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INCLUDING CITATIONS COUNTING
IN THE SYSTEM OF ACTIONS
OF SCIENTIFIC PAPERS

PAPER PRESENTED TO THE FIRST ANNUAL MEETING
OF THE SOCIETY FOR SOCIAL STUDIES OF SCIENCE
4-6 NOVEMBER 1976, CORNELL UNIVERSITY

NOT FOR PUBLICATION

Until now, studies of scientific literature have concentrated on citations made by papers of other papers. Because it is possible to count citations and because it is made easy by such instruments as the Science Citation Index, citations proved to be a fairly good approach to delimit fields and specialties. This follows from the proverb "tell me whom you quote, I'll tell you who you are". But, if we follow the present "quotologists" (2) we would infer that a scientific paper is made only of a few lines of reference, the author's name and the acknowledgements, no matter what is expressed in the text. All scholars have to agree that citations are an outstanding characteristic of a scientific article (3), but they must also agree that other characteristics too often are overlooked. These features could be studied if we bring in the subfield of quotology, analysis made under the structural approach of texts (4). By this operation of cross fertilization, the epiphenomenon of citations becomes an expression of a larger phenomenon typical of scientific text: the transformation of former literature. To situate our approach in the present discussion for and against citations counting, we would say that citations per se are far less important and reliable than has been expected (5), but we would also say that the influence of the written paper is far more pervasive than established in the studies of informal communication (6).

The purpose of this article is to present a structural definition of a scientific paper in "hard" science (7) and to set up a method to describe a network of argumentation in details. This method might show that the discussion about quantification in sociology of science (8) is somewhat empty and that it is more important to devise algorithms objectively describing the quality of a field.

PART I : DEFINITION OF A SCIENTIFIC PAPER

Criticism of the emphasis on citations counting

As soon as one has read a scientific paper or has shared the daily life of its writer (9), the limitations of citation counting are clear. First, a large number of references to well-established former knowledge are implicit. Using acronyms, citing methods by names ("Edman degradation", "Ehrlich reagent"), using instruments ("Sephadex G 25"), employing general statements alluding to well-known and widely shared knowledge ("It has been generally accepted that") are all operations of referencing which may not be accompanied by any explicit citations (10). Secondly, an important activity in writing the citations is the operation of not citing someone else's paper. This activity becomes important when priority quarrels are involved and cannot be, of course, noticed by the counting of citations eventually chosen. For example, in August, a paper comes out from a group which does not cite a paper from another group issued in July and which uses another term for the same substance. The result is that the timing of the discovery is changed and the credit shifted from one group to another. Thirdly, the use of citations may correspond to widely different strategies that this quantitative approach is not taking into account. Fortunately, two recent articles have proposed a new way of counting citations by considering also their quality; technical, principal, perfunctory, negative, etc. (11). As this method implies, at least a quick reading of the articles is necessary. This technique is certainly an amelioration of quotology, but as it sticks to the secondary phenomenon of citations, it cannot replace an internal analysis of the text. We will use some of the distinctions made by these authors, but the quantitative data are somewhat disappointing (12). Lastly, the use of citations is only an emerging and quantifiable part of a complex system of action on

literature which has to be described. Often, an article is drafted without any citations; although it does not exist for the quotologist, it is a scientifically "active" paper.

Justification of the equation of knowledge with literature

From the quotology we will retain the basic postulate that scientific knowledge may be conveniently equated to scientific literature. Although this equation may be discussed from a sociological point of view, it has been of such an heuristic value in avoiding the philosophical traps that any sociologist must start from it.

The role of informal communication does not impair this principle. Even if, in the frontier, a published paper is already known, it is nevertheless always a paper which is communicated faster through phone calls and meetings inside the invisible colleges. The printed or preprinted material is the final product which settles priority quarrels and details of methodology. It is also the only way to being informed of what is happening in the neighbouring fields (13). To settle the question, we define as literature not only the published papers but a continuum which includes: drafts, corrected manuscripts, preprints (confidentials or not), oral presentation of a written papers, "poster sessions". abstracts and, finally, printed papers, reprints and xeroxes. Each of this type of literature has its channel and its soci organization. It will be absurd to limit science to published papers, but more absurd to overlook the omnipresence of the different stages of writing in the making of science.

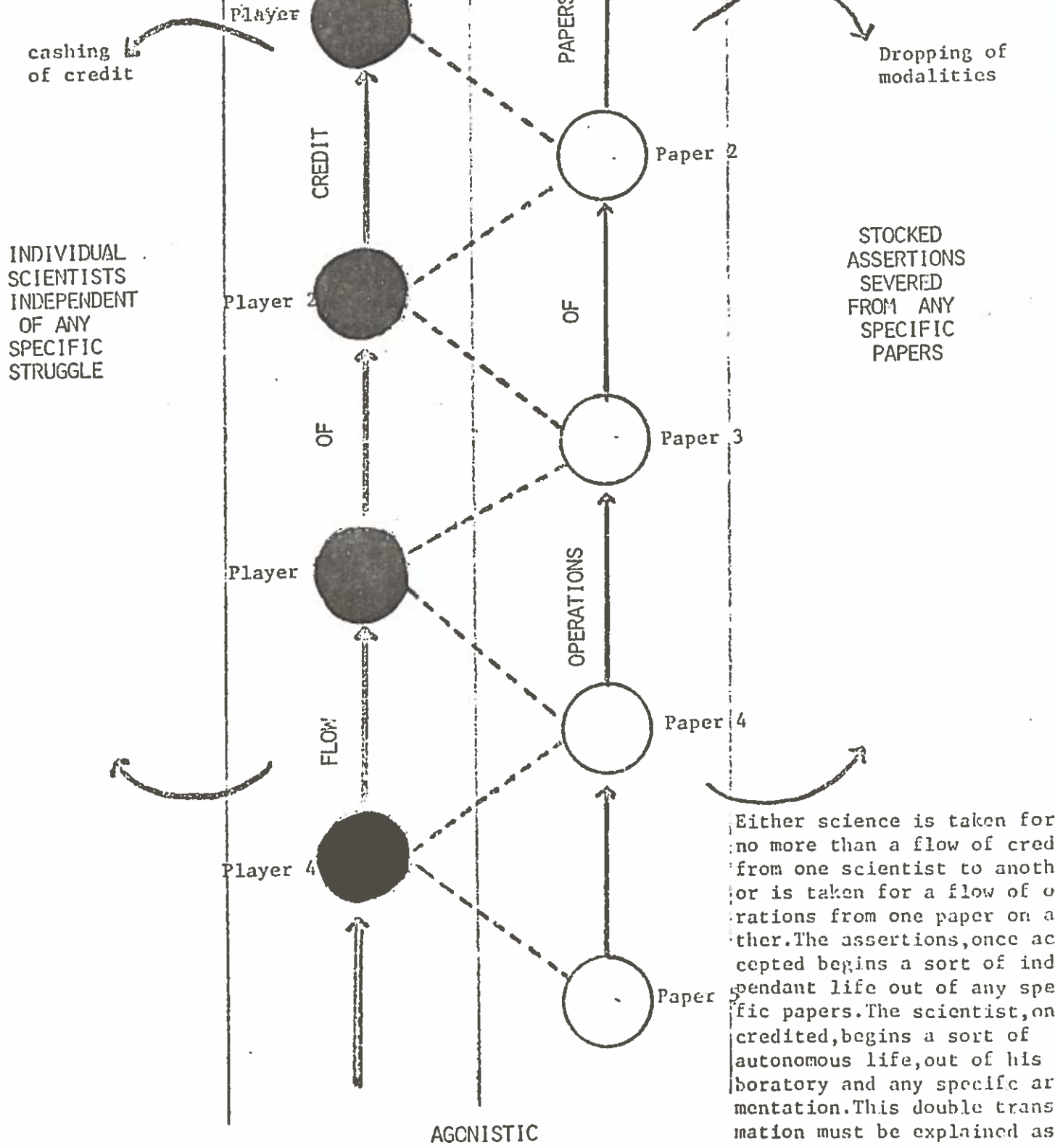
There is a resistance to the equation of knowledge and literature because people, especially lay people, consider a scientific assertion as an atemporal idea. In this view, literature appears only as a transitory medium which should be neglected to study the "concepts" (as a matter of fact, it is what epistemologists do). This impression of out of time assertion is given by text books, lay literature and "cold" sciences, through objective statements such as "TRF releases the thyrotropin hormone". But, when one reads the frontier literature on any "hot" subject

COLUMN A

COLUMN B

COLUMN C

COLUMN D



Either science is taken for no more than a flow of credit from one scientist to another or is taken for a flow of operations from one paper on another. The assertions, once accepted begins a sort of independent life out of any specific papers. The scientist, once credited, begins a sort of autonomous life, out of his laboratory and any specific argumentation. This double transformation must be explained as

the grammar is different and one sees sentences such as: "Shibusawa and his colleagues (ref.) have been studying a polypeptide (...). They call it TRF (Thyrotropin Releasing Factor) and believe it to be a neurohumor. Their findings have not been confirmed so far (ref.). Schreiber et al. (ref.) found that etc. ..." (Bogdanove, Regulation of TSH Secretion, Fed. Proc. 21:623, 1962). On the frontier, instead of having ideas you have someone's idea somewhere in someone's paper and in some institution. Instead of having assertions ("releases"), you have conflicting modalities ("believe", "not confirmed so far"). The unit to be analyzed is not a concept but a complex object: an authored-citing-cited-dated-modalized concept. As shown in the diagram I: we are not interested in the Scientist and in the Idea -by products of a secondary reconstruction- but in the actual mesh of texts acting on one another. In this approach, literature is not a transitory medium but the real milieu in which ideas, methods, reputations, laboratory findings and fundings are at stake.

