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Introduction

Psychology and Psychologies: which Epistemology?

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According to its etymology, psychology is the discipline that investigates the psyche. However, the meaning of the term 'psyche' is not constant across time. Sometimes it has indicated a kind of vital breath, sometimes the soul, and sometimes the series of mental functions. Analogously, psychology has identified its object, its method and the goals of its analysis differently over time. The problem of the unity of psychology arose a relatively few decades after the birth of the science (Driesch 1925; Bühler 1927), and persists to the present day (see, for example, Royce 1970; Staats 1983; Sternberg 2004; Henriques 2004, 2005). The psyche is the object of enquiry in very different scientific disciplines, from neuroscience and neuropsychiatry to behaviourist and cognitive psychology, to historical-existential and psychodynamic therapies. Each of these disciplines studies the mind on a different level of analysis: neurophysiological, functional or narrative. The fragmentation of the object of psychology is mirrored in the variable popular perception of the psychologist: sometimes the psychologist is the neuroscientist, sometimes the psychiatrist, sometimes the psychoanalyst, sometimes the theoretical psychologist or sometimes the psychotherapist whose training may well come from outside of the discipline.

Therefore the issue of pluralism in psychology is unavoidable. Various research paradigms and schools produce knowledge of a different and not immediately compatible kind. This is problematic: the physical sciences, on the contrary, present, at least in principle, cumulative knowledge. The question about the unity of psychology requires us to attend to the kind of pluralism we see in the field. Is this a pluralism of levels of description of the object of psychology, or of methods of enquiry? Or is the pluralism deeper, indicating the existence of different objects of study? If psychology is a human science, and if human nature is an open (at the same time biological, social and cultural) phenomenon, a pluralist account in psychology may be appropriate in virtue of the absence of any possible reduction of the human being to mere natural fact.

The present issue of *Humana.Mente* intends to investigate the problem of the unity of psychology with reference to these questions. These questions presuppose an autonomous domain of the philosophy of psychology within the philosophy of science. Since philosophy of science is also referred to as epistemology – indeed, according to a "continental" rather than Anglophone terminology - we may consequently label such a domain with the name of *epistemology of psychology*. Thus, the epistemology of psychology may be defined as that part of applied epistemology which studies the scientific status of the psychological disciplines.[1]

The choice of the name *epistemology of psychology* distinguishes the epistemology of psychology from two related disciplines, namely, classical philosophy of mind and the more recent philosophy of psychology. In fact, it might be argued that the epistemology of psychology is only a part of the philosophy of mind. The argument might proceed as follows: (i) philosophy is traditionally divided into ontology, epistemology and moral philosophy; (ii) the

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[1] Referring to psychology as a plurality is mandatory in this case since the unity of psychology might be only a possible outcome of the investigation within epistemology of psychology, therefore cannot be assumed a priori.

same distinction can also be maintained with respect to philosophy of mind, which should therefore analyse ontological, epistemological and moral issues connected to the concept of mind;[2] (iii) the mind is the object of investigation of psychology; (iv) the epistemology of psychology is concerned with the epistemological questions of psychology, that is, with the epistemological questions concerning the object of study of psychology; therefore, (iv) epistemology of psychology is concerned with the epistemological issues of the concept of mind, which is just a part of the philosophy of mind.

The argument is sound, but irrelevant, for both historical and theoretical reasons. Broadly speaking, philosophy of mind studies the nature of the mind and its relation to the body. In the last decades of the XXth century, such a general object of enquiry has been refined and investigated through the observation that there is a wide range of terms by means of which we characterise people's minds - e.g., "belief," "desire," "intention," "hope," "fear," only to mention a few. Those terms are related to each other in complex ways, and they form a domain of knowledge that has been called *folk*, or *common sense* psychology. One of the main topics of debate within philosophy of mind has concerned how to relate the well-defined concepts of folk psychology to scientific psychology.[3] Disregarding the particular position one might favour within the debate, its participants have assumed that the central problem concerns how the well-defined concepts of folk psychology are related to the concepts of the mind provided by natural sciences. That is, the participants to the debate concentrated more on the ontological status of the mental with respect to the physical world than on epistemological questions about how we can ground our knowledge of the mental. Therefore, an important part of the philosophy of mind is not concerned with the same issues that the epistemology of psychology addresses.

Issues about how to ground psychological knowledge on a firm basis have nonetheless been raised in the philosophy of mind since its origin until nowadays. Just as a few examples, consider Descartes' discussion about the intuition of clear and distinct ideas, the debate between theory and simulation theorists about the attribution of mental states to the others, or the contemporary interest in first-person authority. Hence, one might still argue that the epistemology of psychology should fall within this part of the philosophy of mind concerned with epistemological issues about the mind. However, note that in this context the mental is again considered a fairly well-established domain to be investigated by philosophers. What makes this possible is the appeal to a kind of psychological knowledge that is often more or less clearly introspective in its nature; alternatively, psychological knowledge is strongly connected to its linguistic vehicle – i.e., propositional attitudes – so that the epistemological analysis about the mind is assimilated to the investigation of our linguistic practices about mental terms. In any case, it has been implicitly assumed that we do possess a definite concept of the mind, which is shaped by common sense.

[2] I am in debt to Richard Kitchener for this proposal regarding the analysis of the philosophy of psychology.

[3] On the one hand, some scholars have argued that folk psychology identifies a basic and autonomous domain, which at the same time cannot be translated in different terms without distorting its nature, and it is essential to provide a complete description of what is in the world. On the other hand, it has been claimed that folk psychology is not autonomous from scientific psychology. Such a thesis has been held by claiming either that the language of folk psychology can be reduced to the language of natural science, or even that it cannot. In the latter case, naturalistic accounts of folk psychology split between those that still accept the pragmatic utility of the mental vocabulary and the more extremists suggesting that it will in the end be eliminated.

However, it is questionable whether common sense provides well-defined concepts of the mental. Indeed, as studies in cultural anthropology and comparative literature show, the understanding of the mind in the common sense framework is historically and culturally dependent. According to Lillard (1998), for instance, the concepts of folk psychology vary with respect to several dimensions across cultures. As another example, Jaynes (1977) analysed the language in the *Iliad* and found that the concepts of mind and consciousness as Homer's heroes express them are radically different from those used nowadays. Furthermore, the common sense vocabulary predates scientific discoveries, and its ontology changes according to the changes in the shared view of reality. With respect to the case of psychology, for example, Freud's psychoanalysis, which was developed to scientifically investigate the origins of hysteria, later became so popular that originally psychoanalytic terms are nowadays part of our common way to characterize the mind. Therefore, it seems that mental concepts undergo gradual change in space and time. Thus, although philosophy of mind exploits a conception of the mental without specifying the limits of its theoretical framework, we should doubt that mental concepts are univocally defined within the common sense.

In talking about the mental, it is very important always to specify the theoretical or methodological framework presupposed by our discourse, for these frameworks define the object of investigation. Thus, the epistemology of psychology addresses the foundation of psychological knowledge by contextualising any analysis with respect to the particular discipline from which it emerges. This does not, of course, mean that we must in such discourse accept every psychological discipline as authoritative: that burden of the proof must be born by each discipline.

For the reasons expressed above, it seems reductive to talk about the epistemology of psychology as a particular chapter of the philosophy of mind. Pursuit of the epistemology of psychology assumes the plurality of psychological disciplines and that the definition of the proper object of psychology - the mind - creates the problem of the unity of psychology. The ontological questions regarding the nature of the mind and mental concepts are strictly connected to the epistemological question about the status of the concepts of psychology as they appear in the psychological disciplines.

I want also to contrast the epistemology of psychology with respect to the emerging field known as the philosophy of psychology. Authors employing this term are numerous; they generally share an interest in explaining the properties of the mental through a naturalistic account of the human psyche, provided by the cognitive sciences. Hence, the philosophy of psychology is "an approach which focuses on problems which are raised for *philosophy* by the results and methods of psychology" (Botterill and Carruthers, p. ix), and it is concerned "with mind and cognition" (Wilson 2005), or "with the nature and mechanisms of cognition, rather than with the metaphysics and epistemology of the mind" (Bermúdez 2005, p. 15).[4]

Despite the similarity in the names, the epistemology and the philosophy of psychology differ considerably in their scope. Indeed, the authors who introduced the latter restrict the term *psychology* either to *cognitive* psychology, or more in general to that set of disciplines – such as the very cognitive psychology, as well as neuroscience, computer science, cognitive anthropology, and psycholinguistics – that form the field of cognitive sciences. The epistemology of psychology aims at a wider target, since, at least *prima facie*, every psychological discipline, from neuroscience and neuropsychiatry to behaviourist and cognitive psychology, to historical-existential and psychodynamic therapies falls within its scope.

A second important distinction between the epistemology and the philosophy of psychology is that the former, and not the latter is committed to an epistemological enquiry.

[4] See also Block (1980), O' Donohue and Kitchener (1996), Thagard (2007), Symons and Calvo (2009).

Indeed, the philosophy of psychology presupposes a naturalistic account according to which a correct and complete explanation of the human mind will come from cognitive sciences. Therefore, in the philosophy of psychology there is a natural inclination to naturalized epistemology (Quine 1969), in which empirical enquiry about how we reason substitutes for epistemological questions about the limits and the constraints on human knowledge.

This substitution is to ignore rather than to solve the problems concerning the warrant of human knowledge of the mind. Moreover, it presupposes a kind of scientific eliminativism about the mental, which portends danger for the very existence of the philosophy of psychology. In fact, naturalized epistemology seems to reduce the philosophy of psychology to a critique of empirical methodology in cognitive science. In that case, the psychologist, the neuroscientist or the linguist who reflectively consider the methods of their discipline and reason about the possible interpretation of their experiments, would to do the same work that the philosopher of psychology. This fact is highlighted by Carlo Gabbani:

> A serious difficulty both for a genuine philosophy of psychology and for genuine explanations of our conscious experience would be created by the conjunction of a fully naturalized epistemology with the so called eliminative revisionism. […] In the case of such a 'monolithic' version of this option (that is in the case where science replaces the personal-level account or produce an unfair 'co-evolution' of it), I think we would have a situation in which the philosophy of psychology would result in being no more than the more general chapter of experimental psychology, without a peculiar identity and autonomous problems. (Gabbani, 2006, pp. 5-6)

The epistemology of psychology runs no such risk because of its greater generality. Indeed, as noted above, its existence is granted by the pluralism of psychological disciplines. Nonetheless, to pursue this inquiry is to acknowledge that cognitive science takes the psychology of individuals broadly as how it was defined at the end of the XXth century in Western culture. Therefore, even though the view of the human mind given by the cognitive sciences is the result of an accurate scientific enquiry, it is still the outcome of a contingent historical situation. This suggests that the philosophy of psychology should pay close attention to the many foundational assumptions in the cognitive sciences that are presupposed but unexpressed.

This discussion of the distinction between the philosophy both of mind and psychology, on the one hand, and the epistemology of psychology, on the other, might be summed up in the motto: *there is no philosophy of psychology without a philosophy of mind, and there is no philosophy of mind without considering ontological, epistemological, methodological and moral issues of the concept of mind*. A philosophy of psychology may be built only by considering all these kinds of questions. In fact, the epistemology of psychology considers the whole range of psychological disciplines because they, as a plurality, address these questions. And recognizing further dimensions in the definition of the object of the psychology forces one to make explicit its fundamental assumptions and to evaluate the alternatives.

I turn now to the problems raised by the epistemology of psychology. As a branch of epistemology, it raises theoretical issues both in general and applied epistemology. The problems of applied epistemology concern the definition of the object, and of the goals, of psychology. Should psychology seek universal invariants in the psyche, apart from the differences in the historical-cultural context, in the ethnic group, in the genre and in the age? Or is psychology rather a science of the individual, aiming at particular characteristics that might vary over time and culture? In regard to the goals of psychology, the debate is about the opposition between the theoretical and the practice-therapeutic nature of psychological knowledge. Many scientific accounts of the mind have therapeutic implications; and every

therapeutic practice presupposes, at least implicitly, a theoretical framework for its analysis. The connection between these two aspects and the need to distinguish normal development and psychic well-being from pathology entail that even theoretical psychology raises deep ethical questions.

For what concerns general epistemology, the pluralism of psychology seems to require an epistemological model different from that appropriate to the other natural sciences (Civita 2003). Such a model should account for the sense of both synchronic and diachronic change in psychology. Hence, it has to allow the evaluation and the comparison between different accounts, at the same time preventing the statement of insuperable antinomies based on different methodologies (Marhaba 1976). But it should also explain the sense in which the scientific enterprise increases the explanatory reach of psychological research. On a more abstract level, an epistemological model should make explicit the set of criteria against which the success of a paradigm is to be assessed. Here again psychology is a special case. The intimacy of psychology with our lives locates the ethics of science at the centre of our concerns when we consider psychology. Finally, the epistemological enquiry in psychology may be the starting point for the development of new, more general epistemological models which may also be applicable to other scientific disciplines.

The contents of this issue of *Humana.Mente* reflect the nature of the epistemology of psychology. In *Epistemic Preliminaries: Normative Priorities and Neuropsychological Kinds*, Jennifer Mundale opens the inquiry by identifying two important problems for the epistemology of psychology. In the first part of this essay, she points out that our theories about human psychology concern objects of different domains: justified beliefs (epistemic norms), mental health (clinical norms), heuristic strategies of thinking (cognitive norms), right actions (ethical norms), and so on. Therefore, "there is surely the likelihood, if not inevitability, that normative conflicts will arise, and it is puzzling to know how to resolve them" (p. 3). This poses the problem of how to judge, and of how to resolve, conflicts; and that introduces the further problem of fixing the framework with respect to which one adjudicates conflicts. In the second part of the essay, Mundale notes that many philosophical arguments (e.g., multiple realisability and multiple functionality arguments) rely on unexamined taxonomic assumptions about the psychological and neuroscientific domains. Therefore, "in the absence of some preliminary classificatory considerations, arguments that depend upon them are nothing more than programmatic guesses" (p. 4). Mundale opposes to such arguments the accurate work that is being conducted in neuroscience to empirically correlate "neural activations and psychological function, flashing out the nomological bridges between the two disciplines" (p. 7). Although research in this field is but to get to the end, the issue of creating a proper taxonomy for physical states, as well as the problem of correlating physical to mental types, are reasonably empirical matters, therefore, to be investigated with the best scientific tools we have at our disposal.

In discussing the first problem, Mundale seems concerned to show how physical levels are autonomous and do not immediately need to surrender to higher levels of explanation in the case of a conflict. However, her argument reaches further. As she notes, "if conflict is inevitable among those [e.g., clinical and ethical] assumptions, the criticism is more telling if placed within the defense of one's larger, normative hierarchies or coordination" (p. 4). That is, comparison between conflicting norms cannot be solved without situating it in the wider context of human (epistemic) rationality.[5] This is what the epistemology of psychology demands.

[5] Mundale explicitly points out that: "If we approach normative conflict, does this interpretation not commit us, at the outset, to the precedence of traditional, epistemological norms? Perhaps even to

Indeed, the epistemologist of psychology does not propose particular solutions to the conflict of norms, but rather proposes that, when a conflict arises, appealing neither to empirical results nor to a priori understanding of mind and subjectivity is sufficient unless the criteria of resolution of the conflict are spelled out. And the problem for psychology is that the definition of those criteria is a part of its object of investigation. Therefore, the problems that Mundale indicates are not "conceptually prior to the satisfactory treatment of various issues in the epistemology of psychology" (p. 1). Indeed, those problems precisely detect the domain of the epistemology of psychology.

The next group of essays is concerned with issues about the epistemology of psychology in cognitive sciences. In *Scientific Psychology: Should We Bury It or Praise It?*, Howard Gardner claims that William James' aspiration for an integrated view of psychology has been abandoned. Indeed, many fields that were once branches of psychology have already been assimilated by other sciences: for example, psychophysics has been assimilated by computer engineering, and comparative psychology has by now entered ethology. Furthermore, in the last years other parts of psychology are gaining the status of independent sciences. This is the case of neuroscience as well as cognitive science. Finally, other fields of psychology, such as social psychology, developmental psychology, and clinical psychology, which "are less 'at risk' of immediate absorption" (p. 16) by a scientific discipline, are nonetheless separating from psychology "to find their way into a general cultural discipline - including sociology, anthropology, and social psychology" (p. 16).

Despite this fragmented view, Gardner's evaluation is anything but pessimistic. Indeed, he argues, psychology achieved important insights in the past; it still contributes to other scientific domains of research; and finally it keeps a core of central notions that are strictly specific of its analysis, and that will hardly be "cannibalized" by other sciences. Gardner refers here to what he calls the "person-centered quartet" formed by the concepts of personality, self, will, and consciousness, which, although they "are clearly central in any delineation of the field" (p. 17), also cause embarrassment to psychologists. He argues that "the study of self or personality is at once a problem of psychology and the home ground of literature" (p. 18). Although he does not suggest a specific form of collaboration between the two disciplines, he acknowledges that psychology is strongly committed to a set of constructs (i) that are resistant to a direct materialist reduction, (ii) that are at the core of human nature, and (iii) that suggest the narrative nature of human life.

Matthew Broome and Lisa Bortolotti agree. In *Mental Illness as Mental: in Defence of Psychological Realism*, they defend psychological realism about the entities (e.g., mental disorders) used in psychiatric explanation against instrumental, eliminative and reductionist views. They support this theoretical claim with two empirical examples, which connect the practice of psychiatry with philosophical investigation and show that psychiatric explanation cannot get along without a genuinely mental level of explanation. In particular, with respect to delusions, Broome and Bortolotti apply Moran's (2001) concept of authorship to show that "the conception of delusions as mental disorders or pathological beliefs relies on the analysis of the reason-relations between the subject's beliefs and on attributions of self-knowledge and rationality" (p. 35). In the case of personality disorders, they report emerging literature according to which neuropsychological deficits "may be linked to the onset of psychosis and perhaps an increasing reliance on external vehicles of cognition" (p. 37). They note that this view about personality disorders matches well with the philosophical thesis of externalism of

recognize potential normative conflict as a subject worthy of attention is to privilege epistemic norms" (pp. 3-4).

the vehicle of mental content. Therefore, they conclude that "for biological psychiatry to have any validity, and to be anything more than neuroscience, the main object of study needs to remain the person. The normal and the abnormal themselves are not properties of the brain" (p. 38). That is, there is a normative dimension of the mind that is both relevant for the personality and impossible to reduce to the non-normative.

Bruce Thyer's article, *Epistemology: a Behavior Analytic Perspective*, clarifies the epistemological position of behavioural analysis by highlighting both its limits and strengths. Behaviourism, he argues, is limited because it restricts itself to "the analysis of behavior-environment interactions" (p. 47), where behaviour is investigated by focusing on single-cases, and it has to be intended as "everything that a person does, overt behavior as well as everything that occurs within the skin, phenomena such as feelings, thoughts, dreams, hallucinations, etc." (p. 49).[6] Furthermore, he argues, behaviourism does not address philosophical questions concerning the nature of knowledge and absolute truth, leaving "large segments of the metaphysical domains of philosophy excluded from serious consideration" (p. 59). Instead, behaviourism provides practical solutions to those questions. It is, he suggests, simply concerned with the aim to "develop satisfactory natural or physicalist explanations for supposedly non-material phenomena" (p. 51). Therefore, it assumes a pragmatic attitude to assess the validity of its results: "to the extent one can effectively predict and control behavior, one has arrived at a limited but truthful understanding of functional relationships" (p. 60). Furthermore, he argues, it dismisses every ontological commitment, such as physicalism, by relying on an economy principle for rational explanations and for the postulation of theoretical entities. Therefore, he concludes, behaviourism is a *non-reductionistic* science since it does not require reducing behavioural explanations to more basic physical levels.

Thyer defends a methodological against an ontological view of behaviourism. But a tension remains between an instrumentalist and pragmatist view of behaviourism, on the one hand, and a more radical eliminative view on the other. That is, it is hard to resolve the merely programmatic aim of substituting physicalist for mental explanations with the view that the traditional questions about knowledge and truth are "seen as unresolvable, and thereby dismissed from serious consideration as pseudoproblems" (p. 60).[7] If behaviourism is not concerned with ontology, it has nothing to do with these issues. Otherwise, to the extent that behavioural explanations are sufficient for the purpose of explanation of sensible cases, behaviourism really advances an ontological position.

Thyer's paper marks a shift in the issue from modern to post-modern psychology. In *Generalizing Through Conditional Analysis: Systemic Causality in the World of Eternal Becoming*, Zach Beckstead, Kenneth R. Cabell, and Jaan Valsiner criticise modern psychology, which subordinates the uniqueness of the situation of every subject to the scientific need of discovering general laws. They argue instead that the model of scientific laws that psychology borrows from natural science is too narrow to account for the complexity of human behaviour because it considers variables independently. Instead, the authors claim that a new conception of causality is needed to reconcile the particular and the general aspects of human behaviour. They find a clue to this conception in Kurt Lewin's work. Lewin stressed that the concrete situation and notions of interdependency and interrelationships are fundamentally constitutive of objects. Using his field theory, Lewin analysed human behaviour as a function of both the person and the environment. Beckstead and colleagues take Lewin's example to

[6] Thyer notes that the received view of Skinner's behaviourism as concerned only with overt behaviour is "an unfortunately widespread misconception" (p. 49).

[7] A similar commitment to ontological claims is also visible, for example, in Moore's (2008) quoted synthesis of behaviourism (pp. 59-60).

argue for a dynamic model of human behaviour, according to which "phenomena are qualitatively organized by the whole system they are embedded within" (p. 72), and generalization is based on the use of systemically dependent rather than separate variables. Therefore, they introduce the concepts of systemic causality and catalysis. In particular, "catalysis is the study the conditions that operate within open, intransitive, and dynamic systems that enable a particular outcome to be produced - *while preserving the functioning of the producing system*" (p. 73). According to their analysis, change in psychological and social phenomena may be explained by referring to *catalysts* - i.e., contextual factors that usually work to regulate and maintain the relationships of parts within the system. The authors conclude by claiming that their "conditional analysis provides fruitful grounds of not only the rare and frequent phenomena, but understanding the particular, the qualitative whole, and the relationships within a general framework" (p. 79).

The next two articles investigate constructivist epistemology and existential psychotherapies. In *The Issue of the Unity and Specificity of Psychology from the Viewpoint of a Constructivist Epistemology*, Gabriele Chiari analyses the subject of the unity of psychology in the light of his proposed reading of constructivism. As Chiari notes, the absence of one shared system of reference for psychological theories makes the epistemological status of psychology clearly different from that of the physical sciences. Moreover, he argues, because this situation was taken to be problematic, psychologists have often tried to solve the problem of the diversity within their discipline either by defending the unity of psychology or by reducing psychology to the neuroscience. Against such a view, Chiari suggests that the pluralism in psychology might "represents a richness rather than a limit of it" (p. 82) and supports his thesis by sketching a path through the wide field of constructivist psychologies.

In the service of this view, Chiari refers to Kelly's (1955) constructive alternativism, to Agazzi's (1976) objectualist epistemology, and to Maturana's (1987) ontology of the observer. All these approaches share the view "that every scientific discipline cuts out its objects by looking at things from a certain point of view and investigating them according to certain methods" (p. 88) so that the truth of the statements of a theory must always be referred to the domain of the theory. It follows that, according to these approaches, "the question on the 'absolute' truth of a single proposition or a theory derives from mistaking 'things' for 'objects' [which are internal to the theory]" (p. 88). Consequently the project of the unification of psychology is "unfeasible", that is, impossible. The same can be said about the project of the reduction of psychology. Indeed, he argues, the assumption of a constructivist perspective denies the possibility to soundly asserting the ontological reduction of the object of a theory to another one. Rather, constructivist epistemology envisages ontologically more neutral theses, such as emergentist materialism.

Louis Hoffman's article, *Knowing and the Unknown: An Existential Epistemology in a Postmodern Context*, investigates the possibility and the extent of the integration of psychotherapies in the framework of existential psychology. Hoffman notes that research on the efficacy of therapy has led us to reassess the importance of the particular method used in psychotherapy. Indeed, "providing a plausible explanation for the client's problems and using this as a foundation for the therapeutic work is more important than the particular techniques used" (p. 100). Such a consideration has "the potential to change the field of psychotherapy and bring a revitalized ethical approach to therapeutic practice" (p. 107) because it urges psychologists towards the integration of different techniques. In fact, the many similarities between existential psychology and postmodern philosophy provide an ideal foundation for psychotherapy integration. According to Hoffman, "the primary benefit of integration may be in the flexibility or adaptability" (p. 104). That is, by avoiding a pragmatic eclectic approach

according to which every technique is good if it works, the therapist must be able to assess "which therapy is the best fit for which clients" (p. 106) and "to adapt to their client's specific needs and style in order to be effective" (p. 106). This brings the ethical concern that therapists "should begin the therapist process […] trying to identify if the client is the right fit for their approach to therapy" (p. 106).

Nicolò Terminio analyses the epistemological standpoint of Lacanian psychoanalysis in *Epistemologia dello studio del caso clinico: note sul metodo della psicoanalisi* [Epistemology of the study of a clinical case: methods and principles of the psychoanalytical model]. Terminio begins with the assumption that the insistence on singular cases and the ethical constraints of the therapy prevents psychoanalysis from satisfying "the request for controllability proper of the research methods of the so-called 'hard' sciences" (p. 112, my translation). In fact, the symptoms in the patients are messages encoded in a way that is known only to them, while psychoanalysts must decode and reconstruct the meaning of these symptoms based on their personal training. This requires psychoanalysts to make "strategic" abductive inferences, which cannot be experimentally guaranteed. Given that there exists a "methodological uneasiness" between the generalizations of the science and their application in the clinical praxis, the psychoanalyst must skillfully interpret symptoms in a way that is conducive to healing, rather than to the production of knowledge. Nonetheless, Terminio maintains that psychoanalysis can to assume a critical stance towards *experimental* methods, while keeping at the same time "the necessity of an *empirical* anchorage for any discussion about the clinical dimension" (p. 122, my translation). Hence, he refers Reichenbach's (1951) distinction between the context of discovery and the context of justification, and he shows that, although the clinical experience cannot fall within the latter, it can still fall within the former.

This collection ends with an article about an emerging discipline, i.e., ethnopsychiatry, that reflects a critical answer to the questions raised by the epistemology of psychology. In *Psicologie, etnopsichiatria, sistemi di cura* [Psychologies, ethnopsychiatry, and healing systems], Piero Coppo and Stefania Consigliere claim that the crises of the Western science in the XXth century should lead us to abandon the old-fashioned positivist idea of knowledge as a universally valid description of nature. Nowadays, they argue, we must acknowledge that all systems of knowledge and know-how originate in specific historical conditions and have equal epistemological dignity. Psychology, they argue, cannot ignore this new epistemological context: it needs a radical shift to recognise both the pluralism of psychological approaches within its Western borders and the existence of other systems of knowledge and know-how with their own internal coherence in other cultures. Within such a framework, psychiatry must be open to a "radical encounter" (Pasqualotto 2005) with other cultures, where "opinions and preconceptions […] are discussed and wiped out" (p. 127, my translation). In this enterprise we risk misunderstanding other cultures, or mistranslating their concepts into those drawn from our own conceptual framework, therefore to betray them. Indeed, given the relativity of every system of knowledge to its own culture, they argue, the development of the theoretical tools necessary to the confrontation between different cultures cannot be guarantee by *any* general framework. The methodology adequate to this problem is that of the ethnopsychiatrists, who act "on the field" and confront their conceptual framework by applying it while living in other cultures. Only in this way can one respect different therapeutic practices and acknowledge their internal coherence.

The collection of essays gathered in this volume explores in several directions the many issues raised by the epistemology of psychology. The different directions in which the authors take the discussion reflect their somewhat different assumptions, and demonstrate that unity even of purpose in this field is not easy to attain. Nonetheless, some common themes emerge: (i) a particular attention to the epistemological issues, not just to the methodology, in

analysing psychology as a science is possible and valuable; (ii) the pluralism of psychology can be accepted, and does not demand reconciliation; (iii) the normative concept of person for the development of psychology as a science is non-negotiable; and finally (iv) rationality both grounds the norms for adjudicating conflicts between different theories and is essential to the definition of human life. Only by considering these issues together can one expect to provide a complete picture of psychology as the science of the mind.

Of course, due to the vastness of the field, the views proposed are but a preliminary sketch, and the final picture is yet to emerge. But it is a coherent sketch, and it suggests how one might proceed to fill in the details of what promises to be a coherent picture. I hope that this volume will have set the stage for a promising field of investigation.

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Rubrica

Ormai siamo tutti psicologi

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Sono passati circa duemilacinquecento anni da quando qualcuno scrisse, incidendolo sulla pietra del tempio di Delfi, un famoso appello al viandante che così recitava: "Conosci te stesso …". Potremmo considerare quel motto la data di nascita della psicoterapia contemporanea. Che altro è infatti la psicoterapia se non una minuziosa e cervellotica ricerca del "doppio", di quello strano personaggio alieno con cui conviviamo dalla nascita e che molto spesso non conosciamo affatto? Addirittura, capita non di rado che "quel tipo" ci accompagni fin nella tomba senza che si sia riusciti a scambiarci qualche impressione, qualche opinione, qualche parola! E pensare che abbiamo vissuto gomito a gomito per tanto tempo! Ci siamo scambiati perfino di posto in qualche rara occasione! Ma non abbiamo mai superato quella tacita barriera del *pudore* che ci impediva di curiosare troppo, di interessarci ai suoi affari più intimi: abbiamo avuto sempre paura di trovarci faccia a faccia con lui senza schermi e senza maschere, nudi e senza veli!

Tornando allora all'esortazione dell'oracolo a conoscersi intimamente, proviamo a chiederci se, dopo così tanti anni, la situazione è cambiata, e come. Per dare una prima risposta bisogna però domandarsi da quali segnali potremmo eventualmente inferire che genere di cambiamento c'è stato (e… se c'è stato!). E se, per caso, questa eventuale migliore conoscenza di sé ha reso più facili e pacifici i rapporti tra gli uomini, se tutta la gamma dei vizi e delle peggiori nefandezze umane ha subìto una riduzione, o addirittura è scomparsa.

Purtroppo e con tutta la buona volontà, mi pare facile prevedere la risposta: nei duemilacinquecento anni fin qui trascorsi non si rilevano progressi significativi in tale direzione, non si rilevano cioè modificazioni sostanziali nel comportamento individuale e collettivo della specie denominata *Homo sapiens* o delle sue più recenti varianti "*Homo videns*" e "*Homo technologicus*". Come appello, il "conosci te stesso" pare proprio non abbia avuto un gran successo!

Eppure, anche Socrate aveva ripetuto più volte quella stessa esortazione, convinto com'era che solo dalla profonda consapevolezza dei propri limiti e, sopra tutto, della propria ignoranza, l'uomo avrebbe potuto raggiungere quello stato di autentica saggezza indispensabile per una vita individuale e collettiva degna di essere vissuta.

Dopo Socrate, in realtà, gran parte della filosofia e della teologia occidentale hanno posto al centro della propria ricerca le insondabili caratteristiche dell'animo umano, quel sé abissale, nascosto, sfuggente e misterioso. A partire dalla splendida immagine platonica dei due cavalli e del cocchiere che li guida alla tripartizione aristotelica in vegetativa, sensitiva e intellettiva, la nozione di anima e le sue caratteristiche sono sempre state al centro della riflessione filosofica in ogni epoca e, possiamo senz'altro dire, in ogni cultura.

Se andiamo a rileggere le loro opere con attenzione ci possiamo convincere che quasi tutti i maggiori pensatori, quelli che hanno contribuito maggiormente a lacerare il famoso "velo di Maya" della nostra ignoranza, ognuno di loro, dicevo, ha cercato di comprendere sempre meglio il rapporto che sussiste tra il comportamento istintivo, animalesco, primitivo per così dire e quello razionale e civilizzato dell'uomo sociale. Come esempio tipico, mi viene in mente Cartesio, che si preoccupa di matematizzare la conoscenza e di fondarla su basi certe, ma non trascura di indagare a fondo la natura delle passioni umane.

Certo che oggi anche Aristotele si meraviglierebbe assai della preoccupante sproporzione e del conseguente sbilanciamento tra la crescita smodata delle anime vegetative e sensitive a dispetto di quelle intellettive. E probabilmente, non sarebbe più tanto sicuro di poter definire l'uomo "animale razionale" a meno di una sostanziale revisione del termine "razionale"! (e qualcuno poi dovrebbe pur avvertirlo che le "essenze" sono ormai evaporate!).

Questa secolare discussione filosofica subisce però una svolta profonda alla fine del 19° secolo quando il motto delfico sembra perdere la sua importanza a favore di una profonda trasformazione della sua stessa natura. Accade infatti che la psicologia inizia a prendere le distanze dalla filosofia e, faticosamente, a camminare sulle proprie gambe cercando con ostinazione di darsi una veste autenticamente "scientifica" e "sperimentale": è una legittimazione di cui non può fare a meno di fronte agli indiscussi successi delle scienze positive, sue contemporanee.

La scoperta freudiana dell'inconscio (mi si permetta questa rozza semplificazione per esigenze di brevità) sembra allora promettere una definitiva minuziosa conoscenza del nero cavallo di Platone, della componente istintiva, animalesca della psiche umana. Ma presto si vede che così non è e che i problemi sollevati sono ben più numerosi e difficili di quelli apparentemente risolti.

Da allora il proliferare dei metodi e delle prospettive, dai principi assunti come fondanti allo sviluppo delle strategie di cura, ovvero delle psicoterapie che oggi vengono praticate, si è fatto incessante. Fino ad arrivare, con una sorta di capovolgimento temporale, a suggerire la pratica del *counseling* filosofico come sostitutivo dei trattamenti dei casi più semplici ma anche più diffusi di disagio psichico. Fino all'attuale dibattito sull'uso disinvolto degli psicofarmaci per curare singole patologie che, in questo quadro molto confuso, vengono semplicemente inventate e si moltiplicano senza sosta specializzandosi fino ad interessare gli aspetti più intimi e marginali della vita quotidiana.

Indubbiamente la confusione è grande sotto il cielo e tutti ne sono ormai consapevoli. Ma, finalmente, sostengono alcuni, abbiamo scoperto anche il sistema di ricondurre tutti i nostri problemi "psicologici" alla verifica, con mezzi sempre più sofisticati, dell'esistenza o meno di fenomeni neurologici che possono essere "visualizzati" con opportune tecniche. Ecco dunque diffondersi l'idea di poter monitorare e forse anche comprendere una buona parte degli eventi mentali superiori per mezzo della "neuroimaging" (come i processi della visione o della memorizzazione, per es.) o addirittura dar conto delle manifestazioni empatiche ascoltando il ticchettio dei neuroni specchio che "sparano" la loro eccitazione!

Esagerazioni, si dirà, con un assennato cenno del capo! È pur vero però che qualche modesto ma significativo risultato le attuali neuroscienze lo hanno raggiunto: come, ad esempio, quello relativo allo scarto temporale tra la scelta inconsapevole e quella cosciente, risultato che pone una seria ipoteca sull'antica disputa circa il famoso problema del "libero arbitrio"!

E allora, tornando al nostro quesito iniziale, se e quanto la psicologia e la sua diretta emanazione, la psicoterapia intesa socraticamente come "cura dell'anima" abbiano influito o condizionato lo sviluppo della società contemporanea, credo di poter rispondere tranquillamente che la loro influenza è stata ed è minima, dal punto di vista degli effetti concreti e duraturi. La conoscenza di sé è un percorso che si rinnova da capo a ogni generazione, e non c'è progresso cumulativo in questo. Anzi, pare proprio che l'umanità sia appena nata e appena trovata di fronte al motto delfico.

Altro discorso invece per la diffusione dei termini psicologici e delle parole derivate dal gergo tecnico che hanno ormai colonizzato il linguaggio quotidiano. Qualche osservatore attento ha scorto in questo progressivo psicologizzarsi delle tematiche sociali e, soprattutto, del modo con cui se ne parla, una sorta di subdolo tentativo di anestetizzare l'opinione

pubblica col mostrare sotto forma di patologia individuale quello che in realtà è un problema di carattere politico e sociale legato a scelte ed opzioni precise. Personalmente, non mi sembra una tesi peregrina. È sotto gli occhi di tutti il crescente imperversare di "consigli" che vengono elargiti spesso da sedicenti esperti e tuttologi da ogni settimanale o trasmissione radio-televisiva e relativi ad ogni aspetto della vita individuale e collettiva. Così come già da tempo una persona handicappata è diventata "diversamente abile", ora una persona triste diventa affetta da "sindrome depressiva" e un bambino vivace (e magari vispo e intelligente…) viene dichiarato affetto da "sindrome ipercinetica"! Col risultato inevitabile, per la persona triste, di sentirsi insicura e inadeguata a vivere come gli altri e per i genitori di un bambino vivace di rivolgersi al pediatra o al neuropsichiatra infantile per farsi prescrivere una terapia a base del famoso Ritalin e sentirsi colpevolmente inadeguati.

Ma ormai siamo tutti psicologi e il disagio e la sofferenza perlopiù nascosta, il senso di straniamento, la solitudine vengono vissuti come problemi di adattamento, come patologie da curare individualmente. Ansia e depressione sono parole comuni che ricorrono con frequenza sempre maggiore nelle conversazioni tra amici, al bar o nei posti di lavoro.

Pare strano che a nessuno venga in mente che forse è proprio il nostro modello di vita a produrre quel disagio, che il ritmo forsennato della vita quotidiana, l'isolamento delle persone (in auto in un ingorgo, con un telefonino in mano o in casa davanti ad un computer o a una televisione "satellitare") e la pratica scomparsa della vita sociale, la sua frammentazione rappresentino le vere cause della situazione attuale, per molti versi drammatica.

Concludo questa breve nota osservando tristemente che oggi un novello Socrate che osasse seriamente riproporre l'appello a conoscere sé stessi attraverso una costante riflessione e prendendosi cura ciascuno della propria anima, farebbe ancor più rapidamente la stessa fine, ossia verrebbe più o meno delicatamente invitato a togliersi dai piedi!

Report

Summer School on *The Social Self*

Alghero, 20-27 September 2009

Chiara Brozzo

Between 20th and 27th September 2009, the Faculty of Architecture in Alghero (University of Sassari) hosted a one-week interdisciplinary summer school, "The Social Self", on the contemporary research on philosophical and psychological models as well as neural mechanisms underlying the sense of the self, shaped through the intersubjective experience. This summer school was organized by Prof. Fabio Bacchini (University of Sassari), Prof. Vittorio Gallese (University of Parma), Dr. Ludovica Lorusso (University of Sassari, Ph.D), Prof. Corrado Sinigaglia (University of Milan), Prof. Silvano Tagliagambe (University of Sassari), and brought together major figures in philosophy, psychology and neuroscience.

Vasudevy Reddy (Portsmouth University) in "Engagement and awareness in infancy" explored the concept of the social self under the viewpoint of developmental psychology, concentrating on the sense of the self as it emerges at early stages of human life. She described the ways in which an infant relating to her own parents starts building up the sense of the self and others, for she argued that to understand the self we have to understand awareness of others. She described the development of attention and engagement, both from and towards others, and the strategies for engaging with others such as playful teasing. She then widened the focus of this study so as to include a reflection of culture, which exist at a dyadic level as well as at large group levels, and emerges through engagement.

Natalie Sebanz (Radboud University) focused on "The social self in interaction", and gave an analysis of that under three different viewpoints: conceptual level, perceptual level, and motor level. The conceptual level analysis was concerned with thinking about self and other, and this discussion included a reference to the contrast between Theory Theory and Simulation Theory. The perceptual level discussion was essentially about shared action planning, and Sebanz illustrated a series of experiments meant to suggest that a person's perception and reaction times when performing a task do change as soon as another's contribution to that task has to be taken into account. Finally, the motor level discussion was about shared motor control.

Barry Smith (Institute of Philosophy, University of London) in "'Sharing the moment but being alone': the extent and limits of social cognition" sought a correct philosophical approach to the concepts of self and others. The Cartesian view, according to which acquaintance with our own mind provides a basis for knowing others' minds, was rejected as it is not clear what the source of our concept of the self would be in that case. Similarly, both Simulation Theory and Theory Theory were criticized on the grounds that they presuppose the perspective of the self. An alternative solution called "Folie à deux" was then proposed, and described in terms of a default identification of own states with others'.

José L. Bermùdez (Washington University) in "The social self in philosophy and psychology" proposed a taxonomy of different sorts of mindreading, starting with the simplest form, which is a form of social coordination in the animal kingdom that involves a sensitivity to the psychological states of other participants in interaction. While this falls in the category of the so called *minimal* mindreading, there are more sophisticated forms of mindreading qualified as *substantial* mindreading. One example of the latter consists of propositional attitude mindreading, which requires:

(a) Attributing propositional attitudes
(b) Explicitly representing the agent's background psychological profile
(c) Reasoning about how (a) and (b) might jointly issue in action.

Georg Northoff (University of Ottawa) in "The self: neuroscience and neurophilosophy" explored the link between the sense of the self and reward on the one hand and the self and emotions on the other hand under the neurological viewpoint, by discussing the role played in this context by midline cortical structures in the brain. Self and emotions are related to each other. Self-relatedness increases in depressed patients as opposed to healthy subjects. That translates in terms of abnormal (positive) correlation of subcortical neural activity with self-relatedness in depression. In schizophrenia, exactly the opposite is observed, namely a decreased sense of the self.

Rebecca Williamson (University of Washington) presented "Imitation and the social self: social cognition and learning in childhood", written with Andrew Meltzoff (University of Washington), on the development of the sense of the self in children, with a focus on imitation. Humans, perhaps uniquely, imitate others' behavior, and this is adaptive. Children imitate body movements, outcomes, exact means. Imitation is a social mechanism that allows children to connect to other people, therefore has a role in establishing cultural, cognitive, and social processes. Experiments were reported showing how imitation is related to (i) the understanding of a goal, (ii) prior experience of self and other (which influences imitation), (iii) the role of intentional demonstrations (which promote imitation).

Salvatore Maria Aglioti (University La Sapienza - Roma) in "Flesh made soul: bodies in the brain" presented different varieties of the disruption of the sense of the self under the neurological viewpoint, including cases of disownership of one's limbs. Representation of bodies is distributed over the brain. For instance, the extrastriate body area is involved in coding bodily parts (visually and somatically), but not other objects. There are studies showing that this area is also active when you observe moving people. Aglioti also illustrated the rubber hands and the full body illusion, as well as the so called "enfacement" (a photograph of your face is slowly turned into a photograph of someone else's face by a computer programme), which may be thought to challenge the sense of the self.

Jessica Hobson and Peter Hobson (University College London) in "Self/other relations in autism: imitation, communication and social emotions" took the topic of the self into developmental psychology, by discussing the sense of self and other in autistic children through the disruption that autism brings into engagement towards the other. Empathy is to "feel for" the other person, so when another person is for instance in distress, you feel some sort of distress too. The Hobsons' hypothesis is that children with autism lack empathy so conceived. To lend support to this, a study has been carried out to test how much concern is shown by children in a situation of potential distress. Among control participants, concern was manifest in a combination of expressions and actions. This pattern of relatedness was relatively absent among participants with autism: children with autism usually do not show any concern / surprise. This suggests that children with autism have a relative lack of person-related organization to their affective states.

Kim Sterelny (Wellington University) in "Evolution and the sociocultural species" explored the self/other relation through an analysis of the evolutionary mechanisms underlying the transmission of culture. Creatures become different because of forms of dynamic feedback, and that is thought to of key importance to the general problem of understanding human behaviour. Sterelny focused on the processes that lead to the establishment of distinctive features of human social learning. Transformation has been so rapid and intense that some

form of feedback learning is needed to explain it. Most competences that we show often involve extensive use of information. Too often we're able to cope with novel challenges, and our capacity to respond to them involves a special relation to information flow.

Matteo Mameli (King's College London) took further Sterelny's discussion by focusing on the transmission of cultural values in terms of natural selection, for it has been suggested that statistical methods traditionally used in biology for the study of genes could be employed to study the transmission of language and skills. We want to know whether something like natural selection, which explains adaptive change on random genetic modifications, may also explain cultural change. This idea was supported through the exploration of different processes. One is to be found in groups where information is shared to do better against threats. Hence you have group reproduction, and increased fitness of a particular group. Another process is one in which natural selection is generated by differences in fitness between cultural variants – one example is memes.

Vittorio Gallese (University of Parma) in "Social self and mirror neurons" illustrated the role of mirror neurons in providing the basis for social interaction by being essentially linked to goal-related actions. In the ventral part of the pre-motor cortex, what we call area F5, there are neurons that activate when an action (movement with an aim) is performed, and that are sensitive to the different aims (see Rizzolatti et al. 1988). No matter how the movement is performed, the pattern of activation remains the same if the aim is the same. So these neurons implement a goal representation whose content is both intentional, because it is a *goal-centered* motor representation, and motor, because the aim is mapped in *motor terms*. Also, the link between empathy and mirror neurons was explored. An experiment through fMRI recording was reported to show that the species of the actor performing a certain action matters to the activation of different brain areas. The conclusion would be that when we look at the face of a conspecific expressing a certain feeling, empathy is possible, but this happens less and less when we it's about species which are farther away from our own one.

Shaun Gallagher (University of Central Florida) in "Primary intersubjectivity" argued that the self is to be studied in developmental terms, as the beginnings of primary intersubjectivity run along with certain aspects of the minimal self. As we perceptually pick up information about the environment and about others, we are given information about ourselves. Interaction theory was presented by Gallagher as a viable alternative, one which rejects the Cartesian idea that other minds are inaccessible and makes a strong appeal to the kind of interaction that happens in primary intersubjectivity. Gallagher pointed out that Theory Theory and Simulation Theory, however, don't necessary exclude primary intersubjectivity.

Gregory Currie (University of Nottingham) discussed the self as a point of view as it features in narrative in terms of a narrator. The narrator enjoys some sort of double perspective on the action, which becomes particularly interesting when author and character being spoken about are connected in some special way. The time of the narration and the time of the events which are narrated are different, the narrator knows more at the time of narration than at the time narrated. The point of view to which we are orientated is the point of view of the narrator, so, we may conclude, narrators are more than simply informants: they may express a point of view, which influences our own; through mechanisms of imitation, we may come to experience the story as something we attend to jointly with the narrator. Narrators may also place us in close relations to the points of view of characters, by exploiting the same imitative mechanisms.

