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Social and Scientific Method or 'What Do We Make of the Distinction Between the Natural and the Social Sciences?"

Karin Knorr

1. Introduction: The Two Sciences

Ever since the rise of the social sciences, social science methodology has been a much disputed issue among social scientists and philosophers of the social sciences[1]. Much like the history of social theory, the history of social science methodology is a history of controversy, and this tradition is quite naturally renewed with every appearance of a new conception of social life. Most recently, methodological discussions have centred around discrediting "positivistic" procedures by showing their essential inadequacy in dealing with the social world. In these discussions, established methodological procedures, such as survey research or laboratory experimentation, were linked to a model of scientific method identified with the natural sciences, and new social methodologies continuously emphasized their rejection of this model. In fact, new rules of social science method have been developed, displayed and defended in a constant dispute of the standard set by this model, and they have made the departure from this standard the declared goal of an indigenous social methodology.

Perhaps not surprisingly, the standard itself has found little consideration in the respective dispute. While the "positivistic" conception is vigorously rejected as a model for social science methodologies, it is more or less taken at face value when it is referred to the natural and technological sciences. Philosophical investigations which for some time now have directly questioned this model as describing correctly the natural sciences appear to be either ignored or declared irrelevant for the discussion. When they are introduced into the picture, they serve as some sort of background noise against which the original conception of the natural sciences is painstakingly reiterated[2]. This paper does not aim at a global rejection of the distinction between the natural and the social sciences, let alone at a rejection of relevant differences in particular methods, modes of analysis and procedures. It does, however, aim at a reconsideration of the distinction as made customarily in discussions of social science methodology. This reconsideration will be based not only on the above-mentioned philosophical investigations, but also on empirical findings which emerged from observation of the actual research process in the natural and technological sciences. Philosophical evidence suggests that method in such sciences is based upon the same kind of cycles of interpretation commonly associated with the social sciences (see Kuhn, 1970; Feyerabend, 1975; or see the summary in Suppe, 1974). Empirical research suggests that natural science investigation is grounded in the same kind of situational logic and marked by the same kind of indexical reasoning which we are used to associate with the symbolic and interactional character of the social world (see Knorr, 1977,1980; Latour and Woolgar, 1979).

Note that we do not refer here to evidence derived from studies of the particularly social dimension of science. Both the philosophical arguments and the observational material focus on what came to be called the cognitive aspects of science, that is, they focus on scientific reasoning and on the technical production of research. Thus the argument here is not that natural and technological scientists act like other social actors when they talk to their peers or when they confront their superiors in an organizational hierarchy. Rather, the argument is that natural science *method and procedure* appears to show sufficient similarities with social science method in order to put into question the by now firmly established and routinely cited distinctions between the two sciences. Given the relatively recent and sometimes preliminary character of the available evidence, this argument is prone to raise, rather than to settle, issues. However, if social method is to be placed in the context of social life, it will also need to be relocated with respect to its natural science equivalent, and even preliminary evidence will be one fruitful step in this direction. It is the purpose of this paper to illustrate the philosophical arguments and the empirical results derived from my own laboratory observations, and to argue for the beginnings of such a relocation.

2. The Universality of Interpretation and Understanding

The basic distinction between the natural and the social sciences as made customarily in more recent discussions of social science methodology (see Filmer *et aL,* 1972; Giddens, 1976) is so well known that it need not be repeated here in detail. On a most general level, the distinction rests upon attributing symbolic quality to social as against natural life, and upon attributing an interpretative, dynamic, and interactional quality, which is sometimes identified with hermeneutics[3], to social as against natural science method. While several different lines of argument are usually derived from these qualities, all seem to endorse the assumption that the difference between the natural and the social world is that the latter does not constitute itself as meaningful. The meanings it has are produced by men in the course of their practical life; in contrast, social life is produced in terms of the active constitution and reconstitution of meanings by the subjects themselves.

In recent discussions of the methodological status of the social sciences, there is often a tendency to leave it with this difference. However, as already pointed out by Dilthey[4], different regions of ''fact" do not exist; rather, they are constituted by a certain methodology and epistemology. Hence, a circumscription of the object domain is not sufficient for a logically cogent delimitation of the two sciences. Dilthey himself considered the re-enactment of meaning on the part of the observer or social scientist as the basis of the interpretative approach to social reality, which he advocated. Since this approach leads to subjectivism, it was subsequently rejected. Criticising Dilthey, Gadamer (1965) pointed out that interpretation is not a question of entering the preconstructed meanings of social life through individual empathy, but rather a question of mediating and translating between two traditions. His "universality of hermeneutics" refers to the fact that *both* social and natural science enquiry involve tradition-bound theoretical presuppositions, a contention with which we are more familiar today in the form of three distinctive lines of argument which are all linked to the notion of interpretation:

(1) The first line of argument centres around the *denial ojbrute facts.* In essence it holds that data which are beyond the challenge of rival interpretations are unattainable by science.

(2) The second line of argument refers to the *circularity* of interpretation. It implies that any interpretation of an event or text ultimately depends on yet another set of interpretations, thus leading to an infinite regress of meanings.

(3) The third line of argument can perhaps be described best in terms of Wittgenstein's notion of a *language game.* It conceives of interpretation as a condition of the possibility of data in general and emphasizes the interconnection and interdependency of various levels of interpretation.

Let us not contend the relevance of these lines of argument for a methodology of the social sciences. The question here is whether the natural sciences can be characterized justifiably by denying the existence of brute observation, by reference to the circularity of their interpretations, and by assuming a language game character of their various traditions. Least concrete and hence most difficult to establish, or to discard, is the language game aspect. It is, of course, the main thrust of Kuhn's work to argue that normal science research is looked into paradigmatic traditions which are constituted by systems of hierarchically structured assumptions and conceptions, these differing sufficiently between traditions as to make them appear as internally coherent but mutually incommensurable language games. This thesis hinges significantly on the role played by scientific observation as an independent arbiter of scientific theories, and on the question of whether scientific theories can be fully defined independently of tradition-bound assumptions and pre-interpretations. Thus the thesis hinges on whether we can assume the existence of some form of brute facts in the natural sciences, and on whether scientific theories are exempt from cycles of interpretation.

