conclusion was

[99] See for the treatment of the non-occurrence of a category in one instance, while it belongs to the previously defined universe, the discussion of 'emergence' and partial decomposition in Section 8.3 above.

generalized: if we have no production rule for relevant change, the best prediction of the future behavior of a *system* is based on the information contained within the system, and not in the information contained in the history of its elements. One has no reason to expect structural change in a system if one has no (theoretically specifiable) production rule for change, just as one has no reason to expect any new phenomenon to occur in a universe when one has no *a priori* probability for it.

The meaningful specification of a production rule for change presumes historical knowledge. Historical insights provide us with the relevant factors, but the specification remains a hypothesis in an empirical design. Both qualitatively new structural *elements* in a system, and new *states* of the system may result during the process. The system may have gone through a transition without implying an easily recognizable emergence of a new category, since the new state may be based on a complex recombination of existing elements. In some cases the designation of the new organization of the system in terms of its historical significance may be problematic, while in others one may have enough historical knowledge to solve this problem.

Note the difference in the analytical steps: in a historical study, one evaluates with hindsight, that is, given the system's delineation *a posteriori* one evaluates previous stages, and then one may attribute properties to transitions. In the evolutionary approach, based on systems theory, an evaluation *ex ante* is additionally possible: the possible developments of the system in time can be predicted on the basis of heuristic assumptions concerning the system and its operation. The assumptions are based on historical analysis, while the evolutionary model is able to signal the need for an empirical reassessment. The events which occur can be used for an update of the expectations.

In other words, a 'path dependent' transition leads to a new state of the system, but what this may mean in historical terms still has to be specified. However, the outcome does not have to be given explicit meaning in historical terms in order to be useful in the abstract model. For example, in a computer simulation program one may define the reaching of certain states in one subsystem as relevant for other subsystems, e.g., when the occurrence of this event triggers a change in the boundary conditions governing the system.

The operator defines the unit of change, and thus the time scale of the process. The need to redefine the system can be considered as a consequence of the definition of the system's previous stage(s) and its operator. The question of whether a (path dependent) transition has actually taken place then becomes an empirical-analytical one, and not a question of interpretation. The ranges of possible events can be expected to be more complex than a single story can cover. The historian can provide the events with an interpretation, but the events may have an effect with or without this interpretation. The reflexive discourses provide us both with the appreciation *ex post*, and with the heuristics *ex ante*: they offer a window of interpretation on the complex dynamic systems under study.

I shall elaborate on an empirical example of testing for the emergence of a new system in the next chapter. There, I return to the questions of various forms of interpretation. However, let me first discuss two applications of this chapter's model which extend beyond the domain of science studies.

10.4 APPLICATIONS

10.4.1 Auto-amplification and Probabilistic Scenario Building

A self-organizing or 'autopoietic' system can be considered as a system which changes by means of the iterative recombination of *its* operator (e.g., Maturana 1978). In the model system above, the operator was the addition or deletion of one link at a time among six actors as structural elements. Initially, we assumed equiprobability for the operator. By the mere iteration of the operation, first, the network structure changed from two groups of three actors into one group of six actors. Second, again by iteration of the operation, probabilities were attached to configurations, in which 'double links,' 'triple links,' etc., could emerge. In this section, I show that in this model system highly unlikely developments may exhibit 'auto-amplification,' that is, when they occur, the chances for their further development increase.

For the sake of simplicity, I limit the discussion to the production rule of the possibility of the *addition* of one link at a time only, and exclude the possibility of deletion. If we now return to our initial Stage I (two separate triangles), we have the possibility either of adding a link to one of the nine ones, or adding a link to one of the six twos. In the former case, Stage II us achieved, while in the latter case the additional link is absorbed into one of the two already existing triangles, leading to what we called Stage IA. The expected information values associated with these transitions were calculated above as 23.79 mbits and 12.65 mbits, respectively. As noted, these are inverse measures of the likelihood of the transitions, and therefore we may now say:

$$p_{\mathrm{II:\,I}} : p_{\mathrm{IA:\,I}} = (9 / 23.79) : (6 / 12.65) = 44.4\% : 55.6\%$$

This 44.4 percent, however, is the total for the chances of any of the nine non-existing links to emerge, while for each specific one of these links the likelihood is only one ninth (and thus appr. 4.9%).

The next transition between Stage II and Stage III has been associated with 12.17 mbits (see *Table 10.2* above). Now, there are seven existing links which may absorb the additional link, and eight left over empty places which may become a link, leading to Stage IIA. The latter transition is associated with 22.83 mbits, and therefore:

$$p_{\mathrm{III:\,II}} : p_{\mathrm{IIA:\,II}} = (7 / 12.17) : (8 / 22.83) = 62.1\% : 37.9\%$$

The probability of the *specific* growth of the link between the two groups, however, is 62.1% / 7 = 8.9%. Thus, the further development of the emerging structure has become more likely.

The probabilities of the subsequent transitions from Stage III into Stage IV, and from Stage IV into Stage V, can be computed analogously as 12.8% and 16.7%. The further development of the specific link is not only in each stage the most likely transition for further development in terms of the information measure, but the likelihood of this development also increases further along the line of development: 4.9%, 8.9%, 12.8% and 16.7%. However, the cumulative likelihood that Stage V will be reached by this process from Stage I is extremely small:

$$p_{\mathrm{V:I}} = 0.049 \cdot 0.089 \cdot 0.128 \cdot 0.167 = 0.01\ \%$$

Thus, the emergence of Stage V by the iteration and recombination of the operator is highly unlikely in itself. However, once the development has crossed the initial barriers, it becomes increasingly probable that further development into this highly unlikely, but also highly organized state will be achieved.

In summary, the information theoretical approach has taught us how to compute the likelihood of alternative possible changes in a configuration which may retain its organization, given the 'eigen' operation of the system. Given a systematic measurement of the actor network–or equivalently, the set of characteristics in the dataset at one moment in time–and the definition of the unit of change–e.g., the addition of one network link–one is able to create a comprehensive overview of all possible future situations in probabilistic terms. As noted, this overview includes the potentially 'emerging' states.

Comparison of each of the three states (either including the previous situation or by extension to the configuration at $t + 2$) makes it possible to establish the status of each possible transition in terms of 'path dependency' (Arthur 1990). Quantitative evaluation of a line of development enables us to predict the probability of certain series of transitions in comparison with each other, and to analyze whether 'auto-amplification' is likely to occur during the process (cf. Maruyama 1963). From these results, the further empirical question arises of whether dense regions of possible future developments are discrete, and whether they can be related to alternative options (cf. Bruckner *et al*. 1994).

Since the information measures are always built up from simple summations, it is possible also to decompose results in terms of subgroups (see Chapter Nine; cf. Theil 1972). This makes it possible, in principle, to develop the dynamic apparatus in relation to the notion of hierarchically structured networks (e.g., Pattee 1973; Callon *et al.* 1983; Lee 1994): higher-level developments set dynamic boundary conditions on lower-level developments.[100] As we shall see in the next section, dynamic boundary conditions have been considered by Nelson and Winter (1977) as crucial to the relations between technological trajectories and selection environments. In our model, dynamic boundary conditions of network developments can be accounted for in terms of *conditional* probability distributions. The various ways in which these conditions can change can then be specified.

Of course, measuring these probabilities in relation to specific conditions remains a separate problem.[101] The measurement may be poor in a first-order approximation. However, an analyst can feed the results of the measurement (or the description with nominal variables) directly into the above type model, since the treatment of specifically conditioned probability distributions is strictly similar to that of probability distributions not so conditioned.

10.4.2 Co-evolution models and innovation studies

Remember the initial question of this chapter: when is change to be considered as 'structural' to a system? It was shown above that 'structure' may change in relation to 'process' in two ways: the system may cross a barrier by a random walk of the operator among possible developments, or it may go through a transition with reference to itself in previous stages. The latter change is 'self-referential,' while in the former the alternative states into which the system may become locked are set by the environment. In the self-

[100] *Vice versa*, lower level developments 'upset the movement towards equilibrium' (Nelson and Winter 1982).

[101] To this end, one may wish to build on measurement in terms of variety and information, as has been pursued for the case of technological developments notably by Saviotti and Metcalfe (1984), and Saviotti (1988).

referential case, the transition is defined only in terms of the systems process. The new stage of the system, however, may still have to be specified historically, that is, may need to be given categorical meaning, but the definition of the environment is not necessarily affected. In the case of a 'lock in,' one has additionally to specify what the transition means for the definition of the relevant environments. These specifications include constituent elements, boundaries, and the operations of the various subsystems involved.

How do developments in a system lead to redefinitions in its relations to other systems–or more generally to its environment–and how do changes in the environment affect the setting of the conditions of the system? One can now begin to envisage how to design empirical research with respect to questions concerning inter-system developments and co-evolutions. These questions are particularly relevant for technology and innovation studies, since in these specialties one studies the interface between the knowledge production and control system and the economy (cf. Weyer 1989; Luhmann 1990; Nelson 1994).

Nelson and Winter (1977) operationalized the 'selection environment' as the *dynamic boundary conditions* to (in themselves probabilistic) 'trajectories.' These authors elaborated the concepts 'technological trajectories' and 'selection environments' using Markov chain models, but they excluded *ex ante* the feedback between trajectories and environments from their models (1977, at p. 49; cf. Nelson and Winter 1982). Economic historians, however, have emphasized the *interactive nature* of the relations among technologies and markets (e.g., Rosenberg 1976). Sociologists have stressed the interdependence of variation and selection in social development processes (cf. Pinch and Bijker 1984; Van den Belt and Rip 1987), and others (e.g., Arthur 1988) have highlighted the *path-dependent* nature of technological developments. Additionally, several authors have noted an *autonomous momentum* in market developments (e.g., Schmookler 1962) or in technological developments (e.g., Winner 1977). Sahal (1981) therefore suggested that technologies can be considered as sometimes self-organizing (cf. Silverberg 1988).

As noted, a self-referential development may trigger a change in boundary conditions for another system, but this is not necessarily the case. A technological trajectory can also be considered as a self-referential process. On the other hand, 'meeting the demand in the selection environment' can be considered as an event within the selection environment. The selection environment is then considered as another (market) system (*B*) with different states. The various types of possible conditioning and interaction between the two systems can now be distinguished (see *Figure 10.8*).

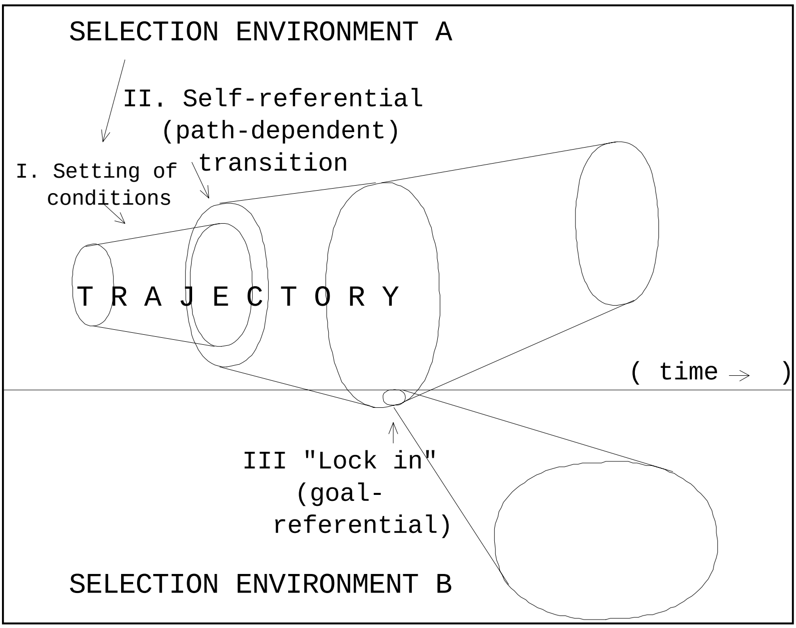

Figure 10.8
Three models for dynamic change in boundary conditions of technological trajectories

First, there is the sense that the probability distribution of a technological trajectory is conditioned *ex ante* by its prior environment (*A*). One can then raise questions concerning the extent to which the technological trajectory is dependent on or only conditioned by factors in this selection environment. Remember that at each moment in time, two variables determine each other in the mutual information and condition one another in the remaining uncertainty (see *Figure 7.1*). I shall show in Chapter Twelve that this static relation is similar between two dynamic systems, but at each moment in time.

Second, a subsystem may develop and thus change its boundaries, by using its own operator. As argued, this development of the system does not necessarily change the setting of the conditions of this system by its environment, except perhaps in the unlikely event that a transition to a new state occurs (see the previous section). The self-referential transitions can be assessed on 'path dependency' using the measures developed above. I shall provide an example using scientometric data in the next chapter.

Third, there is the case of the random walk, in which the subsystem drifts into another relation to its environment without having gone necessarily through a path dependent transition internally (Arthur 1988). However, this presupposes that the environment can relate in different ways to the system so that by chance processes the system may drift from one equilibrium into another. Then, a 'lock-in' may occur. Note that a self-referential transition in the one system may also lead to a 'lock-in': the receiving (other) system always evaluates the disturbance in terms of whether or not the signal can be recognized as a relevant signal. When the sending system changes, this may trigger (potentially fundamental) change at the receiving end.

These *three* kinds of dynamics correspond to three existing models of the innovation process, if the latter is conceptualized as a result of possible interactions between economic demand and technological development. Demand can be specified either in reaction to technological options which have become available thanks to path-dependent technological developments ('technology push'; indicated as mechanism II in *Figure 10.8*), or it can be specified (analytically) independently, e.g., in the economy. In the latter case, the specified demand may have been driving the relevant technological activities, and thus have been part of the *ex ante* conditions of the operation of the system ('demand pull'; mechanism I), or it may have been 'met' by technological developments which were not necessarily conditionalized as a function of this demand (mechanism III). In this latter case, the system has drifted into a 'lock-in.'

The three mechanisms are based on different assumptions. While internal change requires specification of a transition matrix for one system, understanding a 'lock-in' requires specification of the relevant thresholds in the other system, that is, in the environment. In the case of technological developments, these latter thresholds are determined by dynamic demand functions: when does a technological system generate a signal that resonates in a market? However, these demand functions have then to be specified, since otherwise one would have no criterion for assessing whether or not 'demand' was met (as a threshold).

Note the change in analytical perspective from the *ex ante* specification of boundary conditions on technological developments in the model proposed by Nelson and Winter (1982). In the case of a lock-in the analyst specifies what the market (or the selection environment) is expected to consider as a significant signal from the side of the technological system, and not what the latter is to consider as its condition by the former.

As a fourth mechanism the new state can also be a consequence of longer-term adjustments between the two systems (trajectory x and environment y), instead of the passage of a threshold at a specific moment in time. When this occurs, one obtains a co-evolution model between specific technologies and selection environments (cf. Nelson 1994). Co-evolution, however, requires gradual stabilization of the interaction term (xy) between the systems x and y into a super-system that can feed back on both variation (e.g., x) and selection (e.g., y). Upon stabilization of the co-variation between x and y, the interaction term can be considered as a third independent variable ($z = f_{(xy)}$), of which the path-dependent transitions in relation to x and/or y can be described at the next-higher level, but in similar terms.

The operations are recursive, but the higher-order operation requires one more degree of freedom than the lower-level one. Thus, the emerging variable (z) can only co-vary as an independent source of variation with one of the other (sub-)systems *x* and *y*, if this third axis has previously reached a position orthogonal to the ones that contributed to its construction. This third degree of freedom, however, corresponds to a third system of reference that should then be provided also with a theoretical appreciation.

In other words, developments in otherwise orthogonal dimensions can co-vary, and the system can gain in complexity if this co-variation can be stabilized and internalized as an additional dimension of the system. For example, the interactions between technological trajectories and economic environments can be conditioned by organizational factors (Van den Belt & Rip 1987). These institutional dynamics can be carried by an entrepreneur (e.g., Hughes 1987), a sector (e.g., Abernathy and Clark 1985; Nelson 1982; Pavitt 1984), or an interorganizational network (e.g., Clark 1985; Shrum 1985). If this third context is additionally incorporated, this can lead to the co-evolution of a trajectory and an environment into a global system. Both a technological regime and a scientific paradigm can be considered as such global systems, that is, systems at a next-order level.

10.4.3 Implications for firm behaviour and institutional agency

Trajectories can be stabilized in three dimensions, and can therefore be observed in terms of firm behaviour or institutional agency. The technological regime or the scientific paradigm, however, integrates over time three (nearly decomposable) dynamics into a hyper-dynamics that can no longer be fully understood by using geometrical metaphors. While understanding variables as fluxes, the algorithmic approach of entropy statistics enables us to distinguish between change in the value of variables and change in the categories themselves (Chapter Eight).

Discursive theories specify sub-cybernetics by stabilizing a specific reflection on the observed system in the scholarly communication (Hinton *et al.* 1986; Langton 1992, pp. 22 ff.). Each discourse offers a window of appreciation on the more complex dynamic systems under study. I shall now consider the window of reflexive agency in relation to the interaction among the two dynamic systems specified above as the technological trajectory x and the selection environment y.

In order to specify the implications for firm behaviour, one has to disaggregate the distributed context of firm behaviour, and to examine the possible reflections of the interaction between the other two sub-dynamics (trajectories and environments) on this third dimension. Firms differ among them in terms of their position. Therefore, the choice of each focus raises empirical questions about the range of possible reflections. These empirical questions go beyond the scope of the present study (e.g., Pavitt 1984; Faulkner and Senker 1994). However, we are able to indicate which patterns of firm behaviour one expects on the basis of the argument.

For analytical reasons subsequent dynamic redefinitions of the relations between two independent variables have to be generated on the basis of developments in either system or on the basis of their interaction. In formula format:

$$F(\mathrm{x,y})_{t+1} = a\mathrm{x}_t^{\alpha} + b\mathrm{y}_t^{\beta} + c(\mathrm{x}_t^{\gamma}.\mathrm{y}_t^{\delta})$$

Two independent variables (x and y) are assumed that can at a a next moment (that is, at $t+1$) both have interacted ($x_t^{\gamma}.y_t^{\delta}$), and/or have built on their previous states (x^{α} or y^{β}). However, one can only build on a previous state, *if* these systems had previously been stabilized. Both independent variables may thus represent the stabilization of previous developmental processes ('continuity') and/or contingent 'change.'[102]

Three types of interactions are then conceivable: first, both independent variables have been contingent; second, one independent variable is contingent while the other represents a (relative) stabilization of prior developmental processes; or third, both independent variables represent a stabilization of prior

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[102] The relation between auto-correlation and change provides us with options for testing the stability of the system.

developments. Let me specify the expected developments (see Blauwhof 1995):

ad 1. When both independent variables are contingent, the processes characterizing the development are those of variation and stabilization, since there has been no structural basis for selection. In view of its path-dependent nature the (stochastic) variation will eventually be locked into a stabilization (Arthur 1988). At the organizational level, one expects in this case a flexible organization which takes its chances for system building, and hence pushes for stabilization. The dominant metaphor is that of an engineer or entrepreneur who constructs the system.

ad 2. When both contingent and previously stabilized variables are involved in the interaction, the processes to be expected are those of variation (introduced by the contingent variable), selection (introduced by the stabilization of prior development processes into structures), and stabilization. Furthermore, since the previously stabilized factor–by definition–refers to its previous state, feedback loops characterize the dynamics and introduce, depending on the sign, either self-amplification or self-dampening (towards equilibrium).

More specifically, the alteration in the sign of the feedback is expected to lead the life-cycle of the technology (cf. Abernathy and Clark 1985). The organizational emphasis can be expected to be on innovation in the upswings, and on cash-flow in the down-swings. Therefore, one expects a more complex configuration with diversified profit centres in order to profit fully from these alterations. The dominant focus of scholarly attention is in this case on the industry.

ad 3. When both independent variables represent prior stabilizations, the relevant processes are, theoretically, those of selection and stabilization. In this case, the introduction

> of contingency requires an (R&D) organization as a third dimension in which one is able to generate 'newness' from interactions among selections made on either side. For example, Nelson and Winter's (1982) 'search and selection processes' contain such dual selections. The question of stabilization then leads to the question of whether, and for how long, this interface can be sustained. The repetition of the co-variation can lead to co-evolution if the specific interactions can be stabilized.

Note that these analytically distinguished processes do not exclude one another, but one expects the various cycles to be of a different order. The empirical assessment of the relevant contexts in terms of the stability of their operation is therefore of prime importance in model building which seeks to provide normative advice (e.g., Dosi 1991; Brunner 1994).