Also, an opportunity to present a short paper was given to the participants Giulia Battilotti (University of Padova, Italy), Anna Bortolan (University Vita-Salute San Raffaele, Milan, Italy), Massimiliano Cappuccio (University of Stirling, Scotland; University of Bentley (Massachusetts), USA), Sanneke de Haan (University of Heidelberg, Germany), Nevia Dolcini (University of

Report

Summer School on *Mind and Language. Mental Simulation and Knowledge of the Past*

Siena, 8-11 June 2009

Marta Di Dedda e Martina Pantani

The second edition of the Summer School in Mind and Language on *Mental simulation and Knowledge of the Past* took place at the University of Siena from June 8 to June 11, 2009. It was organised by the professors of the Doctoral School of Cognitive Sciences, Gabriele Usberti and Giacomo Romano. The summer school was arranged in two sessions of two days each. The first session, held by professor Cesare Cozzo (Università La Sapienza, Rome), was about Michael Dummett's analysis of truth conditions for past events. The second session, held by professor Pierre Jacob (Institut Jean Nicod, Paris), concerned mental simulation, action-mirroring and mindreading.

On the first day, Cozzo illustrated the notion of truth about statements concerning another place and/or time, discussed by Dummett in *Truth and the Past* (2005). He noted that, according to the principle of bivalence, every statement, like "at 6.00 p.m. in the day of his fortieth birthday the number of Dummett's tie was odd", must be truth or false. However, sometimes the chance to know the truth or falsity of past statements goes beyond our faculties because we are not in that time or place. Therefore, it is difficult to assert that past statements have always a truth value, contrary to the principle of bivalence. An opposition about the transcendent notion of truth arises. According to the realist, truth is unexceptionable, and understanding a statement means to couple it with truth conditions regardless of the possibility to establish them. Instead, according to the antirealist, past statements are true or false only because we provide them with a truth value. In fact, the truth value of a past statement is given by a direct proof, which we can find in the present time, that justifies the attribution of that truth value to that statement. In the case of empirical statements in the present time, such as "here it's raining", then, the antirealist claims that the principle of bivalence is valid because we can access a direct proof to verify them. In the case of a past statement about the same observable event, instead, the principle of bivalence is invalid because we may lack a direct proof of the event. Therefore, it is impossible that the same logical principles are valid for the same empirical statements considered in the present time or in the past.

On the second day, Cozzo discussed the turning point in Dummett's philosophy constituted by the *Dewey Lectures* (2002). Before the *Dewey Lectures*, Dummett endorsed a justificationist view about truth that yielded to a radical antirealism about the truth value of past event statements. However, in the conference *The Reality of the Past,* Dummett came to the conclusion that justificationism can avoid inconsistency only by becoming a radical view, and claimed that radical antirealism is "repugnant". Therefore, he moved to a moderate antirealism. Dummett introduced a spatio-temporal grid to explain the understanding of past

statements, and looked to the attribution of a truth value to past statements by the way in which a child learns to assess their related proof. First, the child learns what is a direct proof, i.e., a proof that is contemporary to the event that it assesses. Then, the child learns both to place present events in other spatial and temporal coordinates and to assess proofs for those events. Since it is not possible to have a direct proof for past events, the child needs to learn to refer to the community of speakers to justify them. Therefore, according to Dummett, language has the fundamental function to allow knowledge to spread over the community of speakers. Cozzo concluded by noting that, as Dummett shows, our capacity to wear the shoes of an observer is fundamental to understand past statements.

On the third day, Pierre Jacob provided a general introduction to mirror neurons (MNs) and action-mirroring by presenting a series of experiments on both macaques and human beings. Jacob noted that we can understand a perceived action either by visual analysis or by mapping the action in the motor repertoire of the observer. This led to two possible versions of action-mirroring. According to the weak version, actions that do not belong to the observer's motor repertoire and cannot be so mapped are categorized on the basis of their visual properties. For example, primates may have the capacity for a detached visual analysis of a bird's flight, but lack a motor understanding of it, since they cannot match birds' wing movements onto their own motor repertoire. On the contrary, following the strong version, an observer achieves a distinctive kind of 'engaged' or immediate understanding by mapping an agent's observed movements onto her own motor repertoire.

On the fourth day, Jacob criticized the proposal of Goldman and Gallese (1998) that mental simulation is allowed by processes of mirroring together with processes of imagination and pretence, so that the MN activity would support third-person mindreading abilities. Gallese and Goldman claim that MNs allow attributing beliefs both to forecast the future behaviour of an agent, and to explain its past behaviour. That is, the activity of MNs is both predictive and retrodictive. According to them,

> the attributor starts with the question, 'What goal did the target have that led him to perform action **m**?' He conjectures that it was goal **g**, and tries out this conjecture by pretending to have **g** as well as certain beliefs about the effectiveness or ineffectiveness of the action **m** *vis-à-vis* goal **g**. This simulation leads him to form a (pretend) decision to do **m**. He therefore uses this result to conclude that the target did indeed have goal **g**. In this fashion, the attributor ultimately makes a 'backward' inference from the observed action to a hypothesized goal state (Gallese & Goldman 1998)

According to Jacob, instead, the proposal is unlikely because of the causal gap between action and belief. It seems that, starting from actions, we can predict other actions, but that we cannot attribute mental states. How might an observer attribute a belief only by watching an action? We cannot form a representation of the agent's intention from the perception of her movement. Therefore, Jacob claimed that it is more likely that the MN activity only predictively computes the best motor command suitable to satisfy the agent's intention.

Finally, Jacob examined the hypothesis that mirroring might be applied to understand emotions and affective states.

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Report

Rutgers-Siena Joint Workshop on *Mind and Culture*

Certosa di Pontignano (Siena), 1-2 June 2009

Nicola Simonetti

The first Rutgers-Siena Joint Workshop on *Mind and Culture* took place at the International Conference Centre of the Certosa di Pontignano on June 1st and 2nd, 2009. The workshop was organized by the Doctoral School of Cognitive Sciences of the University of Siena and the Rutgers University Center for Cognitive Sciences (RUCCS) with the partnership of the Institute of Human Sciences (SUM) of Florence, the Santa Chiara High School, and the Inter-University Center for Experimental Economics.

The programme consisted of two days, in which professors of cognitive science and their Ph.D students from both Rutgers and Siena University gave lectures or presented papers about their research topics.

On the first day, Jerry Fodor (Rutgers University) opened the workshop by speaking about *Prospects for a causal theory of reference*. He criticized Kripke's (1980) causal theory of reference by claiming that it does not provide a naturalistic approach to language and reference. Fodor considers Kripke's conception as a version of associationism, indeed unable to explain the intentionality and the content of mental states. According to Fodor's psychosemantic theory, instead, the meaning of an object is a representation of that object in our mind. Therefore, there is a causal connection between thoughts in our mind and the objects in the world. This theory is coherent with a naturalistic approach to cognitive faculties, grounded on the two hypotheses of the language of thought (Fodor 1975) and the modularity of mind (Fodor 1983).

In his talk on *Latest news from the laboratory: It's indexes and the way down*, Zenon Pylyshyn (Rutgers) accused the computational theory of the mind of lacking an explanation about the representational content of computations. How mental representations are connected to what they represent? The computational theory of mind does not specify the causal mechanism that explains how the representational tokens of things acquire their content. In the case of solving geometry problems in perception, for example, we label lines or vertices to specify the individuals to which they refer. However, causal connection *per se* does not imply conceptualization. According to Quine, Strawson and others, you cannot track individuals without sortal concepts; moreover, you cannot pick out individuals with only concepts. Compare this with Kripke's distinction between properties that fix the referent of a proper name and the referent itself. Is there something special about location? The only content that non-conceptual representation can have is the demonstrative content.

A lecture by Massimo Piattelli Palmarini (University of Arizona), co-authored with Fodor, on *What Darwin got wrong* followed. Palmarini remarked that Darwin's theory of evolution does not make sense of the chromosomes number difference across species. The platypus, for example, an animal with both mammal and reptilian characteristics, has 52 chromosomes versus the 46 chromosomes of the human species. Analogously, salamanders have much more chromosomes than apes. These differences in the chromosome number are paradoxical if we assume that, according to Darwin, more evolved species should have a more complex DNA,

that is, more chromosomes. This sheds a doubt about the criteria according to which we judge the grade of evolution of species.

Sandro Nannini (University of Siena) gave a lecture on *Why is Einstein's relativity theory counter-intuitive? A naturalistic approach to the relation between real time and time intuition*. His aim was to see whether it is possible to assess Einstein's relativity theory in the light of the strong intuition that time is not relative to an observer. Such "realism" about time is shared both by the common-sense view and by many cosmological models under the concept of 'cosmic time' (e.g., P. Davies). Nannini argued that the intuition of time depends on a brain mechanism that brings about the mental construction of phenomenal time. He added that we cannot avoid this mechanism to work. However, this does not imply that phenomenal time mirrors an alleged unique and objective real time. On the contrary Einstein's relativity theory, combined with neurological theories about time perception, explains why we human beings perceive time as mind-independent although it is as a matter of fact mind-dependent.

Adriana Belletti and Luigi Rizzi (University of Siena) gave a lecture on *The cartography of syntactic structures: locality and delimitation effects*. The notion of syntactic complexity has played a central role in the study of several aspects of language intended as a cognitive capacity. Belletti and Rizzi were particularly interested in the dimension of complexity which arises in constructions involving A-bar movement (relatives clauses, questions, etc.), and which manifests itself in both adult grammar performance, and child language acquisition. By reporting experimental studies on the comprehension and production of subject and object relative clauses in children, they showed how the selective delay of certain object relatives is predicted by an intervention approach – i.e., an approach according to which an element which intervenes between the elements involved in a local relation, for instance between a moved element and its trace, and bears a certain similarity to such elements, may disrupt the local relation, or make it hard to process. Moreover, they illustrated some strategies that the child resorts to in order to avoid the complex configuration, avoiding intervention.

Marco Gori's (University of Siena) lecture was entitled *On the birth of cognitive stages: complexity issues*. Gori claimed that Jean Piaget's studies about cognitive development inspire important advances in machine learning. Piaget's genetic-evolutionary approach identified four universal stages, or periods, of development in child learning abilities, where each stage is self-contained and builds upon the previous one. Children firstly develop sensorimotor and preoperational skills, in which the perceptual interactions with the environment dominate the learning process. Then, they develop concrete and formal operational skills, in which they start to think logically and to rely on abstract thought. By referring to Piaget's developmental framework, we can think of learning processes as a physical process that we can try to capture by providing physical laws that explain the variations in the development. Like for physical laws, where variational principles ensure grace and lead to capture the simplicity and elegance of natural behaviour, in cognitive science variational principles give rise to kernel machines that provide an effective model of sub-symbolic tasks like those related to sensorimotor and pre-operational stages in children. In the end, we can keep the same variational framework to impose a semantic-based regularization to relate semantic attributes so as to capture higher levels of cognition.

In his lecture *Cognitive economics in the lab: the power of labels*, Innocenti (University of Siena) claimed that problem solving about decision-making under uncertainty is environment-dependent. In fact, he showed that including within experimental designs contextual clues, such as "label" or "virtual simulations", makes laboratory tasks more familiar. Therefore, the goal to mould context-free experiments is not only elusive, but also inappropriate to investigate cognitive processes. To support this view, he presented an experiment on travel

mode choices, in which subjects acquire *ex-post* information not only through their own experience, but also through external information on actual travel times of all travel modes. He concluded that virtual experiments combine the internal validity of controlled laboratory experiments with the external validity of field experiments.

On the second day, Ernst Lepore (Rutgers) gave the first lecture, *On agreement*. He noted that contextualism is very important in epistemology, metaphysics, and ethics. Such a view is shared by philosophers, linguists, and cognitive scientists. On the contrary, context insensitivity seems to imply both minimalism and relativism. Therefore, a use of context sensitive expressions must be accurate. Structural semantic theories do not provide an account of the conditions under which sentences are true, because they lack relations between expressions and the world. Therefore, many philosophers and linguists endorse model-theoretic semantics, which do not share the same deficit.

Brian McLaughlin (Rutgers) gave a lecture on *Existential feelings and monothematic delusions*. He professed to be sceptical about the prospect of explaining psychological laws starting from Fodor and Pylyshyn's (1988) cognitive architecture for mind and language, which is created to support the systematicity of the mind. He argued against them that systematicity of the mind can be obtained without assuming conceptual abilities and systematicity laws. Furthermore, he claimed that there is not an adequate definition of systematicity in thought and language. Natural languages are not systematic at all. This poses an end to the debate about a scheme in natural language.

In her lecture, *Descartes and qualia*, Raffaella De Rosa (Rutgers University) presented the Standard View (SV) on Descartes' sensations, according to which sensations lack intrinsic intentionality because they appear as purely qualitative features of experience (or *qualia*). Accordingly to SV, in perceiving, for example, the colour red, we are merely experiencing the subjective feel of redness rather than perceiving the property of a body. The aim of the lecture was then to establish that the textual evidence offered in support of SV fail to prove that Descartes held SV. Indeed, De Rosa argued that there are textual and theoretical reasons for believing that Descartes held the negation of SV. *Qualia* are not Descartes' legacy.

Sosa (Texas University) talked about *The varieties of content*. In the talk, he argued that Burge's extension to belief of the sort of externalism that Putnam took in the twin-earth examples is vitiated by a presupposition about the relation between *de dicto* belief ascription and belief content. Burge, in effect, takes our differential willingness to make the same *de dicto* ascriptions to reflect our view that subjects in his examples do not share belief content. However, Sosa argued that we have good reasons to doubt that move. He tried to find a general principle ("Ascription") governing belief ascription that would block making the same ascriptions but without guaranteeing that subjects do not share belief contents (our use of indexicals in belief ascription – which Burge tries to differentiate from his cases – is covered by Ascription as a special case). A general background theme, throughout (though not made explicit or discussed directly), is that the nature of linguistic content, including the semantics of words used in belief ascriptions, is very different from the nature of mental content, such as the content of beliefs.

Usberti (University of Siena) gave the last lecture of the Workshop on *Williamson's arguments against luminosity*. He examined the two versions of Williamson's *Knowledge and Its Limits* (2002) argument against the epistemic transparency of mental states. He questioned the principle, which concerns the assumption of the first version of the argument, that our discrimination capacities are limited. The second version of Williamson's argument relies on a principle about reliability to which a counterexample has been provided by Selim Berker. Usberti defended this counterexample not in terms of a relation of constitutive dependence of certain conditions on beliefs (as in Berker's paper), but in terms of the (absolute) reliability of

certain belief-forming methods.

The workshop was accompanied with talks provided by Ph.D students from Rutgers University and University of Siena. On the first day, the Ph.D students who gave a speech were Giuliano Bocci (*On the insertion and interpretation of intonational events*), Sarah E. Murray (*Evidentials as not-at-issue assertion*), Irene Franco (*Syntactic visibility of discourse related properties: the case of Scandinavian embedded V2*), Carlotta Pavese (*Propositional attitude reports and the puzzle of substitution failure*) and Stefano Guidi (*Probing the structure of a frame using goodness-of-fit*). On the second day, talks were given by Sascia Pavan (*Indeterminacy and truth-functional connectives*), William Starr (*Conditionals, questions and content*), Luca Tranchini (*Proofs and refutations: new directions in proof-theoretic semantics*) and Antonino Freno (*Hybrid random fields for scalable statistical learning*).

Epistemic Preliminaries: Normative Priorities and Neuropsychological Kinds

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ABSTRACT

As noted in the introduction to this volume, "many foundational assumptions continue to be unexpressed", and many foundational issues remain unresolved, if not neglected, in the epistemology of psychology. I focus on two such sets of issues. Briefly, they are: (1) the resolution of conflicting norms (epistemological, clinical, ethical, others), and (2) taxonomic issues, broadly construed. It is not my purpose to suggest resolutions of these issues, but rather, to highlight their importance to a variety of topics that arise in the epistemology of psychology. Though it is difficult to say with certainty, given the unsettled nature of various areas of concern, I suspect that progress on these issues may be necessary, or conceptually prior to the satisfactory treatment of various issues in the epistemology of psychology and related fields.

Many metaphysical and epistemological issues at the intersection of philosophy, psychology and neuroscience remain unresolved. Addressing these issues, I argue, may be preliminary to any resolution or progress in these fields. In what follows, I focus on two such unresolved areas of concern. It is not my intent that I will resolve these issues; rather, I aim to justify a case for more attention to these problems by showing how central they are to a variety of areas, particularly the epistemology of psychology. The problems I highlight are: (1) hierarchies and conflicts among norms, including epistemological, clinical, ethical, sociological and other norms, and (2) taxonomic issues, broadly construed, particularly those involving psychological and neuroscientific taxonomy.

With respect to the first issue, it is useful to give an example of a normative conflict. One example arises in the wake of some studies (e.g., Alloy and Abramson 1979, 1988) which suggest that depressives are "sadder but wiser", or that depressives make more accurate, realistic predictions than non-depressives. Similarly, work by Taylor and Brown (1988), suggests that those who maintain an unrealistically positive self-image are happier and more productive than those who don't. As I and others have noted elsewhere (Flanagan, 1991; Kinney, 2000; Mundale, 2004) such work reveals a potential conflict between epistemic norms, or norms of rationality, and clinical norms. As Kinney (2000) comments, for example, such results prompt us to ask whether epistemologically realistic worldviews are desirable for emotionally distraught patients, and whether some kinds of cognitive errors may serve some kind of palliative or adaptive purpose. This potential conflict puts the standards of mental health and the productivity and satisfaction associated with it at odds with epistemic norms of justified belief.

While this example shows a potential conflict between epistemic and clinical norms, there is also the possibility of a conflict between epistemic and other psychological norms. The latter is highlighted by the recent moves in cognitive science toward approaches that emphasize the situated, embodied and embedded condition of knowers, on the one hand, and naturalized

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epistemology, on the other. Both approaches emphasize the role of heuristics and biases in our thinking, and, similarly, both underscore the non-optimal or "satisficing" strategy that is now thought to be typical of human thinking (see, for example, Herbert Simon, 1957, 1978; Cherniak, 1990). In this way, epistemic standards of rationality that describe how humans ought to think and reason may ultimately conflict with psychological norms that describe how humans actually think and reason, including common cognitive errors, biases and processing limitations. This potential conflict poses difficulties for establishing criteria for rationality; is one to judge according to a theoretical, largely armchair, "maximizing" standard of traditional epistemology, or by an empirically derived, "satisficing" standard that falls within the guidelines describing how human beings actually think? Philosophers continue to educate themselves and their students about common reasoning errors, ever watchful for the attribution error, informal fallacy, availability and representativeness biases, stereotyping, and other breakdowns in reason. At the same time, the naturalistically-oriented philosopher informs students not only of their apparent inevitability, but of their psychological explanation and possible heuristic utility, as well (see, for example, Scott Plous, 1993, or Gary Kirby and Jeffery Goodpaster, 2006, for this combined approach to critical thinking).

Similarly, naturalized ethics (ethics guided by psychological realism) has heightened the potential for conflicts between ethical norms and both epistemic and clinical norms[1]. What if being ethical does not coincide either with being rational or happy? What if the standards for ethical decision making require unrealistic expectations of our cognitive capacity? Flanagan discusses the potential for such conflicts, citing act utilitarianism as an example of an ethical theory that is psychologically unrealistic in that it requires constant evaluation of all possible actions along with their possible consequences in an attempt to maximize the greatest net happiness (Flanagan 1991, pp. 32-34). The unwieldiness of the hedonic calculus is a longstanding criticism of act utilitarianism that Flanagan puts into the broader context of a naturalized ethics that requires normative theories to fulfill the conditions of minimal psychological realism. Optimal rational norms are not consistently, if ever achievable to the extent required by the ethical norms of utilitarianism. The normative requirements of Kantianism are also notoriously unrealistic in presuming a kind of perfect rationality; one must, for example be able consistently to determine which maxims it would be reasonable for *everyone* to follow, and one's self-knowledge must be infallible enough to discern which maxims are guiding one's actions. Also importantly, for the present purposes, Kant famously drove a wedge between happiness, on the one hand, and ethics, on the other, opening the possibility of a conflict between the standards of ethics and those of psychological health and happiness.

Of course, there are some worldviews in which the potential for such normative conflict is greatly diminished, or at least not inevitable. In the Aristotelian system, for example, epistemic and ethical norms go together, and both coincide with the good life, or a happy, flourishing life. Being virtuous is tantamount to functioning well (that is, reasoning well), which is tantamount to employing one's practical wisdom in choosing the mean, relative to one's self. A life lived consistently within the mean conduces toward happiness, or eudaimonia, the end of

[1] Obviously, I reject the argument that naturalized theories, particularly naturalized epistemology, cannot be normative. Arguments for and against naturalized theories themselves go beyond the concern of this paper, but in short, I accept a cooperative version of these naturalized theories such that empirical discoveries about psychological and neurological limitations and capabilities are relevant to the study of human ethics and epistemology.

human life. The standard criticism of Aristotelian virtue ethics is the commitment to essentialism; in order for the elements of Aristotle's ethics to cohere as they do, one must think of humans beings as having a defining human nature or essence, or as being possessed of certain fixed traits, such that the life of moral goodness stems from the exercise and development of our distinctive human essence.

While I am not concerned to defend or criticize any particular ethical theory, there is surely the likelihood, if not inevitability, that normative conflicts will arise, and it is puzzling to know how to resolve them. Somewhat gloomily, Flanagan writes that, «This is not the best of all possible worlds. Happiness, goodness, and psychological health are not inexorably linked.» (Flanagan 1991, p. 332) It is noteworthy that Flanagan reaches this conclusion at the end of a lengthy, detailed work that develops and defends a naturalized (psychologically realistic) approach to ethics. How are we to adjudicate such conflict?

The conflict is one that Flanagan appears prepared to live with, even if grudgingly. Yet, it is worth considering the available options. First, should we accept normative conflict, or should we regard it as a sign that we have at least one normative account wrong? Recalling the above example, Graham disputes the claim that depression must necessarily be an indication of irrational thinking. In fact, he argues that, «…were a person immune to depression in justifiably depressed circumstances, I think we should be inclined to think of him as psychologically deficient.» (1994, p. 419) As he further explains

> If you became severely paralyzed in an accident, the fact that you are paralyzed might be a melancholic truth, an important even if depressing reality, which you must appreciate if you are to make intelligent decisions about the future. How sympathetic could we be to your plight if we described your reasoning as illogical? Should we dissuade you from believing you are paralyzed? Should we deny your future is bleak? (Graham 1994, p. 412).

Faced with a justifiably depressed person, then, should we add further insult by describing him or her as irrational, as well? One could, of course, claim that this is not an example of any hard choices between the norms of psychological health and those of epistemic virtue, in that facing a depressingly bleak future honestly and rationally is a necessary condition for eventually overcoming that depression. This kind of accommodation, for example, might result from adopting a different model or standard of rationality, but not all cases of apparent conflict can be argued away.

To return to the available options, suppose we decide that conflict among norms is an example of what philosopher Nicholas Rescher termed an «aporetic structure»; a set of individually plausible assertions that, taken together, are mutually inconsistent. If one cannot resolve the inconsistency through re-interpretation of the assertions, then, one or more assertions must go, and, furthermore,

> any particular way out of an aporetic conflict is bound to be simply one way among others…. there will always be a variety of distinct ways of averting the inconsistency into which is plunges us. And in this light, the problem for the philosopher is not one of inductive ampliation but of systemic reduction – of a restoration of consistency through choices of priority. (Rescher 2001, p. 100).

Rescher's preference for resolving inconsistencies among philosophical doctrines is clearly to choose and eliminate, rather than to accommodate, as «competition and controversy» governs mutually exclusive doctrines, not «mutual supportiveness» (Rescher 2001, p. 101). If this is how we approach normative conflict, does this interpretation not commit us, at the outset, to the precedence of traditional, epistemological norms? Perhaps even to recognize

potential normative conflict as a subject worthy of attention is to privilege epistemic norms. In other words, what meta-normative position does one take in even approaching the question of how to resolve normative conflict? It's not quite that it leaves us on Neurath's boat so much as it leaves us wondering how to get it out to sea in the first place.

If we decide to take the traditional approach and put consistency ahead of all else, that does not, of course, tell us which norms to privilege or which trade-offs to make. In contrast to Rescher's recommendations, Flanagan's solution suggests a "satisficing" strategy of seeking the very sort of mutual supportiveness Rescher warns against. Flanagan claims that while we cannot have the best of all possible worlds, «There do exist, however, some relations among the three concepts [happiness, goodness, and psychological health], some patterns of co-occurrence, which we can seek to amplify…», and that «Gaining as much coordination as is possible among things is a project requiring human effort suited to its particular time and place and with no guarantees of success» (Rescher 2001, 332). At the moment, we do not even seem to have a clear idea of what would count as a successful coordination among these three, let alone their successful coordination with a fourth element of epistemic virtue.

Apart from the puzzle itself, the clash between standards often forms the basis of a criticism, if not rejection, of a given theory. Condemning one normative theory because it conflicts with another normative theory (or theories) really only pushes the question back a stage further into one's overall normative scheme. It prompts us to ask if one's overall system is one in which normative conflict can be avoided, as in some species of virtue ethics, or if some kind of approximate cooperation can be achieved (as Flanagan describes), and if neither of these, what hierarchies can be defended to support the rejection or accommodation of one norm in the face of conflict with another? It may not be enough, for example, simply to condemn one theory (a clinical theory, for example) simply because it established norms that conflicted with another (an ethical theory, for example), without stating one's larger normative assumptions. If conflict is inevitable among those assumptions, the criticism is more telling if placed within the defense of one's larger, normative hierarchies or coordination.

The second set of issues I consider also has to do with unstated or perhaps unfounded assumptions at the basis of many common criticisms. These have to do with taxonomic issues, broadly construed. Of the pair, perhaps this area has been the most productive source of difficulties, because various methodological gambits have been placed that have depended, in one way or another, on a resolution that has yet to happen. Moreover, these difficulties have been lurking behind the scenes since natural scientists first began to localize psychological function in the brain.

For example, the feasibility of various strengths of identity theory and reductionism, as well as related arguments concerning localization and realization, rely on taxonomic assumptions about the psychological and neuroscientific domains that are usually left unstated, if not altogether unexamined. In the absence of some preliminary classificatory considerations, arguments that depend upon them are nothing more than programmatic guesses. Consider, for example, the unfortunately cast distinction between type vs. token identity theory which has framed the debate about theoretical reduction since the latter half of the twentieth century.

In the now-familiar type vs. token language, types are typically taken to refer to kinds, types or classes, and tokens are held to be specific members of the kind or type of class. (The choice of this terminology, borrowed from its original linguistic context - where it is clear, unlike the application in question - is part of the trouble, as discussed below). For the type identity theories, all instances of a particular type of psychological type (such as having a pain) are identified with instances of a correlated type of neural event (such as an activation pattern in a

particular area or set of areas). Historically, type identity theory was most famously advanced by such philosophers as Smart, Place and Armstrong, though, with some evolution and sophistication of the theory, it has many contemporary stalwarts as well; chiefly, Patricia and Paul Churchland.

In brief, type identity theory is a much stronger claim than that made by token identity theorists, who claim that, while every token of a mental state is a token of a neural state, there is no identity between types of mental states and types of neural states. In other words, for the token identity theorist, whenever one is in a given mental state, that mental state is associated with some brain state or other, but on other occasions, when one is in the same mental state, one might well be in a different brain state. Alternatively, the token identity theorist might also claim that one and the same brain state may, on separate occasions, correspond to different mental states. Since the token identity theorists agree that a given mental event is identical with some physical state *or other*, another way of looking at their position is to say they identify the very broad type - mental events - with the very broad type – neurological events. Thus, an identification (or correlation) for them amounts to nothing more than an identification of a token of the type mental event with some token of the type physical event; this is, after all, a minimalist form of physicalism.[2] This form of identity theory has most notably been held by functionalists such as Fodor and Putnam.[3]

Functionalists of this stripe argue against the type-to-type, or one-to-one mapping relation of type physicalism. If this mapping relation can be successfully refuted, they argue, then type physicalism must be false and functionalism (token physicalism), which doesn't suffer from this problem, wins. One version of this attack claims that different physical kinds may manifest or realize the same psychological state; this argument is known as multiple realizability. The other version claims that different psychological states may be manifested by the same physical type (multiple functionality), though this version is less common. Fodor and Block summarize the first kind of argument as follows:

> The argument against [type] physicalism rests upon the empirical likelihood that creatures of different composition and structure, which are in no interesting sense in identical physical states, can nevertheless be in identical psychological states; hence that types of psychological states are not in correspondence with types of physical states. (Block & Fodor 1980, pp. 237-238)

[2] Though Fodor, perhaps in an unguarded moment, claims that "Token physicalism does not rule out the logical possibility of machines and disembodied spirits having mental properties" (Fodor 1981, p. 127), thus obscuring his claim to *physicalism*.

[3] For the purposes of this essay, I have treated token physicalists as functionalists, since they tend to run together as one finds them. Technically, of course, one doesn't necessitate the other. Functionalists require that mental states be categorized by function, or according to specified input/output relations, rather than by their physical correlates. In principle, this is consistent with a variety of theoretical relations between neuroscience and psychology. Similarly, nothing about being a token identity theorist requires that one assent to a strictly functional categorization, even though token physicalism denies that there is a productive means of physical categorization. Most functionalists, such as Fodor and Putnam, believe that a physical categorization of mental states is either unobtainable or, at best, non-fruitful. Type physicalism, by contrast, is compatible with either reductionistic or eliminativistic materialism, whereas token physicalism is not, since it denies the correlation of mental types with distinct neurological types.

The problem, of course, is that the truth of the claim becomes a matter of careful example picking (see also Mundale, 1997, Mundale and Bechtel, 1999). I will elaborate upon this further, momentarily.

Consider first, the terminological indefiniteness that is let in with the very characterization of these two different forms of identity theory. To take Fodor as a representative example, he writes:

> The identity theory can be held either as a doctrine about mental particulars (John's current pain or Bill's fear of animals) or as a doctrine about mental universals, or properties (having a pain or being afraid of animals). The two doctrines, called respectively token physicalism and type physicalism, differ in strength and plausibility. (Fodor 1981, p. 127)

As mentioned in the beginning of this section, language such as this is unhelpful unless accompanied by some sense of what the mental particulars and universals *are*. Apart from such a context, how are we to take, for example, "Bill's fear of animals"? Fodor lists this as an example of a mental particular or token. If this is just one of many of Bill's fears (of clowns, of heights, of elevators, etc.), then it looks like a particular. Yet, if Bill fears several animals, such as lions and tigers and bears, then isn't Bill's fear of animals better regarded as a kind? The taxonomic difficulties, of course, extend beyond a given case such as this, to considerations of the entire psychological and neuroscientific domains themselves.

To return to the matter of careful example picking, above, Putnam's famous octopus case of multiple realization, apparently, still attracts support. In its original form, it reads:

> the brain state theorist [type-identity theorist] is not just saying that *pain* is a brain state; he is, of course, concerned to maintain that *every* psychological state is a brain state. Thus if we can find even one psychological predicate which can clearly be applied to both a mammal and an octopus (say 'hungry'), but whose physical-chemical 'correlate' is different in the two cases, the brain state theory has collapsed. (Putnam 1967, p. 44).

As a matter of careful example picking, this is a prime example. As Bechtel and I argued ten years ago (Bechtel and Mundale, 1999), the apparent success of the multiple realizability argument depends, in part, on choosing examples so as to mismatch "grain size", or levels of analysis. Psychological states are chosen at a coarse grain, or gross level of analysis (e.g., "hunger"), and coupled with brain states chosen at a fine grain, or micro-level analysis.

The lack of context in which such mismatches occur gives license to any number of artificially concocted examples of multiple realization and/or multiple functionality. It is useful to see another example of this line of thought. It would normally be unfair to cite someone's views from 1974 about the usefulness of neuroscientific research to psychology, but (1) the author's views remain consistent on this point, (2) they remain influential for many philosophers of mind and (3) my focus is on the language and underlying assumptions of the debate, rather than on this particular conclusion, and those also remain part of the contemporary discussion:

There are departments of psychobiology or psychology and brain science in universities throughout the world whose very existence is an institutionalized gamble that such lawful coextensions [between psychological and neurological kinds] can be found. Yet, as has been frequently remarked in recent discussions of materialism, there are good grounds for hedging these bets. There are no firm data for any but the grossest correspondence between types of psychological states and types of neurological states, and it is entirely possible that the nervous system of higher organisms characteristically achieves a given psychological end by a

wide variety of neurological means. It is also possible that given neurological structures subserve many different psychological functions at different times, depending upon the character of the activities in which the organism is engaged. In either event, the attempt to pair neurological structures with psychological functions could expect only limited success. (Fodor, 1974, p.125)

Though Fodor's and Putnam's examples have been rehashed numerous times in the philosophical literature, references to psychological types, tokens, kinds, predicates, particulars (the terminology varies considerably) remain a blank check for nearly any claim one would wish to make about the relation between psychology and neuroscience. Fortunately, despite the injunction of multiple realizability against any such usefulness, neuroscientific research is forging ahead with correlations between neural activations and psychological function, fleshing out the nomological bridges between the two disciplines.

In addition to the problems generated by the nascent state of psychological and neuroscientific taxonomy, there are related problems concerning the identity conditions of mental states and the manner of their individuation. As the foregoing discussion reminds us, philosophers commonly refer to the condition of being in the same mental state or the same brain state, either from one occasion to the next in the same being, or between two different beings. As discussed above, the strength of various identity claims, for example, turns on such considerations as whether one and the same brain state could subserve more than one psychological state, or whether one and the same mental state could be subserved by different brain states. As with the the notion of types, discussion about being in the same state, whether mental or physical, assumes that we can supply some account of the identity conditions for mental events and brain events, though this is not the case. Supposedly, the famous hungry octopus and I can be in the same mental state (or, as it was originally put, share the same psychological predicate of "hungry") while being in different physico-chemical conditions at the same time. Of course, Putnam does not give us any specific criteria for what would count as the same (or same *enough*) *physical* state. He doesn't, and neither does anyone else to my knowledge. It is as if Putnam were assuming that neuroscientific taxonomy was complete, with all its different kinds and instances already sorted out for us. Yet we know differently.

A related problem, of the individuation of states, also floats untethered to any underlying explanation. Where does the mental state of hungry actually begin? In the intervening hours after one meal, do we gradually come to be in the single state of hungry that lasts until our next meal, or do we pass through several distinct mental states, ranging from mildly hungry to famished?

There is nothing predetermined about the level of abstraction at which the type physicalist is required to work, in order to be a "type physicalist". It is reasonable to conclude that the levels of the correlations and the correlations themselves are empirical matters, best left to psychology and neuroscience. Also, as I have emphasized throughout this section, *there is no a priori determination of which psychological and neurological entities are to count as types (universals), nor of the tokens which are to be subsumed under them*. Since the success of the general argument against type physicalism depends on how one construes types and tokens, it is obvious that one must give some preliminary taxonomic basis for how to construe them. Depending on one's other theoretical commitments as well as the state of the art, one might arrive at several different taxonomies; nothing necessitates that there be just one (Mundale, 1997).

Brain mapping and its associated developments in neuroscientific taxonomy is an actively evolving project. Among the possible grounds for division are: evolutionary/phylogenetic typologies, developmental ones, clinical, psychological, cognitive, physiological, sensory, motor and others. There is also a range of levels at which one might classify, ranging from the level of

the individual neuron (or even lower, as with neurotransmitters, for example), to ganglia, lamina, systems, and central nervous system. There are also multiple methods and technologies for individuating brain areas. In some cases, these different systems may fail to neatly coincide with each other. In short, at least at this stage, there is no definitive taxonomy of neurological "types" and "tokens".

In psychology, taxonomy appears to be even messier. We lack both vertical classification (the categories under which psychological states are organized) as well as horizontal classification (an account of the members of a given psychological category). Clearly, if a type-type correlation fails, the problem may not be type physicalism at all, but rather with the typological system(s) at either end. It is easy to find psychological types which *do* map neatly onto a their neuroscientific correlates, and it is also easy to find cases of those that don't. In sum: as it has historically been stated, the case of the functionalists against the type physicalists is entirely founded upon picking the right examples. In order to have a more principled discussion, it is necessary to have an at least preliminary taxonomy of the mental and the neurological. The argument could then proceed to issues such as: Whose taxonomy is more successful (predictive, theoretically fecund, robustly confirmed, etc.)?, and whose claims about the level of identification between the mental and neurological domains are better, given the human behavior, cognitive processes, etc., we are trying to explain? If all we can do now is place our bets, then based on early showings, I'm putting my money on the neuroscientists.

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Scientific Psychology: Should we Bury it or Praise it?♦♣

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ABSTRACT

In 120 years since William James published his *Principles of Psychology,* scientific psychology has made great strides. Yet James' concerns about the lack of coherence of psychology have continued until this day. An analysis of the current disciplinary terrain suggests that large parts of contemporary psychology are being absorbed by the newly emerging fields of cognitive science and neuroscience. Social psychology may become part of a broader field of cultural studies, while other subfields that have primarily an applied status. What will remain central to scientific psychology is a concern with those issues that were pivotal for William James — self, will, consciousness, and personality. This enduring "core" of psychology may benefit from deeper and more extended interactions with literary and other artistic studies.

In 1987, I was invited to deliver an address to the American Psychological Association. I decided to use this occasion to air some of the misgivings that I had about the status of psychology as a coherent scientific discipline. Not surprisingly, the talk received its share of criticism; and when I prepared a written version of the talk, it was rejected by a few mainstream psychological journals. Fortunately, a recently launched publication, *New Ideas in Psychology*, agreed to publish the critique in full and it generated a modest amount of discussion in the ensuing years. The invitation to contribute to the present volume has given me the opportunity to revisit the argument put forth over two decades ago.

WILLIAM JAMES' ASPIRATIONS AND CONCERNS

One and a quarter century ago, the esteemed philosopher-turned-psychologist William James signed a contract to write the first American textbook in psychology. As he wrote to his friend Thomas W. Ward, "I have blocked out some reading in physiology and psychology. It seems to me that perhaps the time has come for psychology to begin to be a science" (quoted in Feinstein, 1984, p. 313). As is well known, a task slated to be completed in two years dragged on for a dozen years, but in the end James had expounded the subject in a way which has seldom if ever been equaled. Certainly it is difficult to think of any other textbook which is read not only for pleasure but also for profit well over a century after its initial publication.

In writing his unsurpassed *Principles of Psychology* (1890), James sought to share his vision of psychology and its relation to physiology, on the one hand, and to philosophy, on the other. He

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♦ This paper was presented initially as an invited address to Division 1 of the American Psychological Association upon the author's receipt of the William James Award. I wish to dedicate this paper to the memory of Henry A. Murray, with whom I was privileged to discuss its contents shortly before his death in June 1988.

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was well aware that these fields of knowledge did not yet fully cohere with one another, but he thought that it would soon be possible to bridge the region «lying between the physical changes in the nerves and the appearance of consciousness (in the shape of sense perceptions)» (Feinstein, 1984, p. 313). James composed memorable chapters on what are now the familiar staples of any psychology text: the senses, emotions, attention, memory, reasoning, and perception. Yet it is probably the central chapters — on the Stream of Consciousness and on the Self — that constitute his most distinctive contributions.

James' opus immediately drew praise among psychologists as well as scholars in related fields. In light of the subsequent fragmented history of psychology, the comments by two colleagues strike me as particularly apt. First there is James' first student and close associate in psychology, G. Stanley Hall:

> The author (James) might be described as an impressionist in psychology. His portfolio contains sketches old and new, ethical, literary, scientific, and metaphysical, some exquisite and charming in detail and even color, others rough charcoal outlines, but all together stimulating and suggestive, and showing a great industry and great versatility. This is through and through *a tendance* book. Its very inconsistencies and incoherences not only reflect but greatly magnify all the unrest, distraction, and conflicts of the present hour. (quoted in Knight, 1954, p. 43)

Then his long-term colleague in philosophy, George Santayana commented:

> It would be pedantry to regret the loss of logical unity in a book so rich and living, in which a generous nature breaks out at every point, and the perennial problems of the human mind are discussed so modestly, so solidly, with such a deep and pathetic sincerity. (quoted in James, 1963/1892, p. xi)

Since the initial appearance of the *Principles,* tens of thousands of works in psychology have been published; psychology as a discipline—academic and practical—has achieved remarkable success. The flagship publication *Psychological Science* received 1800 submissions in 2007. Yet, it is still not clear to many observers that the promise implied by a two-volume text in a new field called psychology has actually come to fruition. Clearly advances have been made in many, if not most, of the topics treated by James and his immediate successors. But have these advances added up to a unified discipline whose components interrelate with one another? Are they worthy to be called a science in the same sense that biology, chemistry, and physics — or, for that matter, economics or demography — merit that label? Are there serious attempts to tie together the “micro” and “macro” levels as are currently underway in the biological and the physical sciences?

In treating the possibility of psychology as a unified science, I am discussing a topic that William James would have found of interest. He himself had often voiced misgivings about the “confused and imperfect state” (Perry, 1935, vol. I, p. 40) and the “ante-scientific condition” of psychology (Allen, 1967, p. 315). In my view, James' concerns have proved all too justified. Psychology has *not* added up to an integrated science, and it is unlikely ever to achieve that status. It no longer makes sense to discuss scientific psychology as a tenable long-term goal. What does make sense is to recognize important insights that have been achieved by psychologists; to identify the contributions which contemporary psychology can make to disciplines which may some day achieve a firmer scientific status; and finally to determine

whether at least parts of psychology might survive as participants in a scholarly conversation that obtains across major disciplines.

THE DREAM, THE DOUBTS, AND THE OPTIONS

In 1780 Immanuel Kant developed arguments which purported to show that a scientific psychology was not possible. The redoubtable philosopher identified three apparently insuperable obstacles: the mind is inherently affected while studying itself; there is nothing of spatial extent which can be studied; and there is no mathematical basis on which a science can be constructed. Kant concluded in magisterial terms:

> Psychology can, therefore, never become anything more than a historical (and, as such, as much as possible) systematic natural doctrine of the internal sense, i.e., a natural description of the soul, but not a science of the soul, nor even a psychological experimental doctrine. (quoted in Watson, 1979, p. 88)

Postulating the impossibility of a field *a priori* is a risky thing to do. In the following century, such formidable scientists as Hermann von Helmholtz, Gustav Fechner, Johannes Mueller, and Wilhelm Wundt devoted their considerable powers to the refutation of Kant's dictum, thus laying the groundwork for James' work and for the potential emergence of a science of psychology.

In the second century after Kant's dismal epitaph, psychology conquered much of the academic world. What followed might be called, in the argot of Chinese dynasties, the Period of the Warring Schools: we had functionalism, behaviorism, structuralism, Gestalt Psychology, learning theory, psychoanalysis, and a pack of other “isms”; we hosted the movements surrounding magnetic scientists like James J. Gibson, Clark Hull, Jean Piaget, and B. F. Skinner; and we experienced a number of worldly successes, such as the intelligence test, various indices of psychopathology recorded in successive editions of the DSM manual, and the integrated commercial sphere that spans persuasion, advertising, and marketing. Psychology has become established as a potent societal force, with its departments, journals, institutions, and huge organizations. Within the United States the most prominent among them is the 150,000 member American Psychological Association (APA); but since 1988 there is a rival group, now called the Association for Psychological Society (APS), having 20,000 members, which styles it as a scientific alternative to the more ecumenical and more clinically oriented APA.

At least on the level of lip service, the dream of a unified psychology continues. It appears at the beginning and end of textbooks, though much less frequently in the intervening chapters. It surfaces as well in university catalogues and in the boilerplate statements of granting agencies. And occasionally, a scholar — more often an outsider or “independent researcher” than a practitioner of “normal science” — actually proposes a formula or “central dogma” for the field—one that purports to link all subfields and to bind the “micro” with the “macro” (Cook, 1986). But for the most part, psychologists (like other academics) go about their daily research and writing without agonizing about the actual or potential coherence of their field.

Occasionally, as exemplified by this volume, there has been explicit concern with the fragmentation of the discipline. Issues of the *American Psychologist* (Bickman & Goodstein, 1987) and *New Ideas In Psychology* (Baer, 1987; Bakan, 1987; Krantz, 1987; Royce, 1987; Toulmin, 1987) have been devoted largely to this topic. The debate has been framed in terms of "centrifugal" as opposed to "centripetal" forces in the field; about alternations between "cohesion" and "splintering" from one decade or from one generation to another; about system-building as opposed to tending one's own little settlement. Controversy has centered on whether physics, biology, or some other discipline should serve as a model for psychology; and whether we should deplore or revel in our pluralism. No less an authority than Sigmund Koch (1981) devoted much of his career to this nexus of issues and over the years assumed an increasingly pessimistic note.

These and other voices call into grave doubt the possibility—in theory or in practice—of a unified scientific psychology. The evidence of the past century gives little consolation to those who would like the textbook vision to become a reality. In what follows I focus on possible reactions to this state of affairs and then propose one plausible topography for the coming decades.

In as much as psychology gives little sign of cohering, we are faced with the following options:

(1) We can simply close our minds to the possibility of disciplinary extinction and continue what we have been doing. No Super-body is likely to announce Psychology as a fraud, and so we can maintain the status quo.

(2) Following a well-known suggestion made with reference to the Vietnam War by the late Senator Aiken of Vermont, we can simply *declare* that psychology is a success — as it has been, according to many criteria — and swallow any lingering doubts which we might entertain.

(3) We can hope that we are simply passing through a temporary phase of fragmentation and that some enterprising researcher, or some brilliant theorist, will discover the "golden thread" that will unify our field.

(4) We can claim that there has been an unjustified romanticization of other disciplines. After all, there are many numbers of subfields of biology: the geneticists or molecular biologists inhabit quite different worlds from the evolutionists, taxonomists, or paleontologists. At one time, my university had nine different departments of biology: and economics is at least as top-heavy as psychology with schools that struggle against one another.

There are certainly other options, but I favor a fifth. Let us recognize that fields of science evolve, often in unsuspected and unexpected ways. Nearly every field of knowledge begins as philosophy; and psychology continues to foreground its philosophical origins more faithfully than any other discipline. There was a period two centuries ago when psychology seemed impossible; a set of discoveries in the nineteenth century which established a number of enduring psychological paradigms and concepts; a complex of social and historical factors in the twentieth century which earned psychology a place in virtually every academic environment.