Debates in the philosophy of science which have a bearing on the question of circularity have been raging for quite some time now, and the results seem to show that interpretative regress is by no means limited to the social sciences or to the humanities. For example, logical investigations of the nature of the "rules of correspondence" between observational statements and theoretical hypotheses have shown that the former are not strictly deducible from the latter (see particularly Nagel, 1961). Consequently, what counts as an observation relevant to evaluate a certain theory can only be established on the basis of certain assumptions. Furthermore, this process is not one of simple bivariate correlation. Observation and the measurement of an observation involve as another dimension of pre-interpretation a series of background theories which need themselves definite justification (see Quine, 1969; Lakatos, 1970). Finally, it has been shown that we cannot require that theories in the natural sciences must be fully interpreted, except in relation "to our overall home theory". Our only recourse is to "paraphrase in some antecedent, familiar vocabulary". In practice, says Quine (1969, p. 49), we end the regress of background languages "by acquiescing in our mother tongue and taking its words at face value". In sum, we seem to be confronted with a situation in which interpretations (observational facts) can only be explained and justified by reference to other interpretations on which they partly depend (theories) and by reference to their relation to the whole, our overall "home theory", which is an exact definition of an interpretative cycle called hermeneutic (see Taylor, 1976, p. 164) in the cultural sciences.

The theory-ladenness of perception, which corresponds to the rejection of brute facts, the first line of argument mentioned before, appears here as just one component of this interpretative cycle in the natural sciences. As stressed by Taylor (1976), theories of perception which claim that natural science observation allows access to brute facts are largely a thing of the past. In the form of a theory of an independent observation *language* the conception of the source material of natural science enquiry as brute facts has recently been challenged most spectacularly by Feyerabend (1975). Feyerabend's thesis of the theory-ladenness of observation is amply documented by historical material. Although the thesis cannot be said to be accepted generally by philosophers of science, new work in the area does tend to endorse some version of the theory-ladenness of observation (see Hesse, 1974).

3. Interpretation and the Laboratory

Logical analyses of scientific inference, of observation and of theoretical statements do seem to support the thesis of the universality of interpretation with regards to the aspects listed above. However, there is another sense in which the issue of interpretation has been denied to natural science enquiry. This is the sense in which interpretations and negotiations of meaning appear to be eliminated from conceptions of scientific action such as the one by Habermas, and from conceptions of scientific reasoning such as the one by Garfinkel[5]. Needless to say, any brief look at a scientific laboratory will easily disprove such conceptions[6].

What would we expect such evidence to look like, given the familiar testimony for interpretation in historical enquiry, in the understanding of action-meaning by the sociologist or in anthropological investigation? According to Taylor (1976, p. 153), the object of interpretation presents itself as "confused, incomplete, cloudy, seemingly contradictory — in one way or another, unclear". It is "describable in terms of sense and nonsense, coherence and its absense" like the symbolic objects which constitute a text. In the laboratory, these symbolic objects are provided by the constantly generated measurement traces, that is, by graphs, figures, printouts, diagrams, tables and the like. They are also provided by the living experience of a colour change and of the consistency of a mixture, by the look of a test animal or by the smell of a chemical reaction. Both the seemingly objectified results of a measurement procedure as well as the objects of living experience require interpretation. First, they must be recognized as an instance of something and thereby assimilated using an everyday term or a scientific concept, of which we have heard that they are subject to interpretation. Secondly, and perhaps more importantly, the scientists must "make sense" of these recognitions. Partly this happens already when an instance is recognized as something, in all cases where simple descriptions in standard observation terms do not clearly fit which then requires conscious-decision making or the application of particular identification procedures. Partly, the question is to establish the "meaning" of some recognized instance in the context of the concerns of the situation, exactly like the social scientist who interviews a person has to establish the meaning of an utterance which he recognized (that is heard) with respect to the concerns of the interview situation. The scientist who exclaims "the staff has gone white" gives an example of a so far unproblematic recognition of an instance in observational terms. His subsequent remark that "the protein was precipitated" established at least part of the meaning of "the staff has gone white" in the respective context. If there was anything incomplete, cloudy or confused prior to these statements, it was at least not apparent. But what do we make of entries in the official laboratory protocol book such as the following:

> A dry run using reagents only and the sep funnel, proceeded smoothly. However, problems were encountered when the first material, 286-6A also 6B, C was attempted. A murky, possibly imaginary interface appeared only after abt. 1 *Vi* hrs., separating an opaque, purplish upper layer which did not clear from a blackish lower layer. Further, the "inferface" could not be seen moving when the outlet was opened to drain off the lower fraction. Lastly, filtration of the extract (drawn from the top of the sep funnel) proved impractical: the cotton plug was overloaded with particulate matter almost immediately . . .

Evidently, the technician who wrote this note had difficulties to estabish in observational terms "what happened" to his material, and from the rest of the entry it is clear that the group had all the more difficulty to interpret the occurrence within the context of the experiments under way. Needless to say, many of the happenings in the natural or technological scientist's laboratory present themselves exactly as "unclear" as the objects of interpretation Taylor postulates for the social sciences. And if anything, quantitative measurements or analogical displays pose even more of a challenge of identification and secondary interpretation. Let us look at just one incidence of a microbiologist and mathematician plunging through data:

> *Question:* "And when you got your data on the relation between moisture and stability, the optimum was immediately apparent?"
> *Answer:* "'It wasn't immediately apparent as a matter of fact, it was a quibblical. . . (inaudible; tries to find a plot). Actually, what happened was, quickly was, that we plotted stability measured by some curve, it does not matter by what, as a function of temperature, and we found something that looked (searches again; cannot find it) . . . We plotted water content at two temperatures, and one was uh, uh (writes on blackboard) something like that, and one was something like (that), so one could draw a line say O.K., this is zero degrees and this is 95 degrees, it was something like this. Now let's just look at it, like that, because this uh, that, that was the first clue, that is how good were the data you see, most people would say O.K. well that's about (it) you know, one is high and one is low, so that, O.K., what is an anomaly there, it looks as though this thing is going this way (points it out), and it turns out in fact that that is in fact what it does, but we only had a peak here. But

> if you wanted to be careless about the observation you could easily say that's a straight line and that's a straight line."
> *Question:* "Why did you not see it as a straight line?"
> *Answer:* "Because I don't, because I, uh, most people do . . . I am always looking for something, some anomaly . . . O.K., that said there is a premis, the premis is that there is a local isotherm, they reflect different kinds of things. . . We looked at the physical chemistry, nuclear magnetic resonance, electron spin resonance, x-ray defraction to try to show that in fact, that this was not just, that these were real, that these were not artefacts, they represented real differences. And I think, people are still questioning some of it, but I think uhm . . .