There is another resistance to the assimilation of scientific process and scientific literature, which comes from the critical sociology (14) also from scientists (15). There is no doubt that a scientific paper is an artificial way of rationalizing ex post facto the actual sequence of events, but it does not follow from this truism that this artificial device is not ruling the life of laboratory. Quite the contrary: Papers are written before any experiments; others are set up as a way of forcing colleagues to begin investigations, the process of thinking in the informal discussion goes from one reference to another, etc. All the anthropological material gathered in our study lead us to define a team of scientists as a society of writers, more immersed in literature than any sect of avant-garde novelists. (cf. below). If the process of writing is "deceptive" (Comroe), "fraudulent" (Medawar), "rhetoric" (Gusfield) it does not follow that the real science is somewhere in the genesis, in the shifting sands of contradictory accounts, or in the "underbelly" (Edge), but that the whole process of making science is an artificial device. In short literature may be a misrepresentation

of reality and the reality of science. There is no contradiction.

The two resistances, both from lay men and from sociologists, stem from a more basic confusion. They all behave as if the scientific paper were a means of communication. Then, having discovered the "informal communications" they give up the study of literature. But not a single feature of writing a paper can be explained if one admits it is for communicating knowledge. First of all, most of the papers are never read. Secondly, the good informative papers are published too late. Thirdly, the good paper does not inform, it transforms the system of assertion of the former literature. In other words, the bad papers are treated, computed, accumulated in publication list, sold for positions, exchanged as tokens, cited in reviews, indexed in reference books, abstracted, but never read. The good papers are read, but they are all action, triggering lectures, experiments, credit, refutations, modifying large part of what has been believed, and in turn, being cited, abridged, transformed, processed and eventually forgotten. If in a novel everything is weighed to build a self-contained world of feelings which concentrates attention by a sort of centripetal vibration, everything, on the contrary, is calculated in the scientific paper to obtain a centrifugal movement. The paper is bristling with references, covered of words devoided of meaning by themselves, and is more action than knowledge, more strategy than speculation (16).

In short, the literature is not limited to the published papers, but is a continuum; this continuum organizes the daily life and the very actions of scientists. This literature never is only transmission of information, but always tactics and mobilization.

Features of a scientific paper

The first feature is that a paper operates on other papers. The operations are of different kinds and a list of them will be provided below. By operation we mean any transformation from one state to another by the action of a paper. For instance, "this note will show that the long-held

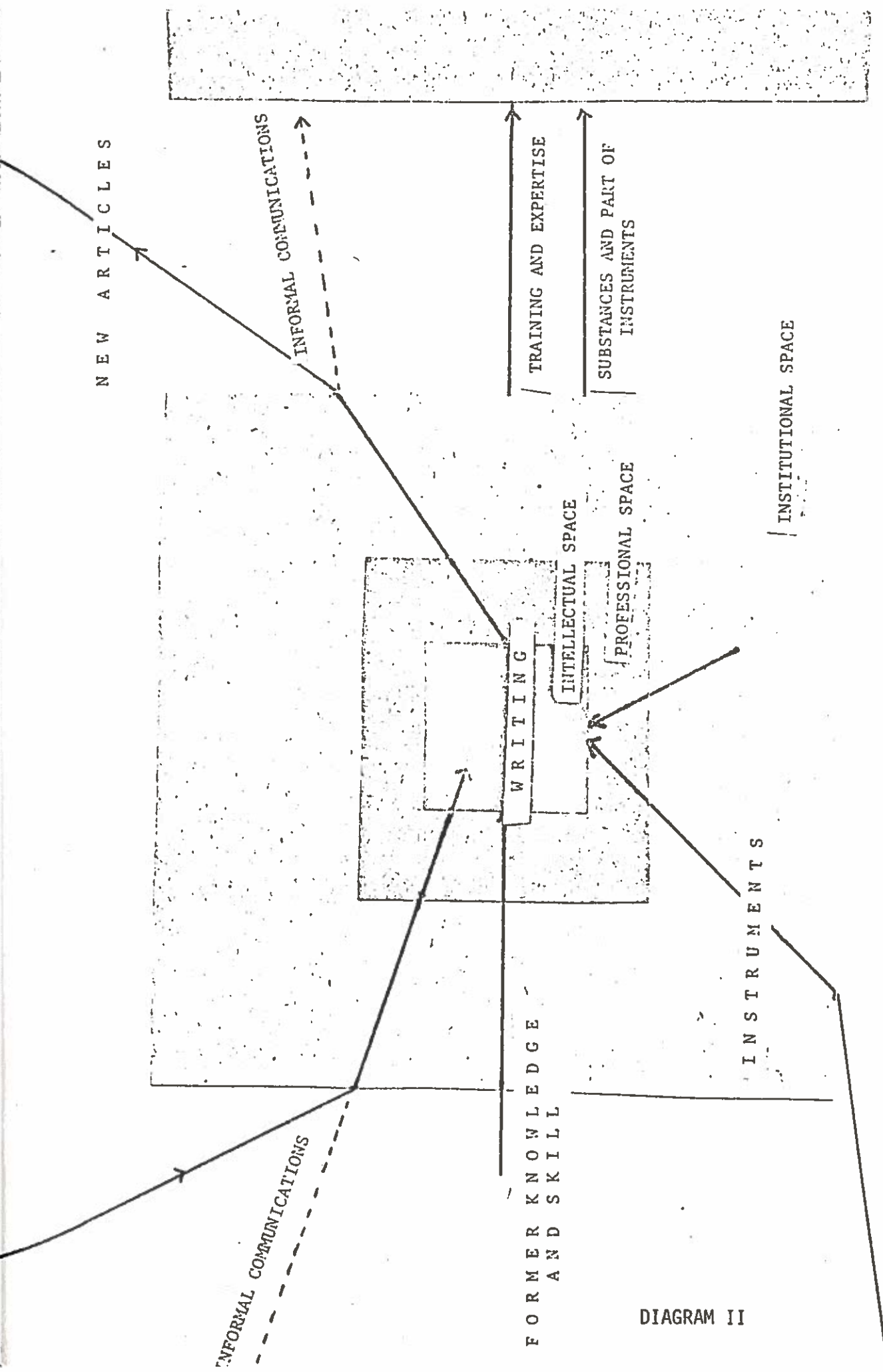


DIAGRAM II

hypothesis is confirmed, refuted, suspended, and so on". The second feature is that these operations on former literature are made possible by narrating the manipulations made in the laboratory. The actions on what you have read outside are allowed by what you have done inside the laboratory. The narrations of manipulations are, in turn, made possible by the accumulation of written tracks of phenomena obtained through instruments (data tape, spectra, diagrams, list of figures, protocol books, etc.)

The first type of narration could be compared to moves on the checkerboard of literature and the second to trials of material systems through instruments. The narration of moves is founded on the narration of trials. From an internal point of view, the paper is an intermediate between the printed literature it modifies and the non-printed literature (written tracks, records) from which it draws its competence.(17).

These two fundamental features react in the stylistic manner of a scientific article. Every sentence is explicitly referring to another, either inside the text (to tables, diagrams, figures) or outside the text (to other texts, ideas, methods, principles). The first part (introduction) and the last one (results and discussions) correspond to what we call action on literature and are not basically different. The middle parts (methods and results) are the narration of manipulations.

These features explain why we should endorse the second postulate of quotology: scientific literature is a referring literature [the first postulate being that scientific knowledge is equal to scientific literature]. But we will not endorse the methodological consequence that counting explicit citations is the way for studying the referring character of scientific papers, because the action of referring is far larger than what the explicit references could reveal. Then, we will set up an alternative methodology to reconstruct the details of the strategies that papers follow (moves and trials). Of course, this method

is not supposed to be extended, because it implies a detailed knowledge of a very small sub-specialty. However, it seems interesting to us to place the feature of explicit citations within a more general system of actions made by a scientific paper (18). The history of this device along with the history of a scientific study will make the history of objectivity clearer.

The internal characters explained above do not draw any differences between hard and soft science. This approach is interesting because it provides a textual definition of objectivity. Objectivity is not a mysterious quality of the things; neither does it lie in the scientist's virtue. Objectivity for our present purpose, is a way of writing. This method of writing may occur in so-called soft as well as in hard sciences. For instance, the sub-specialty of citation counting in the sociology of science has a literature as "tight" and "objective" as any subfield of physiology (19).

