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THE DEVELOPMENT OF FRAMES OF REFERENCES

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Measurement of the effectiveness of science policies is analyzed as a multi-level problem. Journal–journal citations are discussed as a potential candidate for a domain beyond the control of policy-makers and authors or research groups and therefore may function as a relatively stable and easily accessible baseline for the 'calibration' of outputs and outcomes of science policy. A method is developed, using *SCI*'s *JCR*s which is then applied to the two cases of water pollution and humanisation of labor. This method can also be used as a simple indicator for the development of journal–journal citation patterns over time.

Introduction

A central idea in science studies is that the contingent nature of scientific development leaves room for a more rational and effective science policy. If, as many authors in this field claim, science is in its core dependent on social variables, then it is theoretically possible to influence its development in the long run through the systematic alteration of such variables.[1] One programmatic assumption in this so-called 'cognitive sociology of science' has been that the traditional cleft between 'the logic of discovery' and 'the logic of justification' can be overcome by sociological analysis.[2] Consequently, there has been a tendency to reduce the study of science to the sociology of scientific practices.[3]

An important exception to this general tendency needs to be made for the so-called 'Starnberg'-group, whose members stressed the importance of integrating the philosophical dimension and therefore proposed to analyze the development of the sciences both in terms of their social and institutional structures *and* in 'phases' of cognitive development.[4] The main emphasis in the work of the Starnberg authors has been the elaboration of just such a theory of cognitive development.[5] However, in their final study[6] the 'Starnbergers' returned to their original question about the interaction between (a) the supposed dynamics of cognitive developments, and (b) the institutional dynamics in decision-making and in the implementation of science policy programmes. However, because both processes were at work in all their case-studies, they could not decide whether it was cognition or the social structure that

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was primarily responsible for the success or failure of the programmes. They had to conclude, therefore, that in those cases in which the political system had been successful in the implementation of the desired research programmes, this did not necessarily mean that steering on the cognitive level had also taken place.[7] The Starnbergers introduced the concept of 'parametric steering' of science to describe this incorporation of science into processes in which the results of science can be utilized without new scientific developments. The effects of such steering on science are rather trivial: more money for a field of science means more R & D-personnel, more publications, more social processes to be studied by science studies, etc. Such factors, however, do not say anything about the quality of the changes in science, which have been brought about. Therefore, we are introducing the term 'qualitative steering' of science – in contrast to 'parametric steering' – for those instances in which science policy efforts lead to new scientific developments at the cognitive level as well.[8]

Once 'parametric steering' has been analytically distinguished from 'qualitative steering', any study of science as *only* a social process becomes unsatisfactory: the substantive outcomes of processes are as important as the processes which lead to them.

This conclusion is at the root of the Amsterdam Science Dynamics programme: the analysis of scientific contents and the analysis of research institutes must be pursued as related but separate efforts.[9] *Science dynamics* is then essentially the multi-level problem of the combination of insights gained from these two complementary forms of analysis.

The specification of domains

With hindsight, we can state that the Starnberg authors failed to answer the interesting questions they raised because they did not draw from their theoretical deliberations the methodological conclusion that they had to specify domains for their respective hypotheses about cognitive dynamics ('finalization') and institutional dynamics ('transformation'). Instead, they tried to test both these hypotheses in the same domain.[10]

However, the analytical distinction should be used for a further discrimination of the relevant domains. In order to look at the structural 'outcomes' of science policy, it is necessary to adopt one type of theorizing and, by consequence, to specify different domains of empirical research, than in those instances in which we are interested in institutional 'outputs':[11]

1. More important than the social processes in science are the results to which such processes lead.

2. More important than the precise mechanisms by which these results are constructed is their value for the further advancement of science. This corresponds with a shift from social science to information science.

3. Correspondingly, there is a shift of interest from local and unique events to the cosmopolitan and universal meaning of such events.

4. Consequently, the demarcation between science and other types of cognition is again relevant, as it has been relatively less relevant in the more recent traditions of the sociology of knowledge.[12]

However, in our research the analysis of structural outcomes is not an end in itself, but a means to a better understanding of our multi-level problem.[13] Therefore, we are interested in a technique which can indicate developments at these more abstract levels without its conceptualization being colored by local events.

We found the aggregated journal–journal citations presented in the *Journal Citation Reports* of the *Science Citation Index* a very useful and previously underestimated tool for this purpose.

Journal–journal citations

As early as 1965, *Price* pleaded for a study of the topography of science through the analysis of journal–journal citation patterns.[14] In 1971, the Institute for Scientific Information (ISI), the publishing house for the *Science Citation Indexes,* decided to explore possibilities of journal evaluation through citation analysis. For that purpose the data for the last quarter of 1969 were aggregated journal by journal, and listings were composed which show how many times a particular journal was cited during that quarter. These figures can again be broken down to the respective volumes of the journal to which the citations were given. The same can be done the other way round: how many times is a particular journal citing another journal, instead of being cited, and which volume is it citing?

These two listings provide all the essential information about interjournal citing patterns.[15] They form the basis of the *Journal Citation Reports,* which ISI added to its indexing services in 1974. The *Journal Citation Reports* provide us with the data necessary to make the type of analysis, which *Price* recommended.

Carpenter and *Narin* did some systematic work on the relations among scientific journals on the basis of the explorative file for the last quarter of 1969.[16] They found strong, fully transitive hierarchies among journals within each of the disciplines, clear boundaries between the disciplines, and a few well-known cross-disciplinary journals served as cross-field information links. The existence of such strong hierarchies makes it likely that these patterns are stable over time. As yet, however, no longitudinal studies on journal–journal citation patterns have been done.

However, it is *not* claimed here that analysis at this level examines 'cognitive development' and that research conducted at other levels addresses the problems of 'social' or 'socio-cognitive' developments in science. Every development in science and in science policy has both social *and* cognitive aspects. Here we are looking at one particular level which consists of institutions, too![17] As aggregated results, however, the number of journal–journal citations can hardly be influenced by the intentions of authors or policy makers, and therefore we may hope to find at this level a means of describing the *cognitive* differentation of the sciences, one which can serve as an indicator for what we have called 'qualitative change'.[18]

Methods

Exploratory studies using the *Journal Citation Reports* have been extensively reviewed by *Garfield.*[19] To determine the core literature of a scientific field, *Garfield* takes a leading journal as an entry and then lists the journals, cited most frequently *by* this journal. In such a set-up the entry journal is 'citing', i.e., the data base is the Citing Journal Package. 'Citing' tells us more about the relevant environment for authors (in a journal), while 'being cited' tells us something about the influence of an author or a journal.

Although sometimes citing patterns are compared with the patterns of being cited, the general tendency in these studies is to use 'citing' rather than 'being cited'. In our set-up we will also take a core journal as an entry, but we will look primarily at its 'being cited' pattern, and use the citing patterns to check whether we have not overlooked important journals. There are some major reasons for this procedure:

1. It has been pointed out that starting from the citing patterns introduces some bias towards the more established journals (among them *Science* and *Nature*!).[20] On the other hand, starting from 'being cited' focuses more attention on new journals, which is exactly what we are interested in.

