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PHILOSOPHY OF SCIENCE IN NORWAY

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Summary

Norwegian philosophy of science right after the war was empiricistic, scientific, rather undogmatic and heavily dominated by Arne Næss. The positivistic conception of science has been severely criticized in the last two decades, and the attempts to find viable alternatives have led to a broadening of the perspective, philosophically as well as scientifically. This survey tries to map the main lines of that development. After an account of the rise and fall of Næss's programme for a behaviouristic theory of science, we outline first the development of the internal philosophical critique and then the parallel story on the social science scene.

1. Introduction

1.1. When Norway emerged as an independent nation in 1814, it started out with giving itself a modern and for that time fairly democratic constitution. At the same time the first Norwegian university was founded in Christiania (now Oslo). From the beginning it was thought to be important that students should get a general introduction to philosophy, thus a preliminary examen in philosophy was instituted (the examen philosophicum).

Although the examen philosophicum has existed all the time since that beginning and philosophers of some standing have taught (we can mention here Niels Treschow and Marcus Jacob Monrad), philosophy failed to have any important influence on intellectual life in Norway. It is typical that Professor Monrad, a somewhat self-styled Hegelian, who taught philosophy for forty years, has been remembered for his conservative polemics and his participation in student life until old age, rather than for his philosophy.

It was rather a practical, non-theoretical and antispeculative attitude that became the dominant one. As a young man A. M. Schweigaard wrote an attack upon German idealism, a pamphlet that some historians see as a parallel to Heine's polemics. But unlike Heine, Schweigaard did not see the revolutionary potential in German idealism; he looked upon it rather as an obstacle for solving the important and pressing practical problems. Philosophy became more or less an eccentricity.

For this reason our account begins with the year 1936, when Arne Næss published his doctoral thesis, *Erkenntnis und wissenschaftliches Verhalten*. Here the modern development starts. What belongs to the earlier period is mainly of historical interest.

1.2. Recent debates in philosophy of science in Norway have centered around problems in the philosophy of social science. Although Norway has had the eminent scholar in symbolic logic, Th. Skolem, he failed to create any influential school. There is now a group of logicians of international standing in Oslo (Jan Erik Fenstad, Dagfinn Føllesdal, Ronald Bye Jensen, Dag Prawitz), but so far they have been somewhat unrelated to the main debate. This may be due to a general distrust of formal logic, partly deriving from Arne Næss on the one hand, and from recent English philosophy on the other hand, with influences from phenomenology and existentialism adding up to the same effect.

The discussion in the philosophy of social science has not only taken place among philosophers, but also among social scientists themselves. Social science in its modern sense is of recent origin in Norway. It was in the early post war years that the foundations were laid for these subjects, that in the meantime have become so important. Only economics, education and some psychology existed before the war. In the post-war years chairs for sociology, social psychology, social anthropology and political science were created.

In 1950, The Institute of Social Research was founded in Oslo. This is an independent institution, and it is important in our connection because the majority of the scholars we are going to mention has at one time or another been affiliated with that institute.

1.3. There is another important fact we have to mention. In 1945 Norway had one university in Oslo. Now there are four universities, each of them with a department of philosophy. Until 1959 there was only one chair in philosophy; now there are 11 chairs, and 20 other permanent posts (lectureships). The number is still growing.

1.4. If there is some special characteristic of philosophy of science at present in Norway, it is the continuity between discussions in philosophy of science and discussions in the practical political sphere. This is especially the case with regard to university politics and educational politics generally. But also the now influential movement called populism draws upon viewpoints from recent criticism of positivism, more precisely the criticism of technocracy as it has recently been developed. It is in this context that the influence of The Frankfurt School, especially Habermas, has become one that is found not only in academic circles.

This also accounts for the fact that problems in philosophy of science have become of public interest, they have been taken out of the academic ghetto. This again has led to a remarkable development in university life. Philosophy has changed from being an eccentricity to becoming a favorite subject with many of the young generation.

We shall here try to trace the main lines of that development, and it would untrue to the whole situation if we only concentrated on philosophy of science in the narrow sense, not also taking account of discussions in social science and politics.

2. Næss: From science to philosophy

2.1. Arne Næss (b. 1912) has played a unique role in Norwegian philosophy and social science. He was appointed to the chair of philosophy at the University of Oslo in 1939, and for two decades remained the only professor of philosophy in the country. A whole generation has been influenced by his textbooks in logic and philosophy. He played a decisive role in the development of the social sciences after World War II. His influence on the research milieu has been more on the personal level than through published work. Published work tended to be less important in the 40's and 50's when there was only one university in the country.