Scientific literature, although different from Literature, is truly a type of writing. It is a highly qualitative process in which unaccountable choices direct the whole structure as well as the details of style. There is no "systematic" writing except in the telephone directory. An objective paper is a written paper. Even the worst writer (including the ghost writer) has to choose the order of authors' names, the citation patterns, the journals he will submit his paper to, the timing and the title of the publication, etc. More importantly, he will have to choose what data will be shown, how detailed it will be, which hypothesis will be discussed and how much emphasized, not to mention the choice of vocabulary and figures. As soon as you have a text, you are not confronted with a systematic object, but a highly qualitative and historical process in which strategies of prestige and intellectual strategies are entangled. The result is a particular piece of art and it

will be treated as such as long as it stays at the frontier of a field (20). Objectivity is not a perfect statement hidden somewhere inside or outside the text; the "real" science is neither before the text in its elusive genesis, nor after the text in the cold presentation of textbooks. Objectivity is the whole relation between these written, "impure", texts and their contexts. To allow a sociologist of science to go inside the mechanisms of objectivity we propose this method of studying the content of the scientific writing. If not we give up any chance of criticizing objectivity and sociology is limited to studying the social causes or consequences of an object, the scientific "idea", which is left untouched.

II THE SYSTEM OF ACTIONS OF A SCIENTIFIC PAPER

If we admit that science is not the Mirror of Nature, but an action on former literature, we have to define the kind of object on which a paper acts. A paper acts on statements. If one neglects the content of these statements, it is possible to define for any given assertion five different states. The fifth one is commonly the accepted assertion as found in textbooks; A is B or for instance, "TRF is Pyroglu -His-Pro". Before this step, another one includes the name of the scientist who transformed this assertion into a belief; X M (A is B) or, for instance, "Burgus et al. (reference) have established that TRF is Pyroglu-His-Pro". If you go further in the past, and closer to the frontier, you find disputed assertions (state 2 and 3) and contradictory papers which transform in different ways the modalities preceding the assertions; M+(A is B) M-(A is B) or, for instance, "it is claimed by Schreiber that TRF is a simple polypeptide". The first state is the emergence of a new assertion; A is B instead of B is C or, for instance, "TRF" may not be a simple polypeptide" instead of the former example. We can recapitulate these five states in the following table:

(1) $x \ M \ (\underline{A.B})$	new object
(2) $x \ \underline{M2} \ (\ A.B \)$	disputed assertion
(3) $x \ \underline{M3} \ (\ A.B \)$	transformation of modalities
(4) $\underline{x} \ m \ (\ A.B \)$	acknowledgements
(5) $\quad \quad A.B$	common belief

The object of the game is to push one assertion from one state to an other. Of course, from the point of view of credit, the most rewarding move is to get one's own assertion in the fourth state. From the point of view of knowledge, the most interesting step is the fifth, because it increases the stock of credited assertions. But everything depends on what happened in the three first states. If it goes forward, the assertion will pass from a wild hypothesis to an established fact. But it can go backward as well; a believed statement may be transformed in a doubted one (from 3 to 2) and even worse, the object may be altogether dissolved and its matter spread in new objects (from 1 to 0); it was an artefact.

Several tracks could go from one stage to another: shortcut from one to five, revolution from five to zero, acceleration from hypothesis to fact, sudden shift from fact to artefact, or blocking in one position for years. In spite of these differences, the general pattern of these five stages is always visible. We define a move, any change in the state of the assertions.

Now, it is clear from the table above, as well as from the empirical classification of sentences taken out of articles, that a paper can act on only three possible sites: the assertion (A.B), the sign of the modalities, (M2, M3), and the author (X). It is equally obvious that each paper acts on a set of assertions, not only one. It must be possible to define the list of the operations that a paper has to make in order to obtain a change in the literature. We can now describe this list. We will describe the operations on literature (Moves), then what make those operations possible (Trials). Later, we will give an example.

A MOVES

a-Borrowing

"Physiological, experimental, and clinical observations have led to the concept that the hypothalamus controls and regulates the secretion of pituitary growth hormone "somatotropin". A hypothesis is simply borrowed from a group of texts or from a whole specialty and maintained in the article as hypothesis. This excision establishes a genealogical tree in which the article is situated.

"The incubations were performed in a Dubnoff type metabolic incubator";

"The bioassay used in this study is the in vivo method described earlier (ref)";

"Reichlin has confirmed our hypothesis"...In all these sentences, methods, technics, instruments, and other authors' operations are simply imported without any change in the quality of the modalities, and, most often without either modality or even citations.

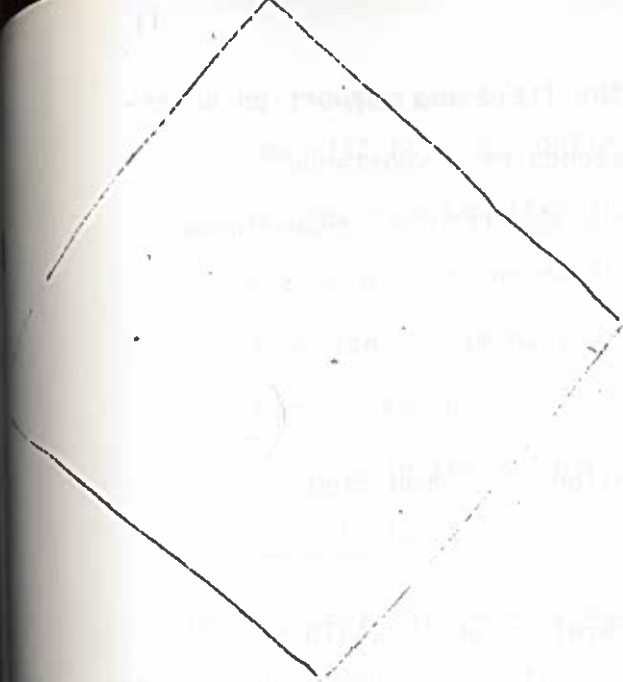
b-Transformation

"On the basis of the results shown here, our earlier report of the presence of TSH activity in crude extracts of hypothalamus tissues is confirmed".

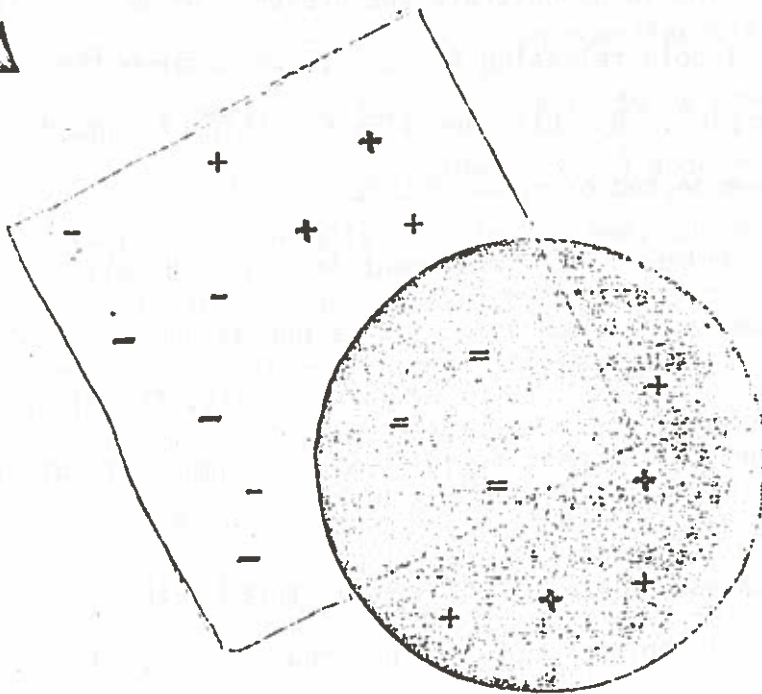
A result is borrowed from former literature and confirmed or simply underlined.

"A precise quantitative study of the amino acid compositions will lead us to put in question the long-held hypothesis of the polypeptidic nature of TRF". In this case some assertion which was taken for granted is made doubtful by the article.

"In this note we show the existence of a substance which corresponds to the expected characteristics of this neurohumor". This operation is the counterpart of the former: hypothesis or a doubtful assertion is replaced



ANALOGICAL STEP



REWORKING STEP



NEW OBJECT

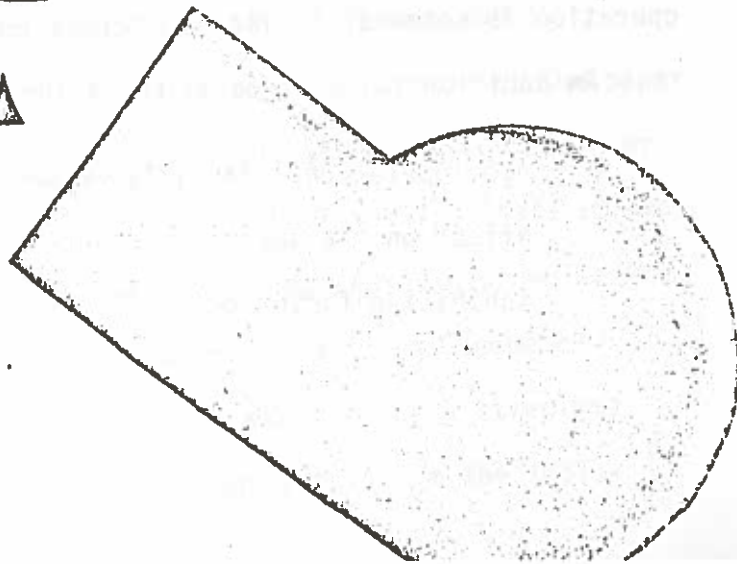


DIAGRAM II bis

by evidence. The Note transforms the field and support an uncertain assertion by showing the existence of a substance.