2. Our interest is in (longitudinal) development. In a certain year a journal *A cites* different volumes (i.e., many years) of journal *B*. The 'citing' rates of *A* vary with the years only and are therefore our variables, while the 'being cited' journals are the units.

3. By taking the journals as units and their citations as variables, our research design becomes analogous to normal citation analysis in which the authors represent the units and their citations serve as the variables. The importance of this correspondence for the long-term goal of multi-level analysis will be evident.

As *entry* we take a core journal – agreed on by experts – and list the three journals which cite that journal most heavily, and then iterate this procedure for the new batch of journals.[21] We can specify a cluster of journals if the method converges. As necessary, the collection was cut off at two arrows from the main entry.

From these data qualitative pictures can be produced which give a first idea of the main citation patterns in the collections under consideration.

For each collection (about 20 to 40 journals) it is possible to fill in a matrix containing the exact citation rates, as far as they are equal to or greater than 5 (because otherwise they are not listed in the *JCR*). In this way a matrix can be constructed for each year and for each collection from 1974 onwards. We will, however, for the sake of simplicity limit ourselves to the 1974, 1978 and 1982 matrices, and only extend our presentation to the intervening years when there are good reasons to do so.

We performed the following analyses:

1. Correlations between entire matrices – as far as they consist of the same journals – were computed and tested for significance.[22] (The collection is in this case regarded as one big cluster.)

2. Correlation matrices were computed both from each matrix and from its transposition, and used for factor-analysis and Q-factoring respectively.[23]

3. By equating a reference from journal A in journal B to a reference from B in A,[24] a symmetric matrix can be obtained for multidimensional scaling, which produces maps of the relative distances between the journals.[25]

Initially, cluster-analysis was explored as well, but because the results were highly sensitive to the choice of clustering criterion,[26] cluster analysis was discarded.[27] Graph-analysis was not pursued because it would not add substantially to the results obtained from multidimensional scaling.[28]

The cases

We selected two areas in which science policy efforts were made during the 1970s:

1. 'water pollution' as a challenge to biology;

2. the 'humanisation of labour' as an object for industrial medicine and chemistry.

Water pollution has been of great concern to the environmental movement which emerged in the late sixties. It has been one of the central topics of environmental research and research-programming in Holland.[29] The *humanisation of labour* is a related topic in that it emerged in the same period. In 1972 the 57th Congress of the International Labour Organization passed a resolution in which the qualitative conditions of labour were given priority. In 1974 the European Committee initiated a special programme on the humanization of labour, and in the same year the social democratic government in the German Federal Republic launched a major research programme 'zur Humanisierung des Arbeitslebens'.[30] The Scandinavian countries have also been particularly active in this area.

Of course, one can solve water pollution quite efficiently by building water treatment installations, without necessarily having to spend more money on hydrobiology. The same is true for the quality of labour: often a simple suction apparatus will perform miracles. However, in the 1970s both the policy makers and the scientists working in these areas (as well as the general public) were convinced that these problems should not only be solved in a practical way by controlling the effects of the modern technologies, but that they should also be used as feedbacks to change the development of science and technology through policy efforts. A decade ago these problems were seen as important issues for science policy, just as micro-electronics and biotechnology are today.

A first quantitative analysis of the development of scientific journals in these areas can easily be performed on the basis of the statistical material provided by *SCI* (Table 1.) Although there is hardly any increase in the total number of source journals

Table 1
Number of journals for different subjects

Years	Source journals	Environmental science (Water)	Toxicology	Analytical chemistry
1970–74 (acc.)	(2.772)	(27)	(6)	(34)
1975	2.540	27	6	36
1976	2.717	35	10	43
1977	2.655	45	19	47
1978	2.572	53	22	51
1979	2.993*	58	19	31
1980	3.067	61	20	36
1981	3.068	52 + 20	19	35
1982	3.246	51 + 20	27	38

*From 1979 appr. 200 monographic series titles are included.

from 1975 until 1978 (a period with a stable counting procedure), 'Environmental Science'-journals doubled during this period, and 'Toxicology' – journals even tripled. However, this method is too crude: if journals for more established fields such as analytical chemistry are counted, important differences appear. (This results from computer categorization performed on the basis of titles.)

Aquatic ecology

On the advice of experts, *Hydrobiology* was chosen as the entry journal for the composition of the matrix. This resulted in a cluster of 24 journals in 1974, 21 in 1978 and again 24 in 1982. The relevant journals are listed in Table 2.

Table 2
'Aquatic Ecology' – journals

*HB	1	Hydrobiologia
*HB	2	Archiv für Hydrobiologie
HB	3	Fresh Water Biology
HB	4	Internationale Revue der Gesamten Hydrobiologie
*LO	1	Limnology and Oceanography
*MB	1	Marine Biology
*MB	2	Journal of Experimental Marine Biology and Ecology
*MB	3	Journal of the Marine Biological Association of the U. K.
MB	4	Marine Ecology Progress series
MB	5	Marine Biology Letters
MB	6	Advances in Marine Biology
*WR	1	Journal Water Pollution Control Federation
*WR	2	Water Research
*WR	3	Journal of the Environmental Engineering Division-ASCE
WR	4	Water Science and Technology
WR	5	Journal American Water Works Association
WR	6	Progress in Water Technology
WR	7	Water Resources Bulletin
*FI	1	Canadian Journal of Fisheries and Aquatic Sciences
*FI	2	Transactions of the American Fisheries Society
*FI	3	Journal of Fish Biology
*FI	4	Progressive Fish-Culturist
FI	5	Aquaculture
*ENV	1	Applied and Environmental Microbiology
*ENV	2	Bulletin of Environmental Contamination and Toxicology
*ENV	3	Environmental Science and Technology
MR	1	Deep-Sea Research
MR	2	Journal of Marine Research
*DIV	1	Canadian Journal of Zoology
DIV	2	Science
DIV	3	Ecology
*DIV	4	Journal of Phycology
DIV	5	Biological Review

The qualitative pictures for 1974, 1978, and 1982 are very much the same. (Fig. 1, for the 1982 – picture.) The only change occurs among the relations between the four major constituent clusters: Hydrobiology (HB), Marine Biology (MB), Fishery

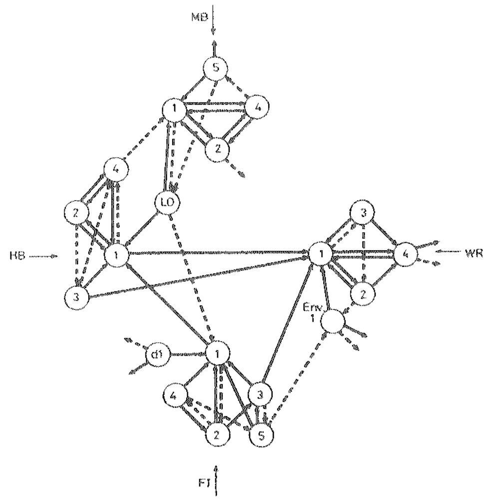

Fig. 1. Qualitative picture for 'aquatic ecology' journals in 1982 (from the Cited Journal Package)

(FI), and Water Research (WR). The 18 most important journals existed throughout the entire period considered here, and it will be mainly among these 18 that we can make longitudinal comparisons. These journals are marked with an * in Table 2.