2.2. Næss's long list of publications includes three important programmatic studies in the field of philosophy of science: the dissertation *Erkenntnis and wissenschaftliches Verhalten* from 1936, a paper entitled "Science as Behavior" from 1965 which modifies the original programme considerably, and lastly *The Pluralist and Possibilist Aspect of the Scientific Enterprise* from 1972, which outlines a radically different picture of the relations between philosophy and science and implies a complete abandonment of his original programme for a behaviouristic science of science.

The dissertation on knowledge and scientific behaviour was, in Næss's own words, "fiercely scientific" (Næss 1972, p. 134). Under the influence of psychologists like Watson, Tolman and von Uexküll, Næss proposed that traditional theory of knowledge, science and meaning should be replaced by a purely behaviouristic science of science.

This positivistic programme for replacing philosophy by science was based on two assumptions, which Næss seems to have taken more or less for granted: (i) the assumption that overt behaviour but not meaning can be directly observed; (ii) the assumption that all meaningful discourse can be fitted into the categories logical – factual (analytic – synthetic, formal – empirically scientific). The first assumption lies behind Næss' long fight against "revelation in semantics", to quote the title of a paper of one of his students (Tennesen 1950). It is implied by the observability assumption that overt behaviour is not meaningful. At this time, Næss accordingly construed meanings as some kind of mental entities accessible only through introspection. Since he shared Watson's impatience with introspective methods, he came to see behaviouristic psychology as the way out of the misery of traditional philosophy and psychology. This proposal was reinforced by the second assumption, which calls for the reconstruction of traditional philosophy as either logic or science. Now Næss has never shared the logical empiricists' and critical rationalists' enthusiasm for a "logic of science". In 1936, he took an extremely pragmatical view of the function of logic: the law of the excluded middle is *just* a rule for "ending everyday disputes as quickly as possible" (Næss 1936, p. 217). Accordingly, he set out on a search for the empirical core of traditional philosophical propositions.

Næss did not claim that it is practically possible or desirable to replace all discourse by purely behaviouristic equivalents, but he did claim that all meaningful discourse *can* be so translated. It would be theoretically possible for an intelligent observer from another planet to describe and explain what physicists do, for instance, merely by observing their movements. Næss went as far as to claim that the foreign observer's account of the behaviour of physicists would provide just a good basis for predictions of the results of future physical measurements as physical theory. These claims for the possibilities of behaviourism were dogmatically asserted rather than argued for.

Næss did not stop to consider e.g. the question how it would be possible for the foreign observer to identify the class of physicists, or what the preconditions are for the ability to distinguish between physicists' activities qua physicists and their non-professional doings.

The claim that it is possible to reconstruct all essential aspects of science from a purely observational point of view was coupled with a doctrine of metascientific neutrality: it was the business of the science of science to describe and causally explain the activities of scientists, but critical evaluation of their activities and results was beyond the metascientist's field of competence. By reducing "true" to "verified" and "valid" to "held to be valid", Næss' original programme in fact gave no room for considerations of truth and validity in the theory of science. Unlike the experimental psychologist who studies the behaviour of rats in a maze, the metascientist has no independent access to the "maze" that scientists are concerned with, viz. the world. To assume otherwise, would be to commit the fallacy of "maze epistemology". To assume, as "subjectivists" do, that scientific propositions and models can correspond to reality, has no point from an "objective-psychological" point of view: "Solange man objektive Psychologie treibt, begnügt man sich damit, die schon konstruierten Modelle in ihrem kausalen Zusammenhange innerhalb des Gesamtverhaltens zu beschreiben" (Næss 1936, p. 64).

This diatribe against philosophy ends somewhat surprisingly on a sceptical note: all "thought models" are "relative", including the relativity model itself (Næss 1936, p. 248). Næss has always had a disarmingly undogmatic way of propounding his dogmas.

2.3. In (1972), Næss talks of his "transition from scientism to philosophy, from theory of (scientific) models to theory of philosophical systems, using Spinoza's as a paradigm", and attributes the development to the influence of Carnap, Popper, and others (Næss 1972, p. 137). In (1965), Naess was, however, still on the scientific side. He criticized Popper's identification of philosophy of science with *logic* of science, but did not consider any alternative conception of *philosophy* of science. Instead, he discussed the prospects and limitations of the programme for a behaviouristic *science* of science. While drawing attention to a number of difficulties in behaviourism, he reaffirmed his belief in that approach. But the contents of the paper point in other directions.

