Controversies in science: Remarks on the different modes of production of knowledge and their use*

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Wissenschaftliche Kontroversen: Über verschiedene Arten von Wissenserzeugung und Wissensgebrauch*

Inhalt: In der Wissenschaftssoziologie haben bisher Probleme der Konsensfindung vergleichsweise starke Beachtung erfahren, während selten danach gefragt wurde, warum wissenschaftliche Differenzen weiterhin bestehen. Eine Untersuchung wissenschaftlicher Kontroversen könnte zur Beantwortung dieser Frage beitragen. Kontroversen entstehen nicht nur, wenn kognitive Differenzen bestehen, sondern vor allem, wenn diese Differenzen unterschiedliche Auswirkungen haben. Dies geschieht dann, so lautet die vertretene Hypothese, wenn immer die Art der kognitiven Beziehung zu anderen kognitiven Strukturen – und daher auch der Gebrauch des Wissens – ein anderer ist. Diese Hypothese wird durch Beispiele aus der kognitiven Anthropologie und der Wissenschaftsgeschichte illustriert. Schließlich wird noch das Phänomen der „Nicht-Kontroversen“ erörtert, worunter jene Fälle verstanden werden, in denen kognitive Differenzen zwar bestehen, aber nicht – oder noch nicht – aktiviert wurden. Aus dieser Interpretation der verschiedenen Arten von Wissenserzeugung und Wissensgebrauch lassen sich eine Reihe von Schlußfolgerungen für die sogenannte Wissensakkumulation und für Engpaßsituationen in der Wissenschaftsentwicklung ableiten.

Abstract: In the sociology of science relatively much emphasis has been accorded to the problem of how agreement in science is produced, while it has seldom been asked why disagreement should persist. An analysis of controversies in science should therefore throw light on these questions. Controversies arise not only when cognitive differences exist, but when these differences come to matter. This happens, it is hypothesized, whenever the patterns of connectedness to other cognitive structures and therefore to the use to which knowledge is put, will differ. This can be illustrated by examples taken from cognitive anthropology and from the history of science. Finally, the cognitive status of 'non-controversies' is discussed, i.e. those occasions where cognitive differences exist but are not – or not yet – activated. Consequences can be derived from this interpretation of the different modes of production of knowledge and their use for the so-called accumulation of knowledge in science and certain bottleneck situations in the development of scientific thought.

I. Introduction

In the sociology of science as well as in philosophy much attention has been accorded to questions how agreement in science is produced. In the sociological camp this interest has focused on the functioning of the by now legendary scientific community, whose prove of existence has turned out to be so elusive (CRANE 1972, KLIMA 1973), while among philosophers the belief seems still widespread that procedures for reaching clear-cut solutions to any kind of problem worth its name ought to exist and enable agreement to be established. BEN-DAVID (1973) has recently put forth the intriguing interpretation that the institutionalization of science in 17th century England was directly linked to the widely felt need for the possession of a secure method

through which intellectual debates could be settled which proved otherwise undecidable through the conventional methods of scholarly, in particular scholastic, debate. According to BEN-DAVID the superiority of the new guild of scientists, the incipient scientific community, over the contemporary guilds of doctors, theologians and philosophers, lay precisely in the fact that the former were willing to abide by the rules of new scientific method which assured them that agreement could be produced. It is quite plausible that this promise opened new visions of an accumulation of knowledge, as the newly acquired knowledge would come to be built upon the firmly cemented foundations of previously agreed upon knowledge. It is not without significance that the new method was explicitly compared to a set of juridical norms, thus reviving the old idea of a Court of Reason, and constituting a guarantee for both parties that a consensus producing procedure was at hand. Upon this skeleton of agreement producing norms and rules, flesh has been put by the sociology of science through its emphasis on the functioning of the reward system, the socialization of scientists into their roles and the esta-

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blishment of effective communication structures — lines of inquiry that can be re-interpreted as direct contributions to the question of how, through what social and cognitive factors, agreement is reached. In spite of this heavy bias, it is nevertheless surprising that the question why there should be disagreement at all, has seldom, if ever, been asked before. Viewed against the impressive results of decades of research in the sociology of science (MERTON 1973) with all its emphasis on the communal advances of knowledge, one is left wondering why disagreement still persists. If scientists are socialized in long years of intimate and intensive training, if they absorb and internalize the elaborate norms and standards of scientific thought and practice, if they have at their disposal an effective communication structure geared to their needs for the exchange of rewards and views, if authority is vested into institutions like Nobel prize winners and eminence is easily recognized — why should there be disagreement and about what?