For a datum to appear as a "real difference", some processes of interpretation, negotiation and of mobilization of contextual information are evidently required. Like the ethnographer in a foreign culture, the scientist in the laboratory is confronted with much noise and with unlimited uncertainties out of which he makes sense by drawing upon concepts and procedures which remain temporarily unquestioned. Like in the case of ethnography, the uncertainties relevant here appear on the level of recognition, identification and making sense of data and observations. It comes as no surprise then that scientists know about the pay-off of living experience in this sense-making process, a benefit which appears to have been almost forgotten in some social sciences. In a case, in which I participated, a scientist manipulated physically six different protein samples before taking measurements. Struck by the difference in ''the feel" between the samples which had been treated in the standard fashion, that is, by a conventionally employed method, he became, as he said, "suspicious" of the method. He then varied the method such as to reach samples of equal' 'feel" before the respective quantitative measurements were taken. This allowed him to dispute, in a particular paper, a method which had lasted for "at least thirty years" and which had been used "almost universally". Upon my question he said that' 'certain things can only be realized if you do the experiments yourself'. He had done the same kind of experiments six months before assisted by a student, but because he had "never looked at the stuff" himself, he did not get any profitable "ideas", and "could not make any sense of the data" the student had obtained.

4. The Feedback Thesis

Let us assume that the scientific laboratory is indeed the locus in which the scientists' sense-making activities constitute, and deconstitute, dynamically what is the case, just like social situations are the locus within which meaning is socially constituted in interaction. Let us assume further that these sense-making activities have more resemblance with ''understanding*' as an act in which experience and theoretical apprehension are fused rather than with "explanation" as the "application of theoretical propositions to facts that are established independently, through systematic observation" (see Dilthey, 1958-77, Vol. 5, p. 143 and Habermas, 1971, p. 144 for these terms). Finally, let us grant that circularity and pre-interpretation of observation and experience mark not only the social and cultural, but also the natural and technological sciences. There remains one more line of argument in the discussion of these matters which we have yet to look at. This is the argument that causal relations in the social and cultural sciences are "malleable in the light of the development of human knowledge", which means that they can in principle be recognized by men, and thus be incorporated into their actions in such a way as to transform them. Such feedback changes are a direct consequence of what Giddens calls the "double hermeneutic" of social science, that is, the fact that it applies its (second level) concepts to the first level constructs through which social actors have already preconstructed the social world[7]. Or to use a formulation of Giddens:

> The concepts and theories produced in the natural sciences quite regularly filter into lay discourse and become appropriated as elements of everyday frames of reference. But this is of no relevance, of course, to the world of nature itself, whereas the appropriation of technical concepts and theories invented by the social scientists can turn them into constituting elements of that very "subject matter" they were coined to characterize, and by that token *alter* the context of their application.

Negal has argued that such "self-fulfilling" or ''self-negating" predictions are not unique to the social sciences, since in the natural sciences observations about a series of events can influence the course of these events as well. However, Giddens stresses that such indeterminacy is "logically distinct" from the social sciences, where ''the point of the matter is that 'indeterminacy' . . . results from the incorporation of knowledge as a means to the securing of outcomes in purposeful conduct".

There seem to be two assumptions on which this and other formulations of the feedback thesis rely. First, human beings have *causal agency* not found in natural reality, and secondly, there is a level of *conceptual mediation* (consciousness) in social reality through which causal agency is stimulated to responsive, the-course-of-events-changing actions. While the issue here it not to debate whether conscious reflection or the level of conceptual mediation is a distinctively human feature, a point can be made, however, as we shall see later, against limiting causal agency to human beings. Yet the consciousness part of the thesis raises questions too. First it is not at all clear that *all* behavioural reaction to knowledge based interference with the course of social events involves a level of conscious reflection. Presumably, consciousness-raising techniques as stressed by political groups would be utterly redundant if that were the case. Secondly, it goes almost without saying that consciousness about some situation does not *automatically* trigger a response which is behaviourally relevant. Again, the conditions under which it does or does not do so are far from clear. We can perhaps speculate that one minimum requirement for a reflection-based response is that the state that is brought to attention is not linked. Yet this dislike would have to be made causally effective in the face of the social, psychological, material and other costs and constraints which confront any change in a course of action. Our own practical experience in social life suggests similarly that consciousness and reflection are but *one* kind of variable open to manipulation in the complex process of ongoing events and not the *sine qua non* of their symbolic change and variation. Furthermore, it can be argued that if social reality is symbolic the fact that interference with social reality (through communication, for example) as well as some potential course-of-event-changing response (via reflection) will also be symbolic refers to not more than the specificity of tools, problems and procedures to a particular domain. This specificity, however, in no way stops short of the natural sciences. After all, nobody claims that the reality of physical bodies and the reality of bee hives are one and the same in the natural sciences which are otherwise characterized as unified, or that they require the same kind of tools and the same procedures of investigation. What matters perhaps is that some previously given conjunction of events can be changed through *appropriate* interference with these events under specifiable conditions. If we accept such a formulation, the fact of human consciousness and the specificities it requires may be distinctive to some social sciences, but they are at the same time the *equivalent* to the fact of instinct-triggered responses and the specificities it requires in biological disciplines, or to the fact of the operation of forces between physical bodies and the specificities it requires in the sciences of these bodies. This reduces our grand model of differentiation between the two sciences to the long-standing insight that different sciences and specialities construe their object domains differently as specific domains, and that they operate, and are called to operate, accordingly.

