ON THE RELEVANCE OF METAPHORS AND MODELS IN THE HISTORIOGRAPHY OF PSYCHOLOGY

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The aim of this contribution is twofold:

The first aim is to plea as convincingly as possible for an approach to the history of psychology which draws -at least in part- from the theories of metaphors, analogies, and models as proposed by philosophers of science in the recent past.

The second aim is to suggest that an approach *via* metaphors, analogies, and models implies a broader conception of the historical evolution of psychology than that usually implied by the historians of this discipline.

The paper first briefly considers some controversial issues relating to the *analytical unit* or *frame* operant in the discourse of the historian of psychology and, generally, in that of the historian of any science. The paper then attempts to define that analytical unit or frame corresponding to the approach *via* metaphors, analogies, and models, and to legitimize this definition on the basis of two examples pertaining to the history of psychology.

In the past fifteen years or so scholarship in the history of psychology has frequently become the subject matter of theoretical discussions and methodological controversies. The debates of both types were partly stimulated by what could roughly be called 'the rise of post-empiricistic philosophy of science', and partly by the elaboration and successful testing of innovative theories in the sociology of science which deal, e.g.,

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with the structure of social networks in science, or with the causes and conditions of the institutionalization of disciplines. These innovative sociological theories offered some new perspectives to the historian of psychology because of their obvious thematic similarity with topics of interest to specialists in the historical evolution of psychology.

But the debates also resulted, again in part, from a more or less clearly perceived dissatisfaction with the then prevailing ideas about the history of psychology which, at a closer examination, turned out to be problematic, pragmatically untenable, or just false for a number of reasons more or less distinctly spelled out.

Yet, what in retrospect appears to have been due either to some personal preference for a certain approach against another one and the need to justify this preference, or to the contingencies of research-biographies at the cross-road of mutually exclusive or mutually incompatible influences, or to some other collection of factors determining the role, the task, and the work of an historian of psychology, may be summarized and rephrased into one interesting problem. For there exists one issue underlying the theoretical and methodological debates, viz. the problem of the *analytical unit* or *analytical frame* which structurally determines the narrative of the historian of psychology, and of science in general.

Thus, the critique of the presentistic approach, e.g., challenges not only the belief into the linearity of scientific development, but also the assumption that the historian's narrative (1) will be in principle sufficiently dense, and hence empirically satisfactory only if it refers to the body of today's well confirmed knowledge as that unit or frame towards which the development of a science has irresistibly tended from its origin on ward. In other words, the proponents of the presentistic approach consider the link between an initial theory and the latest descendent of this theory to be sufficiently meaningful for an historical reconstruction, whereby the current state of the discipline (such as it reflects itself in textbooks or in other printed sources) is used as criterion for the elimination of historically irrelevant instances (for more on this, see ASH, 1983).

Similarly, the critics of the biographical approach have often pointed out that the (intellectual) life of a scientist is not sufficient as a base for an adequate account of the rise of a theory, a discipline, a school of thought, and what not. To put it differently, they have expressed doubts about the idea that the scientific biography of a 'founding father', a 'Promethean hero', a 'giant' on the shoulder of which later generations are said to stand, could serve as the main, or perhaps as the sole, historiographically relevant unit or frame around which to organize the narrative.

As can easily be shown, the controversy about the biographical approach in the history of psychology (and of science in general) runs parallel to the historians' debate about whether they should focus upon unique individuals as actors bringing about historical changes, or rather upon roles of individuals, institutions, networks of relations, etc., as those analytical units which correspond to the 'agents' of historical changes (cf. ELIAS, 1983, 35-59).