II. The ambiguous nature of controversies

Answers to this deliberately provocative question will tend to be of either of two kinds: one set of answers will emphasize the social nature of any scientific controversy. According to this view controversies are what might be called 'conflict by proxy' — they are debates fought for reasons which ultimately are to be found outside the realm in which they purport to be fought and for reasons that are extraneous to the intellectual content of the debate. Pushed to the extreme, controversies and social conflict in science appear to be inevitable, because there are only limited resources in terms of positions, funds or even immaterial rewards like professional recognition. Their scarcity leads to the emergence of intellectual disagreements as the only legitimate mean of carrying out an otherwise inevitable competition for status and rewards. This kind of controversy is at times contrasted with controversies of a supposedly purer version which are fought for their intellectual merits only. Such a view has been expressed by MERTON (1973), limited though to sociology as a discipline. Charges like the one that 'others are busily engaged in the study of trivia while the important questions are neglected', that 'alleged cleavages between substantive and methodological issues' exist, between

'macroscopic and microscopic types of analyses', are but a small sample of the types of conflict in a scientific context which MERTON classifies as social rather than intellectual. "... When we consider the current disagreements among sociologists, we find that many of them are not so much cognitive oppositions as contrasting evaluations of the worth of one or another kind of sociological work. They are bids for support by the social system of sociologists" (1973: 58). Controversies in sociology, or polemics, as they are called, are said to be "less a matter of contradictions between sociological ideas than of competing definitions of the role considered appropriate for the sociologist. Intellectual conflict of course occurs; an unremitting Marxist sociology and an unremitting Weberian or Parsonian sociology do make contradictory assumptions. But in considering the cleavages . . . we should note whether the occasion for dispute is the claim that this or that sociological problem, this or that set of ideas, is not receiving the attention it allegedly deserves. I suggest that very often these polemics have more to do with the allocation of intellectual resources among different kinds of sociological work than with a closely formulated opposition of sociological ideas". (1973: 55). Although MERTON'S observations pertain specifically to sociology, there is no inherent reason why the distinction of what controversies and polemics are about might not be extended with the appropriate modifications to other disciplines as well.

Another set of answers stressed the inevitability of the ideological nature of any scientific controversy, whereby ideology can either be taken in an all-inclusive sense or be limited to scientific norms which bind together scientists who share them. The first view has been expressed most clearly by ROSE and ROSE (1974) who have emphasized the specific ideological issues around which controversies evolve. Speaking about ideological inputs into science, they note the broader social pressure towards the commissioning of research, the engagement of scientists, and the ideology of science and of its institutions. Their analysis of the types of conflict which are conducted in contemporary neurobiology between different groups, clearly reveal the strong ideological component contained in each paradigm. The inborn view of schizophrenia, for instance, held by the reductionists, refuses to admit criticism of social structures while at the same time it en-

courages a manipulative view of treatment. According to ROSE and ROSE controversies cannot be neatly separated on grounds of their purely intellectual merits from the wider social significance which they must assume, willingly or unwillingly. Controversies of the type described by these authors therefore tend to be of global significance, as not different views of the subject matter are opposed, but causes and treatment alike. ROSE and ROSE regard such ideological inputs in science as normal. Controversies appear to be the outcome of divergencies which exist at this inclusive ideological level which in turn are a result of the societal pressures to which science finds itself exposed.

But controversies cannot only be looked upon as bringing to the fore the ideological commitments of the opposing sides, but also as particularly revealing with regard to the common norms and standards of scientific behaviour shared by the adherents of one side in a dispute. It is in this sense that BÖHME (1974) has recently re-analyzed a historical controversy in psychology and has reached the conclusion that controversies, like twin research, "should make explicit rules and norms which otherwise direct the secure march of science only in an inconspicuous way, and at the same time the degree of obligation and the different function of such rules to neighbour disciplines become obvious" (1974: 7). For BÖHME controversies are a direct expression of the kind and extent of agreement on norms that can be found in a scientific community. This view has been criticized by KLIMA (1974) who has argued that it is inadmissible to infer the social cohesion of a scientific community from the logical cohesion of the subject matter.

This last argument points to an interconnection between social and cognitive factors in controversies which is tacitly present in all the writings on the subject, but which has not found a satisfactory analysis yet. An interesting theoretical attempt to bridge the gap has been made by WHITLEY (1973). Starting from the assumption that competition between ideas and scientists is inevitable, controversies would be one form in which the ongoing competition manifests itself, being linked to the prevailing form of division of labour and to differing degrees of cognitive and social institutionalization of specialities and research areas. By considering competition bet-

ween techniques, axioms, interpretative rules and explanations, WHITLEY arrives at a typology of competition in science. According to it, controversies can be characterized by opposing views of the object under study, of the appropriate mode of understanding and explaining it, of the diversity of research techniques, of their validity and utility, of the different meaning of results obtained from applying these techniques and finally of the appropriate application and extension of a technique or a model. The genesis of controversies, as I infer it from WHITLEY'S typology, appears to be the result of an uneven development within each research area or speciality with regard to the degree of the existent institutionalization and the prevailing division of labour, linked not only to the competition between ideas, but between scientists as well.