Pearson-correlations between matrices for these 18 journals are presented in Table 3. The correlations between the matrices are significant at the 0.001-level, i.e., the matrices remain very stable. This means, that in our further analysis we are looking at the development of the 'fine structure' of the cluster.

Factor-analysis of the 1974, 1978 and 1982 matrices yields a sharp insight into this fine structure.[31] Variables (the aggregated citing of journals) generally load on

Table 3
Pearson correlation matrix for 18
'Aquatic Ecology' journals

	1974	1978	1982
1974	1.0000 (0) p=******	0.9477 (142) p= 0.001	0.8526 (138) p= 0.001
1978		1.0000 (0) p=******	0.9127 (171) p= 0.001
1982			1.0000 (0) p=******

one factor and not on others; these dimensions can easily be recognized as the disciplinary structure. Table 4 shows this structure for 1982.

In 1974 eight factors accounted for 77.7% of the total variance.[32]

In 1978 six factors account for 76.2%: 'marine research' has disappeared as a relevant dimension and 'phycology' has joined 'marine biology'. (From now on we shall place intuitive (disciplinary) names for the factors between single quotation marks.)

In 1982 the dimensions are the same as in 1978 (six factors account for 75.7% of the variance); only the relative order of the factors has been changed.

Therefore our next conclusion is: not only is the structure remarkably stable, but there is also a fine structure with a high degree of stability. The variation over the years shows up in the contribution of the respective factors to the variance only.

We can also conclude that in this case *Price* was correct, when he predicted that the analysis of journal–journal citations would show "strips of objectively defined subjects whose description may vary materially from year to year but which remain otherwise an intellectual whole."[33]

Let us now take a careful look at the changes.

The main source of change during this period is the rise of 'marine biology': from a third position behind 'fishery biology' in 1974 it moved to a second position in 1978, and became the first factor, explaining 32.6% of the common variance, in 1982 (as against 21.6% for 'hydrobiology').

In this perspective the change in the position of *Limnology and Oceanography* is most remarkable. This is the one journal which loads on more than one major factor in all years. (May we call such factorial complexity 'interdisciplinarity' from now on?) However, it loads on different factors in different years: from a journal between 'hydrobiology' and 'marine *research*' it becomes a journal mainly loading on 'hydrobiology' and 'marine *biology*'. So, it becomes more exclusively a 'biology'-journal. Doing so, it steadily increases its impact factor over the years: from 1.487 in 1974 to 2.155 in 1978 and to 3.029 in 1982. (It is also the only journal in this field with such a steady increase in impact factor.)

We can analyze the 'being cited' patterns by applying Q-factor-analysis to the same matrices. These patterns are almost the same in the overall picture. However, 'being highly cited' now appears more distinctly as a separate factor in the earlier years. This further legitimizes our earlier choice for 'citing' as variables and 'being cited' as units (see above).

Again, in the Q-factor-analysis the 'marine biology'-factor grows steadily in relative importance, explaining 31.3% of the common variance in 1982, as against 22.9% in 1978 and only 10.9% in 1974. *Limnology and Oceanography* is once more the only variable which loads on different dimensions.

Table 4
Factor analysis 'aquatic ecology' journals for 1982

	FACTOR 1	FACTOR 2	FACTOR 3	FACTOR 4	FACTOR 5	FACTOR 6
HB1	−0.00178	0.47043	0.09813	0.04242	0.04692	−0.07736
HB2	−0.02775	0.46141	0.02832	−0.00864	−0.03150	0.02145
HB3	−0.06524	0.44783	−0.02631	−0.02854	−0.06743	−0.07482
HB4	0.00588	0.46696	0.06807	−0.04596	−0.05032	−0.00742
LO1	0.24817	0.29572	0.03353	0.04356	0.04919	0.14282
MB1	0.44027	0.03335	0.03468	0.00335	0.04404	0.06240
MB2	0.43888	−0.04062	0.01102	−0.01929	−0.01340	−0.00754
MB3	0.30157	−0.11250	−0.06613	−0.02816	−0.03973	−0.14918
MB4	0.44375	−0.00020	0.01552	−0.00784	−0.00753	0.03295
MB5	0.45784	−0.04838	−0.04266	0.02128	−0.01668	−0.11213
WR1	−0.02001	−0.00923	−0.01949	0.47877	0.16727*	−0.06020
WR2	0.00998	0.05624	0.08979	0.50362	−0.01228	0.07073
WR3	0.01941	−0.03013	−0.01684	0.46724	−0.17611*	−0.07520
WR4	−0.01089	−0.04649	−0.03864	0.49681	−0.08948	−0.00067
FI1	0.08028	0.10846	0.45375	0.04576	0.10403	−0.06749
FI2	−0.01056	0.01441	0.48040	0.02660	0.01121	0.11221
FI3	−0.00584	−0.03852	0.39325	−0.04275	0.02858	−0.13903
FI4	−0.05209	−0.06155	0.47639	0.00961	−0.07901	0.13369
FI5	−0.00606	−0.08079	0.35993	−0.07335	−0.10797	−0.08496
ENV1	−0.00949	−0.01678	0.03943	−0.02051	0.01967	0.77546
ENV2	0.00127	−0.02502	0.01092	−0.06081	0.79132	−0.00877
ENV3	−0.00742	0.03365	−0.04844	0.16324*	0.50128	0.05051
DIV1	−0.00465	0.03042	0.11514	−0.04387	0.07023	0.46515
DIV2	0.19727*	0.09451	−0.00745	−0.02882	0.06053	0.17738*

*Minor factor loading.

Multidimensional scaling yields pictures which further illustrate the points made above.

The intersection of the 18 asterisked journals was used to look at longitudinal development. (To include an extra (third) 'hydrobiological' journal, we took (for Fig. 2) the 1975 and 1982 pictures for these 18 journals and *Freshwater Biology*, which was included in the *SCI* in 1975.)