Still, while psychology was developing, so were other fields of knowledge. It is against the background of other evolving disciplines that psychology must be understood and located. In the last century, psychophysics — once the core of psychology — was slowly assimilated into engineering and computer science; and more recently, the study of animal behavior within psychology has been complemented by work from an ethological perspective. Certain approaches—such as simulating thought and behavior via neural nets (Rumelhart and McClelland 1986) — and certain thematic emphases — such as those of positive psychology — have recently come to the fore (Seligman 2004). It is my contention that what we presently call psychology has already begun to be absorbed by a number of more fundamental disciplines, some more scientific (in the classical sense), some less so. The option I favor is to discern traditional psychology's place(s) within this emerging topography.

THE EMERGING DISCIPLINARY TOPOGRAPHY

Roughly paralleling breakthroughs in physics in the decades following the beginning of the last century, and the parallel advances in molecular biology at mid-century, the years at the close of the 20th century can be well described as the coming-of-age of brain- or neuroscience. At every level of the nervous system, from the individual synapse to the blood-flow patterns through the entire cortex, our knowledge has accumulated at a phenomenal rate. Those areas of psychology which were traditionally called physiological psychology and comparative psychology, as well as large portions of what are called sensation and perception, are rapidly becoming the concerns of neuroscientists. Indeed the first nine chapters of James '*Principles'* (I refer hereafter to the shorter version of that text) would all fit comfortably into a basic neuroscience course.

I should stress that I am not endorsing a reductionist position. The phenomena of sensation, perception, or other psychological states will never be reducible to “an account in terms of brain states.” As is well indicated in the pioneering work of neurophysiologists like David Hubel and Torsten Wiesel (Hubel, 1979), the categories and the level of psychological analysis will continue to be essential not only in ordinary discourse but also in the work of practicing neuroscientists. However, in my view, psychologically-trained individuals will increasingly take their places as members of research teams that are probing the structure and functioning of the nervous system. The perceptual psychologist or psychophysicist working in isolation is gradually becoming an anachronism.

If neuroscience will absorb much from the “lower regions” of psychology, an analogous kind of raid will be made by cognitive science — perhaps from the “top”, perhaps more laterally (Gardner, 1985). This emerging branch of science is a self-styled interdisciplinary field which, like traditional psychology, seeks to uncover the basic processes of thought; however, adopting the current vogue, cognitive scientists regard the computer as the most suitable model for all forms of cognition.

While several disciplines are candidates for membership in an ultimate cognitive science, until this point researchers in psychology and of artificial intelligence have been particularly central in cognitive-scientific endeavors. Many of the concepts and paradigms in cognitive science come from psychology, while the methods of research and other key concepts stem from computer science, especially artificial intelligence. Among contemporary areas of psychology, the fields of attention, memory, reasoning, problem-solving, and the “higher forms” of perception and psychophysics are most closely affiliated with cognitive science. Parts

of developmental, educational, and neuropsychology will fit comfortably under the cognitivist label as well. Most of the remaining chapters in James' briefer text, beginning with Chapter 10 on Habit and concluding with Chapter 22 on Reasoning, would also find a proper place in a text of cognitive science.

In the case of cognitive science, there is little danger of a reductionism that will exclude psychological analyses. (When neuroscience is included within cognitive science, it typically assumes a non-reductionist guise in that company.) A greater risk is that, in coming up with a core computational theory, researchers may short change those aspects of reasoning or problem-solving that are characteristic of humans rather than mechanical objects (Dreyfus, 1972). Also undetermined at present is the issue of whether the various subfields of cognitive science — for example, perception, attention, memory, reasoning — will prove any less unwieldy when thought of in computational terms than they were when conceptualized in traditional psychological frames. The dispute about the appropriateness of parallel-distributed-processing models, as against von Neumann symbolic models, indicates that cognitive science may inherit psychology's woes (Pinker & Prince, 1988).

When I wrote my original paper, neuroscience and cognitive science stood as the two behemoths, threatening to absorb many settlements of science, including the mainstream of research in psychology. In the last quarter century, the two subfields have largely merged with one another. Nearly all cognitive scientists think now in terms of brain modeling and many of them actually use the tools of neuro-imaging. And neuroscientists, who once spurned cognitive terms and labels, now regularly immerse themselves in cognitive models. Other fields of psychology, such as social psychology, developmental psychology, or clinical psychology are less “at risk” of immediate absorption; possibly because they lack easily transportable research paradigms, they can continue to evolve with less threat of a takeover by an interdisciplinary “corporate raider.”

My remarks about these latter fields are even more speculative, but I will venture a few words about their possible fates. I see social psychology as continuing to produce striking demonstrations about human social behavior — the kinds of findings associated in the past with researchers like Solomon Asch, Leon Festinger, Fritz Heider, Stanley Milgram, and Muzafer Sherif — and more recently, with work like that of Richard Nisbet, Ellen Langer, and Anthony Greenwald. As impressionistic and suggestive as these findings can be, I do not see them adding up cumulatively into a cohesive science. Indeed, most are more likely to find their way into a general cultural discipline — including sociology, anthropology, and social psychology — than to be absorbed into more classical or aspiring sciences like neuroscience or cognitive science. Still, I note the rowing influence of a field sometimes dubbed social cognitive neuroscience (Damasio 2000; Greene 2003).

A number of current fields or subfields should continue to evolve without serious diversion. I have in mind here subfields like educational psychology, industrial psychology, and clinical psychology. For the most part these areas do not aspire to the status of “pure sciences.” Rather, they are applied fields, which use methods and findings from basic research in the service of problems that arise at the workplace, the market place, the school, or the clinic. They will maintain communication with neuroscience, cognitive science, and cultural studies, but not to the point of being integrally affected by scientific advances, disputes, or “takeovers” in areas with imperialistic design.

Clearly aspects of developmental studies are relevant to each of the four fields so far mentioned; neuroscientists must concern themselves with basic laws and stages of development; cognitive science will examine the development and breakdown of thinking capacities; cultural studies will feature a component dealing with the behaviors of children in different social and cultural contexts; and part of developmental psychology will continue to be intermingled with educational and clinical psychology.

In the past a number of major thinkers, such as Heinz Werner, Jean Piaget, and Jerome Bruner, have articulated the vision of an over-arching developmental science, somewhat along the lines of cognitive science, which would include material from neurology, physiology, evolution, life-span studies, child psychology, psychopathology, and perhaps even the study of different scientific disciplines (genetic epistemology). As a card-carrying developmentalist, I find this vision appealing. But I must note that pursuit of this vision has almost completely vanished in the most recent decades.

THE SURVIVING CENTER

It may seem that, in this Cook's tour of the disciplinary topography of the future, we have drifted far away from William James and his view of psychology. But that is only because I have yet to mention those subjects — and those chapters — that were central in William James' own account. I refer here to Consciousness — treated in Chapter 11; The Self — treated in Chapter 12; Will-the concluding substantive chapter; and Personality, which, while rarely mentioned explicitly by James, is in fact an important presence in these chapters.

For James, the issue of the self or ego — its experiences, its internal and social aspects, its aspirations, and its evolution through life — is key in psychology. James lived in the pre-Freudian era but had already intuited some of the issues which were to occupy Freud. And when he heard that Freud was coming to America, the ailing James made his way from Cambridge to Clark University Worcester, Massachusetts and declared to the visitor from Vienna, “The future of psychology belongs to your work.” As the historian H. Stuart Hughes commented, “there is no more dramatic moment in the intellectual history of our time” (1961, p. 113).

Since the time of James and Freud, the study of personality, self, will, and consciousness (hereafter, the “person-centered quartet”) has occupied a paradoxical position within psychology. On the one hand these topics are clearly central in any delineation of the field, and they occupy predictably pivotal spots in textbooks. And yet I must acknowledge there is a slight embarrassment about these topics. To be sure work continues on each of them, and many of the major figures in psychology have “had their say” on these topics. Also, there has emerged a consensus that personality can be described in terms of five principal factors: Openness, Conscientiousness, Extraversion, Agreeability, and Neuroticism. Nonetheless, in my view, progress here is less compelling than in other strands of psychology.

Interestingly, despite growing speculation about the nature of human consciousness, neither cognitive science, nor neuroscience, nor cultural studies has asserted dominance over these topics. I think this reluctance occurs not merely because these issues are difficult to study. I think it is because, rightly or wrongly, they are seen as central to psychology in a way that

nothing else is-indeed they could be seen historically as the defining features of psychology. Notably, these topics seem particularly resistant to decomposition, elementarism, or other forms of reductionism — and of course, the cannibalizing disciplines exhibit strong tendencies in this atomistic direction. Perhaps equally interesting, this definition might well be shared even in remote cultures. While failing to introspect about perceptual or cognitive processes, and displaying little interest in the study of other cultures or in stages of child development — preliterate societies do introspect and develop folk theories about the person and about personal experiences (Geertz, 1975).

If these fields are so central and yet have witnessed little progress, what can we expect of them in the future? I think that here we find a clue in the expansive psychologies of William James, Sigmund Freud, and Henry Murray. In one way or another, each of these scholars sensed an important truth: that the study of self or personality is at once a problem of psychology and the home ground of literature. In the examples they use and in the approaches they adopt, each researcher signaled the realization that the imaginative writer is tackling the same kinds of issues as the psychologist of personality. In James' case, of course, we have the lengthy and tortured relation with his brother Henry as well as frequent references to other writers and to literary examples; in Freud's case, there is his reliance on the great authors of the past — Sophocles, Shakespeare, Dostoevsky — for so many of his core concepts; in Murray's case, it is his deliberate appropriation of images from literature (e.g., *An American Icarus)* as well as his own pioneering scholarship on Herman Melville.

Literature constitutes an incredibly rich repository of information about human nature and personality, one that students interested in the “person-centered quartet” ignore at their peril. It is not in the least surprising that the three scholars cited here found particularly pivotal leads in the work of the great writers. But crucial insights about human nature are captured as well in other art forms, ranging from the visual arts to music to the dance. The focus in this discussion falls on literature but the same line of analysis can — and should — be extended to other art forms.

But if there is a relationship between the scientific study of personality and the writer's investigation of the world of his or her characters, just what should that relationship be? Should it be mutual support and regular communication? Should the psychologist attempt to locate the novelist's characters in his laboratory? Should the novelist draw explicitly or implicitly on the psychological theories and concepts of the time? Or, following Rorty (1979), should the conversation occur among psychologists and literary critics and theorists? Should the methods developed by literary theorists be appropriated by psychologists to help them in studying the ways in which the individual (reader or writer) conceives of and relates his life? Should psychological insights about memory, sense of time, or identification be utilized by students of literature to explain the ways in which fiction works for different readers or is produced by different writers? Or are any or all of these options fair game?

I remain uncertain just which form this collaboration should take and perhaps several forms deserve exploration. At the very least psychological investigators of the “person-centered quartet” ought to study works of art, including literature, with great care and test their portrayals against the claims of scientific study. Cooperative investigations among artists and psychologists could be very profitable, though the difficulty of such collaborations should not be underestimated. While the distance between psychologists and novelists might prove too

great, psychologists and students of literature can each enrich one another's pursuits. Indeed, they may provide examples and "limiting cases" for one another, the psychologist's precise methods and rigor being balanced by the literary scholar's broad view and skeptical cast of mind, particularly with respect to reductionism as in the five factors of personality. The psychologist's taxonomies and frameworks need to be tested against the rich range of characters found in literature and the powerful insights about the nature of text and of reading put forth recently by literary scholars. If the schemes of psychologists prove inadequate for dealing with these more rounded examples and concepts, then they need to be reconfigured or altogether scuttled. For their part, students of literature can benefit from a study of the way in which psychologists have conceptualized the human personality, operationalized these various conceptualizations, and tested certain tantalizing hypotheses about human behavior in the experimental laboratory.

It should prove possible for psychological writers and literary scholars to do more than read one another's publications. Here, indeed, I think that we can take an instructive leaf from colleagues in cognitive science and neuroscience. These fields have advanced in large measure because researchers reared in disparate disciplines work together shoulder-to-shoulder on problems of mutual interest. Topics like the nature and appreciation of irony, the appeal of fairy tales, or the power relations which obtain among individuals in Shakespearean plays, have already benefitted from cross-disciplinary investigations (Bettelheim, 1977; Brown & Gilman, 1989; Winner, 1988). Our own investigations at Harvard Project Zero have for some time benefitted from sustained collaborations among psychologists, artists, and experts in the systematic study of different art and literary forms (Gardner 1982; Gardner &Perkins, 1989; Winner, 1982, see also pzweb.harvard.edu). The knottiest problems in artistic analysis — such as the question of whether there might be *the* optimal interpretation of a work of art — call for interdisciplinary investigation.

Whatever collaboration eventually obtains among psychologists and individuals involved in literature and other art forms, one point seems clear. The part of psychology most likely to remain after the aforementioned cannibalizations have taken place is the study of the "person-centered quartet." Certain aspects of emotion and motivation may also elude the cognitive and neurosciences. These are topics for which psychologists may have special methods and insights; but they are equally the concern of writers and other artists, and of those who study them, like literary critics and theorists. No hard science â la physics is likely to emerge from the collaborations I envisage. But an interesting and highly useful kind of conversation between behavioral science and the humanities is likely to occur if psychologists and individuals in the arts make common cause. This insight was not lost on our forefathers, and it has been reinforced in promising work undertaken by Donald Spence (1982) and Jerome Bruner (1986), and Irvin Yalom (2005) among others.

WHITHER PSYCHOLOGISTS?

On his better days William James was a determined optimist, but he harbored his doubts about psychology. He once declared, "there is no such thing as a science of psychology" and added that "the whole present generation (of psychologists) is predestined to become unreadable old medieval lumber, as soon as the first genuine tracks of insight are made" (Allen, 1967, p. 315). I have indicated my belief that, over a century later, James' less optimistic vision has materialized and that it may be time to bury scientific psychology, at least as a single coherent undertaking.

Yet scientific psychologists merit praise as well. If we have so far failed in our more ambitious undertaking, we have developed any number of paradigms, concepts, and methods which should prove serviceable in contemporary and future scientific endeavors. There is no need to chronicle these achievements, because they stock our textbooks and are now often of the common lore. In reading these texts I can not help but feel pride about my membership in the psychological guild; it is clearly the work of our field which has provided the lion's share of evidence that the behavioral sciences merit attention and funding. If some psychologists suffer from "physics envy," I have no doubt that many in other disciplines experience "psychology envy."

We can rightly cherish the work of our most eminent practitioners — past and present — and the various concepts, findings, and schemes which they have developed. Whether psychology long endures as a self-contained field, scientists will long honor the discoveries of Donald Hebb and Karl Lashley, Martin Seligman and Mihaly Csikszentmihalyi, Amos Tversky and Daniel Kahneman, the concepts of identity crisis and cognitive dissonance, the laboratory procedures of psychophysics, psycholinguistics, and physiological psychology.

Even as we pay homage to our past contributors, we can participate as full members of research teams in the emerging disciplines of cognitive science, neuroscience, and, perhaps, cultural studies and developmental studies. Individuals researching in these areas will need the insights and methods of psychology — and if our colleagues do not work with us, they will only have to repeat our mistakes and reinvent our fields.

A third point is perhaps more subtle but it is equally important. I think that the major contribution that psychologists can make is to continue to tackle the most interesting problems that emerge and to follow those problems wherever they may lead. To paraphrase an old saw — "some scientists have avoided psychology because it is too easy; but others have avoided it because it is too hard." It is in our bones — as it was in the bones of William James — to pursue the hard issues; to display an audacious curiosity about the human condition and to follow that curiosity wherever it looks.

A century and a half ago, William James' unstinting curiosity led him to physiology and thence to psychology — indeed to founding at Harvard around 1875 the first experimental laboratory in the country and perhaps in the world. The scientists who flocked to psychology in this century are as gifted a lot of scholars as any I can imagine. Perhaps today, some of those who in an earlier era would have turned to philosophy are instead attracted to computer science, to brain science or genetics, to literature, or literary studies. Such shifting of allegiances is understandable and appropriate. But my guess is that a healthy number of the most curious will continue to gravitate to those vexed issues which, at least in their minds, are best described as being psychological in nature.

If one of those bright students were to wander into my office in search of career advice, what would I say? I would counsel the student to look for those issues, problems, and phenomena that seem to straddle the newly emerging fields. I would have in mind those phenomena or problems that lie at the boundary of the individual self and the social self; which straddle stream of consciousness as a psychological concept and stream of consciousness as a presence in literature; which raise developmental issues in a neurological context or tackle neurological issues in a developmental context; which occur at the interface of "pure cognition" and

cognition as it unfolds in the school or at the working place. If psychology indeed turns out to be a field for foxes, rather than for hedgehogs — as I believe is the case — then I would try to convert psychologists into the sleekest and cleverest foxes around.

In closing, then, I find myself taking a leaf from Marc Antony. Having proposed a funeral for psychology as we know it, I have as well engaged in praise for much of what psychology has accomplished. I have suggested that there is much productive work left for those who, for whatever reason, choose to continue to call themselves psychologists and wish to pursue the kinds of issues and questions which are traditionally considered psychological. In so doing, I believe I have been faithful to the vision of William James, a man whose intellect was far too capacious ever to be corralled into a single discipline; and who in fact thrived by alighting on a topic for awhile and then moving on to another one. James reminds one of the proverbial fox, in Isaiah Berlin's figure, the impressionist painter, in the words of G. Stanley Hall. William James' long-time colleague Theodore Flournoy put it well:

> [James] genius is so abundant, so varied, and so little preoccupied with the appearance of contradiction that in gathering in his various utterances, one does not easily frame him into a truly harmonious whole. Indeed it is almost a question whether he himself would have been able to produce a perfectly linked and coherent system from the magnificent treasure of material which he has left us. (quoted in Allen, 1967, p. 495)

As we psychologists move to the second century of a post Jamesian world, we could do worse than to emulate his spirit and his example.

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Mental Illness as Mental: In Defence of Psychological Realism

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ABSTRACT

This paper argues for psychological realism in the conception of psychiatric disorders. We review the following contemporary ways of understanding the future of psychiatry: (1) psychiatric classification cannot be successfully reduced to neurobiology, and thus psychiatric disorders should not be conceived of as biological kinds; (2) psychiatric classification can be successfully reduced to neurobiology, and thus psychiatric disorders should be conceived of as biological kinds. Position (1) can lead either to instrumentalism or to eliminativism about psychiatry, depending on whether psychiatric classification is regarded as useful. Position (2), which is inspired by the growing interest in neuroscience within scientific psychiatry, leads to biological realism or essentialism. In this paper we endorse a different realist position, which we label *psychological realism*. Psychiatric disorders are identified and addressed on the basis of their psychological manifestations which are often described as violations of epistemic, moral or social norms. A couple of examples are proposed by reference to the pathological aspects of delusions, and the factors contributing to their formation.

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0. INTRODUCTION

In current international classifications of psychiatric disorders, the term 'disorder' is used to pick behavioural and psychological states that satisfy the following criteria:

i) they occur as part of a syndrome or pattern;
ii) they are associated with the individual's distress, disability, increased risk of suffering, death, pain, disability, or a significant loss of freedom;
iii) they are not merely a culturally sanctioned response to a particular event;
iv) whatever their original cause, they must be considered as a manifestation of behavioural, psychological or biological dysfunction in the individual.

Concerning (iii) and (iv), it is important to stress that neither deviant behaviour nor conflicts that are primarily between the individual and society are regarded as mental disorders unless the deviance or conflict is a symptom of a dysfunction of the individual.

The authors of the DSM-IV do not to offer speculations about a common aetiology or nature of psychiatric disorders, but other authors have been less restrained. Mental health professionals have viewed the conception of disorder outlined above in three broad fashions (see Harland et al. 2009).

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A first option is a combination of anti-essentialism and pragmatism. It is not worrying that psychiatry cannot be reduced successfully to neurobiology, because psychiatry is essentially pragmatic. Psychiatric classification fulfils certain concerns and purposes, which may well be in conflict. An example of this is offered by a diagnosis of schizophrenia in the early stages of illness: the diagnosis may be useful to the individual in terms of accessing treatment, state benefits and so on, but may also have deleterious consequences in engendering stigma and perhaps therapeutic pessimism in some professionals, carers and patients. According to the pragmatic view, disorders themselves are not necessarily expected to have a discrete essence or to constitute a natural kind.

The second option is eliminativism: if the entities of psychiatry cannot be reduced to their putative biological underpinnings, and psychiatry cannot be successfully reduced to neuroscience, then psychiatric classification is not useful and will be gradually replaced by neurobiological classification.

The third option is essentialism. This conception has a more optimistic take on the success of reducing psychiatry to neurobiology, and views psychiatric disorders as being validated through the discovery a discrete, identifiable biological 'essence'. This is the dominant conception in contemporary psychiatry.

In this paper we shall offer an alternative realist account to biological realism. We shall argue that we cannot come to a proper understanding of psychiatric disorders, and to an explanation of the reasons why psychiatric conditions are pathological, without deploying psychological notions which involve epistemic, moral or social norms, such as rationality and justification in the case of delusions (see Bortolotti and Broome 2008), and appropriateness of emotional responses in the case of personality disorders (see Pickard 2009).

1. CONCEPTIONS OF PSYCHIATRIC DISORDERS

Most classification strategies for psychiatric disorders seek to model themselves on the taxonomies of natural science. The classical taxonomic strategy in psychiatric classification takes for granted that there are robust entities that exist 'out there' in nature.

> If psychiatric symptoms are biological natural kinds, biological variables such as genetic codes and drug response may help us isolate their underlying reality. Biomedical materialists assume that, because physical diseases are the bedrock reality of medical science, in order to be scientifically valid, histrionic personality disorders and schizophrenia have to be understood as physical diseases. As disease they must have underlying biopathological processes. (Zachar 2000, p. 233)

There are different ways of identifying the essence of what exists in nature. Are the entities of psychiatric classification natural kinds with rigid essences, analogous to those entities that make up the periodic table of elements? The constituents of the periodic table of elements are not all identified in the same way. Some elements have a single defining criterion (atomic number). Other elements are identified through several criteria, which may be weighed equally or differentially. In the classical conception of psychiatric disorders there is a single underlying criterion by which such disorders can be identified. Examples of a single criterion psychiatrists may suggest would include a specific cognitive abnormality, a change in functioning of a certain neural circuit, or a distinct genetic polymorphism.

However, this conception of psychiatric disorder is obviously in contrast with the leading paradigm in psychopathology research, which is that of cognitive neuropsychiatry. Cognitive neuropsychiatry is defined as a systematic approach to the explanation of psychopathology in

terms of cognitive neuropsychological deficits. Although the deficits are ultimately cognitive ones, cognitive neuropsychiatry is linked to the basic neurosciences, because it investigates "the neural substrates of impaired cognitive mechanisms" (Halligan and David 2001, page 209). Halligan and David explain that first cognitive neuropsychiatry aims to bridge the gap between the functional description of psychiatric disorders with the framework of cognitive neuropsychology, and then it aims to link this framework to brain structures and their pathology. For the cognitive neuropsychiatrist, the entities of psychopathology are real, but further, they can be traced back to neuropsychology, and ultimately to brain structure and function.

One question is whether cognitive neuropsychiatry assumes that we can talk about intentionality all the way down. If so, that conclusion would perhaps go against some instances of biological realism, such as that of Jerry Fodor:

> I suppose that sooner or later the physicists will complete the catalogue they've been compiling of the ultimate and irreducible properties of things. When they do, the likes of spin, charm and charge will appear upon their list. But aboutness surely won't; intentionality simply doesn't go that deep. It's hard to see how one can be a Realist about intentionality without also being, to some extent or other, a Reductionist. If the semantic and intentional are real properties of things, it must be in virtue of their identity with (or maybe of their supervenience on?) properties that are themselves neither intentional nor semantic. If aboutness is real, it must really be something else (Fodor 1987, p. 97).

The relationship between realism and reductionism about intentionality depends on the preferred conceptual approach to psychiatry, psychopathology, and psychiatric disorders more broadly. For instance, if the reduction to the biological left nothing distinctly psychological in the classification and understanding of psychiatric disorders, then eliminativism would ensue.

The eliminativist option, which would lead to 'eliminative mindless psychiatry' (in the words of Jablensky and Kendell 2002), carries on from Fodor's conclusion above and from the Churchlands' programme of eliminative materialism in the philosophy of mind (see Bermúdez 2006). The view suggests that the entities of psychiatry can and should be eliminated and that new entities should replace them, based on the biological underpinnings of what we now call 'disorders'. This process may start as a process of reduction (from the disorder behaviourally defined to its neurobiological bases), but in the end psychiatry as we know it will not just be given solid scientific foundations by being reduced to neurobiology; it will disappear altogether. First, neurological explanations will be given ontological primacy over psychological explanations, and then the entities making up the neurobiological explanations will be regarded as the only constituents of a scientifically respectable ontology. As a consequence, the terms now used in psychiatry will not be found to pick out real things in the world (a bit like 'phlogiston' after the Chemical Revolution) and psychiatry will be found to offer an inaccurate and misleading account of what disorders are. The process of elimination will mirror that in other branches of medicine, whereby "molecular, imaging and computational tools have largely replaced clinical skills in making a diagnosis" (Jablensky and Kendell 2002, p. 22). Given that the criteria for something to be a disorder depend heavily on diagnostic criteria, when the methods for making a diagnosis change, the various conditions diagnosed will also have to change. This position will lead to the elimination of our 'folk' psychopathology and classification; terms such as 'schizophrenia', 'bipolar', 'depressive psychosis' may no longer exist. Schizophrenia will either be diagnosed on the basis of radically different criteria, and hence mean something different, or a new term will have to be invented. Either way, the traditional taxonomy of psychopathology will be eliminated.

2. THE ELUSIVE OBJECTIVITY OF PSYCHIATRIC DISORDERS

Exemplifying the medical model of psychiatric disorders, Üstün and colleagues (2002, p. 30) suggest that, at its simplest, a disorder is "an identifiable and distinct set of signs and symptoms that commonly produce disability". Further, for such a disorder to become "clinically significant" it must be a product of a "harmful dysfunction". This is a biological concept indexing "a failure of an internal mechanism to perform one of the functions for which it is naturally designed" (p. 31). Function and mechanism are to be understood here in evolutionary terms. Harm is contingent upon the individual's particular social context and as such converts a dysfunction into a disorder (Üstün et al. 2002). The only relevant functions are natural functions of some internal mechanism, that is, functions of the mechanism which depend on evolution (Wakefield and First 2003). Two of the purported advantages of adopting this approach are that (i) the concept of disorder in psychiatry will be analogous to that used in other medical conditions (Wakefield and First 2003; Wakefield and Spitzer 2002) and that (ii) the false-positives arguably created with the over-inclusive definition of the DSM-IV will no longer constitute a problem.

> All disorders (mental as well as physical) are conditions in which some function in the individual is not working as expected and, as a result, the individual is experiencing some form of harm – typically but not always in the form of distress or disability (Wakefield and Spitzer 2002, p. 33).

The classification in DSM-IV was based on the assumption that science itself will tell us how to classify psychiatric disorders. Biological validators were consulted to aid the DSM taxonomic process based on the conviction that patients with the same diagnosis share an underlying pathophysiological process. According to Cooper, the DSM-IV aims to produce a classification analogous to that found in the natural sciences, and assumes that mental disorders are natural kinds (Cooper 2004, page 10). Although some mental disorders may be natural kinds, and other may be partial kinds (cases of the disease similar to each other in some, but not all, fundamental aspects), for many disorders, cases of the same diagnosis won't have in common any fundamental property amenable to natural scientific study. This immediately raises issues about how one determines such 'fundamental properties'. Classification of psychiatric disorders is aided by cluster analysis – the clustering of data on different axes results in a 'psychiatric entity'. The variables are chosen by the classifier on the basis of a theory which is influenced by psychiatrists' beliefs regarding the aetiology of mental illness (i.e., a body of a priori, frequently implicit and unstated assumptions). Thus, the 'fundamental properties' are not identified by direct observation or inference from the evidence about the causal processes leading to the disorder, but are determined by the choice of relevant variables, which is in turn a product of existing background beliefs.

According to Haslam (2002a,b) and Cooper (2004), psychiatric disorders vary widely and very few, if any, will meet the criteria for being a natural kind. Realists assume that the classification we have should somehow be translatable into neuroscience and cognitive psychology and that this is the only kind of realism there is. For the biological realists, it would seem that all diagnoses are equally likely to be natural kinds, whether one is studying personality disorder, dementia, schizophrenia or hysteria. As Cooper (2004) and Haslam (2002a,b) also argue, Kendell suggests that although some disorders are natural kinds, others are not and are falsely treated as if they are - this is the *reification* fallacy, whereby anything that can be given a name becomes *a thing*, a natural category that can be scientifically investigated and advocated in explanation and prediction of scientific phenomena (Jablensky

and Kendell 2002, and Kendell 2002). For instance, Üstün and colleages seem to commit the reification fallacy in that they believe that neuroscience and molecular genetics will "validate and refine our current descriptive categories" (Üstün et al. 2002, p. 41). Validation would imply a discrete and pathognomic biological profile for these disorders. Such an approach is supported by Andreason who seems to similarly endorse an ontological realist view with respect to the categories of psychiatry:

> As a scientific discipline, psychiatry seeks to identify the biological factors that cause mental illness. This model assumes that each different type of illness has a different specific cause. (Andreason 1984, p. 30)

Cooper (2004) is critical of the approach in the DSM-IV and offers a conception of disease that differs from Wakefield's.

> By 'disease' we aim to pick out a variety of conditions that through being painful, disfiguring or disabling are of interest to us as people. This class of conditions is by its nature anthropocentric and corresponds to no natural class of conditions in the world. I suggest that by disease we mean a condition that it is a bad thing to have that is such that we consider the afflicted person to be unlucky, and that can potentially be medically treated. All three criteria must be a condition to be a disease. The criterion that for a condition to be a disease it must be a bad thing is required to distinguish the biologically different from the diseased. The claim that the sufferer must be unlucky is needed to distinguish diseases from conditions that are unpleasant but normal, for example teething. Finally, the claim for a condition to be a disease it must be potentially medically treatable is needed to distinguish diseases from other types of misfortune, for example economic problems and legal problems. (Cooper 2004, pp. 7-8)

Although Cooper's positive account of disease is much more attractive than Wakefield's, its potential shortcomings are that it is problematic to establish whether a condition is potentially treatable: we know that whether something can be treated changes dramatically across time, and depending on the society and individual in which the disorder is manifested and identified. Thus, we may not be able to tell whether something is a disease before we can establish whether there is a medical treatment for it, especially as there is likely to be controversy both about what needs to be medically treated and about what counts as a medical treatment. As Cooper fully acknowledges, whether a condition is bad luck for an individual might depend on characteristics of that individual and of the society in which she lives, thereby suggesting that a condition which might be considered a disorder for some people at some time would lose the label for other people at the same time, or for the same people at other times.

In another attempt to provide an account of the objectivity of mental illness without fully embracing the reduction of the psychiatric to the neurobiological, Thornton (2002) holds that psychiatric classification is based on functional properties and suggests the analogy with poisons and herbs. Members of each group may lack any exact physical similarity but membership of the group is determined by function: (a) in order to be a poison, the substance must have a certain effect on plant or animal physiology; (b) in order to be an herb, the plant must play a certain role in the preparation of food. Functional kinds, as we can see with the example of herbs, are tied up with human practices and as such may reflect value judgments. Thornton then discusses whether a classificatory system based on functional kinds would compromise objectivity.

> I wish to focus on the view that mental illness lacks objectivity because it is defined in psychosocial, ethical, and legal concepts. Why would this follow? It would follow on the joint assumption that such concepts are essentially evaluative and that values are not part of the

> world. If classifications reflect values, then even if they were reliable, that would still not imply objectivity or validity because *had* there been disagreement (about the implicit values) this need not imply any mistake. (Thornton 2002, p. 233)

Thornton outlines and opposes the view that values are not in the world and that the only kind of classificatory system that can have validity is one that is devoid of human interests and values and is created from the standpoint of natural science. If this view is rejected, then many of the tensions between objectivity and relativism in psychiatry dissolve.

> McDowell's argument is that *nothing* can occupy the extreme objective end of a scale of objectivity cashed out in terms of mind independence. The very idea of applying concepts in judgment presupposes a subject who finds it natural to make judgments in a particular way, to find particular ways of going on natural (Thornton 2002, p. 234).

Bernard Williams gave us one of the clearest accounts of objectivity. In both his influential study of Descartes and elsewhere (Williams 1978; Williams 1985), Williams describes the "absolute conception of the world" (Williams 1978, p. 64):

> [I]f knowledge is what it claims to be, then it is knowledge of a reality that exists independently of that knowledge, and indeed […] independently of any thought or experience. Knowledge is of what is there *anyway* (Williams 1978, p. 64).

Williams suggests that if people have true knowledge of the world, they successfully represent it. However, representations may differ. In this case a further representation must be found that explains and resolves the seeming inconsistency. The summation, resolution and iterations of such representations are what Williams terms "the absolute conception of the world". Williams hopes that science will achieve such a goal (Williams 1985, p. 139):

> The aim is to outline the possibility of a convergence characteristic of science, one that could meaningfully be said to be a convergence on how things (anyway) are. That possibility, as I have explained it, depends heavily on notions of explanation. The substance of the absolute conception […] lies in the idea that it could nonvacuously explain how it itself, and the various perspectival views of the world, are possible (Williams 1985, p. 139).

McDowell contends that Williams relies on a fundamental distinction between appearance and reality (Williams 1978, p. 241; McDowell 1998a, p. 117) and conceives of science as "intrinsically non-distorting; as a pure or transparent mode of access to reality" (McDowell 1998a, p. 119). He offers two powerful criticisms of Williams' view. First, any totalizing objective account would have to include local perspectival accounts within it; thus, descriptions of subjective properties may not be "intelligible independently of understanding their use to say how (as one takes it) things are" (McDowell 1998a, p. 124). Second, he criticizes the idea of science as a *pure* means of accessing reality itself.

> Surely whatever is substantive in any actual view of scientific method is itself part of a substantive view of what the world is like, which cannot escape being the product of a particular location in the history of science. One's beliefs about which sorts of transactions with the world yield knowledge of it are not prior to, but part of, one's beliefs about what the world is like; necessarily so, since the transactions themselves take place in the world. If a characterization of scientific method is to be general enough to be able to survive radical alterations in scientific theory, it needs to appeal to highly abstract notions like that of simplicity; and such notions require

> determinate content, and the practical bite in the selection of one hypothesis as superior to others, only in the context of some specific beliefs. (McDowell 1998a, p. 126).

McDowell's point here is that, on the view of scientific method that Williams endorses, we are left with a science that is abstract and free of historical parochialism, but with no determinate content. Inspired by McDowell's position on the scientific worldview, Thornton argues that a taxonomy of kinds can thus be non-problematically and respectably supplemented by kinds that can be identified on the basis of human interests and value judgments. Although there is nothing wrong about pursuing the project of biological reductionism and searching for biological validators for psychiatric disorders, biological realism needs not be the only path to rescuing objectivity, and for providing a framework for scientific research in psychiatry.

In the next section, we suggest that for the purposes of identifying what is pathological about conditions that are classified as psychiatric disorders, we need to be *psychological* realists.

3. TOWARDS PSYCHOLOGICAL REALISM

To what extent should psychiatric disorders be thought of as strictly analogous to other medical disorders? Whilst it is natural to suppose that psychiatric conditions share some feature of physical disorders (e.g. they present obstacles to the satisfaction of an individual's interests and negatively affect the well-being of that individual), they also have distinctive features. We want to argue that some of these features can be adequately characterized only by using the vocabulary of the mental. We do not deny that psychiatric disorders can be described as disturbances of neurobiological mechanisms, but we insist that they are pathological in virtue of their manifestations, and they manifest as disturbances of the mind. Taylor (1999, p. viii) states that "mental illness is not 'mental' at all, but the behavioral disturbance associated with brain dysfunction and disease". Our reply to such statement is that mental illness is mental precisely because in order to establish whether a certain behaviour is disturbed we need to apply psychological concepts. This is of course orthogonal to the question about how these disturbances are caused, and Taylor is right that brain dysfunction and brain deficits will be at the basis of these behavioural manifestations.

As perhaps some of the previous discussion made clear, the assumption in the current psychiatric literature is that there is only one player in the game of validating psychiatric categories, and that is biology: even phenomenology is forced to serve as a tool to render such biological validation possible. With very few exceptions, there is no debate about whether disorders can be validated psychoanalytically, cognitively, socially, or even as Kraepelin did, by clinical course. Equating the natural with what neurobiology can constrain our understanding of mental illness: it is as if currently there were only one way of thinking about classification and diagnosis in psychiatry, and other options were blocked out or hidden from enquiry. Neurobiological psychiatry has become the paradigm in the field and thus it has led to a rejection of the potential contributions of alternative approaches in clinical practice and psychiatric research. Neurobiology can explain why a certain disorder occurred, and how best we should treat it, and can enlighten the connections between that disorder and other aspects of normal or abnormal functioning. But it cannot be an exhaustive and exclusive explanation of the reason why the observed deviation from normal functioning is *pathological*. We shall argue that psychiatric disorders manifest in changes in the mental, and in the way in which people behave with respect to epistemic, moral or social norms.

In the case of delusions, a merely neurobiological investigation becomes problematic early on. Unlike some other symptoms of psychosis, delusions are not discrete either temporally or

in terms of their demarcation from other mental states. It would seem inconceivable to instruct a subject to button-press when deluded and when non-deluded, for example, in a neuroimaging experiment. Delusions are usually thought of as false beliefs that are maintained in the face of strong counterevidence (at least according to the doxastic conception of delusions), but, as any clinician can attest, there is a lot more to a delusion than merely being wrong. Most contemporary accounts of delusion view them as non-discrete mental states, a symptom observed when a number of differing dimensional attitudes to a belief content are adopted. Characteristics of delusions include implausibility, conviction, being unfounded, being distressing, causing preoccupation, and not being shared by others (Freeman 2007). Delusions can take up a lot of the cognitive resources of those who are affected by them and involve inaccurate self-narratives (Gerrans 2009). In addition, some argue that delusions are not merely doxastic attitudes, but bring about a new mode of experience (Stephens and Graham 2006), or involve the access to an alternative, non-actual, reality in which the subject remains trapped (Gallagher 2009).

Delusions may lead to the subject's whole experience of themselves and the world to be altered. What was once banal, and beneath conscious attention, becomes salient and self-referential (Broome et al. 2005; Gray et al. 1995; Hemsley 1993; Kapur 2003; Kapur et al. 2005). The normative, socially conditioned, rules for linking reasons, causes and explanations can be disrupted, and we are left with the hallmark of delusion: namely, that the reasons the person gives for holding her delusional beliefs either do not look like reasons or are not regarded as intersubjectively good reasons. The effect of an inappropriate dopamine-driven generation of salience to otherwise neutral representation leads to the private creation of affect-laden meaning and new reason-relations that cannot be shared or recognized by others as valid. Further, in some cases, the degree of certainty engendered is such that some philosophers have hypothesised that for delusions, as for those beliefs we all hold without doubt, the giving of and asking of reasons does not make sense (Campbell 2001). Alternatively, the tenacity with which a person can hold on to her delusion combined with the implausibility of the content of the delusion has been explained by the estrangement of such person from the shared background of practices and norms followed by the members of the community (Rhodes and Gipps 2008).

Delusions held without doubt are extremely resistant to counterevidence or counterargument, contributing to the isolation of the person reporting and believing them. This shows how delusions manifest behaviourally and interpersonally: it is by observing how the person behaves with respect to her beliefs, and by witnessing such behaviour in the process of the giving and asking of reasons that one suspects delusions, not in viewing a brain scan or a genetic sequence. Listing these features of delusions helps us realize that the idea of what is pathological in delusion cannot be fully captured without referring to psychological notions and an interpersonal dimension. This does not mean that it is *a priori* impossible to reduce the concept of delusion to its biological underpinnings or to characterise delusions on the basis of their physical aetiology, but that focusing on local brain dysfunctions won't give us a sense of why the delusion is a disorder, and why both the clinically-trained and lay interlocutor can spot that something is awry when conversing with a person with delusions. Delusions stretch our folk-psychological categories and practices (Bortolotti 2009).

Although that of delusions is our test-case in this paper, it is by no means a special case. Another example, which we shall not develop here but deserves attention, is that of personality disorders (borderline personality, narcissism, histrionic personality, etc.). Pickard (2009, p. 93) observes that "patients often seem to lack the virtues of, for instance, temperance and moderation, fairness and generosity to others, humility, trust, patience, and

love and respect for self and others", and this is often at least partially due to acute neglect or abuse in childhood, where the opportunities for developing a good character in relation to the expectations of others were seriously compromised. The way in which the behaviour of these patients deviates from the norms of the community in which they live, and the way in which their 'failures of character' manifest contribute to the diagnosis of their condition. As with delusions, surface features are at the basis of the classification of personality disorders. Personality disorders concern both a person's behaviour in relation to herself (e.g., self-harm) and her interpersonal relationships (e.g., unwillingness to share goods and cooperate, and other forms of anti-social behaviour). As Pickard observes, this does not mean that personality disorders cease to be conditions that can be scientifically explained and medically treated. Rather, it is by observing deviations from norms in behaviour that the condition is diagnosed and starts to be treated and traced back to its neurobiological features.

> Are Cluster B PDs medical conditions despite the fact that they involve failures of virtue and character? […] [T]here is good reason to hold that PDs are properly treated in contemporary, multidisciplinary, mental health clinics, involving psychiatrists, psychologists, and psychotherapists of various bents. At least in this respect, they are medical conditions. (Pickard 2009, p. 96)

Obviously, delusions and personality disorders are phenomena that can be studied with the means of neurobiological investigation and can be medically treated. The basic point we attempted to make in this section by reference to these conditions is that we need a psychological and mental vocabulary to explain what makes the behaviours typical of these conditions pathological. Why would anyone think that the use of a mental vocabulary already implies a tendency to *anti-scientific* feelings or a reliance on *supernatural* phenomena? McDowell reminds us that one shouldn't believe that the domain of scientific investigation encompasses all that is natural and real, and talks about there being a second nature:

> The therapy I offer is a reminder of the idea of second nature, which tends, I suggest, to be forgotten under the influence of a fascination with modern science. The idea of actualisations of conceptual capacities does indeed belong in a logical space that contrasts with the one in which modern science delivers its distinctive kind of understanding. But we should not allow the logical space of scientific understanding to hijack the very idea of the natural. The idea of actualisations of conceptual capacities belongs in the logical space of reasons, but conceptual capacities are part of the second nature of their possessors. (McDowell 1998b, p. 367)

It is the structure of this second nature which helps us provide an account of what mental illnesses are, *really.* Mental illness is apparent in the realm of reasons, as abnormal, skewed, or constrained behaviour. Changes in interpersonal behaviour and reason giving map on the broad categories of mental illnesses we are familiar with. Mental illnesses *as illnesses* manifest at the level of observable behaviour and deviations from epistemic, moral or social norms. What one sees in biological terms may be changes in receptor function, abnormal neurotransmitter metabolism, or underdeveloped orbitofrontal cortex. But such changes are not 'disordered' in and of themselves.

Next we offer two examples in which psychological realism seems vindicated by reference to delusions. First, we shall argue that an assessment of the capacity for reason-giving can be profitably used to characterise delusions, a methodology that may bring additional benefits other than the purely taxonomical (see also Bortolotti and Broome 2008). Second, we consider situations in which the disorder might not be affected by factors purely internal to the

subject's brain but also by features of the environment surrounding the subject (see also Bortolotti and Broome 2009a).

4. REASON GIVING AND DELUSIONS

The notion of authorship as the capacity to endorse a thought as one's own and justify it on the basis of reasons has been developed by Richard Moran and discussed extensively in the philosophical and psychological literature (Carman 2003; Ferrero 2003; Lawlor 2003; Moran 2001; Moran 2004). The notion of authorship, as we see it, is not necessarily tied to the rationality of the beliefs endorsed or to the rationality of the process of formation of such beliefs. Rather, authorship lies in the capacity to endorse the content of a belief and make it one's own by justifying it with reasons. Authorship conceived in these terms generates a form of first-person authority. By taking responsibility for the thoughts we report, we can relate to the content of those thoughts in a way no third person can. The belief is not just self-ascribed; it is also endorsed and seen as part of a self narrative that underlies agency. We suggest that the subject with delusions has a psychiatric disorder which becomes apparent in the quality of the subject's reason giving. Thus a radical failure of authorship of beliefs, which is tied to considerations about self-knowledge more generally, can signal the presence of a belief that is not just badly integrated with other beliefs, badly supported by the evidence and imperfectly rational, but truly pathological.

Due to the mentioned features of authorship, namely its dependence upon the capacity to engage in reason giving and its contribution to self knowledge, it is an explanatorily useful notion when applied to delusions. A discussion of authorship in psychopathology can help us identify what (if anything) makes delusions different from ordinary beliefs, and can contribute to the classification of some beliefs as delusional on the basis of the reasoning patterns exhibited by the subjects of those beliefs. But it can also have an added value. It can tell us something about how subjects related to the content of their delusional beliefs, whether they truly endorse their content and can be made responsible for the actions they perform on the basis of those beliefs and other intentional states.

Jaspers' and subsequent accounts of the classification of delusions rely on the distinction between 'form' and 'content'. The theme of the delusion, such as persecution, control, infatuation, determines its content. The structure of the delusional belief and its relationship to reasons the subject can offer the interviewer for holding it constitutes the form. The form of a delusion is affected by whether the reasons provided to the interviewer by the subject are 'understandable'. When these reasons are deemed un-understandable the delusion is said to be primary or autochthonous. This explains why the form of a delusion has been viewed as of particular importance both in terms of diagnosis but also of prognosis: content has been seen as somewhat more 'epiphenomenal' and related to the subject's biography, concerns and culture, whereas the form reflects the pathological processes. The form of a belief so intended can tell us (i) whether a belief is genuine; (ii) whether it is well integrated in an existing system of beliefs; and (iii) to what extent the subject has self-knowledge with respect to that belief. It can help us answer questions about self-knowledge, because the form shapes attributions of authorship. Only the author of a belief is in a position to make the content of a belief one's own and justify it with reasons or defend it from objections.

Failures of authorship come in degrees. If subjects with delusions are unable to give any reasons for their delusional states, then they cannot be regarded as their authors. There are subjects who report to believe something quite striking with conviction and something that would seem to be of great consequence for themselves. But when asked to provide the

reasons why they are committed to the content of these beliefs, that is, to make explicit the grounds for holding the beliefs, they cannot answer. For instance, consider the case of a 21-year old man who has the sudden conviction that certain songs played on the radio used his voice in the role of lead singer, but cannot explain why (Yager and Gitlin 2005). The significance of the belief reported make it an ideal candidate for authorship, but the subject is unable to provide any reason that might convince others that his belief is likely to be true.