The ambiguous nature of the interpretation of controversies in science as either a form of social conflict which manifests itself in intellectual disguise or as a form of genuinely intellectual disagreement which entails social consequences can also be found in the writings of some philosophers of science. Notably KUHN (1962) has maintained that the normal scientific tradition which emerges from a scientific revolution is not only incompatible, but often incommensurable with that which has gone before (1962: 102). Differences between paradigms can mark off different worlds in science, whose respective content is a closed book to the opponent who cannot even understand what goes on within a given paradigm that is not his own. Taking KUHN literally would mean that controversies are utterly futile, if they are conducted between paradigms, because they are unsolvable, while they should not really occur within a paradigm. This extreme view of the non-compatibility of certain paradigms has been mitigated by other philosophers of science in favour of a more dynamic process at work which may eventually help to overcome stated incompatibilities. Notably TOULMIN (1972) sees such a process in the selection and variation of scientific concepts. Although he acknowledges that the conceptual variation and intellectual selection in science are embodied in professional organizations and their social ecology, the question of controversies is more or less solved by an appeal to the evolutionary force inherent in scientific development which will inevitably weed out those concepts which are somehow less adequate

to a given intellectual climate. In a similar way, although on very different grounds, LAKATOS' and ZAHAR'S (1973) theory of progressive research programmes would lead one to believe that controversies are eventually solved by the more progressive research programme superseding the less progressive one.

This brief excursion through the literature, which was not meant to be exhaustive, underlines the ambiguous nature of controversies and their interpretation. There is a marked tendency to accord priority either to social factors, external to scientific thought properly speaking, or to ideas and how they work their way almost irrespective of social circumstances. Yet it is quite obvious that controversies have a dual character: they are cognitive as well as social processes at the same time. But in order to show this duality at work, it seems premature to accord priorities to either side. My own interpretation starts from acknowledging the phenomenon of disagreement in science. Controversies are an integral part of the collective production of knowledge; disagreements on concepts, methods, interpretations and applications, are the very lifeblood of science and one of the most productive factors in scientific development. If argument would cease, this would soon mean the end of the scientific enterprise which would rapidly turn into the administration of knowledge under the patronage of a scientific bureaucracy. In order to learn how disagreements occur, it is however not sufficient to regard them as mere reflections of the world of outside conflict. Rather, controversies are to be looked at as an endemic and vital motor in the production of scientific knowledge: they arise from different modes of production of knowledge and their use.

In what follows controversies will be interpreted as the outcome of different modes of production of knowledge taking place in different minds and contexts, for different uses and linked in different ways to already existing cognitive structures. This differentiation takes place because every individual possesses his unique cognitive map, as the representation of the outer world can summarily be described. It will thus attempt to link newly received information – provided it is not rejected out of hand – to previously received information, it will tend to evaluate and fit it into its own cognitive system. Viewed collectively, this process will generate new cognitive

links not only within an individual, but also across individuals engaged in the process of knowledge production. Some of the new patterns of connectedness which thus emerge may relate to cognitive structures which are already highly ideologized, others may become ideologized in the course of developments and some may isolate them successfully against such links. As a collective process new knowledge is produced through the impact of new information, put into circulation by an individual, on the existing cognitive structure which manifests itself in a change of the patterns of connectedness. Controversies arise, we assume, not so much between opposing content of thought, as between different modes of connecting cognitive structures.

At the same time the acceptance or rejection of ideas brings individuals into certain relations to each other and thus can be said to create a social structure describable in terms of cognitive similarity or dissimilarity (MULLINS 1969, SCHMUTZER 1974). The dominant view of communication in science as purely instrumental, geared to the effective diffusion of knowledge or to trace the channels through which ideas have been processed in retrospect, is a result of its development out of the early public opinion studies (STORER 1973: XXVI–XXVI). It has totally obscured the fact that communication can be looked upon not only as channels through which anything can be communicated more or less effectively, but that it is meaningful to look at social ties among scientists as resulting from the fact that they share or repudiate certain ideas¹. It is in this sense that we have maintained elsewhere that the acceptance or rejection of ideas creates a social structure, although one not freely operating from various constraints (NOWOTNY & SCHMUTZER 1974).

In the collective production of scientific knowledge based on a social structure which is partly a result of those communication processes

1 This is only one factor which may lead to the establishment of communication links, but seems to be the most important in science. Others are neighborhood or social distance, norms and their costs, and status differentials between the communicating individuals. The implications of these factors have been dealt with in NOWOTNY & SCHMUTZER (1974) in their relevance for the creation of a communication structure. A more detailed and technical treatment in form of a simulation model can be found in SCHMUTZER (1974).

through which ideas are diffused – to be accepted, rejected or modified² – the emergence of controversies signalizes that different modes in producing new knowledge are at work. This means that the cognitive ties established with already existing cognitive structures, i.e. the overall patterns of connectedness, will tend to differ. Social ties between individual scientists may help or hinder these processes, but they themselves are dependent on what it is that is to be connected. As the most simple case of connectedness is to be found in classification systems we shall first turn to the examination of such a case.