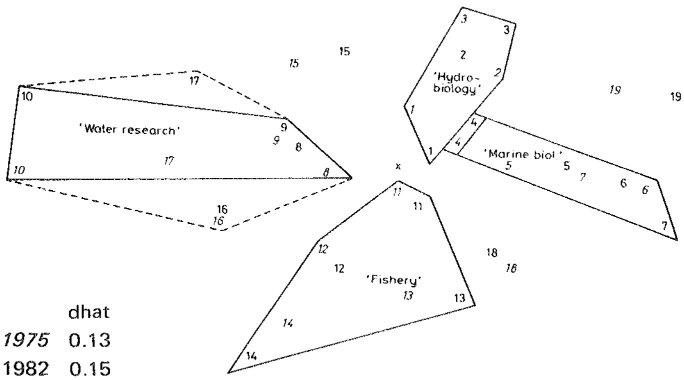

Fig. 2. Distances among 19 'aquatic ecology' – journals in 1975 and 1982.

1.	Hydrobiologia	HB	1
2.	Archiv fur Hydrobiologie	HB	2
3.	Fresh Water Biology	HB	3
4.	Limnology and Oceanography	LO	1
5.	Marine Biology	MB	1
6.	Journal of Experimental Marine Biology and Ecology	MB	2
7.	Journal of the Marine Biological Association of the U. K.	MB	3
8.	Journal Water Pollution Control Federation	WR	1
9.	Water Research	WR	2
10.	Journal of the Environmental Engeneering Division-ASCE	WR	3
11.	Canadian Journal of Fisheries and Aquatic Sciences	FI	1
12.	Transactions of the American Fisheries Society	FI	2
13.	Journal of Fish Biology	FI	3
14.	Progressive Fish-Culturist	FI	4
15.	Applied and Environmental Microbiology	ENV	1
16.	Bulletin of Environmental Contamination and Toxiciology	ENV	2
17.	Environmental Science and Technology	ENV	3
18.	Canadian Journal of Zoology	DIV	1
19.	Journal of Phycology	DIV	4

The 1975 and 1982 pictures are very similar. However, in the intervening years the computer chose different solutions. Actually, 1978 is the turning point: from 1974 to 1978 *Limnology and Oceanography* has moved deeper toward the centre. From 1978 onward *Limnology and Oceanography* has become a more established 'marine biology' journal with a specific function on the interface between 'marine biology' and 'hydrobiology'. The place it leaves behind, namely that of the leading 'hydrobiology'-journal, is thereafter taken over by *Hydrobiology*.

During this period *Hydrobiology* experienced a spectacular increase in contributions, from 73 in 1974 to 334 in 1982. So there is a big shift, but this is mere growth and there is no essential change in the structure of the journal–journal citations. If this growth were regarded as an effect of science policy – which is a separate question – then the case of hydrobiology would look like a good example of 'parametric steering'.

Occupational hygiene

Let us now turn to those sciences which are involved with qualitative problems in labour conditions.[34] The experts advised us to choose the *Scandinavian Journal of Work Environment and Health* as an entry. However, this journal was founded only in 1974 (as the result of a merger) and has been covered by the *SCI* only since 1979. From the cluster of journals around this entry in 1982, it is possible to trace back the lists for 1978 and 1974.

From the qualitative pictures we can see the emergence of a new cluster of journals with the Scandinavian journal in a leading position (Fig. 3). This is remarkable, because a new journal seldom begins as the leader.

In the earlier years most of the other journals held marginal positions in their disciplines. During the period in question they went through a reorientation away from their 'mother discipline' toward the new problem cluster. However, occupational hygiene is not (yet?) an independent scientific specialty: relations with the original disciplines are still clearly evident.[35]

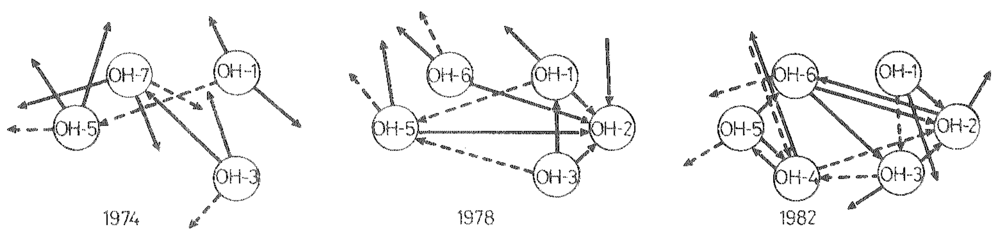

Fig. 3. Emergence of a journal cluster for 'occupational hygiene'.

OH 1 Journal of Occupational Medicine
OH 2 Scandinavian Journal of Work Environment and Health
*OH 3 British Journal Industrial Medicine
OH 4 Annals of Occupational Hygiene
*OH 5 American Industrial Hygiene Association Journal
*OH 6 International Archives of Occupational and Environmental Health

Our first *quantitative* measure is again the Pearson correlation matrix for the 21 journals, which existed during the whole period 1974–1982. The correlations (in Table 5) are even higher than in the former case (above 0.93 and on the 0.001-level), indicating that not much change took place among the existing journals. Instead, change in this field resulted from the creation of new journals.

Table 5
Pearson correlation matrix 'Occupational Hygiene'

	1974	1978	1982
1974	1.0000 (0) P=******	0.9886 (166) P= 0.001	0.9345 (175) P= 0.001
1978		1.0000 (0) P=******	0.9704 (191) P= 0.001
1982			1.0000 (0) P=******

This can also be illustrated with pictures from multidimensional scaling. When the 1974, 1978 and 1982 solutions for these 21 journals are superimposed, we find the stable blocks of medical, chemical, and biochemical journals which form the surroundings of 'occupational hygiene' and all kind of minor movements in the more specialized journals. The process of change becomes clear when all the relevant journals are taken into consideration. This leads us to the Figs 4, 5 and 6, which apply to 1974 (N = 37), 1978 (N = 30), and 1982 (N = 36), respectively. (The auxiliary lines will be legitimized by factor analysis in a moment.)

In 1974 the 'occupational hygiene', 'environmental health' and 'toxicology' journals were somewhere in the vicinity of biochemistry on the interface between medicine and the natural sciences. In 1978 a number of new journals had emerged (see the lower quadrants of Fig. 5), and in 1982 these revealed a clear structure. For example, toxicology has grown into a separate field with its own interface with biochemistry, consisting of journals on carcinogenesis.

However, factor analysis of these matrices is much more complex than was the case for 'aquatic ecology'. Ten or eleven factors show up, explaining again about 75% of the variance (so we get a reduction of complexity of only about one-third). One of the reasons for this is most certainly that 'medicine' is only included insofar as it is relevant for this area, i.e. only up to two arrows from the central cluster (see

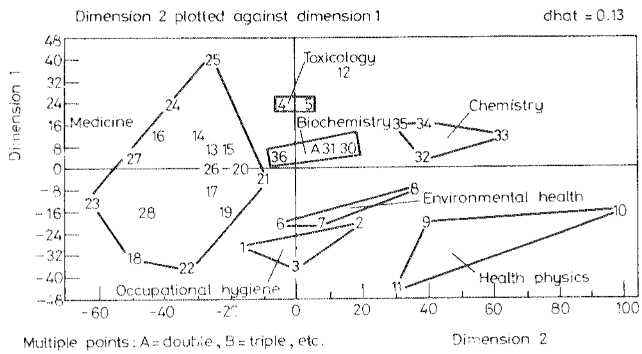

Fig. 4. Distances among 37 'occupational hygiene' journals in 1974.