If the reference to consciousness is not necessarily compelling, what about the assumption of causal agency to which we have referred before? We might even suggest that it is the idea of causal agency which lies behind the whole consciousness argument, since the latter is usually combined with some reference to action or to an active response. To the social scientist, the idea of action as self-governed, interpreted agency in contrast to behaviour is familiar, at least since Max Weber. In contrast to this concept of agency, the classical paradigm in the natural sciences defines events such that they appear directly juxtaposed to any conception of action. As summarized by Bhaskar (1975) the classical paradigm states (1) causation is external to events, (2) matter is passive, (3) fundamental entities are atomic, (4) there is no internal structure and pre-formation of entities, and finally, that (5) qualitative diversity is secondary. Bhaskar claims that the idea that the source, the trigger, the stimulus of natural science events is always *extrinsic* and that natural science objects are patients rather than agents "is a pure prejudice" which can be traced back to a mechanical world view long outdated in physics. It has to be replaced by a conceptions of events as "things" which possess powers and liabilities, and which may have behaved in ways they actually did not behave (see also Harre and Madden, 1975). Statements of *laws* must accordingly be seen as [4] 'statements about the tendencies of things which may not be actualized, and may not be manifest to men''. Yet if * 'laws" in the natural sciences are no longer seen as statements about *constant* conjunctions of events or experiences, the thesis that there *are* no such constant conjunctions of events in social life *because* of an agent causality which is distinctively different from the natural world also misses the point. Let me cite in some detail the conception of a natural world which acknowledges the causal agency of its objects, this giving credit to modern developments in the physical and biological sciences (Bhaskar, 1975, p. 105):

> Reflect, for a moment, on the world as we know it. It seems to be a world in which all manner of things happen and are done, which we are capable of explaining in various ways, and yet for which a *deductively justified prediction is seldom, if ever, possible.* It seems, on the face of it at least, to be an incompletely described world of agents. A world of winds and seas, in which ink bottles get knocked over and doors pushed open, in which dogs bark and children play; a criss cross world of zebras and zebra-crossings, cricket matches and games of chess, meteorites and logic classes, assembly lines and deep sea turtles, soil erosion and river banks bursting. Now none of this is described by any laws of nature. More shocking, perhaps, *none of it seems even governed by them.* It is true that the path of my pen does not *violate* any laws of physics. But it is not *determined* by any either. Laws do not describe the patterns or legitimate the predictions of any kinds of events. Rather, it seems they must be conceived, at least as regards the ordinary things of the world, as *situating limits and imposing constraints on the types of action possible for a given hind of thing.* (My italics)

If causal agency is not to be limited to the actors of the social world course-of-event-changing reactions in response to interference with these agents will no longer do as a distinctive feature of social life, and historicity in the sense of agency-caused changes in courses of events will have to be allowed for in nature as well. If natural laws are to be thought of as specifying the conditions and as limiting the possibilities of types of relevant actions rather than as constant conjunctions of actual events, the apparent lack of such constant conjunctions of events in social life will no longer serve as a distinguishing characteristic between the social and the natural world. A conception of social "laws" as specifying the conditions and limiting the possibilities of types of social action seems on the contrary quite compatible with all the distinctive features which are commonly attributed to social, as against natural, reality, for example, with the "uniqueness" of social events, or with the above mentioned "historical and cultural variability" of empirical generalizations, with the "unpredictability" of social events and with the necessity to adapt procedures and social techniques to concrete fields of action[8]. Analogies which liken *natural* laws to the *rules* of a game and empirical events to its actual play on some particular occasion confirm this compatibility (see Anscombe, 197t, p. 21). Such analogies remind us directly of Winch's (1958) famous thesis that social reality must be explained in terms of rules rather than in terms of natural laws as traditionally conceived. If the laws of nature have to be understood as "normic and transfactuaP' statements *analogous* to rules (Bhaskar, 1975, p. 92), how would Winch's demarcation of the social sciences, which rests upon a presumed essential difference between normic social rules and factual natural laws, have to be modified? Of course, much depends here on the further specification of the rule-like character of natural laws. For example, can we think of these rules as a function of some given and possibly long-lasting state of a specified universe of events which is itself subject to agency-effectuated change in contrast to previously accepted invariance-ideas? But as suggested before, the point here is not to attack the problem of an adequate epistemological conception of statements of regularities in the natural and social sciences, nor to attempt to re-settle the question of the distinction between the two worlds. Neither can it be the point of the present discussion to argue for a re-unification of the respective fields of research with respect to their specific methods and techniques. The point here is rather to argue for a reconsideration of the routinely made and ritually cited distinction between the natural and the social sciences in the light of new conceptions of natural science research and methodology. And the point thus is to argue for a perspective on the natural sciences which uses the same conceptual tools and microscopic procedures which have led to a new understanding of social method and social life.

5. The Indexical Logic and the Opportunism of Research

To apply to natural science research a perspective informed by recent advances in the social sciences means above all to recover the context of scientific action, as it meant to recover the context of social interaction in sociology and social psychology in general. In other words, it means to suspend, for a moment, the disengaged models of scientific practice which have been proposed by philosophers and sociologists of science alike, and to substitute for these models close observation of the black box of scientific action with which these models have left us. Close observation shows not only that the laboratory buzzes with interpretations as argued above, it also shows that the meanings of the laboratory, and consequently also the selections which constitute a piece of research, depend on the *research situation.*

In recent years, the notion of situation and the idea of context-dependency has gained most prominence in ethnomethodological and related approaches, where it stands for what ethnomethodologists have called the "indexicality" of social action. The concept of an indexical expression is taken from Bar-Hillel (1954) and was originally coined by Peirce (1970) to refer to the fact that a sign may have different meanings in different contexts, and that the same meaning may be expressed by different signs. Within ethnomethodology, indexicality refers to the location of utterances in a context of time, of space and also of tacit rules. In contrast to a correspondence theory of meaning, meanings are held to be "situationally determined"; they are dependent upon the concrete context in which they appear in the sense that "they unfold only within an unending sequence of practical actions" through the participants' interactional activities (Mehan and Wood, 1975, p. 23). In the following, I will use the term "indexicality" to refer to the *situational location* of scientific action. This situational location displays the products of scientific research as fabricated and negotiated by particular agents at a particular time and place; it displays these products as carried by the particular interests of these agents and by local rather than by universally valid interpretations; and it shows the scientific actors' play on the very limits of the situational location of their action. In short, the situational location of scientific action displays the products of science as hybrids which bear the mark of the very *indexical logic* which characterizes their production rather than as the outgrowth of some special scientific rationality to be contrasted with the rationality of social interaction[9]. We can also say that it displays scientific method as much more similar to social method, and the products of natural science as much more similar to the products of social science, than we usually assume.