The two most important modifications of the old behaviouristic programme are the following. First, Naess concedes that the idea of a purely observational metascience is theoretically impossible (without it becoming quite clear why that is so). Participation is now said to be "a necessary characteristic of the metascientist's relation to the scientist and his enterprise" (Næss 1965, p. 64), and the doctrine of observability is reduced to a programme for the gradual elimination of "participatory assumptions" (p. 59). Secondly, Næss resolves the ambiguity of the term "behaviour" in (1936) by making an explicit distinction between movements and doings. Behavioural metascience must be based on a study of the doings of scientists rather than on their movements. Since doings in contrast to movements are socially meaningful, this means that Næss has now abandoned the attempt to dispense with all reference to meanings and intentions.

The descriptivist assumption survives in (1965) ; the programme was to study scientific enterprises without engaging in them (p. 51). There is still no room for a critical study of science. Næss

reiterates his critique of "maze epistemology": "The metascientist must ask the scientist for information about the maze" (p. 63). Here Næss seems to have fallen prey to his own methodology. That the metascientist must ask the scientist about the maze follows from his own methodological delimitation of the metascientist's role. The metascientist cannot check the scientist's propositions since his role has been defined as that of an observer.

2.4. Næss's (1965) paper is fundamentally ambiguous. He hints at reasons why behaviourism is untenable, he suggests that structural and phenomenological accounts seem hopeful, but ends up with a reaffirmation of the belief in the idea of a behaviouristic science of science. The ambiguity is eliminated in Næss's latest work, *The pluralist and possibilist aspect of the scientific enterprise* (1972), where the last vestiges of a descriptive and behaviouristic theory of science have been wiped out.

Næss now rejects "the neat image of science" according to which science is a rational, autonomous, and cumulative enterprise which is gradually approximating to Truth.¹ He quotes approvingly from Feyerabend's "anarchistic" philosophy of science ("All that is needed is less moralism, *less seriousness*, less concern for the truth, a vastly deflated 'professional conscience', a more playful attitude..."), and makes a plea for "the new, gaudy image of science" with reference to authors like Agassi, Feyerabend, Koyré, Kuhn, Lakatos, and Popper. The emphatic scientism in Ness (1936) has now given way to an equally emphatic emphasis on philosophy: "Sciences, when articulated with care, are like fragments ; they constitute the most exactly formulated and interpersonally testable *parts of existing, or more often potential general views* about man and the universe" (1972, p. 11).

It is a central thesis in Næss (1972) that total or "near-total" philosophical systems do not exclude each other in the sense that only one of them can be true; on the contrary, two mutually inconsistent systems may both correspond to reality (p. 132). And so Næss is led to a defense of pluralism in science and philosophy. His pluralism is directed against all forms of dogmatism and conformism, both within the scientific community and in society at large. A plurality of competing research programmes is "a powerful liberating force" (p. 128), and generally, change of perspectives "saves us from many dogmas" (p. 79). Instead of his earlier pleas for science, Næss now issues warnings against uncritical appeals to science which may have an impersonalizing and dehumanizing influence (p. 7).

Næss's pluralism ("Let the radically different approach live!" (p. 107)) is coupled with "possibilism", to use his own terminology. "Anything is possible!" (p. 76). At one place in the book, this slogan is said to be directed primarily at the consumers of science, the so-called educated public (p. 76). At another place, the slogan is said to be intended to operate in research situations only (p. 94). But in both cases, the underlying motive is to counteract one-sidedness and narrowmindedness.

The shift from behavioural science to philosophy is accompanied by a shift from metascientific neutrality to critical theory of science. In his youth, Næss wanted to transform all questions of validity and truth into questions about the views that scientists and others happen to have on validity and truth. The Complete Stranger from the foreign planet envisaged in (1936) was to give a description of what scientists do from the neutral observer's point of view. In (1965), the alien

observer was transformed into an Ex-Colleague, a former participant who has turned to observation. In (1972), the metascientist has finally abandoned the observer's point of view and become a Colleague. The philosopher-historian of science should go deep enough into to the scientific debates of the past to be able to take a responsible stand within the debates (p. 13). He is no longer required only to note that certain propositions are considered to be refuted; he is also required to try to decide if they are in fact refuted. The philosopher of science is no longer cast as an intelligent observer from far-away. He is now going to play the part of a sympathetic though somewhat belated participant in the scientific enterprises of the past. And from here there should be no long step to an active and critical participation in the ongoing enterprises of the present.

2.5. Just after the war a group formed around Arne Næss, sometimes called "the Oslo School". To this group belonged Harald Ofstad, Arild Haaland, Herman Tennesen, Stein Rokkan, Ingemund Gullvåg and Peter Wessel Zappfe. The common platform was a broad interest in empirical research generally and empirical semantics specially.

