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COCTA, the Committee on Conceptual and Terminological Analysis of the Social Sciences, has two subcommittees, one of which is called SUBMET (Subgroup on Methodology). It is directed by Alberto MARRADI from the University of Firenze, Italy.

The following is taken from a conference of this subgroup, held in Jablonna, Poland, November 1988 under the topic: "Science and Philosophy".

Naive Epistemological Realism and the Superiority of Propositional Knowledge over Non-propositional Knowledge.

by **Alberto MARRADI**

1. Why ordinary language is less ambiguous than the social sciences' technical languages.

Almost every author reviewing a semantic domain at the beginning of a book (be it on functions, on anomie, on theory, or what have you) denounces the extreme variety of semantic uses of every single term. Whenever the semantic spread of a term in ordinary language and in a social scientific language or metalanguage are compared, the claim that the latter languages are clearer than the former is proved false.

Among the reasons for the semantic spread of technical terms in the social sciences, the thinness of their scientific communities is foremost. Little if any training in the established denotations and connotations of basic terms is provided. Moreover, some mechanisms are at work to enhance semantic confusion. A term enjoying a (more or less contingent) positive connotation is stretched far beyond its original acceptations, due to extra-scientific reasons (e.g. 'democracy', 'freedom', 'development') or endo-scientific ones (e.g. 'law', 'measurement', 'experiment' in the positivist era; 'hypothesis', 'verification', 'scale' in the neo-positivist/behaviorist era; 'falsification' in the recent Popper era).

Some terms may be stretched in order to embarrass an epistemological enemy (see the post-neopositivist rediscovery, and over-employment, of 'theory') or to claim originality (as when an intellectual proclaims that everything is 'power', or 'sign', or 'violence', etc.).

The above factors of semantic confusion are absent or less marked in ordinary language. Families and primary schools are still doing their job of inculturation young people into ordinary language. Subcultures identified by age and sex, occupation, leisure-time activity, etc. do use semantic stretching of old terms, as well as coining of new terms, and of course other symbolic means, in order to mark off their own "province of meaning". But most of those semantic changes only have an impact limited in space and time. Only few are captured by mass media,

and thence spilled over the local borders. However, they are limited to few and semantically close new meanings for few terms, which are central to the subculture, but relatively marginal to the general language.

Barring the (often short-term) adoption of few new terms or new acceptations as subculture markers, there is hardly a real interest in playing semantic stretching with ordinary language terms. Ideological wars (in politics, economics, culture, science, sport, etc.) are hardly fought within ordinary language; rather, each one tends to be fought within the boundaries of its peculiar province of meaning. And an intellectual knows he/she has nothing to gain in entering a milk shop and asking for "a coal flake"; he/she will get neither the desired pint of milk, nor a reputation for originality among milkmen and catering housewives.

In other words, common language is shielded from terminological confusion by the very elementary and essential nature of its function, and by the very number and heterogeneity of its users. Each one of them is a member of many provinces of meaning; in each of them he has different partners, friends, enemies, issues, battles, etc. The vast and dull meeting ground of ordinary language, by its very massive amorphousness, acts as an absorber of myriads of small semantic shocks, preserving in the long run only those new terms and new acceptations which really permit a better adaptation to a changing reality.

Summing up, the empirical finding of the superior stability and clarity of ordinary language should not surprise anybody, as there are excellent theoretical reasons for it.

2. Why social scientist and meta-scientists disdain empirical investigations into the state of their languages, blindly subscribing the thesis of its superior clarity.

As we remarked, most social scientist complain that this or that term in their own preferred domain are ambiguous, and many provide checklists or even classifications of current meanings. However, nobody (except the few members of COCTA, a committee of both ISA and IPSA) sums all those complaints up and declares that the technical languages of the social sciences are in a state of chaos.

Whatever their claimed epistemological sympathies, most of them uncritically subscribe the neo-positivist thesis of the superior clarity of scientific languages vis à vis ordinary language. Being obviously self-gratifying, such a thesis is placed beyond empirical check by the keenly status-oriented social-scientific community (an obvious case of "false consciousness"). Contrary evidence from one's own experience and from the literature is ignored. Attempts to generally document and assess the terminological chaos are either reacted against as "attempts to legislate on words", or snobbed.