Another issue that was debated in the recent past concerns the role of internal (or rational) and external (or social) factors in the history of psychology (and of science in general). Of course, the internalist/externalist debate is located at a level different from the presentist/historicist controversies. For one can be an

internalist and nonetheless reject the presentistic approach, and one can be an externalist and still prefer the biographical standpoint. Yet, it is clear that the internalist/externalist controversy is centred on a pair of alternative questions dealing again with the analytical unit or frame: a) Does an historian who focusses on economic, political and other social factors, causes, conditions, etc., and only on them, succeed at constructing an acceptable narrative of the history of psychology (or of any other science), and b) does an historian who focusses on the internal, rational dynamics of hypothesis construction and hypothesis verification/falsification, and only upon it, preclude the possibility of providing a viable narrative of the history of psychology (or of any other science)?.

Now, since the internalist/externalist controversies have already been amply documented in the literature, it may suffice to note here that there are innumerable cases where either an exclusively internalist or an exclusively externalist standpoint has led to insuperable difficulties, and that several authors have therefore suggested that these standpoints "are complementary and not contradictory, and that any so-called conflicts between them are pseudo-conflicts" (Mary HESSE, 1980, 29; see also BARNES, 1974, 99-124, and MIKULINSKIJ, 1977).

It seems that the theoretical and methodological debates have if not solved all the problems at stake, at least enriched the landscape of research strategies, types of arguments and conceptions in the historiography of psychology, and that these debates have led to an increased awareness of the difficulties inherent in this field of research. All in all, the historians of psychology are today better off than they were some fifteen years ago in responding to Joseph NEEDHAM's statement that "A scientific worker is necessarily the child of his time and the inheritor of the thought of many generations. But the study of his environment and its conditioning power may be carried on from more than one point of view" (NEEDHAM, 1943, 41).

In the remainder of this paper, I shall comment upon only one of these aspects of the historiography of psychology that have not yet been enough clarified from the theoretical and/or methodological standpoint.

Historians of psychology usually study the evolution of a single (psychological) theory, or of a collection of interrelated (psychological) theories, or even of one or several branches of this discipline. Similarly, an historian of physics investigates this or that physical theory, or this or that episode of the history of physics, etc.

In contradistinction, the historian of hygiene, of nutrition research, or criminology, or any other such 'mixed entity' cannot avoid considering and analyzing from the outset more or less complex conglomerates of theories that stem from various fields of scientific activity (in the case of hygiene, from medicine, chemistry, statistics, architecture, etc.).

Finally, the historian of science in general, though more abstractly than the historian of individual disciplines, tries to find a solution to problems concerning the evolution of the sciences within a given period, or concerning the structure, the laws and the conditions of scientific development as such.

A division of labor say, that between the historian of psychology and the historian of botany (or any other individual discipline) is pragmatically justifiable and unproblematic, for it is practically impossible to master more than one or two disciplines (particularly

if these disciplines have a long past). But such a division of labor too often goes hand in hand with a narrowly formulated conception of the development of individual disciplines. In other words, if the division of labor is to be understood as a principle according to which historical processes which occured in disciplines different from that under scrutiny are not relevant to the study of the latter, merely because they do not belong to it, then a pragmatically sound idea about the division of labor cannot be justified in the light of massive empirical evidence. This evidence confirms the tremendously important role of the patterns of exchange and communication (input-output-flow) between the sciences in their historical evolution. Thus, the historiography of individual (and seemingly 'unmixed') sciences like psychology or botany remains a questionable and precarious endeavor if (and as long as) a dogmatic definition of the range or field of a discipline serves to define the analytical frame or unit.

To illustrate this point, I shall mention the more or less mystifying propositions of many historians of psychology about the shift from the metaphysical (i.e. speculative, unscientific) to the experimental (i.e. hard, scientific, serious, etc.) viewpoint in psychology -a shift which is said to have taken place in the nineteenth century and whose first embodiment was the foundation of the psychological laboratory at Leipzig in 1879 (against this view see METRAUX, 1980; and GEUTER, 1983). Such mystifying propositions would not have been made, had the historians of psychology had taken into consideration the publications in the medical, physical, physiological, and anthropological sciences between 1750 and 1850.