1.	OH	3	British Journal Industrial Medicine
2.	OH	5	American Industrial Hygiene Association Journal
3.	OH	6	International Archives of Occupational and Environmental Health
4.	TX	1	Archives of Toxicology
5.	TX	2	Toxicology and Applied Pharmacology
6.	EN	1	Archives of Environmental Health
7.	EN	2	Environmental Research
8.	EN	5	Bulletin of Environmental Contamination and Toxicology
9.	PH	1	Health Physics
10.	PH	2	Atomic Energy Review
11.	PH	3	IEEE Transactions on Nuclear Science
12.	PH	4	Radiation Research
13.	ME	1	Lancet
14.	ME	2	British Medical Journal
15.	ME	3	New England Journal of Medicine
16.	ME	4	Journal of the American Medical Association
17.	ME	6	American Review of Respiratory Disease
18.	ME	7	Respiration
19.	ME	8	Journal of Applied Physiology – Respiratory, Environmental and Exercise Physiology
20.	ME	9	Journal of Clinical Investigation
21.	ME	10	American Journal of Physiology
22.	ME	11	Bulletin de Physio-Pathologie Respiratoire
23.	ME	12	Pneumonologie – Pneumenology
24.	ME	13	Postgraduate Medical Journal
25.	ME	22	Australian and New Zealand Journal of Medicine
26.	ME	24	American Journal of Medicine
27.	ME	25	Chest
28.	ME	26	Scandinavian Journal of Respiratory Diseases
29.	BC	1	Journal of Biological Chemistry
30.	BC	2	Biochemistry
31.	BC	3	Biochimica et Biophysica Acta
32.	CH	1	Analytical Chemistry
33.	CH	2	Analytica Chimica Acta
34.	CH	3	Journal of Chromatography
35.	CH	4	Journal of the American Chemical Society
36.	DIV	2	Annals of the New York Academy of Science
37.	DIV	3	Proceedings of the National Academy of Sciences of the United States of America – Physical Sciences

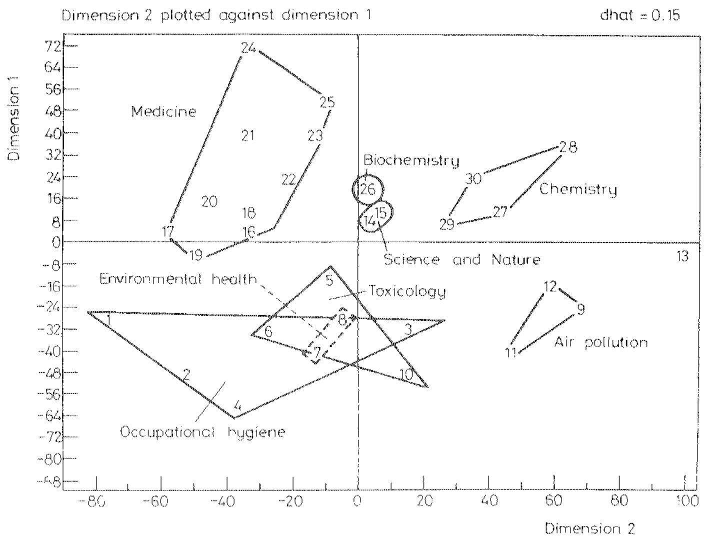

Fig. 5. Distances among 30 'occupational hygiene' journals in 1978.

1.	OH	1	Journal of Occupational Medicine
2.	OH	3	British Journal Industrial Medicine
3.	OH	5	American Industrial Hygiene Association Journal
4.	OH	6	International Archives of Occupational and Environmental Health
5.	TX	2	Toxicology and Applied Pharmacology
6.	TX	5	Toxicology
7.	EN	1	Archives of Environmental Health
8.	EN	2	Environmental Research
9.	EN	4	Atmospheric Environment
10.	EN	5	Bulletin of Environmental Contamination and Toxicology
11.	EN	6	Journal of Air Pollution Control Association
12.	EN	7	Environmental Science and Technology
13.	EN	8	Journal of Applied Meteorology
14.	SCIENCE		
15.	NATURE		
16.	ME	1	Lancet
17.	ME	2	British Medical Journal
18.	ME	3	New England Journal of Medicine
19.	ME	4	Journal of the American Medical Association
20.	ME	6	American Review of Respiratory Disease
21.	ME	8	Journal of Applied Physiology – Respiratory, Environmental and Exercise Physiology
22.	ME	9	Journal of Clinical Investigation
23.	ME	10	American Journal of Physiology
24.	ME	15	Respiration Physiology
25.	ME	16	Journal of Physiology – London
26.	BC	1	Journal of Biological Chemistry
27.	CH	1	Analytical Chemistry
28.	CH	2	Analytica Chimica Acta
29.	CH	3	Journal of Chromatography
30.	CH	4	Journal of the American Chemical Society

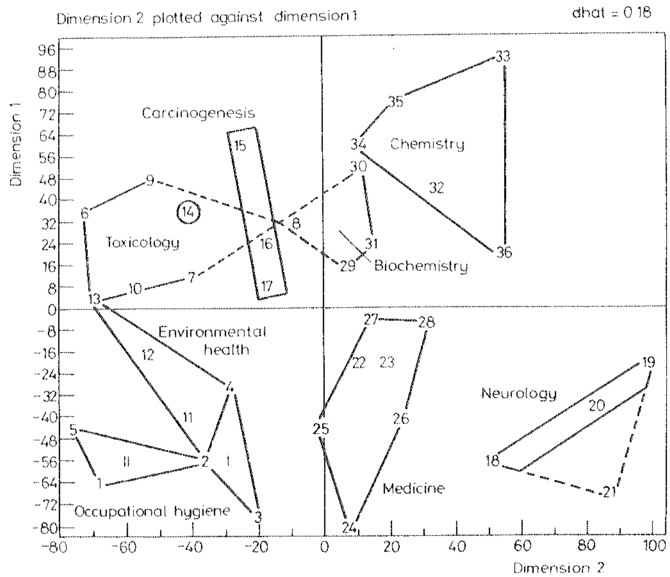

Fig. 6. Distances among 36 'occupational hygiene' journals in 1982

1.	OH	2	Scandinavian Journal of Work Environment and Health
2.	OH	3	British Journal Industrial Medicine
3.	OH	4	Annals of Occupational Hygiene
4.	OH	5	American Industrial Hygiene Association Journal
5.	OH	6	International Archives of Occupational and Environmental Health
6.	TX	1	Archives of Toxicology
7.	TX	2	Toxicology and Applied Pharmacology
8.	TX	3	Biochemical Pharmacology
9.	TX	4	Toxicology Letters
10.	TX	6	Journal of Toxicology and Environmental Health
11.	EN	1	Archives of Environmental Health
12.	EN	2	Environmental Research
13.	EN	3	Environmental Health Perspectives
14.	MU	1	Mutation Research
15.	CA	1	Carcinogenesis
16.	CA	2	Cancer Research
17.	CA	3	Journal of the National Cancer Institute
18.	NE	1	Acta Neurologica Scandinavica
19.	NE	2	Journal of the Neurological Science
20.	NE	3	Neurology
21.	NE	4	Journal of Neurosurgery
22.	ME	1	Lancet
23.	ME	3	New England Journal of Medicine
24.	ME	5	American Journal of Epidemiology
25.	ME	6	American Review of Respiratory Disease
26.	ME	8	Journal of Applied Physiology – Respiratory, Environmental and Exercise Physiology
27.	ME	9	Journal of Clinical Investigation
28.	ME	10	American Journal of Physiology