In other cases, subjects endorse a belief on the basis of reasons, but these are reasons that others fail to regard as relevant to the content of the belief. For instance, suppose that there is a man who believes that his wife is unfaithful to him because the fifth lamp-post along on the left is unlit (Sims 2003). The subject is attempting to justify his belief, but fails to support his belief on the basis of reasons that others can share and understand. Less perplexing cases are those in which the subject with delusions comes up with understandable reasons in support of the reported delusional belief, but such reasons are not intersubjectively good reasons. Here authorship is present, to some extent. A good example is that of the woman who claims that her blood is being injected out of her body in her sleep because she has spots on her arms. When the interviewer says that the spots are freckles and that he has them too, she agrees that the spots are similar to freckles, but continue to believe that she is being injected (Sims 2003).

As the previous examples suggest, delusions can vary widely with respect to the level of commitment that subjects manifest towards the content of their delusional beliefs. Here commitment is not just supposed to track the importance of the delusion in terms of action-guidance, persistence or integration with other intentional states of the subject, but it is also supposed to give us clues as to whether the subject is a genuine author of her beliefs. If the subject cannot provide any reason, or any reason that maintains meaningful relations to the content of the beliefs, for endorsing the beliefs, the interviewer might either doubt that the state reported is a genuine belief (coming to challenge the intentionality of the reported state) or interpret the inability to provide reasons as a breakdown of self-knowledge. This latter move is even better justified in situations where not only the authorship of the beliefs is compromised, but also their ownership, as in the case of inserted thoughts (see Bortolotti and Broome 2009b).

The conception of delusions as mental disorders or pathological beliefs relies on the analysis of the reason-relations between the subject's beliefs and on attributions of self-knowledge and rationality.

5. 'EXTERNALISM' AND PSYCHOPATHOLOGY

Neuroimaging and genetics have made a significant impact on understanding mental illness, and on understanding psychiatry, but this should not blind us to the impact of recent studies in epidemiology and prognosis of mental illness. As is often the case in psychiatry, findings around schizophrenia have determined research trends in wider psychiatry. It has become clear that there is marked heterogeneity in the rates of schizophrenia, and that further, some of this heterogeneity can be explained by urban birth and upbringing, migration, ethnicity, and what has been termed 'social defeat' (Cantor-Graae and Selten 2005; Selten and Cantor-Graae 2005). A particularly important body of research is the MRC AESOP study that demonstrated a twentyfold rate increase in the incidence of psychosis in London, compared with Nottingham and Bristol, with the very highest rates being within the black and ethnic minority groups (Morgan et al. 2005a,b; 2006a,b; Morgan and Fearon 2007; Fearon and Morgan 2006; Kirkbride et al. 2007a,b).

These epidemiological findings were compounded both by continuum models of psychosis (Johns and van Os 2001), suggesting that rates of psychotic experience in a non-help-seeking population were dependent upon many of the same variables that explained cases of the disorder, and by a seeming failure in the neurodevelopmental model of schizophrenia in explaining how someone with odd ideas and developmental delay became a person with a frank psychotic disorder (Broome et al. 2005). Hence, for the problems the study of schizophrenia brings researchers and clinicians, the answers provided by neuroscience and genetics may not be sufficient. Trying to connect psychological, biological, and social models of psychosis is important, and trying to empirically test the relationships between these varieties of variables has become a focus of psychosis research.

Increasingly, accounts of psychosis relating neuropsychological function, dopamine, symptoms, stress, and social isolation have been published. A very influential account is Kapur's salience theory (Kapur 2003; Kapur et al. 2005). Kapur links dopamine dysregulation to the aberrant salience of both internal and external representations and to the symptomatology of psychosis. In a series of remarkable experiments, Myin-Germeys, Van Os, and other colleagues from Maastricht (Myin-Germeys et al. 2001a,b; 2003a,b; 2005) demonstrated a relationship between psychotic experiences and stress and 'daily hassles'. This sensitivity was in part consequent upon the reactivity of the participants' dopaminergic system and their history of life events, and further, neuropsychological impairment ameliorated this sensitivity. Ellet, Freeman, and colleagues (Ellett et al. 2007) demonstrated how the experience of walking through a busy urban street increased anxiety levels, negative beliefs about others, and exaggerated reasoning biases linked to the formation and maintenance of delusions. Hence, for schizophrenia and other psychotic disorders, we are left with the heterogeneity of incidence rates, the role that being part of an ethnic minority group, or being a migrant plays, plus data suggesting that the urban environment has an immediate and measurable impact upon levels of paranoia in both health controls and patients.

In contrast to much of the work of the 1990s, this rebirth of social psychiatry has led to a renewed interest in external factors to the brain in the genesis of psychosis. The idea of an environment, or lived experience, that is somehow 'psychotogenic' is a consequence of the data stressing how much variance in psychopathology could be attributed to context and exposure. Given that many commentators view these findings as a challenge to wholly neuroscientific or genetic accounts of mental illness, can it be suggested that psychopathology may be consequent upon factors *external* to the brain? In philosophy, externalism is linked to the truth of one of two claims (Rowlands, 2003): first, the *location* claim, the idea that some mental phenomena are not spatially located inside boundaries of subject that has or undergoes them; and second, the *possession* claim, the idea that the possession of at least some mental states by a subject depends on features that are external to its boundaries. Based upon how various externalist philosophers accept or reject these claims, two broad varieties of externalism are described in the literature. *Vehicle* externalism, is based predominantly on the location claim and suggests that the very vehicles and processes of cognition in part exist outside the skin of mental subjects. *Content* externalism is driven by the possession claim and argues that the individuation of propositional attitudes is dependent upon properties/relations external to the skin.

Is the focus on vehicle externalism, or 'extended cognition' (Clark and Chalmers 1998; Clark 2005), helpful when we think about psychosis? Given the effect that the world, and specifically urban experience, may have on both the rates and symptoms of psychosis, can external factors to the brain have a role, in terms of information processing and cognition, in the aetiology of psychosis? As a list on a piece of paper may supplement the neurally encoded memory we

have of what we want to shop for in the supermarket, certain cognitive acts may be subserved or supplemented by physical entities or relationships in the external world. Such a theory has been linked to evolutionary concerns: if the environment can encode certain information reliably, and we can easily access and utilize such information, then it would be inefficient to develop internal mechanisms to do such work. Certainly, empirical data suggest that working memory and other neuropsychological, internal deficits may be linked to the onset of psychosis and perhaps an increasing reliance on external vehicles of cognition. Do certain environments yield particular information if the individual is in a given 'internal' (neurochemical, affective, neuropsychological) state? As the empiricist strand of the doxastic account of delusion argues, the delusional content is in the perception of the world.

Do my experiences when walking down Electric Avenue bear the informational content that I am Haile Selassie, and hence God Incarnate for the Rastafarians? In his influential *Mind and World,* McDowell argues that for the thoughts of any of us to have genuine intentionality and to be about the world requires the world itself to be conceptually structured and reality to exert a rational influence on what we can think (McDowell 1996). The deluded are not simply mistaken and, as we have discussed elsewhere (Bortolotti and Broome 2008), the world and internal events become meaningful in a non-public manner. Within the philosophy of mind, externalism has been viewed as challenge to the view that mental content is wholly dependent upon internal variables such as biological states in the brain. Similarly, an externalist psychopathology would put pressure on the idea that we could have a description of mental disorder purely based on internal, biological variables. As such, it can support a 'psychologically realist' position where the mental variables are the focus, and they in turn are determined by both internal and external factors to the organism.

6. CONCLUSION

> The current classification [*International Classification of Diseases, 8*] is a hotch-potch of classifications by cause, pathology, course and clinical pattern. It is an empirical utilitarian scheme such as Hughlings Jackson contrasted with a scientific one…. […] It flies in the face of taxonomic rectitude, but persists for lack of anything better which would be generally acceptable. (Lewis 1979, p. 193)

In thirty years little seems to have changed from Lewis' assessment of ICD-8. Available classifications are heterogeneous, the disorders described under one category do not necessarily have anything in common, and the biological paradigm governs taxonomic efforts with no good reason (see also Broome 2007 and Cooper 2004). In this paper we have argued – largely by examples - that some psychological notions which are epistemic in character, such as those of rationality and self-knowledge, and some features of the external environment that affect the compliance with social and moral norms in the individual, play an important role in the detection and diagnosis of mental illness, and thus in its conceptualisation. These psychological notions also play a role in our attempts at classifying psychiatric disorders.

In the first example we presented, notions of reason giving and authorship of thoughts contribute to a characterisation of delusions as pathologies of beliefs that has the potential to improve our understanding of delusions and to support welcome changes in the treatment of people with delusions. The notion of authorship, when applied to reported delusions, can tell us something about what is puzzling about delusions, why they take us by surprise and why we might doubt in some circumstances whether the people who report those delusions really endorse them as beliefs. But other lessons can be learnt. First, there is no one type, 'delusion', that fits all the experiences of subjects of delusions, because the form of delusions can make a

huge difference to the quality of reason giving in interviews and to the extent to which we take subjects to have knowledge of their own conscious mental states. The other lesson to learn is that we cannot draw a sharp distinction between delusions and other non-delusional beliefs that are also very implausible or badly supported by evidence. In delusions as in other irrational beliefs authorship may fail, although in delusions it generally fails to a greater extent thereby offering some clues about the pathological nature of delusions. This failure to provide a demarcation which is qualitatively sharp is not an indication that psychological or mental notions are hopeless at contributing to classification, but that they are at the right level of conceptual sophistication to provide the tools of analysis that are needed in accounts of mental disorders. Applying such notions does not solve all the classification problems but improves understanding and allows discriminations that other levels of analysis cannot capture.

Externalism serves as an example of a second challenge to biological realism in psychopathology. If mental content is not wholly dependent causally upon internal variables, such as brain states, then a psychopathology relying purely on neuroscience will fail to explain the variance in mental states that we view as psychopathology. Data from psychosis and schizophrenia research increasingly have demonstrated the role of lived experience in both the development of odd experiences but also potentially in the heterogeneous incidence rates of psychotic illness. Psychological realism allows us to keep psychopathology in focus and maintains agnosticism as to the precise causal variables, internal and external, to the individual that may lead to mental disorder.

Currently psychopathological states and mental disorders use criteria that rely on psychological terms and refer to deviations from norms (ethical, epistemic, social, etc.). Mental illness itself can be thought of the kind of disorder one identifies as when normal reason-giving, all other things being equal, breaks down. Thus, concretely speaking, a brain scan, genetic abnormality, blood test, and so on, can never *a priori* serve as the sole criterion for the diagnosis of mental illness. Such tests can contribute to diagnosing disorders when disorders are defined in terms of those criteria, or to further elucidating physiology. In this respect, neuroimaging has undoubtedly made great advances in the study of the brain. However, in order to diagnose mental illness, one talks to one's patients. To bring biological investigations into diagnostic use, we can 'eliminate' mental illness and choose to redefine psychiatric disturbances using other criteria than those we now employ. This approach would lead to a radical shift in both the profession (and possible existence) of psychiatry, as well as to a change in the wider societal perception and understanding of mental illness. It would also generate a conceptual difficulty: it doesn't take an expert to recognise that someone is mentally disordered, but how would one decide whether dopamine quantal size, functional MRI activations, or repeats of genetic polymorphisms were abnormal in the absence of a disordered person? And this is the crux of the issue: for biological psychiatry to have any validity, and to be anything more than neuroscience, the main object of study needs to remain the person. The normal and the abnormal themselves are not properties of the brain.

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Epistemology: A Behavior Analytic Perspective

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ABSTRACT

The field of behavior analysis contains three distinct elements. The practice of applied behavior analysis, the research methodology known as the experimental analysis of behavior (largely relying on single-subject designs), and the philosophy of science known as behaviorism. Behaviorism has well articulated positions on a variety of traditional topics contained within conventional philosophy, topics such as consciousness, aesthetics, ethics and values, ontology, free will and determinism, ontology, and epistemology. A review of the general field of behavior analysis is followed by a description of this field's perspective on epistemology, which is largely realist, parsimonious, and centers around demonstrations relating to the prediction and control over selected behavior. Notions of causality as eschewed in favor of demonstrating functional relationships between environmental events and subsequent behavior. To the extent that one can effectively predict and control behavior, one has arrived at a limited understanding that is truthful.

EPISTEMOLOGY: A BEHAVIOR ANALYTIC PERSPECTIVE

I often called epistemology my first love.
(B. F. Skinner, 1983, p. 395)

One of the ultimate accomplishments of a science of verbal behavior may be an empirical logic or a descriptive and analytic scientific epistemology.
(B. F. Skinner, 1957, p. 431)

I came to behaviorism…because of its bearing on epistemology, and I have not been disappointed.
(B. F. Skinner, cited in Smith, 1986, p. 263)

This paper will present an overview of a behavior analytic perspective on the topic of epistemology. Many readers may not be familiar with the field of behavior analysis, and providing an overview of this thriving discipline will be helpful in placing the epistemological discussion into a larger context. While behavior analysis is often seen primarily as a field within the larger discipline of psychology, the application of behavior analysis within a wide range of professions, including social work, education, sociology, political science, philosophy, economics and many others, is leading to the position that behavior analysis emerging as a credible independent professional discipline in its own right. This view is supported by the expansion of graduate degree programs in behavior analysis, an accreditation mechanism for such programs, the emergence of state-endorsed certification or licensure programs for the practice of behavior analysis, thriving professional associations and journals, and a professional code of ethics.

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Behavior analysis, hereafter abbreviated as BA, has three general areas, the practice of *applied behavior analysis,* a research methodology known as the *experimental analysis of behavior,* and the philosophy of science known as *behaviorism.* BA is supported by a number of professional associations, foremost of which is the Association for Behavior Analysis – International (ABAI, see http://www.abainternational.org). ABAI has over 65 affiliated chapters located around the world, including over 13,500 members, with 5800+ in the United States alone. Its annual conference attracts over 3000 attendees, and the organization sponsors several highly cited journals. The practice of behavior analysis is legally regulated in a number of states in the USA, and a national behavior analysis certification board helps assure that the practitioners of applied behavior analysis possess certain minimal qualifications (see http://www.bacb.com/). We will review each of these three elements of behavior analysis, as they are essential to understanding the field's epistemological positions.

APPLIED BEHAVIOR ANALYSIS

Applied behavior analysis (ABA) is the discipline's practice wing, consisting of certified behavior analysts, as well as professionals within other disciplines who are trained in ABA and make use of its principles in the delivery of professional services in diverse fields such as psychology, social work, medicine, and education. As outlined in Baer, Wolf, and Risley's (1969, p. 91) seminal piece, "analytic behavioral intervention is the process of applying sometimes tentative principles of behavior to the improvement of specific behaviors, and simultaneously evaluating whether or not any noted changes are indeed attributable to the process of application". ABA has several defining attributes:

- The problem under investigation must be socially significant. Applied behavior analysts are not usually interested in studying trivial issues.
- Both the intervention(s) and the outcomes must be behavioral in nature. The interventions studied must be derived from principles of learning theory, and the outcomes include objective measures of what people actually do, not merely what they may say they have done, will do, or marks they place on a piece of paper, as in completing a symptom questionnaire, rating scale, or attitudinal measure.
- The approach is analytic, in that each case is a small scale experiment aimed at providing plausible evidence that the intervention was functionally related to observed changes in behavior.
- The approach is technologically based, explained as "The best rule of thumb for evaluating a procedure description as technological is probably to ask whether a typically trained reader could replicate that procedure well enough to produce the same results, given only a reading of the description (Baer, Wolf, & Risley, 1968, p. 95). Similarly, the ways used to measure the outcome variables should be similarly technologically described.
- The approach must be conceptually systematic, with the presumptive effects of the intervention explicable in terms of contemporary learning principles, and the results having a bearing on the validity of these underlying behavioral principles.
- The approach must be effective, in that behavioral outcomes are clinically improved.
- The approach must be generalizable, ideally yielding long-term changes, not simply temporary ones, and generate effects replicable among other individuals with similar problems.

> Thus, in summary, an *applied* behavior analysis will make obvious the importance of the behavior changes, its quantitative characteristics, the experimental manipulations which analyze with clarity what was responsible for the change, the technologically exact description of all procedures contributing to that change, the effectiveness of those procedures in making sufficient change for value, and the generality of that change (Baer, Wolf, & Risley 1968, p. 97, emphasis in original).

These features are contended to raise the field of ABA to that of a genuine science of human behavior and its control.

The approach of ABA is used in a wide variety of areas, including mental health, health care, education, developmental disabilities, all of the applied health professions, etc. Behavior analysis is distinct from the field of professional psychology by limiting itself to the analysis of behavior-environment interactions. In contrast, the American Psychological Association states that "Psychology is the study of the mind and behavior" (downloaded from http://www.psychologymatters.org/psycdefinition.html on 29 September 2009). Well and good. This seems clear enough, and quite legitimate. Just as geneticists solely focus on genetic contributions to understanding behavior, physiologists the functioning of organ systems for the same purpose, neurochemists the role of hormones and other aspects of chemistry as they effect one's actions, the behavior analyst's niche in science is the intersection of the environment with human activities and their reciprocal interactions.

THE EXPERIMENTAL ANALYSIS OF BEHAVIOR

The aspect of BA called the experimental analysis of behavior (EAB) refers to an approach to empirical research which is generally known as single-case research designs. Much conventional inquiry in the social and behavioural sciences uses nonomethic designs, research involving large groups of individuals and in putting hypotheses to the test using the hypothetico-deductive method. Differences between groups (e.g., one that receives a treatment compared to one that did not) or within groups (e.g., the level of functioning of a group of individuals before and after they received a given treatment) are examined using inferential statistics. The external validity of such studies is typically claimed on the basis of conducting a study on a randomly selected or otherwise representative sample of persons reflective of a larger population of persons of interest (e.g., people with a particular type of problem or diagnosis).

EAB uses a different approach to scientific inquiry. Instead of taking a few observations from many clients, as in nomothetic research, the idiographic EAB method obtains many observations from one or a very few number of people, ideally a number of times before and after their exposure to some environmentally-based treatment. If the pretest measures (known as a baseline) are stable, and change in the outcome measures is seen immediately after the introduction of the intervention, with these changes being both rapid, obvious, and important, this is construed as preliminary evidence of a causal link between what was done (the deliberate change in the environment) and changes in behavior. A single such demonstration is of course very weak evidence, which is why EAB relies on an array of sophisticated research designs, through which using a process of replicated findings involving the deliberate introduction and sometimes the removal of an intervention, increasingly plausible evidence can accrue supporting the contention that there is a genuine functional relationship between intervention X and outcome Y. Sometimes this can occur within an individual client, and sometimes the research involves replicated findings across a number of clients. The 'interventions', experimental manipulations or independent variables of the behavior analyst usually consist of environmentally-based stimuli, often construed as

reinforcing or aversive consequences, changes in the physical environment, the presentation or removal of antecedent stimuli, and the use of verbal methods of control.

The methodology of single-case research is laid out in a number of textbooks on behavior analytic designs (e.g., Sidman, 1960; Johnston & Pennypacker, 1993; Thyer & Myers, 2007; Riley-Tillman & Burns, 2009), and although this approach did not originate with the behaviorists (the field of medicine seems a more likely candidate), they elevated it to their primary approach to scientific inquiry. Certainly when the level of analysis involves individual subjects, single case designs possess numerous advantages over nomothetic research. The primary method of inferring change in single-case designs consists of visually inspecting data presented in the forms of line graphs. This produces a very conservative approach to inference because if the data are highly variable, a change in behavior is minor, or otherwise not obvious, it is easy to overlook. This results in the conclusions drawn from EAB research findings being fairly robust ones in that without compellingly dramatic changes in a client's pattern of data (say from pre-treatment to post-treatment levels of behavior), the natural tendency is to infer that no differences occurred. This EAB methodology of single-case designs has been widely employed in a variety of basic sciences and applied professions, such as psychiatry (Barlow & Hersen, 1973; Chassan, 1967). Within the domain of evidence-based practice, single-case research designs are said to be the highest level of evidence useful in making decisions and represent the most rigorous investigatory methodology for making causal inferences at the level of individuals (Guyatt & Rennie, 2002).

BEHAVIORISM

The philosophy of science known as *behaviorism* represents the third domain of behavior analysis and the term is not intended to refer to the application of learning theory principles to applied problems, or to the research methodology. Rather, «Behaviorism is not the scientific study of behavior by a philosophy of science concerned with the subject matter and methods of psychology» (Skinner, 1969, p. 221). Although the focus of the present article is on a behavior analytic approach to epistemology, behaviorism as a philosophy has its own perspective to most of the traditional concerns of general philosophy, including ethics, consciousness, free will, values, determinism and self-control, language, and aesthetics (see Chiesa, 1994; Thyer, 1997; Lattal & Chase, 2003; Leigland, 2005). There is a wide-ranging journal called *Behavior and Philosophy,* established in 1973 and published by the Cambridge Center for Behavioral Studies (see http://www.behavior.org/behavior/index.cfm?page=http%3A//www.behavior.org/behavior/what_is_behavior_analysis.cfm).This journal is explicitly devoted to examining the behavioral perspective to philosophical issues and of course articles reflecting a behavioral orientation regularly appear in mainstream philosophy journals (e.g., Addis, 1982; Day & Moore, 1995; Hayes, 1985; Moore, 1990, 1998; Reed, 1981). This trinity of applied behavior analysis, the experimental analysis of behavior, and of behaviorism comprises the general field of behavior analysis.

The term 'behaviorism' is subject to different interpretations, including methodological behaviorism, philosophical behaviorism, and radical behaviorism.

Methodological behaviorism contends that the proper subject matter of psychology consists solely of overt behavior - actions or functions that can be detected through direct observation or proper instrumentation. Methodological behaviorism attempts to explain overt behavior, and while accepting the existence of inner states such as thoughts and feelings, holds that they cannot be studied scientifically. Philosophical behaviorism "…stands for analyzing or reducing mental states into dispositions to behave…" (Day & Moore, 1995, p. 78).

The position taken in the present paper presents that known as radical behaviorism, with the word radical meaning 'complete' as opposed to some extreme position. Radical behaviorism views behavior as everything that a person does, overt behavior as well as everything that occurs within the skin, phenomena such as feelings, thoughts, dreams, hallucinations, etc. With radical behaviorism, the phenomenon of the toothache pain is as real and legitimate a subject for theorizing and research as is the act of consuming an aspirin. Both are behavior of the person's body. The fact that one is private, not visible to others, does not vitiate the reality of these inner events.

The position of radical behaviorism is most commonly associated with the psychologist B. F. Skinner and is significantly different from methodological behaviorism in that radical behaviorism *does* accept the reality of private events and attempts to develop effect ways to explain, predict and control them. The view that Skinner only considered publically observable activities as suitable subjects for scientific is an unfortunately widespread misconception. His early 1953 text titled *Science and Human Behavior* contains chapters on topics such as 'Emotion', 'Thinking', 'Private Events in a Natural Science', 'The Self', and his last book, *Recent Issues in the Analysis of Behavior* (1989) published in the year of his death had chapters on 'The Place of Feeling in the Analysis of Behavior', 'The Origins of Cognitive Thought', 'The Initiating Self', and 'The Listener'. Clearly, attempting to develop theoretical accounts of the inner life of human being, while remaining consistent with the natural science orientation of behavior analysis, was an endeavor which consumed Skinner's professional life.

The quotes presented at the beginning of this article suggests that epistemological concerns, explaining what knowledge is and how we know things, was actually a central facet of Skinner's theorizing, methodology, and to a lesser extent his empirical research.

This philosophy of science called radical behaviorism is comprised of an amalgam of axiomatic positions, each of which is individually susceptible to debate, but are also widely held and respectable perspectives. A selected number of these are listed in Table 1 and will be discussed below, and it will be seen how these converge into a coherent behavior analytic approach to epistemology.

Table 1
Selected Philosophical Positions Associated with Behaviorism

An Acceptance of:	A Rejection of:
Realism	
Naturalism	
Physicalism	Mentalism
Determinism	Metaphysics
Positivism	Dualism
Empiricism	Circular Reasoning
Operationism	Reification
Parsimony	Radical Skepticism
Pragmatism	
Scientific Skepticism	

Realism

This assumption accepts the objective reality of an external world which exists independently of the perceptions of the observer. Realism is a commonly accepted assumption of most philosophies, especially philosophies of science. To accept realism is not to dispute the premise that human beings, to some extent, socially construct aspects of our world. For example, when the writer was growing up, science explained that there are nine planets, with the outermost one being Pluto. A few years ago, members of the International Astronomical Union voted Pluto 'off the solar system' in the sense that it is no longer designated as a true planet. Thus children today are taught that our solar system has only eight true planets. The construction of social reality has indeed changed. Pluto, however, has not changed. It swims along the periphery of our solar system, possessing an objective reality that is undisturbed by the machinations of man to classify it. Similarly, homosexuality was defined as a mental illness for many years, and it was not until 1974 that a vote of members of the American Psychiatric Association lead to changes in diagnostic nomenclature so that it was no longer seen as a disease. The reality of human sexual orientation and preferences exists, unchanged in its nature, although the lives of gay men and lesbians have certainly been altered by this more congenial appraisal. The distance between Rome and Paris is an objective fact. Whether this distance be measured in kilometers, miles, furlongs or cubits matters not. Temperature is an objective reality, regardless of how it is measured (using the Celsius, Fahrenheit, or Kelvin scales). A person has behaved in a certain way, and these actions can potentially be objectively measured, and by doing so behavior analytic research comes into closer contact with nature's realities in this regard. One may choose to study how human being social construct the world in which we live, or one can choose to study the objective realities represented by behavior/environmental interactions. Behavior analysis focuses on the latter, although the process whereby people socially construct aspects of reality would of course also be a legitimate topic for behavior analytic inquiry.

The assumption of realism is also associated with a number of similar perspectives, such as physicalism, which has been defined as: «the doctrine that the only measures through which reality comes to be known is through an understanding of physical matter» (Reber, 1995, p. 437) and «the view that only the physical world exists and that mental processes are a mere by-product of physical ones» (Corsini, 2002, p. 605). Here are some representative quotes illustrating the realistic, physical, world as addressed by radical behaviorists:

- «We operate in one world, the world of physics. Organisms are a part of that world, and they react to it in many ways» (Skinner, 1953, p. 139).
- «It might be simpler for the radical behaviorist to say that the world that we observe (which is variously called the 'material' or 'physical' world) is sufficient to account for 'psychological' phenomena» (Leigland, 1993, p. 351).
- «What is lacking is the bold and exciting behavioristic hypothesis that what one observes and talks about is always the 'real' or 'physical' world» (Skinner, 1945/1988, p. 160).
- «Behavior is treated as basic physics…Few have attempted to exhaust the power of a simple, physicalistic description of behavior; that is the goal of this paper…This article sketches the outlines of a mechanics of behavior, in the hope that it will encourage the reconsideration of our procedures from the vantage of a unified physicalist

perspective…The forces that bend an organism toward an incentive are no less real than those that bend light toward the sun» (Killeen, 1992, pp. 429, 457).

- «The behaviorists rejected the structuralists' attempt to distinguish between the world of physical events and the world of conscious states (philosophical dualism); they were steadfast materialists» (Dinsmoor, 1999, p. 1).

Not all behavior analysts endorse physicalism however. Leigland (1993) contends that materialism is a sufficient foundation without the reductionism associated by some with physicalism.

Most behavior analysts however do not actively deny the existence of nonphysical, supernatural, mental, or incorporeal entities. Realizing that it is impossible to prove that «There are no such things as black swans», it is similarly seen that asserting that there is no such thing as the ego, the self-concept, ghosts, or aliens from outer space is a logically weak position. Rather than asserting that, say, 'the mind' does not exist, behavior analysts try and extend the scope of their research to develop satisfactory natural or physicalist explanations for supposedly non-material phenomena. To the extent they can be successful in this effort, the need to invoke metaphysical or mentalistic mechanisms is unneeded. But doing so, it is hoped, the role of the mind as a causal or activating agent explaining human behavior will be progressively more and more circumscribed, and, perhaps, eventually discarded as unnecessary. This leads to the next principle.

Parsimony

Behavior analysis favors *parsimony,* a preference to accept the simpler of the available and adequate explanatory accounts for a given phenomenon. Epstein (1984) presents an excellent review of the philosophical and scientific value of parsimony, and within psychological theorizing this view is widely known by the example of Morgan's Canon, explained as:

> In no case is an animal activity to be interpreted in terms of higher psychological processes, if it can be fairly interpreted in terms of processes which stand lower in the scale of psychological evolution and development (Morgan 1903, p. 59).

For example, the so-called mirror test is said to be an indicator of 'self-awareness' or of a self-concept. A child or other animal 'passes' the mirror test and is thus said to demonstrate self-awareness when it can use a mirror to locate an object on its body which it cannot directly see (Gallup, 1979). The behavior of using a mirror in this manner could perhaps be an indicator of an inner-initiating mechanism of behavior called a self-concept (the view favored by most animal ethologists) *or* it could be more parsimoniously explained via an animal's (or child's) experiences with mirrors, and history of being shaped to use them for various purposes, such as to obtain reinforcement. Very few non-human species can pass the mirror test, and thus self-awareness was conventionally seen as a property limited to the higher primates, including man. Skinner and his colleagues attempted to see if passing the mirror test could be accomplished by a naïve pigeon, using conventional training techniques of reinforcement via successive approximations. The answer was clear – pigeons could pass the mirror test, after proper training. This leaves researchers with at least two explanations for an animal's ability to use mirrors to locate an object on its body which it cannot see. The traditional account involves hypothetical, invisible, and mental inner mediators such as a 'self-concept', an account which poses the risks of reification and circular reasoning (e.g., «The chimpanzee has self-awareness. How do you know that? He passes the mirror test. Why does he pass the mirror test? He has self-awareness»). The second account is much more parsimonious, and

attributes the animal's abilities to it history of interactions with its environment, and of its past successful (e.g., leading to reinforcement) use of mirrors to detect unseen objects. The latter explanation is completely naturalistic, much simpler and does provide an adequate explanation for the phenomena. It is also a common-sense accounting which most lay-people can understand, especially after watching a film of the initially naïve bird slowly being shaped into passing the mirror test criterion. The need to use hypothetical mental entities such as self-concept completely evaporates once a thorough account is provided of an organism's learning history. The behavior analyst is interested in developing similarly parsimonious explanations (defined as the ability to both predict and control behavior) for a wide range of infrahuman and human activities, particularly those commonly considered to reflect the operation of supposedly mental mechanisms, whose existence is inferred from the behavior they are said to cause. Examples include thoughts, wishes, dispositions, impulses, drives, insight, self-concept, and so forth. In general, any occult entity said to reside within the individual and cause him or her to act in certain ways is grist for the conceptual and methodological millstones of the behavior analyst. To the extent that the behaviors said to be caused by these inner entities can be more parsimoniously explained, predicted, and controlled for by a retrospective examination of a person's learning history, and/or the contrived prospective manipulation of contingencies of reinforcement, punishments, and other aspects of one's environment, the need to hypothesize these inner causes is eliminated in favor of a more parsimonious accounting requiring fewer assumptions and inferences. Theoretical and experimental attempts, considered successful by many, include efforts to behaviorally operationalize a variety of these presumptive inner causes, including various psychological terms (Skinner, 1945), anxiety (Skinner & Estes, 1941), psychosis (Skinner, 1956), the ability to solve problems (Skinner, 1966), consciousness (Skinner & Blanshard, 1967), value judgments (Skinner, 1971), and feelings (Skinner, 1987). Many other behavior analytic writers have continued this tradition in the hope to move the field away from attempting to explain behavior on the basis of hypothetical inner causes to variables external to the individual.

Just as physical science gave up concepts such as the aether and phlogiston when more parsimonious and empirically supported ways to explain the transmission of light through a vacuum, or of the process of combustion were developed, it is hoped that similar advances can be promoted in explaining human behavior. For example, various racial groups in the United States score differently, on average, when taking standardized tests of intelligence. Rather than viewing these differences as a function of some innate attribute called 'intelligence', the behavior analyst explores the role of the stimulating (or lack thereof) intellectual environment in which children are raised. It has been shown that racial disparities on intelligence tests are largely attributable to the degree to which children are exposed to a varied and challenging verbal community and to stimulating parental and familial interactions (Hart & Risley, 1995). African-American children tend to do less well on standardized tests, likely due to a more impoverished intellectual and family environment, whereas Asian students tend to out-perform Caucasian children because of the greater number of hours in the school day, number of days of school attended to per year, and to cultural practices involving students attending enrichment classes after regular school hours. This behavior analytic perspective is also an optimistic one. It is technically possible to enrich children's intellectual environments, and to improve their test score, but we have yet to develop a method to improve 'intelligence' directly, such as by manipulating one's genetic endowment.

Morgan's Canon was important in the latter part of the 1800s and early 1900s in reducing anthropomorphism in comparative psychology, and behavior analysts attempt to use it as an intellectual and methodological lever to pry out the lingering tendency to ascribe human

actions to internal and hypothetical entities, cleverly labeled dryads (an inner spirit inhabiting trees) by Ebel (1974) in a brilliant essay on this topic.

A Rejection of Dualism and Mentalism

The behavioral perspective on two issues has been alluded to above and are closely linked. If one is committed fully investigating a materialist, naturalist, physicalist account of behavioral phenomena, little attention is given to dualist (the human consists of the material body and the immaterial mind) or mental mechanisms as meaningfully coherent explanations.

- «Being opposed to mentalism is at the very heart of what radical behaviorism is all about…When talking of inner states, Skinner does not deny that they exist. Rather he disputes the causal role that functionalism assigns to them» (Day & Moore, 1995, p. 81).
- «I am a radical behaviorist simply in the sense that I find no place in the formulation for anything which is mental» (B. F. Skinner,, cited in Day and Moore, 1995, p. 83).
- «Behaviorism … rejects a dualistic view of the person that divides the person into behavior and something else and which consequently treats behavior as the superficial manifestation of processes taking place at some other, inaccessible, unobservable, and usually hypothetical level» (Chiesa, 1994, p. 201).
- «The practice of looking inside the organism for an explanation of behavior has tended to obscure the variables which are immediately available for a scientific analysis. These variable lie outside the organism, in its immediate environment and in its environmental history» (Skinner, 1953, p. 31).
- «The philosophy of a science of behavior treated as a subject matter in its own right apart from internal explanations, mental or *physiological*» (Skinner, 1989, p. 122, emphasis in original).
- «A radical behaviorism denies the existence of a mental world, not because it is contentious or jealous of a rival, but because those who claim to be studying the other world necessarily talk about the world of behavior in ways which conflict with an experimental analysis» (Skinner, 1969, p. 267).

Avoidance of Reification and Circular Reasoning

Reification involves transitioning from discussing a hypothetical entity as a descriptive label to ascribing reality status to that entity. Here are two similar formal definitions:

- «The error of regarding an abstraction as concrete, and attributing causal powers to it» (Corsini, 2002, p. 822), and
- «Acting as if one believed that the abstract or hypothetical were real. From a purely rationalistic perspective it is a cognitive/emotional act of children and other unsophisticated folk; in reality it is one of the more seductive ways in which social scientists distort and misrepresent the status of many of their hypothetical entities and constructs » (Reber, 1985, p. 651).

Behavior analysts contend that viewing human activity as the expression of some inner, causal agent of the mind usually represents the logical error of reification. For example, in 1977 Bandura postulated the existence of an inner agency he labeled self-efficacy. Self efficacy was said to be causally responsible for much that human beings did, or failed to undertake. A PsycINFO search undertaken by the author on 3 October 2009 found no articles with the term self-efficacy in the title, prior to 1975, and over 5000 published since 1975. This concept of

self-efficacy, as originated by Bandura (1977), has proven to be incredibly generative of conceptual research in a wide array of areas. Productive and heuristic, self-efficacy possesses these theoretically desirable qualities to a very strong extent. However from the perspective of behavior analysis, when used to *explain* the actions of people the term is both a reification and promotes the logical error of circular reasoning. Bandura (1977) himself said that the best way to change self-efficacy in a positive way is to provide individuals with successful experiences in mastering some task. One can explain a person's reported high levels of self-efficacy largely on the basis of their past successful *behaviors*, behaviors which have been reinforced. Their subsequent behavioral skills, *and* sense of self-efficacy are seen more parsimoniously as a direct function of these past person-environmental interactions. There is no need to postulate that success enhances self-efficacy, and this enhanced self-efficacy in causally responsible for one's future anticipated and real success in mastering other task. The behavior analyst is more likely to explain currently successful behavior directly on the basis of past successful learning experiences, bypassing the need for any internal causative agents such as self-efficacy.

Similarly, Seligman (1976) hypothesized that exposure of an animal to inescapable aversive events engendered an internal state he labeled 'learned helplessness'. Moreover, this internal state of learned helplessness, once created by the negative vicissitudes of life, was causally responsible for an individual's subsequent lethargic behavior, depressed affect, and inability to resolve life's difficulties, in a word, depression. The behavior analyst asks 'Why is it necessary to insert this hypothetical inner causal mechanism?» It is more parsimonious to explain depression (behaviorally, affectively, and cognitively) on the basis of exposure to punishing, often difficult to escape, life experiences. Seligman himself found that the best way to help animals overcome his experimentally-induced learned helplessness and learn to escape from aversive stimuli was to manually move them away from uncomfortable shocks to a safe part of the cage. With some negatively reinforcing experiences like this behind them the animals soon resumed their previous practice of moving away from aversive stimulation to a safe part of the cage, as opposed to passively lying there without attempting to escape.

Here is what Ebel (1974) had to say about the practice of inventing inner causal agents:

> in the physical world today there is less room for supernatural influences than there used to be. There are fewer homes for dryads. But in the world of the mind, some of them still seem to lurk in the woods and thickets. Of course they are not called dryads. Those who speak with scholarly care call them hypothetical constructs. Sometimes they are called traits…Those who accept them as real and important do not inquire too closely what stuff they may be made of. But they are cherished because they seem to explain why different people behavior in different ways (p. 485)…as the mind took up residence principally in the brain, the dryads reappeared in the guise of mental faculties; attention, perception, memory, reasoning, imagination, will power and the life (Ebel 1974, p. 486).

The invention of these dryads of the mind can give rise to circular reasoning, as in:

- «The only evidence we have that a person is more or less intelligent is that he behaves more or less intelligently. To say he behaves intelligently *because* he possesses intelligence is completely circular…The only evidence we have of a person's creativity is the quality of his creations. To say that he creates effectively *because* of his creativity is completely circular…Careful psychologists are no doubt aware of this kind of circularity and do their best to avoid it. But when hypothetical constructs are used to explain observed behavior, it is very difficult to avoid…In none of these cases do we

have any evidence for the existence or nature of the presumed cause apart from the effect it is supposed to produce» (Ebel, 1974, p. 486).

- «When we say that a man eats *because* he is hungry, smokes a great deal *because* he has the tobacco habit, fights *because* of the instinct of pugnacity, behaves brilliantly *because* of his intelligence, or plays the piano well *because* of his musical ability, we seem to be referring to causes. But on analysis these phrases prove to be merely redundant description» (Skinner, 1953, p. 31).

How can one escape this potential trap?

> To validate an explanatory construct one must show that it is functionally related to home behavioral variable rather than the one is was invoked to explain (Ebel, 1974, p. 490)
>
> and
>
> The objection to inner states is not that they do not exist, but that they are not relevant in a functional analysis. We cannot account for the behavior of any system while staying wholly inside it: eventually we must turn to forces operating upon the organism from without (Skinner, 1953, p. 35).

Thus to say that my client meets the current diagnostic criteria for specific phobia, because she is observed to scream and run away at the sight of dogs, has a very high heart ate in their presence, and talks a great deal about her fear of dogs, is to perhaps legitimately describe in a summary form her condition. To say that she *has* a phobia commits the error of reification. There is no evidence for the existence of the entity labeled a phobia apart from the avoidant behaviors, fearful self-reports, and physiological arousal the phobia is said to cause. This is reification. To further say that she runs away screaming from dogs *because* she has a phobia commits the error of circular reasoning. The solution is to always refer to labels such as phobia (or self-efficacy, learned helplessness, intelligence, the superego, etc.) as unproven hypothetical constructs, and to seek causal explanations external to the individual. For example, my client displayed no phobic behavior towards dogs until, at the age of 67, she was savagely attacked by a large dog. Since that time she avoided dogs whenever possible and was very fearful in their presence. The presumptive *cause* of her phobic behavior, elevated heart rate when near dogs, *and* self-reports of high anxiety related to dogs, can all be parsimoniously accounted for by her terribly aversive experience of having been attacked by a dog. This is a true anecdote, by the way, (see Thyer, 1981) and demonstrates the superior explanatory utility of seeking environmentally based explanations over hypothetical inner mechanisms of causation.

Here is other ways behavior analysts have addressed this problem:

> behavior itself can be taken as nothing but 'evidence' for a disposition to behave at a particular time in one way rather than another. The dispositions then become inner entities of focal importance in any analysis. As a result, when people speak of dispositions to behave, the opportunity all too frequently presents itself for reifying these dispositions to behave back into some special kind of causal inner state. Doing so, of course, turns dispositions into causes that are just as occult as any mental cause, an ironic turn of events for a position trying to circumvent any appeal to mental events (Day & Moore 1995, p. 79).

Pragmatism

Pragmatism is «A philosophical doctrine in which values, meanings and truths of proposition are taken as equivalent to the practical, empirical consequences derivable from them» (Reeber, 1995, p. 587). Here is how a number of writers have linked behaviorism and pragmatism:

- «Skinner's (1956) autobiographical case study describing this approach to psychology is a powerful endorsement of pragmatism…The description is of research methods designed to obtain useful results, that is, results that lead to the prediction and control of behavior. The greater significance of the review is that it is an epistemological statement: Knowing about something is achieved by making it work» (Lattal & Laipple, 2003, pp. 49-50) «…Skinner's position with respect to theory and in more general terms is thoroughly and undeniably pragmatic…» (Lattal & Laipple, 2003, p. 51).
- «It is clear that Skinner's views on the goals of science, as well as his general view of truth, could be described as strongly pragmatic in nature» (Leigland, 1999, p. 483).
- «The philosophy of radical behaviorism is a descendent of the pragmatism of C. S. Pierce. Truth is 'successful working' in the words of one modern behaviorist» (Staddon, 2001, p. 96).
- «Explanations that assist the scientist in dealing with behavior in a productive way, of that support the expert in solving the same kind of problems efficiently, will be considered valid» (Toueinho & Neno, p. 64).
- «The ultimate criterion for the goodness of a concept is not whether two people are brought into agreement but whether the scientist who uses the concept can operate successfully upon his material – all by himself if need be. What matters to Robinson Crusoe is not whether he is agreeing with himself but whether he is getting anywhere with his control over nature» (Skinner, 1945, p. 293).
- «Responses to some forms of stimulation are more likely to be 'right' than responses to others, in the sense that they are more likely to lead to effective behavior» (Skinner, 1953, p. 139).
- «Scientific knowledge is verbal behavior, though not necessarily linguistic. It is a corpus of rules for effective action, and there is a special sense in which it could be 'true' if it yields the most effective action possible» (Skinner, 1974, p. 235).

Herein we obtain glimpses of the epistemological position of the radical behaviorist. We 'know' something truthful about behavior-environment relations when we can effectively predict and control behavior. Note that this is a limited form of 'knowing'. Behaviorists avoid discussing knowing or truth in ideal terms. Typically the search for causes of behavior is eschewed in favor of the more modest goal of attempting to establish functional relationships between the manipulation of contingencies of reinforcement and punishment, and changes in behavior. This functional analysis of behavior (FAB) is illustrated in the practice of applied behavior analysis, wherein (typically) a problem behavior is identified, careful observations in the person's natural environment are made about what precedes the targeted behavior, and what consequences follow it. Analysis may also be undertaken of possibly relevant biological factors (illness, medications, states of deprivation such as thirst or hunger, etc.). These observations may lead to tentative guesses about factors which appear to influence the target behavior. These guesses are tested by the deliberate alteration of environmental stimuli to see if they effect the target in the predicted manner. If this is actually observed, and the effect

replicated, then in a very real sense we know how to predict and control behavior (at least in a limited sphere) (see discussion by Tourinho & Neno, 2003).

A concrete example may help. A behavior analyst is asked to consult regarding an elementary school child who is constantly demanding attention from the teacher. It has reached the point of being intrusive to the classroom's normal activities and to teaching. The behavior analyst observes the child in his/her classroom setting and notes what happens before and after episodes of demanding attention. It may appear that attention seeking is mostly likely to occur when the child is not working on her assigned tasks, and it may also be obvious that the child is generally ignored while she is working, and that she appears to enjoy the teacher's attention. Attention-seeking behavior could be operationally defined and measured over some representative time period (called the baseline) during which no specific intervention is introduced. Then, after the child's baseline is stable (e.g., not clearly going up or going down) the behavior analyst could introduce a simple contingency-based intervention, namely the teacher is instructed to periodically walk over to the otherwise demanding student, *but only* when she is appropriately working, and quietly praise her for being on task. When the student is off task and demands attention, she is quietly directed to get back in her seat and resume working, with no other attention being paid to her. This manipulation is maintained for a sufficiently long period of time for it to be conspicuously obvious if the child has increased her on-task work and decreased her demands for attention. These data would be formatted in a simple line graph and inferences made about changes in behavior via visual inspection. If there were no obvious changes, then it would appear that the behavior analyst missed picking up on the genuinely relevant factors responsible for the demanding of attention. If however, under the new regimen on-task behavior increased and inappropriate demands for attention declined, then this would support the view that teacher attention was inadvertently reinforcing the child's demands for attention. To complete the functional analysis the behavior analyst would arrange for the teacher to resume her prior practices of ignoring the student when she was diligently working, and of paying attention to be when she made inappropriate demands. If the child's behavior reverted to near baseline levels, this is stronger evidence that teacher attention was inadvertently maintaining the inappropriate demands. The final step to complete the FAB would be to reinstate the new contrived contingency program, and have the teacher resume the practice of attending the student only when she was on-task, and of redirecting her to her desk when she made inappropriate demands. If improvements were again seen, it would appear that the behavior analyst had completed the functional analysis and found a way to predict and control the behavioral patterns which were the cause for the original referral.