In the three cases cited above, the sign of the modality is transformed:

+ ----- ++ confirmation
 + ----- - falsification
 - ----- + verification

In none of these cases is the content of the assertion modified.

c-Building

"Since no method was available in the literature which would adequately meet our criteria, it was decided that a new method should be set up". "Searching to demonstrate the presence of this still hypothetical somatotropin releasing factor ... we observe the decrease of somatotropin". By this operation something is added which was either unexpected or non-existent.

In these sentences the content of the statement is modified. Although most of the philosophers' attention has been focused on this operation, it is not the only one and does not imply any extraordinary quality. The diagram II bis is borrowed from Mary Hesse's theory of metaphor (21), shows the simplicity of the step of making a new statement. An element $A(a,b,c,d,e)$ and another $B(c,d,e,f,g)$ are brought together. The predicates are then classified in three categories: positive, negative, equal. A new object is obtained C , with the predicates b,c,d,e,g . The operation is somewhat similar to "crossing-over" in genetics.

An addition to this operation is the nomination of the new built objects.

"For operational facility we decided to attribute this inhibitory effect on the secretion of growth hormone to a "somatotropin-release inhibiting factor or SRIF".

d-Planning

"We will have to confirm that the synthetic peptide is metabolized in the organism like the TRF." "Obviously a much larger number of brains will be necessary to provide enough of the polypeptide to approach its amino acid sequence." This operation prepares the future moves, discusses the possibilities, and defines what should be done in the future. We could call this set of operations deontic (what must be done).

To this operation may be added the signaling of alternative paths and everything which somehow plans the future and anticipates the obstacles.

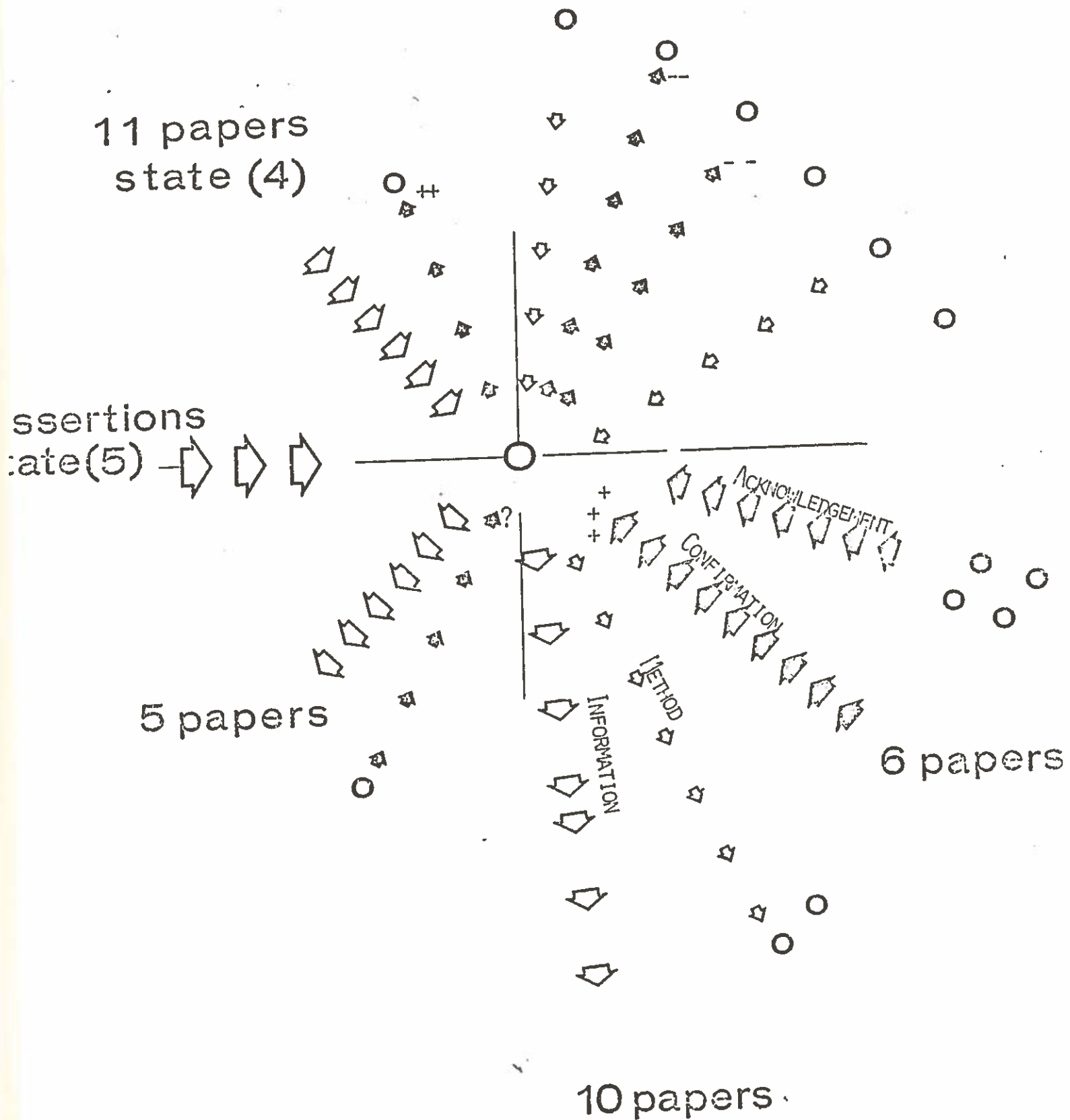
e-Evaluation

"Despite earlier claims based on questionable methodology and which proved to be wrong, turned out also to be unwillingly correct, the first incontrovertible evidence (...) appeared in 1962". This operation, especially used in reviews, concerns priority repartition. It is a part of the general operation of evaluation. The other part of evaluation concerns the importance of statements such as: "we have clearly established", "these results are more interesting", "this substance may be of considerable significance", etc. The priority repartition is the scientist's history, the evaluation of importance is the scientist's system of values.

Each of the operations listed may be observed in two different modes.

They may be direct or indirect. They may be aiming at other papers or at the paper itself. They are indirect when the article reports an operation made by a former paper ("It was believed that this substance was a neurohumor until S.(ref) showed that etc"). They may be a modification that the author imposes on his own text:

"Although the molecule of TRF does not correspond to the sequence Pyroglu-His-Pro-NH₂, the possibility is not excluded of a structure with a secondary amid group". "It is too early to claim the isola-

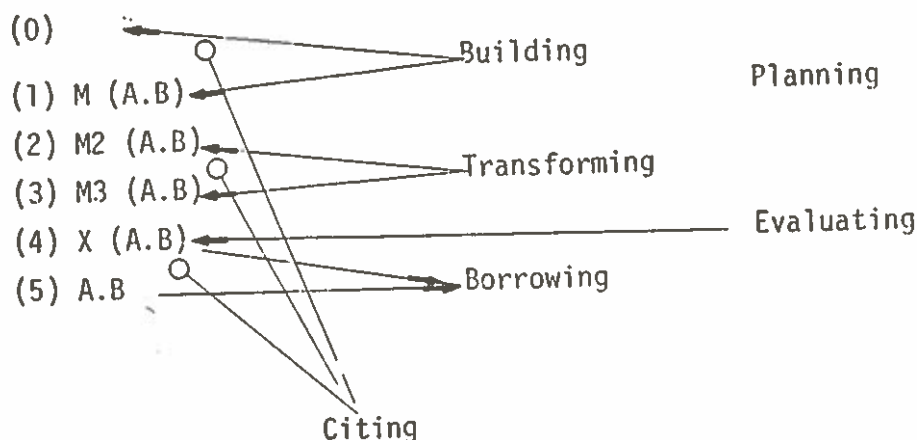


tion". In this case the author doubts of his own assertion.

f-Citing

The last operation in the part of the paper we call "moves" is the strategy of citations. This strategy is a secondary modulation of the others. It is part of the polishing of the final version and cannot be taken as the complete action of one text on an other. We have here to make a distinction between the citations in the technic part and in the polemic part. In the former, it is a purely deictic activity and kind of shorthand; it is a way of not repeating a long development of method easily available. In the latter, the citing corresponds to complex motivations: a debt has to be payed in exchange of the mass of information borrowed and transformed. But, this debt may be paid in different ways in which intervene codes of manner, nationalistic quarrels, robberies, influence of referees, perfunctory routines and competition. For instance, an American group does not quote a recent English paper considered a breakthrough by the referee (who ask the authors to add the reference, the text being unchanged). In another instance, a group gives itself priority in a discovery and cites the competitor's group just by saying "meanwhile B (reference) have similarly investigated this substance". Here, the position of the reference (at the end) and the casual way of presenting it, transform the competitor's discovery in a simple coincidence. To be aware of all the tricks counting the citations is not enough (22).

