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THE FIGHT AGAINST REVELATION IN SEMANTICAL STUDIES

It is a well known phrase to say that the difference between religion and belief on the one hand and knowledge and science on the other, may be described as follows: the former depends on revelation, as we say in Dutch and Norwegian on "openbaring", the latter not. As a matter of fact, however, I very often have the impression of struggling revelation, even in semantical and sociological studies. I suddenly find myself on the point of presenting something as a result of my brain-work without being able to give a somewhat detailed description of my assumptions and the auxiliary hypothesis which reasonably must have led me from the observations of any kind to this "result".

In the following I will mainly deal with certain means of resistance against this temptation of revelation. This is to say that I will try to give a hint of a kind of investigations which will be involved in all philosophical or logical analysis and semantical or signific studies of our Oslo-group, and which we at the time regard as being the most important part of such studies. Arne Naess has suggested the name of "elementary analysis" for those kind of investigations.

As an illustration of elementary analysis I will use my own study on "private enterprise".

The most usual procedure so far applied, when analyzing a linguistic expression would be one of the following.

The analyst or investigator makes a single subject, namely himself object of an investigation and records the ideas immediately. The analysis might also include a criticism (unfavourable) of the accessible or potential, but frequently less successful attempts in the same direction of other authors. Or the analyst may back up his hypotheses of usage by quotations which may be interpreted in such a way that they directly or indirectly are *supporting* his ideas. Or fellow human beings might be asked what they mean by or *maintain* to mean by the

linguistic expression in question, according as how refined the questionnaire is.

In all the above-mentioned circumstances the analyst might pretend to have revealed he proper meaning, *the* meaning of the expression, the only possible direction of precization, definition, delimitation of the concept etc., which every person that attains sufficient insight eo ipso consequently must acknowledge. Or he restricts himself to pretend that he has tried to delimit *one* type of meaning, usage or direction of precization, which the author himself prefers or, for other reasons, wants to draw attention to. Or he might try to describe a *series* of usages, for instance, all imaginable or generally important ones or do so for detailed purpose. In the latter case the analysis might be completed by making propaganda for a certain usage or a combination of usages while giving reasons for the selection in question. This might be done either by maintaining that the preferred direction of precization is the only "true", "correct" one in consequence of etymological, historical or similar deliberations or *the best one,* for instance, the didactically most usable one in relation to specified or unspecified purposes.

Our procedure was in many ways rather different from these.

We started collecting what we call occurrencies, that is here quotations from the newspapers. We went through two annual series of all the newspapers in Oslo and quoted every passage were the word "private enterprise" was used.

We then read the quotations pretending to know nothing about the connotation of the designation of "private enterprise" and just recording what we thought we learned from the different quotations about the usage of this word by studying the contexts of these quotations. This *occurrence analysis* together with a thorough going pretesting of so called "big shots" within different sociological groupings, at last enabled us to construct a questionnaire.

The respondents were chosen from different sociological groupings and given different kinds of questions, 102 in all — most of them rather complicated. It happened, that respondents had to spend six hours to answer it.[1]

The most interesting problem from a semantical point of view is however this:

Regardless how refined and complicated the questionnaire is constructed how is it possible on ground of the respondents' answer to it, to map out the different usages of the word "private enterprise"?

1 Later on we have made use of still much more complicated questionnaires.

The first step is to pretend to be ignorant of what the word "means", and to describe what we learn from each of the many question-answer-constellations about the use of the word. But we should not be content if the *occurrence-analysis* did not lead to a *regularity-analysis which tries to find certain regularities in the use of the term.* At last this work enabled us to set up hypotheses concerning the different ways of using the word "private enterprise" within a Norwegian society of language.

However it is rather easily done to maintain that a linguistic expression has certain meanings or *usages,* as we are putting it, which might be described in detail. It is probably more difficult to *check* such an assertion or theory in a way generally recognized as reliable. This *verification* presupposes the most important of elementary analysis, the so called subsumption-analysis. Of course, the difficulties might be reduced by confining this theory to the theory: *if* the usage B tells how the expression is used within the linguistic system S, then the occurrence of the expression exemplifies B. In that case the exemplification reasonably could be conceived as a link of the description of B, as a didactical instrument to facilitate the process of understanding more precisely what B involves, what kind of usage is assigned to by the description of B and so on. *But* it is important to keep in view that the case is quite different *when the description moves into establishing a theory of actual, current usage,* a statement that within S is used in accordance with the rules disclosed in B. The function is then transformed from deepening the reach of the rules of usage into confirming that they are actually in force within the society of language S. From being an example of B, the results change into a verification of it—partly or, at worst, the whole material which the theory of B rests upon.