III. Why the Cassowary is not a bird or what potential controversies are about

For the *Karam*, a highland New Guinea tribe, the cassowary is not a bird (BULMER 1967). The *Karam*, we are told, place in the taxon 'yakt' all the 180 or so kinds of flying birds and bats they recognize by names. Cassowaries are large, ostrich or emu-like birds and are not included in 'yakt', but constitute the constraining taxon, 'kobity'. Why, we may ask with the anthropologists, is the cassowary not a bird?

To find an answer to this seemingly simple ques-

tion, we can go through all the explanations given by the *Karam* themselves – such as that it is not a bird, because it doesn't have plumes, but 'hair', that it has no 'brain' but only bones on its head – only to discover that there are other birds that also have 'hair' or 'no brain' and are still regarded as birds, until all possible morphological explanations have been exhausted and have proven insufficient to explain the cassowary's special taxonomic rank. Nor does an investigation of classification by habitat prove any more successful; the cassowary seems to defy this kind of analysis. To make matters even more complicated, if one looks at the objective physical and biological differences between the cassowary and other birds and bats, as they may be perceived at the level of common sense natural history, a number of differences stand out. But this is not, at least not for the *Karam*, a sufficient reason to place the cassowary into a special category. We also learn that some other New Guinea highlanders who have just as much common sense and just as much knowledge of the cassowary as the *Karam*, nevertheless place it into the same taxon as other birds. The answer to which we are finally led, is, very briefly, that the cassowary enjoys a special place in *Karam* taxonomy, because it enjoys a special and unique relation in *Karam* thought, to man. This is outwardly manifest by an elaborate set of rules which regulate when and by whom the cassowary may be hunted and how it is to be eaten.

- 2 So far, citation index measures have failed completely to take into account the controversial character of knowledge. These measures, like the ones based on the Science Citation Index, are usually thought to measure the impact of a publication on the corpus of knowledge. As a side effect they are believed to enable the evaluation of previous papers, by the amount they are invoked in the course of new works. It is generally assumed that any reference to previous work means positive evaluation, while I would maintain that a citation could also imply negative evaluation – in the sense that the present author disagrees and takes issue with the cited work – or neutral evaluation – in the sense that the work referred to is merely cited to invoke a routine summary or a position which is well-known to the readers. For the citation technicians it should not be too difficult to broaden the depth of their tool of analysis by incorporating this simple distinction. I can also imagine that, technically speaking, citation indices could be employed to locate controversies in science and to be of help in establishing the empirical patterns of variation to be found in different disciplines and across time. I doubt, however, that such an analysis will lead very far, if it is not embedded into a theoretical framework.

It would be utterly naive to put aside this story as an esoteric illustration of primitive thought. The construction of the world, as we see it, is in the most simple case achieved through assorting objects and grouping them together into classes, thus resulting in more or less elaborate classification system constitutive of social reality. It can be mainly animals which are assigned to special categories and kept there by rules of behaviour (TAMBAH 1969, DOUGLAS 1966) or occupations which form the bases for images of society, of classes, power and its sources (COXON & JONES 1974). They are systematic in the sense of following their own internal logic (TYLER 1969) which can be extracted and analyzed by means of such methodological devices as the method of free grouping and multi-dimensional scaling. From many studies in cognitive psychology (BERRY & DASEN 1974), it seems extremely plausible to argue that the world of experien-

ce can be 'sliced' in different ways. Certain ways of slicing exclude others, but there is always some overlap, as well as there are gaps – objects, persons, animals, concepts, which do not fit either way are therefore given the status of the anomalous, either to be explained away, or to be considered as dangerous and thus to mark by their conceptual ambiguity the interstices of social realities (LEACH 1969).

Scientific theories are certainly much more complex, elaborate, multi-leveled than the relatively simple classification systems which serve everyday purposes. But they also tend to slice the world of scientific experience in different ways, although most of the time within the framework of a legitimized scientific pluralism. There are partial overlaps and inconsistencies, even within mathematics as we know for certain since GÖDEL'S proof. The way in which it is sliced may appear arbitrary, but it is, like the cassowary, ultimately tied to its user by some special relation to other categories in his thought. "Knowledge is for the sake of action, and action is rooted in evaluation", writes KARL PRIBRAM and this holds in a very elementary psychological sense as well as for the more elaborate uses of knowledge. Knowledge, even in its most esoteric, seemingly useless and unpurposive forms, is still tied to other knowledge. This stems from the dynamic, constructivist nature of the production of knowledge, as PIAGET has demonstrated. Knowledge can be stored, but then it becomes subject to the processes of recall. As an active process, however, knowledge has to be used, by building up cognitive structures which in turn continue to produce new knowledge. This holds for the production of individual as well as collective knowledge, where individually produced knowledge is shared and standardized through the working of the communication structure.

The cassowary is given a special place in Karam taxonomy because it enjoys a special relationship to man in the Karam view of the world. This makes its classification at least potentially controversial, as it is easy to imagine contradictory ways of placing it into a taxonomic system. In order to become activated, controversies are not only dependent on social factors, such as a process of interaction, but they must be mobilized also within each cognitive system. This happens, we have suggested, through the kinds of links

which are established between cognitive structures, and the patterns of connectedness which emerge from linking cognitive operations to each other. It is not sufficient to arrive at different kinds of knowledge, to assign birds or concepts to different categories. This is merely the basis for potential controversies. In addition, this different kind of knowledge has to be linked to other cognitive structures such that *the difference will matter*.

