

Pluralism in Cultural Anthropology: Comments on an Article by Hans Skjervheim

1990H

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This article was written in 1990 and is published here for the first time. As the title implies, it is in response to an article written by Norwegian philosopher Hans Skjervheim. That article, "Ad Skjervheim," is in Norwegian only; it appeared first in a journal and was reprinted in 1997 in a collection of Skjervheim's work edited by Hermund Slaattelid.

What can I do except join Skjervheim¹ in his efforts to clarify and reject the absolutistic 'scientism' of Lévi-Strauss? But I can do it only with some trepidation because of the difference between the methodological jargon of Lévi-Strauss and that of myself. An example: the former says that "the model should be so constituted as to make immediately intelligible all the observed facts."² Either Lévi-Strauss uses the word 'model' so differently from myself that no 'yes' or 'no' is relevant in the discussion, or I must say NO with the feeling of desperation. The function of models is such that if it covers, and even makes intelligible, 'all' pertinent observations, then it is not a *model*. Models are highly selective. And how can he use the word 'fact' in this connection? An anthropologist notes down that two members of such and such culture play (fight, make an axe, blame each other . . .). His conceptual framework is his own, however talented he is in putting "himself in the place of the people living there"³ and, also, however clever his theoretical models.

In my favourite terminology of the '30s and later, the anthropologist *witnesses* things rather than observes. With his witness reports functioning as a sort of raw material, he successively eliminates or changes those sentences which manifestly reveal the provinciality of his original concepts. Thus, if he stated "then the friends shook hands," he may be correct because of increasing evidence that 'friendship' as "we"

understand it is not *their* concept, and because the reaction sequence named 'shaking hands' does not have that significance.

So, where are the 'facts'?

Suppose anthropologists from cultures A, B, and C compare each others' reports as witnesses and their laboriously constructed accounts of the social relations of the Balinese. Why should we believe in a kind of *true* scientific account of those relations? The greater the differences between A, B, and C, the greater, on the whole, will be the differences in their accounts. If *very* great, the classification of all three as 'anthropologists' gets to be rather shaky. Which are the maximum methodological divergencies possible with the conceptualization 'anthropologist'?

We may assume that by means of scientific methods of some sort we are able to reduce the differences between the three accounts of the social relations of the Balinese. But even if against all odds, the number of differences could be reduced to zero, the appearance of a fourth anthropologist, D, belonging to a young generation is likely to shatter the artificial joint account constructed by A, B, and C.

The problems faced are essentially the same as those facing us when we try to formulate a definition of a term, e.g., 'democracy,' on the basis of a set of use occurrences of the term. Or, the construction of a characterization of a *Verhaltensweise* corresponding to the cognitive content of 'the distance between Sun and Earth is 149.5 million km.' based on a set of descriptions of behaviours (verbal and non-verbal) of scientists.

Pluralism is inescapable, and nothing to lament. Reality is one, but if accounts of it are identical, this only reveals cultural poverty. Excessive belief in 'science' favours acceptance of poverty as a sign of truth.

According to Lévi-Strauss, good models will save us from pluralism because of their intimate relation to what he calls 'reality,' sometimes even 'deep reality.'

Strauss's references to Freud are important in this connection.

The concepts used by Freud in order to describe the function of the unconscious are socially understandable. "I intended to kill my father" may be unacceptable to the patient at a certain stage of the psychoanalysis, but *patricide* is part of a set of understandable social

concepts. Physicists may introduce completely new concepts like “quarks” in fundamental explanation (i.e., on the top level of hypoductive systems). But social scientists must, I presume, narrow down the areas basic concepts in his models to that of socially amused acceptance, such as that we *really* are mere appendages of selfish genes,⁴ but this conceptualization is useless in social science. The ‘realness’ Dawkins asserts is only one in relation to certain *biological* models.

It is amusing to imagine that we are only appendages of certain kinds, just as it is entertaining to think of ourselves as swarms of atoms, or as many-dimensional oceans of probability waves. If we live long enough we can look forward to a series of new wonderful models superbly popularized by eminent natural scientists.

In short, if Lévi-Strauss relies on models from the most modern natural science, logic, and mathematics, the pluralism is not only inescapable, but very clearly stated in modern general methodology. The models are mutually inconsistent; otherwise, change of models would be only moderately interesting. If we think of reality as one, and I do not see how we can avoid that, the relation of the models to reality cannot be but indirect. They cannot put us in touch with deeper layers of reality in the way Freud tried to do. His conceptualizations served the aim to make our feelings and actions more understandable.

The astonishing success of physical models since Galileo justifies the belief that they *somehow* picture something real and independent of cultural differences among scientists. The equations seem ‘themselves’ to be independent, but their use and interpretations are always dependent.