Has the behavior analyst discovered the true 'cause' of the child's inappropriate attention-seeking? Yes, if the goals of prediction and control have been satisfactorily met. Further analysis beyond a satisfactory demonstration of the functional relationship between an intervention and behavior change often leads to the problem of infinite regress. For example it could be asked why the child listened to the teacher, or followed her pointed finger. At one level, an answer to this question could involve explanations related to optics or acoustics. From the eye, further explanations would involve the anatomy of the eye and physiology of the retina and from the retina to the conduction of electrical impulses along the optic nerve to the brain. From there the neurochemistry of synapses, the actions of molecules, the operations of atoms, neutrons, protons, and electrons, and then down to the level of other sub-atomic particles, could all be invoked as possible 'causes'. However, from a pragmatic perspective, the problem was solved by manipulating how the teacher provided attention to the demanding student. No further level of analysis is required. This illustrates how, curiously, behavior analysis is a *non-reductionistic* science. The analysis focuses on the level of person-

environment interactions, delves no deeper, and invokes no more fundamental levels of analysis. Further investigations may indeed be useful, say of physiological variables, but these are outside the field of behavior analysis.

Epistemology

Given the above, it would appear the epistemological position of the behavior analyst is a rather limited one, involving the effectiveness of the techniques of behavioral control derived from experimental research. In a real sense the position is «If it works, it is right» This is a more modest goal than that undertaken by traditional philosophical approaches to epistemology, as described by Garrett (1999, p. 69): «As responsible thinkers we all want to hold a belief if and only if it is true. The central goal of epistemology is, therefore, to help us distinguish truth from falsity» and «our goal is not simply to believe what is true, but also to *avoid believing what is false*»(p. 78, emphasis in original). Garrett (1999) rightly points out that both criteria, believing 'truth' and disbelieving 'falsity' are important. If one only values the first principle then one, to be secure, should believe *everything*, as truthful beliefs would be thereby encompassed. If only values the latter principle, avoiding falsity, then one should adopt the position of the radical or Pyrrhonian skeptic and believe *nothing*, since false beliefs would thereby be excluded. The crux of the matter is arriving at the right balance to successfully adopt true beliefs and reject false ones. For the behaviorist, the solution consists of experimentation, what the scientists of the mid-1800s called a positive demonstration. Using the methodology of an experimental analysis, the behaviorist can make a valid determination of the relationship between selected environmental manipulations (e.g., introducing certain arrangements of reinforcement, punishment, shaping, discrimination learning, schedules of reinforcement, etc.) and their subsequent effects on behavior. Given sufficient successful demonstrations of an apparently functional relationship, one tentatively generalizes to other circumstances, including the future. The supposed problem of induction is largely ignored. If something has been reliably demonstrated in the past, one may assume with modest assurance that the functional relationship will be valid in the future. Not with certainty, but with some measure of practical assurance. An assurance every critic of induction makes use of when they seek to turn on the lights, flush the toilet, or start an automobile. Certain operations have produced certain effects fairly reliably in the past, and we both infer and act on the supposition that these operations will remain similarly effective in the future.

In Skinner's view:

- «We may now take that more humble view of explanation and causation which seems to have been first suggested by Mach and is now a common characteristic of scientific thought, wherein, in a word, explanation is reduced to description and the notion of function substituted for that of description» (Skinner, 1931/1972, pp. 448-449).
- «The terms 'cause' and effect' are no longer widely used in science…A 'cause' becomes a 'change in an independent variable' and an 'effect' a 'change in a dependent variable'. The old 'cause-and-effect' connection becomes a 'functional relation.' The new terms do not suggest *how* a cause causes its effect; they merely assert that different events tend to occur together in a certain order…We want to know why men behave as they do. Any condition or event that can be shown to have an effect upon behavior must be taken into account. By discovering and analyzing these causes we can predict behavior; to the extent that we can manipulate them, we can control behavior» (Skinner, 1953, p. 23).

- «So Skinner's view is that knowledge is action, or the capacity to act. To know a thing in the world is to act or to have the capacity to act differentially with regard to it. To know a thing slightly is to have a limited capacity for differential action regarding the thing; to now a thing thoroughly is to have a comprehensive repertoire of behavior regarding it» (Schnaitter, 1987, p. 59).

The search for absolute truth is dismissed by the behavior analyst as an essentially unresolvable problem. And this dismissal is a feature the behaviorists share with the logical positivists, who postulated as their *verificationist principle*, the assertion that a statement is meaningful if and only if, it is, in principle, capable of being convincingly verified or falsified. Statements incapable of such resolution were dismissed by the logical positivists as essentially meaningless or as pseudoproblems. Pseudoproblems are themselves problematic if for no other reason as they distract the attention of researchers from solvable issues, and divert large resources of time and energy in their investigation. This of course leaves large segments of the metaphysical domains of philosophy excluded from serious consideration (What is the good? What is beauty?) , and similarly the behaviorist tends to avoid (if not dismiss) discussions of ultimate truth, goodness, or cause.

The perspective that the utility of some behavior (its truthfulness, in a limited sense) in helping us interact with the world says nothing about the ethical appropriateness of the behavior. If I am the strongest passenger on a life boat, I may survive, through recourse to cannibalism, long enough to safely reach land. In terms of my personal survival this could be seen as a reinforcing outcome. That does not necessarily make it a desirable one. Staddon (2004) addresses the problems of the naturalistic fallacy implicit in behaviorist epistemology, the view that what *is* is synonymous with what *ought to be* or what is good. For example,

> Skinner gave us no real answer to the ancient questions 'What is the good?" Uninterested in pure speculation, driven to action, he was much more interested in using science to do good than in using philosophy to discover what good is. Radical behaviorism purports to tell us how to modify the environment so that people will act virtuously. It neglects to specify what virtue is (Staddon 2001, p. 97).

This is not necessarily a weakness. Extending the purview of science to domains in which it lacks adequate expertise commits the sin of scientism, the unjustified application of scientific opinion into non-scientific areas.

Moore (2008, pp. 435- 436) provides a series of statements which summarize the behavior analytic position on epistemology:

> 1. The most significant and relevant form of human behavior said to show scientific knowledge is operant behavior, which is analyzed in terms of contingencies of reinforcement that control both the verbal and nonverbal operant behavior of the scientist.
>
> 1.1 Scientific knowledge does not differ in principle from any other kind of knowledge…
>
> 1.2 Knowledge is power, and the fundamental issue is the extent to which claims of scientific knowledge function as forms of discriminative stimulation that contribute to effective action with respect to the environment.
>
> 2. Claims of scientific knowledge (e.g., theories, explanations) and the terms or concepts therein are instances of verbal behavior; they are always and have only been matters of differential behavior in differential circumstances. Any such claims of scientific knowledge may therefore be analyzed in terms of contingencies of reinforcement that control the verbal behavior of the scientist in question.

2.1 The same principles that apply to understanding the sources and development of operant behavior in general apply to understanding the sources and development of (verbal) behavior said to show scientific knowledge.

2.2 The sophisticated (verbal) behavior said to show scientific knowledge develops and is maintained through the action of environmental consequences, which selects its most important forms.

3. Some elements may well be accessible to only one individual. Those elements may be parts of the contingencies controlling the behavior of the scientist doing the speaking or of the person being spoken about…

4. Accounting for knowledge claims is not a matter of appealing to unobservable acts, states, mechanisms, processes, structures, or entities elsewhere in some other dimension, at some other level (e.g., neural, mental, cognitive, conceptual, psychic, hypothetical, subjective), for which observable behavior is the license that makes such appeals scientifically respectable […] (Moore 2008, pp. 435- 436)

This summary by Moore (2008) encapsulates much of the preceding discussion.

Behaviorism is keen interested in epistemology, and define knowing as our ability to predict and control behavior. It established a rigorous methodology for exploring, developing and demonstrating such predictive and controlling technology, and applies this successfully to human affairs.

SUMMARY

A short anecdote is appropriate to include in this closing section

> When the young Skinner was told by Alfred North Whitehead that a psychologist should closely follow developments in philosophy, Skinner replied, It is quite the other way around—we need a psychological epistemology (Skinner, 1984, p. 29) (cited from O'Donohue & Ferguson, 2001, p. 70).

B. F. Skinner and those associated with behaviorism have attempted to develop an epistemological position, one based on a number of conventional philosophical assumptions, each of which is axiomatic, being seen as almost self-evident but incapable of strong philosophical or experimental proof. With its central concern over developing effective demonstrations of the ability to predict and control behavior, behavior analysis has an epistemological position that is both limited yet far-reaching. Limited in the sense that traditional concepts of how one arrives as knowledge of 'truth' are seen as unresolvable, and thereby dismissed from serious consideration as pseudoproblems. To the extent one can effectively predict and control behavior, one has arrived at a limited but truthful understanding of functional relationships which are valid across persons, and over time, relationships between environmental events and their effects of behavior. However this limited epistemological understanding is at the same time far reaching in the sense that when tested using the methods of the experimental analysis of behavior and extrapolated into the world of helping persons solve socially significant problems through the field's practice domain, applied behavior analysis, the results have been astonishing. As demonstrated in an incredible array of disciplines and problems, behavior analysts have, over the past 50 years, developed an effective technology of human behavior change with practical and effective applications to education, medicine, psychology, social work, economics, and most human service professions. As a conceptually consistent model, behavior analysis enjoys an evidentiary foundation, conceptual richness, and applicability achieved by no other approach

to human behavior change. This is a significant achievement, one made possible by the field's narrow focus on only one aspect of epistemology – If a technology works in reliably controlling human behavior, then one has achieved some level of a valid, truthful understanding of the real world. And that, says the behavior analyst, is what epistemology is all about. As inscribed on the tomb of Karl Marx in London's Highgate cemetery, «The philosophers have interpreted the world in various ways. The point however, is to change it».

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Generalizing Through Conditional Analysis: Systemic Causality in the World of Eternal Becoming

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ABSTRACT

The chief goal of any scientific endeavor is generalization. Kurt Lewin was a crucial figure in psychology who advocated for and created a system in which both the particular phenomenon in its richness and general laws could be established. Lewin emphasized the need for broad ranging concepts that would do justice to human existence and social reality. His credo - unify without undue simplification - is the cornerstone of science. We can see, on the one hand, Lewin's desire to remain loyal to the basic assumptions of science (e.g., laws, generalizability, explanations) and his dedication, on the other, to treating the whole person and the richness of human and social reality. It is clear from Lewin's look at science - *Wissenschaft* - that the epistemological treatment of generalization takes the form of catalysis (in Lewin's terms *conditional-genetic* explanation) – the study of conditions under which something happens. The study of catalytic nature of phenomena views the parts of a system, interacting with the other parts, to form a Gestalt - a whole that entails the person, the environment, and their relationship (*Umwelt*). In this approach, the phenomena cannot be studied as a-contextual, but embedded within a dynamic and changing system of becoming. There is a creative tension between the general and particular in Lewin's system of thought and his emphasis on holism. We address how the goals of finding regularities and laws co-exist with the notions of a dynamic and ever-changing world. How can social reality be modeled in any static terms if it is always in flux?

Keywords: Generalization, Catalysis, System, Holism

It is mildly ironic that the question of generalization - a self-obvious goal of any *Wissenschaft* - is being disputed in the beginning of the 21st century. There are two general impulses - seemingly contradictory - undergirding much of the thought of psychologists such as Gordon Allport, Abraham Maslow, Carl Rogers, William Stern and Kurt Lewin. The first is an impulse towards a psychology that embraces the unique human being in the process of becoming. The second regards ensuring and promoting psychology as a science that establishes basic knowledge. Thus the former reaches towards concreteness, richness and fullness of human experience and the latter towards abstraction and the derivation of laws. Reconciling these dual projects has proven to be problematic since they have typically been regarded as antithetical and one must be sacrificed at the altar of the other. We here prove that such either-or attitude misses the point - it is precisely in the rich details of the phenomenology of

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the whole where generalized laws operate. In the biological, social, and psychological systems generality of laws results in amplified variability and uniqueness (Maruyama, 1963, 1999).

However in psychology today, the goal of generalization looms large in discussions of external validity and whether a researcher's findings apply beyond their sample to a wider population. This is a result of narrowing down psychologists' search for knowledge to the use of inductive generalization techniques. This inductive focus has been further fortified by the postmodern approaches that emphasize particular descriptions and localized knowledge construction - and deny the possibility of generalizations all together. That is, the very particular, relational and situated conditions of phenomena have seemingly precluded the establishment of general laws. The impulse of transferring knowledge from one context to another and establishing general laws are contradictory to the impulse of a postmodern approach that tries to do justice to the richness of human existence. Ironically, it fails precisely in that. Indeed, great novelists who describe the dramas in the lives of their invented characters - always particular single cases! - are appreciated precisely because they intuitively trigger generalization tendencies in their readers. Post-modernist ideology has made social sciences not into a depository of local knowledge, but of local stories of no generalizability. Anyone who wants to learn about the realities of the human condition gets more insight from the fictional characters such as Anna Karenina, Aureliano Buendia, or - even - Harry Potter. Something has gone dramatically wrong in the social sciences over the past few decades that the question of generalization could be not only found difficult to solve, but even denied as a problem in need for a solution.

As irreconcilable as the fidelity to the phenomena in its concreteness and the generalization of universal and lawful knowledge may seem to contemporary social scientists, we will visit this possibility. Does the uniqueness of psychological phenomena preclude generalization? If not, what then is the path towards generalization in the dynamic world of becoming?

GENERALIZATION *THROUGH* CONCRETE UNIQUE SITUATION

We address and elaborate generalization *through* the concrete situation and the uniqueness of human living. This challenging endeavor has been undertaken by various thinkers, including those we mentioned above. Gordon Allport (1955) succinctly described the nature of the dilemma:

> The first fact that strikes us is the uniqueness of both the process and the product. Each person is an idiom unto himself, and apparent violation of the syntax of the species. An idiom develops in its own peculiar context, and this context must be understood in order to comprehend the idiom. Yet at the same time, idioms are not entirely lawless and arbitrary; indeed they can be known for what they are only by comparing them with the syntax of the species. (Allport 1955, p. 17)

The crux of the dilemma stems from the view that uniqueness - including context dependence - and transitory nature of human life and lawfulness are incommensurable. Kurt Lewin is widely considered the founder of social psychology and action research and, as we will discuss below, considered generalizability to be a chief aim of science and his research. Lewin was concerned with describing particular, concrete, and dynamic social worlds, as we will discuss below. Yet, Lewin also considered the development of abstract conceptual ideas and general analytical tools crucial to creating social change (De Rivera, 1976). Here we can specifically see the dialogue between the particular - a dynamic social world - and the universal or general law - how to change the social world, or social change. In this sense, Lewin's work created a

dialogue between what we might call "basic psychology" and "applied psychology". For such a dialogue to occur, Lewin had to consistently deal with generalization from the particular to the universal. Instead of finding this an insurmountable problem and avoiding or reducing the complexity and uniqueness of human existence, Kurt Lewin argued that it was only by taking into account the very concrete and unique situation which includes the person that generalization is possible.

How did Lewin come to this insight and what are the implications for psychology today? We address these questions by examining Lewin's treatment of the relationship between individual events and law in his treatment of the conflict between what he called the Aristotelian and Galilean modes of thought (Lewin, 1931, 1935, Valsiner, 1984). Lewin offers ample inspiration for possible pathways towards generalization while emphasizing the situation. We will then discuss *systemic causality* and how the notion of catalysis offers a promising conceptual tool that enables generalization.

Theory and Action: Tensions in the Lewinian System: Kurt Lewin posited that psychology had reached a stage where empirical work and the *piling up of facts* had become a detriment to the progress of psychology (Lewin, 1936). This evaluation is even more true now, 70 years later. Although empirical investigations provide a corrective to speculative philosophy, Lewin argued, it is the combination of theory and empirical research that is needed in psychology. Indeed, Lewin clearly emphasized the need for broad ranging concepts that would do justice to human existence and social reality and, therefore, called for concepts that could "unify without undue simplification; they must include both person and environment, both law and individual case" (Lewin, 1936, p. 7). We can see, on the one hand, Lewin's desire to remain loyal to the basic assumptions of science (e.g., laws, generalizability, and explanations) and his dedication, on the other, to treating the whole person and the richness of human and social reality (Reber & Beckstead, 2009).

This posture - natural for any scientist at his time - can be found throughout Lewin's writings; however, his analysis and discussion of the shift between Aristotelian and post-Galilean physics offers a penetrating analysis of the theoretical shortcomings of psychology as well as providing a nice philosophical grounding of his work and thought, especially in relation to the theme of generalization and overcome "the opposition between universal concept and individual event" (Lewin, 1936, p. 8). Notably, Lewin's (1927, 1931) reflections on the conflict between Aristotelian and Galilean modes of thought illuminate the development of lawfulness that continues to undergird psychology and how this is interrelated with the process in Aristotelian thought of fixing the locus of causality in the object with the upshot that objects and events become detached from, analyzed, and understood apart from their surrounding environment and isolated from other objects.

Aristotelian and Post-Galilean Physics: Lewin's critique of psychology and the development of his ideas stemmed in part from his comparison of Aristotelian and post-Galilean approaches to physics (Lewin, 1935). For Lewin, the use of statistical analyses - based on large samples - is largely derived from an Aristotelian framework that grouped objects into binary and homogeneous classes (e.g., cold and warm, light and heavy) and required events to be regular and frequent (e.g., orbit of heavenly bodies) in order to be considered lawful (and thus the object of inquiry for science). At the time of his thinking about the issue - late 1920s - the avalanche of the "empire of chance" (Gigerenzer et al., 1989) onto psychology was only starting.

The classificatory system in Aristotelian physics grouped objects and phenomena together around the *principle of commonality*. Accordingly, objects and events were grouped together

based their apparent similarities - what they had in common. In return, the explanation of a particular object's behavior was derived from these apparent similarities. For instance, *things that were light* (e.g., smoke, balloons, sheets in the wind) were grouped together because of their apparent quality of "lightness". Things that were light typically went up in normal conditions. This meant certain objects had an *upward tendency*. Objects within the classificatory group which exemplified this behavior/movement - an upward tendency - was explained to be caused by the essence of the object - the characteristic of being light. The immediate result is that phenomena become grouped *and* explained by the characteristics shared by the group. In our example, things were grouped by their shared light quality and the objects upward tendency was then explained by light quality. In this sense Aristotelian mode of explanation is inherently tautological in that phenomena are explained by the essence of the group in which it was included. All members of "class X" are characterized by the quality of "X-ness" being attributed to each and every member of the class.

Lewin pointed out that such kind of circular reasoning was quite common in psychology. He noted that circular reasoning leads to a kind of logic that, since negativity (i.e., Lewin is referring to a trait such as stubbornness) is frequently observed in many three year olds, negativity is inherent in the nature of three year olds (Lewin, 1935, p. 15). This logic further serves as the explanation for the appearance of a concrete case of negativity (ibid). This tautology is evident today throughout psychology - especially in cross-cultural psychology as differences between groups are interpreted to be caused by the static essences of "gender" or "culture" (Valsiner, 2007, p. 26). This approach and logic makes possible the sample-to-population form of generalization; however, these generalizations are undergirded by grouping individuals into homogeneous classes and transforming classes such as "American" into essences that then become projected into the psychological systems of the individuals and are posited as the causes of differences that are assumed by the *a priori* categorizations. Negativity.

Equally problematic for Lewin was that the classificatory system of Aristotle had profound implications for notions concerning lawfulness, and hence generalization. To reiterate, the essence of a thing and its behavior was derived by the class of objects it was grouped with based on abstract, common features. These common features, further, are based on the tendencies of objects (e.g., smoke and balloons have the tendency of upwardness) and linked with how frequent an event occurs. As Lewin (1935) commented, «It is these frequency rules …. that determine the nature and tendency to be ascribed to each class of objects» (p. 7). That is in order to glean the tendency of any object it must be occur repeatedly and frequently. Thus the criteria for lawfulness from the Aristotelian view centered on commonality and frequency of objects and necessarily exclude one-time or individual events. Lewin (1935) argues that this approach inevitably leads to the conclusion, "that which does not occur repeatedly lies out of the realm of the comprehensible" (p. 14). This frequentist model, Lewin argued, permeated (and currently – 70 years later - dominates) psychology through the use of statistics and the deriving what the group has in common via the mean. Of course psychology's reliance on the mean remains unabated - whether this is in experimental or survey methods - and replication remains a crucial (and yet seldom non-utilized in current psychological research) criterion for generalization.

The consequences of a frequentist approach are at least threefold. First, individual events are viewed as incomprehensible and arbitrary since "lawfulness is believed to be related to regularity, and considered the antithesis of the individual case" (Lewin, 1935, p. 17). Of course, this emphasis on frequency and regularity has lead psychology to institutionalize statistics as *the* path for constructing knowledge and laws. Furthermore, as Lewin (1935) notes, "such

statistical investigations are … unable as a rule to give an explanation of the dynamics of the processes involved" since they adhere to the same tautological traps mentioned above (p. 21). Individual cases must be combined to form averages. De Rivera (1976) describes the implications of this approach

> The effect of this focusing on averages rather than individuals is that the experimenter makes the objective conditions the "subject" of the experiment, rather than his subjects. It is presumed that a particular experimental situation[1] has the same meaning for each subject (or each subject within some designated class). Since, in fact, different subjects may give an objective situation quite different meanings, and since apparently similar responses may also have different meanings, it is evident that different processes may be occurring in different subjects. Consequently, there are many different possible interpretations, and one is not sure of their theoretical significance. (Rivera 1976, p. 11)

As a result of focus on regularity and the high value placed on observation and classification instead of theory-building, the individual and non-repeatable events - and thus uniqueness - are forcefully eliminated from psychological investigation. Consequently, as Gordon Allport observes, psychologists interested in studying human beings are lead to "look for universal processes common to the species" and study "mind-in-general rather than mind in particular" (Allport, 1955).

Move to a post-Galilean view: The shift from an Aristotelian approach to Galilean or post-Galilean can be seen in the notion of lawfulness and the change from a strictly historical approach leading to an emphasis of frequency and regularity to the concrete situation and notions of interdependency and interrelationships as fundamentally constitutive of objects. Importantly, the post-Galilean view rejected the Aristotelian idea that regularity, frequency and repetition were the hallmarks of lawfulness. The Galilean mode of thought, then, extended law from solely repeated and frequent events to one-time and unique events. Significantly, this transition to an "all-embracing homogenization and harmonization of the whole field … gave to Galilean physics the intoxicating feeling of infinite-breadth, because it does not, like abstract class concepts, level out the rich variety of the world and because a single law embraces the whole field" (Lewin, 1935, p. 23). The homogenization of law, for the Galilean thought and for Lewin, does get rid of heterogeneity or the "rich variety of the world" but rather allows for both law and uniqueness. Individual cases and events are not opposed; instead, generality is reached through the particular and singular phenomenon. Yet this law (and form of generalization) did not derive from an abstraction that lumped together objects based on common features and tendencies regularly and frequently observed; rather, it was through taking the concrete situation as the basis and then determining the conditions under which different events occur. Every event, then, should be considered lawful in the sense of not a random and ultimately mysterious occurrence.

The Galilean mode thought, therefore, changed the locus of causation away from the *intrinsic properties of* the object to the structural relationships *between* objects. That this, the criteria of frequency and regularity, along with the teleological bent in Aristotelian framework, fixed the lawfulness and causality in the object irrespective of the environment and the relationships with other objects in this environment. Instead, with the shift to the Galilean view "the situation assumes as much importance as the object" and it is "only by the concrete whole which comprises the object and the situation are the vectors which determine the dynamics of the event defined" (Lewin, 1935, p. 29). That is while properties of objects

[1] We can add here questionnaires and surveys (Wagoner & Valsiner, 2005).

belonged to the object irrespective of its surrounding, the Galilean view considered "weight", for example, to be depend on the relationship between the object and environment (Lewin, 1935, p. 28).

Field theory: A holistic framework: The implications Lewin drew from the conflict of Aristotelian and Galilean modes of thought is reflected in his theory. As we have illustrated, Lewin attempted general law and particular situation, and to broaden psychologists' subject matter or view of the human being. Lewin's work, in contrast to many of his colleagues, focuses on developing a psychological science of the whole human being. For Lewin any recourse to inherent personality or essence of an individual independent of the environment could not assist in understanding human behavior. Understanding any human behavior entails understanding the situation of an individual as it exists for the particular person. This was captured in Lewin's classic formula for behavior, B=f(P,E), which states that behavior is a function of the person and environment for that person. Lewin's field theory attempts to describe and specify the here-and-now field in which the person is situated in its totality (e.g., interrelations). The basic postulates of field theory, according to Lewin (1951), "are that (a) behavior has to be derived from a totality of co-existing facts, (b) these coexisting facts have the 'dynamic field' in so far as the state of any part of this field depends on every other part of the field" (p. 25). The whole situation was not static but instead dynamic and comprised of many different non-reducible elements.

Lewin devoted considerable effort to how to represent the field or life space of a person. Again, the person was considered to be interdependent with her environment and therefore could not be understood separately from it. The environment and here-and-now situation was a constitutive part of the person's behavior and one cannot disentangle a person from their environment. Lewin (1936) thus tried to represent all "relevant" aspects of the situation as it existed for the person, and he offered the general guideline that "*what is real has effects.*" In other words, Lewin attempted to represent the heterogeneous field or life space of the person through detailed observation with an eye towards deriving behavior from the "conditional-genetic characteristics" of an object or event. These characteristics were dynamic processes that existed beneath the phenomenal properties.

Towards a new understanding of generalization: As discussed above, Lewin stressed the necessity of theory, but theory had to be tied to and informed by rich descriptions of all relevant aspects of the life-space of the individual. De Rivera (1976) has noted that, "because of the unique situation of every subject, [Lewin] argued for the study of each individual case" (p. 18). Yet for psychology to be a science -in the sense of *Wissenschaften* - it must also discover laws and allow for generalizability. Stressing the value of theory, Lewin asserts that the accumulation of facts through empirical studies cannot "answer … the question that is most important for practical purposes - namely, what must one do to obtain a desired effect in given concrete cases?" (Lewin, 1936, p. 4). Laws, for Lewin, "define functional relationships between different characteristics of an event or situation … [and] are nothing more than principles according to which the actual event may be derived from the dynamic factors of the concrete situation" (Lewin, 1936, p. 11). Field theory was Lewin's attempt to bring theory building into dialogue with the concrete situation. This concrete analysis required the use of conditional - rather than causal - ways of thinking about experimentation (Lewin, 1927) that was a forerunner to our proposal for a catalytic focus in psychology here.

Field theory is not without flaws at the conceptual and practical levels. It has been criticized for its limitations on what can and should be represented in the field and weakness in the

mathematical formulizations (London, 1944). However, de Rivera reminds us that field theory is more of an approach than a rigid system of concepts to be applied to different psychological issues (De Rivera, 1976, p. 4). The richness of Lewin's concepts and his attempts to stay close to the phenomena of concern without undue simplification is impressive. Thus we take Lewin's ideas as suggestive ways to honor both basic knowledge construction and the uniqueness of human life. Paradoxically, it is just this emphasis on the situation as essential to generalized knowledge that is so provocative. Lewin (1935) pointed out that Galileo did not take an average of balls rolling down hills, but rather abstractly conceived of ideal situations and conditions and demonstrated that the behavior of a ball depended on the situation (p. 26).

Instead of ignoring the individual peculiarities of the situation, Lewin's "experimental phenomenology" (see De Rivera, 1976 for a discussion of this approach) and emphasis on describing the situation in as much detail possible reinstated the situation (i.e., person-environment relationship) as the key unit of analysis. Single events were no longer considered incompressible and outside the grasp of science. An additional implication to restoring the single and unique event to investigation is that it has the corollary effect of being closer to human experience. That is, we experience life as meaningful and purposeful, and not arbitrary and random as the Aristotelian mode of thought would imply. Furthermore, Lewin's approach was to connect the concrete to the conceptual and deal with the essential structures (i.e., relationships) that occur beneath observation and description. Instead of generalization based on similarities between common, abstract features, generalization is based on processes underlying the phenomena. Thus the focus on essential structures moves us from breaking phenomena apart and studying elements in isolation to a systemic orientation that is oriented towards *systemic causality.*

FROM CAUSAL VARIABLES TO SYSTEMIC CAUSALITY

The open dynamic system model is essential when studying the effects of the qualitative whole. Parts and their relationship to each other, as well as to the whole, have interrelated functions and effects and therefore cannot be treated as separate variables. Typical analytic study of psychological systems reduced the whole to its constituent parts, their relationships ignored, and only some parts are selected picked for studying. The system cannot be studied unless the aggregate of the qualitative whole is observed as a system - as interrelated parts functioning in relation to one another. This systemic approach was inherent in the Galilean mode of thought and was manifested in Lewin's focus on the concrete situation and behavior as a function of the person-environment relationship.

Modern experimental psychology attempts to reduce a complex system to its constituent parts. This allows for experimentation to produce causality in a one-to-one model - If X is present then Z follows. Such a formula begins to give birth to modern experimental methods, fortified by "If-then" logic (Slife & Richardson, 1995). In order to make sense of phenomena, psychology reduces the complex whole to variables that presumably "interact". Psychologies reduced one-to-one variable equations begin to acquire a mechanistic quality. Stern, who disagrees with the one-to-one causal and mechanical portrayal of the psyche, says,

> The individual is more than an aggregate of physical and psychological phenomena; just what more is entailed here will now be expressed in terms of nonphenomenal attributes. Thus, the nature of these hypothetical components is not mechanical but "personal": the many and fragmented phenomena present within the individual are explained by and unified through attributes that originate or reside in the individual, insofar as the latter is a unitary whole. (Stern 1911, p. 48)

Emotion, cognitions, and actions are not isolated entities, but rather are enmeshed together and grounded in the whole person. Moreover, these inter-related domains are always dynamic. Dynamics implies change, and change implies development. Therefore the generalizations must address and encompass change as long as the individual changes - thusly forcing psychology into a dynamic systems theory and requiring a dynamic model of generalization. Already back in the beginning of the 20th century, William Stern emphasized this point when saying that "the particular is ever subordinate to the general [even as] the general must ever accommodate the individual." (Stern, 1911, p. 31).

Modern psychology, which selectively chooses and ignores parts of the whole in order to make causal statements, must develop a dynamic model - in theory, practice, and generalization. According to Lewin, the properties of a thing are not intrinsic to it but, rather, stem from its interrelationship with other elements in a given system - again reinforcing the dynamic interaction of parts within a system. Indeed, all psychological and behavioral functioning is part of an open and dynamic system. This means that all the processes and components of the system work as a qualitative whole to produce the resulting phenomenon. The validity of causal statements based on reductionist approaches is untenable because they fail to take into account essential aspects of the whole. A study of reduced or selected parts of the qualitative whole may yield an entirely different phenomenon (e.g., attempting to study the affect or religious ecstasy through the firing of neurons).

If phenomena are qualitatively organized by the whole system they are embedded within (and interrelated with) then generalization should not be based on premises of separate variables, but rather *generalization should be based on the premises of generalizing under what systemic conditions something occurs.* There are two important concepts to this epistemological view of generalization. The first is that of Holism - discussed previously. The second concept is that of catalysis - the study of conditions under which something happens (Salvatore, Valsiner, Straut-Yagodzinki and Clegg, 2009). The integration of these two concepts within an open and dynamic system can allow for the production of generalized knowledge that does not "butcher" the phenomena by reducing it to its elemental parts, but rather, describes, understands, and analyzes the phenomena as a qualitative whole.

The Relationships and Interactions in System Theory: In order to generalize the conditions under which something occurs, it is important to study the general models of open and dynamic systems. Open systems account for exchange and relation given the particular parts in the whole environment. This implies open ended and intransitive interactions - that is, provided that A, B, C stand for the parts of the system, and A > B stand for a relation, it might be the case that A > B and B > C while at the same time C > A. In general, an intransitive and open system can also suggest the relationship between two parts as equivalent to, or indeterminate (see below). Each of these relationship or qualities of "relating to" suggest an asymmetric relation. An asymmetric relation implies that A's relation to B is not the same as B's relation to A. A greater than sign implies a relationship of dominance. Therefore, A > B is equivalent to "A dominates B" or "A has dominance over B". Consequently, a "less than" sign (<) implies an asymmetric relationship of the dominated, an "equal" signs (=) implies an equivalent power relation, and indeterminate implies an unknown relationship (Valsiner, 2000).

Model 1	Model 2	Model 3
A > B	A > B	A > B
B > C	B > C	B > C

C > A C = A C--[?]--A

Each of these models display different parts within a system and their relationship to some other part. The parts and their interactions with other parts as a whole is a system. But how can we conclude causality from such a system of interacting and exchanging parts? Two systems, with the same parts - A, B, C - but different relationship of the parts will yield different results. In each of the models the specific relation between the parts is of key relevance - if a relationship is taken out, the system collapses, and the result does not occur. Since the result is dependent on the interaction of *all* parts in the system, and not of the individual variables, we can conclude that there exists a causal relationship between the system and some outcome. *Systemic causality* fits the notion of causes of the qualitative whole and is necessary in constructing the generalization of conditions under which something occurs.

From causality to catalysis: Chemistry and biology have accepted the open and dynamic nature of systemic phenomena. Catalysis is the study the conditions that operate within open, intransitive, and dynamic systems that enable a particular outcome to be produced - *while preserving the functioning of the producing system*. Such model can provide sufficient foundations for generalization while still keeping the quality of the whole intact. This generalization can be reached through a process model that allows for the dialogical exchange between the particular and the universal.

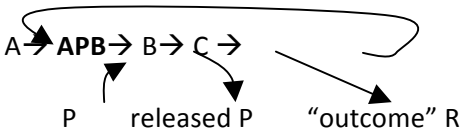
Catalysts offer an alternative conceptual tool to investigate systemic causality (Valsiner, 2007). We can see the basic notions of catalysts in everyday life. For example, individuals walk down the crowded streets of Boston, Massachusetts on a daily basis. As a whole, the streets in Boston may be busy - a conglomerate of multiple people going various places. But, it is not everyday when the street is busy that the crowd becomes considered a parade, a rally/protest, or an anarchist mob. For example, it takes a declared holiday to transform the normal street environment and crowd into a parade. The elements - people, street vendors, police etc - co-exist and co-occur in everyday life. However, it is the creation and systemic implementation of the holiday that catalyzes and organizes these elements into the phenomenon of a “parade”. Likewise, it takes the specific catalytic conditions of a controversial socio-political event to modify the crowded street into a protest or rally. Thus the notion of catalysis can be employed to understand how repeated *and* unique events occur without reducing the complexity of life to more elemental parts.

The Psychological Catalyst: In many systems, there are constrains or barriers that inhibit the occurrence of certain phenomena. It takes the help of a *catalyst* to lower the activation barrier - the barrier or constraint inhibiting a phenomena’s occurrence - in order to achieve the emergence of the phenomena. However, because the phenomena is a result of interacting and exchanging parts within a system, the catalyst has an important function of changing the relationships and interactions between one or more parts within the system. Similar to our open, dynamic, and intransitive system models, the catalyst changes the relationships of parts within the system to regulating, maintaining, ambiguous, or other relationships. As we stated above, system causality implies the change of one relationship within the system will yield a different result. Therefore, the catalytic overcoming of a barrier, resulting in the changing of a relationship not usually changed, alters the system as a whole, causing some novel (sometimes rare) phenomena.

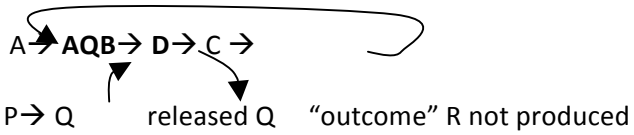
Assume a system contains parts A, B, and C where A--[?]--B, B > C and C > A. Obviously the open locus in this system is A--[?]--B—where [?] can take different forms (e.g. let these be <

and >). If a condition P is present, the [?] takes the form of > and the system operates as a dynamic intransitive cycle (A>B>C>A… etc). The condition P is a catalyst—a condition necessary that is in itself not part of the causal system A-B-C—operates as the maintainer of the dynamic steady state of A-B-C.

How does the catalyst work? It may *temporarily bind with* the process of transition in the system, so the process observed in time may be



In the processes of transition there is an intermediate form - APB - that facilitates the transition in its stable form, and enables the production of the "outcome" (consequent)- R. Now, consider the scenario where the catalyst P is not available and is replaced by another one (Q). The system is now challenged and can change its organization:

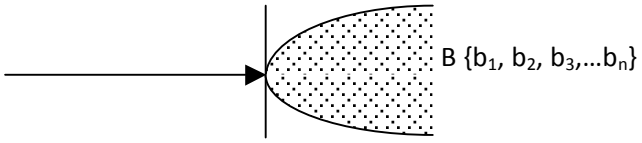


The new system ADC entails a loss of C and emergence of D as its part. Such qualitative transformation within the causal system itself is an example of development - brought by through the alteration of the conditions of functioning of the system. By way of changing itself the system ends up producing an outcome (R) - yet it keeps replicating itself in its "mutated" (B replaced by D) form. The causal system may return to producing R if the catalyst P replaces Q and if C can be replaced by B as a result. If not, the system has developed into a new irreversible form - triggered by the change of the catalysts.

The generalized feature of the causal system is its abstract quality—all causal processes are based on cyclical systems that reproduce themselves and may be triggered to modify themselves. All "outcomes" of these catalyzed causal systems are by-products of such self-preservation of the system. It is the catalysts—not "causal agents" that would produce "direct effects"—that lead to outcomes as always *indirect effects* of the causal system.

THE TRANSITION STATE IN HUMAN LIVES: MAKING OF THE FUTURE

Past, present, and future forms bring in the notion of becoming, emergence and development over time. The Trajectory Equifinality Model (TEM - Sato et al, 2007; 2009) explains the temporal relationship between a unilinear actualized past, an infinitesimal present, and the multiple potential trajectories of the future. Combining a catalytic model with the Trajectory Equifinality Model (TEM) gives the following: A person (A) has a unilinear actualized past and multiple future possibilities (Figure1):



past present future

Figure 1. The unilinear actual past facing the indeterminate future

The specific present conditions/context act as a catalyst (C) that help in the actualization of one possibility (b_1) within the set of possibilities B {b_1, b_2, b_3,…b_n} through the alteration of a specific possibilities (b_2) relation and interaction with the rest of the possibilities within the set/system (Figure 2):

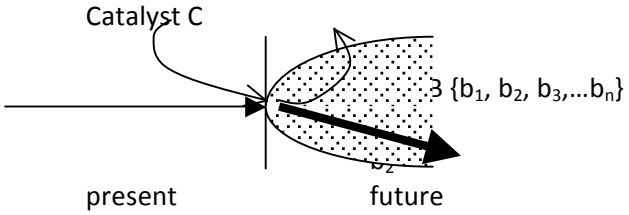


Figure 2. Catalytic movement towards determinate future

The catalyst lowers the "activation barrier" - the barrier inhibiting (or blocking) future potential from actualizing - for the binding agent. Human psyche operates through the enablement of future possibilities - allowing some of them to be constructed into actualities. Along the TEM model, intermediate catalytic development occur in the face of the future, along a series of possible future options within a set B {b_1, b_2, b_3,…b_n}. The catalyst accelerates actualization of one of these possibilities by acquiring a regulatory role. This regulatory role changes the relationship of one of the parts with the others in the system, yielding a different result. For example, the catalyst (C) for the given person (A) may change the function of option b_2 - bifurcating the trajectories so that anything other than option b_2 is blocked and inhibited, while option b_1 is promoted as an alternative – oppositional - course (Figure 3):

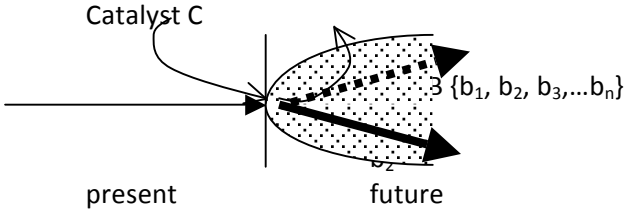


Figure 3. Differentiation of the trajectories for dialogue with the future

The options for the future are now limited to the {b_1, b_2} contrast, while the rest of the field of possibilities vanishes (Valsiner, 2009). The actual life course becomes negotiated by the future contrast {b_1, b_2} in its relation to recollected past contrast {a_1, a_2} - a bifurcation process that is reconstructed in the present based on the memories of the past.

An example. Processes of cultural catalytic kind can be observed in an example of cultivating and modifying the environment - what we call *graffiti*. In English, that term has undertones of illegality, rule-breaking, vandalism, and courage. This is not the case in other languages - Italian

for instance - where it designates graphic designs. Individuals have multiple ways of communicating messages (Figure 4). Under ordinary circumstances, the act of drawing on walls or filing cabinets is automatically blocked by our cultural system of catalysts. When confronted with the affordances of an empty wall and of pens in our hands, we nevertheless do not rush to use these affordances. The situation is very different if we are operating under the catalytic conditions of "protest" - here the usual regulatory mechanisms are overridden by the generalized feelings that turn the person into a creator of destructive acts. An individual may have a present message to be communicated, and they have an infinite number of ways to communicate this message. However, the minute the individual sets foot within a social setting, there is a regulation on what can be said and how messages can be communicated. Under no ordinary circumstances would an outside visitor enter an office and start writing messages on spaces not meant for those. Signs located in the activity setting (e.g., "No Talking") and generalized norms (e.g., "one doesn't talk about politics at the dinner table") seek to restrict the range of possible messages. Furthermore, a person carries around a luggage of such cultural regulators within one's own personal culture. Therefore, certain options of communication means are blocked by the catalytic condition of the social setting that is set up by various social institutions. These options begin to vanish away until they become extinct - or can be re-activated under conditions of change in the social atmosphere.

Figure 4. Ubiquitous takeover of (previous) blank wall on city street with 'graffiti'

When a second set of catalytic conditions is present - the fear of condemnation and judgment (social punishment - the only options left are those of anonymity. This can se observed most often when one person writes what they truly feel by the means of graffiti. Observing graffiti then becomes a catalyst for others looking for freedom of expression to do the same - responding and adding to the original graffiti to the point that a graffiti dialogue may occur. A demonstration of disorder operates as an indexical sign - in C. S. Peirce's terms - for allowing further construction of disorder. Yet when the general mode of operation for the self is constructive a sign of disorder calls for its elimination ("cleaning up"). A piece of rubbish next to a garbage bin calls for *putting it in* that container - while the same sign for a person on a mission of destruction suggests *pulling all the contents of that container out* to join the piece of rubbish.

Emotional-Semiotic Mediators as Catalysts: The regulatory function of signs can be seen through emotional-semiotic markers that re-configure the person-environment relationship. Josephs (2003) discusses emotional-semiotic and illustrates how catalysts synthesize parts where "direct and unmediated synthesis is not possible" without the catalyst (p. xii) through the following example [note: this example was initially used by Fritz Heider (1958)].

> If I generally hate warts on the face, yet the man I fall in love with happens to have a permanent, hairy (and thus particularly disgusting) exemplar right on his chin, unavoidably tension is built up. Yet soon, if not immediately, I may find myself in a position to like, even tenderly touch this originally disgusting body mark … There is neither mystery nor pathology in this process … It is the powerful emotional-semiotic catalyst (my felt notion of love, and the value I attach [and I learned *should* be attached] to my partner) which radically transforms my relation to the world (here to a drastic exemplar of a wart) through an immediate overgeneralization of this value orientation …. What happened is an emotionally triggered transformation of my meaning-system due to a strong and felt value orientation - a higher level regulator (the catalyst) which leads to a new, generalized quality of the meaning-system. (Heider 1958, p. xiii)

In this example, the feeling and value of love, in this example, serves as the catalyst in reorienting the individual's meaning-system and person-world relation. The feeling of love generalizes from something desirable - the loved one - to encompass what was previously disgusting. This emotional-catalyst acts as a higher order regulator and, as affective catalysts, function rapidly and beyond rational and step by step effort (Josephs, 2003).

Especially for our purposes concerning generalization, this brings us to two important points. The first concerns the holistic nature of catalysis and emphasis on the situation. This example includes a shift in meaning-system and value orientation based on the relationship of the particular person to the particular lover. Additionally, as Josephs' parenthetically notes, the social suggestion that one should value one's partner was embedded in the social context in which the person was interrelated. In order to understand the phenomena (i.e., meaning-change), we have to investigate the whole and not isolated parts. Second, catalysts are about (often) unique events and processes, and not averages. It would be pointless from this perspective to see if this particular shift (transformation of disgust to value and appreciation) occurs frequently and/or by many people. The matter at hand is the general process (emotional-semiotic catalysis).

Generalizing the Catalytic Whole: Since phenomena are not caused, but rather, are catalyzed, we must develop a concept of generalization that encompasses the general system in which something occurs (or results). The catalytic systems model not only incorporates the past, present, and future, of a phenomenon's development, but also incorporates the qualitative whole of the system. The construction of the phenomena within this framework forces the observer to view the whole of the system, and not to forget or selective ignore those variables that, if forgotten or ignored, could result in a different phenomena, or no phenomena at all. This presents a model for experimental procedures that *must* consider person and environment as a qualitatively organizing whole.

From the Particular to the Universal: Dialogical Generalization. Lewin presented the dialogue between the particular and the universal. Generalization is the ability to abstract from the concrete, producing general representative features that can be applied to similar phenomena within a "fuzzy category". Therefore, generalization is a dialogue between the concrete and the abstract or the particular and the universal. The catalytic model allows for understanding system causality in a qualitative whole, as well as the dialogue between the concrete and the

abstract. The catalytic model is abstracted from specific concrete phenomena - not of high frequency, but rather individual phenomena of rare occurrences - that are then applied (or generalized) to other rarely occurring phenomena. The generalization is not for the understanding of the likelihood, probability, or average frequency of the rare phenomena, but rather, to understand the systemic processes that promotes or inhibits the rare occurrences of the phenomena. School shootings do not happen everyday. It is hard to study the single variables, one by one, that "cause" that individual to shoot fellow students at the school. In fact, people and guns are present in our world with high frequency, but their synthesis and modification into school shooting rarely occurs. The experimental model of one-to-one causality is not sufficient in explanation. Rather, the catalytic model - showing the dynamic interaction of individuals, conditions, contexts, and catalytic agents - can aid in the understanding of things that do not normally occur without the presentation of a catalytic condition or agent.

CONCLUSION: GENERALIZATION TAKES PLACE BASED ON PARTICULAR WHOLES

The world exists in particulars - and any generalization from those is based on single unique encounters with the world. Psychology's reliance on the multiple examples (samples) to generalize to an abstract sample (called "population") is an impoverished proxy for the single case to generic case generalization. Lewin's epistemological stance was clear and productive - proven by any practitioner's inevitable reliance on the direct single encounters with the world.

Psychology seems to have confused the notion of *abstraction* and *democratic majority dominance* in its past century. The latter is what the trajectory of inductive generalization entails - the majority of the individual cases in a population is - poorly but representatively - captured by the reliance on the averages. Sure, an average is an abstraction as well - otherwise statements like "the children in the study were, on the average, in the 5.67 grade age level" would be immediately perceived as meaningless.