How, then, can we illustrate this indexical logic in more detail? The first aspect of indexicality is the *opportunism* it implies. This opportunism manifests itself in a mode of operation comparable to that of a "tinkerer":

> — a tinkerer . . . does not know what he is going to produce but uses whatever he finds around him . . . to produce some kind of workable object . . . The tinkerer, in contrast (to the engineer) always manages with odds and ends. What he

> ultimately produces, is generally related to no special project, and it results from a series of contingent events, of all the opportunities he had — Often, without any well-defined long term project, the tinkerer gives his material unexpected functions to produce a new object . . . (These objects) represent, not a perfect product of engineering, but a patch work of odd sets pieced together when and where opportunities arose —[10]

The tinkerer, it appears, is an opportunist. He is aware of the material opportunities he encounters at a given place and he exploits them to achieve his projects. At the same time he recognizes what is feasible and adjusts and develops his projects accordingly. While doing this, he is constantly engaged in producing and reproducing some kind of workable object which successfully meets the purpose he has settled on temporarily. When we observe scientists at work in the laboratory, such opportunism appears to be the hallmark of their mode of production. To refer to the opportunism of research is not to suggest that scientists are unsystematic, irrational or career-oriented in their procedures. They may or they may not be, depending on a variety of circumstances. The opportunism I have in mind characterizes *a process* rather than individuals. It refers to the *indexicality* of a mode of production from the point of view of the *occasioned* character of the products of research, in contrast to the idea that the particularities of a given research situation are irrelevant or negligible.

As in the example of tinkering, the occasioned character of research manifests itself first of all in the role taken on by local resources and facilities. For example, in the institute which I observed the existence of a large scale laboratory in which proteins could be generated, modified and tested in large volumes was treasured as a valuable opportunity to do certain kinds of research which could hardly be done anywhere else because of a lack of such facilities. The laboratory was well equipped and well staffed by technicians, and it was supervised by an experienced older technician described as extremely reliable and "clever", a series of additional advantages. Much effort consequently went into gaining access to this laboratory in order to "exploit" this "resource", and special research which required this laboratory was eagerly picked up or invented. A newly purchased electrone microscope operating on the basis of laser-beams exerted a similar attraction. Needless to say, the two scientists who controlled these facilities spent an equal amount of effort to attempt to prevent others from using them, perfectly aware of the increase of value achieved through making an already scarce resource even more scarce. In science, as elsewhere, particular interests and opportunism mutually sustain each other. But it is not only the highly scarce and hence attractive resources and facilities which guide what is done in scientific research; I have seen a whole paper on functional properties of proteins being built almost exclusively on chemical determinations of various contents of the proteins which were supplied, among other tests, by a specially designed "service" laboratory of the institute. The scientist who wrote the paper made it clear that he would select completely different tests if he had to or to supervise the work himself rather than to have it done by the service laboratory. Given their offer of certain, rather than other, tests, these tests were quite naturally given preference whenever they could be used. Needless to say, preference is also given to technical instruments and apparatus of which scientists know that they are "somewhere around". Thus projects take certain turns because, as scientists say, * 'we did have a piece of equipment that had been developed in another project that we could use", certain measurements are taken because "the machines were here so it was very easy to go down and use them", or certain results are obtained because "well we were looking for a way to get the foam off you see and it (instrument) was something it was there . . .". Of course, the particular resources and facilities available at a certain place and time are not just picked up and used. They are also the object of constant negotiation and manipulation. Equipment earmarked for certain purposes is frequently converted such as to serve some other goal, or it is simply ''misused". For example, because a density measurement device was broken, one of the scientists who needed the measurements had the material to be measured centrifuged, and he calculated an approximation for density from the difference between volume measurements taken before and after centrifuging. Since the centrifuging provided for compression under fully controlled and standardized conditions, the idea, inconspicuous as it seemed, was in fact quite ingenious. Similarly, a pressure meter, which a scientist happened to see used in one of the laboratories, was subsequently borrowed and "misused" to determine the gas absorption capacity of a substance, and chemicals which happened to be lacking in the storage room were routinely substituted by others which happened to be around such as to not impede the process of ongoing events.

Less tangible than research products, but no less circumstantially determined is the emergence of ideas in the research process. Partly of course ideas are triggered by the very resources and facilities at a given place and time. Partly, they emerge from the dynamics of interaction between researchers, and partly, they are the contingent result of other occasions. Scientists constantly refer to this circumstantial nature of ideas when they say that they "happened to come across a paper" which triggered an idea, that some idea "occurred" to them when they looked at something or read about something, or that they ''ran into" an idea on account of some other occasion. I suppose that there will be no need to illustrate the situationally contingent emergence of ideas by further examples, particularly since historians of science have often demonstrated the point. Let us look instead at the relevance of the larger environment in setting the conditions out of which new research results are bred, and in supplying the criteria upon which the selections of the research process are based. These conditions and criteria often reflect relatively short term concerns of exclusively local relevance, or concerns which are locally emphasized, which are almost fashionable. For example, when I asked a chemical engineer whether the then current interest in saving water (due to the fact that Northern California was at the time in its third year of a severe draught) had played a role in his attempt to use foam instead of water for certain surface treatments of plants, he said

> Oh yes, water savings, and pollution, or reduction. You see first of all water saving and secondly the less time that you expose and the less volume of water you expose to the surfaces the less leaching. And we were hoping that by using a replacement for water which in this case was foam . . . that you would leach less out of the product. But I mean the first thing was water . . . In other words volume of foam to volume of liquid used to generate the foam is like 20 to 1, so that you could occupy a volume of that or cover surfaces with a 20ieth of the volume of water.

Another example refers to the local emphasis, at the time of the observation, to chemical compositions which include only few and carefully selected ingredients such that the adverse effects which result from the interaction of ingredients in complex compositions and which are often counteracted by still more complex compositions are reduced. When I asked a chemist whether I was correct to assume that he used this criterion he said:

> Absolutely. Well in the prevention of lycinol formation we started out adding systein and from that we thought well we could probably accomplish the same thing by sulfite, which is cheaper, and simpler, and then we thought well no, if we just keep the air away from it we could do the same thing. And that's where we ended up . . . it was reducing the amount of treatment really, and still reaching the same end. You know if you control the air incorporation I control most of the reaction.

At the time of the observations, the most obvious and conspicuous cases related to the form and amount of energy used. As is to be expected, energetic criteria were introduced into the *'cognitive" operations of the laboratory together with the rise of the energy crisis. The emphasis placed on the energy-implications of a research effort reflected more or less directly the respective stage of the crisis, which was relatively pronounced during my stay in the laboratory. An important step in protein recovery, for example, is the precipitation of the protein. In general this is done by using heat coagulation methods. One of the scientists working on proteins had come across a paper in which the use of ferric chloride was mentioned as an effective means of precipitation at low temperature when removing proteins from waste water. In the context of energy shortage, the use of ferric chloride instead of heat coagulation struck the scientist as an excellent alternative: because of the low protein yield of the source material, the energy consumption associated with heat coagulation for any substantial amounts of protein was enormous. Since the scientist needed such substantial amounts for bioassay tests with rats, and since he thought the method to be of * 'wide interest" if it could be made to work in contexts other than that of the original paper, he promptly started a series of experiments using ferric chloride. In the same test series, he also favoured filtration over centrifuging because of the energy savings it implied.