Although the reasoning was elaborated here in the categories of technology studies, the application to other inter-system dependencies is straightforward. For example, the 'internalist approach' in the scholarly tradition of history of science focuses on developments within the science system, while the 'externalists' emphasize that the system may have to meet different requirements from its environments. In the internalist approach the externalities may be accounted for in the conditional part of the *a priori* probability distribution of the system, but the operation of the system itself remains the revolutionizing agent; while in the externalist approach additionally the system's environment may change independently. The function of reflexive and institutional agency can be specified, *mutatis mutandis*.

In relation to the original scheme of a multi-dimensional space of texts, authors, and cognitions (*Figure 1.1*; see also the title page), reflexive agency can be considered as prerequisite for the *re*cognition. Whereas the texts and the scholars provide relevant environments for each other as relational systems, the third dynamics of cognition and recognition propels this complex into the controled organization and production of scientific knowledge under evolutionarily specifiable conditions. (See also Leydesdorff 2001, at http://www.upublish.com/books/leydesdorff.htm.)

PART III

COMMUNICATION, PROBABILISTIC ENTROPY, AND SELF-ORGANIZATION

The methodological chapters in Part Two showed that some major problems of data analysis in science studies can be solved if taken seriously and addressed with sufficient methodological rigor. Remember that a number of requirements for methods in science studies were formulated in Chapter Six on the basis of the theoretical analysis in Part One. In Chapter Seven, we then focused on *heterogeneity*, both in terms of categories and in relation to levels of aggregation. By using information theory, the *multi-level* problem could be reduced to a multi-variate problem, since aggregation implies the use of a grouping rule. This problem was elaborated in Chapter Nine where the relations between various forms of multi-variate analysis were discussed.

In Chapter Eight, the focus was on the longitudinal *reconstruction* of multi-layered developments. Using word occurrences at different levels of aggregation, it could be shown how and why the reconstructions differed when structural factors (e.g., sections) or perspectives over time varied. Note that these problems of data analysis are independent of the type of measurement or the measurement scale. The focus here has been on data analysis, and not on data gathering.

In general, the dynamic measurement of previous events enables us to specify an expectation with respect to future events, and thus systematically to learn from occurrences of (unexpected) events. In the final section of Chapter Eight this mechanism of self-referential, that is, reflexive, processing was elaborated into the perspective of building scientific expert systems on the basis of scientific texts. In principle, the evaluation of newly submitted texts provides us with an update for the weight factors of the dimensions which were used for the construction of the system.

Chapter Nine synthesized and generalized the insights gained from the two model studies of texts for use with other *network data*. Systems of complex data can be compared at different moments in time, and then be studied in terms of Markov chains. Path-dependent *transitions and irreversibilities* can be defined operationally, and then analyzed independently of the measurement scale or the dimensionality of the data. In Chapter Ten, the focus changed to the *agent of change*, that is, the operator. If the unit of change can be specified, for example on the basis of historical analysis, this provides us with a range of expectations on the basis of the current state of the system(s) under study. The developments are not equiprobable, and subsequent events may change the probability distributions.

In general, empirical situations have a previous state. Thus, change and development can be caused by two types of incoming arrows: one from the various contexts which condition and disturb the system under study, and one from previous state(s). As long as there is no system, contexts prevail. But then the question arises whether and when the (perhaps incidental) interaction terms can be considered as an emerging system. As soon as the contexts are no longer juxtaposed in the aggregation but also interacting, the emerging network provides an opportunity for co-evolution by repeating the co-variation. In Chapter Ten we discussed these mechanisms in terms of a model system. The formal mechanisms for emergence, irreversibility, path-dependency, auto-amplification, and co-evolution in and between network systems could be specified.

In my opinion, this completes the development of an *information calculus* which complies with the criteria specified in Chapter Six. The remaining task is the theoretical application. Can the fruitfulness of the approach be illustrated by applying it to other data? Does this lead to new insights and new questions? I indicated some of the applications of these methods in domains other than science studies (e.g., technology and innovation studies) in the last sections of the previous chapter. In this part, I return to science studies by focusing on a number of urgent questions in this domain. These examples will subsequently be elaborated as a *science policy* question (Chapter Eleven), in relation to *knowledge representations* and issues in the *philosophy of science* (Chapter Twelve), and with reference to issues in the *sociology of science* (Chapter Thirteen). Chapter Thirteen can be considered as a summary of the findings of the study.

In Chapter Eleven, information measures are used to study the question of whether the large R&D programs of the European Commission have induced the emergence of a transnational publication system at the level of the European Union. Publication data are measured using standard scientometric techniques. In Chapter Twelve, the development of systems, on the basis of fluxes of probabilistic entropy generated in their interaction with systems in their environment, is discussed in relation to models for belief update in computer science and in the philosophy of science. By evaluating Bayes' Rule in an information theoretical framework, the iterative update of belief systems, and more generally structure/action contingencies, can be modelled. This algorithm will be applied to the same citation networks as analyzed in Chapter Nine.

Iterative updates contain self-referential loops which may exhibit self-organizing characteristics. As noted in Part One, this combination of perspectives from communication theory, information theory, and self-organization theory is not incidental. I shall argue that the study of scientific communications makes it possible to understand reflexivity in human communication reflexively without becoming confused. In scientific communications the reflexivity in human communication is to a large extent codified: the study of the dynamics among texts in the formal scientific literature made it necessary to distinguish between the changing meaning of words and their co-occurrences, and the information content of the distributions. By generalizing these conclusions to other domains, one is able to infer the reformulation of sociology as a positive science of reflexive communication systems from the mathematical theory of communication. The relevance for existing programs in the sociology of scientific knowledge can be specified, and perspectives for further research are considered.

Chapter 11

The Impact of EC Policies on the Transnational Publication System

In order to legitimate its intervention in science policy at the supra-national level, the European Commission is in need of instruments to demonstrate the positive effects of its funding in helping to create a unified Europe. Since science and technology policies are key factors in the Community's interventions, these questions are also raised in connection with the evaluation of the EC's large R&D programs.

Can one assess the impact of this intervention quantitatively by using scientometric methods? A number of projects have been initiated on behalf of the Commission in order to study this problem. With hindsight the various results indicate that the following research questions should be distinguished:

1. Is it possible to *measure* national performance in terms of scientific publications, and if so, how should one then define the European level?

2. How should the *process* of 'forming one Europe' be operationalized in relation to the descriptive statistics? Can an emerging network be discerned in the data?

3. If this network can be made visible, how is the *role* of the Commission's transnational research effort to be assessed in relation to this development?

In the course of the last decade, scientometricians have reached some agreement about the measurement problems related to the first question. More recently, the focus has shifted away from descriptive statistics and towards the testing of significance. However, an explanatory perspective requires the formulation of a theoretically informed expectation, against which the observed data can be tested. The concept of an emerging network at a higher (e.g., European) level of aggregation refers to systems theory. But how can the wealth of scientometric observations be integrated with the systems perspective?

The question of the *emergence* of a higher-level order in relation to dynamic changes in lower-level distributions is studied in non-linear dynamics. The scientometric data can be measured as lower-level distributions at various moments in time; this measurement provides us with an observational domain for the application of the probabilistic entropy measures which can be deployed within this theoretical framework.

The conclusions will be that the EC does not function currently as a single publication system, but the development of international co-authorship relations among member countries does exhibit a systematic character. However, one would still like to know *when and in which dimensions* this system emerged. Irreversible transitions can be shown exclusively in terms of co-authored *articles* (as opposed to reviews, letters, notes, etc.). The possible effects of policy intervention in such a process are then discussed.

11.1 THE MEASUREMENT OF PUBLICATION PERFORMANCE

The main database for scientometric evaluations, the *Science Citation Index*, has been organized primarily as an aggregated representation of all the sciences at the international level. However, it has proved particularly useful in national science policy, since the author addresses made possible bibliometric evaluations among institutions and nations, and therefore it allowed the strengths and weaknesses of national research systems to be further specified in terms of numbers of publications and citations (e.g., Narin and Carpenter 1975; Moed *et al.* 1985; Irvine *et al.* 1985; Schubert *et al.* 1989). Rather early in the development of the scientometrics program this led to an understanding of the aggregate of publications in an analytical grid of nations versus specialties (Narin 1976).

The dynamic decomposition of the *SCI* in terms of specialties is still a research problem in scientometrics (e.g., Tijssen 1993; Leydesdorff and Cozzens 1993), but the precise disaggregation among *nations* using institutional addresses seems now warranted for publication data (Anderson *et al*. 1988; Moed 1988; Leydesdorff 1989d). Furthermore, researchers using the *NSF/CHI Science Literature Database* have advocated using only articles, notes and reviews for the comparison among nations, while Braun *et al.* (1989) have argued in favor of the addition of letters to this subset (cf. Martin 1991; Braun *et al.* 1991; Martin 1994).

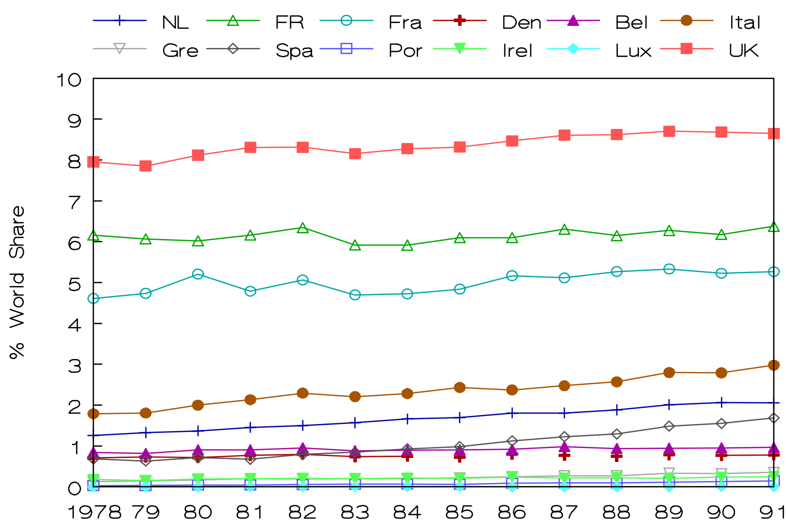

Figure 11.1
World Share of Publications for 12 EC Member States

In practice, these are technical problems of disaggregation which can be solved. For example, *Table 11.1* shows both the aggregated data and–in italics–the so-called *CHI-indicator* for only articles, reviews and notes. The data is expressed as a percentage share of respective publications contained in the *Science Citation Index*. In addition to data for the twelve EC countries, data for the US is also given for the information of the reader. *Figure 11.1* visualizes the development in this data for the 12 EC countries over the period 1978-1991.

The measurement of internationally co-authored papers has raised another problem: using the noted analytical grid, it seemed logical to distribute internationally co-authored publications over the contributing countries (in order to keep the sumtotal at one hundred percent). However, one may argue that such 'fractional counting' would make a country's national publication share decline if its scientists took part increasingly in international coauthorships *ceteris paribus* (Anderson *et al.* 1988; Leydesdorff 1988; Nederhof and Moed 1993; Martin 1994). The question of how to attribute internationally co-authored articles to each national percentage share of publications has therefore been an issue in methodological discussions of the measurements.

My argument in this debate has been that one should distinguish between international co-authorship as an indicator of a trans-national network and national publication performance as an attribute of each of the countries (Leydesdorff 1991b). Accordingly, *Table 11.2* shows the data for international co-authorship among EC countries in a format like that of *Table 11.1*. The data exhibits a spectacular increase for this indicator during the period under study. The strong similarities among the increases for some countries suggest the growth of a single system, which is reflected in this data (see *Figure 11.2*).

	1978	1979	1980	1981	1982	1983	1984	1985	1986	1987	1988	1989	1990	1991	(1992)
UK	7.952	7.852	8.118	8.309	8.312	8.154	8.278	8.318	8.469	8.606	8.621	8.700	8.685	8.649	8.624
	8.002	*7.985*	*8.081*	*8.168*	*8.210*	*8.067*	*7.937*	*8.155*	*8.168*	*8.266*	*7.988*	*7.971*	*8.159*	*8.254*	*8.399*
FRG	6.161	6.068	6.019	6.161	6.347	5.919	5.916	6.098	6.099	6.306	6.149	6.278	6.178	6.376	6.435
	6.819	*6.653*	*6.497*	*6.591*	*6.704*	*6.370*	*6.293*	*6.628*	*6.610*	*6.710*	*6.593*	*6.810*	*6.903*	*7.047*	*7.169*
FRANCE	4.611	4.734	5.209	4.789	5.064	4.698	4.725	4.838	5.169	5.115	5.272	5.332	5.227	5.266	5.264
	5.169	*5.433*	*5.622*	*5.365*	*5.422*	*5.154*	*5.201*	*5.242*	*5.500*	*5.523*	*5.498*	*5.657*	*5.726*	*5.747*	*5.793*
ITALY	1.791	1.807	2.000	2.135	2.295	2.207	2.284	2.431	2.371	2.476	2.570	2.799	2.792	2.978	3.096
	2.033	*2.071*	*2.178*	*2.236*	*2.346*	*2.407*	*2.521*	*2.510*	*2.578*	*2.674*	*2.837*	*2.991*	*3.069*	*3.235*	*3.355*
NL	1.259	1.328	1.368	1.456	1.503	1.570	1.663	1.696	1.809	1.811	1.883	2.013	2.066	2.058	2.134
	1.385	*1.468*	*1.495*	*1.585*	*1.604*	*1.670*	*1.772*	*1.812*	*1.861*	*1.928*	*1.969*	*2.115*	*2.198*	*2.193*	*2.271*
SPAIN	0.683	0.633	0.714	0.675	0.797	0.847	0.929	0.989	1.127	1.230	1.293	1.487	1.553	1.686	1.780
	0.805	*0.800*	*0.850*	*0.798*	*0.899*	*0.966*	*1.017*	*1.111*	*1.295*	*1.392*	*1.459*	*1.512*	*1.654*	*1.742*	*1.848*
BELGIUM	0.845	0.819	0.905	0.906	0.954	0.884	0.895	0.905	0.918	0.987	0.937	0.945	0.953	0.965	0.974
	0.901	*0.909*	*0.904*	*0.954*	*0.979*	*0.919*	*0.939*	*0.957*	*0.957*	*0.996*	*0.950*	*1.005*	*1.042*	*1.046*	*1.067*
DENMARK	0.706	0.734	0.718	0.771	0.797	0.739	0.749	0.728	0.801	0.774	0.746	0.787	0.771	0.782	0.780
	0.795	*0.836*	*0.797*	*0.863*	*0.864*	*0.824*	*0.804*	*0.814*	*0.857*	*0.836*	*0.808*	*0.847*	*0.852*	*0.860*	*0.867*
GRECE	0.134	0.142	0.171	0.199	0.194	0.201	0.198	0.223	0.251	0.274	0.272	0.335	0.322	0.357	0.384
	0.156	*0.166*	*0.192*	*0.222*	*0.233*	*0.240*	*0.236*	*0.256*	*0.299*	*0.321*	*0.322*	*0.368*	*0.355*	*0.404*	*0.434*
IRELAND	0.181	0.155	0.193	0.201	0.209	0.200	0.213	0.206	0.242	0.216	0.224	0.211	0.238	0.247	0.267
	0.175	*0.161*	*0.193*	*0.190*	*0.199*	*0.202*	*0.199*	*0.181*	*0.205*	*0.212*	*0.210*	*0.202*	*0.231*	*0.227*	*0.231*
PORTUGAL	0.032	0.034	0.046	0.046	0.057	0.064	0.070	0.062	0.088	0.095	0.094	0.116	0.135	0.146	0.164
	0.037	*0.041*	*0.051*	*0.053*	*0.064*	*0.068*	*0.069*	*0.071*	*0.092*	*0.099*	*0.110*	*0.130*	*0.153*	*0.164*	*0.184*
LUXEMB	0.001	0.003	0.003	0.005	0.005	0.005	0.004	0.004	0.003	0.003	0.002	0.003	0.006	0.004	0.004
	0.001	*0.004*	*0.003*	*0.005*	*0.005*	*0.006*	*0.005*	*0.004*	*0.003*	*0.004*	*0.003*	*0.004*	*0.006*	*0.004*	*0.004*

EC	23.874	23.794	24.852	24.986	25.788	24.754	25.110	25.661	26.444	26.883	26.981	27.697	27.458	27.919	28.338
	25.734	*25.926*	*26.180*	*26.271*	*26.682*	*26.043*	*26.057*	*26.764*	*27.363*	*27.783*	*27.516*	*28.183*	*28.641*	*29.147*	*29.642*
USA	37.320	37.314	36.623	37.084	37.087	36.134	35.730	35.799	35.637	36.183	35.902	34.715	36.737	36.622	36.568
	33.504	*34.163*	*34.277*	*34.624*	*35.462*	*34.492*	*34.750*	*34.573*	*34.553*	*34.519*	*34.630*	*34.763*	*35.264*	*35.707*	*36.188*

Table 11.1
Publication Performance as a Percentage of World Share of Publications
(CHI-indicator in italics: only Articles, Reviews and Notes)

	1978	1979	1980	1981	1982	1983	1984	1985	1986	1987	1988	1989	1990	1991
UK	0.215	0.233	0.276	0.300	0.325	0.326	0.350	0.348	0.363	0.400	0.430	0.499	0.586	0.614
	0.235	*0.267*	*0.307*	*0.338*	*0.361*	*0.365*	*0.395*	*0.398*	*0.415*	*0.457*	*0.470*	*0.533*	*0.638*	*0.675*
FRG	0.164	0.185	0.217	0.237	0.271	0.257	0.281	0.302	0.320	0.352	0.381	0.464	0.500	0.540
	0.189	*0.217*	*0.246*	*0.272*	*0.303*	*0.296*	*0.318*	*0.354*	*0.373*	*0.413*	*0.435*	*0.513*	*0.566*	*0.602*
FRANCE	0.193	0.203	0.244	0.258	0.288	0.276	0.301	0.314	0.331	0.366	0.400	0.490	0.509	0.575
	0.227	*0.240*	*0.274*	*0.294*	*0.333*	*0.324*	*0.354*	*0.371*	*0.399*	*0.432*	*0.464*	*0.541*	*0.573*	*0.647*
ITALY	0.092	0.096	0.114	0.135	0.153	0.158	0.178	0.178	0.192	0.216	0.234	0.285	0.310	0.342
	0.106	*0.115*	*0.129*	*0.151*	*0.177*	*0.187*	*0.212*	*0.213*	*0.232*	*0.258*	*0.271*	*0.311*	*0.351*	*0.383*
NL	0.087	0.088	0.109	0.118	0.131	0.133	0.149	0.155	0.161	0.179	0.198	0.246	0.268	0.290
	0.097	*0.101*	*0.118*	*0.135*	*0.144*	*0.150*	*0.167*	*0.172*	*0.182*	*0.202*	*0.220*	*0.261*	*0.294*	*0.316*

SPAIN	0.018	0.028	0.034	0.036	0.045	0.045	0.059	0.061	0.088	0.104	0.118	0.158	0.186	0.221
	0.022	*0.034*	*0.040*	*0.046*	*0.055*	*0.057*	*0.072*	*0.077*	*0.113*	*0.128*	*0.143*	*0.175*	*0.217*	*0.255*
BELGIUM	0.072	0.078	0.091	0.099	0.105	0.103	0.107	0.123	0.126	0.142	0.145	0.173	0.185	0.205
	0.082	*0.092*	*0.096*	*0.114*	*0.113*	*0.117*	*0.121*	*0.137*	*0.144*	*0.156*	*0.162*	*0.189*	*0.208*	*0.231*
DENMARK	0.045	0.045	0.052	0.054	0.064	0.060	0.058	0.066	0.065	0.074	0.070	0.080	0.100	0.108
	0.050	*0.055*	*0.058*	*0.062*	*0.074*	*0.070*	*0.065*	*0.076*	*0.077*	*0.089*	*0.081*	*0.087*	*0.112*	*0.118*
GRECE	0.011	0.012	0.014	0.018	0.018	0.019	0.024	0.025	0.031	0.032	0.036	0.046	0.050	0.060
	0.013	*0.014*	*0.016*	*0.021*	*0.023*	*0.022*	*0.029*	*0.031*	*0.039*	*0.039*	*0.044*	*0.052*	*0.058*	*0.068*
IRELAND	0.013	0.018	0.020	0.020	0.022	0.023	0.025	0.023	0.028	0.027	0.032	0.037	0.046	0.048
	0.014	*0.020*	*0.025*	*0.022*	*0.024*	*0.026*	*0.028*	*0.025*	*0.029*	*0.032*	*0.033*	*0.040*	*0.047*	*0.053*
PORTUGAL	0.007	0.006	0.008	0.008	0.013	0.012	0.012	0.014	0.018	0.021	0.022	0.029	0.040	0.043
	0.008	*0.008*	*0.011*	*0.010*	*0.017*	*0.016*	*0.016*	*0.019*	*0.022*	*0.026*	*0.028*	*0.033*	*0.047*	*0.048*
LUXEMBOURG	0.001	0.001	0.001	0.002	0.001	0.002	0.001	0.002	0.001	0.001	0.001	0.001	0.002	0.002
	0.000	*0.001*	*0.001*	*0.002*	*0.001*	*0.001*	*0.001*	*0.002*	*0.001*	*0.002*	*0.001*	*0.001*	*0.002*	*0.002*
(SUM)	0.918	0.993	1.181	1.286	1.437	1.414	1.545	1.610	1.725	1.914	2.068	2.509	2.781	3.047
	1.043	*1.163*	*1.321*	*1.467*	*1.625*	*1.632*	*1.778*	*1.874*	*2.026*	*2.234*	*2.351*	*2.738*	*3.113*	*3.398*

Table 11.2
In Between Membership Co-authorships as a Percentage of World Share of Publications
(CHI-indicator in italics: only Articles, Reviews and Notes)

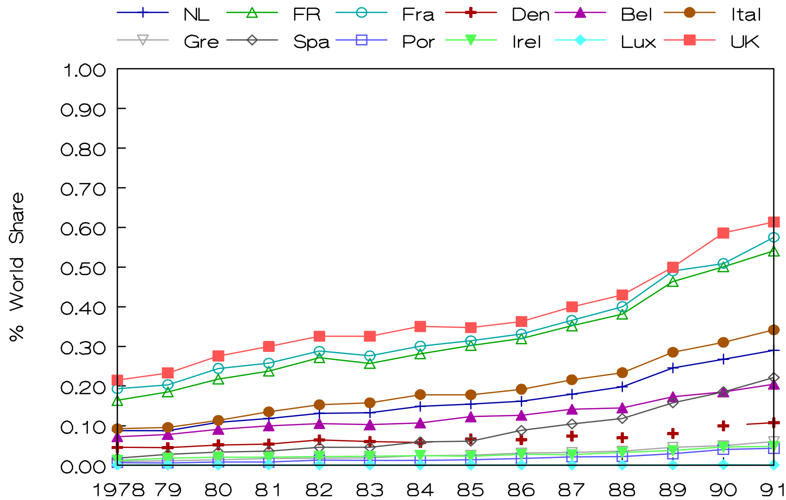

Figure 11.2
In Between Membership Co-authorships as a Percentage of World Share of Publications

In an exploratory study Lewison and Cunningham (1989) used the indicator of international co-authorships to investigate whether EC-funded programs led to a significant increase in the number of publications with author addresses in more than one EC country over publications in the same specialties from groups which had not received EC money, and whether these co-authored papers were received differently in terms of citations.[103] Their main conclusion was that papers from EC-funded projects share in the overall visibility effects of trans-nationally co-authored publications, and that they 'peak earlier' in the citation distribution in some cases, but not in all. These authors suggested that the latter effect could be a consequence of the fast dissemination of results within an EC program.