Of these people Stein Rokkan has later turned to political science, Arild Haaland to a wide variety of philosophical and political problems. Harald Ofstad (in Stockholm since 1956) and Herman Tennesen (now Alberta, Canada) have for external reasons lost contact with the Norwegian debate in the sixties.

Peter Wessel Zappfe has in a much discussed work laid down the principles of a science of literature on empiricist grounds (Zappfe 1961).

But the man who is closest to Arne Næss also in his latest phase of development is Ingemund Gullvåg. He shares with Næss the broadness of scope, and has discussed in detail the problems of intentionality, total views and scepticism. Due to a certain inconclusiveness his work has not yet aroused the attention it might deserve.

3. Antipositivism

3.1. The development of Næss's thinking about science is also a reflection of the changes that have taken place in the intellectual climate since the end of the 50ies, which may be summed up under the vague heading "antipositivism". The theory and practice of science rightly or wrongly associated with logical empiricism, American social science, and the early Næss was for some time a favourite target of criticism, sometimes rather negative but more often combined with efforts to find viable alternatives to the received conceptions of science.

3.2. The first articulate attempt to criticize the philosophical assumptions behind positivist science was made by Hans Skjervheim in the economically written thesis entitled *Objectivism and the Study of Man* (1959).² (An earlier paper on "Participants and Observers" has also had considerable influence (Skjervheim 1957).) Skjervheim's aim was to demonstrate the untenability of one of the central doctrines of logical positivism, the thesis that all sciences are basically of the same nature (the unity of science doctrine). Reaching back to older continental traditions, he formulated this as an attempt to "rehabilitate the Diltheyan view that the transcendental problems of social and

historical science are essentially different from the parallel problems with respect to natural science" (Skjervheim 1959, p. 3).

"Objectivism" in this context stands for a naturalistic approach to the human world, which treats everything as objects in the world. "Objectivism, which must not be confused with objectivity, means just this: to treat everything as an object in the world, or as relations between such objects, exclusively" (p. 9). Skjervheim's own "subjectivistic" alternative could be summed up in the form of three theses : an ontological thesis to the effect that the ontological categories presupposed by the human sciences cannot be wholly reduced to those presupposed by natural science; a linguistic thesis to the effect that the language (terms, concepts) of the human sciences cannot be wholly reduced to that of natural science; and a methodological thesis to the effect that there are methods which are characteristic of the human sciences and which cannot be reduced to the methods used in natural science.

To demonstrate the untenability of objectivism on the ontological and linguistic plane, Skjervheim pointed to the difficulties of extensionalism. As Quine and others have shown, it seems impossible to eliminate all intensional expressions from our language. Indirect speech is a good example. The sentence "John says that snow is white" is not equivalent with e.g. "John says: 'Snow is white'" (perhaps John says "Schnee ist weiß"). And the inference from "John says: 'Snow is white'" to "John says that snow is white" presupposes the truth of the following statement : "By 'Snow is white' John means that snow is white", which makes essential use of an intensional idiom ("means"). Carnap's attempts to solve such problems on behaviouristic lines are not successful, and Skjervheim contends that all attempts to step outside the intensional circle are bound to fail.

The anti-extensionalist thesis is further backed up with reference to the hermeneutic circle (in order to understand what somebody means by a particular expression, we must understand what language he uses, but "how can we get to know that otherwise than by trying to understand what he says?"; p. 25), and with reference to the importance of the subject's own definition of his situation. (If John makes love to Eva, who happens to be his sister, the nature of the situation depends crucially on whether John is aware of the fact that she is his sister (p. 27)). Human behaviour poses essentially the same problems of interpretation as human language (p. 28).

The conclusion is that the ontological and linguistic categories of meaning and intention etc. are presupposed by the human sciences in a way which differentiates them from the natural sciences. From this Skjervheim drew the methodological inference that the human sciences must make use of methods of understanding and interpretation in a way which sets them apart from the natural sciences. Tacking on to Max Weber's concept of observational understanding (*aktuelles Verstehen*), he went on to propose the view that meanings must belong to "the given" (p. 33). Meanings (rather than sense-data, for instance) are the epistemological basis for empirical knowledge (p. 44).

Skjervheim did not attempt to draw any detailed methodological or practical conclusions from his inquiries in *Objectivism and the Study of Man*. At that stage of the debate, philosophy was still politically innocent.

3.3. The next attempt to criticize positivism was made in 1961 by Dag Østerberg in the thesis *Den sosiale realitet* (The Social Reality). In his general approach he is influenced by Skjervheim, and he

discusses also partly the same problems : the problem of intentionality, participant and observer, the logic of action concepts etc.

But what is new is that he shows in a decisive way that the whole problem is one of the relation between theory and practice, rather than internal problems in pure theory.