Let me conclude this paragraph by emphasizing that scientists are well aware of the situationally contingent nature of their products. As the emerging studies of laboratories consistently show, they refer to these contingencies as *explaining* a particular kind of result when the result becomes questioned, identifying it with the very indexical selectivity that actually constituted the result[11]. Yet there is another sense in which scientists *directly play* on contextual limitations, and they do this when they attempt to expand their own horizon of action and of opportunities as against those of others. Thus the tinkerer is not just a passive opportunist who responds to what strikes his eye as potentially interesting in a local situation. During a discussion about further plans of projects for example, a member of the protein group told me that he had come across a Russian paper "that hopefully nobody knows here". The paper implied that the results of the experiments the scientist was at the time engaged to do could be significantly improved by using a particular plant juice. What turned this suggestion into a profitable "idea" was precisely that "nobody here" seemed to know about this possibility. When asked directly about his intentions to quote the source of the idea the scientists said that he would "nevertheless cite the paper somewhere". Ideas need not be stolen (although they undoubtedly are sometimes) in a universe in which particular transgressions of contextual limitations not only serve as routine strategies of resource mobilizations, but in which these transgressions also count in themselves towards increasing the credit of the author. Other examples which refer to the use of literature are implied when scientists pride themselves that they " do not miss out on things published in other languages" as most of their colleagues do, correctly considering this as one of their "major strengths". Or when they consider it a, "tragedy" that they cannot get all the material they ask for, as a biochemist who told me that

> there is a certain, a high percentage, may be uh . . . 40% of what I ask for which I never get. . . The authors don't send you a reprint, the library can't get it, for one

> reason or another, I don't get it. It makes me mad, but I do have the reference, so that when the time comes when it becomes real critical to know about it . . . I pound doors and I get it eventually. But you know if I did this for everything that I can't get I would do nothing but (this).

Since he could not spend the time, the scientist knew that he missed out on much of the relevant material, and he had to because of a variety of barriers to the internationality of science in the (published!) literature itself, barriers which exist apart of and in addition to the barriers posed by language. At the same time he played on these limitations by transgressing them on particular occasions in order to add to the "originality" of his research group, or in order to add to the "excellence" of the book he wrote. Actualized contextures and their borders set the scenes in which the meanings of the laboratory emerge, and they impose the limits within which the scientists operate. But they also constitute a resource in the very same scientists' mode of operation.

6. Local Idiosyncracies

There are many other examples which illustrate how spatial and temporal contingencies become relevant for the decisions and selections which mark the results of natural science research. Some are as routinized as to be hardly noticeable, as when local employment regulations prohibit testing after 4.30 p.m. or on weekends, so that freezing and storing procedures not specifically mentioned in the resulting papers must be used to compensate for these unmethodical interruptions. Perhaps more interesting for the sociologist who wants to compare social and natural science procedures are the *local idiosyncracies* observed, a phenomenon almost completely ignored in the literature on science. Like any other organization, research laboratories develop *local interpretations* of methodical rules, a *local know-how* referring to what is meant and to how to best make things work in actual research practice. For example, the research institute observed had several "service" laboratories designed to perform standard analyses of chemical composition which were needed in many different research efforts. Many of these analyses were not only standard methods, but also "official" in the sense that they had been tested, documented and recommended for standard usage by the American Chemical Association or some other association of this sort. When one of the scientists of the group who had come to the institute from another place used these facilities for the first time, he was surprised to find that the tests were performed without replication, apparently under the assumption that such measurements were standard routines which carried no risks or uncertainties.

The scientist brought with him exactly the opposite interpretation. He frequently complained to me about such practices, explaining that measurements become routine precisely because they are important, which meant that precision was their foremost requirement, and precision without replication was invalid. He once illustrated his point by saying that single chemical ingredients in a substance are reported as percentages of the dry substance of that product. If as simple a measurement value as the water content of a substance from which the dry substance is calculated is only slightly imprecise, the error will appear in all other measurements taken. Consequently, he said, *'when I read *one* figure in the literature, I would automatically assume that I have been confronted with a mean value (based upon several replications)".

In this case, each side stuck to its interpretation. The scientist repeatedly asked the analytical laboratory for the same analysis twice, using different codings for the samples such as not to raise their suspicion. The clash of two locally developed systems of interpretation only became apparent when the expectation of a scientist who had moved from one system to the other were constantly violated.

Local idiosyncracies also bear upon questions of composition and quantification, that is questions concerning how much of a substance and what kind of substances are used in an experiment. Standard formulations of what and how much to use exist in certain areas, but apart from the fact that their being a standard procedure does not make them immune to local idiosyncracies as we have seen above, scientists often reject these methods for other than routine composition analyses. They were said to "lag too far behind" current knowledge and to be "too old", since it takes much time for a method to become an officially acknowledged procedure. There was also a more basic reservation. In the words of a biochemist:

> — The more basic work is usually done on . . . on something similar but not the same. You know, if it's done on what I am interested in than it's not worth doing again. So usually it's been done on something similar . . . And see, I think you almost always have to adapt (a method) in some way. You know sure, occasionally you find something (a method) that just fits in perfectly to solve a problem — I'd say that's the exception rather than the rule.

Interest in the difference rather than in the identity of procedure promotes local idiosyncracies, but so does the material itself which is used in experimentation. This material constitutes an additional source of constant variation because it is itself locally grown (plants and organisms), bred (animals), or produced (substances prepared or isolated in the laboratory). For example, the plant protein on which the scientists spent much of their effort came from a local variety of plants, and so did much of the raw-material from which scientists started in other groups. As seen by the head of a chemical engineering group:

> The big variability is getting the raw material. We have never been able to get the same raw material ever again, and this is the . . . (inaudible) . . . every researcher has to face. It's the same in microbiology. You have to scratch yourself the same place every time you play and everything has to be the same or else the accounts are meaningless.