On the basis of Lewison and Cunningham's (1989) exploratory study, the European Commission commissioned a series of projects

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[103] See also, among others: Pendlebury 1989; Schubert and Braun 1990; Schott 1991; Luukkonen *et al.* 1992; Leclerc *et al.* 1992.

on the subject of trans-nationally co-authored publications. These studies showed, among other things, that on average, internationally co-authored papers, independently of their EC funding, had a significantly higher citation impact (Narin and Whitlow 1990; Narin *et al.* 1991). This result, however, did not yet address the original question of whether one can use this indicator to show the emergence of an increasing internationalization in publication behavior among European scientists, and if so, the extent to which this development can be attributed to Community programs. The concept of 'Europeanization' introduces a systems perspective which cannot be clarified by measurement and descriptive statistics alone.

In summary, the discussion about measurement problems for national publication systems has led to a focus on internationally co-authored publications as a possible indicator of a transnational network. Citation studies of these papers have indicated that they constitute a specific subgroup with significantly higher impact in terms of citations. The evidence that this might be a direct effect of EC intervention is meager.

11.2 DISTRIBUTIONS AND SYSTEMS

The crucial problem with scientometric information is that it remains in need of interpretation. Part of the message of scientometrics has been that quantitative information can be analyzed using databases that may be useful for feedback into the policy process. However, when the information is uncertain and/or complex, the qualitative interpretation can no longer be based on visual inspection of the data and/or common-sense agreement *ex post*, but must be warranted by the specification of a hypothesis *ex ante*. Observational data enables us to distinguish among the different models if the models lead to different predictions.

Indeed, the choice of whether one models the European countries as separate publication systems or as a single publication system will result in different predictions of whether indicators for these nations co-vary when changing over time. A system contains a specific organization of the data, and it is a system precisely insofar as it is able to maintain this structure. Change is expected to occur at the systems level, while without the hypothesis of a system, the individual elements are expected to change independently. In other words, these two alternative hypotheses enable us to specify relevant information with respect to expected future behavior: in a system, the history of the lower-level elements will no longer be of relevance for the prediction at the level of the system itself; the system is then developing a history of its own.

On the one hand, the historical trendline for each individual element contains an expectation for its next value (see Chapter Nine). On the other hand, the current state of the system as a multi-dimensional construct contains a better prediction of its future state than can be constructed from the history of its individual elements.[104] In summary, the hypothesis of a higher order (e.g., European) system allows us to make two best forecasts: one on the basis of the values of each individual element of the system, and another for the system as a whole, i.e., on the basis of the previous year's distribution. By comparing these forecasts with actual values, one can in principle develop a test to distinguish whether or not these elements developed as a single system.

In Chapter Nine, it was shown that the value of an indicator *F* for the year *n+1* can be predicted using the following function of the value of the same indicator in the years *m* to *n*:[105]

$$F_{n+1} = \left\{ \frac{(\Sigma_m^n F_i) - F_m}{(\Sigma_m^n F_i) - F_n} \right\} \cdot F_n$$

[104] In systems analysis this is also known as the *Markov* property of a system. A system has the first order *Markov* property if the current state of the system is the best predictor for its future state. If one takes more previous states of the system into account–as we shall do in a later section–this raises the order of the *Markov* chain. However, the strongest test to be developed below is based on the current state of the system only.

[105] Instead of using the values F_i one could use the moving averages during a certain number of years in order to smooth out disturbances in the values for various years.

It was also shown that the extension of this formula to multi-variate prediction leads to the noted *Markov* property, i.e., to the prediction that the current state of the system contains the best prediction of the distribution in its next state.

Let us now confront these two predictions with the observed data for national and European performance as described above. Does the percentage of world share of publications of the EC countries exhibit a systemic character at the supra-national level? Second, what about the proposed network indicator of internationally co-authored publications within the Community?

11.2.1 Does the EC develop as a single publication system?

The 1978-1987 period provides us with a ten-year time series, which we can use as input in the above formula, then make a best estimate for 1988, and compare this prediction with the alternative prediction on the basis of the distribution of the data in the previous year (1987) only. The observed values for 1988 can be used as a first test of the hypothesis of the development of a system in the data. This provisional conclusion can subsequently be tested against the data for the years following 1988.

One complication is that the best prediction for a uni-variate series is not necessarily based on the whole time-series if there was, for example, a reversal in the trend. The choice of a first year of the time series implies always an additional hypothesis. For this reason, I analyzed the series for each consecutive year as the first year, and I selected the prediction which minimized the addition to the expected information content of the message that a value for the year *n+1* was predicted. Remember that an observation contains no information if it corresponds to the expectation, so that the best prediction is thus associated with the lowest expected information content.

	1978 . . . 1987 Actual	1988 Prediction	1988 Actual
UK	7.95 . . . . 8.61	8.75 (1985)	8.63
FRG	6.16 . . . . 6.31	6.41 (1983)	6.15
France	4.61 . . . . 5.12	5.22 (1983)	5.27
Italy	1.79 . . . . 2.48	2.56 (1978)	2.57
Netherl.	1.26 . . . . 1.81	1.88 (1978)	1.88
Spain	0.68 . . . . 1.23	1.34 (1982)	1.29
Belgium	0.85 . . . . 0.99	1.01 (1983)	0.94
Denmark	0.71 . . . . 0.77	0.78 (1978)	0.75
GrECe	0.13 . . . . 0.27	0.30 (1984)	0.27
Ireland	0.18 . . . . 0.22	0.22 (1980)	0.22
Portugal	0.03 . . . . 0.09	0.11 (1978)	0.09
Luxemb.	0.00 . . . . 0.00	0.00 (1984)	0.00
EC	23.87 . . . 26.88		26.98

Table 11.3
Predictions for 1988 compared with actual 1988
(all figures as % world share of publications)

Table 11.3 exhibits the results of the analysis for the twelve EC countries. All values are given as percentages of world share in order to promote intuitive understanding. The figures are based on the top lines in *Table 11.1* (that is, for the aggregates; all analyses were additionally checked using the CHI-indicator). The predictions for 1988 in the third column are based on the time series from the years indicated in the fourth column up through 1987. *Table 11.4* shows both the Pearson correlations among these distributions and the expected information contents of the differences in millibits of information. The information measure is more sensitive to differences than, for example, the Pearson correlation among the distributions, since the latter tests primarily for the significance of similarities.

Pearson correlations

	1988 actual 1987 actual	1988 actual 1988 predicted
EC	0.99963	0.99971
EC - Luxembourg	0.99960	0.99969
EC - UK	0.99927	0.99944

***I* in mbits**

a posteriori *a priori*	1988 actual 1987 actual	1988 actual 1988 predicted
	0.54	0.45
EC - Luxembourg	0.54	0.45
EC - UK	0.78	0.64

Table 11.4
Pearson correlations and dissimilarities in millibits

The distributions for subsequent years are obviously highly correlated. However, the difference between the observed distribution for 1988 and the one for 1987 corresponds to 0.54 millibits of information, and is therefore 20% larger than that between observed 1988 and the best predictions on the basis of the time-series for each country separately. Therefore, we have to reject the hypothesis that the EC develops as a single publication system in terms of an aggregated world share of publications.[106]

[106] Since one may wish to argue that Luxembourg or the UK should be left out of consideration in assessing the 'European' system, the values for these situations are also provided. However, these corrections do not affect the conclusion.

11.2.2 Between member-state co-authorships

Let us now apply the same methods to the development of 'between member-state co-authorships.' As noted, in contrast to the number of publications, this indicator suggests by its nature a type of network relation. *Figure 11.2* can be more easily interpreted upon visual inspection as a representation of a systematic development than the pattern depicted in *Figure 11.1*.

	1978 . . . 1987 Actual	1988 Prediction	1988 Actual
UK	0.215 . . . 0.400	0.429 (1985)	0.430
FRG	0.164 . . . 0.352	0.380 (1983)	0.381
France	0.193 . . . 0.366	0.392 (1983)	0.400
Italy	0.092 . . . 0.216	0.242 (1985)	0.234
Netherlands	0.087 . . . 0.179	0.192 (1980)	0.198
Spain	0.018 . . . 0.104	0.120 (1979)	0.118
Belgium	0.072 . . . 0.142	0.153 (1978)	0.145
Denmark	0.045 . . . 0.074	0.078 (1983)	0.070
GrECe	0.011 . . . 0.032	0.036 (1978)	0.036
Ireland	0.013 . . . 0.027	0.029 (1979)	0.032
Portugal	0.007 . . . 0.021	0.026 (1985)	0.022
Luxembourg	0.001 . . . 0.001	0.001 (1978)	0.001
EC	0.918 . . . 1.914		2.068

Table 11.5
Co-authored publications with other EC member states
(all figures as % world share of publications)

On the basis of figures for 1978-1987, again 1988 was predicted and compared with 1988 actual. The results are listed in *Table 11.5*. As one can see from the comparison of the third and fifth columns, the prediction of 1988 values on the basis of the uni-variate time series is almost perfect, up to the second decimal of the percentage. However, despite this excellent prediction, the resulting distribution is 15% more different from actual 1988 than the previous year's (1987) distribution. Therefore, we need *not* reject the hypothesis that the indicator for between member-state co-authorships is developing more as a property of the EC as a system than as an attribute of each country separately. (See *Table 11.6.*[107])

a posteriori	1988 actual	1988 actual
a priori	1987 actual	1988 predicted
Pearson correlations	0.99953	0.99948
I in mbits	0.94	1.08

Table 11.6
Pearson correlations and dissimilarities in millibits for the EC-network of multi-laterally co-authored papers

In summary: by using entropy measures statistics can be developed which enable us to estimate a prediction for next year's value non-parametrically and independently of the length of the time series (as long as it is larger than or equal to three). The basic assumption of the statistics–namely that without further information, one has no reason to expect change in terms of relevant distributions over time–is much less restricting than the assumptions necessary for the mathematical idealizations implied in regression analysis or ARIMA time-series analysis. The latter, for example, requires at least some fifty measurement points.

The conclusion was that in the case of inter-member-state co-authored papers, the multivariate prediction for 1988 was better than the sum of the uni-variate ones, in spite of the excellent fit of the uni-variate predictions. Therefore, the data suggest the existence of a single publication system in Europe in terms of multi-laterally co-authored papers, but not overall for the publication system as a whole.

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[107] In this case, the omission of Luxembourg makes the pattern more pronounced.

11.2.3 Extension to the period 1988-1991

This conclusion raises two further questions. First, if such a system of multi-laterally co-authored papers has developed among European member-states, one may wonder when it emerged, what its nature is, and whether and how it is related to the European science policies. Secondly, how stable is this system?

The above observation was based on data for the period 1978-1988 only, but we did not exhaust our data. The above conclusions can therefore be considered as hypotheses which remain testable

Total publications as percentage of world share	Observed versus predicted	Observed versus observed in the previous year (*Markov*)
1988	**0.45**	0.54 (mbits)
1989	**0.60**	1.26
1990	0.50	**0.41**
1991	**0.39**	0.58
In-between EC memberstate co-authorships as percentage of world share		
1988	1.08	**0.94**
1989	**0.36**	0.92
1990	2.70	**2.51**
1991	2.66	**1.02**

Table 11.7
Information values for predictions based on uni-variate time series and on the basis of distributions for the previous year, respectively. **The best predictions are boldfaced.**

against new data as they are produced each year by the operation of the system(s) under study. *Table 11.7* exhibits the quality of the various predictions for 1988, 1989, 1990, and 1991 in a format comparable to *Tables 11.4* and *11.6*. In the lower half of this table, one can note that the hypothesis of a network of multi-laterally co-authored papers is falsified in the data for 1989, but corroborated for both 1990 and 1991. The irregularity in the data for 1989 can with hindsight also be discerned by visual inspection of the curves in *Figure 11.2*.

The prediction of the publication data reveals a deviance from the previous pattern in 1990, but in this case by using the CHI-indicator the system's prediction could not be rejected in either 1990 or 1991. However, the differences are small. Thus, the prediction based on the assumption of a system in the data is relatively improving, although there is (yet) not enough base to warrant the hypothesis of development at the European level in terms of the relative power of the various predictions.[108]

In summary, the extension to the period 1988-1991 illustrates how hypothetical all conclusions about structure remain. Just as the scientometric data above remained in need of an interpretation, so the systems-theoretical interpretations of the data remain expectations in relation to a next update.

11.3 FROM 'WHETHER?' TOWARDS 'WHEN?' AND 'WHY?'

The above support for the hypothesis of an emerging system in the network of multi-laterally co-authored papers does not yet show the emergence of this system in relation to Europeanization. What does 'emergence' mean in this context? In order to answer this question, the various kinds of dynamic transitions within and among systems specified in Chapter Ten have again to be distinguished.

First, systems may condition one another, and these boundary conditions may change over time. The question of the European

[108] A further argument in favor of the rejection of the hypothesis of an emerging network of publications is the disappearance of the effect when the raw data are replaced with two-year moving averages. This otherwise neutral manipulation suppresses noise in the data representation.

Commission about whether Community funding has changed international co-authorship patterns within the EC refers to such a change in dynamic boundary conditions for the system allegedly brought about by the Commission's interventions. Did the imposition of a resource structure significantly change the probability distribution of international co-authorship relations for those authors who participated in the program in comparison with those who did not?

In terms of a research design, this question requires the decomposition of the European international co-authorship network into a subset of authors who were funded by the EC and a subset of those who were not.[109] Since all formulas in information theory are composed of sigmas, the computation of the relative contributions of the two subsets to the overall uncertainty (and consequently to the processes of change) is straightforward.

In practice, it may be costly to gather the data necessary to undertake this research. Let us therefore anticipate what a significant difference between these two subgroups might teach us. It might teach us that the authors in each subgroup perform differently (or not).[110] Perhaps the authors who received EC funding were even selected for this reason. This result would, however, not be a sufficient indication of a qualitative effect of the funding on the operation of the whole system. In order to be able to measure an effect on the operation of the system, one has first to specify what such an effect would mean in operational terms.

How can the emergence of higher-level (that is, European) organization be modelled in relation to this type of lower-level (that is, nationally) distributed data? As noted, a 'lock-in' between a system and its environment can occur if a system drifts over a threshold in relation to a specific environment (Arthur 1988). Thereafter, the system cannot return to the previously existing

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[109] Note that there may additionally be 'in-between group' uncertainty.

[110] Significance can be tested, for example, by using chi-square. The so-called log-likelihood ratio chi-square is based on information theory, and therefore its results can be directly related to the results of statistical decomposition analysis.

alternative pathways, and thus the transition is 'path-dependent.' In our case this would, for example, correspond to a situation where the subgroup funded by the European Commission would (after a while) no longer be able to co-author without this funding. Intuitive examples of this type of lock-in are space scientists or nuclear physicists, who can no longer make progress without costly equipment which requires state (or supra-national) intervention.

The question of the European Commission, however, was not whether it had succeeded in making part of the relevant scientific community dependent on its funding, but whether its funding had an effect on the whole European scientific publication system by promoting the emergence of a qualitatively different multi-national co-authorship network. Thus, the specific stimulation of the subgroup tied within the programs should have triggered a more general development within the larger system, e.g., a significant growth pattern in this indicator.

One well-known example of a significant growth pattern is the sigmoid growth or diffusion curve (see *Figure 11.3*). First, a system is in a lag-stage, then it begins to grow exponentially (log-phase), and after a while it reaches a new equilibrium (saturation). The log-phase corresponds to a period of irreversible transition; the system may decay again, but if so, only along another pathway. In general terms, a system will have undergone a path-dependent transition if its further development is independent of its previous history as a system.

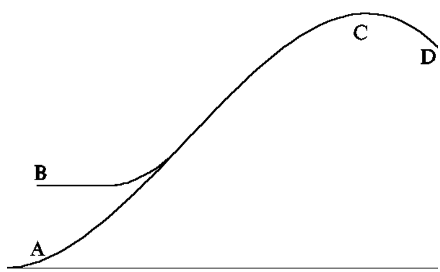

Figure 11.3
Sigmoid curve

For example, after reaching the steady state (**C** in *Figure 11.3*), the system contains a likelihood of decay into state (**D**), independently of whether it arrived at state **C** from state **A** or from state **B**, i.e., the initial lag-states. In other words: as long as the previous history matters for the system's future development, the system is not really new. (Note that this issue is quite different from that of whether the system's expected behavior is dependent on the history of its elements.)

The sigmoid growth curve is a uni-variate trendline. We are interested here in whether the *set* of curves as exhibited in *Figure 11.2* shows the characteristic of such a transition. Does the resulting network show the system's property of path-dependency, and if so, in which years? This cannot be assessed by visual inspection of the collection of curves, since the interesting part of the variance is contained in the interaction terms.

As argued in the previous chapters, this question can be studied by evaluating the following inequality:

$$I\,(\text{year}_t : \text{year}_{t-1}) + I\,(\text{year}_{t-1} : \text{year}_{t-2}) \;<\; I\,(\text{year}_t : \text{year}_{t-2})$$

The inequality enables us to distinguish between a 'normal' in-between year, and one that improves the prediction to such an extent that the transmission of information cannot be properly evaluated without taking this in-between year into account. If the inequality holds, the development of the system can be considered as path-dependent in the year *t-1*. Since *I* is defined in terms of probability distributions, the extension to the multi-variate case is straightforward.

11.4 HAS A SYSTEM OF CO-AUTHORSHIP RELATIONS EMERGED?

As noted, we found evidence for the existence of a system at the EC level only in terms of multi-laterally co-authored papers. When one assesses the distributions of multi-laterally co-authored papers for the 12 EC countries over the period under study, *1983*, *1986* and *1988* can be shown to represent path-dependent transitions as defined above.

Since three dispersed measurement points seemed a weak base for drawing conclusions, I followed two strategies to remove noise from this data. First, the chance of finding regularities at the level of the system increases if one evaluates the system's behavior over longer periods of operation. Therefore, I widened the operative cycle to two years by replacing the yearly data with two-year moving averages. In this case, I found path-dependency *in all years from 1981 onwards*.[111]

Second, when using the CHI-indicator instead of the aggregated dataset, *1979* joined 1983, 1986 and 1988 as a year of path-dependent transition. Further disaggregation leads to the conclusion that path-dependency is found only in the dimension of multinationally co-authored articles, but never when comparing among distributions for the number of multi-nationally co-authored reviews, notes, or letters.