All generalization involves abstraction. In the case of generalization from a single case,

> to generalize is to recognize likeness which had been previously masked by differences; *to recognize the likeness is also therefore to recognize these differences as irrelevant*, and to *disregard them* from the point of view of the general conception (Baldwin 1901, p. 408, added emphasis)

It becomes clear that our cognitive facility to discount immediately observable differences and replace them with an opposite focus - that of similarity - is based on our making of Gestalts in our meaning system. Such Gestalts come at different hierarchical levels - the higher levels entail generalization of the whole fields of experience. Thus, the perceptual detection of a "cloudless sky" and its color ("blue sky") can lead to aesthetic generalization ("beautiful sky") with hyper-generalization ("how beautiful!").

Thus, generalization takes place by two routes: (i) the analytic-to-synthetic (recognition of differences and turning that recognition into recognition of likeness); and the (ii) holistic move to higher order Gestalts (within which there are no differences—the cloudless sky, or on overwhelming feeling of happiness, have no distinct parts to compare). In both cases the concrete details are substituted by signs. Generalization is possible through human preponderance for semiosis.

Social Reality is dynamic. That is too say, our world is constantly in flux and phenomena are constantly emerging (and in the state of becoming). When looking at science – *Wissenschaft* -

the epistemological treatment of generalization should develop out of the constraints formed by the post-modern push, that is, the very particular, relational and situated conditions of phenomena have seemingly precluded the establishment of general laws. However, other epistemological treatment of generalization and general laws need not be of a homogenous and classificatory nature (Aristotelian notion of principle of commonality), fundamental for the inductive generalization of frequent numbers (Aristotelian foundation of the objects "tendency" based on frequent recurrences) in a sample to a target population. Post-Galilean thought not only denied the epistemological treatment of generalization through "frequency", but also changed the locus of causation away from the *intrinsic properties of* the object to the structural relationships *between* objects. It was Lewin's development field theory that allows the particular and the general to engage in dialogue within the open and dynamic "field" or system. The development of systemic causality from Lewin's field theory suggests that generalization should not be based on separate and independent variables, but rather, generalization should take place out of a catalytic model and the conditions under which something occurs. This conditional analysis provides fruitful grounds of not only the rare and frequent phenomena, but understanding the particular, the qualitative whole, and the relationships within a general framework.

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The Issue of the Unity and Specificity of Psychology from the Viewpoint of a Constructivist Epistemology

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ABSTRACT

A certain constructivist psychology converges with a certain epistemology of psychology in rejecting as meaningless the project of the unification of the various psychological schools and theories, as well as the trend to the progressive reduction of psychological phenomena to neurophysiological processes. I shall discuss the subject by referring to the work of the Italian epistemologists E. Agazzi and S. Marhaba, the American psychologist G. A. Kelly, and the Chilean biologist H. Maturana.

1. INTRODUCTION

I imagine that it could be very reassuring to study a discipline like physics, mathematics, biology, chemistry, and move on a ground that considers only the possibility of choosing which field of inquiry to dedicate oneself: mathematical analysis rather than mathematical logic, quantum mechanics rather than astrophysics, biodiversity rather than molecular biology, chemistry of materials rather than organic chemistry, not to mention the possibility of dedicating oneself to fields of inquiry deriving from intersections of the above disciplines: mathematical physics, biochemistry, physical chemistry, and so forth. A graduate in one of these disciplines can easily discuss and confront with graduates of other cognate disciplines (those envisaged not casually in the one faculty of mathematical, physical and natural sciences), due to the possibility of making reference to a well-established body of knowledge and a shared experimental method. Things seem to go in a very different way for someone interested in psychology. Even though, in order to facilitate the course of learning by fragmenting it, the subjects of the graduate program in psychology appear divided into specific topics (general psychology, developmental psychology, social psychology, psychology of personality, clinical psychology, and so on) in the same way as it happens in mathematical, physical and natural sciences, the student in psychology soon finds out that the same subject is taught in a different (sometimes in a very different) manner in other faculties, or even, in the same faculty, by different teachers. The panorama of psychology appears even more variegated if one considers it in its relation either with other "psy-disciplines" – some of them of medical matrix (like psychiatry and psychopharmacology), others deriving from disciplinary intersections (like psychophysiology, or psychosomatics) – or with the increasing number of "neuro-disciplines" (from neuropsychology to neuroeconomy and neurotheology!), which are filiations of the more and more prolific and popular cognitive science[1].

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[1] See the recent *Neuro-mania* by Legrenzi and Umiltà (2009).

All this is well known, and someone tried to explain the reason of such difference. Marhaba, in *Antinomie epistemologiche nella psicologia contemporanea* [Epistemological antinomies in contemporary psychology] (1976) made it in a way I see as particularly effective since over thirty years:

> While the physicist deals with theories antagonist each other, but all within the same system of reference, the psychologist has to choose between *different and opposite* systems of reference. In other words, the epistemological tissue of psychology is covered with *lacerations*, as opposed to the substantially unitary tissue of the traditional natural sciences. (Marhaba 1996, p. 29)[2]

The result is a manifest disunity of psychology or, we could even say, the emergence and development of various psychologies, characterized by numerous and various theoretical assumptions that, in turn, could refer to various epistemological choices (which Marhaba systematizes in terms of nine antinomies, each of them defining two radically different solutions), choices of which the psychologists are more or less aware.

Now, if we accept that the difference between psychology and traditional natural sciences consists in the presence/absence of an unambiguous system of reference, we could ask (a) where does this difference derive from, (b) if it is possible to eliminate it, and how, and (c) if it is to be hoped, and even necessary, to do so, or if, on the contrary, the antinomic articulation of the epistemology of psychology represents a richness rather than a limit of it. Periodically there is someone who struggles to suggest how to eliminate this difference between psychology and the traditional natural sciences, by considering this elimination both possible and necessary for the development of psychology as an actually scientific discipline. In my opinion, the possible success of such a project would be equivalent to a sort of "epistemocide", which anyhow has no chance to materialize. I shall defend my position by starting from a specific approach of contemporary psychology, generically known as constructivism, which, due to its peculiarities (or at least to the peculiarities of some of its expressions, in the absence, as we shall see below, of an unambiguous definition), appears respectful of epistemic and theoretical differences.

2. THE ATTRACTION FOR THE NATURALIZATION OF PSYCHOLOGY

The first question, relative to the difference between psychology and the traditional natural sciences, refers to the attempt to define what the system of reference of these latter consists in, and what its (at least seeming) univocity and success derive from.

At a first analysis - certainly slightly superficial compared to the complexity of the epistemological reflection - one could assert that the system of reference of natural sciences consists in the assumption of the existence of a given reality, independent from the observer, and in the belief that the application of the scientific method allows the accumulation of fragments of knowledge of such a reality. Such an assumption actually appears so widely shared by the community of scientists to be considered beyond dispute. However, the clear discrepancy between the scientific praxis and the philosophical reflections, have for long led contemporary epistemologists to criticize the idea that it can be defined a scientific method (Kuhn, 1962), or that the development of science has been allowed from its actual application (Feyerabend, 1976).

[2] Throughout the article, if not otherwise specified, all the translation of the Italian quotations into English is mine.

In the attempt to establish itself as a science, psychological knowledge – already existing as the "doctrine of the soul" in the sphere of philosophical speculation – has striven to assume the research methods of the sciences par excellence – indeed, a difficult attempt when the object of study is not clearly, and, therefore, unanimously identifiable. For example, when the object of study was represented by the immediate experiences of consciousness, as for the structuralists of the Leipzig's laboratory, the method of choice was "introspection", which allows the empirical observation of the contents of individual consciousness in place of the "inspection" addressed to the contents of the external world (Marhaba, 1980). Moreover, when the object of the psychological investigation was represented by mental functions meant as adaptive behaviors - as in functionalist psychology - the method consisted in the subjectivist observation. Such a method was replaced by the objectivist or behavioral observation with the success of behaviorism, whose object of study is the observable behavior. And one could go on mentioning the objects of study of gestalt psychology, cognitive psychology, psychoanalysis, etc., and the corresponding methods regarded as more suitable to the "scientific" study of such objects.

What may appear peculiar and may contribute to the perception of a difference between psychology and the natural sciences is that all the above "schools", as well as their derivations, survive and carry out their research programs rather than progressively substituting each other according to a logic of development for psychological knowledge. This phenomenon represents itself within the same schools. Therefore, for instance, within what is defined on the whole as "cognitive psychology", the developmental theories of Piaget, Vygotskij and Bowlby live together and thrive without the happening of substitutions deriving from the proven groundlessness or the abandoning of the rival theories. Their success seems to be more tied to questions of socio-cultural (nationalistic, academic, affiliative) or of personal order («I feel this theory closer to my point of view», a criterion that would horrify physicists, or that, whenever held, would certainly not be disclosed by one of them).

Consequently to such a view, it appears both inappropriate to talk of *one* psychology, and unjustifiable to search for a solution to what seems to some psychologists as a hindrance for a discipline which aspires to define itself scientific. Such research can lead to two paths, that is, either (i) the *unification* of the various schools and theories by using a single metatheoretical framework able to incorporate the main theoretical perspectives into a coherent whole, or (ii) the *extinction* of psychology as a consequence of the progressive reduction of psychological phenomena to neurophysiological processes (and the resulting recovery of one frame of reference). The first solution has been recently reproposed by Henriques (2003) in the form of a *Tree of Knowledge System*, giving birth to a debate in two special issues of the *Journal of Clinical Psychology* (Henriques & Cobb, 2004; Henriques, 2005). On the other hand, the reductionist temptation, which has always gone along with the history of psychology, is enjoying a particular revival, due to the development of neurosciences and, in particular, to the use of neuroimaging technology. I shall criticize both projects from the point of view of constructivist psychology, and show how the latter converges with a certain epistemology of psychology.

3. THE CONSTRUCTIVIST EPISTEMOLOGY

The spreading of constructivist perspectives in psychology represents a phenomenon that would deserve to be analyzed by the sociology of psychology. The distinctive features of those epistemological assumptions, nowadays easily identified and grouped under the label of "psychological constructivism", have been recognised *a posteriori* as already present both in

the work of Jean Piaget on cognitive development[3], and in a ponderous two-volumes book on personality and psychotherapy, *The Psychology of Personal Constructs* (1955) by George A. Kelly, which went unnoticed in times dominated by psychoanalysis and behaviorism. It is only at the beginning of the Nineteen Eighties that the adjective "constructivist" progressively begins to be used in psychology, both in Italy and the Anglo-Saxon countries, to incur later such a widespread use of the term leading to its inflation. It appears that the main catalysts of such phenomenon have been Ernst von Glasersfeld, a psychologist with an eclectic training and excellent capacities of popularization, and two Chilean biologists, Humberto Maturana and Francisco Varela. Von Glasersfeld had the merit of providing a convincing interpretation of Piaget's theory in terms of a "radical constructivist epistemology" (1980), distinguishing it from the "trivial constructivism" of so many cognitive psychologists endorsing metaphysical realism (von Glasersfeld, 1984). Maturana (1978), subsequently in collaboration with Varela, proponed a biological theory of knowledge that refers to the "ontology of the observer". Their "theory of autopoiesis" (1980, 1987), though complex and not always adequately understood, has had an unexpected success in psychology, particularly in psychotherapy and family therapy - maybe due to its systemic formulation. The brilliant analyses of a cybernetic, Heinz von Foerster (1981), the "revival" of one of the founders of the pragmatics of human communication, Paul Watzlawick (1984), and of its inspirer, Gregory Bateson (1972, 1979), together with the rediscovery of Kelly's (1955/1991) theory and psychotherapy of personal constructs (Butt, 2008), contributed to the spreading and popularity of constructivist epistemology, as well as the affinities of constructivism with the movement of social constructionism (Gergen, 1985), and the narrative turn both in psychology (Bruner, 1986, 1990) and psychotherapy (Angus & McLeod, 2004).

In turn, the spreading of constructivist epistemology has produced particularly wide effects in the field of psychotherapy, causing the emergence of new perspectives of treatment, and constructivist interpretations and developments of already well-established psychotherapeutic approaches[4]. But how can psychological constructivism be defined?

The task is not easy, given that many psychologists define themselves as constructivists simply for their belief that personal knowledge requires an active participation by the individual (see for instance Mahoney, 1988). However, this definition is clearly too generic to be applied to a great part of the psychological schools. On the contrary, von Glasersfeld regards as discriminatory the giving up of metaphysical realism in favour of a view of knowledge as viability rather than representation (like in cognitivists). That is, one's knowledge of reality is among the possible compatible with the environmental constraints: all of them are legitimate, and none of them allows to know reality as it is.

The distinction between radical and trivial constructivism is not the only proposed in order to try to define more clearly an epistemology which would otherwise risk no longer having a specific meaning[5]. In a previous article, *Psychological constructivisms: A metatheoretical differentiation* (Chiari & Nuzzo, 1996), we suggested that constructivism is essentially an attempt to transcend the realistic and idealistic views of knowledge (which see it, respectively, as a *reflection* or a *representation* of a given reality, or as an *invention* without any foundation), pointing out a "third way" consisting in the metatheoretical assumption that «the structure and organization of the known — the knower-as-known included — is inextricably linked to the structure of the knower» (ibid., p. 78). In terms of the relationship between

[3] One has only to mention, among his numerous writings, *La Construction du Réel chez l'Enfant*, 1937.
[4] As I documented in my recent *Constructivist Psychotherapy: A Narrative Hermeneutic Approach*, 2009.
[5] For a description of the most important distinctions, see Chiari and Nuzzo, 2009, pp. 44-54.

knowledge and reality, this connection can take the shape of an ordering and organization of a world constituted by the person's experience (a stance we defined as *epistemological constructivism*), or the sense of a reciprocal specification between knower and known, resulting in the overcoming of the subject/object dichotomy (*hermeneutic constructivism*).

Psychologists and psychotherapists definable in our terms as epistemological constructivists adhere to an ontological realism since they acknowledge the existence of a real world that nevertheless, from a gnoseological standpoint, they believe possible to know only by means of *personal constructs*, that is, heuristic narratives useful to its understanding. Kelly's personal construct theory can be (and is) more commonly interpreted in such terms, such as Piaget's theory according to von Glasersfeld's interpretation: therefore, both Kelly and von Glasersfeld postulate the existence of two realities, the extra-linguistic and the experiential, thus sharing a subject-object dualism.

The alternative to such opposition/separation between subject and object derives from considering such "entities" as, in turn, the product of a personal construction rooted in a background of biological, social and cultural practices. Persons are enmeshed in a world they cannot observe and describe from the outside: persons are *in* the world, and their knowledge can only be a specification, «an interpretation historically founded rather than timeless, contextually verifiable rather than universally valid, and linguistically generated and socially negotiated rather than cognitively and individually produced» (ibid., p. 174). The different disciplinary approaches springing up in the last twenty years, widely interconnected and to a great extent expressions of the movement of social constructionism, all refer to the adhesion to a hermeneutic constructivist epistemology: narrative psychology (Sarbin, 1986), cultural psychology (Bruner, 1986, 1990), discursive psychology (Edwards & Potter, 1992; Harré & Gillett, 1994), postmodern psychology (Kvale, 1992). Though not directly psychological (but with ample psychological implications), the theory of autopoiesis, with the ontology of the observer that characterizes it (Maturana, 1988), can be duly included in the list.

Even though the constructivist perspective can be regarded as the avant-garde of contemporary psychology, its affinities clearly appear with the numerous *-isms* that philosophical reflection has proposed over the centuries in the attempt to question the possibility to come to absolute truths: among them, skepticism, relativism, nominalism, perspectivism, till the more recent pragmatism. Furthermore, the affinities between the constructivist perspective (at least that one we defined as hermeneutic constructivism) and the ontological premises of phenomenology and hermeneutics have been pointed out[6] – in particular, the overcoming of the objectivist and subjectivist positions through the consideration of the interdependence subject/object – so much that one could state that the constructivist movement represents the present attempt to recover Husserl's (1976) project of a re-foundation of science (in particular psychology) without foundations, that is, on the basis of lived experience of phenomena (Armezzani, 2002; Chiari & Nuzzo, 2000).

In the second section, I mentioned the discrepancy between scientific praxis and the epistemological reflections on it. In the case of psychology and the human sciences in general, this discrepancy has the effect of a polemic confrontation among "schools" as if the truth of their propositions could be verified on the basis of common criteria. Another effect equally unsustainable from an epistemological viewpoint consists in syncretism, that is, the acceptance of propositions deriving from different approaches as if they had a value independent from them and could therefore be added to enrich the body of psychological knowledge. The attempt to give unity to psychology derives from the shareable unsatisfaction for these two opposing attitudes, but goes along with an underestimation of the complex

[6] As we tried to document in Chiari and Nuzzo, 2009, pp. 29-34.

epistemological questions implied. On the contrary, such questions find a "natural" consideration within the approaches that see themselves in a constructivist view of knowledge - or, at least, in those referable to the above definition of hermeneutic constructivism. The consequence is a common rejection of the attempts to make psychology more "scientific" (by naturalizing it), both through the pursuit of unity and the project of a reductionist extinction, because, so to speak, "there is no case to answer": if any, the problem of the scientificity of psychology dwells elsewhere.

To show the substantial agreement about the issues of unity and reductionism between the epistemological reflection and the constructivist trend in psychology, I shall mainly refer to the work of two authors: the Italian epistemologist Evandro Agazzi (in particular *Criteri epistemologici fondamentali delle discipline psicologiche* [Basic epistemological criteria of the psychological disciplines], 1976), and the American psychologist George A. Kelly (1955/1991). Of course the choice is not casual, but brought forth by the particularly striking affinities between *a certain* epistemological reflection and *a certain* constructivist approach in psychology[7]. I shall also refer to some elements of Maturana's theory of autopoiesis and the ontology of the observer peculiar to it, since its level of abstraction is such as to produce considerations about the matters in hand, both of epistemological and psychological order[8].

4. THE CONSTRUCTIVIST PERSPECTIVE WITH REFERENCE TO THE UNITY OF PSYCHOLOGY

Kelly is one of those rare psychologists who regard philosophical reflection as essential for theorization and scientific research, so to make explicit the philosophical assumptions from which he chooses to start before beginning the exposition of his theoretical construction. Among these assumptions, a prominent place is occupied by *constructive alternativism*. Kelly proposes an alternative to that *accumulative fragmentalism*, predominant in science, which consists in believing that knowledge derives from the accumulation of fragments of truths about the world, gathered through the observation of facts and the generation by induction of laws and theories. On the contrary, Kelly writes, «we assume that all of our present interpretations of the universe are subject to revision or replacement» (1955, p. 15).[9]

For Kelly, then, knowledge is a personal interpretation, deriving from the separation into segments of the undifferentiated flux of events, on the basis of the construction of recurrent themes, or regularities. In order to "seize" regularity at least three elements are needed: two of them allow abstracting the aspects of similarity, while the third allow abstracting the aspects of difference. For this reason the *personal constructs* are conceptualized as bipolar, and included in a *construction system*. In this context, what is of interest to us[10] is the

[7] I imagine however, as an outsider, that not all the epistemologists of psychology share Agazzi's theses, and aware, through direct experience, that not all the psychologists defining themselves as constructivists share the assumptions and implications of Kelly's personal construct theory.

[8] On the affinities between personal construct theory and the theory of autopoiesis see Chiari and Nuzzo, 2009, *passim*.

[9] Who is not familiar with psychology, but is maybe acquainted with the epistemological reflections from Popper onwards, could think that this statement is outdated by now (after all, it dates back to over fifty years ago) and that its innovative character has gone lost for long. On the contrary in human sciences (psychology included) the abandonment of an Enlightenment or positivistic view of knowledge is a process still in progress, so that the approaches, indicated as "postmodernist", that criticize the possibility of "ultimate" knowledge of reality by underlining its social and cultural matrix are still placed on the fringe of academic psychology.

[10] For the in depth study of the theory I refer to the texts above quoted.

implication as to the relationship between knowledge and reality: namely, the way by which, through their constructs, persons segment their experience, thus "cutting out" the "objects" that compose their personal experience by giving them properties (meanings), and assigning the relationships with other "objects" (corresponding to the placements of the constructs within the hierarchically ordered system which they are part of). Therefore, personal knowledge is meant as a theoretical system whose hypotheses – i.e., what the single constructs allow to anticipate in the course of events - are continuously verified by means of behavior, with the function of an experiment, thus giving shape to a circular relation between knowledge and reality, similar to that recognizable, not casually, in Piaget. It is the process that Kelly illustrates by recurring to the analogy of the "person-as-scientist": a scientist, whether striving to give structure and meaning to the personal world around him or her, or applying him or herself to the study of particular sets of objects (those traditionally belonging to physics, biology, psychology, or anything you like).

The person-as-scientist described by Kelly clearly refers to the figure of scientist sketched by a certain epistemology, and particularly, as preannounced, by the epistemological reflection of Agazzi (1976). Each science, meant as established heritage of knowledge, presents itself as a *language* talking about a "universe of objects". The "objects" of a science are not to be mistaken for "things": a single thing can become object of different sciences depending on the "point of view" from which one chooses to consider it, «in the sense that it is the assumption of a certain point of view on 'things' rather than another, to place ourselves within this instead of this other science» (p. 11). An example will make clear the concept:

> Consider, for instance, a watch: If we ask ourselves how much it weighs, or what are the laws regulating the motion of its balance, we make it an "object" of physics. If we ask ourselves what is the composition of its case, or the degree of pureness of its rubies, we consider it an "object" of chemistry. If we ask ourselves what is its exchange value compared with other goods, we conceive it as an "object" of economics. If we ask ourselves what is the relation between that particular model of watch and the personality of the purchaser who chose to buy it, we make it in some way "object" of psychology. And the list could go on for long. (Agazzi 1976, p. 11)

A same "thing", therefore, is a "bundle of objects" potentially infinite, since the points of view from which one can choose to consider it are endlessly multipliable.

Kelly shows to have a similar view of science in the very definition of the "Fundamental Postulate" of his theory: «a person's processes are psychologically channelized by the ways in which he anticipates events» (1955, p. 46). In the scientific reasoning, Kelly goes on, the postulate is an assumption, a proposition, which is true as long as it is not questioned; it is as though we would say, «let us suppose, for the sake of the discussion which is to follow, that a person's processes are channelized by the ways in which he anticipates events» (Kelly 1955, p. 47), and let us see what would ensue. But what we are interested in underlining in this phase of the discussion is the use of the adverb «psychologically». Why does not Kelly use the adjective «psychological» when referring to the person's processes? The answer is that, consistently with the philosophical assumption of "constructive alternativism", Kelly does not regard the substance of psychology as psychological, or physiological, or sociological in itself:

> A person's processes are what they are; and psychology, physiology, or what have you, are simply systems concocted for trying to anticipate them. Thus, when we use the term *psychologically*, we mean that we are conceptualizing processes in a psychological manner, not that the processes are psychological rather than something else (Kelly 1955, p. 48)

If his theoretical system is psychological, it is only because he considers it similar to other systems having a similar domain: because, we could say in the epistemological language of Agazzi, its "universe of objects" is similar (even though not exactly alike) to that of other psychologies which regard certain "things" from a certain "point of view". The following quote by Bannister, one of the most important representatives of the psychology of personal constructs, is particularly illustrative of the consistency between Kelly's constructive alternativism and Agazzi's epistemological position:

> If we contemplate a young lady crossing a bridge (a lay construction) then we may equally construe her as a 'series of moments of force about a point' (engineer's construing), as 'a poor credit risk' (banker's construing), as 'a mass of whirling electrons about nuclei' (physicist's construing), as 'a soul in peril of mortal sin' (theological construing) or as 'a likely dish' (young man's construing). We do not have to assume that she is *really* any of these. We can accept that they are all constructions which have some explanatory value and predictive utility, depending on the networks of constructs from which they stem. (Bannister 1968, p. 229)

Maturana's ontology of the observer leads to similar conclusions on scientific disciplines meant as domains of experiences:

> All descriptions constitute configurations of co-ordinations of actions in some dimensions of the domains of experiences of the members of a community of observers […] Physics, biology, mathematics, philosophy, cooking, politics etc., are all different domains of languaging, and as such are all different domains of recursive consensual co-ordinations of consensual actions in the praxis of happening of living of the members of a community of observers. In other words, it is only as different domains of languaging that physics, biology, philosophy, cooking, politics, or any cognitive domain exists. Yet, this does not mean that all cognitive domains are the same, it only means that different cognitive domains exist only as they are brought forth in language, and that languaging constitues them. (Maturana 1987, p. 372)

Let us go back now, on the basis of what above said, to the main issue of this section about the possibility/advisability of a project aimed at giving unity to *the* psychology. If we accept that every scientific discipline cuts out its objects by looking at things from a certain point of view and investigating them according to certain methods, we must accept the existence not of *one*, but *many* psychologies. As a consequence, in the words of Agazzi,

> The behaviorist can no longer blame who uses the instrument of introspection for a defect of methodology […], a supposed incorrectness or lack of scientific criticism: this would be acceptable if the problem were that of confronting *the same* object and of wanting to deal with it by means of methods so diametrically opposite. On the contrary, the question is another: the use of the two different methods actually "cuts out" two different kinds of objects, therefore opting for this rather than that methodological choice simply means choosing to deal with something more or less different, or, if you like, practicing another psychology. (Agazzi 1976, p. 16)

At this point someone might ask if one or more psychologies are better than others (perhaps in the sense of "more true"), or if one has to come to an epistemological Dadaism à la Feyerabend (1976), against *the* method and in favour of inventiveness and creativity. The question on the "absolute" truth of a single proposition or a theory derives from mistaking "things" for "objects". Again, should we admit that every scientific discipline cuts out its objects, a proposition will be true or false "with regard to" those objects; and the various psychologies, dealing with objects at least partly different, could be simultaneously true. «The conflict of methods, therefore, is only apparent when one understands that it expresses itself

in a differentiation of objects and is not a brawl about the better way to take possession of an only and same object» (Agazzi 1976, p. 17, translation mine). It would be important, instead, that each psychology makes explicit its "protocol criteria", that is, that set of propositions acknowledged as "immediately true" on the basis of which to be able to decide about the truth or falsity of the propositions. These protocol criteria have a high level of explicitness in the case of physics, whereas the psychological disciplines operate at a more implicit level, due to the lack of a clear definition of the "praxis" in accordance with certain prearranged "operations"; praxis which, by itself, can establish the ground of intersubjectivity, that is, of objectivity meant as *independence from the subject*, as agreement within a community, not as *pertinence to the object*.

Agazzi uses the term *construct* to denote the object as an abstract entity, a «*bundle of relations* that are extracted from "things" by means of instrumental manipulations» (p. 26, italics in the original, translation mine). The affinities between Agazzi's epistemological reflections and Kelly's philosophical and theoretical position go beyond the use of the same term. Also for Kelly «constructs are not to be confounded with the factual material of which they are personalized versions; they are interpretations of those facts» (1955, p. 136), and nevetheless they are real, as, within a system of hierarchical relationships among constructs, the subordinate constructs represent a form of reality which is construed through the use of the superordinate constructs. It is important not to substantiate these "objects" by treating them as "things" or cognitive entities, as someone who did not understand the assumptions from which Kelly derives his theory might do. The introduction in science of predicates, constructs and theoretical entities (the electron or the atom in physics; the unconscious, the superego, the personality in psychology; the genetic code in biology; the affinity in chemistry, and so on) allows, according to Agazzi, to go beyond the propositions "immediately true" based on operational protocol criteria, and to make use of "theory".

> The importance is not to substantiate these entities, by conceiving them as "things" of common sense; but, to the extent that one realizes that they are constructs, any suspicion towards them has to fall because, after all, we saw that also the so-called "empirical objects" are constructs. (Agazzi 1976, pp. 27-28)

Again, the analogy with what sustained by Kelly is striking:

> One of the hazards of operationalism is its tendency to make researchers think concretistically. It encourages experimenters to see things rather than principles. Yet, it is not things that a scientist accumulates and catalogues; it is the principles or the abstractions that strike through the things with which he is concerned. […] The principle is not the aggregate of all the events; it is rather a property, so abstracted that it can be seen as pertinent to all of them. (Kelly 1955, p. 30)

5. THE CONSTRUCTIVIST PERSPECTIVE WITH REFERENCE TO THE REDUCTIONIST THESIS

The same considerations that make the project of a unification of psychology unfeasible (meaningless) can be used to maintain the unjustifiability of the reductionist thesis, which upholds the translatability of the propositions of a discipline in those of the disciplines more basic in a supposed hierarchical order, with the advantage of a more complete resolution of the problems. On the contrary, the anti-reductionist approach «demands that every scientific problem is discussed and solved exclusively in the terms in which it puts itself from the beginning» (Marhaba 1976, p. 53), in the terms of the interpretive constructs that cut out and give properties to the "objects" of any specific discipline.

The *reductionist belief* rests on what I shall term *psychophysical belief,* which consists in assuming the existence of a relationship between events to whom an intrinsecally different nature is recognized. In such cases a greater value of "reality" or "scientificity" is sometimes attributed to the event that, in the psychophysical relationship, has a more "basic" placement in the hierarchical order of scientific disciplines.

What the assumption of a constructivist perspective denies is not just the fact that such relationships "exist", but the ontological character of such an existence. Whatever exists, in fact, exists as element of a personal construct (Kelly), as a unity brought forth by an act of distinction (Maturana). Mind and body are not exceptions: mind and body are constructs, «matter, energy, ideas, notions, mind, spirit, god, ... are explanatory propositions of the praxis of living of the observer» (Maturana 1987, p. 376). How to explain then the numerous correlations – to which is often attributed a causal value – between certain physical (physiological) and mental (psychological) phenomena? How can we explain the relationship, that many of us have personally experienced, between an immoderate taking of alcohol and a sense of exhilaration? or the relationship, often reported in the psychosomatic literature, between a mother's overprotection and the development of bronchial asthma or peptic ulcer in the child? or the relationship, studied in the pharmacological research and used in psychiatry, between the effect of certain chemical substances on certain neurotransmitters, and the changes in the course of certain mental disorders? or the relationship between the activation of certain cerebral areas and certain mental processes? A detailed list of such relationships could occupy hundreds of pages.

In personal construct theory terms, such possibilities derive from the fact, widely described in the above section, that a "same" event can be construed through different construction systems:

> Are those facts "psychological facts" or are they "physiological facts"? Where do they really belong? Who gets possession of them, the psychologist or the physiologist? […] The answer is, of course, that the events upon which facts are based hold no institutional loyalties. They are in the public domain. The same event may be construed simultaneously and profitably within various disciplinary systems – physics, physiology, political science, or psychology.
> No one has yet proved himself wise enough to propound a universal system of constructs. (Kelly 1955, p. 10)

Therefore, the observation of psychophysical relationships derives, from a constructivist perspective, from the simultaneous use of protocol criteria defining different universes of objects: namely, those of physiology and psychology. As Maturana (1978) remarks from the viewpoint of the ontology of the observer,

> for the observer who beholds simultaneously both phenomenal domains [...] the changes in the relations of the components appear as changes in state in the living system that modify its properties and, hence, its interactions in its environment - all of which he or she describes by saying that the physiology of the organism generates its behavior. Yet, since these two phenomenal domains do not intersect, the relations that an observer may establish between the phenomena of one and the phenomena of the other do not constitute a phenomenal reduction, and the generative operational dependency of behavior on physiology that the observer asserts in this manner does not imply a necessary correspondence between them. Accordingly, in no particular case can the phenomena of one domain be deduced from the phenomena of the other prior to the observation of their actual generative dependency. (Maturana 1978, pp. 37-38)

In the essay quoted above, Bannister (1968) regards physiological psychology a sort of epistemological hybrid since he argues that from the personal construct perspective a question such as «*physiological events* and *psychological events* are related?» appears meaningless. It would rather be adequate to ask if the *constructs* being part of the theoretical systems of physiology and psychology can be usefully related with the object of a better scientific understanding. His answer is that, considering the distance between the semantic networks (the languages) of psychology and physiology, a physiological psychology does not have greater possibilities to develop as a science than there are to constitute a chemical sociology or a biological astronomy.

Nevertheless we must acknowledge that, whereas one does not know attempts to found a chemical sociology or a biological astronomy, there are numerous disciplines that intend to trace back the psychological phenomena to (neuro)physiological processes (firstly, physiological psychology and psychophysiology), medical pathologies to psychological variables (psychosomatic medicine), and even more numerous the studies that intend to disclose the relationship between mind and brain. In all these cases, those processes, which from a constructivist point of view acquire their "psychological" or "physiological" properties by the light of a specific language, are considered as intrinsically (ontologically) psychological or physiological. This view opens the way to the possibility of an interactionist dualism (Popper & Eccles, 1977), or of a monism that in the philosophical debate is declined in the two main views of reductive materialism and emergentist materialism (expounded in Bunge, 1980). The reason of the success of such psychophysical relationship can be traced to the fact that some of the "things" which physiology and psychology are interested in are "objects" of both the universes; universes which – not properly from a constructivist perspective – come to an undue interpenetration.

Consistently with the assumption of constructive alternativism, which implies the possibility to construe the "same" set of events within different scientific systems, Kelly points out the boundary of the range of convenience of his psychological theory (the boundary of its universe of objects) with the notion of *core constructs*, that is, «*those which govern a person's maintenance processes* – that is, *those by which he maintains his identity and existence*» (1955, p. 482, italics in the original). A great many of these maintenance processes (such as digestion) can be more adequately construed in terms of a physiological construction system. Moreover, Kelly rejects as deriving from a dualist view the notion of "emotion" so central in the traditional psychologies, in favour of *constructs relative to transitions* meant as *professional constructs*, i.e., as constructs that the psychologists may profitably use with reference to certain changes relative to the person's core structures[11]. The only correct way, from a constructivist point of view, to give a psychological interpretation to "physiological" processes (or vice versa) should consist in extending the range of convenience of the psychological construction system, that is, in including in the universe of objects of psychology also some of the "things" at present objects of physiology (as we tried to argue in Nuzzo & Chiari, 1992).

6. CONCLUSION

At this point, one might think that the psychologies based on a constructivist epistemology are in a privileged position in comparison to other psychologies, because they share the assumptions of the epistemology of psychology. However, as I have repeatedly underlined, these are the assumptions of *a certain* epistemology of psychology. Indeed, foundationalist

[11] See Kelly's original work, or Chiari and Nuzzo, (1985, 1988, 2009), for an analysis of these aspects of the theory.

(empiricist, rationalist) epistemologies, which are well-represented (not to say dominant) also in psychology, do not share the definition of science as a language which cuts out a universe of objects, to be meant as distinct from "things". The only privilege (if one likes to see it in this way) that one might recognize to the psychologies of constructivist matrix derives from their way to understand both science and the criteria of truth for scientific propositions; this way allows the psychologies of constructivist matrix to confront with psychologies based on different epistemologies, viewed as legitimately interested in different objects. But then, what is the universe of the objects of constructivist psychologies made of?

Considering constructivist psychologies as a whole, their universe of objects might consist in the personal ways to organize human experience; their point of view concerns personal points of views. For instance, Kelly's personal construct theory can be properly viewed as a metatheory: a psychological theory about personal theories. Therefore, constructivist psychologies share a phenomenological and interpretive approach, similar to the one of the variegated humanistic psychologies. On the other hand, to the extent that the scientificity of a discipline derives from clear protocol criteria, the adhesion to a constructivist epistemology is not in itself an element of strength; on the contrary, often the protocol criteria of many "foundationalist" psychologies are more clear and explicit of those of many "interpretive" psychologies, sometimes so vague in the operationalization of their objects as to appear almost "spiritual". Constructivist psychologies can carry also out progressive research programs (Lakatos, 1978) in the same way as the traditional natural sciences, without losing their psychological specificity, once they satisfied their protocol criteria.

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Knowing and the Unknown: An Existential Epistemology in a Postmodern Context

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ABSTRACT

Existential psychology and postmodern thought share skepticism about the ability of human beings to grasp objective truth, if it even exists. However, there are also important distinctions between postmodern and existential ways of knowing. This article provides a critical comparison of postmodern and existential epistemologies, with particular focus on how a dialogue between these viewpoints can strengthen both perspectives. Particular attention is given to the existential givens, which represent universal struggles without providing any claims of universal answers, that is, objective truth. The paper concludes discussion how a postmodern existential epistemology could provide an ideal framework for psychotherapy integration.

Existential psychology and postmodern philosophy share many similarities in their epistemology and general world view; however, there are also some essential differences. Often, these differences are tendencies rather than absolute distinctions. This paper will provide a comparison of the two epistemologies suggesting that through dialogue both approaches can be strengthened. Furthermore, it is maintained that an existential-integrative approach with postmodern sensitivities provides an ideal foundation for psychotherapy integration.

COMPARISON OF EPISTEMOLOGIES

Postmodern epistemology

A primary challenge of writing about postmodern epistemology is that there are not one, but many, postmodernisms. Furthermore, postmodern epistemology is best understood in a historical context. Early postmodern theories were primarily reactionary, focusing on deconstructing the modernist paradigm (Hoffman & Kurzenberger, 2008). This led to many mischaracterizing the primary feature of postmodernism as being deconstruction. However, contemporary postmodern theories incorporated more constructive tendencies. Even these more constructive movements maintain that objective truth is not something that can be known.

Rosen (1996, as cited in Erwin, 1999) provides a summary of constructivist epistemologies which applies to most, if not all, postmodern epistemologies. He wrote**,** "While there are a variety of constructivist models, they all hold in common the epistemological belief that a totally objective reality, one that stands apart from the knowing subject, can never be fully

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known" (p. 354)**.** Rosen identifies two cornerstones of postmodern epistemology: (a) calling into question the possibility of a known objective truth and (b) shifting the primary focus to the individual's subjective experience. Erwin (1999) takes a less antagonistic view toward objective truth than many postmodernists. Although he agrees with the majority of postmodernists that objective truth is not attainable outside of "mind and language" (p. 354), he does not go the next step to state there is not an objective reality.

This stance against objective truth is most strongly revealed in postmodernism's animosity toward metanarratives. Metanarratives are "all-encompassing intellectual frameworks such as science, logic, philosophy and religion, that are supposed to settle arguments about what is true" (Legg & Stagaki, 2002, p. 386). Postmodernists often use the term *oppressive* to describe metanarratives because they are typically imposed upon people. For example, those adhering to scientism, an ideological belief in the superiority and objectivity of the scientific way of knowing, often attempt to "convert" others to their way of thinking and are condescending to those who align with different epistemological positions. In fact, adherents of scientism remind one of the religious individuals who are so convinced of the superiority of their position that they spend much of their time trying to convert others. Postmodernists, by contrast, are critical of any singular way of knowing and opt for multiple ways of knowing or what is referred to as "epistemological pluralism" (Hoffman & Kurzenberger, 2008). In other words, postmodernism prefers to draw on many different approaches to knowledge instead of relying excessively on any one approach. Here, however, postmodernists are often quite inconsistent. Although they explicitly advocate for varied epistemologies, they often rely exclusively upon rational ways of knowing.

In a widely cited article on the differences in what he calls "constructivisms," Raskin (2002) states that although there are many types of constructivisms**,** the similarities among are greater than the differences. Common across the constructivisms is the belief that a material reality cannot be known,[1] with a few exceptions called "limited realism." This is consistent with the idea that an individual's worldview is constructed, not simply a subjective mirror of what is "real." Although most constructivists allow for both social and personal influences on the construction of how one sees and experiences the world, personal constructivists for more on personal constructions while social constructionists[2] focus more on the social influences.

It should be pointed out that postmodernism's break from modernism is not about the *content of knowledge* as much as *how individuals relate to or hold knowledge*. Anderson (1995) states, "[postmodernism] has to do with a change not so much in what we believe as in how we believe" (p. 2). In other words, one can believe the same content in premodern, modern, and postmodern paradigms, but why and how they believe it, as well as how they support it, differs. For example, in the West during the premodern area the primary epistemology was revelation, or revealed knowledge from God or an ultimate authority (see Hoffman & Kurzenberger, 2008). Religious knowledge was viewed as objective truth and, generally speaking, was unquestionable. In the postmodern era, the same knowledge would be viewed as constructed and not "real" (of course, some postmodern religionists might consider their beliefs a personal or social interpretation of the objective truth and therefore a close approximation of the truth). Thus, the same belief is approached and supported in different ways in different epistemological paradigms. One can see how the postmodern approach challenges the more rigid adherence to specific religious beliefs and creates a greater openness to other interpretations.

[1] Material realities are a special type of objective truth that are often referred to as "realism."
[2] Raskin points out that social *constructivists* prefer the language of *constructionism* over *constructivism.*

For a second example of how one may hold to the same belief but interpret and support it in a different way, consider the following: In the modern era, scientific knowledge, especially knowledge by using the scientific method, is typically viewed as objective truth. However, in the postmodern era, the same knowledge or belief is understood more in terms of one's interpretation and *construction* of the research findings. Also postmodernism and constructionism emphasize the social and political factors which impact the interpretation of the scientific research and often determine the approach and methods of the research itself. Thus in the postmodern era science is viewed as a therapy or as simply one epistemological stance among others. Religious beliefs, too, are viewed as theory. This does not mean they are not true, but the postmodern perspective emphasizes our inability to know what is objectively true. In short, what was considered objective fact in the premodern and modern epistemology is typically considered theory in postmodern epistemologies.

Finally, although all postmodern epistemologies agree that we do not have the ability to know objective truth, one of the debates in postmodernism is whether there is such a thing as objective truth. Raskin (2002) points out that there are postmodernists who believe there is some kind of objective truth and our knowledge, while subjective, may approximate it, at least at times. Other postmodernists, however, maintain that the only "truth" is our interpretation. Thus, postmodernists disagree on whether or not there is objective truth but almost all agree that whether there is or not, it can never be fully known.

Applications to psychotherapy

Hansen (2006) begins his application of postmodern epistemology to psychotherapy practice by asserting that "All traditional counseling theories, then, were conceived in a modernist epistemic context … that is, there was an assumed correspondence between the theoretical map and the actual psychological territory" (p. 291). Although overstated, Hansen's observation is important in contextualizing psychological theories in the history of psychology. If personality theory and psychotherapy practice do not begin to take postmodernism into consideration, they will quickly become outdated and irrelevant to many of the clients they are intended to serve. This does not require that we disregard the current theories that were developed during the modern period. Instead, we need to view them in a new, postmodern way and allow the theories to evolve. For example, most early psychoanalytic and psychodynamic approaches, including Freudian, neo-Freudian, and object relations theory, were primarily modernistic. However, the newer contemporary psychoanalytic theories, such as those by Stephen Mitchell (1988) and Martha Stark (2000), are clearly postmodern. Thus, in the psychoanalytic movement there is a clear evolution of theory from modern to postmodern perspectives.

The most powerful postmodern influence on the field of psychotherapy is the changing paradigms in outcome research. Psychology, through the 1990s and early 2000s, still relied primarily on a very modernistic outcome research modality: the empirically supported treatments. This outcome paradigm maintained that quantitative research, understood as objective science, could determine which therapies were most effective. The standards of research and outcome measures used in this paradigm were highly biased in favor of what Elkins (2009) refers to as "short-term, linear therapies." Gradually, there has been large scale discrediting of the empirically supported treatment movement. Interestingly enough, it is not a postmodern deconstruction, but rather what could be deemed as a highly sophisticated modernist analysis, that has called this movement into question. Through various independent meta-analyses and reviews of the literature, it was determined that the outcome studies used to sustain the empirically supported treatment movement did not, in reality, provide the

evidence claimed and were thus a farce, even from a modernist perspective (Hubble, Duncan, & Miller, 1999; Norcross, 2002; Wampold, 2001; see also Elkins, 2009, for a detailed review).

Thus, modernist analyses and arguments deconstructed the modernist fallacy of the empirically supported treatment (EST) movement and paved the way for the potentially postmodern movement of *evidence based practice in psychology (EBPP)* that is now beginning to dominate the field. The definition of EBPP, however, is still being constructed. Wampold, Goodheart, and Levant (2007) note:

> Evidence is not an unambiguous term and is usually undefined, but the term exists in a context and has a particular meaning. Evidence is not data, nor is it truth. Evidence can be thought of as inferences that flow from data. (Wampold, Goodheart, and Levant 2007, pp. 616-617)

Compare this to Hansen's (2006) position: "Investigative efforts, even supposed objective scientific ones, cannot yield truths that transcend the assumptive mind-sets of the investigators" (p. 293). Clearly, postmodernism is influence the thinking in psychology and the paradigm is shifting. The modernistic *truth* that characterized the empirically supported treatment movement is being replaced by the postmodern approach of EBPP.

Evidence has been intentionally defined in EBPP much more broadly than it was in the empirically supported treatment movement, although, as noted, this is still being debated. A broader view of evidence includes established theory, qualitative research, case studies, and clinical experience as well as empirical research. In postmodern style, EBPP does not so much change the value of scientific research but, rather, it changes the way that we understand scientific research. It is now simply one of many voices in the pluralism of epistemologies considered to be useful.

EBPP has introduced *principles* to replace the narrowness of specific *techniques* of the EST's. For example, those committed to EST's advocated for manuals that were typically step-by-step guides for the administration of techniques. In contrast, EBPP recognizes that contextual factors are the primary determinants of change in therapy. These contextual factors include such things as the therapeutic relationship, empathy, and a Rational that explains the client's problems and how to alleviate them (see Elkins, 2009). Although lacking the specificity and concreteness of techniques, a contextual factors approach to psychotherapy allows one to adapt the therapeutic approach to fit the uniqueness of the client. Thus, by understanding postmodern outcomes we arrived at postmodern therapy - a therapy that is adaptable to the personal and social particularities of the individual client and is not rigid and prescriptive as was the case with manualized procedures and so-called empirically supported techniques.

Hansen (2006) advocates that postmodern applications to psychotherapy should view theories as narrative structures with pragmatic utility. He states, "theories are narrative structures utilized for rhetorical purposes, designed to persuade the sufferer to consider experience from a different vantage point" (p. 293). These theories, then, are evaluated by their pragmatic utility. The contextual factors approach to EBPP shows that providing a plausible explanation for the client's problems and using this as a foundation for the therapeutic work is more important than the particular techniques used. In other words, postmodern and EBPP perspectives suggest that modalities and techniques are relative and that one therapy is as effective as the next as long as it is adapted to the client and the client embraces it.