Variability in the source material of the biological sciences have often been recognized as some sort of a "nuisance" by researchers and students of science alike. But apart from being a "nuisance" they also enhance the differentiation and the distinctiveness of research products as sought by scientists themselves[12]. And as mentioned before, they attribute to the idiosyncracies of research, but they are by no means their only constituents as is sometimes suggested in arguments which refer to the variability of results. I have already mentioned the scientists' often treasured know-how, particularly obvious in questions of composition and quantification, as another part of these idiosyncracies. For example, when the proteins mentioned earlier were subjected to high temperature and to fermentation, differently processed versions of these proteins were mixed with several other substances in order to compare reactions. The number and quantity of these substances reflected the respective scientists' attempts to achieve control over the process, their knowledge on what quantities had been used in the past with what outcomes in previous research, and their bets on what might be successful in the case they confronted. The procedures used in these and in other experiments were also largely influenced by routinized local interpretations. For example, the time needed to manipulate the mixtures above before they were put into the fermentation cabinet was counted as belonging to the "fermentation-time" in the respective laboratory, while in other places it figures separately. In the same test series, the volume and weight of the samples were measured immediately after they had been exposed to high temperature. According to the scientist who came from another institute, this was "problematic", since the volume changes during the cooling period. Thus, the results depend on *when* the measurement is taken. In general, the time during which the test material was exposed to a treatment was based on local knowledge about what works best, as in the case of composition. The treatment of substances before experimental use also illustrates local differences. In the example above, the organisms used for fermentation were stored and employed for several weeks, whereas in other laboratories they are exchanged after one week maximum. Note that variations such as these do *not* indicate that the storage time of a micro-organism is irrelevant for the results obtained, according to the scientists when I raised the argument. Rather, these variations indicate differences in local interpretations as to what is relevant, and why.

The above argument can be extended to include measurement devices and instrumentation as further sources of potential local variation. Let me stress instead that at least part of the information which characterizes this variation is provided in the published papers by reference to brand names, by identifying the firms which supplied a measurement instrument, by giving the details of a procedure, and so on. The argument here is *not* that science is *private* or non-public, but that the information obtained in natural and technological science research as observed is idiosyncratic. Phrased differently, the argument is that the selections of the research process reflect interpretations which are crystallizations of order in a local contingency space. Contrary to what we are accustomed to think, criteria of "what matters" and of "what does not matter" are neither fully defined nor standardized throughout the scientific community, nor are the rules of official science exempted from local interpretations. In sum we can perhaps say that these interpretations refer to at least three areas of selections. These areas are:

(1) Questions of composition, or questions which relate to the selection of specific substances, ingredients or to specific means of instrumentation.

(2) Questions of *quantification,* or questions which bear on the selection of how much of a substance is to be used, of how long a process should be maintained, of when a measurement or a sample should be taken, and so on.

(3) Questions of *control,* or questions which refer to such methodological options as simplicity of composition versus complexity, strict versus indirect comparability, and so on.

Given these choices, research in the natural and technological sciences cannot be partitioned into a part which is open to situationally contingent selections and to contextual influences such as the part in which a research problem is defined, and into a part which consists of the internal, objective and standardized execution of the enquiry necessary. Since the choices exist throughout the process of experimentation, there is no core of the production of research which is in principle left unaffected by the circumstances of production. Another way of saying this is that as much as in the social sciences, natural and technological science research is in principle underdetermined both by the scriptures (the authoritative writings) of a field as well as by its tacit knowledge, if both are thought to represent generally available information. Closure of this situation is achieved locally, with the help of idiosyncratic interpretation, which itself results from this underdetermination.

7. Occasioned Selections and the Oscillation of Decision Criteria

If idiosyncratic interpretations and an opportunistic logic mark the selections of the research process, what then is the role decision criteria play in these selections? Presumably, decision criteria are of more than local relevance, and, presumably, they overrule at least part of the contingencies of a local situation by suggesting which decisions should be made in face of the indeterminacy of the choices scientists confront. Let us first consider the question of how we are to conceive of a decision criterion. As suggested before, the making of a piece of knowledge involves series of decisions and of negotiations, or phrased differently, it requires consistently selections to be made. Selections, in turn, can only be made on the basis of other selections. In other words, selections require translations into further selections. For example, when the scientists were about to choose between a filter and a centrifuge as a means to eliminate chemical precipitation agents from protein samples they translated the problem into a problem of energy consumption of the two methods and subsequently turned to the less consuming instrument. We can also say that they recurred to the criterion of energy consumption. Of course this criterion is nothing but a further selection, since numerous other problem translations can easily be imagined. In fact, when it turned out that the less energy consuming filter method did not work, the scientists used the centrifuge, and invoked the criterion of the practical availability and adequacy of this instrument as a ground for their decision. Not surprisingly hence, decision criteria are frequently scrutinized as a specific selection out of possible other selections by the scientists themselves, for example, when an earlier decision becomes questioned in the course of the research or when a research result is evaluated in the light of the decisions which account for its specific characteristics.

Thus decision criteria are seen here as translations of selections into further selections, and there can be no doubt that some of these translations appear more frequently than others. For example, in the group observed and in discussions with other scientists of the institute, references to costs, to simplicity, to feasibility under local circumstances and particularly, to whether something would "work", came up frequently. Yet the invocation of such general criteria by no means precludes the impact of a locally contingent situation. To begin with, decision criteria are invoked in specific circumstances, with reference to a specific *aspect* of the research whose costs are considered, and with respect to a specific *equivalent* such as money, time, effort, and so on. These aspects and equivalents provide the indexical meaning of the criterion. We can also say that general criteria like that of costs are nothing but a schematization of specific translations. These specific translations vary not only with the problem at stake in that the aspect which is costly and the equivalent of "cost" depend on this problem. They also vary with local interpretations in the sense that, given a specific local context, certain specific translation will be preferred locally. In the institute studied, the money necessary to buy an expensive technical instrument could be more easily obtained than an equivalent amount of money to pay for assistance by a student or technician. Consequently, scientists frequently preferred instrumental procedures to those which involved manpower assistance, and the institute was generally overstocked with apparatus as judged from the number of technical instruments present which were more or less unused. Other examples were brought up already above, when I mentioned the locally developed know-how of "what works" in certain problem-situations. Note that the selection of a substance, a technique or a formulation of composition "because it works" points up the relevance of *success* rather than of truth in actual laboratory work. Successes, have nothing of the absolute quality of truth. Not only is success, as the scientists said, "a different trip for everyone of us", but what works and what consequently counts towards success depends on routine translations which arise out of the practical concerns at a research site, as it depends on the dynamics of negotiation and of renewal and modification of these translations.