In summary, in the period under discussion (1978-1991) the international co-authorship network among EC-member countries underwent a number of path-dependent transitions, but only in the dimension of multi-nationally co-authored *articles*.

11.5 CONCLUSIONS AND DISCUSSION

Where does this conclusion leave us with respect to the question of the role of the Commission's intervention? While it seems unlikely that all these transitions were brought about by the Commission's interventions, we cannot exclude the possibility that its policies may have induced one or more of the path-dependent transitions in the network of multi-nationally co-authored papers.

The problem thus concerns the *origins* of the indicated transitions. Is the European Commission effectively inducing change in the transnational co-authorship system among the EC member states, or is this system changing and thereby proving

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[111] It is apparently not unusual that the system should undergo path-dependent transitions into new states. A check for the aggregated publication system taught us that in nine of the twelve years between 1978 and 1991 path dependency in the dimension of articles occurred. However, remember that we rejected the hypothesis of a European system as the best predictor for next year's publication data. (A non-system cannot be evaluated as if it were a system.)

useful as an outlet for funding and a source of legitimation for EC interventions? What kind of empirical evidence could be used to corroborate or falsify either of these hypotheses?

Both hypotheses are corroborated by the above evidence which shows that the system changed qualitatively over the relevant period. But let us not forget the chain of intermediate steps required for the hypothesis that the Commission's interventions induced this change. Commission funding changed the boundary conditions for only a certain set of projects. This change could lead to the emergence of a subset of the multi-laterally co-authored papers with characteristics significantly different from those of the larger group. However, the evidence for the existence of such a subset was unconvincing. The higher visibility can be explained also in terms of the higher degree of multi-nationally co-authored papers within the subgroup.

Second, and in terms of the dynamics, the Commission created a different selection environment for the subset; but foreseeably, the effect of this intervention could be more than incidental only if a more stable lock-in, i.e., a specific relation, were developed between this subset and its newly created environment. The objective of the Commission, however, was not to tie a relatively small subset of European scientists to its policies and funding, and the major research question was therefore to evaluate the effects of its funding policies with reference to the system at large. Indeed, the hypothetical creation of a lock-in for a subset would have an effect on the remainder of the system, but it would not necessarily lead to path-dependent transitions in the larger set, which we actually found. The development of the subset may also have been more marginal.

In summary, a number of hypothetical assumptions are required to specify a theory of how the intervention could have led to the prediction of the observed effects. However, the previously reported fact that internationally co-authored papers receive significantly higher credit in terms of citations (Narin *et al.* 1991), clearly supports the hypothesis of a positive feedback which might explain the behavior of the system. The carriers of the network of internationally co-authored articles, that is, the scientists involved, seem to enjoy a higher chance of success in terms of (e.g.) citations, and can therefore be expected to seek out new sources of funding for this type of communication. The data strongly suggests that this network is still under construction, notably in the dimension of articles. In terms of this theory the European institutions have served mainly as one available office-window enabling scientists to organize a system of communication at this level.

Chapter 12

Knowledge Representations, Bayesian Inferences, and Empirical Science Studies

The use of probability theory in philosophy of science (e.g., Hesse 1974; Howsen and Urbach 1989), artificial intelligence (Pearl 1988), and empirical science studies enables us to compare and combine insights from these three hitherto only weakly connected traditions. In this chapter, I show that the importation of Bayes' formula into Shannon's information theory provides us with a model for structure/action contingencies and their dynamic interactions.

Bayesian philosophy can be considered as the special case in which a network of hypotheses provides the structure, while the evidence acts upon it. The differences among the three bodies of theory can be understood in terms of whether the information content of the messages about events is evaluated in terms of the *a posteriori* or in terms of the *a priori* information content of the system, and whether correspondingly it adds to the information content of the system or to its redundancy. The research program of empirical science studies can then be delineated as containing a specific philosophy of science.

12.1 INFORMATION THEORETICAL EVALUATION OF BAYES' FORMULA

The derivation of Bayes' formula follows straightforwardly from the third law of the probability calculus:

$$p(\text{A and B}) \quad = p(\text{A}) \cdot p(\text{B}|\text{A}) \tag{12.1}$$

or equivalently:

$$p(\text{A and B}) \quad = p(\text{B}) \cdot p(\text{A}|\text{B}) \tag{12.1'}$$

Therefore:

$$p(\text{A}) \cdot p(\text{B}|\text{A}) = p(\text{B}) \cdot p(\text{A}|\text{B}) \tag{12.2}$$

and thus *Bayes' formula:*

$$p(\text{A}|\text{B}) = \frac{p(\text{A}) \cdot p(\text{B}|\text{A})}{p(\text{B})} \tag{12.3}$$

While $p(\text{A}|\text{B})$ is the *a posteriori* probability–the probability of A given B–and $p(\text{A})$ can be considered as the *a priori* one, one can evaluate also the expected information content (I) of the message that A is conditioned by B as follows:

$$I_{(\text{A}|\text{B} : \text{A})} \quad = \Sigma\, p(\text{A}|\text{B}) \cdot \log\{\, p(\text{A}|\text{B}) \,/\, p(\text{A})\} \tag{12.4}$$

Combining (12.3) and (12.4) leads to:

$$\begin{aligned} I_{(\text{A}|\text{B} : \text{A})} \quad &= \Sigma\, p(\text{A}|\text{B}) \cdot \log\{\, p(\text{A}) \cdot p(\text{B}|\text{A}) \,/\, p(\text{B}) \cdot p(\text{A})\} \\ &= \Sigma\, p(\text{A}|\text{B}) \cdot \log\{\, p(\text{B}|\text{A}) \,/\, p(\text{B})\} \end{aligned} \tag{12.5}$$

This formula can be written as a difference between two logarithms, and therefore it can be considered (see Chapter Eight) as an *improvement of the prediction* of A given B:

$$\begin{aligned} I_{(\text{A}|\text{B} : \text{A})} \quad &= \Sigma\, p(\text{A}|\text{B}) \cdot \log\{\, p(\text{A}|\text{B}) \,/\, p(\text{B})\} + \\ &\qquad - \Sigma\, p(\text{A}|\text{B}) \cdot \log\{\, p(\text{A}|\text{B}) \,/\, p(\text{B}|\text{A})\} \\ &= I_{(\text{A}|\text{B} : \text{B})} - I_{(\text{A}|\text{B} : \text{B}|\text{A})} \end{aligned} \tag{12.6}$$

In words: the expected information of the message that *A* is conditioned by *B* is equal to an *improvement* of the prediction of the *a posteriori* distribution ($\Sigma\, p(A|B)$) if we add to our knowledge of the *a priori* distribution ($\Sigma\, p(B)$) how it is conditioned by the other distribution ($\Sigma\, p(B|A)$). The crucial point is the shift in the respective system of reference: if we know how *B* is conditioned by *A*, this improves our prediction of how *A* is conditioned by *B*.

Although the derivation of these formulas is completely analytical–the circularity of this reasoning has always been the major objection against the Bayesian perspective–the dynamic interpretation is not trivial. *A* and *B* can be considered as coupled systems that mutually inform each other. Thus, we obtain a co-evolution model for the problem of structure/action contingencies as specified in the final section of Chapter Nine (see *Figure 9.11*). This model can be generalized to a research design of dynamic relations between two (or more) structurally coupled systems.

12.2 Application to Social Network Analysis

If we assume a structure *A* and a distribution of actors *B*, at each moment in time, the actors will take action *given* the structure; thus, *B* is conditioned by *A* when it operates. Action may have an impact on structure *A* at the next moment. This effect can be expressed in terms of the amount of information expected from the message that *A* has at the later moment become conditioned by *B*. (See *Figure 9.11* at p. 192 for a visual representation of the problem.)

Let us write equation 12.5 (above) as follows:

$$I_{(A|B\,:\,A)} = \Sigma\, p(A|B)_{\text{posterior}} \cdot [\log\{p(B|A) \,/\, p(B)\}]_{\text{prior}} \qquad (12.5')$$

The formula explicates how action at the nodes and the network are conditioned by each other dynamically: the right-hand factor $\{p(B|A) \,/\, p(B)\}$ describes the instantaneous conditioning of any action by structure at the prior moment, while the left-hand factor contains the description of the network after action, that is, $p(A|B)$ at the posterior moment.

The crucial point is that the formula allows for a shift in the system of reference: by observing the actors one is able to infer knowledge about the development of the network, although the latter is a different unit of analysis. The right-hand factor has B as its system of reference; it can be evaluated on the basis of inspection of how the actors are conditioned by the network at one moment in time. The left-hand factor indicates the impact on the network at a next moment. The I then expresses the *improvement* in the prediction of the later (*a posteriori*) state of the network, when we know how the action was conditioned by the network at the previous moment (*a priori*).

The resulting improvement of the prediction of the network A at the later moment is due to the mutual information between A and B at the earlier moment. The mutual information (T) between distributions is defined in a static model in terms of the uncertainties (H) in the distributions as follows (see Chapter Seven, notably *Figure 7.1*):

$$H_{AB} = H_A + H_{B|A} = H_B + H_{A|B} \qquad (12.7)$$

$$T_{AB} = H_A - H_{A|B} = H_B - H_{B|A} \qquad (12.8)$$

Thus, the relation between two terms in the right hand factor of equation 12.5' ($p_{B|A}$ and p_B) informs us only about the static transmission, i.e., about the impact of the arrow(s) in *Figure 9.11* which indicates the conditioning of action by structure at the prior moment (cf. Giddens 1979). The combination with Bayes' formula allows us to draw the inference about the update value of the event(s) which happened, for the network even if one is not able to observe the latter directly.

At any moment, knowledge of the uncertainty in the network improves our prediction of the uncertainty in the action systems, but only for the part of the transmission, that is, $H_B - H_{B|A}$ ($= T$), and not for the remaining part of the uncertainty ($H_{B|A}$). The two coupled systems inform each other in interaction or co-variation, but the remaining uncertainty in either system remains 'free': *A* represents a self-referential system which determines its own total uncertainty. At each moment, how *B* is conditioned by *A* does not inform us about how *A* is conditioned by *B*, but only about how the conditioning reduces the remaining uncertainty. The uncertainty that prevails in the network is minimally equal to its (*a priori*) expected information content (H_A), and maximally limited only by the logarithm of *A*'s elements ($\log n_A$), by definition.

In other words: when two *systems* condition each other in a static relation, one may expect also a dynamic interaction. After the cycle (*a posteriori*), the fact that a specific interaction with *B* has occurred belongs to the history of *A* (that is, 'is a given for *A*'). If the dynamic interaction is repeated over time, a form of dynamic coupling may emerge. Note that dynamic coupling is thus a consequence of the (static) mutual information between *A* and *B*.

Furthermore, the improvement in the prediction is necessarily positive, since formula 12.5 is equivalent to formula 12.4; and the latter can be shown to be positive (Theil 1972, pp. 59f.). Therefore, the prediction of 'structure given action' at the later state is always improved if we know how structure conditioned action at the previous stage. In each cycle, there is an increase of expected information content, since the new value $H_{A|B}$ is the initial value (H_A) for the next cycle. In other words, a structure/action contingency produces 'probabilistic entropy,' that is, Shannon-type information, and thus has a history.[112] It can be considered as a dynamic source of variation.

A communication network *A* is *structurally coupled* to the nodes (*B*) of this network (Maturana 1978; Luhmann 1984) like the columns of the matrix are contingent upon the rows. The rows represent the actors who relate; the columns can be considered as the communications. If the nodes pursue their own respective

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[112] As noted in previous chapters, having a history does not imply that this history is always important for the system's further development. Historical information can sometimes lose its relevance (for example, if the system has the Markov property or goes through path-dependent transitions).

operations, the effects of the boundedness of *A* and *B* to each other is not the same in either case. Since *I* can be used as a measure of the quality of the prediction, the model allows us to develop a measure of whether (and to what extent) the one type of data in empirical research on structure/action contingencies represents structure, while the other represents action, or vice versa.

One can easily imagine designs in which the system that represents action or 'micro-variation' at one moment in time will operate as structure or 'macro-selection' at a later moment. Methodologically the two perspectives are symmetrical tests, just as they are conceptually symmetrical in the idea of a 'mutual shaping' of structure and action by each other during a co-evolution. However, if the systems *A* and *B* are completely coupled in one operation–that is, exhibit no independent development–the $I_{(A|B\,:\,A)}$ is equal to $I_{(B|A\,:\,B)}$.

12.3 AN EMPIRICAL EXAMPLE IN SCIENTOMETRICS

Let me provide an empirical illustration by using the transaction matrix of the same aggregated citation data of 13 major chemistry journals (listed in *Table 12.1*) among each other as in Chapter Nine. (As noted there, these matrices can be compiled using the *Journal Citation Reports* of the *Science Citation Index*.) The 1984 matrix for the journals can be considered as the *a priori* distribution for the 1985 matrix as the *a posteriori* distribution. The matrix for 1984 was given in *Table 9.2* (at p. 161); the one for 1985 is exhibited in *Table 12.2*.

As discussed in Chapter Nine, 'citing' can be considered as the running (action) variable for each year, while 'cited' refers to the archive of the respective journal's literature. Therefore, the 'being cited patterns' can be considered as structural. Since the citation matrix contains the mutual information between the cited *and* the citing dimension, it should provide us with an opportunity to make predictions about the impact of citation behavior on citation structure in the next year by using the above derived formulas.

We first discuss a representation in which the two dimensions are completely coupled in a *single* operation, *viz.* citation. In 1984, a static interpretation provides us with a mutual information *T* between 'citing' and 'cited' of 964.2 mbits. For 1985, the equivalent value is 972.4 mbits, that is, 8.2 mbits more. However, in the dynamic model (that is, by using formula 12.5'), we find an improvement in the prediction for 1985 to 969.7 mbits on the basis of 1984 data, that is 5.5 mbits more than in 1984. This means that 5.5 mbits of the 8.2 mbits change in the transmission between 1984 and 1985–that is, 67.1%–can be attributed to the previous transmission. In other words: the increase in the coupling is 2.7 mbits *above expectation*.

Journals:	*Grouping:*
Chemical Physics	*chemical physics*
Chemical Physics Letters	*chemical physics*
Inorganic Chemistry	*inorganic chemistry*
J. of the American Chemical Society	*organic chemistry*
J. of Chemical Physics	*chemical physics*
J. of the Chem. Society-Dalton Trans.	*inorganic chemistry*
J. of Organic Chemistry	*organic chemistry*
J. of Organometallic Chemistry	*inorganic chemistry*
J. of Physical Chemistry	*chemical physics*
Molecular Physics	*chemical physics*
Physical Review A	*chemical physics*
Tetrahedron	*organic chemistry*
Tetrahedron Letters	*organic chemistry*

Table 12.1
Thirteen chemistry journals used for the construction of an aggregated journal-journal citation network

Since the operation of 'cited' and 'citing' are mutual in this universe of 13 journals, this result would remain the same if the matrices were transposed. However, one may also assume a higher-order communication network between the cited and the citing journals, in which case the operations are mediated, and thus in principle asymmetrical. In this case, one would need an independent operationalization of the communication network.

(cited) \ (citing)	1	2	3	4	5	6	7	8	9	10	11	12	13
1	936	705	44	173	1168	-	-	-	487	170	87	-	-
2	965	2371	201	828	2819	38	60	-	1669	438	286	72	-
3	22	122	5892	1968	144	1177	87	1176	351	27	-	26	24
4	328	979	4891	15855	1235	1132	7632	2710	3077	160	25	3593	3639
5	2630	4602	639	2316	14328	139	152	111	4775	1827	1472	167	-
6	-	25	1091	529	-	1199	30	869	58	-	-	20	-
7	-	25	102	2314	-	49	5661	616	100	-	-	2020	2509
8	-	-	689	1041	-	541	338	3480	-	-	-	152	248
9	256	868	491	1238	1568	67	193	37	4121	155	80	108	33
10	322	445	86	221	1175	-	-	17	328	1165	149	25	-
11	221	291	-	-	1064	-	-	-	87	214	4273	-	-
12	14	-	31	813	-	23	1114	179	38	-	-	1116	908
13	-	-	75	1942	-	23	3171	683	58	-	-	2047	3748

Table 12.2
Citation matrix of thirteen chemistry journals in 1985.
(See Table 9.2 for the corresponding matrix in 1984.)

Let us use the eigen-structure of the matrix as this independent operationalization of the communication network. (The eigen-structure of an asymmetrical matrix is asymmetrical indeed.) In order to keep the analysis simple, let us make the assumption that this set of 13 journals can be grouped in three sets, namely: one of inorganic chemistry journals, one of organic chemistry journals, and one of journals which belong to the specialties of physical chemistry and chemical physics. (The attribution of the journals to these groups is also exhibited in *Table 12.1*.[113])

This assumption of a (latent) structure provides us, in a *second* research design, with a matrix of three cited clusters which represent the cited structure, and 13 citing journals which represent action. At a *third* stage, we may then also group the citing journals, and analyze the three-by-three matrix which represents the interaction between the presumed cited and citing structures.

As above, on the basis of the 1984 matrix we can make a prediction of the transmission in the 1985 matrix. In the case of the asymmetrical matrix of three cited journal groups versus 13 independent *citing* journals, the observed transmission is 726.75 mbits in 1984 and 732.23 mbits in 1985.[114] The prediction on the basis of 1984, is 731.81 for 1985, that is, the prediction now covers 92.3% of the 5.48 mbits increase in the transmission (as against 67.1% in the previous case).

If we subsequently assume that the citing action is not independent but also completely grouped into the same three groups as the cited structure, the transmission is 669.33 mbits in 1984, and 672.43 in 1985. Now the prediction on the basis of 1984 is 673.02 mbits, which is 19.0% more than the observed increase in the

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[113] The attribution is based on the default factor solution for this matrix. See, for further details, the extensive discussion of this matrix in Chapter Nine.

[114] These relatively smaller transmissions in absolute terms are larger parts of the total uncertainty in the respective matrices. But the matrices are differently shaped, and can therefore hold less entropy.

transmission in 1985. Obviously, the assumption of complete grouping on both the cited and the citing side *overestimates* the structural coupling between the latent eigen-structure and the observable relations using these matrices.[115]

In summary: I have elaborated this model as an example of the predictive power of the derived formulas. The elaboration has been crude, among other things, since I did not allow for more groupings than the one into three groups, and I assumed that the one-year difference was an adequate time-scale. But using this model, we could account for more than 92% of the change between the two years on the assumption that the cited side is structured, and that on the citing side the journals behave independently. The mutual dependencies on the citing side are underestimated in this model; they may account for the remaining 8%.

12.4 BAYESIAN REASONING

12.4.1 Bayesian philosophy of science

The probabilistic turn in the philosophy of science has mainly been based on the use of Bayes' formula for the evaluation of belief updates: according to Bayesian philosophers, empirical evidence can be assessed in terms of changes in the probabilities of the various hypotheses which may be invoked for its explanation (e.g., Howsen and Urbach 1989).[116] In the Bayesian philosophy of science, structure is a set of hypotheses H, and action then is the relevant evidence e. Thus, the argument for structure/action

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[115] The assumption of grouping in *only* the citing action overestimates the coupling with 5.9%. However, this model has no clear interpretation in the context of the assumptions concerning citation behavior specified above.

[116] Whether these probabilities are empirically measurable or more subjective is a matter of considerable controversy among Bayesians (cf. Phillips 1973b). The issue is of relevance to empirical science studies (cf. Giere 1988), but not to my argument in this chapter.

contingencies is repeated, but now not with respect to social structure, but to cognitive structure operationalized in terms of hypotheses. Accordingly, Bayes' formula is used in the following format:

$$p(\mathrm{H}|\mathrm{e}) \quad = \frac{p(\mathrm{e}|\mathrm{H}) \cdot p(\mathrm{H})}{p(\mathrm{e})} \tag{12.9}$$

and the information content of the message is correspondingly:

$$I_{(\text{posterior:prior})} = \Sigma\, p(\mathrm{H}|\mathrm{e}) \cdot \log \{\, p(\mathrm{e}|\mathrm{H}) \,/\, p(\mathrm{e}) \,\} \tag{12.10}$$

This I, the expected information value of the evidence for the *a posteriori* distribution of the hypotheses, is equal to the *information improvement* for predicting the posterior hypotheses from the evidence, given the prior belief distribution, over the prediction without the prior belief distribution. Since $p(\mathrm{e})$ is essentially a normalization term, we may more simply state that the expected information value of the evidence for changing the *a priori* belief distribution into the *a posteriori* distribution is equal to the improvement of the prediction from the evidence brought about by accepting the *a priori* hypotheses. This result illustrates how intrinsically hypotheses and evidence shape each other in Bayesian reasoning: one can look at the belief update either in terms of data becoming more informative by accepting an *a priori* belief structure, or in terms of a belief structure being informed by new data.