A modernist problem still remains in EBPP, one that may need an existential solution. The dominant perspective within EBPP is still one of hedonistic impulse: to increase pleasure and decrease pain. The good of happiness and the bad of suffering remains intact as an oppressive

metanarrative for many who would look beyond these shallow conceptions of humanity and what ought to be sought in therapeutic outcomes. A true postmodernist psychology not only allows for varied means of psychotherapy, but also remains open to a variety of outcomes that can be deemed successful. It is necessary to keep the possibilities open to good suffering, beauty in sadness, and tragic happiness.

Existential epistemology

Existential psychology from its origins can be understood partially as a rebellion against modernism. As modernism was dispelling with emotions, human limitation, and subjective ways of knowing, Kierkegaard led the way into deeper emersion into these experiences. In psychology, while the behaviorists advocated for excessively rationalistic and materialistic views of psychology, Rollo May and James F. T. Bugental again rebelled through introducing existentialism to psychology. Existentialism, it could be argued, paved the way for postmodernism. A somewhat different position could also be held maintaining that existentialism was an early form of postmodernism.

At its core, existential psychology is ontological and epistemological. It is ontological insofar as it is concerned with the basic study of existence: What does it mean to exist? Many approaches to psychology, in particular the psychotherapeutic orientations, seek to control or overcome one's existence. Existentialists, instead, seek to be honest about the human condition and understand it as it is (Hoffman, 2009a). Human nature is not something to overcome, but rather some with which we all must face. Successful outcomes in existential therapy are about learning how to embrace our humanity, not overcome it.

Existentialism is epistemological in that it is always asking the question of "how we know" about existence. It is the epistemology of existentialism that really sets it apart from most other branches of psychology. While much of the field of psychology has focused on objective truth, quantitative research, and generalizability, existentialism has focused on subjective truth, qualitative research, and the individual's unique experience. This is not to say that objective truth or quantitative research are not considered or accepted, but rather that it was not give the same unquestioned authority that it enjoyed in the branches of psychology rooted in modernism.

Myth as a primary unit of existential epistemology

May (1991) advocated that myth is a basic unit of meaning and knowing. Myth also suggests a certain way of knowing that emphasizes that knowledge is inherently incomplete and must rely to some degree on faith. May was deeply concerned about the growing degradation of the idea of myth and the gradually accepted change in the popular understanding of myth as being false. Instead, May used the very postmodern understanding that myth was something that could not be proven true. Psychological theory, most scientific "facts," religion, and political ideology are all examples of mythical systems. They are ways of attempting to make sense out of the world, or an organization structure for meaning.

When myths become reified, or when they are believed to be fact, they often lose their sustaining power and become destructive. Fundamentalist religion is a prime example of this. When religion is reified into fact, instead of belief or faith, then it is often used to justify a lack of tolerance for that which is different. Since the September 11 terrorist attacks on the Twin Towers and Pentagon, Islamic fundamentalism has been used as a popular example of this. However, all major world religions have their destructive fundamentalists groups. Myth, then, is a more powerful form of meaning, or of the truth, than "the known facts."

Hoffman (2009b) built upon the ideas of May stating that "myths represent the universality of the existential givens and the particularity of the cultural response" (p. 264). An important

and, in my view valid, criticism of much of the existential writing is that it too often became an excessively individualistic approach that does not adequately take into consideration systems and culture. In *The Cry for Myth*, May addresses this very implicitly, but yet directly. He uses many illustrations of Western cultural myths as sources of meaning, and even necessary in developing personal meaning systems. He viewed the decreasing value of myth as directly connected to many problems in the West such as suicide and substance abuse.

This use of myth places meaning and worldview at the intersection of the personal and the collective. An individual is responsible for their worldview and their meaning systems, just as they are responsible for their behaviors. However, it would be naïve to believe that one can have a worldview independent of culture. For existentialists, limitations, or finiteness, is a basic given. In all things we are limited.

Experience and knowing

Existential psychology, despite its abstract appearance, is focused on lived experience and embodied ways of knowing. Similar to postmodernism, this generally means that multiple ways of knowing are given credence. Schneider (1998) illustrated that existential therapists often use the word "experiential" to mean different things. For some, such as Yalom, it refers primarily to the here-and-now experience. Schneider, however, uses it to refer primarily to kinesthetic experience, which is essentially bodily ways of knowing. The kinesthetic realm often is preverbal and held at the unconscious realm. For individuals who lack the more conscious awareness, the kinesthetic realm can be informative at the unconscious or subconscious levels of understanding. The bodily or kinesthetic realm is also a highly subjective realm, indicating that it is heavily based in one's personal experience.

Epistemological Similarities and Differences

At this point, it should be fairly evident that there are many similarities between the postmodern and existential approaches. In fact, the similarities strong enough that it would be easy to maintain that existential psychology is a postmodern psychology. I would argue, however, that this depends upon *which postmodernism* and *which existential psychology* is being referred to. With that in mind, I would maintain that an open dialogue could produce a unique postmodern existential psychology that builds upon the similarities of these approaches.

Similarities

The list of similarities is not intended to be exhaustive, but rather to focus on the most important similarities, especially pertaining to epistemological issues:

1. Existential and postmodern psychologies both take seriously the philosophical and epistemological aspects of psychological theory.
2. Both approaches emerged early on as a rebellion against modernism and, in particular, modernism's lack of acknowledgement of its limitations.
3. Existential psychology and postmodernism are deeply concerned with subjective ways of knowing and place greater emphasis on this than what is typical in most philosophical and psychological theories.
4. Both approaches emphasize multiple ways of knowing, including many non-traditional ways of knowing.

5. Postmodernism and existential psychology are skeptical of the ability to know objective truth, if it does exist. When it is granted that some objective truth exists, there is recognition of our inability to fully grasp it.
6. Existential psychology and postmodernism both recognize the personal and cultural elements involved in knowing, with variations on which is emphasized more.

Differences

Most of the differences listed in this section could be debated, which again focuses back to the discussion that there are many postmodernisms and many existential psychologies. Among the key differences are:

1. Postmodernism tends to be less embodied in its ways of knowing than existential psychology.
2. Postmodernism often ties reality closely to language. While existentialism would often agree with this, it also allows for other ways of knowing including the possibility of preverbal experience and knowledge.
3. Postmodernism is intentional about advocating for multiple ways of knowing, yet little attention is really given to the non-rational ways of knowing, such as experiential knowing, that is emphasized more in existential psychology.
4. Existential psychology is more open to certain types of universal truths. For example, the existential givens, that is death, freedom/responsibility, and the human desire for meaning, are intended to be universal struggles that all people must inherently face. While postmodernists may not deny existential givens such as death, they often struggle with using the label of this as universal.

IMPLICATIONS OF THE EXISTENTIAL-POSTMODERN DIALOGUE

Productive dialogues allow for something new to emerge out of the exchange. Furthermore, if dialogues are truly open, then it requires one to suspend judgment to a degree on beliefs which contradict one's own while simultaneously holding on loosely to one's own assumptions.

Lessons for Existential Psychology

Existential psychology has traditionally been underdeveloped on the topics of culture and diversity, which are important themes in postmodernism. Until recently, diversity had been virtually ignored in the existential literature (see Hoffman, Yang, Kaklauskas, & Chan, 2009; Schneider, 2008; Schneider & Krug, 2010). For existentialism to have an impact in the postmodern, globalized word, it will be necessary to develop more formalized theory and application in cross-cultural and diverse settings. Although this process has begun, existential psychology is still behind on this topic.

The existential psychology literature is largely devoid of clear, comprehensive epistemological statements. In many ways, it fell prey to the same mistake of the early deconstructivists in postmodernism, but existential has been less successful at moving beyond this. Although it is evident that existentialists use multiple epistemologies and favor subjective approaches, these are not sufficiently developed. Some of this is because existentialism has been less concerned about abstract theory for theory's sake. Its attention has been more on the lived applications of epistemology. Postmodernism historically struggled with the similar problems; however, it began facing these challenges earlier than existential psychology. Given the similarities between the values and interests between these approaches, especially related

to ways of knowing, existentialism may benefit by incorporating many of postmodernism's language and theory relevant to epistemology.

Lessons for Postmodernism

Postmodernism often does not live out its advocacy for multiple epistemologies and often relies heavily on an overly rationalist way of knowing. Furthermore, postmodernism too often remains in the realm of abstract thought not making it to the point of action. Existentialism could help ground postmodernism through integrating the more embodied ways of knowing and connecting the abstract thought to the lived realms of action.

Second, postmodernism's insistence that there are no objective truths often reaches the point of absurdity. For instance, it could be debated as to whether the existential givens really are a claim to be universals. On the surface, it appears evident they are universal claims, or metanarratives, in that it the existential givens explicitly state these are issues that all people will face. However, I would maintain that the existential givens do not qualify as metanarratives. Although the existential givens are realities one much face, as I have noted elsewhere (Hoffman, 2009b), these givens do not necessitate any particular answer. Existentialism has, in fact, intentionally remained neutral on claims at universal, or "correct," answers to the givens, sometimes questioning if such answers exist. If the givens also provided a claimed universal answer, such as eternal life through God, then they could rightly be considered a metanarrative. Instead, existentialism purports that the answer is culturally and personally determined, which parallels the personal and social constructivisms.

Postmodernists, it seems, are often so afraid of accidently advocating for metanarratives that they sometimes struggle to say anything meaningful about shared reality. Relevant to the claims of universals, it is hard to deny the reality of some givens – that we will die, the inevitable connection between freedom and responsibility, that we are relational beings, that humans seek meaning from their experience, and that emotions are part of the human experience.[3] However, although all people are impacted by these themes to existence, it is hard to find any universal agreement on the "right" answers to these struggles. Metanarratives are better understood as universal narratives on how one should respond to the existential givens, not the givens themselves.

EXISTENTIAL-INTEGRATIVE THERAPY AS A BASIS FOR INTEGRATION

Rational and process of integration

Before beginning to integrate psychotherapies, it is important to begin the questions, "why is integration desirable?" and "how is integration done?" The why of integration is to increase the effectiveness and adaptability of psychotherapy. However, as I will discuss, this may have little to do with integrating more effective techniques and approaches to therapy. Instead, the primary benefit of integration may be in the flexibility or adaptability it adds to the psychotherapy approach against fundamentalism in psychotherapy.

Indeed, all psychotherapy orientations have their fundamentalists, or therapists who believe their approach to therapy is superior to all others and should be applied in a rather specific manner. This mentality is strongly contradicted by contemporary outcome research that suggests most psychotherapies are equal in effectiveness and it is the common or

[3] There are many different takes on how to label and classify the existential givens. Frequently, most authors identify four to five givens. For a more detailed overview of the givens, see Yalom (1980), Hoffman (2009a), Heery (2009), or Schneider and Krug (2010).

contextual factors that extend across therapy orientations that really account for change (Elkins, 2009). The challenge to psychotherapy fundamentalisms is that they are not able to adapt to particular client needs and styles, which should become more evident shortly. Additionally, it seems there is something inherent in the approach of individuals that adhere to rigid fundamentalisms in therapeutic application that negatively impacts therapy relationships. Generally, fundamentalism tends to have a narcissistic and condescending tone associated with it.

How integration occurs is maybe the more important question. This can be answered by comparing *eclectic* and *integrative* approaches. Eclectic approaches to therapy drawn on a highly pragmatic frame of *if it works, use it*. They allow the therapist to rather randomly draw from different psychotherapy orientations utilizing whatever appears to fit best in the moment. Integrative psychotherapy also intends to allow therapists to draw on techniques from different therapy orientations, but does so in a more thoughtful way. From an integrative perspective, there still needs to be a foundation to one's approach to therapy which serves as a guide to what should be integrated when (Hoffman, 2009a). For example, Wolfe (2008) and Bunting and Hayes (2008) provide two examples of integrating cognitive-behavioral therapy with existential therapy. These two approaches to therapy are highly different and an eclectic approach drawing on these two approaches is likely to fail. To illustrate, existential therapy often tries to utilize or harness anxiety for motivation and as a guide to the therapy process. Conversely, many cognitive-behavioral approaches seek to reduce or eliminate anxiety, which would then decrease the effectiveness of anxiety as a guide. Drawing from both these approaches eclectically could easily lead to therapists working against themselves.

The integrative approaches by Wolfe (2008) and Bunting and Hayes (2008) demonstrate a greater awareness of this potential problem. These models suggest that cognitive-behavior therapy can be used in at least two distinctive ways when integrated with an existential approach. First, the cognitive-behavioral strategies can be used to help alleviate the initial crisis, then allowing for clients to move on to working on the more substantive, existential issues. Second, the cognitive-behavior strategies can be used to provide coping resources for when clients move into working on difficult existential issues, which often bring up some painful experiences. In using cognitive strategies in this manner, the productive side of anxiety can still be utilized when doing the depth work while managing more intense or extreme emotional states that may interfere with productive depth work.

The basis for effective integration can be elucidated through a discussion of the contextual factors of psychotherapy effectiveness. Elkins (2009) provides one of the most useful and thorough reviews of these contextual factors drawing from the various meta-analyses and critical reviews of the research. Many of these factors pertain to relational elements of psychotherapy. However, some of the most interesting contextual factors for the purpose of this article focus on the therapist's and client's belief plausible explanation for the client's problems and healing or growth. According to Elkins, the research literature suggests that techniques do make a difference; however, it is not the specific technique, but rather that technique is one in which the therapist and client can believe. From an effectiveness perspective, *believing in what you do as a therapist is generally more important than what you actually do!*[4] This basic understanding is beginning to change the face of psychotherapy. The prior debate of which approach to psychotherapy is best is quickly becoming nonsensical.

[4] There are limitations to this. Some types of therapy, such as Rebirthing Therapy and Reparative or Conversion Therapy, have been shown to be dangerous and often do cause harm. This statements should not be interpreted in the relativistic extreme that if you can explain and justify it, it should benefit the client.

Instead, what is important for therapy to be effective is to believe in what you are doing. From the client's view, it is important that they, too, are able to believe in what is being done by the therapist. From this perspective, it is less important to determine which approach to therapy the research suggests is best for a client who checks the particular demographic boxes and more important to give consideration as to which approach to therapy fits with the client's values, belief system, and relational style.

In returning to the discussion about therapy orientation fundamentalism, at first glance it would appear that this may be good for therapy. After all, these therapists often strongly believe in what they do. However, they also are not as able to adapt to the specific needs of the client and tend to impose their beliefs on their client. Fundamentalist therapists focus on the therapist's beliefs about therapy without adequate consideration to the client's needs, desires, and preferences. This disconnect impacts the client's belief in therapy as well as the relational factors. Therapists, therefore, need to be able to adapt to their client's specific needs and style in order to be effective. From postmodern and existential perspectives, this should not be a surprise.

From the postmodern perspective, earlier I referred to the Hansen (2006) quote, "theories are narrative structures utilized for rhetorical purposes, designed to persuade the sufferer to consider experience from a different vantage point" (p. 293). This perspective if highly consistent with May's (1991) view on myth and Hoffman's (2009b) application of this. Psychotherapy orientations are different myths or narratives. Clients often enter therapy because they have lost faith in their myths or their myths are no longer working for them. Therapists, then, help clients explore their mythology while offering some alternative perspectives. The obvious danger, here, is that it can become quite easy to impose a mythology upon a client, particularly if the therapist is persuasive enough. The job of the therapist is to help the client reconsider, adapt, and change their myths, not to change them for them. Part of this may, at time, include sharing a new mythology, one rooted in the psychotherapy orientation of the therapist. However, the therapist should remain vigilant in assessing when therapy is moving against the client's values and desires.

It is my hope that the new paradigms in psychology – Evidence-Based Practice in Psychology, contextual factors, and postmodernism – bring a new approach to assessing which therapy is the best fit for which clients. Therapist-client matching should not be an objective process, but rather is highly subjective. Clients should be encouraged to consider which therapist is the best fit for them and not just be compliant with whomever they get assigned to or randomly choose from a therapist list. Similarly, the ethical therapist should begin the therapy process not trying to convince the client that they are the best therapist for them, but rather trying to identify if the client is the right fit for their approach to therapy. The profession of psychology should look suspiciously on the therapist who thinks every client is a good fit for him or her, and should develop better recourses to help clients become informed consumers. I will close this section with a discussion of client-therapist matching in postmodern times.

Postmodern client-therapist matching

In the practice in which I see clients, we adhere to a policy of providing free initial consultation in order to help clients find the best therapist. When a client schedules a consultation, we work to help them understand the variety of therapists that we have available at our clinic as well as understanding what options are available elsewhere in the community. We clearly state to the potential client that we are invested in helping them find the right therapists for them. As our practice provides existential and other related depth psychotherapies, this means that many clients are not the right fit for our practice. For example, when a client comes to an

initial consultation and makes it clear that their priority is expediency and skill development, it is evident that they are not the right fit for our practice and we refer them elsewhere. Other times, a client may report that they are overwhelmed currently, and in need of some quick relief, but also want to explore the deeper factors that helped contribute to this situation. In this case, our practice can be a good fit through employing an existential-integrative approach, such as that discussed by Wolfe (2008) or Bunting and Hayes (2008) previously.

Unfortunately, most therapists are not taught how to lead a client through a process of client-therapist matching in this manner. Additionally, economic pressures and competition for clients often taints the therapist's motivation when considering if a client is the right fit. Many therapists, too, provide the type of therapy that keeps them in the good graces of third party providers instead of providing the best and most appropriate therapy for clients. In many countries today, therapy cannot be separated from the economic and political power structures. When this occurs, it is the client who is negatively impacted more than the therapist.

The contextual factors revolution does have the potential to change the field of psychotherapy and bring a revitalized ethical approach to therapeutic practice. The biggest barrier to this revolution may be these political and economic forces. If the contextual factors movement is to succeed in a broad way, changes will be required beginning at the training level. Students will need to be trained on how to match clients with the appropriate type of therapy based upon the client's values, not the objective research. This means therapists will need to be familiar with a wide array of therapeutic approaches and their values, not just their own, in order to make effective referrals. Therapists in training will also need to be trained on how to work ethically within the political and economic pressures that impinge upon the field of psychotherapy. This will not be an easy shift in a field, but it is one that is desperately needed.

A postmodern existentialism for integration

Several factors make a postmodern existential approach a fairly ideal framework for integration. First, both postmodern and existential values advocate for recognition of limitations. No one approach, even an integrative approach, can be adapted to work with all clients. Additionally, no therapist, no matter how adaptable, can work successfully with all clients. The best therapies, and the best therapists, are ones which begin by clearly acknowledging their limitations.

Second, the emergent trend for the contextual factors is highly postmodern and existential. This should not be intended to claim that either postmodern psychology or existential psychology is the basis for the common factors. Indeed, the common factors lie beyond the therapy orientations as a model for how to understand the way various orientations work. However, these approaches very naturally advocate for a value system consistent with the common factors approach. An existential-integrative approach also is one that has long advocated for many of the relational common factors as essential in the client change process.

Third, postmodern and existential approaches are anti-dogmatic. There could be nothing less postmodern than a fundamentalist approach to postmodernism, and nothing less existential than a fundamentalist approach to existentialism. Of course, there are people who could rightly fit these labels; however, their approach is necessarily internally inconsistent.

Fourth, existentialism is a very fluid approach to therapy that is not rooted in techniques. This allows for great adaptability when used as a foundation for integrating other approaches. Some existential therapists, such as Yalom (1980), even go as far as stating that existential psychotherapy is not an orientation at all. Although I would disagree with this appraisal, I would agree that existential therapies lack of structure, which makes it look very different

from other therapy orientations, also allows it to be more adaptable when integrating psychotherapy approaches.

CONCLUSION

Existentialism and postmodernism, in the philosophical and the psychological realms, share a great deal of commonality. However, there are also points of divergence that provide a basis for dialogue. In this paper, I have examined some of the key similarities and differences, primarily as they related to epistemology and psychotherapy. This was not intended to be a comprehensive comparison, and indeed there are other challenging differences if one moves beyond the delimiting factors or psychotherapy and epistemology. However, there is great potential for an integration of postmodern and existential ideas into a foundation for broader psychotherapeutic integration.

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Epistemologia dello studio del caso clinico: note sul metodo della psicoanalisi

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> *Se non ci fosse scelta, perché autorizzare un soggetto a rimettere in gioco nell'esperienza analitica la sua propria posizione? A mio avviso, l'analista non riceve casi clinici: siamo noi a trasformarli, per le nostre elaborazioni di sapere, in casi clinici. Lo psicoanalista, se riceve dei casi, riceve casi etici. […] L'etica concerne l'ex-sistenza, cioè la dimensione impensabile in cui si decide la posizione soggettiva, la scelta.*
>
> J.-A. Miller, *Non c'è clinica senza etica*[1]

ABSTRACT

The experience that a subject makes of his own unconscious is at the heart of clinical psychoanalysis: psychoanalysis places the subject at the centre of the therapeutic process, following the peculiarity of the singular existence. In this paper, the epistemological basis of a clinical case study and the process of psychoanalytic cure are clarified by referring mainly to Freud's and Lacan's theoretic and clinical perspectives. Particular attention is paid to the methodological basis of the psychoanalytic model and to the fundamental criteria for the study of a clinical case so as to provide a wider and more systematic knowledge of the psychoanalytic experience.

1. CLINICA E RICERCA

Nel più ampio panorama della ricerca psicodinamica e relazionale si parla molto di valutazione e si dice che bisogna dimostrare con i fatti quello che succede in una cura. Gianluca Lo Coco e Girolamo Lo Verso (2007) interrogandosi sui presupposti etici e scientifici della psicoterapia, si fanno promotori della necessità di uno studio sempre più attento al modo in cui la pratica clinica viene concepita, pensata, progettata, messa in atto, e su ciò che in essa avviene tenendo conto delle variabili strutturali e di processo che la costituiscono e che possono essere osservate. A tal proposito Lo Coco e Lo Verso si pronunciano così:

> garantire trasparenza ed 'oggettività' alle pratiche psicoterapeutiche rappresenta oggi un valore etico non solo nei confronti della comunità scientifica e professionale, ma anche verso i pazienti che necessitano di psicoterapie meno caratterizzate da una rigida autoreferenzialità e più centrate sui bisogni terapeutici della persona. (Lo Coco & Lo Verso 2007, p. 194)

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[1] Miller J.-A. (1983), *Non c'è clinica senza etica*, cit., p. 275.

La ricerca rappresenta un ambito fondamentale per la riflessione epistemologica sulle caratteristiche e sul funzionamento della psicoterapia, in quanto si propone di garantire i requisiti di base di una professione scientifica.

La storia e il progresso della ricerca in psicoterapia[2] si sono sviluppati mantenendo in tensione tre vettori principali: 1) lo studio dell'esito del trattamento *(outcome research*); 2) lo studio dell'efficacia della psicoterapia (*efficacy and effectiveness*); e 3) lo studio del processo di cambiamento (*psychotherapy change process research*). Nell'ambito della ricerca in psicoterapia si è andata definendo una sorta di gerarchia delle prove scientifiche mostrate nelle differenti tipologie di studi: quelle che sono risultate più solide fanno riferimento ai *randomized controlled trials*, dove il livello di efficacia delle psicoterapie può poggiare sul rigore degli studi sperimentali (*efficacy*). Tuttavia il *furor misurandi* che ha caratterizzato una certa fase della ricerca in psicoterapia si è rivelato piuttosto inefficace nel tradurre i risultati degli studi sperimentali in contributi che potessero essere davvero utili per la complessità della realtà clinica. Recentemente si è privilegiata una soluzione di compromesso che potesse congiungere il *contesto della verifica* (la ricerca) con il *contesto della scoperta* (clinica), cercando di raggiungere un livello di efficacia (*effectiveness*) sempre più vicino alle problematiche reali della pratica clinica routinaria.

Negli studi sull'efficacia clinica (*effectiveness*) è sottolineata la differenza e la distanza da quelle prospettive che considerano il paradigma sperimentale come il *gold standard* della ricerca. Ciò non vuol dire che venga respinta l'esigenza di verifica e controllo empirico che dovrebbe caratterizzare ogni studio serio sul processo e sull'esito delle psicoterapie. La ricerca si configura infatti come una forma di rigore nell'indagine di quei fattori che effettivamente intervengono in una cura, ossia come un campo metodologico che permette di aggregare validamente le osservazioni e le inferenze dei clinici.

La clinica, tuttavia, sembra conservare la possibilità di attingere a quel campo di fenomeni ("contesto della scoperta") che la ricerca di laboratorio sembra tagliare fuori nel momento stesso in cui si costituisce come contesto della verifica. È in tal senso che si può assumere un atteggiamento critico nei confronti del termine *sperimentale*, mantenendo invece ben chiara la necessità di un ancoraggio *empirico* per ogni riflessione scientifica sulla dimensione clinica. L'esperienza clinica fornisce infatti una base empirica che si sottrae alla replicabilità sperimentale. Gli eventi di una psicoterapia non riescono a soddisfare le richieste di controllabilità delle metodiche di ricerca delle cosiddette scienze "dure". L'oggetto di studio della ricerca in psicoterapia è piuttosto un "viaggio",[3] il percorso di un soggetto nell'ambito di una relazione terapeutica. In ambito psicoanalitico lacaniano tale questione viene esplicitata sottolineando che si tratta di una «clinica-sotto-transfert» (Miller 1987, p. 95). Ciò condiziona e definisce le stesse possibilità di conoscenza del processo terapeutico da parte di ogni

[2] Cfr. Migone 2006.

[3] Nell'*Introduzione* al volume *La ricerca in psicoterapia. Modelli e strumenti* i curatori descrivono con la metafora del viaggio il percorso che paziente e psicoterapeuta compiono nell'arco più o meno lungo di una serie di sedute: «Uno degli scopi della ricerca in psicoterapia è studiare se e quando i nostri viaggiatori arriveranno alla meta. È questa la ricerca sull'outcome, che vuole rispondere alla domanda se la psicoterapia *funziona*. Per la verità conosciamo già la risposta, la psicoterapia è un viaggio che raggiunge il suo scopo. Ma questa conoscenza non arresta le nostre domande e più che un traguardo rappresenta ormai una partenza. Come si misura la riuscita di un viaggio? Cosa condiziona la scelta del metodo che adotteremo? Qual è il mezzo migliore per viaggiare? Quale il prezzo? E quali variabili dovremo considerare? […] E se la 'riuscita' del viaggio dipendesse più dalla qualità della relazione tra i due viaggiatori che dai mezzi impiegati (fattori aspecifici vs specifici)? Il viaggio è un'esperienza 'unica' o può essere scomposto nelle sue componenti?» (Dazzi et al. 2006, pp. XXVII-XXVIII).

ricercatore, anche del più discreto, che magari non entra mai nella stanza d'analisi se non attraverso un piccolo registratore in grado di afferrare indistintamente tutti i significanti emessi dal paziente durante la seduta.

La funzione dell'analista non consiste soltanto nel conoscere e studiare ciò che avviene in una cura: l'intervento clinico mira soprattutto a produrre nel tempo una trasformazione soggettiva. Si tratta di una questione che pone dei limiti e dei vincoli etici per ogni procedura conoscitiva che viene applicata in psicoterapia. Tuttavia, l'orizzonte del metodo scientifico è sempre necessario affinché il clinico possa seguire e condividere il rigore richiesto dalla struttura e dalla contingenza dell'inconscio.

2. LA PSICOANALISI TRA CLINICA E SCIENZA

La psicoanalisi è un'attività empirica di tipo clinico-terapeutico che si confronta elettivamente con casi individuali o con gruppi e, allo stesso tempo, è una disciplina a carattere scientifico che nell'ambito delle malattie mentali va alla ricerca di regole, concetti generali e strutture invarianti dell'esperienza.

La questione con cui l'esperienza clinica interroga il tipo di conoscenza messa in gioco dalla stessa pratica psicoanalitica riguarda dunque la necessità di individuare dei principi generali che permettano di ascoltare e leggere le manifestazioni della soggettività del paziente e in tal modo di orientare il clinico nella conduzione della cura. A tal fine è innanzitutto necessario individuare quegli elementi – i cosiddetti "significanti *maître*" – che concorrono a strutturare l'esperienza soggettiva del paziente. Si può così ottenere quell'insieme di elementi che consentono un'analogia tra determinate strutture cliniche e gli svolgimenti particolari del progetto di mondo del soggetto. Tale ri-conoscimento non è però assimilabile a un procedimento conoscitivo che si può applicare in modo standard, poiché – come ricorda Bion – c'è sempre uno scarto tra il modello e l'astrazione.

Nel ventunesimo capitolo di *Apprendere dall'esperienza* Bion sottolinea la differenza tra modello e astrazione, riservando

> l'impiego del termine "modello" per indicare una costruzione nella quale immagini concrete vengono combinate fra loro; il legame tra le varie immagini dà spesso l'impressione di un racconto tra i cui elementi sussiste un rapporto causale. In altri termini, il modello è costituito di elementi tratti dal passato di una persona, mentre l'astrazione è, per così dire, tutta impregnata di presupposti (*preconception*) relativi al suo futuro. (Bion 1962, p. 118)

L'astrazione, pur somigliando al modello, ne differisce

> in ragione della sua maggiore flessibilità ed applicabilità ottenute grazie alla scomparsa di qualsiasi particolare immagine concreta: gli elementi di un'astrazione non sono combinati come in un racconto, ma sono tenuti assieme da un metodo che vuole evidenziare i rapporti anziché gli oggetti in rapporto. (Bion 1962, p. 118)

Quindi il modello ci permette di individuare degli oggetti concreti e di raccordarli secondo leggi note. L'astrazione subentra quando il modello lascia qualcosa di "insaturo"; rimane infatti sempre qualcosa di insaturo che richiede le inferenze del clinico o del ricercatore. Questo è un limite che vincola il ragionamento clinico, ossia il modo di organizzare i dati a disposizione: il ragionamento tenta di istituire un legame tra le formulazioni teoriche del clinico e la soggettività storico-esistentiva di ciascun paziente in analisi.

3. LA CONOSCENZA E IL SUO USO CLINICO

Ogni disciplina che intende entrare nel campo della scienza è confrontata con due tematiche cardine che assumono una valenza filosofica: che cos'è la conoscenza e che cos'è la realtà. «La prima tematica viene affrontata dalla teoria della conoscenza (o gnoseologia o epistemologia), la seconda dalla metafisica o dall'ontologia» (Vassallo 2003, p. 4).

Lo stile di conoscenza che viene privilegiato dal *logos* scientifico fa riferimento a un sapere predittivo e calcolante, ma allo stesso tempo richiede una ragione strategica che è congetturale e qualitativa. Le generalizzazioni e le argomentazioni del *logos* scientifico sono caratterizzate dall'induzione e dalla deduzione. La scienza però si muove a partire da una base instabile che implica un'operazione logica definita da Peirce con il concetto di abduzione.[4] L'abduzione non si confronta solo con stime dei fenomeni di tipo quantitativo, poiché interpreta indizi e tracce. Si tratta di un tipo di inferenza che consiste nella decodifica di un evento in riferimento a contesti o circostanze non codificate. «Essa serve anche per individuare il codice (o il sottocodice) corretto per un messaggio impreciso» (Eco 2008, p. 186).

Nella scienza la logica dell'abduzione è operativa non solo nei cambiamenti di paradigma, poiché continua a essere necessaria ogni qualvolta il paradigma teorico prescelto viene applicato nella prassi. Ciò si rivela di estremo interesse per comprendere il tipo di ragionamento che viene impiegato dallo psicoanalista nella conduzione della cura. L'abduzione infatti contempla uno iato che si frappone tra le conoscenze già acquisite e la loro applicazione al caso particolare. Il procedimento diagnostico – ma anche l'interpretazione – chiama in gioco non solo un conoscere teoricamente ma anche un ri-conoscere praticamente.

> Questo è punto centrale per l'epistemologia della diagnosi: la diagnosi è un ri-conoscimento, un ri-conoscimento della *situazione* che ha portato allo stato morboso; è il ri-conoscimento della *situazione*, o del *modo* (spesso complessissimo) in cui elementi svariati possono essersi intrecciati ed aver agito gli uni sugli altri fino a condurre a quell'esito che è la malattia. Diagnosticare significa ri-conoscere. *E ri-conoscere non è possibile se già non si conosce*, se non si conoscono le leggi (chimiche, fisiologiche, fisiche o, per esempio, biologiche) che presiedono al *mutevole* configurarsi ed intrecciarsi di condizioni singole che conducono a stati patologici. È esattamente questo mutevole intrecciarsi di condizioni che il clinico *ipotizza* e che, in accordo con leggi generali, dovrebbe spiegare lo stato patologico. Ed è qui, nell'*immaginare* questi possibili intrecci di condizioni, che si esercita soprattutto la genialità e la creatività del clinico (e, ovviamente, queste sue ricostruzioni dovranno essere passate, come vedremo, al vaglio delle prove più severe).
>
> […] Dunque: diagnosticare vuol dire ri-conoscere: per ri-conoscere occorre prima conoscere. *Ma conoscere non significa possedere un meccanismo automatico di ri-conoscimento. Possiamo conoscere ed essere, simultaneamente, incapaci di riconoscere*. (Antiseri 2001, p. 78)

Il passaggio "dalla conoscenza al suo uso nella clinica" implica un'interpretazione (un'inferenza) che viene rivolta a un messaggio non codificato. Inoltre nel campo dell'esperienza psicoanalitica le variabili che dominano la struttura del soggetto scartano la

[4] L'abduzione è un «ragionamento comune sia nella vita quotidiana sia nell'ambito scientifico con cui si va alla ricerca delle cause di un fenomeno o di un fatto sperimentale. Ad esempio, 'L'assassino ha lasciato tracce di fango sul tappeto. Chiunque fosse entrato dal giardino avrebbe lasciato tracce di fango sul tappeto. Quindi l'assassino è entrato dal giardino'. Da un punto di vista logico l'inferenza non è corretta (l'assassino avrebbe potuto lasciare le tracce di fango senza essere passato dal giardino per sviare le indagini). In generale, se si verifica *B* ed è noto che 'Se *A*, allora *B*', si assume *A* come causa di *B*, almeno fino a prova contraria» (Palladino & Palladino 2005, p. 100).

possibilità di una misurazione ripetibile, si tratta piuttosto di sfumature particolari che possono essere colte nella loro pertinenza clinica solo dopo un'opportuna formazione. La formazione e l'esperienza clinica sono necessarie affinché l'analista sia in grado di decodificare non solo il messaggio, ma anche il contesto e le circostanze in cui esso si realizza. Nei termini della psicologia della Gestalt, potremmo dire che la stessa forma può rappresentare figure diverse e che la stessa figura può avere a sua volta una funzione e un significato diverso a seconda dello sfondo che ha.

4. ABDUZIONE E SOGGETTIVAZIONE

La formazione analitica consiste non solo nella trasmissione e acquisizione di una conoscenza teorica ma anche, e soprattutto, nella possibilità di soggettivare ciò che si conosce a livello teorico: la soggettivazione è intimamente collegata al legame che si stabilisce tra i nomi e le cose, tra la sintassi dei concetti e le smagliature del reale. In ambito clinico la soggettivazione è necessaria in quanto è un presupposto, non la garanzia, della possibilità di ri-conoscere. Il clinico compie delle scelte sullo sfondo di un'assenza di garanzia, di una garanzia in grado di certificare in anticipo l'efficacia e la pertinenza di una diagnosi, di un'interpretazione o di un atto analitico. In altri termini, non c'è un modello per applicare i modelli e dunque nessun modello può determinare quali siano le sue applicazioni corrette. Non c'è un metamodello, un metalinguaggio: non c'è una regola che ci dica come applicare le regole, per dirla con Wittgenstein.[5] È in questo momento di solitudine che le questioni metodologiche poste dalla clinica trovano il loro punto di giunzione con la questione etica dell'interpretazione analitica.

5. UNIVERSALE E CONTINGENTE

Gli oggetti di studio della conoscenza scientifica sono degli oggetti che per quanto possano essere mobili non impediscono la ricerca di costanti e di leggi esprimibili mediante relazioni matematiche. Sebbene la conoscenza psicoanalitica si muova poggiandosi sul ragionamento abduttivo, è caratterizzata dal fatto che «deve affrontare situazioni in bilico, dove basta una piccola variazione per evitare o determinare la catastrofe» (Bottiroli 1987, p. 133). Se «un esperimento è giudicato scientificamente interessante quando permette di raggiungere un risultato *universale*» (Dalla Chiara & Toraldo di Francia 1999, p. 5), un intervento clinico risulta invece valido quando non smarrisce il suo interesse per l'evento *contingente*. Da questa angolatura la psicoanalisi, in quanto prassi clinica, è più vicina al paradigma delle scienze storiche e applicate piuttosto che a quello delle scienze teoriche.[6]

Ora, possiamo osservare che il clinico che fa teoria a partire dalla sua esperienza con i pazienti si muove appunto su un livello di inferenza che chiama in causa da un lato il versante

[5] «Una regola non può determinare alcun modo di agire, poiché qualsiasi modo d'agire può essere messo d'accordo con la regola» (Wittgenstein 1953, *Ricerche filosofiche*, I, p. 201).

[6] «Dalla prospettiva metodologica le scienze si dividono, come si sa, in tre grandi gruppi: *scienze pure, scienze storiche, scienze tecnologiche o applicate*. Nel primo caso, nel caso delle *scienze teoriche* (o teoretiche o generalizzanti o pure), noi siamo interessati all'invenzione e alla prova di ipotesi universali o leggi (come quelle della statica, dell'elettrologia, dell'ottica, della genetica, e così via). Nel secondo caso, quello delle *scienze storiche*, le leggi (insieme a condizioni iniziali) sono usate per spiegare (cioè dedurre) un asserto descrivente un fatto già accaduto (sia questo un fatto sociale, un evento geologico come il corrugamento ercinico, la scomparsa di una specie, un comportamento aggressivo, ecc.). Nel terzo caso, quello delle *scienze tecnologiche*, le leggi (insieme a condizioni iniziali da porre in base ai comandi delle leggi) sono usate per fare predizioni» (Antiseri 2001, p. 73).

ipotetico delle formulazioni sui singoli casi osservati e dall'altro la ricerca di una serie di principi che possano generalizzare il funzionamento individuato.

Per lo psicoanalista il rapporto tra universale e singolare è un luogo di tensione sia sul piano delle generalizzazioni della scienza (induzione) sia sul piano di applicazione nella prassi clinica (abduzione): c'è un'inquietudine metodologica che viene generata da questa tensione. Nella scienza l'incontro con le *anomalie* che mettono in crisi i precedenti paradigmi apre la possibilità per il cambiamento e il progresso scientifico.[7] La pratica psicoanalitica rimane vincolata alle *questioni* del singolo caso clinico e lo studio delle strutture soggettive (*teoria*) viene sempre mantenuto in tensione con il suo complemento necessario: l'analisi del metodo nella fase della sua applicazione (*critica*). Un'applicazione che viene permeata dallo stile e dalla creatività del clinico.

La creatività dell'analista è una necessità di struttura prescritta dal metodo clinico. La combinazione di leggi e la conseguente applicazione al singolo caso richiede una tipologia di inferenze che possono oscillare dall'abduzione "automatica o dizionaristica" fino a un tipo di abduzione definita "strategica":

> A colui che si serve di un'abduzione strategica – al soggetto definito dalla *metis* – non si può chiedere quella pazienza, nelle verifiche, che presupporrebbe l'immobilità o la quasi-immobilità del Caso e del Risultato. Lo stratega si trova ad agire in situazioni che la sua interpretazione influenza o modifica. […] Il suo fallibilismo potrebbe apparire come una proprietà deprecabile anche se necessaria, un limite di fronte a cui rassegnarsi e non un carattere positivo nella propria specificità. È questo carattere che bisogna invece rivendicare: nell'abduzione strategica si mostra il lato *anteriore* del tempo, l'anticipazione incerta, fascinosa, legata a scelte irreversibili. (Bottiroli 1987, p. 135)

Si può inoltre individuare una precisazione ulteriore sulle finalità dell'intervento dell'analista: l'interpretazione muove da un tempo logico anteriore e mira a riprodurre per il paziente stesso il medesimo iato che separa il prima e il dopo che caratterizza l'anticipazione incerta di ogni scelta, compresa quella clinica. La seduta psicoanalitica viene così concepita come un dispositivo simbolico in grado di far «*fare presa all'aleatorietà dell'incontro*» (Recalcati 2002, p. 245). In tale prospettiva si può comprendere quanto l'approccio clinico della psicoanalisi si prenda cura delle questioni del soggetto alimentando il rapporto che ciascuno intrattiene con la dimensione creativa dell'esistere.

6. STORIA E INCONSCIO IN PSICOANALISI

La psicoanalisi si trova di fronte a dei fenomeni clinici che non possono prescindere dal contesto e dalla storia del soggetto che li manifesta.

> Nel sintomo, è mediante un codice privato che si stabilisce un nesso tra espressione e contenuto. L'interpretazione dovrà muovere al riconoscimento di associazioni strettamente soggettive, riportando alla coscienza la regola che è stata sottoposta a rimozione. (Bottiroli 1987, p. 135)

La psicoanalisi è una cura dove il racconto che un soggetto fa della sua storia è finalizzato alla scoperta della peculiarità singolare del suo desiderio inconscio. La psicoanalisi si avvicina ai pazienti privilegiando ciò che essi dicono in seduta. La preminenza data all'esperienza di parola conduce al di là della semplice considerazione – purtroppo tanto diffusa in alcuni ambiti della

[7] Cfr. Kuhn 1970, pp. 75-89.

psichiatria e della psicologia – per cui i sintomi psicopatologici vengono assimilati a dei deficit da riparare o ad epifenomeni del biologico. La psicoanalisi è invece tutt'altro che la riduzione dell'uomo al solo piano biologico: i sintomi acquistano un significato e non sono semplicemente delle secrezioni bizzarre del cervello. In psicoanalisi i sintomi si configurano quindi oltre che come «un evento di corpo» anche come «un avvento di significazione».[8]

In una seduta psicoanalitica la comprensione dei fenomeni clinici rimanda necessariamente alla loro articolazione. E la struttura che sottende il manifestarsi dei fenomeni non è altro che un'ipotesi che viene formulata rispetto ai rapporti che ne regolano l'insorgere. L'inconscio è appunto l'ipotesi freudiana rispetto alla sofferenza del sintomo e si configura come il principio della pratica analitica.

L'inconscio, in quanto struttura significante, ci consente di costruire una trama nelle storie che ascoltiamo. L'anamnesi e la storia di un soggetto possono essere intese come "il dispiegamento dei significanti": si tratta di individuare quegli *eventi*, quei *detti* in cui riconosciamo (per inferenza) una trama narrativa che ritorna con una certa ripetitività nella storia del soggetto. L'occhio clinico deve essere quindi rivolto verso ciò che si ripete con analoga struttura di rapporti.[9] I significanti che si ripetono ci segnalano il marchio dell'incontro del soggetto con certi eventi e detti dell'Altro.

La psicoanalisi è dunque un'esperienza dove vengono mobilizzati i significanti e dove si producono degli effetti di significazione che ricevono un ascolto il cui obiettivo è di isolare "i significanti *maître*" che hanno tracciato la figura del destino del soggetto. La serie delle sedute si configura come una scansione che intende far produrre al paziente una nuova elaborazione della sua storia, con il fine di modificare i rapporti del soggetto con il reale.

6. STORIA E SCRITTURA DELL'ESPERIENZA CLINICA

La necessità dell'elaborazione di una forma logica degli eventi permette di accostare le procedure di conoscenza della psicoanalisi a quelle della storiografia. Psicoanalisi e storiografia si strutturano infatti sulla base di questioni analoghe:

> ricercare dei princìpi e dei criteri in nome dei quali comprendere le differenze o assicurare forme di continuità tra l'organizzazione dell'attualità e le configurazioni passate; attribuire valore esplicativo al passato e/o rendere il presente capace di spiegare il passato; ricondurre le rappresentazioni di ieri e di oggi alle loro condizioni di produzione; elaborare – a partire da dove e in che modo? – modi di pensare e dunque di oltrepassare la violenza (i conflitti e gli azzardi della storia), ivi compresa quella forma di violenza che si sviluppa all'interno del pensiero stesso; definire e costruire quella narrazione che costituisce, per entrambe le discipline, la forma privilegiata che tende ad assumere il discorso esplicativo. (De Certeau 1978, p. 80)

La storia implica una marcatura del tempo e il gesto, che mette a distanza il tempo vissuto per farne oggetto di un sapere trasmissibile, è indissociabile dalla prospettiva che viene assunta nella scrittura della storia stessa. In ambito psicoanalitico scrivere una storia clinica vuol dire generare un passato, circoscriverlo, organizzare il materiale eterogeneo dei fatti per costruire nel presente una ragione, un filo logico. In altre parole, scrivere una storia clinica è un modo

[8] Cfr. Miller 1999.

[9] Interviene in questo punto l'importanza della teoria delle strutture cliniche a cui si fa riferimento: è il paradigma di osservazione di una cura che orienta gli elementi da isolare e la rete di rapporti da individuare. Al di là della terminologia specifica per ogni paradigma, ciò che risulta determinante è la strutturazione del campo fenomenico che si opera.

per esorcizzare il malinteso dell'oralità e per evitare un discorso autoreferenziale, dove la parola è l'inesistenza manifesta di ciò che designa.

> L'esposizione di casi clinici rappresenta la modalità elettiva tramite cui lo psicoanalista dà una testimonianza concreta della sua pratica. Essa è un esercizio inaggirabile di dimostrazione e di trasmissione di un'esperienza e, giustamente, occupa un posto di primaria importanza nell'avanzamento del lavoro della Scuola. (Mazzotti 2002, p. 7)

La comunicazione scientifica del caso clinico implica un taglio che separa "mondo" e "scena". La comunicazione scientifica si realizza dopo l'istituzione di una scena, di un'altra scena che è differente dal mondo che rappresenta. Il resoconto di un fenomeno clinico si costruisce introducendo un clivaggio tra la materia (i fatti, il mondo-della-vita) e la presentazione, l'*ornamentum* (la messa in scena del mondo). La storiografia dei casi clinici – cioè la storia e la sua scrittura – porta nella sua stessa definizione il paradosso della messa in relazione di due termini antinomici: la realtà e il discorso.

Il resoconto clinico non coincide con i fatti della seduta, la selezione dei dati empirici è già un'operazione di taglio tra la realtà e il discorso. Il discorso non è la realtà così come la fisica non è la natura. La scrittura di una storia clinica prende gli eventi e li articola e, laddove questo legame non è pensabile, cerca di ipotizzarne delle possibili forme di articolazione. Più che di una operazione di lettura o di interpretazione si tratta di un modo per produrre una relazione tra l'opacità silenziosa della realtà fenomenica e il discorso con cui la storia cerca di custodire il suo oggetto. La storia suppone e costruisce l'esistenza di un rapporto tra l'immensità sconosciuta che minaccia e seduce il sapere e il luogo dove la scrittura instaura l'intellegibilità del mondo osservato.