We can now summarize the operations by coupling them with the table of the five states in which assertions are obtained. Borrowing is concerned with state (4) and (5) statements, transforming with (2) and (3) and building by state (1). In order to be more specific we show in the diagram III the operations of one paper. In this case the operations have been reduced to Borrowing and Transforming (black arrows) -the sign + and - indicating the direction of the transformation. The chosen article is in the center; it is acting toward the top on already published papers, and it is acted on by other papers.



B. TRIALS

Until now, we have dealt only with the first part of a scientific paper (moves or actions on literature). However, the originality of a scientific paper lies in the other part, the trials, which make possible the first one. This second part is the narration of the material manipulations made inside the authors' laboratory. By themselves, the manipulations would be uninteresting, no more than a protocol book. Their action on literature is impotent without this narration. The impact of a paper consists of articulating these two kinds of stories.

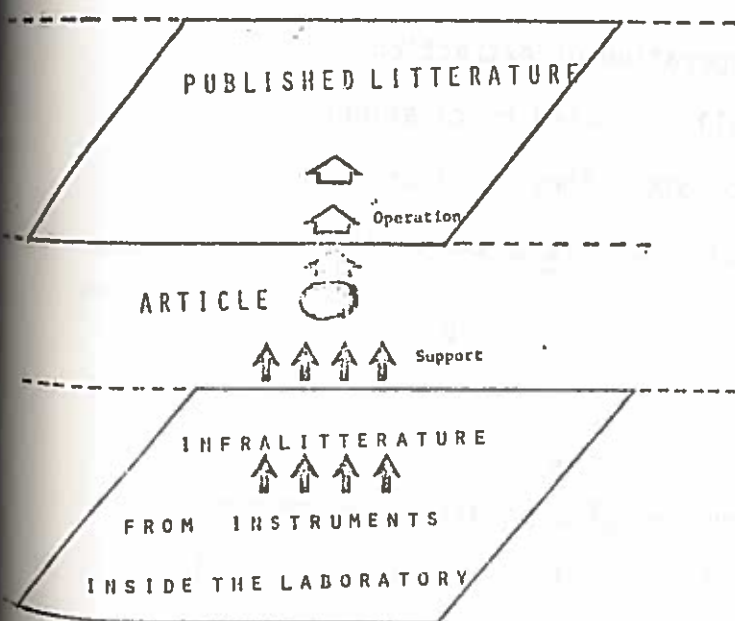
It must be restated that the experiments are narrated. The chemical reaction itself does not challenge the hypothesis of the literature, but the narration of this reaction included in a paper may challenge an hypothesis (also narrated in a paper).

How the literature will be modified is explained by the narration of how material systems have been manipulated. This founding of a narration upon another is explicitly expressed by sentences such as: "this note will show that", "These data lead us to confirm the hypotheses". I can change what I

have read (or written before) by what I have done in the laboratory. The authority is created by this foundation of competence and may be discussed by any other texts scrutinizing the quality of the credit.

The first characteristic of these narrations is the presentation of another sub-literature specific to the laboratory made of all sorts of written tracks (computer punch tapes, diagrams, spectra). These tracks provide a rich symbolic matter (figures, peaks, spots and curves), from which the narration of manipulation will be abstracted. This narration is in the middle of two layers of literature, one the printed literature on which the first part

DIAGRAM III



works, and second an infra-literature which is the daily production of the instruments inside the laboratory. The diagram represents this double situation. The second characteristic is a consequence of the first. The narration of meaningful manipulations is extracted from a large body of infra-literature. The first operation in this

part is a sorting to decide which written track will be accepted or rejected in the narration of experiments to support the actions on literature. Even the mass producer in science cannot just publish his protocol books. The elaboration of a narration is a very

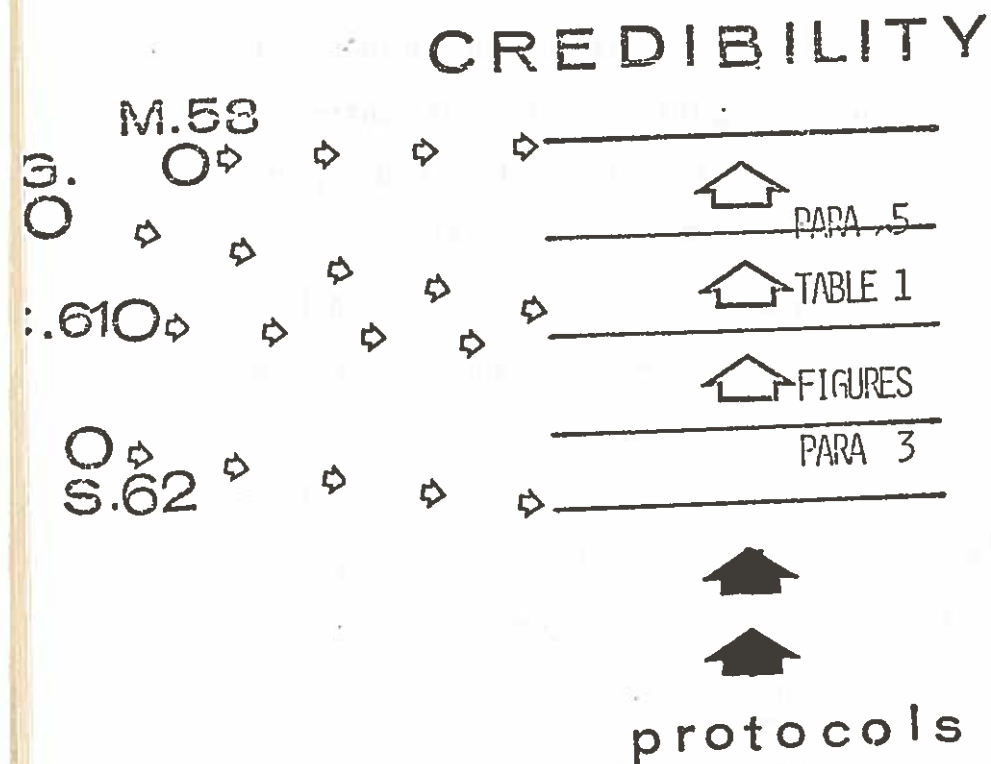
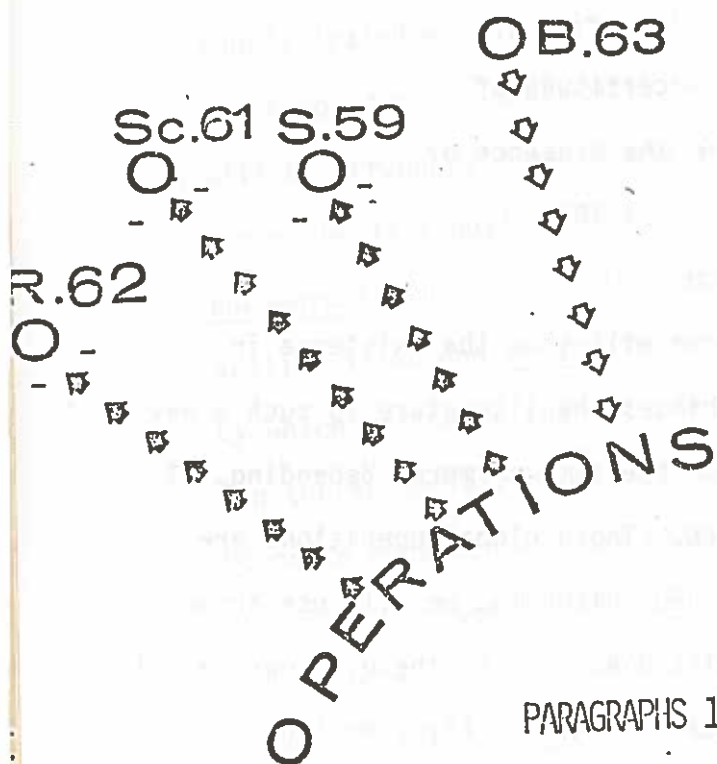
sophisticated way of writing in which detours, blind alleys, mistakes, and bungled experiments will be excluded. Also, the relevant facts will be arranged, cleaned and displayed in the text. This literary operation is a qualitative one. It is a process which is added downward to the qualitative tactic of choosing an instrument and setting it up and upward to the qualitative writing elaboration by which action on the literature is possible or not. By considering the accumulation of choices and contingent tactics, one is bound to consider the way objectivity is obtained as a highly qualitative process. Objectivity has qualitative degrees that the sociology of science should study if it does not want to be deceived by the ideology of objectivity.

Then, the narration of experiments is delimited by two general operations, one toward the Moves operation of competence-making, the second toward infra-literature operation of extraction. These operations, which are not usually signaled by citations, are completely overlooked by quotologists. They consider this as mere technique when it is, in fact, the core of scientific literature.

III EXAMPLE OF ONE SCIENTIFIC PAPER

To give flesh to these formal differentiations, we will take one paper basic for understanding the history of the substance we will describe. We will use diagrammatic representation of each move described. Considered as a whole, each article may be viewed as a series of operations which transform situation A into situation B. The chosen example transforms the long-held hypothesis that there is a hypothalamic substance regulating the secretion of thyreotropin in a quasi-certitude

This operation may be symbolized:



just confirmed our point of view after having studied the material coming from Shibuzawa's laboratory." Here, like on a billiard table, the operation of doubt falsification is reinforced by citing a text which confirms the informative operation. "The results presented by Schreiber et al. (ref) are more interesting; however, even in this case, all the necessary conditions have not been observed."