From the Bayesian perspective, the latter is the correct approach: 'evidence' is the motor of further development. It relates to the belief-structure of the scientists as action relates to structure in the action/structure contingency model discussed previously. Evidence can be aggregated, and the total impact on the belief-structure can be decomposed in terms of pieces of evidence. In other words, the (sociological) structure/action contingency relation can be seen as the general problem, and Bayesian philosophy as a specific elaboration of it with respect to a certain domain (that is, 'cognitive structure' and 'cognitive action') and with the help of certain assumptions and idealizations (e.g., concerning measurement).

12.4.2 The use of the Bayesian theorem in artificial intelligence

In artificial intelligence a more pragmatic use is made of Bayes' formula (e.g., Pearl 1988). The use of Bayesian statistics localizes the relevant environment for new knowledge, and therefore limits the amount of computation needed for updating a knowledge representation in response to evidence. Such updating had been seen as a major disadvantage of the probabilistic approach to artificial intelligence, which is otherwise attractive since it allows for handling (local) context-dependencies as conditional probabilities. As Pearl (1988, at p. 35) put it:

> the power of Bayesian techniques comes primarily from the fact that in causal reasoning the relationship $P(e|H)$ is fairly local, namely, given that H is true, the probability of e can be estimated naturally and is not dependent on many other propositions in the knowledge base. For example, once we establish that a patient suffers from a given disease H, it is natural to estimate the probability that he will develop a certain symptom e. The organization of medical knowledge rests on the paradigm that a symptom is a stable characteristic of the disease and should therefore be fairly independent of other factors, such as epidemic conditions, previous diseases, and faulty diagnostic equipment. For this reason the conditional probabilities $P(e|H)$, as opposed to $P(H|e)$, are the atomic relationships in Bayesian analysis. The former possess modularity features similar to logical production rules. They convey a degree of confidence in rules such as 'If H then e,' a confidence that persists regardless of what other rules or facts reside in the knowledge base.

I shall discuss the assumption that 'the probability of *e* is not dependent on many other propositions in the knowledge base' in the next section–it contradicts the so-called *Quine-Duhem thesis* in the philosophy of science–but I first specify the relation between the recursivity in the Bayesian algorithm and information theory.

Bayesian probability can be shown to be recursive (Pearl 1988, p. 37): if $\mathbf{e_n}$ denotes a sequence of data observed in the past, and e denotes a new fact, one does not have to include all the data of the sequence from the past in order to compute the posterior probability $P(H|\mathbf{e_n},e)$, but one can instead use the old belief $P(H|\mathbf{e_n})$ as the prior probability in the computation of new impact; it completely summarizes the past experience, and for updating need only be multiplied by the likelihood function $P(e|\mathbf{e_n},H)$, which measures the probability of the new datum e, given the hypothesis and the past observations.

By using the information theoretical reformulation above, these problems become tractable as problems of aggregation and disaggregation.[117] By consequence, the *extent* of the recursivity can be evaluated numerically in empirical cases. For example, if one assumes (provisional) grouping among part of the evidence ($\mathbf{e_i}$), the amount of 'within-group' uncertainty and 'between groups' uncertainty can be specified, and 'hard core' groupings can be provisionally dealt with as such (cf. Lakatos 1970). On the one hand, the properties of Bayesian and probabilistic reasoning make it possible to limit the effects of new evidence, that is, the 'propagation' of belief update, which otherwise would make it necessary to recalculate all the probabilities in the light of new data. On the other hand, substantive assumptions about non-linearities in the data are not excluded on the basis of *a priori* assumptions using the information theoretical approach (cf. Krippendorff 1986).

[117] The incremental nature of Bayesian updates can also be seen from the so-called *log odds log likelihood* formulation of Bayes' theorem, since each additional datum adds an independent piece of evidence to a summation:

$$\Omega_{(\text{posterior})} = \Omega_{(\text{prior})}\, \pi\, \text{Li}$$

$$\log \Omega_{(\text{posterior})} = \log \Omega_{(\text{prior})} + \Sigma \log L_{\text{i}}$$

in which Ω stand for the *odds*, and L for the likelihood of evidence$_{\text{i}}$ (see, e.g., Pearl 1988, pp. 38f.).

Should we count the computational advantages as an argument in favor of Bayesian philosophy? In my opinion, that would imply a category mistake: the argument for using Bayes' formula in artificial intelligence is a pragmatic one, concerning the role which the formula can play in offering shortcuts in computation in connection with an otherwise technically daunting approach to certain problems in constructing expert systems. Whether the knowledge contained in the expert systems should also be expressed in terms of Bayesian belief distributions depends on the type of expert system which one wishes to develop.

The Bayesian framework provides us with an elegant inference engine. For pragmatic reasons, however, the knowledge engineer may wish to use other inference engines; and, if one develops intelligent knowledge based systems for other purposes, for example, for classification and retrieval, one may still use the properties of Bayesian formulas for updates without necessarily requiring the expression of beliefs only.[118]

12.4.3 The decomposition of the *a posteriori* state in terms of the *a priori* one (The Quine-Duhem Thesis)

The recursive formulation of Bayes' formula provided above is based on the quoted assumption that 'given that *H* is true, the probability of *e* can be estimated naturally *and is not dependent on many other propositions in the knowledge base*.' The negation of the possibility of assessing *e* independently of other propositions in the knowledge base, however, is known in the philosophy of science as the *Quine-Duhem thesis*: new evidence *e* does not necessarily update *H*, but can also be evaluated in relation to other (auxiliary; e.g., instrumental) propositions in the knowledge base.

[118] One argument in favor of choosing a Bayesian framework at all relevant levels of analysis may be that of elegance, that is, of using the same method in knowledge engineering, inferencing, updating, etc., with probable advantages in computing and interfacing.

Dorling (1979) has argued that this problem can be solved within Bayesian philosophy of science by showing that the effects of e on the hypothesis (H) and on other propositions (e_n) are asymmetrical. I shall argue that this solves the problem only computationally; the problem of the philosophy of science can be solved by using the information theoretical approach, since this enables the analyst to specify groupings among hypotheses (as bodies of knowledge) or among coherent pieces of evidence.

Let us again generalize the problem for action/structure contingency relations, and return to the previous notation, with A indicating structure and B action. After a given event B, the total uncertainty in the structure A can be written as follows:

$$H_{(A|B)} = - \Sigma\, p_{(A|B)} \cdot \log(p_{(A|B)})$$

By using Bayes' formula, we can evaluate this *a posteriori* result into its *a priori* components, as follows:

$$H_{(A|B)} = - \Sigma \frac{p_{(A)} \cdot p_{(B|A)}}{p_{(B)}} \cdot \log \left\{ \frac{p_{(A)} \cdot p_{(B|A)}}{p_{(B)}} \right\}$$

$$= - \Sigma \; [p_{(A)} \cdot \{p_{(B|A)} / p_{(B)}\}] \cdot [\, \log\{p_{(A)}\} + \log\{p_{(B|A)} / p_{(B)}\}]$$

(I postpone the issue of the interpretation of $\{p_{(B|A)} / p_B\}$ as an *a priori* system to the next section, and proceed with the decomposition.)

$$\begin{aligned} H_{(A|B)} = &- \Sigma\, p_{(A)} \cdot \log\{p_{(A)}\} - \Sigma\, \{p_{(B|A)} / p_{(B)}\} \cdot \log\{p_{(B|A)} / p_{(B)}\} \\ &- \Sigma\, p_{(A)} \cdot \log\{p_{(B|A)} / p_{(B)}\} \\ &- \Sigma\, \{p_{(B|A)} / p_{(B)}\} \cdot \log\{p_{(A)}\} \end{aligned}$$

$$\begin{aligned} = &\; H_{(A)} + H_{(B|A)/(B)} + \\ &- \Sigma\, p_{(A)} \cdot \log\{p_{(B|A)} / p_{(B)}\} \\ &- \Sigma\, \{p_{(B|A)} / p_{(B)}\} \cdot \log\{p_{(A)}\} \end{aligned}$$

$$= H_{(A)} + H_{(B|A)/(B)} + \\ - \Sigma\, p_{(A)} \cdot \log\{p_{(A|B)} / p_{(A)}\} \\ - \Sigma\, \{p_{(B|A)} / p_{(B)}\} \cdot \log[p_{(A|B)} / \{p_{(B|A)} / p_{(B)}\}]$$

$$= H_{(A)} + H_{(B|A)/(B)} + \\ + \Sigma\, p_{(A)} \cdot \log\{p_{(A)} / p_{(A|B)}\} \\ + \Sigma\, \{p_{(B|A)} / p_{(B)}\} \cdot \log[\{p_{(B|A)} / p_{(B)}\} / p_{(A|B)}]$$

Thus, the total uncertainty of the system *a posteriori* is equal to the sum of the uncertainties of two *a priori* systems (*A* and *(B|A) / B*) plus the sum of the information values of the messages that these systems have merged into one *a posteriori* structure. The sum of the two additional terms[119] is equivalent to what may also be called the 'in-between' group uncertainty (H_0) upon decomposition of the total uncertainty in $H_{(A)}$ and $H_{(B|A)/(B)}$.

Note the analogy between 'later' and 'more aggregated': both contain more information. The 'in-between group' uncertainty is composed of two terms, that is, the difference which it makes for the one *a priori* subset in relation to the *a posteriori* set, and the difference it makes for the other. Indeed, this result is consistent with Dorling's (1979) thesis: an update cycle affects two (or more) *a priori* systems *asymmetrically*.

In the above formula, the *a posteriori* expected information content $H_{(A|B)}$ was decomposed into various parts which were given a meaning (in the right hand side of the equation) in terms of the *a priori* states of the respective systems. Paradoxically, this is precisely what Bayesians always do, although they use a different rhetoric. The Bayesian frame of reference is not the *a posteriori* situation, but the *a priori* one. For example, the philosopher asks what it means for the prior hypothesis that a piece of evidence becomes available. That the hypothesis (or, analogously, his belief) itself may have changed, and thus no longer be the same hypothesis, is for the philosopher of little concern. The Bayesian is not interested in the further development of the *a priori* stage into the *a*

[119] Since these are information contents of messages about change, they can be shown to be necessarily positive (Theil 1972, pp. 59f.).

posteriori one thanks to the new evidence, but only in the corroboration or falsification of the *a priori* hypothesis.

From a social science perspective, however, one is interested in what happened empirically, and not only in what this means in terms of the previous stage. Explication of the latter only adds to the redundancy. Of course, this may in itself have a positive function for the reflexive understanding (Luhmann 1990, pp. 469ff.). However, in this respect, Bayesian philosophy of science is normatively oriented. The fundamental shift of perspective in empirical science studies towards giving priority to the study of what happens in terms of its information content also has implications for assessing artificial intelligence.

12.4.4 The evidencing of the evidence

Let us for a moment turn to the interpretation of the *a priori* system which can be described with $\Sigma\, p_{(B|A)} / p_{(B)}$. Obviously, this is the ratio between the uncertainty in the action system which is left free by structure and the total uncertainty in the action system. Since $p_{(B)} = \Sigma_A\, p_{(B|A)}$ if the actions are independent, one may write in this case:

$$p_{(B|A)} / p_{(B)} = p_{(B|A)} / \Sigma_A\, p_{(B|A)}$$

The denominator can be considered as a normalization term for the size of the action system.[120]

If the actions (events) are not independent (e.g., one action triggers other actions before there is an impact on structure), the above equality does not hold, and there can be a size effect on structure. In the sociology of scientific knowledge, for example, attention has been given to how evidence is brought to bear (Pinch 1985), and thus the relation between various actions may be a

[120] In Bayesian philosophy, this term is a normalization constant because of the logical complementary of the hypothesis and its negation (Pearl 1988, at p. 32).

prerequisite for the presentation of the evidence. Action can then begin to behave as another self-referential system.

In general, a self-referential system does not merge with the uncertainty in events within its environment, but only with the uncertainty to which it relates in these events, after normalization for the size of the system. In this weak sense, the *Quine-Duhem thesis* is not yet refuted by the Bayesian approach: the theoretical impact of the evidence can only be assessed in comparison to another organization of the hypotheses, and given an assumed time horizon. If these delineations can be specified, the information theoretical approach allows us to solve the problem empirically, since the two models can be compared as windows in terms of the quality of the fit.

12.5 EXPERT SYSTEMS IN SCIENCE AND SCIENCE STUDIES

We know that expert systems function rather well in certain environments (diagnosis, etc.), particularly if the underlying knowledge base is rather codified. Langley *et al.* (1987) developed expert systems (BACON I, BACON II, etc.) which by using elementary assumptions were able, for example, to infer Boyle's law when provided with Boyle's laboratory notes, etc. The astonishing successes of expert systems based on simple assumptions concerning the psychology of discovery ('the heuristics') suggest that the task of building more informed models should be not too complex. However, one should keep in mind that the purpose of (knowledge) engineering is pragmatic, e.g., to produce a user interface which is sufficiently reliable according to some criteria of *use*; while the purpose of model formulation is primarily theoretical, that is, aiming at a better understanding of structure in the data. Knowledge engineers often skip the corroboration of the model by pragmatically assuming relations among decision criteria (e.g., Langley *et al.* 1987; Bakker 1987; De Vries 1989; cf. Giere 1992).[121]

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[121] Extensive research has been done at the level of individual scientists,

An intelligent knowledge-based system (IKBS) that is based on rules from psychology concerning human reasoning, or on mathematical assumptions concerning decision-making, invokes models of how *scientists* (should) reason when doing research or making decisions. They inform us about the subjective side of science, but not about its knowledge content. The crucial question for science studies in relation to the challenge of these knowledge representations can therefore be formulated as follows: can science studies provide us with theoretical insights which are (additionally) useful for the specification of inference rules in relation to scientific knowledge? In other words: is there specific knowledge about science in comparison with other forms of organized knowledge that is of relevance for the construction of artificial intelligence in a scientific environment?

Thus, the contribution of science studies should make a difference not on the formal side, but on the substantive side. If in a scientific environment the data are intrinsically related to theories, then this relationship should have consequences precisely for one of the most problematic issues in the construction of artificial intelligence: the so-called 'frame problem'.

12.5.1 The frame problem

In the empirical sciences, theories are never purely formal; they are embedded in meaning. Consequently, the data are not independent but theory-laden, and over time the data change both independently and in their relations to the inference rules. While in other (e.g., artificial) intelligence systems one may be able to separate the two, and keep one of the two dimensions constant over a relevant period of time, in science the two dimensions are mutually contingent: the data change with their interpretation. As

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both from historical and from psychological perspectives (e.g., Gooding 1990; Gorman 1992; Thagard 1992). However, the generalization to organized science as a communicative network of interactions has not yet been achieved (cf. Tweney 1992).

noted, this is the *Quine-Duhem* problem in its weak sense. In artificial intelligence, the problem is also known as the frame problem (see, e.g., Chabris 1989, pp. 61-6).

At each moment in time, we can feed the knowledge base with the best of our knowledge after appropriate knowledge engineering and/or on the basis of reconstruction on the basis of scientific texts. As I specified in the last section of Chapter Eight, given the data and given the decision criteria, the database may begin to learn when confronted with new data if one is able to specify inference rules. But in relation to the later events the expert system is necessarily *a priori*; it can only assess the meaning of what happens next in relation to what it already contains, even if, in the longer term, one may be able to provide the systems with algorithms for dealing with new patterns.

For example, one can instruct the system to disregard, for the prediction of the system's future behavior, data which describe stages before a path dependent transition has happened (see Chapter Ten). In practice, this may prolong the life-cycle of the system, but it does not solve the fundamental problem of how the system operates in empirical reality. One is not able to instruct the system when to give way to a completely different representation: the system has no external point of reference for the quality of its operation in a universe which allows for emergence.

Every knowledge representation necessarily has a time index attached to it; it is stamped by the time when it was engineered and framed. In this perspective, an IKBS is not essentially different from a textbook, although it may be additionally interactive and also have some capacity to learn. As in literature searches, creative combinations within it may lead to new applications. Like the library, an IKBS may have a service role in research. One might say that in an IKBS what was previously a structural condition of the system (e.g., the library) might begin to behave as an interactive system (cf. Swanson 1990). However, whether this logical possibility is actually the case, and in relation to which contexts, remain empirical questions from a science studies perspective.

In summary, we have seen that the Bayesian philosophy of science and artificial intelligence share an emphasis on learning with reference to the *a priori* situation, and with a focus on logical possibilities contained in this situation. The two traditions differ mainly with respect to the normative aspect. However, the choice of an *a posteriori* perspective makes it possible to raise also evaluative and empirical questions with respect to the functions and impacts of an IKBS in the science system, and to relate its development systematically to developments in other dimensions.

12.5.2 Genesis and validity

Assessing the relevance of events *a posteriori* (that is, the empirical question of 'what happened, and why?') brings us to another important issue which may easily lead to a confusion of normative and empirical perspectives. This is the issue of the analysis of the *a posteriori* state in terms of its genesis, and in terms of its validity. In systems theory one sometimes finds the notion that the new state is in a sense contained in the older one, and that it is therefore important to follow the development of the *a priori* system as a process in order to investigate how it shapes the *a posteriori* state. For example, Luhmann (1984, pp. 148 ff.) discussed the *auto-catalysis* which is supposedly contained in the double contingency of interpersonal relations, and which then 'produces' the communication system. Similarly, constructivists advocate that one 'follow the actors' in order to describe the emerging system (e.g., Latour 1987a).

However, one should clearly distinguish between information and redundancy. As was shown above, the evaluation of the *a posteriori* state in terms of the *a priori* states leads to a surplus of uncertainty which cannot be reduced to the *a priori* states, and which was indicated above as the 'in-between group' uncertainty. In other words, the process through which the new state has come about may be only one of the possible pathways which might have led to this (posterior) state. *Ex post*, other decompositions remain always possible. Note that the communicative function in the double contingency as discussed by Luhmann (1984) is a *consequence* of the existence of communication, and thus *a* *posteriori*. Hence, it cannot sufficiently be explained in terms of actor behavior (Leydesdorff 1993b).

The contingent pathway of emergence is not in itself an indicator of the validity of the description of the *a posteriori* system. The theoretical specification of the pathway creates *redundancy*; this may be useful for the understanding, but it reduces the information. The specified analogy between the dynamic problem and the multilevel problem may be helpful: at the higher level, each case or each subgroup of cases contributes only a part of the overall uncertainty. Additionally, there may be 'in-between group' variance. Analogously, the *a posteriori* situation may be a result of various *a priori* ones and the processes attached to them.[122] Once the *a posteriori* situation is a given, the pathway of its genesis becomes one contingent case, and thus explains only part of the uncertainty that prevails in the network. As noted, other decompositions remain possible. The evolutionarily complex system is contingently constructed, but it can be reconstructed by using other composition rules (Simon 1969). A specific representation of the system, for example by using a narrative or a confabulation (Hinton *et al.* 1989), can be used as a heuristic for the specification of hypotheses concerning the building blocks of the complex dynamic system.

In other words, the results of a development do not structure the development *ex ante*, but only with hindsight: there is simply no *causa finalis* in an empirical model. We need the events to update our expectations. Note that this priority of the *a posteriori* stage in empirical science studies makes its philosophy of science progressive, but 'progressive' in an empirical sense. The contingency of science and its progress does not imply that

[122] The analogy is a consequence of the Second Law of Thermodynamics: the later state of the system contains the entropy of the previous states plus the entropy generated by the process. Analogously, the aggregate contains the entropy of the previous subgroups plus the 'in-between group' entropy. If the system is additionally able to increase its redundancy (e.g., by self-organization) the relative information may nevertheless decrease (see Chapter Thirteen).

'anything goes,' but only that it goes when it has gone (Luhmann 1990, at p. 177). Science develops with time, like society (to which it belongs as a part) and all other autopoietic systems.

Analogously, the rationality question can also be reformulated as an empirical question: what was the rationale of what happened? In this research program, even the universality question becomes an empirical question: everything in a science is defined with reference to a universe, and all questions about things defined with reference to a universe are also empirical.

Chapter 13

The Possibility of a Mathematical Sociology of Scientific Communications

In the last decade, the sociology of scientific knowledge has increasingly focused on discourse (e.g., Mulkay *et al.* 1983) and communication systems or so-called ‘actor-networks’ (Callon *et al.* 1986). These theories inform us about the specificities of scientific communication systems (cf. Hacking 1992). For example, if scientific ‘groups’ are considered as local networks that relate to scientific ‘fields’ as more global networks (Pinch 1985; Callon *et al.* 1986), such a formulation signals the crucial mechanism of communication. Does the focus on discourse and communication offer new perspectives for an integration of qualitative and quantitative approaches in science studies?