If criteria are seen as schematizations of specific translations of choices which originate in local laboratory situations, we cannot automatically assume that the same criteria are consistently called upon in a variety of situations. It comes as no surprise then that the scientists' reasoning, which indicates the making of decisions, is marked by criteria-variation. More specifically and more interestingly, it is often marked by the *oscillation* between criteria which can be seen as directly contrasting with each other. Let me take the example of a piece of research which the scientists qualified as thoroughly "applied". It was part of the protein research in the institute observed to test the suitability of plant proteins for human consumption. In a major test series, this was done by exploring the behaviour and effect of the proteins when used as food additives. The tests were done in a special laboratory designed to experiment with the baking qualities of various foods. The laboratory was part of the effort of the institute to emphasize the practical relevance of its research. In the present case, one of the questions posed was how differently treated proteins of various origins would influence the texture of test-breads to which they were added.

Given that the experiments did not involve artificially composed mixtures of chemical ingredients of interest only to scientists, but actual, albeit sample size "breads", one would expect the non-protein ingredients of the samples to somehow simulate the composition of some standard bread. Thus the criterion for the choice and quantity of ingredients would be their presence and their quantity in standard breads as found in bakeries and supermarkets. However, the scientist who supervised the tests considered the choice of ingredients a matter of experimental control rather than of practical application, choosing a minimum number of "absolutely essential' * components. He consequently ended up testing protein as a food additive in * 'breads" of a kind which could not be found in practice, and except for a famine would not be considered as "food". The principle upon which he implicated six months of a line of research and more than one paper was that of basic science. He explicitly referred to his interest in these terms, as an attempt to find out what happened in the samples under maximally controlled conditions. This principle contrasted sharply with the otherwise extremely "applied" make-up of the project, and with the criterion of practical relevance chosen to account for the idea of testing the usefulness of the proteins as food additives in the first place.

Criteria switches such as the above are not new. But the point here is that they are neither the exception nor the mark of misdirected, "subversive" research in which the personal interests of a scientist take over what should be done. Rather, this oscillation between criteria depending on occasioned preferences, advantages and opportunities appeared to be an everyday feature of scientific practice. In general, it remains less visible than in the above case where a whole research effort was implicated upon a criteria switch, since many choices of the laboratory are implicitly, rather than explicitly, reasoned and discussed choices. If selections are not brought into the focus of attention, it is only from side remarks and occasional comments that an implicit change of a criterion emerges. I have referred before to the example in which a heat coagulation method was replaced by the use of ferric chloride as a means of protein precipitation which works at low temperature. The rationale scientists gave for the appeal of ferric chloride was energy savings and hence a substantial reduction of costs when larger quantities of protein had to be generated. After several months of (successful) testing the scientist who supervised the work mentioned that he had ''no idea what the ferric chloride costs" and that this did "not interest" him. Costs in this project were defined in terms of energy costs and much ignored for the rest of the material and procedures whose importance was that they allowed the initial idea to work and to gain strength. I am not denying here that some conspicuously high costs of ferric chloride would soon have discredited or endangered the whole "idea" in the eyes of the scientists. But short of such threats which impose themselves, selections were *not* translated into problems of costs, but rather into questions of making things work, and others.

8. Conclusion

I suggest that we consider the oscillations of "criteria" and the local idiosyncracies of research, the opportunism of the research process and the scientists' play with contextual limitations as different aspects of a *situational logic* of research. Scientific facts are the hybrids constituted by the situational selections of this logic, hybrids in which the composing contingencies can no longer be differentiated. Their originality and their distinctive value, in the sense of information theory, of low expectancy derive from the idiosyncracies of their situated construction. It is the argument of this paper that the situational logic of natural and technological science research appears similar to the situational dynamics inherent in social method, and that this similarity is strengthened by the apparent universality of interpretation in both social and natural science method. Given this similarity, it is time to reconsider customary routine distinctions between the social and the natural sciences which ascribe to the former what they deny to the latter. And given this similarity, it may be time to reconsider scientific method in general as just another version of, and part of, social life.

Notes

1 Major examples in this century are the disputes related to Max Weber's conception of social science methodology and the controversies about behaviourism and functionalism.

2 For example, Giddens (1976) reviews some of these philosophical results, but only to return to the original distinction between the natural and the social sciences which he reemphasizes.

3 The term "hermeneutics" will be used subsequently in this general sense and not in the more specific sense of a particular methodological approach which contrasts with other approaches such as phenomenology, or in the sense of a specific technique of text analysis which contrasts with other techniques such as semiotic procedures.

4 As cited in Habermas, 1971, p. 141; see also volume 5 of Dilthey's Collected Papers (1958-77).

5 Habermas (1971, pp. 192-93) characterizes scientific inquiry by a "restricted language" and a "restricted experience", by which he means that scientific action is no longer embedded in interaction, that action is severed from communication and that theory and experience are divorced. Garfinkel (1967, p. 272) ascribes to science the kind of systematic, ''scientific" rationality which he otherwise dismisses as non-existent in everyday reasoning.

6 The examples which I will give below come from a one year study.based upon participant observation and interviewing in a large research institute located in Berkeley, California. The observations focused on plant protein research, and the interview material was obtained from scientists working in different areas and groups in the same institute. For further details see Knorr (1977; 1980).

7 The idea of a double hermeneutic and the point about first and second level constructs dates back to Schutz; see Giddens, 1976.

8 The last thesis is found more often in discussions of problems of application in the social sciences than in epistemological debates (see, for example, Lazarsfeld and P^eisz, 1975). The thesis seems to rest upon a mistaken conception of what goes on in the natural world since the whole problem of technology *is* a problem of developing knowledge which is *adequate* to a particular field of action.

9 I am referring here to Garfinkel's contrast between everyday rationality and scientific rationality, a contrast all the more surprising since it comes from ethnomethodology rather than from the philosophy of science (see Garfinkel, 1967).

10 This description of tinkering is taken from Jacob (1977), who uses the image of the tinkerer to illustrate biological evolution as a non-optimal, redundant, playful chance process rather than as a systematic planful process in which everything has a purpose and nothing is wasted.

11 The phenomenon is referred to under different titles. I have mainly used the notions of indexicality, of opportunism and of situational contingency (see Knorr, 1977, 1980). Others have also referred to the importance of milieu, to local disorder or to the circumstantial nature of scientific research (see Latour and Woolgar, 1979).

12 This is developed in more detail in Knorr (1980, Ch. 5).

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