Against what I have said so far, it may be argued that it is absolutistic: the pluralist conclusion is asserted ‘monistically’ at a higher meta-level. The pluralism is part of a total view which is not mentioned. If Lévi-Strauss has a different total view, my arguments for pluralism may be misconceived, irrelevant, or simply not understandable from his point of view.

It would lead too far to go into the problems of total views. Suffice it to say that some of my formulations are purposely made absolutist in order to remind the reader of those problems. This holds, e.g., for the bombastic sentence “Pluralism is inescapable . . .” When I work toward a total view, pluralism gets increasingly ‘obvious,’ or gets to be

derivable with increased rigour from fundamental norms and hypotheses.

With Skjervheim I agree that Lévi-Strauss seems to favour a kind of scientific absolutism. I would add “with an atomist flavour” considering his ideal of “reintegrating culture and nature and finally of life within the whole of its physico-chemical conditions.”⁵ Atomism I understand as the opposite of gestalt thinking, that is, thinking in terms of ever more comprehensive wholes, and stressing the non-reducibility of higher order wholes to lower order wholes. Classical examples: melodies to sequences of noises or even tones, propositions or units of intended meaning to sentences or formulations, *Verhaltensweisen* or operations to reactions.

Mathematics, chemistry, socio-biology, and other branches of science are of heuristic help to social scientists as ‘auxiliary sciences.’ But they do not help to dissolve sciences of man. They do not dissolve any *thing* or *process*. ‘Reduction’ as a part of the scientific enterprise must be understood in a different way.⁶

Skjervheim’s way of saying essentially the same or similar things to what I have so far said in this article is often not my way. The difference is instructive, but not easy to pin down in clear words. In what follows I shall try to clarify disagreements about how to interpret some points in my *Erkenntnis und wissenschaftliches Verhalten* (“E”).

The biologist and philosopher von Üexküll worked out clearer than others that each species has a set of different reactions applied in different kinds of situations (defined by the biologists!). The world each lives in as an experienced world has the number of discriminable features which manifest in a number of kinds of reactions. This is all very inspiring except that every reaction and situation is of course described in terms of the *human* observer, as features of *his* world.

Now, if we as cultural anthropologists try to “study men as if they were ants,”⁷ the reactions and situation we describe in our witness reports on culture B is in terms of our own particular culture A. The culture B cannot be described except as something *completely* within the cognitive framework of A. The world of B is either naively pictured as a part of the world of A or else said only to have an abstract structure corresponding (isomorphically) to a non-abstract structure within the world of A.

When it is said that a person, N. N., within a culture, B, thinks he or she is actually *identical* with a particular animal, the terms *person*, *identical* and *animal* (etc.) possess the connotations they have in A. We, members of A, get an exciting picture of B, but we should not pretend we can experience the world of B through the connotations of words belonging to A. If we do, we succumb to the mistake of ‘maze-epistemology.’ (We describe the movement of an animal in a maze without clearly recognizing that the maze is a feature of our experienced world rather than of that of the animal.)

The cultural provinciality of anthropologists can be reduced through successive (endless) modifications of their accounts. In the example given above, the member of culture A will try to find out how people in B experience what *we* call animals, persons, and identities. Another example: how can Sherpas seem to say (in their language) that the mountain Tseringma *is* a mountain *and* a princess? How can we translate, *if at all*, their verbal utterances in such a way that we approximate *their* experience of Tseringma? The attempts to do so result in a plurality of versions.

Strangely enough, the successive approximations—if such are at all forthcoming—are reached by successive steps of methodological alienation: We *start* treating the members of B as colleagues: the Sherpas assert ‘mistakenly’ that Tseringma also is a princess. Then we retreat from them, allowing for the possibility that their concept of a mountain is not the same as ours, nor is that of a person, nor their use of language. We may even start looking for evidence that identification somehow is differently conceived.

If very successful we may be able roughly to experience the world as a member of culture B, still retaining the ability somehow to describe the experience in the language of A. But with further approximations this ability gradually vanishes: it is a basic feature of the experience within culture B, that it does *not* somehow contain the consciousness of being different from A. As long as the cultural anthropologist walks round *with his notebook in mind*, his experience is in one fundamental way different from that of B.

The eminent cultural anthropologist Fredrik Barth sometimes writes as if humans are capable of going in and out of many different cultures, participating in each word they define. I presume he does not pretend to grasp more than fragments.⁸ If he went in deep I presume it would be next to impossible to get out.