Differences will tend to matter, however, when ever the clashing of different views on subject matter, methods, rules or interpretation, has deeply-felt and far-reaching consequences – in other words, when the uses to which knowledge will be put by connecting cognitive structures, will differ. The chain of consequences from the original innocent theorizing to its plugging into other cognitive structures and therefore to its use, may be visible from the beginning or may become visible only much later; it may be ideological, when it is tied to socially relevant action, or remain within the realm of thought to impinge on action only in indirect ways. The unfolding dynamics of controversies tend to render visible the connections between different sub-fields of the common collective map, either by creating new links or by making visible what has been present as hidden connectedness before. Controversies are therefore cognitive upheavals, through which the common collective map is restructured by providing new links or by bringing out into the open those that were hidden. The often observed appeal to a widening circle of arguments in the course of a debate is only one sign for it. The emotional commitment of the opponents is another which is not merely a blind ideological folly, conducted for reasons quite foreign to the debate, but it is an engagement for a specific use of the produced knowledge.

The interpretation of controversies as generating and depending upon the generation of new and different links to other cognitive structures, implies the rejection of a current view: the view that the logical structure of controversies, especially their logical incompatibility, is sufficient to explain their emergence. Logical incompatibilities, partial or total, may exist and yet go unnoticed or not be responded to for a variety of reasons, as is also the case with the so-called inconsistencies within an individual's mind. The indi-

vidual can live quite well with a number of inconsistencies, simply by shifting them around in different contexts, by forming ad-hoc or sometimes more general rules of exception, inclusion or exclusion, by developing an astonishing resourceful casuistry of definitions of a situation in ways that avoid cognitive hardships. It is only under special circumstances – and the laboratory conditions under which balance theory experiments operate are among them – that the usual escape-hatches of time and circumstance have been closed and the individual is forced to cope with his inconsistencies. The same holds for scientific theories: we have learned to live with their inconsistencies which are partially explained away or may go unnoticed or not cared for most of the time. It is only under exceptional circumstances that they become relevant and that the difference matters (it might be added here that information has once been described in BRUNNER'S phrase as "the difference which makes a difference").

Thus, to look for the different uses to which knowledge will be put and to ask how potentially controversial cognitive structure are tied to others and thus to possibly contradictory uses of knowledge, gives us some very general lines of inquiry to follow. What ought to be done therefore, is not merely to analyze differences in the sense of locating them and pinning them down, but to analyze the kinds of links which emerge in the cognitive system of those who are involved in a controversy.

IV. Minor incidents and major struggles: Social perception in the production of scientific knowledge

In a recently published account of a little known incident in the debate on spontaneous generation which confronted biogenesis with abiogenesis in the 1870's, FRIDAY (1974) shows how HUXLEY, a major proponent in the debate, incidentally discovered in 1875 that a secretion from the mould *penicillium glaucum* was able to inhibit bacterial growth, unconnected with oxygen deprivation. He recorded this observation in his notebook and in a single letter to TYNDALL, which has recently been found and served as starting point for FRIDAY'S work. Neither HUXLEY nor TYNDALL looked for an explanation

and neither told anyone about it. The reasons for this neglect are analyzed by FRIDAY, whose exposition shall be closely followed and serves well to illustrate the more general remarks made above. Basically, the reasons for the neglect were that both, HUXLEY and TYNDALL were fully engaged in the controversy over spontaneous generation which was, in FRIDAY'S words, "only one battle in a much bigger war". The abiogenesis debate was more or less settled in 1876, when TYNDALL was able to explain away much of the remaining evidence in favour of abiogenesis and as a consequence, the chief opponent, H.CH. BASTIAN, conceded defeat. From the documentary evidence presented by FRIDAY, it becomes very clear that TYNDALL regarded the abiogenesis dispute only as symptomatic for the larger struggle between the forces of materialism and idealism in science and society, and himself as a sort of fied marshal for the materialist forces. TYNDALL was so engaged in "clearing away the Bastian fog", as he called it, that he did not react to HUXLEY'S discovery. It simply remained an unanswered question. It would have been troublesome for TYNDALL to turn away from his central aim of fighting BASTIAN, just to clear up the question of mould toxins, which must have appeared to him a very peripheral concern. For HUXLEY too, there were obviously other matters to attend to. "And finally", FRIDAY concludes (1974: 69) the rejection of the Pasteurian explanation of the phenomenon in terms of oxygen deprivation might have left other findings of PASTEUR in doubt. If so, a tactical advantage would have been won by the supporters of spontaneous generation. TYNDALL was unwilling to yield such advantages".