According to Østerberg, the main mistake of an objectivistic social science leads to an interpretation of action in the social field as analogous to technical action. The application of social science is then conceived as "social engineering". But such an attitude leads in practical situations to insincerity. Although this seems to point to a moral mistake, what Østerberg is hinting at is rather an intellectual mistake. This is shown by the following steps of reasoning:

- "1) Objectivistic sociology tries to create a causal theory of social relations, and this theory should be applicable to social relations. In order to create such a theory one seeks to reduce social phenomena to – i. e. to define them by means of – observable, "molar" physical facts.
- 2) The application of the theory, "social engineering", turns out to be a special kind of social interaction, namely systematic calculation and insincerity.
- 3) Make-believe and insincerity presuppose just the distinction between meaning and facts, between the intention, the act itself, and its outer manifestation, the action-product.
- 4) The social engineer is in principle in danger of being "disguised" by the people he manipulates, in a way that a natural scientist never is disguised. The social engineer can try to get into the calculation that his social objects know that he calculates them. And that he knows that they know that he tries to calculate them, etc., etc., but this is a regress that never stops. The engineer has landed in the tempo-riding for the meaning of the social, where the meaning in principle outruns the facts.
- 5) The analysis of "social engineering" shows once again that "meaning is an irreducible category."

(Østerberg 1961, p. 89. Translated by H. S.).

Soon after the completion of this work, Østerberg came to think that he had overstressed the "idealist aspect" of human action. Under the influence of Jean-Paul Sartre he has revised his platform in later essays. This meant a turn in the direction of Marxism, and for some time Østerberg had an important influence on "the new left", at that time just beginning to form itself in the country.

3.4. Audun Ofsti, a mathematician by education, approaches the problem from a different angle. He raises the problems involved in identifying physical and social phenomena. Physical phenomena are the result of a kind of analytical abstraction. Two numerically different phenomena are identified as identical if they are instances of the same *prototype*. Physical phenomena are of interest to a physicist only as prototypes, not as individuals. A physical phenomenon is identified in an exact way by defining its extension in space and time, and within this frame the quantitative degrees of certain qualities can be measured, which makes the description of the phenomenon more precise and definite. The close relation between mathematics and physics is due to the prototyping function of mathematics, at the same time mathematics also makes a precise description possible.

Social phenomena on the other hand are not results of analytical abstraction. They are given in contextual wholes, and have to be identified in terms of their meaning. This requires a hermeneutic approach which is fundamentally different from analytical abstraction. A physical phenomenon can be most precisely described if it is to a high degree isolated, a social phenomenon is described in a

more definite way the more of the context is taken in. Two numerically different physical phenomena can be repeated as identical; with social phenomena this is different, because "to repeat" has a social meaning. Painting a certain picture for the first time is different from repeating it ; when it is repeated it is a plagiat, not the original performance.

But Øfsti goes one step further. Not only is the way of identifying physical and social phenomena different, but the problems of the social world are in a certain sense the primary ones. Analytical abstraction is an activity on the background of the *Lebenswelt*, and he seeks to give a foundation to the whole in a transcendental theory of the *Lebenswelt*, here drawing upon the late work of Edmund Husserl, although in the last interpretation closer to Merleau-Ponty than to Husserl himself.

3.4. At this point it is necessary to make clear that Arne Næss and his critics in spite of the disagreement have a common platform. Using the distinction between syntax, semantics and pragmatics, they all assume that pragmatics is the fundamental discipline.

But while Næss tended to look upon the problems of pragmatics as empirical problems, his critics tend to look upon the problems of pragmatics as problems of the presuppositions of any empirical inquiry at all. If the term may be used: they are seen as problems of transcendental pragmatics.

This accounts for the turning away from traditional empiricism; instead authors like Husserl, Heidegger, Sartre and Merleau-Ponty have become important.

This has been worked out in an independent way by Jakob Meløe, a former pupil of Næss.

In a paper written in 1965 but only recently published (1972a), Meløe starts out with what at first seems to be trivialities about language and communication. A statement is uttered by somebody to somebody. But in such a situation there is not only a content which is communicated. Certain meta-communicative acts have to be presupposed in order that the communication shall take place. "It applies to statements generally: In order to say that something shall be possible, it must be possible to *say to* (somebody) (*henvende seg*). It is perhaps possible to imagine a language where no statement is about something. But is it possible to imagine a language where a statement is not directed to somebody?" (Meløe 1970). What Meløe does not say but shows definitively is that an analysis of language must start with the problems of pragmatics, and pragmatics is here conceived as an analysis of what necessarily must take place in a dialogical situation. The central propositions in pragmatics Meløe conceives as synthetic a priori : "It does not belong to the definition of a (mathematical) "proof" that a proof is made by somebody as something (e.g. not as an ornament), or that it shall be read (e.g. in distinction to being looked upon as a collection of discrete objects or that it serves some purpose, etc. But it is just viewpoints like these that can serve to explain what kind of existence a proof has. The central propositions in such an explanation will be necessary propositions." (Meløe 1970) Pragmatics is here conceived neither as a formal nor as an empirical discipline, but as transcendental pragmatics.³