In the diagram IV we show the type of trials which are necessary to back up the operations. Instead of considering objectivity as "adequatio rei et intellectu" we have to consider it as the adequation of multiple layers of literature. Each of these layers is in turn supported by an other paper, and each may be criticized or doubted by a competitor ruining the credibility of the whole paper (2 4).

The action of this paper on literature cannot be severed from the actions of the following papers on them. Table I shows the number of times the group and authors outside the group cite this paper. By going back to these texts and reading the citations, differences in the way other scientists act upon a text are seen. The insiders and 12 of the outsiders are citing this paper as the beginning of a long and successful endeavor which ended in 1969. They usually confirm the cleaning operations made by this paper. For instance, "we were able to show", "the first incontrovertible evidence was reported in (ref)". Seven others are citing this text among others, repeating the confusion this text tried to avoid. For instance, as late as 1974 one author cites this TRF text by saying "in the late 50's, extracts were prepared which could stimulate TRF" and gives several references including Shibuzawa's and Schreiber's papers. Other articles cite it for details of methodology without giving it any stepping-stone character. In the table we have listed this paper's offspring and its influence as it is understood outside the group.

CITATIONS OF THE TEXT BY OTHERS

	NUMBER OF CITATIONS.											SOURCE: SCI POSTERIOR TO 1964
	1964	65	66	67	68	69	70	71	72	73	74	
Citations made outside the group	5	5	8	4	3	1	4	1	5	2	2	= 40
Citations made inside the group	2	2	1	1	1	2	4	1	0	0	0	= 14
(Citations unavailable)	1	2	1	0	0	0	2	0	3	0	0	= 9

AMONG THE CITATIONS AVAILABLE, MADE OUTSIDE THE GROUPS, THE FOLLOWING OPERATIONS ARE OBSERVED:

No text of contestation.

12 papers are confirming the operation made by the paper by which the presence of TRF is unequivocally shown. This confirmation is either explicit or by not citing any other paper but this one.

Schreiber, Ann. Endo. 25, 1964.

Lederer, Ann. Endo. 25:745, 1964.

Harris, Br. Med. Bull. 22:266, 1966, "ref. has demonstrated that..."

Schally, Endo. 78:1002, 1966, "only the reports of (ref and Reichlin seems to offer the necessary validation".

Hamilton, Develop. Med. 9:497, 1967.

McCann, Physiol Rev. 49:240, 1969, "The first unequivocal demonstration..."

Diassi, Ann. Rev. Pharm. 10:219, 1970, "the first unequivocal demonstration..."

Hall, Br. Med. J. 2:274, 1970, "more convincing evidence was provided by (ref)..."

Yates, Ann. Rev. Physiol. 33:393, 1971 "the history...began in 1962 with (ref)..."

Leppaluoto, Act. Endo. 1, 1972 "the first significant chemical characterization ...was made in 1962 (ref)..."

Editorial, Br. Med. J., 1973 "the presence ... was first reported a decade ago".

Rosenberg, Ann. Rev. Physiol, 27:71, 1965, "the most convincing evidence..."

7 papers confirm the operation but place the text without pointing it out in the middle of several others. These other texts are usually the papers this text wanted to be distinguished from:

Redding, Endo. 79:229, 1966, "it is generally agreed..." (ref) among 8 others.

Schally, Biochem. Biophys. R. 25:165, 1966, "various investigators clearly established"...ref. among Schreiber, Reichlin, Schally, Bowers

Schwartz, Ann. Pharm. Fran 24:301, 1966.

Sinha, Endo. 78:1002, 1966. (Ref.) in the middle of d'Angelo, Reichlin, Bogdanove.

Leeman, Endo. 81:803, 1967.

Redding, Endo. 81:918, 1967, (Ref) among Reichlin, Schally, Shibuzawa.

Browaste, Sci. 185:267, 1974, (Ref) among Shibuzawa and Schreiber

THE OTHER PAPERS ARE CITING ONE DETAIL OF THE TECHNICAL PART OF THE TEXT WITHOUT ENDORSING THE OPERATIONS IT MAKES. FOR INSTANCE:

Labella, Can. J. Physiol. 42:75, 1964; allusion to the oxitocin and vaso-pressin activity.

Schally, Endo. 79:1097, 1966; allusion to the introduction of Gel Filtration.

Balke, Neuro. 2:315, 1968; allusion to the potentiation by TSH.

Lipscomb, Cancer Res. 28:378, 1968; allusion to the flow rate for Filtration.

... Physiol. 30:589, 1968; allusion to Amino Acid Analysis

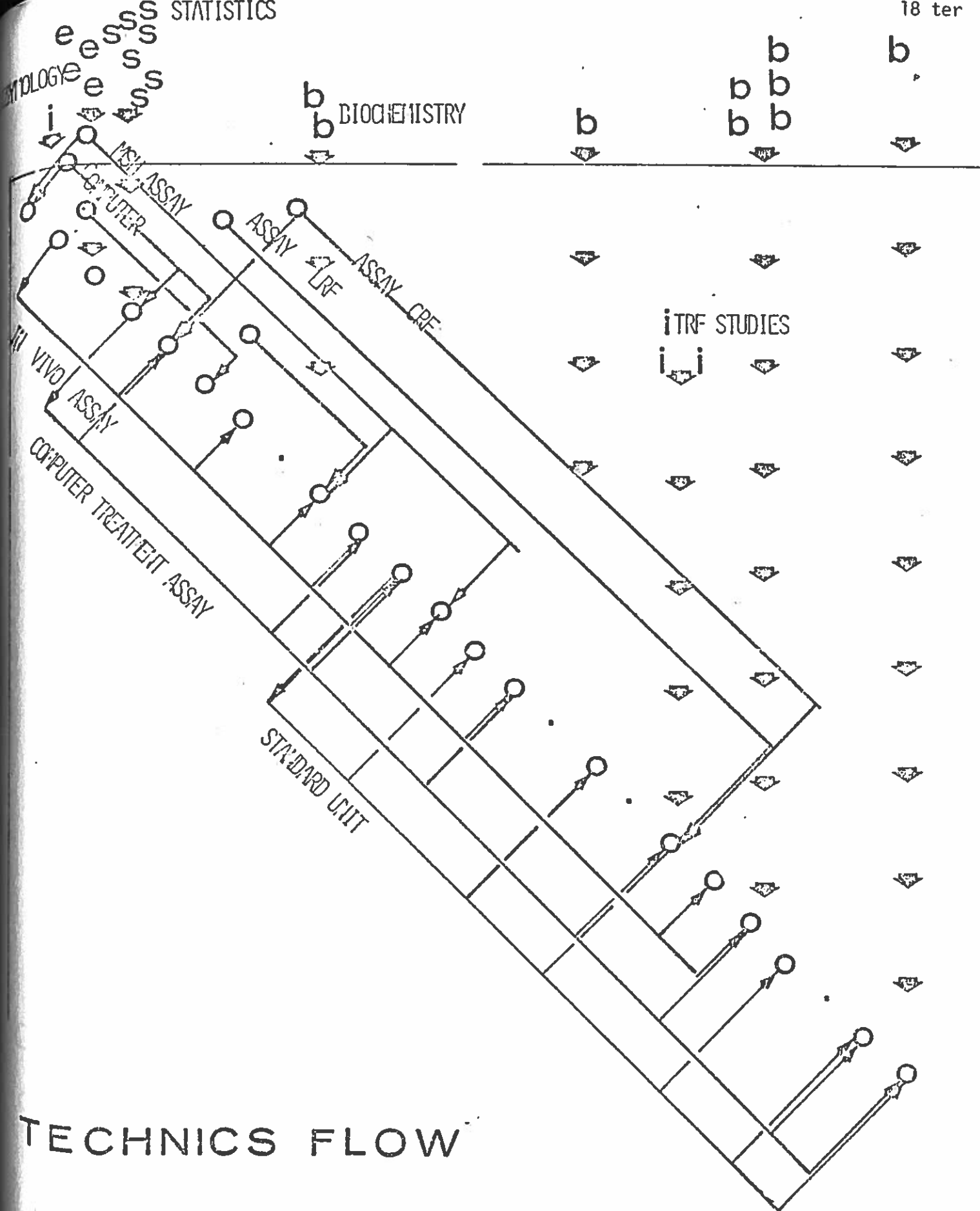


DIAGRAM V

Although a complete study of the actions of one single paper may be useful, it is clear that the main interest lies in describing a network of papers. This has not been attempted even by the proponents of a "qualitative" study of "citations"(25). An application of our method to the history of several substances will be written later. Let us extract from it a few diagrams showing the possibility of describing objectively the quality of an argumentation at a given time. Diagram V shows the operations of borrowing during a eight year period. The paper we have just studied is the first at the top left corner. The offspring of the article is this long string going to the bottom left. During this drift the assertions are constantly modified. The first and last papers correspond to state (4) assertions. The middle papers to (2) and (3) with a considerable number of state (1). In this diagram we show only the operations accompanied by explicit citations in order to simplify the picture. Of course, all the imported statements are in state (4) and none in state (5). Most of the activity is inside the group (line parallel to the main line) but massive imports of enzymology and statistics at the beginning and of basic biochemistry at the end, are clearly visible. The resistance of this complex organism to the attack and operations of other scientists, defines the solidity and the stability of an object; in the present case, an hormonal substance.