Literally speaking, everything is affected by a culture—including the primary sense qualities like length and breadth. The *size* of coins perceived by members of the culture in which the coin is used is perceived differently according to its value.⁹ An axe is full of cultural traits, and in at least one culture it is perceived as a status symbol in a way so complicated that it would take years to adjust one's behaviour to its cultural ontology. The repugnance toward lack of intelligence is so pervasive in our competitive civilization that rats *classed* as dull are treated worse than those *classed* as bright—with the effect that the former perform even worse than necessary.¹⁰ Cultural priorities determine behaviour in ways completely unknown to the participants.

The terms *behavioural* and *behaviourism* are usually associated with the doctrines of J. B. Watson and other 'molecular' behaviourists, not with the 'molar' behaviourism of E. C. Tolman. The difference is philosophically essential because the molar view is an intentional view.¹¹ In E, I use the Tolmanian conception of behaviour as action, not reaction. The term *Verhaltensweise*, way of behaviour, is *not* observable as a succession of movements or reactions.

Skjervheim suggests that in my earlier work (E) "behaviour" did not yet have intentional import. I disagree. Perhaps I did not make it clear enough that the 'stranger from another planet' whom I introduce for certain purposes, is not a Tolmanian behaviourist? My *Verhaltensweise* was meant to offer a synthesis of *Erkenntnisinhal* (cognitive content) and *Erkenntnistätigkeit* (cognition as act). Only inherently purposeful ways of behaviour could represent cognitions as acts and content—in the kind of science of science envisaged.

When operationists like Bridgman insisted that in science of science one should not listen to scientists but see what they do, I stressed that in that case one should not even listen to how scientists define their various operations, but must take up the attitude of the anthropologists or even the rat psychologist in observing the scientist. An illustration:

A: What do those two scientists do?

B: They each perform an x-ray measurement.

A: But they move very differently. Look at their arms.

B: Nonsense: The one is left-handed, the other right-handed. You must understand that those differences in reactions are totally irrelevant.

A: But what do they do now? They behave very differently.

B: Nonsense. They still perform an x-ray measurement. One of them smokes a cigarette, the other sings. You must understand that it is a completely irrelevant difference.

In so far as 'x-ray measurement' is a kind of *Verhaltensweise*, it is a complicated task to describe it in terms of relevant and *only* relevant traits. But if that could be done, the intentional aspect would be included, simply in terms of relevance. Intentionality does not manifest itself in pure consciousness, but in units of verbal and non-verbal behaviours.

This is the point of view of E, and I accept it. But just as a cultural anthropologist starts out using uncritically the conceptual framework of his own culture, the science of science specialist starts out as a colleague of the scientists, that is, methodologically as a 'witness.' The kind of scientist of science envisaged in E tries step by step to get rid of the prejudices due to cultural affiliation. But where does this end? I agree with Skjervheim that there is a limit beyond which the scientist of the science B cannot any longer be said to *understand B*, or more precisely, does not produce an account of B that makes B *understandable*. I think the molar behavioural (*objektiv psykologiske*) account envisionsal in E is inside the limits, whereas that of the "researcher from a strange planet" is outside. He establishes a science of science of sorts, and according to the stated assumptions, one in a strange, but perfect way, *explains* the difference in cognitive content between various human hypotheses about the distance of the sun. He is in one way completely inside (and perhaps very proud of this), but at the same time hopelessly outside in another way. Call it the hermeneutical way, if you insist.

Notes

¹ Hans Skjervheim, "Ad Skjervheim," in *Regime under Kritik: Om Hans Skjervheim I Norsk Filosofi Og Samfunnsdebatt*, edited by Hermund Slaattelid, Oslo: Aschehoug, 1997. pp. 49–53. Skjervheim addressed the same topics in "Objectivism and the Study of Man," in *Positivism, Dialectikk, Materialisme. Den Norske Debatten om SamfunnsuitenKapenes Teori*, edited by R. Sladstad, Oslo: Universitetsforlag, 1976.

² Quoted on p. 6 in Skjervheim's article.

³ See quotation on p. 8.

⁴ For example, Richard Dawkins, *The Selfish Gene*, 1976, New York and Oxford, Oxford University Press.

⁵ Cp.p.13.

⁶ Cp. Bonner, J. T., *The Evolution of Culture in Animals*, Princeton 1980, about culture in animals and man.

⁷ Lévi-Strauss, cp. p. 13.

⁸ F. Barth, *Andres Liv - og vårt eget*, Oslo: Gyldendal, 1980, p. 32.

⁹ E. Brunswik.

¹⁰ R. Rosenthal & K. Frode.

¹¹After the War when E. Brunswik and myself were going to write the psychology volume for the *Encyclopedia of Unified Science*, even the mellow and sophisticated behaviourism of E. C. Tolman and Brunswik was too rigorous and antimentalististic for me. Our ways parted. Brunswik wrote a highly competent volume alone.