In Part One, I argued that these theoretical analyses have not provided us with clarity about how to relate the complexity, the reflexivity, and the fluidity of the communicative operation. In Part Two, communication was explained as a complex process that cannot be taken unproblematically as a basis for understanding. For example, communication systems cannot be observed directly; one observes only their instantaneous operations. Furthermore, scientometric operationalizations have focused on the communication of *information*, while concepts like ‘actor-networks,’ ‘structure,’ etc., are used in postmodernist sociologies and semiotics mainly for studying the generation, communication, and decoding of *meaning* (e.g., Eco 1976; Courtial 1989).

The common point of interest is the question of how reflexive communication systems communicate both information and meaning. The elaboration of the mathematical theory of communication into a theory about the development of communication systems enables us to distinguish reflexive layers of communication as degrees of freedom. An analyst is able to attribute the observed uncertainty to hypothesized systems that interact in the events. The implications of this perspective for programs in the sociology of scientific knowledge will be specified.

13.1 UNCERTAINTY, INFORMATION, AND SOCIOLOGICAL MEANING

The Chinese language distinguishes between two concepts of 'information' by using two different words (Wu Yishan, *personal communication*). Both words contain two characters as depicted in *Figure 13.1*. The above one, 'sjin sji', corresponds to the mathematical definition of information as uncertainty.[123] The second, 'tsjin bao,' means information but also intelligence.[124] In other words, it means information which informs us, and which is thus considered meaningful.

The mathematical definition of information as *un*certainty is counter-intuitive (Bailey 1990). Shannon (1949, at p. 3) detached himself from the implications by stating that '(t)hese semantic aspects of communication are irrelevant to the engineering problem.' However, his co-author Weaver (1949, pp. 116f.) observed in this connection:

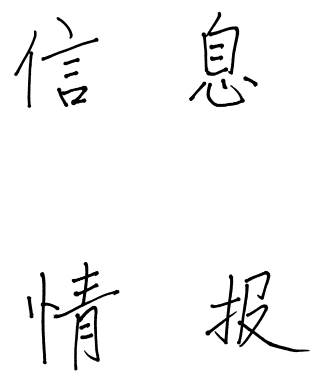

Figure 13.1
Two Chinese words for information: 'sjin sji' and 'tsjin bao'

[123] 'sjin' means letter of reliability, and 'sji' means message.

[124] 'tsjin' means situation or status, and 'bao' means report.

> The concept of information developed in this theory at first seems disappointing and bizarre–disappointing because it has nothing to do with the meaning, and bizarre because it deals not with a single message but rather with the statistical character of a whole ensemble of messages, bizarre also because in these statistical terms the two words *information* and *uncertainty* find themselves to be partners.
>
> I think, however, that these should be only temporary reactions; and that one should say, at the end, that this analysis has so penetratingly cleared the air that one is now, perhaps for the first time, ready for a real theory of meaning.

While Shannon addressed the transmission of signals trough *fixed* communication channels (e.g., telephone lines), meaning refers to an observing system that can use the information. An observing system can sometimes be 'informed' by the information–that is, the system is able to reduce its uncertainty. Information which adds to the confusion for an observing system may then be considered as noise. Anyhow, the expected information content will be *changed* by receiving a message.

When the focus is no longer on a fixed communication channel, but on an evolving communication system, one should distinguish between the *expected* information content of the receiving system, and *observed* information that is positioned by this system in a subsequent update. For an observing system each communication may make a difference with reference to its previous state (*Figure 13.2*). This reflexive information has also been identified with 'meaning' or 'meaningful information' (e.g., MacKay 1969; Bailey 1990).

The observing system can meaningfully position the incoming information with reference to its *previous* state. It was shown in Chapter Twelve that with reference to the *a priori* system the probabilistic entropy of the interaction may sometimes add to the redundancy, and therefore have a negative value. With reference to the *a posteriori* system, however, the uncertainty was shown always to increase. Note that the derivation in Chapter Twelve proves that the Second (Entropy) Law has an interpretation in terms of information theory (cf. Georgescu-Roegen 1971; Smolensky 1986; Swenson 1989).

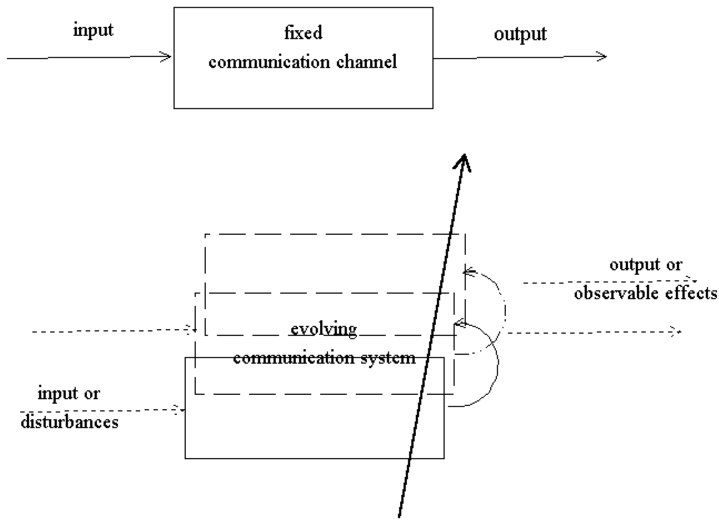

Figure 13.2
A communication channel and an evolving communication system

Some authors have defined this potential reduction of the uncertainty in the observing system as information or 'negentropy' (Brillouin 1962; Bailey 1990). This definition has caused semantic confusion, since a negative contribution to the uncertainty can be considered also as a redundancy (cf. Georgescu-Roegen 1971, pp. 401ff). The crucial point is the distinction of the perspectives: what can be considered as an increase of the uncertainty from one perspective, can be considered as a decrease from another (Hayles 1990). However, the potential reduction of the uncertainty in the observing system is a consequence of its potential to organize the incoming information by using previously established grouping rules. The grouping provides the system with a second degree of freedom, which conditions and determines the uncertainty in the first dimension (see Chapters Seven and Nine; *Figure 7.1*). Thus, the information contained in a message and the reflexive grouping into meaningful information can be considered as two dimensions of the uncertainty.

In other words, reflexivity should not be understood in terms of (potentially hierarchical) layers, but in terms of orthogonal dimensions of the system under study. The events generate uncertainty which can be provided with a meaning. The reflexive dimension and the information are expected to interact in the events. Note how we have now generalized the multi-dimensional framework of Chapter One to an understanding of what information may mean in relation to meaning: messages can carry information because they can hold information using a second dimension. For example, the same word can have a different meaning and the same meaning can be communicated with different words. One is able to understand the meaning of a message without grasping the information or *vice versa*. Information and meaning have to be cross-tabled, and then the time dimension has to be added so that one obtains a system that is not only able to transmit, but also to translate, that is, to change the meaning of the information reflexively.

Luhmann's (1984) crucial contribution has been that the network between the actors can be considered as a communication system that is added to the actors who carry it at the nodes, and that this network can provide a communication with a meaning ('Sinn'). But since the network is substantively not an actor, this sociological meaning should be distinguished from psychological meaning. Luhmann (1984) has wished not to specify this additional meaning (e.g. at p. 110). As noted, a meaning can only be attributed to an uncertainty if the latter contains two degrees of freedom. The evolutionary achievement in human communication is this possibility of holding two dimensions of the uncertainty in a single communication, notably the information and its meaning. As noted, receiving and sending actors can provide the messages with additional (psychological) meanings.

The difference between the information and meaning in human communication has been codified in language. Remember that we encountered this potential duality of the linguistic communication as a problem in Chapters Five and Six, when we were studying words and their co-occurrences. It was shown in Chapter Five that the network of words changed both substantively, and in terms of meaning among texts. This problem motivated us to list criteria for a useful methodology in science studies (Chapter Six). We chose information without being able to justify this choice at that moment otherwise than pragmatically: information theory seemed to comply with all the listed criteria.

How is it possible that information theory can help us in distinguishing between information and its meaning? I shall argue that reflexivity can be operationalized in terms of the recursivity of the probabilistic operation. (For example, one is able to ask for the probability of a probability.) Since probabilistic entropy is both recursive and content-free, this operationalization enables us (epistemologically) to objectify 'reflexivity' without (ontologically) reifying it. The formalization leaves room for substantive specification. The latter is potentially different at various levels, and therefore this reformulation allows us thoroughly to solve the so-called reflexivity problem, that is, the problem that one cannot claim priority for a specific reflection concerning reflexive actions. It becomes possible to ask for the quality of the reflection.

As noted, the reflexivity problem is pertinent to science studies (and sociology in general). For example, one may wish to raise the question of whether (and what) science studies can be expected to add to the insights of practicing scientists. In later sections of this chapter, I shall show that the reformulation of reflexivity as a property of the communication solves a number of theoretical problems in science studies, but let me first turn to the question of how reflexivity can be operationalized in the mathematical theory of communication.

13.2 THE RECURSIVITY OF COMMUNICATION

Hayles (1990) has noted that Shannon's (1948) decision to equate expected information with *un*certainty, and to choose a mathematical identity in the formulas for information and entropy, was extremely parsimonious. The resulting concept of communication is highly abstract: it specifies communication as an operation that generates probabilistic entropy or, in other words, communicates information. Since communication, probabilistic entropy, and information are here defined as content-free, these concepts *precede* their operationalization. Probabilistic entropy can be considered as a dynamic equivalent of concepts like 'degree of freedom' or 'dimension' in the static analysis. Its substantive definition–that is, the specification of references–spans the window of the reflexive representation, but dynamically.

A concept is by definition cognitive and reconstructive. 'Probabilistic entropy,' however, captures an operation. Since the measurement itself is an operation, this concept is additionally recursive–that is, it can be applied to the results of its own measurement. Consequentially, the mathematical theory of communication enables us to study both communication systems and the communications among them, that is, to systematically distinguish among different levels of communication and to relate the results of these observations reflexively. However, engineers who study the transmission of signals through *fixed* communication channels need not consider the possibility that the expected information content of a message has changed because of the operation of the communication system. For them, the operation of the communication channel is considered as only a potential source of noise. But for the study of changing systems, one has to generalize from Shannon's point of view to the probabilistic entropy which is generated when the communication channels themselves are affected by the operation.

Communication systems that are themselves subject to change are not in equilibrium; they evolve by exchanging information with their environments. Evolution theory, however, has assumed traditionally that the 'natural' environment only selects. From this perspective the natural environment has remained an external given for the evolving system, which itself exhibits variation. However, if information is exchanged between a system and its environment, the environment can no longer be conceptualized as a given ('nature'), but it must be considered as another communication system that contains information, that is, exhibits variation. The system/environment relation is consequently a relation between communication systems. The communication systems inform each other by communicating.

In general, communication systems can communicate information only with other communication systems. Communication systems communicate through 'mutual information' or co-variation (Chapter Seven). When a pattern of co-variation among them is maintained over time, systems may begin to co-evolve, that is, mutually to shape one another (Chapter Twelve). Co-evolution, and not evolution, is consequently the general concept for understanding dynamic developments. The concept of co-evolution enables us to understand, among other things, how new information can enter a system from its environment.

In traditional evolution theory, 'natural' selection was additionally supposed to lead to the survival of specific variants. In the case of co-evolution theory, the *stabilization* of specific co-evolutions adds a third mechanism to the previous pair of variation and selection. Among these three mechanisms (variation, selection, and stabilization) at least two cybernetics can be defined. While selection can occur at discrete moments in time, stabilization presumes the assessment of variation and selection over the time dimension. Stabilization is consequently a second-order selection: which selections are selected for stabilization?

The operation of two selections on each other potentially closes the system. Temporarily, the two selections can sometimes balance each other as action and counter-action, and thus, the system may exhibit phases of stabilization. For example, words may have stable meanings within a specific context and during a period of time. In a next selection, self-organizing systems (e.g., Prigogine and Stengers 1979; Maturana and Varela 1980) are additionally able to select among reflexive meanings. In short, self-organizing systems can be defined as communicative in relations (first degree of freedom), able to position the information (because of differentiation in a second dimension of the probability distribution), able to reflect on the information (using time as a third dimension), and additionally able to reflect on the arrangement in these three dimensions using a fourth degree of freedom.

Although the ('macro'-)selection is evolutionarily built upon the ('micro'-)variation during the shaping of the complex system, the dynamics change when the system is in place: each degree of freedom in a complex and dynamic system may select on the other ones, and thus 'variation' and 'selection' have to be considered as sub-dynamics of such a system. The category which was specified as the selecting instance at one moment in time, may have to be respecified as generating the variation at a next moment.

13.3 THE EMPIRICAL DELINEATION OF COMMUNICATION SYSTEMS

How can one delineate complex and dynamic communication systems in empirical research? A description at one moment in time would not serve, since communication systems change over time. As noted, the observed data indicate interaction among the systems, and therefore one may expect these systems to have changed after interaction. How are communication systems able to maintain structure over time despite variation in the observable phenomena?

A communication system operates through the deconstruction of relations among its composing lower-level units, and their reconstruction into a new (that is, updated) structure. By relating in the network, in terms of aggregations and disaggregations, and over time, the 'actants'–that is, whatever may be relating given the nature of the communication–develop a network with an architecture. When written as a matrix, this architecture can be analyzed in terms of its so-called eigen-structure; and when repeated over time, these structures may be expected to contain eigen-time, that is, options for further developments which are more or less likely to occur.

In a matrix representation, the relating agents are conventionally shown as row vectors, and communications as column vectors. The observed communication of an uncertainty (e.g., the change of a cell-value) has an update value for all systems involved: it informs us not only about external references to the communications, but also about the behavior of the communicating systems over time. Additionally, the option to vary among the delineations of multi-variate datasets and the lengths of time-series provides the analyst with yet another degree of freedom. The duality of the reconstruction in terms of eigen-structure *and* eigen-time allows him/her to distinguish datasets which increasingly behave as distributed identities, that is, which tend to maintain their (complex) structure over time.

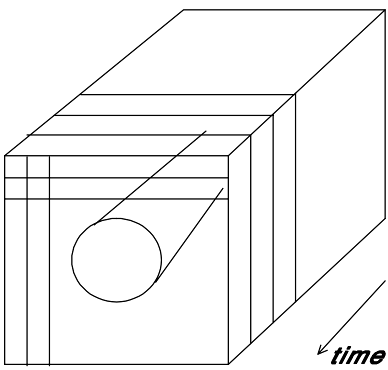

Figure 13.3
An observable trajectory of a (potentially complex) system in three dimensions

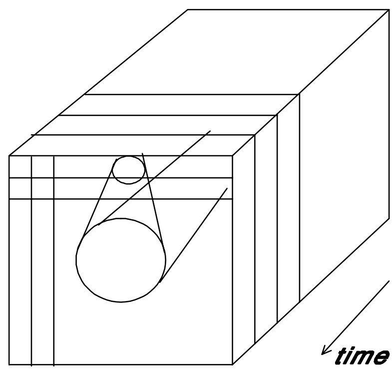

Figure 13.4
Selection among representations of the past using a fourth degree of freedom

Structure, stability, recognizable trajectories and (distributed) regimes remain hypotheses, since they have to be inferred on the basis of a reconstruction. In other words: the analyst can reconstruct the state of the system in terms of its eigen-structure. This eigen-structure can be pronounced, and the maintenance of a specific pattern can be attributed to the self-referentiality of the system under study. Time series of data can be assessed on whether a system can be expected to have developed an eigen-time (e.g., a life-cycle). Using the degree of freedom between eigen-structure and eigen-time, one can hypothesize that the system has options to organize itself increasingly in terms of its operation. The self-organizing system can be considered as the construction which is able to use this additional degree of freedom for maintaining the character of its communication over time, despite alternatives.[125]

The expectation values for the entropies on the basis of the *hypothesis* of self-organization can be tested against the data which are to be observed: if a complex data structure operates as a system, it is expected to exhibit other (co-)variations than when its elements change independently. A self-organizing system is expected to recover from temporary losses of structure.

For example, the clear factor structures which have so often been reported from studies of aggregated journal-journal citation data (e.g., Carpenter and Narin 1972; Doreian and Farraro 1985; Leydesdorff 1986; Tijssen *et al.* 1987; Tijssen 1992; see Chapter Nine) are not the incidental results of one clustering algorithm or another on this 'data' as given in a natural history; they are rather the results of operations (i.e., 'facta') among the various specialty structures involved. There is nothing in these journals which make them cluster, except that they refer to specialties and disciplines as higher-order communication systems. These cycles of communication are not observable in terms of the communicating agents (that is, the citing texts in the journals), but they can be distinguished analytically as virtual hyper-cycles of communication for explaining the observed structure and continuity in this data. Only when these higher-order systems of communication are hypothesized, can they sometimes be made visible by appropriate aggregation of the lower-level data (cf. Leydesdorff and Cozzens 1993).

Without such a hypothesis, the complexity at lower levels may be overwhelming. Although the journals can vary, the clusters exhibit a tendency to remain stable in the time dimension. By interpreting lower-level communications with reference to the higher-order system, hypotheses with respect to this latter system can also be updated by observing the composing units (see Chapter

[125] The degrees of freedom in the probability distribution correspond to the dimensionality of the uncertainty because of Shannon's formula.

Twelve). The self-organizing systems and subsystems remain analytical possibilities which are contained in the empirical distributions within and among such units of observation. The super-systems, thus, should not be reified at the meta-level in terms of the gods and demigods who supposedly govern history at lower levels. They remain empirical uncertainties. However, the eigen-vectors of eigen-structure and the eigen-frequencies of eigen-time cannot be attributed to any of the constitutive elements; they are latent properties of the matrix which represents the network, and of its development over time.

The situation is analogous with nominal (e.g., historical) data, although the measurement scale is more relaxed: the analyst can only tell a story ('a reconstruction') on the basis of the (sometimes implicit) assumption that, for example, it was the science under study which, in order to develop, had to reorganize whatever relevantly happened in terms of what it meant for its structure, its historical development, and its identity. However, the analyst has to tell the story by using a metaphor: the system under study remains a construct, and reflexively the analyst should never reify the reconstruction. If the specification is sufficiently precise, alternative hypotheses can be tested by comparing the observed variations with the expected ones during the operation of the hypothesized systems.

13.4 THE HYPOTHETICAL STATUS OF COMMUNICATION SYSTEMS

This definition of communication systems as probabilistic constructs highlights why it is not enough to study only the observable 'interactions,' for example, between social and cognitive developments (e.g., in terms of 'socio-cognitive' actions).[126] The observable interactions exhibit the mutual information or the co-variance among the hypothesized systems during the event of the

[126] 'Within this interpretation "paradigm" is taken to be a term which emphasizes the combined *socio-cognitive* nature of scientific activity.' (Pinch 1982, at p. 17).

operation.[127] Additionally, the remaining variances in participating systems have to be specified. The sum of the co-variance and the remaining variance (that is, the total variance) contains the information about the state of a system, and is thus relevant for the expectation of the system's further development. In other words, the latent dimensions provide us with the frame of reference of what the incoming (relational) information means in positional terms. Consequently, the observable interactions inform us only partially about the systems under study.

Not all communication systems can retain the probabilistic entropy, and only specific ones can use their history for their own further development. As noted, Luhmann (1984) postulated that social systems are communication systems that (among other things) reflexively process their history. Since the operation of a social network is different from that of individual actors, concepts like information, meaning, and reflexivity have to be substantiated differently with reference to this system.

Historians and sociologists sometimes find it difficult to generate a semantics for communications that can have meaning for systems other than actors. However, one has to reverse the causality in the reasoning: communications can have meaning for actors, since actors are self-referential systems that can retain and internally process the information. In addition to the actors, the network can be considered as a relevant context for the interaction: what is action with reference to an actor, is communication with reference to a social network. These operations are orthogonal like the rows (which represent the actors) and the columns (which represent the communications) in a matrix representation. The co-variation between rows and columns can be given meaning with reference to both dimensions of the matrix. Over time, the interaction is generated both by the self-referential loop of the network, and by the self-referential loops of each of the actors involved. The systems

[127] Although differently defined, the variance and the expected information content of a distribution can both be considered as measures of the uncertainty. Analogously, the concept co-variance is equivalent to the mutual information (see Chapter Seven; Theil 1972).

inform one another mutually through the windows of these interactions.

Thus, the thesis is not that the network behaves like another actor, but that the dynamics of the two systems, including the question of how meaning is generated, can be analyzed within one formal framework. Note that the actor has the same hypothetical status as the network: one observes the event which can be attributed as an action to the actor and as a communication to the network (Maturana 1978). The (orthogonal) systems constitute relevant environments for one another; to the extent that they co-vary in the events, they inform one another about their behavior. It depends on the substances of the systems whether, and if so how, they are able to inform themselves about their relation with this environment, that is, whether and how they can use the information that they observe through the window of interaction for the further development of their expected information content.