4. Distrust of Methodology

4.1. When the study of sociology started in Norway after World War II, it was heavily influenced by American sociology. The empiricist mood which prevailed in the group around Arne Næss resulted in a fairly narrow selection from the broad spectrum of American sociology. It is not a coincidence that Paul Lazarsfeld was invited to lecture on the nature of sociology at Oslo in 1948. C. Wright Mills has later taken this lecture ("What is sociology?", mimeo., Oslo 1948) as an extreme example of what he calls "abstracted empiricism" (*The Sociological Imagination*, ch. 3). G. A. Lundberg was rather influential at this time. The outcome was a tendency to stress the importance of methodology, especially quantitative techniques.

This emphasis is still an important factor on the social science scene in Norway. The heaviest contribution to methodology is Johan Galtung's *Theory and Methods of Social Research* (1967). Galtung explicates the problems involved in the application of hypothetico-deductive methodology to sociology and other social sciences, with more weight upon technical matters than upon the philosophical foundations of the enterprise. The main tendency of the work may be characterized by saying that Galtung takes the Popper – Hempel view of scientific explanation for granted, and develops his more specific problems within that framework.

4.2. The views of Galtung belong to a great international tradition. Among Norwegian sociologists there has, however, developed a sceptical attitude towards this tradition, not so much in the form of proposing alternative methodologies, as in a distrust of General Methodology as such. Vilhelm Aubert is one of the most representative figures in this context. In the last chapter of *Det skjulte samfunn* he discusses problems concerning methods and theories in sociology. The first, English edition of that work had prompted a discussion in Sweden about "soft data sociology". *The Hidden Society* was evaluated from a methodological viewpoint and judged as deviating from the current positivism in meta-sociology with its requirements for hard data, writes Aubert (1969, p. 192). But Aubert refuses to see his work in these terms, because he does not accept the primacy of methodology.

Instead of this point of departure, Aubert starts with a substantial interest in what is going on in society, and with the relation of the sociologist to the society he is studying. The sociologist will then use explanations that he finds reasonable, and it becomes a secondary problem how these explanations eventually can be reconstructed logically.

To give the primacy to methodology is to put the cart before the horse. Rather than to define the problems in terms of methodology, critical sociologists should take care to concentrate on problems that also the common man experiences as essential.

When Aubert is sceptical about general methodology, it is because the most highly appreciated methods have not grown out of the study of the most important problems in sociology. The methods have been taken from natural science, or from normative theories based upon one or another interpretation of the procedures in natural science. Aubert does not think it is good luck for the social sciences to have the opportunity to learn from the natural sciences *in this way*.

He does not deny that methods of this kind can be useful within limited fields. But their success in some fields tend to give them the role of generating the problems worth studying, countless investigations are made in that way, but it might well be doubted whether they contain much of

value as knowledge. He prefers to look upon them in terms of what functions they have within the academic community: they tend to promote the advancement of learned men rather than the advancement of learning. "One is in danger of having to define science as an activity which is necessary for a certain period in life for those who wish a chair at a university" (Aubert 1969, p. 200).

Aubert prefers to look upon problems of methodology in the context of a form of life, as in the psycho-analytic situation or in the field work situation of a social anthropologist. Only in this way, by participating in social life, can one get to know what is really going on in a social context. Much data processing sociology lacks that reality contact. "In the worst case masses of data lead systematically into blind alleys, but even in the best case they do not generate fruitful problems in the way life in a real milieu does" (op. cit., p. 198).

Aubert makes another important point when he stresses that one of the differences between natural and social science is that in a certain sense all knowledge about social behaviour is already in the world, but often it is only regarded as knowledge if it is there where the higher social status and power is. Social research often means just that the knowledge there is, changes place.

As he ironically remarks: "It was first at the moment when Ishi had told Waterman and Kroeber about the life of the Yahies that his knowledge came into the world" (op. cit., p. 221).

The force of Aubert's viewpoint lies in the fact that he sees the sociologist in terms of his relations to the social context he is trying to get more knowledge about, where the sociologist as the knowing subject has no absolute privileged position. It is suggested that the problem of knowledge and research with regard to social matters ought to be seen in terms of the role that the investigator has in the social context.