→

above). Therefore, the set of journals is incomplete. This generates several 'medical' factors which are not immediately intelligible.

Nevertheless, the results make sense. (The solution for 1982 is presented in Table 6.)

In 1974 the common variance is dominated by factors which can clearly be recognized as 'biochemistry', different branches of 'medicine', and 'analytical chemistry'. Five such factors corresponding to groups of established journals in the medical and natural sciences represent some 73.5% of the common variance (which is in turn 75.8% of the total variance). Toxicological and environmental journals follow as the sixth factor, which contributes only 6.9% to the common variance. 'Occupational hygiene' is present only as the ninth factor (4.6%), and this despite the fact that these very journals were taken as the entries for the constitution of the collection! However, even in 1974 these journals were already markedly dissociated from 'toxicology', a dimension with which they did not correlate, or did so only negatively.

In 1978 the situation has changed drastically. 'Environmental health' and a factor which we may regard as 'occupational hygiene' now contribute 32.2% to the common variance.

We can now see a pattern which may be typical for medicine: journals which belong prima facie to the same 'discipline' may in fact split up over two factors. This distinction must be explained by another major difference. (In this case: whether the journals emerged within 'medicine' or within 'environmental health'.)

For the group in question here, this becomes very clear in 1982 (Table 7), when half of the *occupational hygiene* group loads on a fourth factor and the other half on a fifth factor, each contributing about 10% to the common variance.[36] The decisive variable for this split seems to be *Mutation Research:* One group correlates positively with this journal, while the other exhibits a negative correlation. (Are techniques from mutation research such as the Ames-test used in these literatures?)

Q-factor analysis yields approximately the same results. This confirms that we are dealing with clear clusters.

29.	BC	1	Journal of Biological Chemistry
30.	BC	2	Biochemistry
31.	BC	3	Biochimica et Biophysica Acta
32.	CH	1	Analytical Chemistry
33.	CH	2	Analytica Chimica Acta
34.	CH	3	Journal of Chromatography
35.	CH	4	Journal of the American Chemical Society
36.	CH	5	ACS Symposium Series

Table 6
Factor analysis 'occupational hygiene' journals for 1982

	FACTOR 1 toxicology	FACTOR 2 biochemistry	FACTOR 3 carcinogen.	FACTOR 4 occ. hyg. I	FACTOR 5 occ. hyg. II	FACTOR 6	FACTOR 7	FACTOR 8 neurology	FACTOR 9	FACTOR 10 anal. chem.	FACTOR 11 chemistry
OH2	-.06225	.02710	-.05340	.07717	.48192	-.05450	.16773*	-.05845	-.13486	-.07086	-.03802
OH3	-.02980	.08907	-.03140	.22166*	.37162	-.10546	.00006	.00263	.07098	-.04046	.00212
OH4	-.01216	.00275	.05560	.48731	.09472	-.12906	-.09665	-.01411	.21302*	-.01513	-.01799
OH5	.02357	-.06500	.08263	.53427	.11382	.13694	-.17646*	.03708	-.10690	.11424	.00677
OH6	-.01458	-.04918	-.02636	-.04837	.56699	.09443	-.05966	.00034	-.11325	-.03504	-.02519
TX1	.36696	.01228	-.00271	-.14271	.08076	.02842	-.08131	-.01015	-.09196	-.05148	-.03068
TX2	.45260	.01564	-.05813	.05657	-.05686	.08020	-.05406	.01538	-.04243	-.00366	-.00291
TX3	.10603	.35668*	.06924	.00434	-.04251	.08623	-.01793	.01242	-.05520	-.00280	-.05451
TX4	.35466	.16947	.08337	-.07077	.00969	.00344	-.05836	.00374	-.04007	.00305	-.01277
TX6	.47580	-.06083	-.02983	.02018	-.03588	.03440	-.03938	-.00992	-.01996	-.00983	-.00530
EN1	.15003	.05997	-.06868	.18810*	.04728	-.25957*	.29759*	-.02076	.25673*	-.03340	-.00269
EN2	.27634*	.04588	-.02132	.20045	.04886	-.22733*	.03559	-.02182	.15921	-.02392	-.01785
EN3	.40595	-.13184	.02881	-.04579	-.04506	-.06467	.14303	-.03513	-.01574	.03855	.03220
MU1	.03851	-.03109	.09260	-.50525	.33435	-.12285	-.18962	.03104	.16516	.03071	-.01698
CA1	-.00125	-.01078	.55540	-.07167	.02445	-.03987	-.07076	-.00841	.01356	-.01394	.00147
CA2	-.00241	.05175	.57313	.03438	-.01098	.00983	-.00085	.01912	.00169	.00397	.00003
CA3	-.01853	-.05078	.55299	.07126	-.05290	-.00853	.11123	-.03449	-.03282	-.01950	-.01416
NE1	-.03520	-.02642	-.02375	.02771	-.00325	-.04057	-.01192	.50756	-.06453	.09101	-.03030
NE2	-.00357	.03650	-.00740	-.02376	-.00741	-.03475	-.05070	.55923	.01271	-.04555	-.00611
NE3	.00146	.00083	-.00724	.01017	-.06645	-.03451	-.05287	.51073	-.00826	-.07066	-.01336
NE4	-.08130	-.01754	-.06049	.11121	-.32674	-.09257	.00903	.03357	-.18739	-.23622	-.10458
ME1	.05854	-.01943	.04906	-.02875	.16357	.09283	.32016	.32223*	.06450	.08054	.05294
ME3	.04710	-.05028	.03042	-.02492	.01698	.19545*	.44375	.16369	.11823	.05569	.03781
ME5	-.05868	-.00062	.01735	-.05428	-.02214	-.03288	.63302	-.11538	-.10530	-.04148	-.03545
ME6	-.02982	.01803	-.00300	-.01679	-.07243	-.03375	.03175	-.00613	.63831	-.01153	-.01691
ME8	-.03652	-.05726	-.01475	-.06600	-.02664	.18324	-.14348	-.01740	.53504	-.02865	-.01777
ME9	.00889	.17374	.00901	.03980	.01801	.50586	.11899	-.01206	.08821	.00433	-.02047
ME10	.02629	-.06253	-.03406	.06403	.01511	.64545	-.02749	-.04642	.01902	-.03291	-.01370
BC1	.00473	.51078	-.01082	.00999	.01720	.02106	-.00043	.02244	.00497	.01706	-.00621
BC2	-.05914	.50103	-.02421	-.03062	-.02037	-.06308	-.00379	-.02036	-.00838	-.01744	.05480
BC3	-.02705	.48264	.00389	-.01551	-.00295	.01783	-.01503	.00988	-.00234	.00362	.00302
CH1	.00200	-.00475	-.00920	.03382	-.01677	-.01724	-.01202	.00576	-.01471	.63374	.01510
CH2	-.00581	-.01068	-.00512	.04345	-.01540	.00969	-.02827	.00575	-.02415	.58093	-.00972
CH3	-.03933	.06499	-.04363	-.11272	-.06675	-.11205	.07517	-.09779	-.00253	.38082*	-.10320
CH4	.00521	-.03113	-.00111	-.01732	-.01901	-.00448	.00460	-.00962	-.00376	-.03438	.70151
CH5	-.00962	.04546	-.00583	.02219	.00205	-.01349	-.01749	-.00474	-.01721	.03028	.68313