What the 'minor incident' in a major struggle illustrates is not only that the conception of science of these involved in the debate was a political one in the sense of advancing progressive over orthodox thought, as TYNDALL expressed it himself, but also that neither HUXLEY nor TYNDALL were interested in natural phenomena for their own sakes. They took interest only in so far as the phenomena "fitted with their own materialist view of the world and provided ammunition for their war with the idealists". Although it is a case history, the case is not exceptional. Only a very naive conception of the production of knowledge in science or outside science could assume that natural phenomena are looked at

for their own sake. All knowledge is tied to other knowledge, be it in the making, or seeking confirmation. The boundaries of how far the uses of knowledge are to be extended, are flexible and malleable. Although there is no way of ascertaining what went on in TYNDALL'S and HUXLEY'S mind, it is reasonable to assume that they both saw their concerns as legitimate scientific concerns. When TYNDALL wrote that he wants "to act the part of a conservative rather than a destructive by gradually preparing the public mind for inevitable changes which without this preparation might take revolutionary form", it is not politics that he talks about, but science couched in the language of political metaphor.

There can be no cognitive structure so isolated and turned upon itself that it would not tend to link up with others. Even in the case of very cohesive and closed theoretical systems, like the research programmes described by LAKATOS and ZAHAR, the tendency to link up with others and to be used, is observable. Contemplating the question why EINSTEIN'S research programme superseded LORENTZ', ZAHAR asks at one point (1973: 242).

"If the merit both of Kepler and Einstein *only* consisted in ridding physics of unnecessary epicycles, then the importance of these two physicists in the history of science is very much overrated: Copernicus and Lorentz did all the creative work, and Kepler and Einstein only applied Occam's razor in order to demolish the expendable metaphysical scaffolding used by their predecessors. Moreover, Copernicus knew that the path of the planets were not circular, hence that his epicycles were part of the scaffolding Lorentz realized that he did not need the Galilian coordinates in order to deduce the null results which he set out to explain. *If so, Kepler and Einstein contributed to the economy of thought and not to the growth of knowledge. This is an unacceptable conclusion.*"

What is of interest here from our point of view are the reasons provided by ZAHAR for the rejection of the conclusion at which he arrived. It is unacceptable, he argues, because getting rid of the COPERNICAN epicycles was not an end in itself, but was "subordinate to the needs of the new heuristics". Thus, it are the heuristic devices of the new research programmes which regulate and control from a higher level of thought the theoretical developments which take place on the lower level. We are presented here with a hierarchical image of scientific connectedness, as the

uses of the lower level knowledge structures are regulated by the higher ones.

The connective ties can, as in the TYNDALL and HUXLEY case, but need not lie outside what are in any case rather arbitrarily defined boundaries of science or a discipline. In a penetrating analysis of a controversy opposing two evolutionary theories in paleontology, GRENE (1958) has shown the different structures of theories by pointing out their different uses as theories. These are different conceptions of what evolutionary theory in paleontology should look for and what kind of theory a theory explaining evolution should be. After having drawn attention to the parallelisms between the Neo-DARWINIAN theory advanced by SIMPSON and the theory advanced by the Swiss paleontologist SCHINDEWOLF — parallels which yield "a nicely balanced pair of theories", as one theory stresses continuity and the adaptive character of all evolutionary change, while the other stresses discontinuity (novelty of forms) and the non-adaptive character of major changes (1958: 113). GRENE proceeds to show how different levels of widening disagreement appear. The ones on the merely verbal plane could easily be cleared up, only to bring forth those on the plane of visual imagery as each proponent employs a different visual model. On the plane of attention, it is very obvious that what are central concerns in one theory are at best peripheral ones in the other. On the conceptual plane, it finally becomes apparent that the opponents stand at different places in relation to their subject-matter, their outlook is very different. But from these differences emerge different conceptions not only of what is to be explained — continuity or discontinuity — and what is to be accorded priority as a consequence, morphology or phylogeny — but how, through what kind of theory these differences in outlook are to be translated. The DARWINIAN explanation denies structure in the sense of reducing it to its conditions. It provides an explanation of evolution in terms of the mechanistic tradition of explanation, i.e. it must be both logically simple and automatic (1958: 188). It is simple in that it unites the relevant phenomena under one hypothesis which is conceived in terms of existence and non-existence. It is automatic since its only hypothesis interprets organisms mechanistically, and their evolution as produced by random errors and by natural selection which are

both mechanistic automatisms. In the SCHINDEWOLFian kind of theory, however, a duality of logical levels can be found, as between continuous and small-scale conditions versus discrete and comprehensive patterns, which means that the explanation must forego its unitary character.

What is of interest from our point of view are the parallels which emerge between these two highly complex and elaborate theories on evolution and certain very elementary processes of cognition, such as categorization. What are in SIMPSON and SCHINDEWOLF'S case procedural rules to create structure or to avoid it, to assign priorities to different steps which must lead to different procedural routes to be taken and which, as an end product, manifest themselves in different *types* of theories, is an elaborate and refined version of certain elementary cognitive processes. In the case of categorization, for instance, elements are divided into groups, the same elements are grouped into classes and people have rules on how clusters are to be formed and on the meaning of these identical groups (COXON & JONES: 7ff.). Categorization of any wholistic image and producing theories on evolution can both be conceived as problem-solving tasks, which must be analyzed in their procedures and procedural rules, if one is to understand their construction.