If we focus upon the operations of transformation and if we add to the string of articles from one group the articles published by another group, it is possible to get a map of their exchanges as shown in Diagram VI and VII. Again, in these two maps we limit the activity to the explicit citations and chose only the papers which are both cited and citing (all the papers are entirely devoted to the elucidation of the same substance). The pattern of relation between the two groups is strikingly different. The first group (G I) is borrowing only a few methods, mainly from the year 1966, and most of the operations are of transformation. Without any exception, all these transformations are negative and correspond to strong falsification of what has been "claimed" or

Operations
of G.I on G.II

←--- b
→--- t

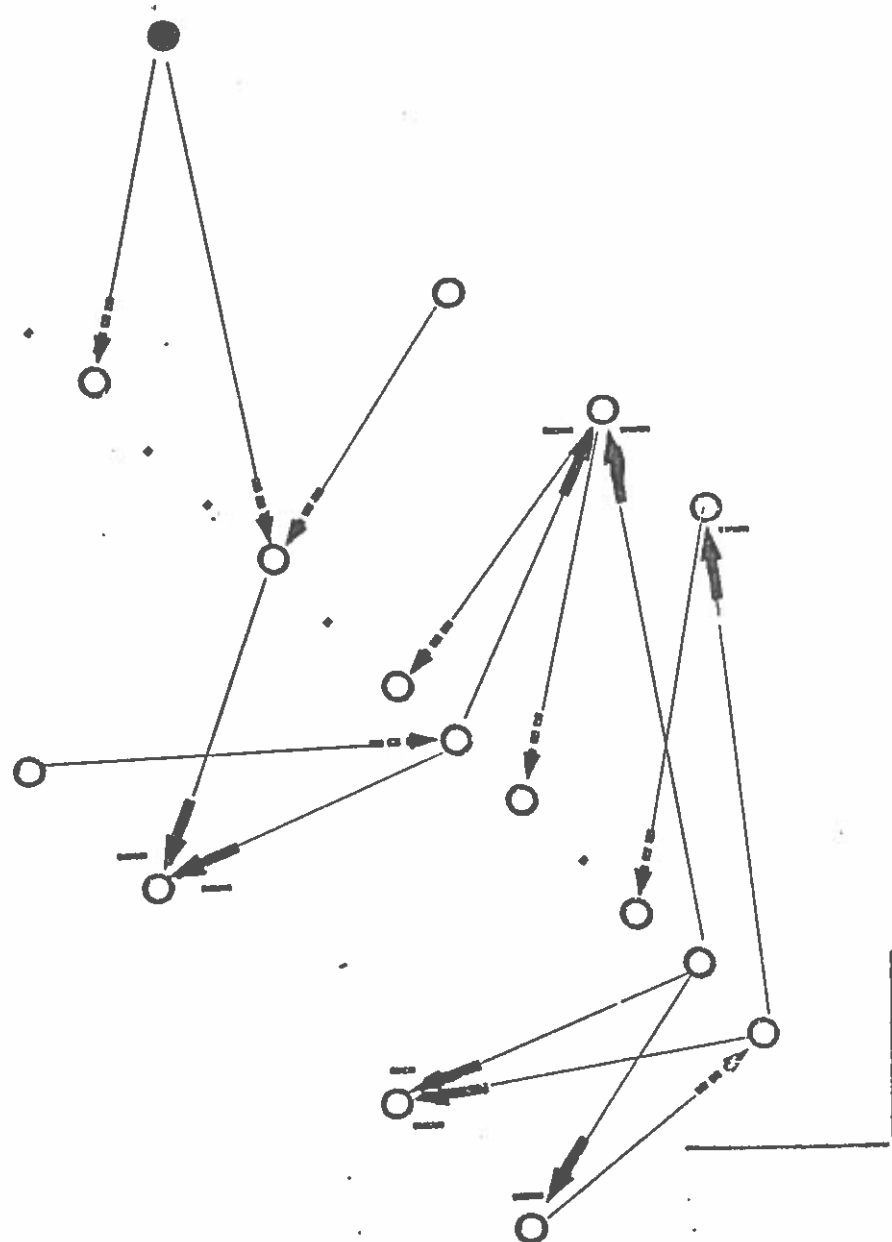
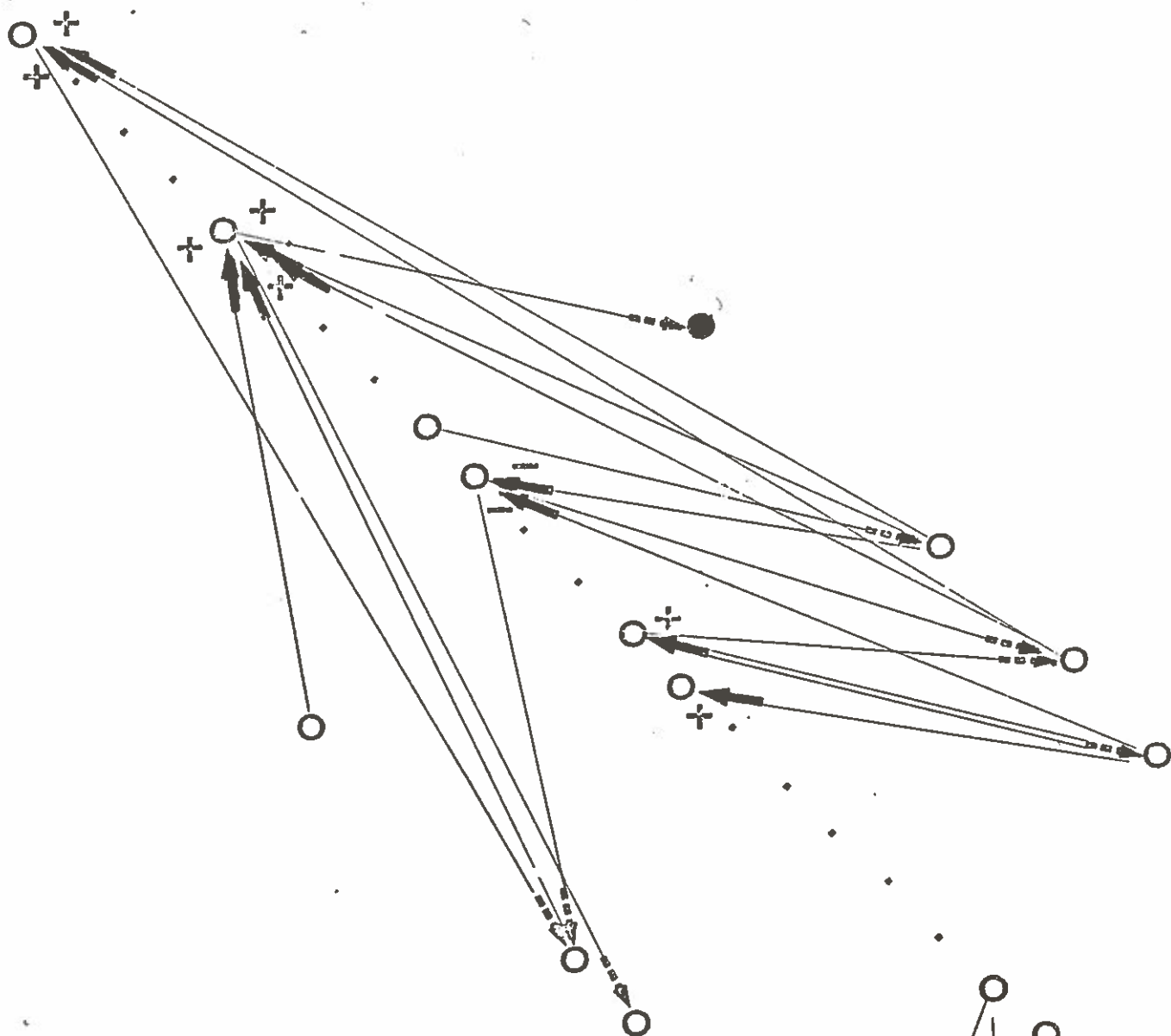


DIAGRAM VI



Operations
of G.II on G.I

DIAGRAM VII

"discovered" by the second group. The main exchange, at the end of the story, represents a strong, and sometimes bitter struggle for priority. The operations of the second group (G II in Diagram VII) on the first one are completely different. There is an intense borrowing of methods and concepts almost entirely concentrated in the years 1962-65. The transformations are all positive and stand for acknowledgement of priority or for confirmation. It is worth noting that these relations are not statistics but correspond to every single explicit relation established in this little area of which all the papers are exhaustively studied. In other word, what we map in these diagrams, is the movement of the detailed, contingent and meandering history of a substance, as it is reflected in the literature. Each move in this eight year game of Go may be traced and reconstituted.