* MINOR FACTOR LOADING

Conclusions

We think it is best to leave our analysis here. Many interesting things can be said about the other factors, but they would lead us away from our central concern. However, these results allow us to ascertain the presence of a structure which can be understood, plotted, and followed over the years on the basis of the data in the *Journal Citation Reports*. The method can probably be made more sophisticated, but with respect to our cases some conclusions can already be drawn:

1. In the case of aquatic ecology and the related problem of water pollution, the analysis demonstrated that the existing structure of journals has grown, but that it has not changed fundamentally. The idea of a qualitatively different, integrated approach has not led to a noticeable emergence of interdisciplinary research areas.[37]

2. In the 'quality of labour' – case we observed that a cluster of journals emerged between 1974 and 1982. Most of these journals were formerly marginal journals in established disciplines (such as medicine, chemistry, toxicology, etc.). In 1982, the cluster consisted of two schools, one medically oriented and the other oriented toward environmental health studies.

These conclusions raise new questions. For example, *why* has incorporation into the existing scientific structures occurred in the one case, while in the other a new multidisciplinary specialty has emerged? The answers to these questions require research at lower levels of analysis: what processes account for the emergence of such a 'labour cluster'? This brings us back to the central question posed in the introduction: what is the relationship between the analysis of the results of scientific development and the analysis of its genesis? However, before we address this question again, some technical and practical possibilities of the method will be explored in somewhat greater detail.

Some technical perspectives

As we mentioned earlier, the method described here is simple, which in itself is a major advantage. Interesting qualitative pictures can be produced in a few hours, and such pictures already tell us a lot about the dynamics of science as a structure of journals. The more quantitative approach refines the pictures considerably. The 'interdisciplinarity' of journals and the existence of 'schools' within clusters can be discerned.

As a method, the simple procedure of mapping journal–journal citations has an intrinsic value. First, such a general topography of the field(s) under consideration can be useful in science studies. Moreover, among other policy applications, this method can, for example, be used as an indicator in debates over current research policy in many faculties. Faculty committees evaluating the performance of research groups more and more often consider the impact-factor of journals in which members of the groups have published, in addition to the mere number of publications (and sometimes citations). Because citing behaviour is known to vary considerably between different scientific fields, such a measure introduces a substantial bias. For example, impact factors are high in immunology and low in toxicology. Using the methods proposed here, it is easy to determine whether an article was published in a journal within the discipline or not.

Theoretical perspectives

Let us now turn to the theoretical relevance of this method for the study of the relation between science policy and 'qualitative change' in science. As I explained in the introductory paragraphs, the problem is that the development of science takes place at other levels than the development of scientific practices or science policies. The use of concepts, which have a related meaning at different levels (e.g., 'cognitive development') to bridge the gap between these levels is not correct: one is not allowed to extend the conclusions drawn from data at one level to another level by using the same word(s) at both levels.[38]

In order to address the multi-level problem of science dynamics, we are facing the task of constructing a hierarchy of different levels. At one extreme, the above results offer hope that we may be able to explain the variance among journal–journal citation patterns in dimensions that can be associated with the cognitive differentiation of the sciences (as *Price* once predicted, see above).[39] At the other extreme, 'demands' and (political) programmes with hardly any scientific content will be decisive in a policy debate. Many in-between levels can be discerned: the level of research programming and planning efforts, institutional levels of research groups, scientific communities, etc. The concept of 'transformation' of issues from one level to another can be given a more strict meaning, as we may be able to construct networks on each level and to analyze the variance among such networks in terms of their structural properties.[40] In which transformations do factors from higher (or lower) levels emerge or disappear? Do these patterns provide us with some rationale for a hierarchy? Are we able to specify which inter-level interactions will prove to be crucial for the transformation of political issues into scientific disciplines?

If we are, indeed, we may in the long run be able to reduce considerably the complexity of the problem of the social orientation and the political direction of science.