It would be utterly misleading, and the danger exists when one analyzes controversies in science, to satisfy one's curiosity after having been able to state individual differences, such as those between 'lumpers' and 'splitters' in categorization, or between 'unitarians' (or mechanistic theorizers) and 'multi-level' theorizers in science. If such differences emerge they can merely serve as starting point for the further study of the underlying cognitive processes which lead to different theorizing and therefore to different world-constructions. Our previously advanced hypothesis would again lead us to assume that these differences – in cognitive style one is tempted to say – are linked to the further uses of the knowledge thus produced and are part of the overall connectedness patterns of different regions of thought.

The important part played by perception, and thus by cognitive processes, in controversies, can also be illustrated by the debate which went on between LEIBNIZ and NEWTON and their followers for almost 20 years. The controversy was funda-

mentally a clash of philosophical world views on the nature of God, matter and force (ILTIS 1973: 343). The debates were conducted between two different organizations of knowledge based on metaphysical and mechanical principles. From ILTIS' analysis it seems that the factors of social interaction played a major part in colouring the perception of the adherents. Emotional commitments and loyalties to their masters formed the specific social conditions under which the followers of NEWTON and LEIBNIZ worked and led them to defend their respective world views by reinterpreting the challenging experiments from the other side in a way that supported their own philosophy. They were unwilling and unable to see the other side's valid arguments. But if social factors influence perception to such a degree, if theological and metaphysical commitments on the part of the experimenters caused them to interpret the mechanical experiments in a manner consistent with their natural philosophies, despite the errors and the lack of conviction in their results, as ILTIS demonstrates, then these underlying cognitive processes should be investigated in their own right, in order to determine the procedures upon which they are based and to discover when they are no longer self-supportive, but begin to collapse internally or to converge externally. The recognition that both viewpoints could be valid began to take place some 20 years later, when integration between the two theories began to occur. Apart from the social dynamics involved in the processes which contributed to a partial solution of the controversy, we would again expect that what changed were the uses to which the opposing systems were put. How could shifts in the evaluation of theories be explained otherwise, even, or maybe, precisely in those cases where the incompatibility of the two viewpoints was uncontested, regardless of how strong the influence of external and notably social factors might have been in leading to the perception of incompatibilities.

Such was the case, for instance, for EINSTEIN'S equivalence in gravitation theory (WOODWARD & YOURGRAU 1973). It is on the historical record that EINSTEIN himself thought of them as being incompatible and we are told by one of MACH'S biographers (BLACKMORE 1972: 259) that "no rational person could or should hold both" (views). Nevertheless, after a generation of physicists had pursued the vision of a unified theory

without much success between the 1930's and 50's, a kind of MACH renaissance can be witnessed today. Although it is not clear yet where the onslaught of papers on MACH will eventually lead, whether there will be a revaluation of MACH'S principle and a partial convergence between the formerly incompatible theories, the development as such points again to processes of cognitive restructuring through changing connectivity patterns. It is not merely the fact that the younger generation of physicists, those who have not personally participated in the frustrating search for a unified field theory, are courageous enough to tackle the problem, as WOODWARD & YOURGRAU suppose, but also the fact that "since 1960 a shift from relativity theory as a mathematical formal science, divorced from the main stream of physics, to a science drawing inspiration from observation and, in turn, stimulating observation" has occurred. This general change in direction of the further uses of knowledge may take the form of connecting a theory to developments that go on in what is seen as the mainstream, or by putting the existing relations into question. At the same time we are forcefully reminded that incompatibility — in itself a perceptual phenomenon — is always relative and may undergo varying evaluation in the course of time, not because new knowledge has emerged that could serve as unifying platform for the formerly incompatible theories (although this could also happen), but because the outside cognitive links have been altered.

The negative test case is provided by all those controversies which continue without ever finishing, truly unexhaustable and intractable. For some reasons, these kinds of controversies appear to occur particularly frequent in the field of philosophy, where arguments can go on without successful termination "even, or perhaps specifically, by people who have been carefully trained in the technique of logical thought" (CRAWSHAY-WILLIAMS 1957: 3). In his analysis of a series of such intractable controversies, CRAWSHAY-WILLIAMS reaches the conclusion that their characteristic nature is their indeterminacy with respect to context or purpose. They cannot be terminated, we can translate, because they are not tied to any specific use, because they have been cut off from being connected to other cognitive structures. Thus, they are merely shifted around and around, aloof in their cognitive unconnectedness.