The difficulties of subsumption might be described as the difficulties of giving reasons why one supports or does not support the proposition of B. In other words, it is the difficulty to decide whether a given occurrence or expression represents, exemplifies, or is a special case of a disclosed, more general rule of usage within a fixed society of language. The aim of subsumption-analysis is to find the arguments pro and contra subsumability and to weigh them against each other.

As a rule, in analytical and lexicographical procedure the difficulties of subsumption are slurred over by not explicitly disclosing whether the exemplifications are meant to serve as didactically useful illustrations of a certain theory of usage *or* they are meant to furnish material to a decisive verification of the theory. This brings the proponents of the theory in a favourable position from the point of view

of tactics in controversies. The favourable position reinforces the self-deception concerning the unassailability of the theory without giving much support to its practical tenability in practice. "To bring out what I mean more precisely, I will give an example. . . ." is a stereo-type cliché. Under circumstances like this every reader tends to perform the subsumption, because the kind of example in question exercises influence on the interpretation of the described usage in such a way that they almost by definition imply the subsumability of the example. Secondly, this is appropriated for the tenability of the theory, a symptom that the theory covers the field of application represented bij the example.

We have even worked out questionnaires including small texts where definitions were used. Some of the texts were worded after the following pattern: "The word x seems to be used in different ways. Occasionally it is used in the sense of y, as for instance in the sentence: .....". We inserted a sentence where it seemed preposterous to believe that the word was used as indicated in the text. In spite of this there was a tendency among the respondents to agree to the subsumability. Some questionnaires were formed with questions of the following kind: "Do you think this x, is a good and bad example of y being used in the sense of z?" The respondents revealed lack of definite criteria of subsumability.

One of the main reasons for the uncritical attitude adopted by the respondents towards such definiens formulation might hus be described as follows.

If "private enterprise" is defined, and the author uses the sentence "x is private enterprise", the reader will tend to change his interpretation of the definiens formulation if the properties he attributes to x seem not to allow subsumption if one sticks to one's initial interpretation of the definiens. This procedure radically destroys the function of the definition. Instead of giving us precise hypotheses and norms for usage to be tested by observing usage, the definitional formulation is looked upon as a formulation, the meaning of which is to be understood by means of the use of the definiendium within the field of application.

To break with this vicious circle, we tried to make clearly known that all the different kinds of occurrencies are regarded as *material* fit to check the mentioned hypotheses of the usage of the word "private enterprise".

Then the problem is set. How can we decide whether an answer to a possibility of answer (possibly a certain type of answers to a certain type of possibilities of answers) supports our hypotheses of

usage and, in case the hypotheses imply two or more diverging usages, how can we decide which of those usages, if any, the respondent has intended to express in his answer, which usage the respondent accepts in accordance with the requirements of the question?

In view of this situation, it is difficult to see how it should be possible to avoid another investigation where the respondents have to face the whole quantity of material with the double task 1/ to isolate the most diverging usages and 2/ to classify each answer in accordance with the rules of usage which the respondent supposes they intend to follow. It has been maintained that such deliberations would lead this method of questionnaires into an endless chain of metaquestionnaires, because the same difficulties would appear by judging the "questionnaire of subsumption" and consequently demand another "questionnaire of subsumption", of the second class, which in its turn would presuppose a questionnaire of the third class and so on. Of course, this is, indeed, possible when a very high degree of certainty is demanded as regards the subsumption. But in practice this scarcely happens. For instance, as far as the situation in question is concerned, the proposals of usages and a subsumption of possibilities of answers of a single investigator is considered a too hazardous foundation of inferences and predictions of a general interest. This is very much so owing to the minute possibilities of checking. Among other things the investiagator has immense difficulties in giving a some detailed description of the auxiliary hypotheses which presumably have led him from the observation of the material to the finished subsumption. Then it gives a certain confidence to know that others have tried to do the same kind of work and that they have recorded results which seem to come near to those of the investigator. Even a fairly superficial comparison of the subsumption which shows a high degree of positive correlation could, at any rate, reasonably be conceived as symptoms that the assumptions of the investigator are not quite individual. Of course, in this connexion a "public opinion research" would be the best but it is out of the question. We must keep within the limits of consulting a competent group of specialists in semantics, especially in what we call interpretation and precization. Such a semantical panel consists of students from different parts of Norway, from different social strata who have passed the examination in semantics with the very best marks.