However, it is clear that this description of a network of moves, is not sufficient per se and that it is necessary to add to these maps the rich material provided by the interviews, the archives and the anthropological immersion in the setting. But, our experience is that this kind of mapping clarify to a great extent the history of a discovery and even help in the analysis of the interviews. For instance, one paper from the second group, (in black in Diagram VI) has never been really mentioned in the interviews with the G I's scientists. In Diagram VIII, we find this paper on the top right of the page. The diagram recapitulates two letters by the two main scientists of the two groups; each of this letter establishes priority by reconstructing the chronology, emphasizing this or that paper and underlining specific informal communication. On the right you find the G II's, and on the left the G I's subjective representation of the chronology. On the left column of each part is represented the G I's papers, and on the right column the G II's ones. Then, each paper exist in two states; first, as they are seen by their author, and, second as they are seen by the competitor. An horizontal line link the different interpretations of the same paper. (The vertical lines stand for the lenght of time between submission, publication and, in one case, reading of one paper).

19.6

WRONG



p.1



p.2

JAN

meeting 1



CONFIRMED NOT PUBLISHED

FEB

MAR

AVR

MAY

p.1



READ ONLY END OF MAY



JUN

meeting 2



INSTRUCTED NOT TO TELL

JUL

p.2



RIGHT DISCREPANCIES EXPLAINED LATER



AUG

SEP

WRONG



WRONG



ONLY PAPER WHICH COUNTS

OCT

p.3

NOV

p.3



letter 1

letter 2

Conclusion

In part I , we have reduced the importance of citations and justified the assimilation of scientific activity with scientific literature on the condition that the definition of a paper would be enlarged to include a continuous chain of activities. The two main activities are: operating on former literature and obtaining an infraliterature in the laboratory to back up these operations.

In part II , we defined five states in which statements can be found in former literature. Moving a statement from one state to another (and cashing the credit or the credited information that is triggered by the change of state) is possible because of the multiple operations a paper can perform. A list of the most usual operation was provided. The citation is only one of those and certainly not the most important. More interesting than the list is the focus: everything is action in the writing of a paper, and since the work in the laboratory is also action and manipulations, we eliminate the problems linked with the speculative representation of science.

In part III , we gave one example and compared this rich multidimensional object to the poor empty point that citations counting would have made of it. Next, we have shown how it is possible, starting from one paper, to generate the network of arguments through which a statement is pushed from one state to another.

From these three parts, it is possible to draw three conclusions for sociology of science:

-a: Once you have made the first "copernician revolution" by which science goes from a study of Nature to an action on literature for credit and credited infor-

mation, you have to make a second "copernician revolution"() and state that there is no "knowledge" but only strategy, investments and operations to convince. If there is no reason why a scientific paper should "reflect" Nature, there is no reason either why it should "reflect" the genesis which gave birth to it: it does not reflect at all, it transforms. Then, there is no contradiction between studying the literature and studying the multiple "accounts" of a discovery. Moreover, it is only by putting together these two approaches, that it is possible to get an idea of the scientific object.

-b: In contradiction with Aristotle's motto, there is science only of the detail; no part of science is true "in general". Also, there is making of science only through a contingent succession of unique moves. Then, a method which does not describe the details of a field and which cannot exhaustively trace the different moves, does not grasp the real object of science but only a belated reconstruction and the "general ideas" of textbooks. The smallest unit which is easily available is the frontier papers. It would better to take smaller units (gestures, shreds of informal communication, birth of ideas,) and it is what our anthropological approach is trying to study. but it would be certainly meaningless to take larger units, except if the will of grasping real science is given up.

-c: Once it is admitted that science is all action and the focus is adjusted on the smallest available unit, the difference between internal and external factors just disappear. The idea that there are two spheres, one political and social and the other logical and speculative, comes only from an ill adjusted focus. Sociology never examines the details enough to realize that the hardest core of knowledge is itself strategy, moves and operations for credit and credibility. When it does examine the details, it is to oppose the shambles of the accounts to the rationalized papers, missing the most interesting feature: rationality in itself is a struggle moving assertions from one state of acception to another, by the operations of papers.

FOOTNOTES

1. This work has been supported by a Hay-Fulbright Fellowship and a special grant from the Salk Institute.
2. Expression coined by P. Bourdieu: "The specificity of a scientific field and the social conditions of the progress of reason." *
Soc. Sci. Infor. 14 (6) p. 19.
3. For an analysis of citations as stylistic effect, see M. de Certeau, "l'Ecriture de l'Histoire". Le Seuil, 1972.
4. In addition to the classical work of Propps, see Greimas: "Sémiotique et Sciences Sociales", Seuil, PARIS, 1976.
5. Cf. G.N.Gilbert and Steve Woolgar: "The quantitative Study of Science: an examination of the Literature". Science Studies 4 (1974), 279-294 for the accusation and for the defense the classical works of D.de Solla Price in reference there.
6. Cf. especially H.M.Collins: "The seven sexes: a study of the replication of experiments in physics". Sociology Vol.9 (May 75) p.205-224.
7. The examples will be exclusively chosen in the physiology subfield of Neuroendocrinology, specialty of "releasing factors", subspecialty of isolation and characterization.
8. International Symposium on quantitative methods in history of science, held at Berkeley, from August 25 to 27, this year. The debate settled by misfortune between two artificial camps: heavy large scale statistical methods versus small scale interpretative methods.
9. The author is presently doing a two year field study of sociology of science. Part of the material gathered consists in drafts of articles at different stages.
10. By referenciation we mean the deictic activity as such, by citations the use of another paper (name of its author and the journals); and by quotation the use of one sentence of the cited paper. It has been often noted that in hard sciences there are no quotations.
11. Cf. M. J. Moravicsik and P. Murugesan: "Some results on the function and quality of citations", Soc. Study of Sci. 5, p. 86-92, 1975.
D. E. Chubin and Moitra: "Content Analysis of References: "Adjunct or Alternative to Citation Counting?", Soc. Study of Sci. 5, p. 423-441, 1975.
12. A more promising approach is used by J. Gusfield: "The Literary Rhetoric of Science", Am. Soc. Rev., Feb., Vol. 41, No. 1, 1976. But with a traditional approach of literature analysis.
13. A recent confirmation came from P. K. Woolf in "The Second Messenger: Informal Communications in Cyclic AMP Research", MINERVA, Vol. 13, No. 3, 1975: "The published journals remain the point around which all revolves and towards which all must ultimately aim," pg. 373.

14. Cf. the papers presented by D. Edge, S. Woolgar, P. Weingart and H. Nigel at the meeting referenced note (8).
15. Particularly J. H. Comroe "tell it like it was". Amer. Rev. of Respiratory Disease Vol. 113, 1976 and "Retrospectroscope: The inside story". idem, Vol. 113, 1976.
16. Actually, comparing science to military occupation would be better than comparing it to speculation. A scientist is more a businessman, a military, a craftman, an artist, than an "intellectual".
17. By "competence" we mean, according to Greimas, the power of doing an action.
18. One paper has no significance per se. Cf. below for the analysis of a network.
19. But certainly not as meaningful. The observation in the laboratory that quantification may be inflationary or absolutely useless. Here again evaluation is first, and quantifying is a tool for evaluation.
20. Once severed from the frontier, the statements are untied from any particular texts and any particular authors, and begin an independent life as impossible to criticize as $2+2=4$.

Objectivity, in the current meaning of the word, appears to be a secondary reconstruction, begun indeed in the frontier paper but completed behind the front in the "cold" literature.

21. Cf. Mary Hess: "Models and analogies in science". N.D. Uni. Press, Indiana, 1974
22. It is possible to compare the tables used by Moravicsik et al. and Chubin et al. with operations proposed here, although these two papers cause confusion between action on literature and one of its operation, explicit citations.

Moravicsik et al.

Conceptual/
Operational

Moves
Trials

Organic/
Perfunctory

Action on Literature
Code in expliciting
citations

Evolutionary/
Juxtapositional

Specific cases of
problematization

Confirmational/
Negational

borrowing/conformi-
tion/verification/
falsification

Chubin et al.

Affirmative

Included in
problematization

Negational part
or total

idem

Basic
Subsidiary

in first part
(irrelevant)

add information
perfunctory

in second part
secondary
explicitation

- 23 . In the Chubin et al.'s classification this citation will be counted as partial negation when it is in fact a strong falsification. If these authors found few strong negations, it is because a stylistic effect such as "they say they have shown" is far more efficient and more in keeping with scientific code. .
24. In classical citation counting this rich combination of moves would have been reduced to seven citations and would have been replaced in Chubin's system by the expression "four negative, one positive" citations.
25. The addition of qualification to citations -a good move- has been followed by the usual crude statistics. Then the attention to quality is lost and it would have been better not to raise expectations. In fact, statistics are especially not suitable for studying scientific literature, since science is a network of specific-dated-contingent argumentations. Topology, "qualitative" mathematics, would be more adapted to this phenomenon.
26. The copernician revolution made by Kant, implied that Nature turns around Knowledge and not the contrary. But he still maintained these two independent worlds, in which all theories of objectivity have been trapped since. We expect that, with these two little changes, sociologists of science can be freed of this trap, without being philosophically naive.