Without the specification of the network as a system of reference, the analyst is at risk of attributing meaning to the wrong system, and consequently of category mistakes. For example, if a scientist communicates, this communication has more meanings than the (intentional) meaning which it has for the sender, and the various meanings which the message may be given at each receiving end of the communication. Additionally, the message takes place in the network of communications. This situational meaning can be processed reflexively ('translated') within this communication system if the network has previously gained a specific identity (e.g., a specialty structure).

However, if the sociologist (or the historian) studies exclusively the scientists and their observable actions, both at the sending and at the receiving end of the communication, the additional meaning with reference to the network can no longer be specified. If subsequently one becomes aware of an additional (sociological) meaning that has to be attributed to the situation–i.e., not to any of the actors involved, but to their communication–this analyst would have no other frame of reference for the specification of this meaning than his or her own reflexive identity (e.g., Woolgar 1988). But what one has not distinguished empirically in the world, that is, the network as a communication system, cannot fully be compensated for by resources which are internal to the analyst.

For example, in Pickering's (1992) collection of discussion papers in the sociology of scientific knowledge, subjective reflexivity prevails to such an extent that these scholars no longer need empirical data for the update of their expectations; they need this data at most to illustrate their programmatic claims about how the sciences should or should not be analyzed. Luhmann (1990)–and to a lesser extent, Collins and Yearley (1992)–have already formulated this critique of the recent sociology of scientific knowledge. However, these authors did not elaborate an alternative in terms of empirical hypotheses which analytically relate the meta-theoretical reconstructions to the theoretical constructs under study in terms of expected and observable variances.

Luhmann (1984) identified the social system with communications among people, but did not formulate this sociology in relation to the mathematical theory of communication. Had he done so, he would have noted that communication is content-free: all systems which process uncertainty are communication systems. As communication systems they can be identified in terms of the (hypothesized) substance of the information which they process. On the one hand, Luhmann's equation of society with *the* communication system, in contrast to, for example, individual consciousness systems ('actors'), highlighted the qualitative difference between the substantive meanings of information in either system, that is, the 'Sinn' of the communication with reference to individuals and with reference to the relevant networks (cf. Habermas and Luhmann 1971). On the other hand, Luhmann's definition of information as reduction of complexity (e.g. Luhmann 1984, at p. 103; see section 13.1 above) obscured the relation between the formal mechanism and the substantive meaning of the various operations for these different systems of reference.

The reduction of uncertainty is achieved by organizing the uncertainty, that is, by providing it with a meaning. All systems which process information (that is, change in terms of their information content) are communication systems among other communication systems which by communicating make and possibly process their own history. However, systems with a memory function can process their history with reference to their structure. The memory function of the social systems is distributed among the actors, while the actors contain a memory function which is distributed in their brains. As systems of reference, social and psychological systems are 'incommensurably' different, since the self-referential axes are orthogonal, while they disturb one another in the interaction. Therefore, one expects the systems to be primarily delineatable in terms of the substance of their transmissions (and less so in terms of their reflexive operations).

13.5 METHODOLOGICAL AND THEORETICAL CONCLUSIONS

The advantages of understanding the sciences as dissipative structures that produce probabilistic entropy in various dimensions are manifold. I shall briefly summarize some methodological advantages for sociology that have been specified in previous chapters, and then elaborate on theoretical implications.

13.5.1 Methodological conclusions

First, since probabilistic entropy can be considered as a measure of complexity (Theil 1972), the mathematical theory of communication enables the analyst to relate the uncertainty in the content of scientific knowledge systematically with variation in its social and cultural contexts (cf. Whitley 1984). Each variation contains an *un*certainty which can be expressed in terms of the expected information content of a message. Since information and its communication are defined mathematically, and are therefore content-free, the analyst gains access to the study of interactions and relations which hitherto remained the domains of separate fields of study.

For example, by using information calculus one is able to relate the variance in sociological data to probability distributions for scientific hypotheses (e.g., Hesse 1974; Howson and Urbach

1989) or co-occurrences of words in scientific texts (cf. Hesse 1980; Callon *et al.* 1983). All these uncertainties can be considered as dimensions of the probabilistic entropy of the system(s) under study (Chapter Seven; cf. Krippendorff 1986).

Secondly, in addition to the analysis of relations, all formulas can be developed into dynamic equivalents so that the time dimension can be addressed systematically (Chapters Eight and Nine). For example, the scientific journal system produces a yearly distribution of publications (citations, etc.) over nations. But what do these distributions indicate? The observed distributions can be assessed with reference to different systems, e.g., the journal system, the division of scientific labor over nations, and/or the development of the international science system. With reference to which system(s) do the indicators exhibit change, and which systems were (sometimes implicitly) presumed to remain stable during the period under study? When is change to be considered as significant, structural, and/or irreversible? Using information theory, the uncertainty which is observable at different moments in time can be understood as part of the probabilistic entropy of the systems which generated this variation over time.

Thirdly, since entropy measures can be written as a summation, this methodology allows for the modelling of uniqueness: any development can be considered as a result of discrete events. Communication systems operate by communicating information with other communication systems in *contingent*, that is, historical relations. The observable data is then considered as information that is communicated among communication systems which themselves contain only expectations. The historical uniqueness of the events is a consequence of the contextual complexity (and not the other way round). However, if the historical development is not to be considered only in terms of what can uniquely be observed–as in an ideographic account–the sociologist has to specify also the systems that communicate through this data. This places an observable event among ranges of possible alternatives.

The discussion of the development of distributed systems over time led us from Chapter Ten onwards increasingly to the specification of possible interactions and co-evolutions among operations in a second-order cybernetics. The distributions under study can be considered as systems of expectations about the ranges of their possible developments: irreversibility, path-dependence, co-evolution, and auto-amplification could be specified as special cases. In Chapter Eleven, the focus was on testing for the possibility of emergence as an inter-system dynamics, and in Chapter Twelve, the consequences for the update of distributed knowledge representations (in networks) were specified. In this chapter, finally, I specify the theoretical surplus value of the second-order paradigm for the sociology of scientific knowledge.

13.5.2 Theoretical conclusions

As is well known, Kuhn's (1962) *The Structure of Scientific Revolutions* has been seminal to the development of science studies during the last decades. By focusing on paradigms as products of social relations among scientists, issues from sociology could be brought to bear on the study of the sciences as intellectual enterprises. Attention was drawn to contexts as opposed to contents, while the latter have been the traditional domain of the philosophy and history of science.

I have introduced the multi-dimensional model in Chapter One in order to indicate how different discourses may refer to the same phenomena, while they at first appear to be incommensurable. The scientometrics enterprise, however, indicated that there is also a sense in which the sciences are amenable to measurement even if, for the time being, one is not able to understand what scientometric indicators mean in other than pragmatic terms. Scientometrics therefore has challenged our understanding of what is measurable about science, and why.

If one extends the picture in *Figure 1.1* (p. 4) with the time dimension, each plane provides us with another window on the subject of study. If these reflexive windows are understood as subsystems for the understanding, the question arises whether a meta-system can be conceptualized. But the message of the postmodern philosophy of science has been precisely that such a comparison among 'incommensurable paradigms' is no longer possible (cf. Kuhn 1962). I shall argue below that this conclusion has been based on understanding the sciences in terms of evolving language games without sufficient reflection on the geometrical character of narrative metaphors (cf. Rorty 1979; Shinn 1987; Haraway 1988).

A (scientometric) meta-understanding requires the transition from geometrical metaphors (like trajectories) to calculus and algorithmic simulation. While discursive reasoning tends to become confused when both the categories that span the window of the representation, and their values are allowed to change, the understanding in terms of fluxes (dx/dt) precisely enables us to distinguish between structural change and variation. However, let me first return to the sociological understanding.

As noted, the multi-dimensional model of *Figure 1.1* was still static. The various contexts condition the interaction, and they determine one another in the interaction at each moment in time (*Figure 7.1*). Thus, the contexts are the structures that 'enable and constrain' the development of the sciences as aggregated action systems (Giddens 1979; *Figure 9.10*). The dynamic extension of this configuration (*Figure 9.11*) provides the analyst with a dual perspective: the static perspective of the multi-variate system–indicated as horizontal arrows in *Figure 13.2*–and the dynamic perspective of the evolving system–indicated with the vertical arrow–use orthogonal metaphors for the understanding. Therefore, the two perspectives imply a 'gestalt switch' in the window of the representation.

The sociology of science has focused on contextual disturbances, while the self-organization paradigm takes evolutionary development as its frame of reference. From the latter perspective, Kuhn's paradigms redefined the sciences as evolving communication systems with a life-cycle. Paradigms can be considered as self-organizing communication systems that have stabilized at the supra-individual level: the paradigm delineates what can be communicated within it. The delineation is in terms of the substance of the supra-individual communications, and therefore the system opens itself to whoever is able to communicate in these terms. The self-organizing system is (partially) decomposed and recomposed in each local operation. But what is to be considered as relevant, that is, what is communicated in these interactions and what not, is controlled by the communication system, while the communicating agents only relate to the development of the communication.

Given a new update, the self-organizing system may have to be redefined in terms of its substance, and its history may have to be rewritten. The paradigm itself, however, is not able to write or rewrite its history. Although social systems may have storage capacities, they have no central memory available for operations other than the distributed memories of actors. The social network is a virtual system–that is, the dynamic equivalent of a latent network–that operates at the addresses of the actors involved (Luhmann 1984 and 1990). Therefore, each reconstruction with respect to the systems under study remains an empirical hypothesis ('a representation') by one or more local actors.

Alternative reconstructions introduce uncertainty at the reflexive level. When this uncertainty can be communicated, there is sufficient complexity (since there is both a substantive and a reflexive layer of the communication, and thus an additional degree of freedom) for (provisional) closure of the communication system. The paradigm can be closed when one is able to alternate between substance and reflection in processes of mutual shaping, and to improve on the trade-off with hindsight. 'Variation' and 'selection' can then be considered as sub-dynamics of the co-evolving systems of discursive reasoning.

From this perspective, a *paradigm-shift* can be considered as a fundamental reorganization of the communication system; what previously could only be understood as anomalies can henceforth be integrated. In other words, the nature of the communication, that is, what is expected to be communicable within the communication system, shifts. Kuhn (1962) has used the concept of 'incommensurability' among paradigms: self-organizing systems differ among themselves in terms of *what* can be considered as communication. This is the case not only synchronically, but also diachronically:

self-organizing systems may go through path-dependent transitions. Thereafter, they are expected to have changed in the nature of their communication. Since change has to be produced locally, subsystems in differentiated systems (e.g., specialties in the sciences) may go through path-dependent transitions in one dimension while maintaining significant continuity in others. This opens the possibility of various intersecting discourses. For example, both participants and meta-theoretical analysts can communicate about the system on the basis of their reconstructions; these reconstructions may partially overlap.

In other words: in each round of operation the self-organizing system ('the paradigm') is confronted with a surplus of information ('variation') which is offered for processing at the interfaces. Without such 'variation' there can be no 'selection' by, nor 'stabilization' of the reconstructed system. Stabilization requires selection with reference to the history of the system under reconstruction. In order to maintain identity, the static and the dynamic selection–the two selections are formally equivalent!–have to operate on one another. A scientific paradigm is expected to contain both selection mechanisms: for example, new evidence can be countered by an *ad hoc* hypothesis that is otherwise unrelated to the historical organization of uncertainty in the discipline, or in the other extreme the new evidence can be 'counter-factually' disregarded as an anomaly ('noise') by means of an appeal to the stabilized 'hard core' of an existing body of theories (cf. Lakatos 1970). Only if a specific trade-off between the two selections is elaborated can the paradigm be strengthened as a theoretical system. The elaboration of the trade-off implies a (third!) selection with reference to the current state of the system.

Keep in mind that all actions of the communication systems have to be taken locally; all reconstructions contain uncertainty that can be socially communicated. The systems do not exist otherwise than in terms of uncertainties, that is, as expectations with reference to distributions. Since the systems are different with respect to *what* they communicate, the primary specification for the reflexive reconstruction is a theory about the systems under study in terms of the substance of their communications. The meta-theoretical analyst is reflexively aware of the reconstructed nature of the subject matter, while a participant may wish to build on his/her reconstruction for making an argument or for taking action. Neither an analyst nor a participant, however, can abstract from content, but the meta-theoretical analyst has to formulate this hypothesis reflexively.

The specification of the systems at the meta-level enables the reflexive analyst to *position* the observable *relations* as action with reference to the relating units, *and* as communication in the hypothesized networks (e.g., the specialty under study). Since the systems of reference have to remain empirical hypotheses, the networks under study remain uncertain, for example, in terms of their boundaries. But without a (sometimes implicit) hypothesis, the relevant networks cannot be specified, and thus there can be no empirical delineation. Analogously, the question of whether a *paradigm-shift* has occurred or not can only be evaluated empirically with reference to the communication network, however uncertain it may be in terms of observables.

When studying the history of social networks, one should never equate the uncertainty in the constructed network with the sum of the uncertainties in the constructing units that are observed. First, one expects interaction terms that have to be attributed to the network. Second, not only the analysts reconstruct the networks under study, but also (some of) the actors under study are able to do so. Reflexive actors are expected to contain more uncertainty than their representations ('actants') in the network. The representations of the actors in the network are equal only to the mutual information in the window between the actors and the network (see Chapter Twelve). The network is selective with respect to the variations produced by the actors, since it is able to transmit messages only in its specific medium.

The incoming information in the first (yet undifferentiated) dimension of a communication is positioned by the transmitting system using the network's extension as a second dimension. The information in the message has to be reconstructed by receiving actors who have then to perform a second selection. If these reconstructions are again communicated, a reflexive dimension is added to the probabilistic entropy of the network. The network contains then potentially three dimensions of the probabilistic entropy at each moment in time: the substance of the information, its (positional) context, and its (reflexive) meaning. In principle, such a system is sufficiently complex to redirect or to translate input into output at each moment in time. The addition of the dynamic perspective to this 'unit of communication' creates an entropical system that communicates in a hyper-space of four dimensions: it not only reflects, but the quality of the reflection can also be changed with hindsight.

A reflexive communication system translates input into output; a next-higher-order self-organizing system is additionally able to select among various reflexive meanings with hindsight, and with reference to its identity or its *regime*. A regime can be considered as the 'identity' of a distributed system.[128] Note that the regime of a system can be globalized in relation to a stabilized representation of its past or its complexity in three dimensions (e.g., a trajectory). If the distributed system is operationally closed, the input no longer functions as an instruction, while the output is the probabilistic entropy generated by the operation of the system (see *Figure 13.2*; cf. Maturana 1978).

In summary, by specifying the sciences in terms of reflexive communication systems that can exhibit evolutionary life-cycles, the sociology of scientific communications has provided us with a mental model for the reformulation of sociology as a science about social communication systems. Since the social system is a distributed system without central memory, one can only gain access to it through the reconstruction at one's local node. Participants and analysts can communicate about their reconstructions, but in potentially different layers of reflexive communications. Reflexivity in the communications provides the network with sufficient complexity for allowing the operation of various sub-dynamics in the communication network. If a self-organizing paradigm or a technological regime (Dosi 1982) emerges

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[128] Thus, an identity can be operationalized as a 'difference' (cf. Lyotard 1979).

the uncoupling of control in this system from the generating institutional structures and localized actors is a possibility, since self-organization is based on changing *network* properties.

The needed paradigm switch requires the reader to combine Luhmann's (1984) understanding of society as an 'autopoietic' communication system with Shannon's (1948) decision to take probabilistic entropy as the single fundamental concept. The parsimony in Shannon's definitions has enabled us to integrate the systems-theoretical notion of probabilistic entropy with the sociological notion of statistical variance and the theoretically important notion of scientific expectations. The recursivity of the concept allows for the repetitive extension of the system with reflexive layers. Additionally, Luhmann's distinction of society from (aggregates of) actors has enabled us to objectify the study of social phenomena.

Remember that this objectivation means the specification of an uncertainty, and not a reification. For example, one expects a self-organizing science system to be able to balance theoretical uncertainty against social variation within certain margins. The specification of the mechanisms in these translations is an empirical task. As noted, the development of communication systems is content-specific, but content is not the only relevant dimension. Case studies can teach us relevant contexts, and thereby inform our hypotheses with respect to possible selections and translations.

13.6 RELEVANCE FOR THE SOCIOLOGY OF SCIENTIFIC KNOWLEDGE

While in more traditional sociologies and philosophies of science the social and the cognitive were conceptualized as separate domains (e.g., 'the context of discovery' and 'the context of justification'), the post-Kuhnian paradigm has emphasized the indivisibility of 'socio-cognitive interaction.' The new line of research has focused on the dynamic development of the interaction between the cognitive and the social, for example, in 'socio-cognitive practices' (see, e.g., Pickering 1992).

If one is reflexively aware that the outcome of interaction is again a complex which can be analyzed in terms of the various dimensions involved, then one must assume that in each instance the complex has changed in some respects, but may have remained the same in others. Subsequently, one is in need of a model to attribute the observed changes to the various dimensions and/or their interaction. In terms of methods, this problem can be formulated as the problem of combining the multi-variate perspective (e.g., of the relations between the research group *and* the scientific field in cognitive *and* social terms) with the dynamic perspective.

The study of the relations between multi-variate data-structures and dynamic developments is not a sinecure. The time dimension adds a specific variate to the multi-variate complex: methodologically, it introduces auto-correlation in the observations, and theoretically, this auto-correlation in the data indicates self-referentiality in the systems that generated the variations under study. In other words, some complex data structures are reproduced during the operation, but the resulting structures may have changed with reference to their previous state(s). (As shown in Chapter Ten, whether or not distributions develop as systems remains an empirical question in each instance.)

Given the then unsolved methodological problems of how to attribute the observed data to the communication systems involved and/or their interactions, researchers in science studies had to make programmatic assumptions. Although results based on such assumptions may highlight important theoretical aspects, a mathematical sociology of scientific communication enables us to reflect on the value of these results with hindsight.

For example, if one wishes, as some micro-constructivists or post-modernists do, to deconstruct all structure in terms of actions or interactions, one has to assume that all structure is only a result of (inter-)actions *during the period under study*. In other words, the analysis in terms of contingent relations among actors (that is, lower-level units) does not deconstruct all structure, but only that part of structure which is decomposable in terms of relations among actors within the relevant time horizon. If, given this time span, structure is partially self-referential, that is, refers to itself at a previous moment, the micro-constructivist will be able to explain only the interaction terms between actors and structure *within this time span*.[129]

The history of the system under study never begins *de novo* during a period under study. As noted, even the emergence of a newly constructed system can be specified historically only with reference to previously existing networks. The eigen-vectors of the network of relations are latent dimensions which condition ('enable and constrain') action, but cannot be decomposed in terms of relations. However, the analysis of the observed distributions in terms of *relations*–as pursued by the constructivists–cannot be used for the study of *positions* with reference to the (sometimes implicitly) assumed systems under study (Burt 1982). The programmatic assumption in the relational or micro-constructivist program is therefore self-defeating: in empirical research the micro-constructivist is no longer able to distinguish the extent to which action and structure are each other's determinants, or only each other's enabling and constraining conditions (Giddens 1979 and 1984). The distinction between aggregated relations and positions with reference to latent dimensions of the network has not yet been sufficiently reflected in this tradition (Leydesdorff 1993b). I return to this distinction below, but let me first point to a related issue.

The strong program in the sociology of scientific knowledge (see Chapter Two) has wished to explain all cognitive variation in terms of its socio-cognitive construction. But is it feasible to explain cognitive variation in terms of socio-cognitive variation? It makes no sense to explain a larger variance in terms of a smaller one. The programmatic assumption that the cognitive uncertainty in science

[129] Knorr-Cetina (1981, at p. 27) argued that 'unlike the natural sciences the social sciences cannot hope to get to know the macro-order conceived in terms of emergent properties: they are methodologically bound to draw upon member's knowledge and accounts, yet ramifications of unintended consequences by definition cannot be part of social knowledge.' However, this argument ignores the fact that a reflexive analyst can develop an hypothesis that is not (yet) available to the participant.

is contained within the socio-cognitive interaction, and would thus be explainable in sociological terms, implies a highly unlikely answer to an empirical question on *a priori* grounds (Slezak 1989).

This programmatic assumption has been introduced in science studies by Bloor (1976, pp. 40ff.) with reference to Durkheim's analysis of the forms of religious life. Indeed, the thesis that the sciences sometimes function as belief systems can be empirically fruitful (e.g., in controversy studies), but it does not prove that the sciences are not also different from belief systems in important respects. The analogy misses the point that as a scientist, one is free to theorize in modern societies, that is, that the sciences, unlike normatively integrated forms of religious life, can also be considered as functionally differentiated subsystems. For example, someone's disbelief in a scientific 'truth' no longer necessarily creates a schism between two religious communities, as in the Middle Ages; nowadays it often raises only a variety of further research questions. Thus, the mechanism for the communication of uncertainty is different. Modern sciences are not hierarchically organized belief systems, but at least to a certain degree, they are also juxtaposed discursive constructions.