Against this bias of a discipline-bound approach, the study of 'mixed sciences' (see above) and of science in general may serve as a palliative. But then, it is no longer such and such a discipline (with its naive historical self-representation), or such and such a theory conventionally labeled 'psychological' or 'botanical' or what not, etc., which offers itself as unit or frame around which to organize the narrative, but rather a *theoretically more or less determined domain,* involving possibly one, but possibly also many disciplines. Such a theoretically more or less determined domain is *sense perception* upon which converge theories of physics, chemistry, biology, physiology, neurology, psychology and sociology.

In order to substantiate the argument for a conception or approach which refers to theoretically more or less determined domains as analytical units or frames, I shall give two examples and then draw some conclusions for the historiography of psychology.

The first example is taken from the history of research on the nature of sense perception, and the second from that of memory research. Both examples clearly belong to the history of psychology. But their analysis makes necessary the study of events which took place (simultaneously or not) in several other sciences.

Due to a number of primarily social reasons, olfactory perception was given a great deal of scientific attention from 1780 on (on the social history of the role of olfactory perception, see CORBIN, 1982). The rate of scientific production concerning the mechanism of olfactory perception grew rapidly up to 1820 and then remained more or less steady up to the last decades of the nineteenth century. The contributions of

this domain of theorizing and research came from the medical profession as well as from chemistry, physiology, psychology, and hygiene.

Two events of importance to the study of the history of research into the mechanism of olfactory perception took place shortly before 1800. On the one hand, BOERHAAVE's theory of aroma was rejected and replaced by a thoroughly empirical theory of odors; on the other hand, the doctrine of the nervous fluid, which dominated the physiology of the nervous system up to the last quarter of the eighteenth century, was definitely overthrown by GALVANI's famous experiment of 1791, which legitimized an electrical definition of the *vis nervosa* (see JEANNEROD, 1983, 11-34).

According to BOERHAAVE, a vegetable aroma is to be understood as a principle, i.e. as a substance which is part of a body (the plant) and whose ontological status is quite similar to that of the phlogiston. Against this doctrine which turned out to be useless for the classification of odors as well as empirically weak, since there also exist non-vegetable aromas and odors, FOURCROY and other chemists hypothesized that odors were but molecules of bodies which dissolved themselves in the air and which, when reaching the nose, called out a specific sensation (see FOURCROY, 1798, 238-239). One of the crucial experiments that supported the new theory was that of BERTHOLET who put a piece of camphor into the top of a barometric column completely filled with mercury and who observed that the camphor dissolved itself into an odorous gas, the pressure of which pulled down the mercury in the barometer.

The shift to the chemical theory of odors was extensively commented in the literature of psychology, physiology, medicine, and hygiene. It had, indeed, several consequences for the theory of olfactory (and gustatory) perception. The following remarks will make this clear.

The volatile principle aroma of BOERHAAVE was conceived as homogeneous (see FOURCROY, *op. cit.*, 235 (2). It therefore was expected to permit to explain the phenomenal variety of vegetable odors by reduction to one -or at worst to a very few- substances. Now, Lecat's theory of the mechanism of olfactory perception, published prior to the new chemical conception, fits well into the substantialist standpoint of BOERHAAVE and the pre-1780 chemists. For it suggests that the organism is able to discriminate between various odors because the aromatic particles (which, for LECAT, are essentially vaporous) touch the receptors which, in turn, react to the geometrical properties of these particles. The mechanism of olfactory perception is thus identical with that of gustatory perception (aromatic particles are mediated by fluids); and gustatory perception, in turn, is a derivative of haptic perception (see LECAT, 1744, 25-26 (3)) Or, to put it differently, if the phenomenal variety of odors can be reduced to one or very few principles, then this variety reduces itself to some geometric properties discriminated by some haptic sense. Thus, LECAT's theory is not chemical at all, but mechanical; and even if his explanation of olfactory-gustatory-haptic mechanisms may owe quite a bit to a revival of Democritean atomism, it accords theoretically with the substantialist doctrine of odors.