4.3. Yngvar Løchen elaborates this suggestion in *The Sociologist's Dilemma* (1970). But according to Løchen the role of the sociologist is full of tensions. There is no clearcut distinction between his role as a scientist and his role as a citizen; this distinction breaks down in the actual research situation.

First, the sociologist is faced with internal value conflicts. To be a scientist is a value, and to have meaningful relations with others is a value, and these two values can easily come into conflict with each other. And secondly, the sociologist is confronted with conflicting role expectations coming from the social context where he is living and doing his research. As a sociologist he has to work out a solution to these conflicts. It is one of Løchen's main points that none of the existing solutions is really satisfactory.

The first type of solution is the science-oriented sociology, where the main value is to do research according to generally accepted standards of science. In order not to become involved in problems within the system he is studying, the sociologist will have to keep a distance to it. His main object is to arrive at reproducible and cumulative results, and if this comes in conflict with human significance, the latter is the less important value. The danger here is a loss of significance and sense of reality.

The second type stresses practical relevance, but at the same time takes care to apply objective and standardized methods. This brand of sociologist is in many ways close to the science-oriented sociologist, but he will select his problems on the basis of possible applications rather than in terms of the requirements of a strictly cumulative science. This kind of sociology also has its dangers. The stress on practical relevance may lead to a loss of perspective, research may become too particularized. It easily loses real life-contact with those being studied, and the relations to sponsors, to those who pay for the research and want to use it, is very problematical. If such problems are forgotten in the daily research routine, this kind of sociology can easily become a prop for particular interests and powers in society.

The third type Løchen calls engaged sociology, and in works of this kind he himself has been mainly engaged in. A sociologist of this type will find it necessary to become much more involved in the problems of those he is studying, and his problem is one of closeness versus distance. If he becomes too closely involved in his subjects' problems, he runs the risk of "going native" in the system he studies; if he keeps too much distance, he may fail to understand what is going on in the system.

The fourth type is action research. Here the research group defines its duties more widely than in other types of sociology. The sociologists are not only interested in helping those studied to a better understanding of their situation, but also to help them furthering their interests and improve their practical situation. The difficulty here is that the sociologist may become so involved in politics that the research side of the project dwindles into the background. Although action research is looked upon by many as the most hopeful way out of the sociologist's dilemma, it is the most unclarified sociological praxis form.

Løchen's intention is not to give solutions to the dilemmas facing the sociologist, but rather to sensitize people to the problems there are. He points to problems that social scientists as well as philosophers of science had better not overlook.

5. The Political Turn

5.1. The discussions within philosophy of science had never been quite unrelated to political discussions, but this relation became much closer from 1968 onward, due to the new student movement. The Norwegian student movement was at that time strongly influenced by the student movement in Germany, and by the ideas coming from The Frankfurt School. Especially the writings of Jürgen Habermas came to play an important role in these discussions.

This influence became even broader than might have been expected, due to a special circumstance. The philosophical criticism of technocracy turned out to coincide in a certain way with a practical criticism of technocratic planning, which was a central point in a new political movement then just emerging, namely the populist movement. The main ideas in this movement came from Ottar Brox, a social anthropologist.

In 1966, Ottar Brox published a book where he severely criticized the system of economic planning practiced in Northern Norway.

Brox makes a distinction between two types of economic planning. One type takes as its point of departure how to improve the economic condition for the people, but this is done in a certain abstract way. A certain sector of economic life, e.g. fishing and fish industry is chosen, and the problem is to maximize the general income level within this sector. Better and bigger fishing boats may be one of the solutions. But such a solution requires a redistribution of the population towards the smaller and not quite so small towns. That is, the distribution of population is the dependent variable, and the maximizing of economic benefits *according to a certain kind of national budgeting* the independent one. This kind of planning has to be done by specialists, and it is done more or less "above the heads" of the people; the plain man will have nothing to say but to adjust to the situation. This kind of planning Brox calls technocratic planning.

The second kind of planning takes the distribution of the population as it is (with some reservations) as the independent variable, and asks the question how it can be made possible for the inhabitants of a local community to improve their economic benefits, keeping the local community intact. This second kind of planning Brox calls populist planning. Populist planning presupposes a fundamental democratization of the planning process, which technocratic planning does not. Further he suggests that the benefits technocratic planning seems to give are in a certain sense spurious, because they are dependent upon a certain kind of economic book-keeping, which gives a partly false picture of the real costs and benefits.

These ideas of Brox have become rather influential. Populism has indeed become a kind of popular movement, and the sceptical attitude towards technocratic planning has tended towards a redefinition of political problems generally.