The members of this panel have to face the whole quantity of material with the double task to isolate the most diverging usages which they would guess the different respondents intended to follow and to classify each possible answer to the questions in the question-

naire in accordance with the thus delimited rules of usage. The latter is to say that the semanticists were asked: "If someone answers this question number so and so like this....? what rule of usage do you guess he then intends to follow? Or what would you think of the respondent's linguistic intentions if he chose this possibility of answering....? Or this one....? Etc." The semanticists were not only consultants to the investigator, but they even formed a sort of committee (together with the investigator), which after having classified and subsumed possibilities of answering, severally, met and coded them jointly. At last we thus were able to chart the trends of the usages of "private enterprise", and the real slogan analysis and bias analysis might start.

It is evident that our methods are much more troublesome than the revelation-procedure. On the other hand it might seem as evident that our procedures would appear to be more advantageous for everyone being interested in fairly exact and testable methods. And without elementary analysis as e.g. the subsumption analysis we will tend to doubt that so-called logical analyses and semantical studies ever will reach a scientific status. Actually this will be the case even within other fields of more established science – e.g. within social science and with special regard to the problem of coding free answers, etc.

However, from one critical point of view it might seem reasonable to consider that the elementary analyses make our type of studies unnecessarily comprehensive and detailed. It might be said that in so far one of the purposes only consisted in procuring a survey of certain common usages of the linguistic expression "private enterprise", it would be sufficient to consult those encyclopaedias and dictionaries which have as a matter of fact been worked out exactly with a view of giving assistance demanded in questions like these.

We have not been blind to this easy way out either, but have considered it most advantageous to dispense with it for several reasons as earlier mentioned.

In addition is to say that the encyclopaedia articles often exclude many of the plausible and cognitively different interpretations which even tutors in "discussion technique" often discerned, in favour of far fetched, rare and cognitively identical proposals for synonyms – especially uninteresting for our purposes.

Furthermore, because of their usually low level of precization it proved difficult from the lexicographical indications to derive directions of use appropriate for clarifying misinterpretations and other terminological errors, pseudoagreements and pseudodisagreements, etc.

In short, philologists and lexicographers are not much more than

human beings and nearly similarly constituted. The s.c. "apperception mass" which lies at the base of all their individual word explanation intuitions has presumably been shaped by the contact with human beings as philologists and dictionairies, and is stamped by it.

If the observation material is rich and representative and easily expressible by an adroit philologist, there are reasons to attach certain hopes to the results of their diligence. It is however exactly this which in the different cases may be questioned, namely whether the conditions actually exist, the fulfillment of which are of primary importance in inducing a reasoned confidence in the reader. There seems to be wanting some checking procedure of one kind or other.

It is hardly particularly unreasonable to assume that this checking procedure in most cases should be carried out according to the directions as indicated in the Private Enterprise study. It would thus only make sense to consult a dictionary so far as its so called "word explanations" were based on rather deep going and comprehensive investigations of the usages of the word in question e.g. after the pattern of our slogan analyses.

We do not trust revelations within any branch of science, not even within humanities and not even when received by semanticists, logicians or philosophers, — not to speak about more or less advanced philosophers, — not to speak about more or less advanced philologists and lexicographers.

Certain presumably important movements in modern philosophy, most frequently designated as "analytical", show an inclination to a programmatic delimitation of their field of research mainly or exclusively to a certain kind of investigation of language and reflection on these, commonly coined "logical analyses".

Among the many terms of abuse which members of the analytical movement use to characterize and devaluate certain allegedly objectionable aspects expecially of non-analytical philosophical movements are "cathedra philosophy", "word magic" and "verbalism". The "cathedra philosophical" problem constellations are, for those who use this term of abuse, atavisms from those olden days when foreheads were wrinkled and thoughts profound.

Somewhat more characteristic, however, is the cathedra-philosophical treatment of the problems. The cathedra philosophical procedure is characterized especially by the fact that it submerges the amazed reader in a true Amazonflood of results of the supposed brainwork of the thinker, while the activity in itself with admirable discretion and heroic selfforgetful reticence is shrouded in a stubborn silence heavy with profound thoughts.