The refutation of the Boerhaavean doctrine forced the theorists of olfactory perception to admit as many different chemical substances as there are odors, and to redefine the predicate 'aromatic' in terms of a disposition to "act upon the organ of

olfaction" (FOURCROY, *op. cit.,* 237). From this is followed that the stimulation of the receptor had to be chemical rather than mechanical. At the same time, due to the rejection of the doctrine of the nervous fluid, one had to find a new, non-mechanical conception of the nervous processes concerning the information transfer along the afferent tract. However, it took some time before the theory of the *vis nervosa* as electricity became an integral part of the theory-formation in psychology. For had it been fully appreciated and correctly applied in the construction of workable models for the explanation of the mechanisms of sensory perception, a leading author of the early nineteenth century could not have maintained that olfactory sensations produced weaker impressions than visual ones *because* the former were chemical, whereas the latter were physical (see CLOQUET, 1815, 174).

This first example indicated that the study of olfactory perception changed its whole outlook around 1800, mainly under the influence of non-psychological disciplines, and that discarding the developments in chemistry and neurology must lead to an incomplete or even wrong portrait of an episode of early nineteenth century psychology, which, to be sure, was already quite empirical.

The second example is of a different type than the first one. The history of memory research can be seen as a nearly unending chain of models designed to explain the mechanisms of memorization, information storage, information retrieval, etc. All these models, whether successful or not, i.e. whether empirically and/or experimentally supported, belong instrinsically to the history of psychology, though their origin is more often than not to be found either in disciplines which are not psychological (the label 'psychological' is understood here it is accepted by the majority of today's psychologists and historians of psychology) or in the extra-scientific environment.

Thus, PLATO's model of the wax tablet has its origin in the everyday practice of engraving and writing; HOOKE's model of memory is drawn from seventeenth century atomism (see HOOKE, 1705; on HOOKE's theory of memory, see SINGER, 1976); GUYAU's phonogram model rests on analogies with a technically sophisticated invention (see GUYAU, 1880), and today's computer models of memory are likely to turn out to be reifications of the memory model of computers.

The constituents of the psychologically relevant models mentioned here may be listed as follows:

OLFACTORY PERCEPTION:

1.- Odors as principles (identical/homogeneous substances) with geometrical properties (chemistry/atomism). → olfactory/gustatory perception perception = haptic perception

(psychology of sense perception)

nervous system as mechanical system

2.- Odors as molecules of a certain chemical structure each (chemistry) → olfactory perception as chemical action upon the organism

(psychology of sense perception)

nervous system as complex bio-system

MEMORY

3.- Wax tablet engraving (everyday practice) → information storage (psychology of memory)

4.- Atoms as bodies (atomism) → physical engram (psychology of memory)

5.- Phonogram (technical invention) → information storage (psychology of memory)

6.- Memory (psychology) → computer (technical invention) → memory (psychology of memory)

Such a list may, indeed, be indicative of the interrelations that exist between various disciplines and psychology throughout the history of the latter. Furthermore, it shows that there are connections between psychology and the extra-scientific environment. But this and similar lists have merely a diagnostic value. The task is therefore to exploit the possibilities inherent in a consideration of the roles of models for the historical narrative, whereby it is assumed that such models benefit from the realization of the interrelations existing between the sciences.