5.2. Within one field critical theory has had a fairly strong practical influence: the field of education and educational planning. Due to critical works of Dale, Hellesnes, Skirbekk, Skjervheim, and others, this has resulted in a public debate about the science of education and its practical applications. The main target of criticism has been the technological conception of educational practice. This conception has been a fairly dominating tendency, more implicitly than explicitly, but becoming rather explicit in the latest programs for an educational technology.

The impetus of this criticism is at present strongest in the field of university politics. It has influenced fairly strongly the foundational policy of the new University of Tromsø, as well as the new district colleges (*distriktshøgskolar*). But it is still too early to say definitely how this will work out in practice.

6. Conclusion

Norwegian social science is, as some of its leading practitioners have emphasized, diversified and pluralistic. The same is true of Norwegian philosophy and *Wissenschaftstheorie* at the present time. The empiricist mood of the early post-war days has given way to a plurality of approaches, often represented within the same institution or even the same individual. Thus one finds philosophers with a broadly empiricist and analytical background along with phenomenologists, critical theorists, Marxists, and structuralists. The confrontation between analytical and continental

traditions has led to a lively debating milieu, but so far there are few substantial research results of a more general interest to point at.

In conclusion, we should like to draw attention to some recent work illustrating various approaches which seem promising at the moment.

(i) *The historical approach* is represented, programmatically, by Næss's latest work (section 2.4. supra).

(ii) Several social scientists tend to see *action research* as the most promising way out of the impasse of empiricist social science (e.g. Løchen 1970, Mathiesen 1972). The philosophical aspects of this approach to social science are only beginning to be elaborated (e. g. by Jakob Meløe in a short paper (1972b)), but not least because of the emphasis given to this approach at the new university at Tromsø, one might expect this to be a growing point also in the philosophy of science.

(iii) Others see more hopes in a programme of *logical analysis* with a twist in the direction of *systems analysis* (Dalseth 1972, Hernes 1971, Elster 1971). The most substantive philosophical illustration of this approach so far is a critical survey of recent work in economic history by Jon Elster (1972). Illustrating profusely with concrete examples from historical research, Elster criticizes the use of "genealogical explanations", which he defines as "a method which attempts to explain the development of a system through a sum of explanations for the development of each system factor in isolation from the rest" (Elster 1971, p. 18). Instead, he argues for the necessity of a functionalist and historical view of society. The proper object for historical study is "systems in change", and there is only one method for studying systems in change, viz. the "genetic" or "dialectical" method (pp. 40, 51). Having thus disposed of traditional genealogical and functional modes of explanation in history, Elster goes on to a critique of the use of counterfactual hypotheses etc. in recent economic history, using Fogel's *Railroads and American Economic Growth* (1964) as his main example.

(iv) The *ethics of science* is an aspect of the scientific enterprise which has been grossly neglected in recent philosophy. Knut Erik Tranøy has been working on such problems in the last few years, and has recently published a "preliminary survey" of the field as he sees it (Tranøy 1972). Tranøy distinguishes five main components in the current "ideology of science and scholarship" : the principle of value neutrality, general scientific methodology, the doctrine that science should be for the benefit of mankind, the idea that knowledge is valuable in itself, and the norm of freedom of inquiry; and points to the necessity for rejecting at least some parts of this internally inconsistent set of ideals. The young political scientist Regi Enerstvedt has tackled similar problems from a Marxist point of view, emphasizing the relationships between scientists and societal conditions (Enerstvedt 1971). Whereas Tranøy tends to stress the logical aspects of the ideology of science, Enerstvedt concentrates on the historical conditions for the current ideology of science and its function in society.

(v) Ferdinand de Saussure's project of a general theory of signs, a *semiology*, is, lastly, attracting the attention of a number of researchers within the human sciences. A research group consisting of philosophers, social anthropologists and others has been working on semiological problems over the last few years, inspired by such figures as Derrida, Greimas and Levi-Strauss, and with close

connections to the Copenhagen group (Peter Madsen, Per-Aage Brandt and others). Some preliminary results have been published in the Norwegian journal of philosophy (e.g. papers by Kjell S. Johannessen, Atle Kittang, Arild Utaker, and Audun Øfsti in the 1970 volume of *Norsk Filosofisk Tidsskrift*), and there is more to come.

Notes

¹ It is worth noting that Næss himself has never accepted the unilinear conception of the growth of science; see Næss (1936), pp. 233f.

² To avoid misunderstanding, we should like to point out that the next few paragraphs have been written by Tore Nordenstam.

³ The transcendental point of view is stressed also by Skirbekk (1970) and Rossvær (1972).

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