Quite different, however, are the every-day ideals and the beggarly claims which lie at the base of most works of the Oslo-school of the analytical movement. Here we have diligently tried just to avoid setting forth anything we might call "results" of our activity without being able to refer to explicit and relatively detailed descriptions of what we have done to reach it.

The most pleasent and confortable way of doing philosophical analysis and semantical studies is of course to sit in an easy chair in a good library and just record your "results" without worrying about their origin, how they came about in your head. If you are sufficiently uncritical, you may have the most exciting experiences. You might shock yourself by discovering that "nothing exists" not even the sentence saying that "nothing exists". Or you may rest assured that "there is rationality in reality", "laws in nature", and that "the will of man is free" and "determined" just as you wish — every man to his taste.

In few words: you are blessed with cathedra philosophical revelation.

I have however the suspicion that the exciting or consolatary conclusions arrived at by your activity in the easy chair will not have the minimum degree of testability which you would require within your special field of established science.

The sentence from Gorgias for instance: "Nothing exists" will become exciting only if you do not use "exists" somewhat in the sense of "exist" of Parmenides, and rather tenable only if you *do* use it strictly in that way. The question if there is a "rationality in reality" is only interesting if you slure over that you use the two words in such a way that it makes the statement *by definition* true or false, and that your answer to the question usually depends upon which of the possible interpretation you chose.....

On the other hand, any kind of analysis of words like meaning "exist", "reality", "free will" etc., will not either be much more than a magical play with words if they do not involve the trivial and troublesome inquiries which we have called "elementary analysis".

To quote Arne Næss in the introduction to the fifth volume of his book on "Interpretation and Preciseness" [1], Arne Næss believes that a great many of the allegedly important assertions in the writings of modern analytical philosophers presuppose more strict, exhaustive and unbiased elementary analysis. Some might answer that such inquiries are uninteresting and unphilosophical, or that when philosophers have indicated the principles and frame of such inquires, the rest may be safely turned over to scientists. This answer seems to

1 V. Principles of Elementary Analysis, Oslo 1949.

be based on an understanding of the difficulties of establishing new fields of scientific method and an overestimation of achievement of vague, preliminary, socalled philosophical discussions. [1] The conclusions from discussion seems generally much too pretentious and cocksure. The degree of accuracy of the analysis is not proportionate to the level of aspication of the investigator as judged from the expressions used to indicate that level. There is rather an inverse relationship.

Further development of analytical philosophy and semantics presuppose a development of *analysis as a science".*

Arne Næss expects however that sufficiently strict and unbiased elementary analysis will be found too troublesome and annoying and that consequently the output of statements of semantics will decrease among persons being aware of these difficulties.

And he ends up with the assertion that anybody being convinced that it is easy to "see" what a word or sentence "means" in a given case, and easy to "see" whether a term is used in harmony with a given definition or not, he should find his monograph useless or even confusing. "He should, however, remember that his conclusions are questionable not only as regards their tenability, but even as to their meaning as long as the way to test them is superficially described or left wholly unmentioned. I hope that such a person either will accept the following descriptions in their main features as descriptions of how it would be necessary to test his conclusions, or that he will be so kind as to indicate how *he* would test them."

I will here leave unmentioned the impulses which may have induced the members of the Oslo Group of the Analytical Movement humbly to leave the via triumphalis of the classical philosophical revelation in favour of such a completely charmless back-alley. Suffice is to mention that according to our — and some others — view, a favourable development of that branch of scientific activity which is concerned with semantics or significs must continue exactly along this road. And our aim should be to arrive at methodes of descriptions that will be so different from the cathedra philosophical revelation procedure that even those which have been applied e.g. in the "Private Enterprise" analysis and others, will be included in the old category and designated as "revelational", as "verbalistic", as "a magical play with words" etc.

"Then at least there will reasonably lie some comfort and satisfaction

1 In the Journal of Politics 1946, G Niemeyer reviews the important philosophical work of F. Kaufmann, "Methodology of the social sciences", and he indicates there a critical attitude which seems to us sadly justified not only towards he work of Kaufmann, but towards nearly all philosophy of science including my own writings.

in the illusion that we have contributed a tiny bit to call forth that be based on an underestimation of the difficulties of establishing new ideal situation, even if only by a miserable little mite, a scarcely audibly call in the wilderness." [1]

1 Herman Tönnessen: "On Concepts of Type", published (in two volumes) as "Filosofiske Problemer" nr. 12, p. 231, Oslo 1949.