The latter reaction is linked with a more general epistemological attitude, whereby attention devoted to any form of non-propositional knowledge (not only concepts and terms, but definitions, classifications, typologies, taxonomies) is considered futile, old fashioned, pre-scientific or even anti-scientific, or at best belonging

to philosophy rather than to science. What makes up a science, its stuff as well as its aim, is propositional knowledge: theories, explanations, laws, predictions, etc. It is precisely Homans' argument against Parsons, but it can be found, with little variations, in many other authors, both earlier and later.

In our opinion, the thesis of the superiority of knowledge in propositional form depends on two deep-seated epistemological assumptions, which most social scientists would reject – especially the former – if they were to confront them in explicit form: in order to duly circumscribe them I will call them 'categorical realism' and 'propositional decidablism'.

By 'categorical realism' I mean the assumption (die natürliche Einstellung) that our concepts faithfully portray referents (objects, events, "states of the world" – as Wittgenstein put in his early realist phase).

By 'propositional decidablism' I mean the assumption that the truth or falsity of statements about the world may be ascertained, at least asymptotically (hence, the propositions expressing those statements are "decidable").

If our concepts (including classes, types, taxa, etc.) do reflect objects/events/states-of-the-world, then of course we may take them for granted as non-problematic. This focusses attention away from concepts and toward theories, explanations, etc., which can be ascertained as true or false, and which will give us precious certain information on how objects behave, how events cause other events, how "states of the world" evolve.

On the other hand, a conventionalist epistemology argues that propositions may be submitted to empirical check (and hence decided as true or false) only within a many-layered framework of conventions, which makes their truth/falsity contingent upon the framework chosen. As a consequence, the attention is no more neurotically fixed only on theories, explanations, etc. (which at any rate will never give a definitively certain knowledge of the world) and may in part be devoted to the choice of concepts, terms, definitions, types, etc. as adequate as possible to the task of organizing in the most insightful, effective, parsimonious way our perceptions/ideas about reality. Renouncing the ideal of an absolutely certain knowledge of reality entails a growth of attention for our categorical apparatus, whose crucial mediating function is better recognized.

3. Is empirically corroborated propositional knowledge the real weapon in the battle between schools of thought in the social sciences?

If one looks critically at the current debates in social sciences, one is led to wonder whether the official emphasis on empirical checks, with the consequent emphasis on knowledge in propositional (i.e., decidable) form, is another (giant) instance of false consciousness. From several quarters it has already been remarked that replication of another scientists research is at most a rare exception, rather than the rule, in natural as well as in

social sciences. And on replication rested the claim to intersubjectivity – the comfortable, never fully explicated surrogate for good old objectivity in the official epistemological doctrine.

Further, somebody (see e.g. Alexander's paper on classics) is beginning to remark that in the clash between schools, arguments based on empirical checks only are a small fraction of the total array of weapons, covering the whole gamut of possibilities, from ontology to axiology, from the re-interpretation of classics to the assessment of (ir)relevance. And this is by no means surprising, as empirical arguments not only require technical background and hard labour, but are – contrary to official wisdom – far from conclusive: in order to check a theory against gathered empirical evidence, many and various methodological decisions are needed so that one can never claim the result of the check to depend on such decisions. This is exactly what Duhem said in 1906: only it is much more dramatic in the social sciences which face additional epistemological problems (the relevance of context, to mention just one).

Among the instruments that are currently being recognized as the actual weapons of scientific debate, one that has received unduly little attention so far is the aggressive marketing of one school's preferred concepts and associated terms. It is almost a tautology to suggest that the frequency of occurrence of terms like 'behaviour', 'stimulus', 'function', 'structure', 'system', 'action', 'negotiation', etc. in the literature is strictly dependent on the fortunes of the associated school of thought. It is almost a tautology, but how to reconcile it with the claim that concepts and terms have but secondary importance in the scientific debate?

The latter thesis, besides being legitimated by "categorical realism" and "propositional decidablism" – as we saw above – may owe its popularity to a sort of "cunning of Reason" – or "stage artifice", if you prefer. Whatever is the social scientists' official doctrine, they actually behave as if they knew full well that the real battle is about imposing their own concepts/terms. However, each player feels freer to make his own moves in the game that actually counts (imposing own concepts/terms) if the official wisdom declares that game to be nonexistent, and the game being played to be quite another one (providing empirical support to own theories/explanations).

Post script: those interested in the activities of the SUBMET group should contact Professor Alberto Mar-radi, Viale Duse 32, I-50137 FIRENZE; Italy.

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