Models are to be defined dynamically, as the product of the metaphoric usage of words or terms on the basis of a perceived, imagined, deliberately established, etc., analogy. According to Mary HESSE, the use of metaphors implies descriptions in literal language of two separate situations or referents, which are called 'primary system' and 'secondary system'. A metaphoric use of language in scientific theories consists in tranferring a word or a collection of words normally employed in the description of a given system referring to a situation or a referent used in the description of another

system in order to explain some unknown mechanism in the situation described by this system. The transfer of words or terms from one system to the other one modifies the literal description and changes it into a metaphoric one. "In a scientific theory the primary system is the domain of the explanandum, describable in observation language; and the secondary system, described either in observation language or in the language of a familiar theory, from which the model is taken..."(HESSE, *op. cit.,* 112). Thus, the primary system is the above examples is that of the psychology of olfactory perception or that of the psychology of memory (the domain of the explanandum), whereas the secondary system is that of a description of everyday practices, technical achievements, and familiar theories ('familiar' is, of course, not necessarily to be equated with 'true' or 'verified' or something like that) pertaining to a whole range of situations and referents (atoms, odors, molecules, geometry, wax tablets, phonograms, computers, organisms, etc.).

An interesting point made by Mary HESSE is that the metaphoric usage as means to generate a model goes hand in hand with an interaction between the primary and the secondary system, so much so that "the two systems are seen as more like each other (than before)...and seem to adapt to one another, even to the point of invalidating their original literal descriptions if these are understood in the new, post-metaphoric sense" (HESSE, *op. cit.,* 115). This is likely to be the case for the computer metaphor of memory and the memory metaphor of the computer.

Now, though the conditions that must be fulfilled so that metaphors become explanatory are far from being determined up to today, the already obtained results of the analysis of the dynamics of model construction suggest that a purely discipline-bound conception of the history of individual sciences (including psychology) cannot stand up to the test. There are therefore enough arguments in support of the plausibility of the conception outlined here (which, to be sure, is not al all meant to be exclusive).

For an historical narrative focussing upon theoretically more or less determined domains as analytical unit or frame some further consequences ought to be drawn from the theory of metaphors and models. Thus, it would be important to stablish how disciplines interact with one another over time, and to study more specifically the patterns of interaction of scientists, the channels along which knowledge is propagated within a science and among the sciences, the institutional boundaries and the communication networks of the disciplines. This would permit the historian to grasp the reasons why a certain metaphor was used at a certain time, by whom it was used, and for what purposes it was used; this would, in turn, enable the historian to write an effective narrative of the scientific models and to take into account the diachronic specificities.

Furthermore, it would be important to see how everyday understanding of the world (or of regions thereof) as well as everyday practices, technical devices, etc., influence the rise of metaphors and, eventually, the development of successful models (in psychology and elsewhere).

It is therefore clear, that a conception of the history of psychology (or of any other science) *via* metaphors, analogies, and models is far from restricting itself to merely internal factors of scientific development.

NOTES

(1) I use the term 'narrative' to denote any kind of historiographic discourse, whether *'verstehend'* or *'erklärend'*, which tells the reader what happened and how it happened to happen at a certain time or within a certain period of time. The term as used here is suggested by the recent development of a theory of the narrative, to which Jean-Pierre FAYE has greatly contributed.

(2) In speaking of his own hypotheses, FOURCROY (1798, 234-245) says: "Ainsi l'*esprit recteur* dans toutes les hypothèses proposées jusqu'ici, n'étoit point un principe identique, un genre de principe végétal homogène ou uniforme...".

(3) On the mechanism of stimulation of the olfactory and gustatory receptor, LECAT (1744, 25-26) writes: "Quand les Sels qui sont introduits dans ces pores de l'organe du Goût sont entiers, presque seuls, & non mitigés par quelque alliage, alors ces Sels sont des espèces d'épées qui font dans l'organe des impressions violentes, & on les apelle *désagréables*, quand cette violence révolte la substance sensitive...Quand les Sels sont envelopés par les parties huileuses, ou sulphureuses, de façon que leur tranchant est estièrement caché, que leur pointes mêmes embarassées ne peuvent qu'ébranler légerement les houpes nerveuses, alors cet ébranlement léger fait une saveur *douce*, & elle est *agréable*...".

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