'Belief' refers to one or more actors who expect something to be true. However, the sciences have been socially constructed as *discursive systems of rationalized expectations* not only in terms of their daily operations as practices, but in a much more profound sense, i.e., in their relation to society at large, and notably with reference to religious systems. At various places, Weber, Parsons, Merton, and Luhmann noted the importance of Protestant values for the emergence of modernity in general, and the differentiation of science in particular.

How is one to understand the thesis of functional differentiation in relation to 'the Durkheimian program' in the sociology of scientific knowledge? In contrast to theoretical expectations, a belief system must be normatively integrated and hierarchically organized with reference to a codified meaning of Truth. As noted, a belief can be attributed to a community of people (e.g., a church). In Catholicism, the institutional organization of the church is hierarchical: since the Pope is considered the Vicar of

Christ, His Holiness is at the top of a *stratified* organization which covers the whole world ('kat' holèn gèn').[130] In Protestantism, each individual is equal before God, and therefore the world is given to people as *a latent structure in their network of relations*. But the Word tends to remain unambiguous in the religious communication.

With reference to the network, truth in scientific theorizing, the prices on the market, love in personal relations, etc., can become functionally differentiated from normative integration into individual or collective belief systems. These differentiated media of communication are potentially delocalized network functions; in a secularized society normative integration can eventually be considered as a recursive network function (cf. Maturana 1978). The different functions tend to be orthogonal (Simon 1969), and thus hierarchical stratification is then fully replaced with functional differentiation.

Scientists have had a particular need for functional differentiation, since they need room for provisional interpretations that they may wish to change with hindsight. The sciences can allow for normative control only over the conditions of the communication (e.g., resource allocations), but not on the substantive or the reflexive contents of these communications. Thus, the differentiation from normative integration has been a functional requirement for the further development of natural philosophy, that is, the new sciences. This crucial conflict was fought in Western Europe between the appearance of Galileo's *Dialogo* in 1632 and the publication of Newton's *Principia* in 1687. From that time onwards, functional differentiation has been further institutionalized in the social system (Leydesdorff 1994d).

What has been the precise cultural role of the new philosophy? Why was it able to drive this development? By reconstructing 'nature' in an experimental setting, the observation is transformed into an instantiation with reference to an expectation. Insofar as this reconstruction proves successful, i.e., historically stabilized, nature may then be *replaced* with the representation of nature.

[130] The word 'hierarchy' is derived from the Greek words 'hieros' and 'archein' which respectively mean 'holy' and 'reign.'

Consequently, the one paradigm can overwrite the other. In terms of the model, this may be an evolutionary event or a more gradual replacement. But after the replacement of the old paradigm, it tends to lose also its meaning. For example, it is difficult for us to understand *why* Huygens rejected Newton's concept of 'gravity' as completely 'absurd', while he was otherwise impressed by the *Principia*.[131] Equally, we no longer understand *why* it seemed important to Medieval physicians to let sick people bleed. Nowadays, we understand 'gravity' and 'blood pressure' as intuitively meaningful concepts. This translation mechanism in the reconstruction drives cultural evolution or, if one wishes, modernization. Scientific discourses provide the other subsystems of society with a reflexive window on the uncertainty contained in whatever is represented in the representations. By understanding the events as instances of ranges of possibilities, recombinations can be invented that may be better adjusted to the new contexts than the coevolutions which hitherto occurred in 'nature.'

This specific function of scientific communications for social communications at large, makes the problem of 'science and society' subsequently so difficult. The 'socio-cognitive (inter-)action' of the scientists under study operationally couples two communication systems (e.g., the 'social' group and the 'cognitive' field). But the social system cannot operate without actors contingently doing so, and thus the scientists involved are structurally coupled to the socio-cognitive communication in both dimensions. Each action has therefore at least four meanings: one has to crosstable actor(s) and communication networks as systems of reference versus the dimensions of differentiation in order to explain the interaction as a result.

Thus, the student of issues of 'science and society' has to analyze two problems at the same time, *viz.* the sociological question of the dynamic relations between individuals and society (social action and social structure), and the epistemological question about the relations between the knowing subject and the sciences as

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[131] Letter to Leibniz, 18 November 1690. (C. Huygens, *Oeuvres Complètes*, Vol. IX, at p. 538.)

cognitive communication structures. 'Socio-cognitive' communications contain both substance and reflexivity, and thus the observed data can be attributed to more than one dimension of the differentiated system. Consequently, there is reconstructive flexibility in the attribution of action over the social and the cognitive contexts (Hesse 1980).

Additionally, the update frequencies in the time dimension have to be specified. Both the research group and the scientific field are part of the larger social system. In a hyper-cycle the various cycles, i.e., the one of the individual and society, and the one of the knowing subject and scientific truth, can be integrated into one (super-)system, but with potentially different frequencies. The (sub-)systems constitute at a next-higher level the units for a second-order cybernetics which reproduces their differentiation, and thereby also secures the integrity of the lower-level units (Maturana and Varela 1980; cf. Simon 1973). Historically, the semantics for this configuration have been shaped by the Protestant demand for freedom of religion; systematically, the internal processing of meaning in (otherwise juxtaposed) systems is crucial. Note that the specific form of the differentiation is also historically contingent, and thus subject to evolutionary change (Teubner 1987).

13.7 RELEVANCE FOR THE SOCIOLOGY OF TRANSLATION AND CO-WORD ANALYSIS

The integrity of lower-level units was propounded in science studies by representatives of the sociology of translation, or the actor-network approach. It was extended beyond the domain of human beings: 'non-humans,' such as electrons, scallops, and texts, may equally be related to the actor-network as 'actants' (see, e.g., Callon and Latour 1981; Callon *et al.* 1983; Latour 1987a). In the sociology of translation, all heterogeneity is engineered into an actor-network by relations (Callon and Law 1982; Callon *et al.* 1983). The resulting (semiotic) network contains nothing but relations ('associations'); on the basis of this programmatic assumption there is simply no room left to discuss positions.

Correspondingly, Latour's 'macro-actor' is not an 'eigen-vector' or a 'density' in the network, but an *identifiable* 'translator' (e.g., 'The Prince' in Latour (1987b)) who relates hierarchically to all the units involved. Analogously, the 'macro-term' in the co-word network is a star in a graph, and not only an abstract position in a multi-dimensional space (cf. Callon *et al.* 1989; Courtial 1989). The relational algorithm which was used for the reconstruction does not allow for positional analysis.[132]

This lack of options to specify heterogeneity in terms of hypothetical factors deprives the analyst of the possibility of distinguishing among causes for events in the network, and *a fortiori* of distinguishing between causes and reasons (of actors) with reference to these events. Consequently, Latour (1987a) can no longer decide whether Galilei was condemned because of a social failure or a cognitive error, or whether Lysenko was celebrated for his theoretical contributions or his ideological commitments (cf. Amsterdamska 1990). Again, the programmatic assumption has become counter-productive: the assumed unity of the network, which seemed fruitful for its dynamic analysis–since it projected the multi-dimensionality in the construct on the single dimension of associations along the time axis–prohibits with hindsight the systematic distinction of aspects in which the network changed from those which remained the same. Correspondingly, the relational algorithm used in co-word programs (e.g., LEXIMAPPE and CANDIDE) signals change, but it cannot find continuities (Leydesdorff 1992c).

One should keep firmly in sight that a network has an architecture, and that this architecture can be represented in terms of an eigen-structure at each moment in time. The implied structure makes it possible for the actors involved, among other things, to distinguish between experience and action (Luhmann 1990).

[132] Callon (1990) allowed for institutional differentiation between 'poles' in the network. However, this differentiation is 'given' for the network, and not conceptualized in relation to it. In another study, Latour (1991) advocated de-differentiation as the main programmatic message of this sociology to political philosophy.

Receptive experience is dependent upon one's position, and the reflexive attribution of meaning to the incoming signal remains internal and discretionary to the actor who experiences and reflects. The 'actor' generates uncertainty according to a different program from that followed by the 'actant,' that is, one's representation in the communication network. The representations provide us with network addresses, but not with sufficient information to infer whether the represented units are reflexive agents or not. The reconstructive analyst needs to add this information as an hypothesis. This distinction between reflexive agents and irreflexive 'actants' is crucial: without *reflexive* agents the network would not be able to translate, but function only as a Shannon-type–that is, transmitting–communication channel.

The distinction between perception and reflexivity which remain discretionary to the actors, and observable communications in a network, is a prerequisite for the understanding of what 'truth' means in social communications. The reflexive processing of meaning by each of us according to our internal program belongs to our heritage from the individualistic, and among other things 'scientific,' revolutions–whatever form this differentiation may take today. If, for programmatic reasons, one wishes to focus only on externally observable actions, without the specification of the various systems of expectations for which these actions are relevant events, one is no longer able to distinguish between internal processing (including the reflexive attribution of a truth-value to an information) within a system and its external effects (e.g., performance or economic success); and thus one deprives oneself *a priori* of the possibility of inspecting uncertainties in the truth and objectivity of scientific knowledge itself other than in terms of representations or measurable proxies (e.g., Whitley's (1984) reputations).

Paradoxically, therefore, while Bloor (1976, p. 1) stated that '(t)here are no limitations which lie in the absolute or transcendent character of scientific knowledge itself, or in the special nature of rationality, validity, truth or objectivity,' he proceeded to deprive himself of the possibility of explaining these concepts by insisting on their operationalization exclusively in terms of observable 'socio-cognitive actions.' He, and others who followed him in this respect, have equated the unit of analysis with the unit of observation.

It matters for the development of the discourse at the level of the network *what* the actors believe. The specification of these interactions requires a richer model than *a priori* assumptions about symmetry allow for (cf. Mulkay *et al.* 1983; Chapter Two). Bloor was right that there is no need for the assumption of an absolute or transcendent character of scientific knowledge; self-referential and interactive processing of sociological and psychological meaning may or may not generate, for example, 'truth' as a function of the scientific communication. However, these are not symmetrical operations, since the actor's actions and beliefs are expected to make a difference in *other* dimensions. The assumption of symmetry is counter-productive because it may lead foreseeably to distorted representations.

In summary, 'rationality, validity, truth or objectivity' are attributes of specific interactions between the different self-referential systems involved. No scientific truth can be communicated without reflexive scientists contingently doing so; without a message in a medium (e.g., a text or a discourse) which is codified enough to communicate the complex information; without a cognitive structure (e.g., a theory) which is sufficiently differentiated so that, among other things, the truth of the message can be distinguished from its information value.

The analyst should *not* take the manifest 'socio-cognitive action' as a unit of analysis or at face-value. These 'socio-cognitive actions' should themselves be deconstructed as events in which the social and the cognitive dimensions co-vary with reference both to the actors involved and to the relevant (differentiated) networks. Two structurally coupled systems, the scientific agent and the scientific discourse, couple operationally in a single communication, but in various dimensions.

Analogously, scientometricians should *not* take words or citations by themselves as units of analysis and at face-value. Words are the prime examples of nominal variables; variables are attributes of units of analysis; and therefore, variables have meaning only with reference to their unit of analysis. For example, words have no meaning except in the context of a sentence (Bar-Hillel 1955). Changes in word-patterns are only the observable outcomes of various processes of change which may have taken place in the systems indicated by them. For example, while in a given scientific text one may find amazingly consistent word patterns, between two texts changes in word patterns may indicate substantive and/or semantic changes. The specification of these dimensions, and of whether change is expected to occur in them stochastically or systematically, may lead to different expectations for their occurrences in subsequent operations.

13.8 THE FURTHER CHALLENGE OF SCIENTOMETRICS

The program formulated above seems to share with the program of logical positivism the feature that a model of science from the natural sciences is generalized to the social sciences. However, this is not the case: logical positivism specified a normative (*a priori*) model in order to achieve a basis for ruling out categories of statements as meaningless, and to guarantee increasing certainty about the truth-likelihood of the other statements. I have emphasized the empirical, that is, *a posteriori*, perspective without reducing this position to an empiricist one. The central question has been what one is able to learn at the theoretical level from the various sciences which are relevant for the study of the sciences.

For example, the sociology of scientific knowledge and the sociology of translation, because of their emphasis on constructivism, have provided us with a semantics for studying the sciences reflexively, as discourses and in terms of historically changing networks. However, the sociology of scientific knowledge became entangled in the so-called 'reflexivity problem': the various layers in the reflection could not be sufficiently distinguished. These authors eventually did not attribute reflexivity to the network (e.g., Woolgar 1988; Collins and Yearley 1992), while the sociology of translation postulated that the network could be translated without any theoretical distinctions between reflexive actors who are (sometimes) able to represent themselves actively, and 'actants' that are represented passively.

Reflexivity has to be specified with reference to a communication network differently from its specification for a human being. Otherwise, one either has to overburden the reflexive actor with hyper-reflexivity or to underestimate the complexity of the problem by claiming equal potentials of reflexivity for all 'actants' in the network. Furthermore, the reflexive argument may easily become overburdened, when the same term can have different meanings with reference to different systems, and when these meanings can additionally change over time. This has been noted ironically within this tradition, but it has not been elaborated systematically (e.g., Hicks and Potter 1991).

In a sociology of scientific communications, there are first the systems of communication under study. These systems contain and process information; they disturb one another in the interactions at local nodes (actors). The variation at each moment in time contains uncertainty that, in the dynamic analysis, can be considered as part of the probabilistic entropy of the system(s) that generated the variation. Second, the discursive reflection in human communication adds a layer of communicating 'meaning' to the information exchange. I have argued why one should not reduce information to meaningful information (cf. Eco 1976); the two categories have to be cross-tabled instead.

In *scientific* discourses these reflexive operations are more transparent than in common language because of the higher degree of codification. Initially, the sciences shared this high degree of codification with the religious system, but the achievement of the scientific revolution can be understood with hindsight as resolving this 'fixed' codex into a reflexive discourse (Leydesdorff 1993d). Both levels (that is, the substantive and the reflexive) have from that time onward been developed as degrees of freedom, and the interaction between the two can sometimes be stabilized into a specific paradigm.

The difference between a belief system and discursive reasoning in a scientific communication system is that reflexivity can be understood as a degree of freedom and not as a higher-order priority (like the Truth). The truths are constructed in the discourse. Thus, the concept of layers on top of one another can be replaced with the concept of a complex construct with orthogonal dimensions.

The formal analogy between reflection within the sciences and reflection about the sciences as subject of study (Kuhn 1962) should not be taken as indicative of a substantive similarity. On the contrary, the reflections are *a priori* expected to be orthogonal, and therefore substantively different, i.e., 'incommensurable.' They interact in the event, but what the events mean is different for each reflexive system. The various subscripts of different reflections and different moments in time have to be kept analytically separate. But as noted, the discourse may become confused when such 'hyper-reflexivity' is demanded: when both the categories and their values are in flux, one needs algorithmic code to specify subscripts to all the categories in the different loops. The geometrical metaphors of theorizing in natural languages are no longer sufficiently codified for handling the complexity. Remember how we met this problem of meta-theorizing already in Chapter Five when we had to conclude that word co-occurrences changed both in frequencies and in meanings among texts.

In summary, a mathematical sociology of scientific communications challenges the epistemological status of *discursive* reasoning in science studies: in order to understand the reflexive systems under study, one has to proceed from the (partial) understanding in terms of geometrical metaphors in the various disciplines to algorithmic simulations (Andersen 1993). The theoretical discourses constitute a layer of complex (reflexive) windows on top of the complex social system(s) under study. Each reflexive understanding, however, implies a reduction of the underlying complexity by choosing a specific–i.e., selective–perspective (Hinton *et al.* 1986). In other words, the 'phenotypical' behavior of the model system is more complex than its composing ('genotypical') dynamics, while only the latter can be made the subject of substantive theorizing (Langton 1989).

An algorithmic reformulation would enable us to identify the various dynamics in terms of fluxes. However, respecification of categories 'on the fly' has implications for the epistemological status of theorizing. On the one hand, discursive theories specify expectations about the sub-dynamics of the complex system under study (cf. Blauwhof 1994). On the other hand, the algorithmic model may enable us to reconstruct second-order interactions, and to distinguish the relative positions (and weights) of substantive specifications by comparing them in terms of the dynamic analogon of the part of the variance (that is, the probabilistic entropy flux) that they are expected to explain. The model outcomes subsequently challenge the theoretical understanding. The iterations of specification and testing then constitute the empirical research process.

If the theoretical discourses are considered as competing windows of understanding, the evolutionary expectation for further theoretical development is that they will tend to differentiate, since a differentiated system can process more complexity than an undifferentiated one. The various theories span a phase space of possible variations that can in principle be scanned algorithmically for the possibility of other sub-optima than the ones which have been achieved through evolution. Theoretical insights then specify expectations with respect to empirical states of affairs: the expectation implies a selection, and thus formulated it can be incorporated into the model system as a reduction of the complexity. (Without theoretical restrictions the number of possible combinations would rapidly grow non-computable.)

The further *Challenge of Scientometrics* is this positive and reflexive appreciation of the partiality of each perspective as part of the post-modern condition of scientific theorizing. The specification of an algorithmic model sometimes provides us with the possibility provisionally to close, and thereby to update, a hyper-cycle of interdisciplinary theorizing.

LIST OF ORIGINAL COMMUNICATIONS

The study is based on (revisions of) the following original communications:

Chapter I
- "The Scientometrics Challenge to Science Studies," *EASST Newsletters* 9 (1990, Nr. 1) 5-11.

Chapter II
- "The Relations Between Qualitative Theory and Scientometric Methods in Science and Technology Studies. Introduction to the Theme Issue," *Scientometrics* 15 (1989) 333-47;
- "The Knowledge Content of Science and the Sociology of Scientific Knowledge," *Journal for General Philosophy of Science* 23 (1992) 241-63.

Chapter III
- "Exchange on the cognitive dimension as a problem for empirical research in science studies," *Social Epistemology* 8 (1994) 91-107; 117-21.

Chapters IV and V
- "Words and Co-Words as Indicators of Intellectual Organization," *Research Policy* 18 (1989) 209-23;
- "In Search of Epistemic Networks," *Social Studies of Science* 21 (1991) 75-110.

Chapter VI
- "Some Methodological Guidelines for the Interpretation of Scientometric Mappings," *R&D Evaluation Newsletter* 1989, Nr. 2, 4-7;
- "A Validation Study of "LEXIMAPPE"," *Scientometrics* 25 (1992a), 295-312.

Chapters VII
- "Relations Among Science Indicators I. The Static Model," *Scientometrics* 18 (1990) 281-307.

Chapter VIII
- "Relations Among Science Indicators II. The Dynamics of Science," *Scientometrics* 19 (1990) 271-96.

Chapter IX
- "The Static and Dynamic Analysis of Network Data Using Information Theory," *Social Networks* 13 (1991) 301-45.

Chapter X
- "Irreversibilities in Science and Technology Networks: An Empirical and Analytical Approach," *Scientometrics* 24 (1992) 321-57;

(with Peter Van den Besselaar), "Technological Development and Factor Substitution in a Non-linear Model," *Journal of Social and Evolutionary Systems* 21(2) ([1998] 2000) 173-192.

Chapter XI

"The Impact of EC Science Policies on the Transnational Publication System," *Technology Analysis and Strategic Management* 4 (1992) 279-98.

Chapter XII

"Knowledge Representations, Bayesian Inferences, and Empirical Science Studies," *Social Science Information* 31 (1992) 213-37.

Chapter XIII

"The Possibility of a Mathematical Sociology of Scientific Communication," *Journal for General Philosophy of Science* 27 (1996) 243-65.

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Author Index

Subject Index

Scientometrics—the quantitative study of scientific communication—challenges science and technology studies by demonstrating that organized knowledge production and control is amenable to measurement.

First, the various dimensions of the empirical study of the sciences are clarified in a methodological analysis of theoretical traditions, including the sociology of scientific knowledge and neo-conventionalism in the philosophy of science. Second, the author argues why the **mathematical theory of communication** enables us to address crucial problems in science and technology studies, both on the qualitative side (e.g., the significance of a reconstruction) and on the quantitative side (e.g., the prediction of indicators).

A comprehensive set of probabilistic entropy measures for studying complex developments in networks is elaborated. In the third part of the study, **applications** to S&T policy questions (e.g., the emergence of a European R&D system), to problems of (Bayesian) knowledge representations, and to the study of the sciences in terms of 'self-organizing' paradigms of scientific communication are provided. A discussion of directions for further research concludes the study.

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