V. 'Non-controversies' and mutual indifference

This is how I would like to call the numerous occasions where cognitive incompatibilities, in varying degrees, exist and are visible either retrospectively or to an outsider. Yet they are not articulated or have not — or not yet — been activated. The other side, against which contrasting interpretations or results appear to be directed, fails to respond to the challenge, or is simply unimpressed by the differences. Such occasions are 'non-controversies', as they take the place of a controversy that has not materialized despite the cognitive potential being there. The lines of fission that separate the two sides can be as great as those that become visible during a controversy, yet neither side is willing or bothers to commit itself and to engage fully into a controversy. *The difference does not matter*. Why not? To be sure, explanations can always be found, by pointing to the absence of some factor, most probably social in nature, which might account for the failure to light up a controversy. But it might be more fruitful, from our perspective, to look at non-controversies as being due to different *modes* in the production of knowledge. Although the results might be similar enough to yield potential for disagreement over interpretation and the like, the methods by which knowledge is produced are apparently too divergent in non-controversies.

One side, for instance, may consider as data what the other side completely fails to consider (this may be one of the reasons for the mutual indifference prevailing for instance, between anthropologists and psychologists, as neither is much impressed with the instruments and methods for producing knowledge of the other side, as COLE et al. (1971) have observed, or methods and techniques may be used which are completely unacceptable for the other side. Yet, hostility does not take the place of mutual indifference, as there is no interference in the way how each side produces knowledge it considers valid. There is a cognitive overlap on the phenomena to be explained, but it is neatly separated at the same time through the whole set of procedures, standards, methods, concepts and techniques, which consistently mark the mode of production of knowledge of each side. Interference arises, we would suggest, whenever the mode of production of knowledge itself is called into question, which is likely to occur only then, when the

knowledge produced is put to uses at contradictory ends.

From the existence of non-controversies we may infer that it might be quite useful to analyze controversies and non-controversies alike from the more general point of view of knowledge production as information processing (NEISSER 1967, KLIX 1971). In line with the application of some general concepts of information processing to this specific area of knowledge production, theories could be compared to the cognitive mechanisms or "schemes" as they are sometimes called (NEISSER 1967), through which data that have been received as input stimuli, are recalled and transformed and thus processed to serve the production of additional knowledge. Disagreements will emerge whenever the "data" are processed in different ways, which implies that they will be used – still in the realm of cognition – for different ends (NEISSER 1967: 286ff.).

This view has also consequences for the so-called accumulation of knowledge. What can be accumulated, in the sense of amassing and storing, is merely input information. This input, however its size or content may be measured, is clearly of minor importance in the overall production of knowledge, compared to the processing mechanisms. For science, these processing mechanisms are essentially theories with their research paraphernalia. They constitute the scarce factor in the overall production process, as they impose limits on what can be done with the received and stored input information. They are the ones that create the typical 'bottleneck' situations in the development of knowledge. Conflicts are therefore likely to emerge over what is to be adopted as the appropriate way of processing input information, i.e. how it is 'best' to be organized. This again however, depends on some kind of end that is to be achieved. Although it may be assumed that some processing schemes are inherently superior to others, because they unite a greater number of acknowledged advantages, there are many others for which clear-cut criteria of their respective advantages cannot be found, nor hopefully be developed. This situation is the result of the heavy dependence of any scheme of knowledge production on its later uses: this holds for the processing of elementary visual stimuli, for the classification of birds and for any scientific organisation of knowledge as well. The direct-

edness in information processing has been shown to be present already in the most elementary cognitive processes, and manifests itself there as 'foci of attention' or 'critical configurations of stimuli' and the like. They are operative in science, where they manifest themselves in controversies. It is for this reason that controversies are endemic and inevitable in any production of knowledge, including the production of scientific knowledge.

Accumulation of knowledge is therefore an illusion if it is meant in the sense of amassing, possessing and storing more and more knowledge. This could merely be the case for input stimuli, which are only potentially useful in view of their further processing occurring through organized 'schemes', notably through theories with their operative research practices. Which organization schemes are better, however, depends on what knowledge is to be produced for, including the extremely important cognitive links to be established to other forms of knowledge production. The success of the Western type of science production seems to depend partly on unification of purpose and the wide-spread acceptance of a general sense of direction, at least until recently. In addition, Western science is characterized by a relatively high internal cognitive cohesion, meaning that the cognitive links within different regions in scientific thought are relatively well tied to each other, while there is a low degree of cognitive cohesion and only weak ties to other forms of knowledge production outside science. This makes it possible to regard science as another form of eco-system, beset by its own contradictions which manifest themselves above all between the amount of energy needed for its maintenance and the energy needed for innovative processes (BLACKBURN 1973). Controversies emerge at those strategic locations where not only the choice between alternative processing schemes of knowledge is given, but where the choice is linked to other cognitive structures and forms of knowledge production. Non-controversies seem to signal that the form of knowledge production itself is – still or already? – differentiated.

Finally, we do not know what an analysis of the empirical patterns of variation in controversies as well as non-controversies, when compared over time and across disciplines or sub-fields, would yield and how these empirical variations are to

be interpreted. Are there more or less controversies, more or less non-controversies in science today, when compared with 20, 70, or 150 years ago? And if there are more, or less, *what does it mean?*

These are only some of the many questions which this essay on different modes in the production of knowledge in science hopes to have raised.

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