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Notes and references

1. Sources for such variance have been sought in the social and intellectual organization of the sciences as well as in the organization of the external relations of scientists. An attempt to systematize the former perspective can be found in: R. D. WHITLEY, *The Intellectual and Social Organization of the Sciences,* Oxford University Press, Oxford, 1984. For the latter perspective, see: K. KNORR-CETINA, *The Manufacture of Knowledge. An Essay on the Constructivist and Contextual Nature of Science,* Pergamon Press, Oxford/New York, 1981. See for a recent review: H. M. COLLINS, The Sociology of Scientific Knowledge: Studies of Contemporary Science, *Annual Review of Sociology,* 9 (1983) 265.
2. "Whatever one's final assessment of *Kuhn*'s view may be, it is clear that his diagnosis succeeds in a way that *Merton*'s does not, because it embraces the specific activities, theories and concepts of the scientist." (S. B. BARNES, R. G. A. DOLBY, A deviant viewpoint, *Archives Europeennes de Sociologie,* 9 (1970) 3.)
3. This point has been most vigorously made by the representatives of the so-called 'strong programme', but is at the background in other sociology of science programmes as well. For 'the strong programme' see: D. BLOOR, *Knowledge and Social Imagery,* Routledge and Kegan Paul, London/Boston, 1976, pp. 1–19.
4. In 1970 a research programme on science studies was launched at the Starnberg Max-Planck-Institut. The two programmatic articles of the groups were: G. GÖHME, W. v. d. DAELE, W. KROHN, Alternativen in der Wissenschaft, *Zeitschrift für Soziologie,* 1 (1972) 302; G. BÖHME, W. v. d. DAELE, W. KROHN, Finalisierung der Wissenschaft, *Zeitschrift für Soziologie,* 2 (1973) 128.
5. G. BÖHME, W. v. d. DAELE, R. HOHLFELD, W. KROHN, W. SCHÄFER, T. SPENGLER, *Die gesellschaftliche Orientierung des wissenschaftlichen Fortschritts,* Suhrkamp, Frankfurt/M, 1978.
6. W. v. d. DAELE, W. KROHN, P. WEINGART, (Hrsg.) *Geplante Forschung,* Suhrkamp, Frankfurt/M, 1979. See also: W. v. d. DAELE, P. WEINGART, Resistenz und Rezeptivität der Wissenschaft – Zu den Entstehungsbedingungen neuer Disziplinen durch wissenschaftspolitischen Steuerung, *Zeitschrift für Soziologie,* 4 (1975) 146.
7. G. KÜPPERS, P. LUNDGREEN, P. WEINGART, Umweltprogramm und Umweltforschung, in: *Op. cit.,* note 6, pp. 239–286.
8. Cf. I. SPIEGEL–RÖSING, *Wissenschaftsentwicklung und Wissenschaftssteuerung,* Athenäum Verlag, Frankfurt a. M., 1973, pp. 106–131.
9. W. v. d. DAELE, et. al., *Mission Orientation in Science,* Science Dynamics, Amsterdam, 1982, p. 10.
10. *Op. cit.,* note 6.
11. Y. DRORR, *Design for Policy Studies,* Elsevier, New York/Amsterdam, 1972.
12. Cf. KNORR-CETINA, 1981. *Op. cit.,* note 1.
13. See also: K. E. STUDER, D. E. CHUBIN, *The Cancer Mission. Social Contexts of Biomedical Research,* Sage, Beverly Hills/London, 1980, pp. 269 f.
14. D. de SOLLA PRICE, Networks of Scientific Papers, *Science,* 149 (1965) 510.
15. Other indicators, such as impact and immediacy factors of journals, can easily be computed from these data. See: E. GARFIELD, Citation Analysis as a Tool in Journal Evaluation, *Science,* 178 (1972) 471.

16. F. NARIN, M. CARPENTER, N. C. BERLT, Interrelationships of Scientific Journals, *Journal of the American Society of Information Science, 23* (1972) 323. See also: M. P. CARPENTER, F. NARIN, Clustering of Scientific Journals, *Journal of the American Society of Information Science, 24* (1973) 425.
17. There is some evidence that particularistic criteria may be influential even at this level, particularly in the social sciences. See also: W. C. YOELS, The Structure of Scientific Fields and the Allocation of Editorships on Scientific Journals: Some Observations on the Politics of Knowledge, *The Sociological Quarterly*, 15 (1974) 264.
18. However, there also exists the concept of qualitative, cognitive change within existing disciplines. It will probably not be easy to use quantitative measures for this type of change. In our opinion such analysis should be left for historians of science, who are deeply familiar with a research area.
19. E. GARFIELD, Citation Analysis of Scientific Journals, *Citation Indexing*, Wiley & Sons, New York, etc., 1979, pp. 148–239.
20. G. HIRST, Discipline Impact Factors: A Method for determining Core Journal Lists, *Journal of the American Society for Information Science*, 29 (1978) 171.
21. F. NARIN et. al. 1972 (*op. cit.*, note 16) used a two-step model instead of this three-step model.
22. One relevant question is whether citing rates are interval variables. At the aggregated level of journals we think they are since citations can be regarded as simple counts. This justifies the use of Pearson's r as an indicator of the ressemblence among matrices.
23. Missing values were ignored to prevent singularity of the correlation matrix. SPSS was used for the factor analysis, and for the transposition of the matrix we used a programme ZAMMO, provided by the Technical Centre FSW of the University of Amsterdam.
24. Of course, it is possible to introduce a weight factor here. For simplicity we kept all such parameters equal to one. (See also note 22.) The main diagonal is set equal to zero, i. e., it is assumed that the distance of a journal to itself is always zero.
25. E. E. ROSKAM, J. C. LINGOES, *Nonmetric Multidimensional Scaling GKLR MINISSA-1. Standard Version*, June 11, 1974.
26. Cf. A. J. ASHTON, *The use of cluster analysis techniques on biotechnology journal citation data*, MSc Thesis, Technology Policy Unit, University of Aston, Birmingham, 1980.
27. A factor, like a cluster in cluster analysis, has to be labelled with hindsight. A variable's loading on a factor is analogous to the distance of that variable from the centre of a cluster in cluster analysis. (The higher the factor loading, the closer to the centre of the cluster.) Unlike cluster analysis, a journal can appear in more than one factor, and hence participate in more than one subject area. Moreover, with factor analysis we get a measure of the proportion of the common and unique variance of each variable. Cf. E. NADEL, Commitment and Co-Citation: An Indicator of Incommensurability in Patterns of Formal Communication, *Social Studies of Science, 13* (1983) 255, notes 22 and 23.
28. Through graph-analysis it is possible to determine the centrality of an element in a collection. This, however, is also visible in multidimensional scaling, pictures, in which the larger journals are closer to the centre.
29. J. CRAMER, R. HAGENDIJK, Aquatic Ecology in the Netherlands: What is being done by whom? *Hydrobiological Bulletin*, 17 (1) (1983) 77.
30. Bundesministerium für Forschung und Technologie (Hrsg.), *Ein Programm und seine Wirkungen. Analyse von Zielen und Aspekten zur Forschung "Humanisierung des Arbeitslebens"* Campus, Frankfurt/New York, 1982.
31. Factor analysis was done using principal factors with iterations. The results presented here are Varimax rotated with Kaiser normalization (default values in SPSS). To control for orthogonality, oblique rotations were performed as well, but are left out of the presentation, because they yielded no essentially different results.

32. The cut-off point is where a factor accounts for less variance than an individual variable would, i. e. 1/n. 100%.
33. *Op. cit.,* note 14.
34. For the political backgrounds of this field, see also: L. LEYDESDORFF, P. v. d. BESSELAAR, Squeezed between Capital and Technology. On the Participation of Labour in the Knowledge Society, *Acta Sociologica* (forthcoming).
35. Again most relevant journals have low impact factors. This may be caused partly by the citing behavior of the relevant scientists, but it also reflects the status of the field in the academic community.
36. In this case the common variance is 76.6% of the total variance.
37. *Op. cit.,* note 7; see also: J. CRAMER, W. v. d. DAELE, Is Ecology an 'Alternative' Natural Science? , *Synthese* (forthcoming).
38. Of course, this is also the case for 'citation', which is used here as a measure of the proximity of journals to one another. In citation analysis, for example 'citations' can be a measure of indebtedness.
39. *Op. cit.,* note 14.
40. *Op. cit.,* note 13.