7. CIÒ CHE L'OSSERVATORE NON PUÒ VERIFICARE

Nell'esperienza psicoanalitica il presente e il passato si trovano sovrapposti da una trama di significanti che ripropone dei frammenti di verità che osano dirsi solo nella storia raccontata dal sintomo. Un osservatore esterno potrà riconoscere questa storia perché leggerà il sintomo come la testimonianza di una struttura soggettiva. In psicoanalisi lo studio dell'esperienza clinica implica la scansione di quegli eventi che articolano la configurazione psicopatologica e la trama narrativa[10]: la semiologia fondata sulle strutture patologiche si combina nello stesso testo con la storia della sofferenza del soggetto.

In seduta c'è però una dimensione della parola del paziente che non può essere registrata attraverso la scrittura del caso clinico, c'è un resto semantico dell'esperienza che rimane non trasmissibile e che viene vissuto solo dal paziente, infatti è inevitabilmente preclusa, all'Altro che ascolta, la possibilità di verificare il legame tra la parola e il corpo di chi si pronuncia. In un certo senso l'Altro deve e può solo fidarsi, e il soggetto non può affidarsi alla garanzia del consenso dell'Altro. Quindi la parola che l'analizzante esprime nell'esperienza psicoanalitica non può essere verificata come se fosse una formulazione scientifica sul reale, una formulazione cioè sganciata dal soggetto che la enuncia. La formulazione scientifica infatti può essere verificata solo perché c'è una distinzione tra il soggetto che enuncia una formula e il reale messo in forma: nella scienza c'è una distinzione tra il soggetto e il reale, tra la parola soggettiva e il reale. Nella scienza la parola può essere messa alla prova di un reale verificabile per tutti o almeno per tutti coloro che dispongono delle stesse possibilità di sperimentazione.

[10] Cfr. Ogden 2005.

Nella scienza la parola ha il suo referente in un oggetto che si può vedere e oggettivare, nell'esperienza psicoanalitica invece la parola trova il proprio referente in un reale che appartiene al vissuto particolare del soggetto. In questo secondo caso, il soggetto è l'unico testimone possibile del reale.

8. LA CURA PSICOANALITICA: DALLA DOMANDA DI TRATTAMENTO AL TRATTAMENTO DELLA DOMANDA

La questione che anima il percorso analitico di un soggetto riguarda il "non sapere" che cosa testimonia attraverso il suo sintomo. Il sintomo è un messaggio attraverso cui il soggetto si pronuncia, sebbene non abbia il codice per decifrare il significato di ciò che gli è più intimo. L'ascolto dell'analista mira a consentire al soggetto di riappropriarsi della sua storia e della sua verità.

In ambito psicoanalitico nei colloqui preliminari l'obiettivo verso cui si vuole condurre il paziente è il raggiungimento di un modo diverso di interrogare il sintomo e di intendere la cura. Si tratta di un passaggio che va dal lamento sul sintomo alla verità insita nel sintomo: il "sintomo-disturbo" diventa un "sintomo-questione" in cui il soggetto è coinvolto e rappresentato, sebbene non ne sappia il motivo.

La dimensione della domanda è centrale nella concezione psicoanalitica del colloquio clinico: la domanda di cura è una domanda di aiuto che richiede una sua "soggettivazione", ossia che il soggetto sofferente decida di voler conoscere la causa della propria sofferenza. La soggettivazione indica quel processo in cui la domanda di cura si trasforma in una domanda di sapere.

Se il soggetto che chiede una cura può essere diagnosticato come un nevrotico, allora il *focus* dei colloqui preliminari viene centrato sul passaggio da un primo tempo, quello della domanda spuria (ad es.: *ho un disturbo, me lo tolga*), ad un secondo tempo in cui cambia l'atteggiamento del paziente nei confronti del suo sintomo. I colloqui preliminari vengono quindi intesi come un percorso che va dalla domanda di trattamento al trattamento della domanda[11]: L'obiettivo è che la domanda iniziale del paziente si rivolga alla questione enigmatica (inconscia) che affiora tra le maglie del sintomo.

9. IL PAZIENTE È ANALIZZANTE E NON ANALIZZANDO

Il compito dei colloqui preliminari è quello di condurre il paziente verso una dimensione analizzante. Il paziente è attivamente impegnato nell'analisi e per questo viene chiamato *analizzante* e non analizzato. Si tratta di un rovesciamento semantico che implica anche un cambiamento della prospettiva epistemologica della cura: il paziente non è oggetto della cura ma soggetto della cura. Il soggetto è analizzante sia perché è attivamente coinvolto nella decifrazione del testo del colloquio sia perché nel colloquio clinico diventa centrale il sapere che l'analizzante scopre nel suo stesso dire. La disciplina di ascolto del testo dell'analizzante limita la spinta interpretativa dell'analista: il testo cosciente del paziente non viene ricondotto a significati o contenuti che appartengono ai vissuti o alle teorie di riferimento dell'operatore. Il testo dell'analizzante viene infatti valorizzato come il luogo di un senso soggettivo. Ecco perché ciò che conta è innanzitutto quello che il paziente dice.

[11] Cfr. Freda 2001, p.73.

10. LA PAROLA DEL PAZIENTE TRA ENUNCIATO ED ENUNCIAZIONE

Gli enunciati sono infatti delle espressioni linguistiche che veicolano dei contenuti (le proposizioni). La stessa proposizione può venir espressa da enunciati diversi: l'enunciato italiano "la neve è bianca" e l'enunciato inglese "snow is white" sono infatti due enunciati inequivocabilmente diversi che esprimono la stessa proposizione. Per contro, lo stesso enunciato può anche esprimere due (o più) proposizioni diverse, si possono quindi osservare degli enunciati ambigui. La distinzione tra enunciati e proposizioni si offre dunque come paradigma per la lettura del testo del colloquio: c'è infatti una non coincidenza tra il piano dell'enunciato e quello del significato che gli può essere attribuito. Il piano dell'enunciazione si definisce appunto a partire da questa distinzione: c'è una scissione tra ciò che il paziente dice e il significato che può assumere quel che dice.

Nella psicoanalisi si centra molto l'attenzione sul fatto che la dimensione della parola produce una separazione tra enunciato ed enunciazione. In ambito psicoanalitico tale discrepanza viene espressa dalla divisione interna alla stessa parola: infatti anche quando un significante trova un suo significato producendo un enunciato, la frase non riesce comunque ad assorbire il piano dell'enunciazione: «è appunto il fatto che un testo è sempre in qualche modo reticente» (Eco 2002, p. 25). I significanti – intesi come "segni linguistici", anche se non riducibili a essi – differiscono dunque dai "segnali" e non corrispondono mai in modo univoco a un significato specifico (polisemia del significante).

Nel corso delle sedute da parte del paziente si registra abitualmente la presenza di un "resto"» che rimane insaturo rispetto al potere rappresentativo della parola. In ambito lacaniano tale fenomeno viene osservato non come un deficit cognitivo ma come indice della discrepanza strutturale (e non accidentale) tra significante e significato.

Il livello dell'enunciazione compare appunto nel margine di non coincidenza tra significante e significato, la sua manifestazione dipende dal fatto che sebbene il significato sia effetto dell'articolazione significante, non cessa di sottrarsi alla presa del significante: c'è sempre uno slittamento del senso che consente a ogni enunciato di caricarsi di una "significazione" peculiare. Possiamo così osservare la "significazione" particolare che ricevono certe frasi o eventi relazionali. Lo stesso evento, infatti, può avere effetti e risonanze opposte in soggetti diversi. Oltre ai detti, osserviamo dunque il "dire" del soggetto, l'"enunciazione" a cui rimandano i suoi "enunciati".

È importante sottolineare che il piano dell'enunciazione sovverte la padronanza dell'io cosciente, il soggetto dell'enunciazione non è il soggetto (classico) della conoscenza. Il livello dell'enunciazione rimanda ad un io (*je*) che non coincide con l'io narrante (*moi*). Ad esempio chi è il soggetto del lapsus? Ciò che il paziente dice e ciò che può comprendere di quel che dice non sono sullo stesso piano: la parola del soggetto dice sempre di più di quanto il soggetto possa comprendere. Il piano dell'enunciazione mostra un soggetto che non sa ciò che vuole dire, c'è un'intenzione a dire (enunciazione) che si situa al di là del campo di giurisdizione del soggetto padrone del senso. La psicoanalisi ritiene che l'enunciazione sia l'indice di una divisione soggettiva che separa il sapere che un soggetto ha su di sé dalla sua verità. Il piano dell'enunciazione si configura infatti come l'indicatore semantico della presenza dell'inconscio nelle parole del paziente e durante una cura bisognerà "saper leggere" nella sequenza degli enunciati la trama significante che sovradetermina le manifestazioni dell'inconscio.

11. IL RISCHIO ERMENEUTICO E LA CENTRALITÀ DEL TESTO DEL PAZIENTE

La lettura degli enunciati del soggetto è un aspetto fondamentale per la pratica interpretativa in psicoanalisi. Se consideriamo il funzionamento della catena significante l'interpretazione è aperta a tutti i sensi, il senso soggettivo delle parole del paziente è suscettibile a una continua indeterminazione, dal momento che, in quanto effetto della catena significante, è sempre predisposto al rimando verso un altro significante. Un sogno ad esempio può essere analizzato in modo interminabile, con l'aggiunta di un significante dopo l'altro, nell'oscillazione tra le diverse significazioni possibili. Ciò però potrebbe condurre a un'infinitizzazione dei significati attribuibili ad ogni singolo sintomo e al testo del colloquio nel suo complesso: il testo di un paziente rimanda ad un secondo testo, non evidente ma sottostante, fino ad una proliferazione potenzialmente infinita di ulteriori amplificazioni di senso.

L'ermeneuticità intrinsenca alla psicoanalisi può inoltre ridursi alla concezione ingenua secondo cui il testo dell'Io cosciente non esaurisce mai il senso dell'esperienza soggettiva, per cui il testo del paziente ha sempre un lato sottostante che può chiarire l'apparenza del testo manifesto. In tal modo la seduta analitica può assomigliare alla codifica del testo in un secondo testo che rivelerebbe il senso autentico del primo.

La distinzione tra manifesto e latente può altresì entrare in gioco nell'uso illativo dell'interpretazione: il testo del paziente può venir tradotto dal sapere dell'operatore in un altro testo. Le parole del paziente, così come i suoi sintomi, vengono ascoltate a partire dal sapere dell'operatore: si può verificare una sorta di traduzione da un codice all'altro, così come può avvenire nel caso della diagnosi descrittiva dove alla fine il testo del paziente può ridursi a mero oggetto dell'indagine del sapere "psi".

Le illazioni semantiche possono inoltre verificarsi con un uso indiscriminato del controtransfert, dove la codifica del testo del paziente avviene in base ai pensieri e ai vissuti che appartengono esclusivamente al campo inconscio del terapeuta. È per tal motivo che la lettura del colloquio clinico deve essere intesa piuttosto come

> la pratica di un testo significante […]. Non si tratta, cioè, di concepire l'attività dell'analista come un'attività di smascheramento, perché la verità non abita dietro le parole ma si manifesta attraverso le parole. La lettera manifesta, e non nasconde, l'enunciazione del soggetto. (Recalcati 2001, p. 17)

Gli elementi da individuare nel testo del colloquio sono già nelle parole del paziente, il testo del colloquio rivela infatti una sua organizzazione che mostra la funzione dei singoli elementi. In ambito psicoanalitico lacaniano l'inconscio strutturato come un linguaggio non è un contenuto sottostante il discorso cosciente, ma piuttosto coincide con la trama del testo, con le sue incrinature e i suoi inciampi.

La psicoanalisi è dunque un'esperienza dove vengono mobilizzati i significanti e dove si producono degli effetti di significazione che ricevono un ascolto il cui obiettivo è di isolare "i significanti *maître*" che hanno tracciato la figura del destino del soggetto:

> di fronte a una serie significante prodotta dal paziente, l'intervento dell'analista offre come risposta un'interpunzione che mette il soggetto in condizione di cogliere ciò che dice effettivamente. In questo senso la risposta dell'analista è, innanzitutto, quella di fare ritornare al soggetto il suo messaggio in forma invertita. È un modo per tradurre l'immagine freudiana dell'analista come specchio opaco, la cui funzione non è aggiungere significanti assenti nel discorso del paziente, ma permettere al paziente di cogliere le significazioni che si sono prodotte dalla messa in serie di certi significanti. (Recalcati 2001, p. 21)

12. CONCLUSIONI

In questo lavoro è stato presentato un percorso argomentativo volto a chiarire i concetti che caratterizzano la prospettiva epistemologica della psicoanalisi. Il rapporto tra *dimensione clinica e ricerca* è di cruciale importanza per comprendere in che modo sia possibile studiare e verificare l'efficacia terapeutica della psicoanalisi. L'accento posto sul tema della verifica empirica in psicoanalisi ha permesso di evidenziare quanto la clinica conservi la possibilità di attingere a quel campo di fenomeni ("contesto della scoperta") che la ricerca di laboratorio sembra tagliare fuori nel momento stesso in cui si costituisce come "contesto della verifica". È in tal senso che si può assumere un atteggiamento critico nei confronti del termine *sperimentale*, mantenendo invece ben chiara la necessità di un ancoraggio *empirico* per ogni riflessione scientifica sulla dimensione clinica. L'esperienza clinica fornisce infatti una base empirica che si sottrae alla replicabilità sperimentale. Gli eventi di una psicoterapia non riescono a soddisfare le richieste di controllabilità delle metodiche di ricerca delle cosiddette scienze "dure".

Il confronto tra *psicoanalisi e scienza* ha permesso inoltre di approfondire il rapporto tra teoria e prassi: l'applicazione clinica del modello teorico richiede un uso delle conoscenze che chiama in causa l'inferenza abduttiva: il passaggio "dalla conoscenza al suo uso nella clinica" implica infatti un'interpretazione (un'inferenza) che viene rivolta a un messaggio non codificato. Il clinico compie delle scelte sullo sfondo di un'assenza di garanzia, di una garanzia in grado di certificare in anticipo l'efficacia e la pertinenza di una diagnosi, di un'interpretazione o di un atto analitico. In altri termini, non c'è un modello per applicare i modelli e dunque nessun modello può determinare quali siano le sue applicazioni corrette.

Le argomentazioni successive hanno sottolineato quanto la questione dell'applicazione del modello sia strettamente legata alla questione della *formazione dell'analista*. La formazione analitica si configura infatti come un addestramento particolare che da un lato implica l'apprendimento teorico e dall'altro richiede un percorso di soggettivazione in grado di consentire al clinico di applicare i principi psicoanalitici "caso per caso".

L'attenzione alla singolarità del *caso clinico* si traduce in un lavoro terapeutico che mira innanzitutto a ricomporre la trama storica del percorso esistentivo del paziente. L'inconscio è appunto la trama narrativa che il paziente ricostruisce di seduta in seduta e attraverso cui viene riletto il significato del sintomo. Un altro vincolo fondamentale che caratterizza la psicoanalisi riguarda la concezione del *sintomo*. Il sintomo non viene considerato come una disregolazione neurofisiologica o come un deficit da riparare, ma piuttosto come un messaggio da decifrare, come un significante di cui il paziente ignora il significato. È stata inoltre approfondita la concezione del *linguaggio in psicoanalisi*. In particolare, sono state evidenziate le ragioni per cui *l'ascolto clinico* debba essere rivolto alla connessione dei significanti: in tal modo sarà possibile favorire l'elaborazione del paziente sul significato che insiste nella ripetizione sintomatica.

Sin dalla prima fase della cura il paziente viene orientato verso l'assunzione di una responsabilità soggettiva nella causa della sofferenza del sintomo. Il fatto che la psicoanalisi sottolinei la valenza etica della sofferenza mentale distingue in modo netto la cornice epistemologica dello studio del caso clinico. È proprio per tal motivo che nell'epigrafe di questo lavoro veniva riportata una frase di Miller a proposito del legame tra clinica ed etica, un legame che condiziona da un lato le scelte del clinico e dall'altro il lavoro analizzante del paziente.

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Psicologie, etnopsichiatria, sistemi di cura

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ABSTRACT

Excluding universalism posits that *any Weltanschauung* different from ours is a bulk of more or less folkloristic beliefs. However, both the epistemological adventure of Western sciences (especially life sciences and "psy" sciences) and the anthropological data collected during the 20th century converge into indicating that this kind of universalism is inadequate and untenable. Today, remaining loyal to the original purpose of science means granting equal epistemological dignity to all systems of knowledge and know-how, which sprung, like ours, from specific historical conditions. Also, it means being able to read the coherence of these systems not according to our parameters, but according to the logic and the constraints that they inherited from the cultures they belong to. According to this analysis, we can find "psychologies" in the plural form within the boundaries of Western science. Moreover, multiple mind models and therapeutic systems are present both within and outside the Western world. Recognising such a plurality does not mean "anything goes", and requires an epistemology worth the times and complexity we live in.

1. LA CRISI DELL'UNIVERSALISMO

Impresa di un determinato gruppo umano in un determinato periodo della sua storia, la scienza – fondata, come l'intera *koiné* occidentale, sulla contrapposizione di natura e cultura – mira a descrivere la *natura*. Essa si vuole e si assume, pertanto, come strutturalmente universale, descrittiva di ciò che, del mondo, è invariante, indipendente dall'interpretazione, sganciato dai valori e dalla loro mutevolezza. Questa è ancora, in larga misura, l'immagine che l'Occidente si dà della propria forma di conoscenza: ma basta ripercorrere, anche distrattamente, le vicissitudini dell'epistemologia novecentesca per rendersi conto che si tratta al meglio di una pia illusione, al peggio di una manovra ideologica.

Niente, nella scienza, ha superato indenne la crisi dei fondamenti: non la matematica (vedi il teorema di Gödel), non la fisica (vedi la storia dello iato fra fisica quantistica e teoria della relatività, e i tentativi di ricomporlo), non la biologia (vedi le discussioni sull'evoluzione e sulla definizione stessa di *bìos*), non la medicina (vedi ad esempio i lavori di Georges Canguilhem), e neanche la psicologia. Ma mentre alcune discipline scientifiche hanno saputo mettere a frutto la crisi fin da subito, assumendo direttamente l'onere dell'incompletezza e della discontinuità, altre, più legate alle vicende alterne della società e dei valori, si sono aggrappate alla presunzione di universalità come a una carta di nobiltà – nobiltà che tuttavia, nel frattempo,

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aveva assunto in altre scienze tutt'altre forme, più interessate alle obbligazioni interne e alla produttività concettuale che non alle rivendicazioni in merito a "verità universali".[1]

Nel frattempo, ma sempre nel XX secolo, si è assistito in Occidente al passaggio da storie di gruppi umani evolventi in ambienti *marginalmente* artefatti a storie di gruppi tendenzialmente omogeneizzati dalla domesticazione ad opera di un ambiente *prevalentemente* artefatto e abitato da merci. La forma-merce non riguarda qui solo oggetti materiali prodotti tecnicamente, ma anche oggetti immateriali e ideologici, e i soggetti stessi.[2] Le linee fondamentali di questo passaggio sono già state descritte a metà Novecento: negli anni '40 e '50 Adorno, Anders e Mumford, dall'America, vedono quello che in Europa si sarebbe visto così chiaramente solo un po' più tardi; e alle diagnosi di Debord e Cesarano sulla macchina capitalista c'è ancora poco da aggiungere[3].

Ora, nel nostro secolo, ciò a cui si assiste è lo straripare incontenibile, in ogni punto del globo, della dinamica originatasi e sperimentata in Occidente. Non si tratta solo dell'imposizione di un modello economico, come nelle analisi classiche marxiane; ma dell'imposizione di una forma di umanità, di un modo particolare di stare al mondo, caratterizzato dal divenire-merce di ogni oggetto e ogni relazione. Questo processo modifica in profondità i rapporti fra umani e ambiente, quelli fra sessi e generazioni; e, ovviamente, incontra resistenze che l'ideologia occidentale ha buon gioco a descrivere come sacche di arretratezza. Ma se altrove il *way of life* occidentale si presenta come irresistibile luccicare delle merci, è all'interno dell'Occidente che i suoi effetti perversi sono più evidenti – almeno a chi li voglia vedere. In questo senso, gli sviluppi "eretici" nelle scienze sono alleati fattuali di tutte le resistenze che, in giro per il mondo, la macchina capitalista si trova a incontrare.

2. DIALOGO RADICALE

Questi fattori, tra gli altri, hanno obbligato le scienze umane applicate a una profonda revisione dei propri assunti epistemologici, e in particolare all'autocritica della loro presunzione di Verità. Se l'atteggiamento generale davanti all'impegno e al lavoro necessario al dialogo radicale con altri saperi e altre discipline è solitamente di dismissione, l'intera peripezia delle scienze umane del Novecento testimonia di una lunga e paziente ricerca, spesso delicata e stupita, presso le culture e i saper-fare altrui, orientata a comprendere la coerenza e l'intelligenza di altri modi di vita. Nella prima metà del Novecento tale indagine era declinata in termini prevalentemente linguistici: la teoria di Sapir-Whorf formalizzava l'irriducibilità delle diverse lingue e la corrispondente esistenza di altrettanti "mondi concettuali", ciascuno dotato della stessa complessità e profondità che attribuiamo al nostro. Questo relativismo valorizzante è stato poi accantonato verso metà secolo, quando l'ottimismo della volontà post-bellica proponeva – in modo benintenzionato ma un po' ingenuo – l'accesso di *tutti* a un'*unica* forma di benessere, quella dell'Occidente industrializzato.

Alla fine del Novecento, in condizioni politiche e geopolitiche nuovamente critiche, l'interesse per altri modi di esserci nel mondo si è riaperto, e dall'antropologia della salute e dall'etnopsichiatria sono arrivate nell'ambito delle discipline della psiche descrizioni accurate di altri modi di pensare salute e malattia, di altre modalità di curare. Sono state descritte altre

[1] Due autori in particolare possono essere utili per la comprensione di questi sviluppi e delle implicazioni propriamente scientifiche che essi portano: Bruno Latour (1991, 1999) e Isabelle Stengers (1994, 1997).
[2] Klossowski 1970; Tiqqun 1999.
[3] Adorno 1951; Anders 1956, 1980; Mumford 1956; Debord 1967-1988; Cesarano 1974.

nosologie, risalendo poi, a partire dai modelli di malattia, ai nessi causali e ambientali che fanno, ovunque, da sfondo. Al centro dei "sistemi di cura" (*health systems*) messi a punto dai vari gruppi, sono state per esempio identificate delle "stringhe": insiemi coesi e coerenti depositati nel cuore delle culture in seguito alla storia di specifici gruppi umani e alla loro dialettica con particolari ambienti; come quella che lega a una coerenza interna la visione del mondo, gli ideali di salute, le forme delle malattie e le modalità di cura. Sono apparse così in letteratura descrizioni accurate e profonde di altri modi di pensare "psiche" e prendere in carico i suoi disturbi.[4] Una volta lavorato, come raccomandava Devereux[5], il proprio controtransfert culturale, questi materiali rivelano al ricercatore occidentale stratificazioni sapienti e coerenti, la cui efficacia può essere misurata solo nel contesto entro cui agiscono. Le interrogazioni portate qui da altri sistemi e il lavoro critico da essi messo in moto hanno scosso in particolare la presunzione di Verità delle discipline che, per la stessa caratteristica del loro oggetto immateriale, risultano, nell'ambito della Scienza edificata sul misurabile, quantificabile e riproducibile, particolarmente esposte alla riduzione, alla vaghezza e al culture-boundage.

Coloro che hanno preferito non sprofondare nelle melme del pensiero debole, come i rappresentanti della migliore etnopsichiatria, hanno potuto così mettersi a lavorare, finalmente fuori dalla dicotomia obbligata universalismo naturalizzante/relativismo culturalizzante, a un approccio all'alterità insieme aperto e critico che d'altro canto fa capo alla tradizione del pensiero critico occidentale. È il «dialogo socratico» di Giangiorgio Pasqualotto (2005) o il «dialogo dialogico» di Pannikar (1988). Quello in cui:

> ciascuno degli interlocutori è capace di mettersi in gioco radicalmente, ossia rinunciando alla pretesa di verità delle proprie opinioni, e si rende in tal modo disposto a essere trasformato dalla pratica dialogica. Con "dialogo" intendiamo ovviamente non un semplice e superficiale confronto di opinioni, ma un incontro "radicale" dove proprio le opinioni – e l'intera gamma di preconcetti, presunzioni e presupposti di cui esse si alimentano – vengono implacabilmente messe in discussione, e, spesso, demolite. (Pasqualotto 2005)[6]

È ovvio che questo tipo di dialogo ("dialogo radicale") non si avvia nel chiuso di uno scambio ristretto nel tempo (come 45 minuti di lezione) e nello spazio (per esempio, un'aula accademica); ma passa necessariamente attraverso un fare comune, un confronto che metta in luce compatibilità e incompatibilità spesso mascherate dalla convenienza o dalla traduzione linguistica.[7] La pratica del "dialogo radicale" portato dentro le scienze della psiche obbliga a un lungo e fertile lavorìo, un andare a frugare nell'area a cavallo fra filosofia, epistemologia e sociologia per afferrare le nostre categorie profonde, quelle che ci fondano culturalmente e flettono il nostro pensiero e il nostro modo di stare nel mondo nella maniera specifica che ci caratterizza.[8] È la dinamica che raccomandava Ernesto de Martino a proposito dell'etnologia:

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[4] Vedi per esempio, Devereux 1969; Zempleni 1968; Coppo 1990.

[5] Devereux 1967.

[6] Si veda anche, sulla comparazione con la tradizione zen, Pasqualotto 1990.

[7] Come nell'esperienza di attività terapeutiche integrate, dove un rappresentante della psicologia o psichiatria visita insieme a un terapeuta di altra tradizione il paziente (Mali, Progetto Medicina Tradizionale Mali Senegal, www.medtrad.oriss.org).

[8] In particolare, valgono qui i riferimenti a Descola (2005), monumentale analisi delle diverse maniere culturali di ripartire il mondo, che evidenzia la specificità di quella, per noi solitamente indiscutibile, che separa natura e cultura; agli scavi concettuali e al "lavoro di specchio" di specialisti dotti di altre culture (vedi ad esempio l'opera di François Jullien (1989, 1990) sul diverso modo d'intendere il movimento che soggiace al divenire fra oriente e occidente); e ai lavori di Foucault, e poi dei foucaultiani, sulla storicità della verità e dei modi della soggettivazione (Butler 1997; Foucault 2001, 2009).

> L'etnologo è chiamato […] a esercitare una epoché etnografica che consiste nell'inaugurare, sotto lo stimolo di determinati comportamenti culturali alieni, un confronto sistematico ed esplicito tra la storia di cui questi documenti sono documento e la storia culturale occidentale che è sedimentata nelle categorie dell'etnografo impiegato per osservarli, descriverli e interpretarli: questa duplice tematizzazione della storia propria e della storia aliena è condotta nel proposito di raggiungere quel fondo universalmente umano in cui il "proprio" e l'"alieno" sono sorpresi come due possibilità storiche di essere uomo, quel fondo, dunque, a partire dal quale anche "noi" avremmo potuto imboccare la strada che conduce all'umanità aliena che ci sta davanti nello scandalo iniziale dell'incontro etnografico. In questo senso l'incontro etnografico costituisce l'occasione per il più radicale esame di coscienza che sia possibile all'uomo occidentale. (De Martino 1977, p. 391)

Nelle discipline della psiche il lavoro critico, più sotterraneo e, per così dire, meno spettacolare rispetto a quello delle scienze fisiche, giunge solo oggi a piena maturazione – ma i suoi frutti tardivi potrebbero essere preziosi nell'inverno che si prefigura. Alcuni fattori generali hanno incrinato anche qui, tra '900 e 2000, la presunzione universalistica. Da un lato, c'è stato un lavoro critico condotto da specialisti all'interno delle diverse discipline della psiche, lavoro sostenuto e alimentato dall'esterno dalla "gente comune" sotto attacco nelle loro esistenze da parte della razionalità economica, del razionalismo scientista, del dominio delle protesi tecnologiche e delle loro applicazioni "terapeutiche" al "dolore morale" e alla "sofferenza mentale". Dall'altro, la cascata di fenomeni relativi alla cosiddetta "globalizzazione" ha permesso l'incrocio strutturale, ineludibile, con altri e diversi sistemi culturali, largamente irriducibili al nostro. Tale incrocio è stato spesso tragico, funzionale all'estensione del dominio delle nazioni occidentali sul pianeta; ma anche, ha fatto sì che portatori di modi differenti di stare al mondo incontrassero coloro che, all'interno dell'Occidente stesso, rappresentano istanze differenti da quelle dominanti. Le voci "altre" rispetto all'Occidente portano con loro modi e potenze specifiche che non è più possibile, oggi, sopprimere o ignorare: non per un astratto rispetto del "diverso" o adesione a un insulso relativismo culturale, ma perché hanno conquistato, anche sul terreno delle discipline, il diritto a farsi ascoltare e perché solo l'incrocio delle pluralità serba in sé la possibilità non utopica di «un altro mondo possibile» (Pignarre & Stengers 2005).

3. PSICOLOGIA O PSICOLOGIE?

Per saggiare la portata di questa domanda, cominciamo con una questione interna, di epistemologia in senso classico. Nell'area delle scienze umane applicate sono presenti non una, ma molte psicologie, ciascuna intesa come un coerente apparato teorico e operativo: dal cognitivismo alla psicanalisi, dalla psicologia della Gestalt a quella umanistica, dalla bioenergetica al comportamentismo. Questa molteplicità si può già configurare come un primo puzzle composito di letture diverse relative a parti diverse di un medesimo oggetto. Tendenzialmente, l'insieme delle letture potrebbe andare a ricostituire l'unità dell'oggetto – non fosse che, in questo caso, i diversi approcci utilizzano tutti un medesimo background metodologico, un medesimo linguaggio di base comune. Esse trovano la loro sintesi, la loro cifra, nella genealogia delle ideologie (nelle teorie) degli osservatori, varianti di un'identica modalità conoscitiva: quella, in senso ampio, dell'Occidente scientifico. Per questa ragione, esse riescono a originare solo delle rappresentazioni di paesaggi diversi, tutti visti, però, attraverso la medesima lente. In ogni caso, questa pluralità – pur figlia di un unico padre – apre già a un pluralismo antagonista all'universalismo precedente, aperto alla negoziazione fra

sistemi e alla loro compresenza, e tendente, in definitiva, a demolire la naturalizzazione dell'oggetto "psiche" e la sua ontologizzazione.

Questa compresenza di diversi sguardi impone, già all'interno della scienza, di trovare una definizione del termine "psiche" che sia in grado di comprendere il processo storico che lo ha prodotto, la lunghissima evoluzione culturale che fa sì che qui, da noi, si ragioni in termini di natura vs. cultura, di psiche vs. soma. Una volta comprese le ragioni storiche di questo peculiare *modus pensandi*, si tratta di fare una scelta – scelta che solo per paura o per interesse può essere considerata come extrascientifica, giacché mette a tema gli assunti stessi, fondativi, dell'impresa conoscitiva nata in Occidente e nota come "scienza". La scelta si compie fra una posizione *esclusiva*, che accetta una sola definizione di ciascun ente, ovvero quella propria al mondo della scienza cosmopolita e universalista; e una posizione *implicativa*, che consente di mettere sullo stesso piano di dignità epistemologica le diverse psicologie ma anche gli altri sistemi concettuali e operativi. La prima scelta, nel suo universalismo rivendicato, prospetta l'orizzonte della guerra o, quantomeno, dell'*Anschluss*: si tratterà quindi di cooptare i poverelli che ancora credono agli spiriti a una visione finalmente moderna che, con grande disincanto, crede all'inconscio e all'Edipo. La seconda scelta, invece, fedele alla "psiche" come dimensione immateriale degli umani, apre alla possibilità del conflitto e della negoziazione, ovvero alla possibilità del dialogo radicale, e coinvolge il ricercatore, lo *implica* – sia come rappresentante di una cultura che come soggetto – nel processo di trasformazione.

Sia detto ancora una volta, e a chiare lettere, a scanso di equivoci: accettare i sistemi degli altri non significa, come nel relativismo da salotto, smettere di farsi e fare domande e lasciare che il mondo vada a rotoli come meglio crede. Al contrario, il *dialogo radicale* significa andare a vedere come funzionano i sistemi altrui, in quali circostanze, attraverso quali dispositivi, da quale visione dell'uomo dipendono e quale contribuiscono a costruire, quali zone esplorano e con quali esiti.

L'etnopsichiatria ha preso questa seconda strada, e si trova oggi a operare in una zona dove la semplice concettualizzazione della *psiche* è problematica. In molte culture non esiste traduzione possibile; a volte, l'area semantica di "psiche" può essere tradotta attraverso il ricorso a una molteplicità di vocaboli; altre volte, rispetto a diverse concettualizzazioni, "psiche" risulta vocabolo troppo riduttivo. Lo sforzo di traduzione, tuttavia, non conduce a un *non sequitur* e permette, semmai, un guadagno di conoscenza, obbligando a scavare tanto le altrui quanto le nostre categorie. In generale, dunque, l'etnopsichiatria utilizza oggi la parola "psiche" dandole un senso più vicino a quello preomerico (dove è soffio vitale, respiro che accompagna e sostiene il vivente)[9] che all'"anima" indagata della teologia; e indica con essa l'intero spettro semantico che si riferisce alle componenti immateriali e invisibili delle persone.

Alla moltiplicazione delle "psicologie" si è quindi aggiunta, negli ultimi decenni, l'emersione di sistemi altri di concezione del lato immateriale degli umani e di lavoro su di esso, provenienti da aree esterne alla Scienza e all'Occidente. Questi sistemi si nominano e delimitano in modo differente rispetto ai nostri, e talvolta non si nominano affatto, poiché non si delimitano poiché non si delimitano come saper-fare separati. [10]

[9] Per il percorso che dalla *psyché* omerica ha portato alla psiche della psicologia e psichiatria, si veda per esempio Vernant 1965; Vegetti 1985; Coppo 1998.

[10] Vediamo un esempio. Altrove le pratiche che noi concettualizziamo come "medicina" somigliano a volte a un misto fra divinazione, terapia e politica sociale. Per questo campo semantico non disponiamo di nessuna parola univoca e continuiamo, *faute de mieux*, a usare il vocabolo "medicina", che tuttavia rischia di oscurare definitivamente quello che già nell'osservazione risultava poco chiaro. Singleton (2007) fa notare, ad esempio, che in alcune aree culturali africane ciò che noi chiamiamo "guaritore" sarebbe meglio definito come "chiaroveggente", nel senso etimologico di "colui che deve vederci

L'insieme di tutte queste considerazioni fa cenno a una teoria dell'umano che è bene, a questo punto, rendere esplicita, e che sta a fondamento di tutto quanto segue. Essa fa capo all'antropologia filosofica della prima metà del Novecento, ma i temi che connette attraversano, sotto diverse forme, tutto lo sviluppo del pensiero occidentale. In sintesi estrema, la tesi di fondo può essere espressa così: *umani si diventa*. È la questione, scottante, della natura umana, di ciò che fa di un esemplare di *Homo sapiens* un individuo umano.

4. DIVENTARE UMANI

Secondo alcuni (e in particolare, secondo un certo modello scientifico) la configurazione della specie, la biologia umana, sarebbe sostanzialmente universale per forme e contenuti, necessaria e sufficiente alla produzione di un umano adulto dotato di uso del linguaggio, razionalità, strutturazione emotiva etc. La cultura, in questo scenario, non farebbe altro che colorare diversamente un medesimo quadro, e le medesime forme che s'incontrano in un posto dovranno necessariamente ritrovarsi anche da ogni altra parte.[11] In quest'ipotesi è possibile immaginare una sorta di *adulto assoluto*, esito del miglior processo di crescita immaginabile: quello che produce il minor numero di inflessioni culturali e che lascia emergere al massimo grado le caratteristiche naturali degli esseri umani (nell'ipotesi ottimista, *à la* Rousseau; oppure, nell'ipotesi pessimista hobbesiana o freudiana, quello che più abilmente e con minore sofferenza tiene sotto controllo le pulsioni naturali).

I dati dell'antropologia biologica e di quella culturale, tuttavia, indicano altro e l'ipotesi che qui si sostiene è che essere umani, far parte dell'umanità, non significa solo essere membro della specie *Homo sapiens*. Non c'è continuità fra l'appartenenza biologica alla specie e ciò che consideriamo come genericamente umano: per arrivare a essere umani bisogna attraversare un lungo processo di umanizzazione. Fatte salve alcune caratteristiche fin troppo banali relative alla forma e al funzionamento generico degli umani, forse il più importante universale biologico della specie è una *naturale insufficienza* del corredo bio-genetico a produrre un umano adulto: ciò che si può considerare universale a livello di specie *non basta* a produrre un umano adulto. C'è uno scarto fra la nostra biologia e il nostro essere umani, come se la forma biologica di *Homo sapiens* comportasse una paradossale sospensione delle stesse determinazioni biologiche; un'apertura al mondo che richiede, per poter proseguire col processo di individuazione, l'immersione del vivente biologico in una cultura.[12] Tale apertura è stata interpretata per lungo tempo come deficit (l'uomo animale mancato, bipede implume etc.), i cui vuoti sarebbero colmati per via culturale. Recentemente, sono state proprio le ricerche in campo neurologico a permettere di superare il "modello deficitario" e di elaborare un modello, più consono ai dati e alle osservazioni provenienti dalle scienze umane, di apertura e di permanente incompletezza, in cui la cultura, lungi dal riempire, sfronda una primitiva sovrabbondanza, plasma per potatura anziché per aggiunta.[13]

Nell'apertura biologica che ci caratterizza a livello di specie, è la dimensione collettiva a fare di noi degli individui attraverso un lungo lavoro che non può non esserci, pena la non

chiaro". Il suo compito non è quello di curare una malattia, ma di rimettere ordine nel disordine del mondo, discernendone le cause.

[11] Vedi ad esempio, in ambito psicologico, tutta la discussione sulla presunta universalità del complesso di Edipo, e le rielaborazioni che essa ha reso possibili.

[12] Gehlen 1978; Virno 2003.

[13] Remotti 2005; Favole & Allovio 2005.

sopravvivenza del piccolo o quantomeno il suo sviluppo dimidiato.[14] In termini di antropologia filosofica, il neonato umano arriva sulla terra in una situazione di totale inettitudine e quindi in condizioni ontologiche di pericolo estremo: se non viene immediatamente accolto da una comunità, non solo non sviluppa le facoltà proprie dell'umanità ma, più radicalmente, non sopravvive.[15] La necessità ineludibile dell'antropopoiesi è uno dei pochissimi dati davvero universali ricavabili dall'antropologia: i diversi modi dell'umanità non sono accomunati da nessun particolare contenuto culturale, da nessuna natura umana specifica che ne predetermini le forme, da nessuna costante culturale; da niente che non sia la necessità di diventare umani lungo forme storiche specifiche. Questo significa che l'"uomo di natura", l'"uomo in generale", il mero esito del programma biologico, *non esiste*; che per produrre un adulto è strettamente necessario che un piccolo di *Homo sapiens* venga accolto e plasmato da una cultura, che l'aperto che lo caratterizza trovi confini, forma, contenimento. In questa ipotesi, le diverse culture non sono rivestimenti estrinseci di una medesima natura, ma modi differenti di plasmare un vivente altamente potenziale, declinazioni diverse di una medesima *apertura* esistenziale, di una biologia che resta, per così dire, in sospeso e che, in quanto tale, espone continuamente al rischio.[16]

Il lavoro primario di ciascuna cultura è dunque quello di garantire la presenza degli individui che ne fanno parte mettendo in forma le potenzialità che il nostro programma biologico lascia aperte. Ciò è fatto costruendo dei ripari (i "legami fondanti" di Bruno Latour o la "continuità culturale" di Chandler e Lalonde[17]) che permettono ai soggetti di non sperimentare più come angosciose o paralizzanti situazioni che potrebbero altrimenti essere mortali. Si tratta quindi di qualcosa ben diverso da una coloritura aggiunta sopra una natura materiale vera e immodificabile: nel processo che ogni cultura mette in atto per umanizzare i propri membri ne va non solo delle idee e delle credenze, ma degli stessi parametri biologici. Non solo le teste vengono plasmate, la cultura entra nei corpi: modifica le reazioni fisiologiche; struttura i sentimenti; abitua a un regime particolare rendendo impervi tutti gli altri regimi possibili; piega a determinati lavori e a determinati sforzi; determina le vie e i modi del dolore e del piacere; attiva alcune piste lasciandone silenti altre.

In questo quadro, in cui neppure la biologia resiste immutata al lavoro di plasmazione, è evidente che le forme della psiche non possono, neanche per ipotesi, essere considerate come universali. Ogni cultura produce umani che le sono compatibili secondo linee specifiche che possono essere integralmente note solo a chi le condivide, le esplora, lavora alla loro permanenza o al loro cambiamento. Per questo, come propone l'etnopsichiatria a cui qui si fa riferimento, i terapeuti locali, iniziati alla cultura del gruppo in cui operano, devono essere considerati dai professionisti della psiche come altrettanti colleghi, esperti di un modo specifico di costruire l'umano, alleati nell'impresa rischiosa di farsi carico di una sofferenza che, sa ha una comune radice filo-ontogenetica, si declina poi anche in una miriade di rami ontogenetico-storici, ciascuno col suo orientamento, le sue fragilità, le sue curvature.

[14] Notiamo, di passaggio, che questa caratteristica non è esclusiva della specie *Homo sapiens*, che la condivide con le specie facenti parte dei cosiddetti "mammiferi superiori" (primati, elefanti, mammiferi marini etc.): tutte queste forme viventi hanno bisogno di accudire i piccoli per un periodo più o meno prolungato, di farne degli adulti tramite un processo di acculturazione. Ciò che è eccezionale nella nostra specie è la durata delle cure parentali, la loro intensità, il grado della loro necessità per lo sviluppo del piccolo e la notevole "prematurità ontogenetica" dei bambini alla nascita.

[15] Impressionanti, a questo proposito, i dati sulla mortalità dei neonati istituzionalizzati e sulla sindrome del nanismo da deprivazione affettiva (Gardner 1972).

[16] De Martino 1977.

[17] Chandler & Lalonde 1998.

Per capire gli "altri", de Martino (vedi sopra) consigliava di risalire al fondo in cui noi e gli altri fossimo «sorpresi come due possibilità storiche di essere uomo». Piuttosto che verso una sbrigativa omologazione/ibridazione, con perdita di diversità culturale e quindi anche di possibilità di conoscenza e intervento, le alterità, le impossibilità di traduzione immediata e le diversità delle pratiche preventive e terapeutiche dovrebbero essere rispettate, osservate e studiate nella loro coerenza interna, "emica". C'è una dialettica sempre complessa, spesso raffinatissima, che lega le "stringhe" concettuali, le storie e gli ambienti. Saperi e saper-fare si depositano ovunque, e ovunque sono oggetto di lavoro continuo, di rimaneggiamento, di negoziazioni. Nessuno sta fuori dalla storia – e non perché l'espansione dell'Occidente abbia infine tirato dentro al tempo storico anche i più recalcitranti, ma perché l'essere stesso degli individui umani è storico: esposto a, e plasmato da, *un* tempo, *un* luogo, *un* modo di stare al mondo.

Le "psicologie" che si riferiscono alla Scienza e quelle che si riferiscono a saperi e saper-fare altri dovrebbero pertanto essere considerate allo stesso piano di dignità epistemologica e messe nello stesso modo alla prova. La prova stessa, poi, non dovrebbe consistere nello squalificare gli altri saperi, ma nella capacità di rispettare i vincoli che ciascun sapere, autonomamente, si è dato[18]: non si possono imporre ad altri le proprie scelte e i propri vincoli più di quanto si possano imporre loro i nostri antenati.[19] E richiedere ad altri le stesse fedeltà a cui abbiamo scelto di soggiacere noi equivale, a livello logico, a chiedere a tutti indistintamente di essere fedele al proprio coniuge, quello che *noi* abbiamo sposato.

5. PROSPETTIVE

Una simile intenzione e un simile lavoro potrebbero, in prospettiva, produrre qualcosa di diverso da ibridi o *patchwork*, ed essere ben altrimenti efficaci. Nei *patchwork* in cui a volte si risolvono i tentativi interdisciplinari e interculturali, gli elementi giustapposti, malamente cuciti fra loro, sono assai spesso frutto di sbrigative traduzioni/tradimenti: la nostra medicina accanto alla medicina degli Yanomamo, la nostra psicologia accanto alla psicologia dei Bambara, e così via; ma, come abbiamo visto, le nostre categorie *etiche* (di osservatori esterni a una cultura) non corrispondono affatto a quelle *emiche* (di coloro che tale cultura rappresentano e fanno). Bisogna allora tentare per davvero il salto raccomandato da De Martino e riconoscere infine che l'insieme delle culture è un repertorio di specifiche possibilità originali, generate in epoche, ambienti e storie specifici; e che ciascuna di esse ha in comune con le altre la tensione verso la coerenza interna, e la necessità e l'intenzione, genericamente umane, dalla quale è nata.

Tutte le culture hanno bisogno di "mettere in forma" i propri membri tramite il processo di antropopoiesi; tutte hanno bisogno di gestire il disordine (disordine mentale, sociale, dei cicli naturali, dei cicli di vita etc.) e di ristabilire l'ordine; tutte sono sempre, inevitabilmente,

[18] Stengers 1997.

[19] Un esempio, nato nell'ambiente del *Centre Devereux* di Parigi. Catherine Grandsard ha studiato le logiche di appartenenza di ebrei e cristiani, per comprendere i problemi specifici dei discendenti di matrimoni misti. In sintesi estrema: mentre si è ebrei per nascita (per biologia, si potrebbe dire anche), e la cosa non ha granché a che fare con la fede, si è invece cristiani solo per scelta: una scelta che dev'essere riconfermata di volta in volta e che è sempre in questione. Detto altrimenti, mentre si può scegliere di non essere più cristiani, scegliere di non essere più ebrei equivarrebbe, nella logica ebraica, a scegliere di non conoscere più la propria lingua madre. La cosa più strana, per l'Occidente cristiano, è rendersi conto che, di tutte, è la nostra logica quella strana, minoritaria: la gran parte degli umani adotta infatti una logica di appartenenza analoga a quella ebraica.

esposte alla trasformazione e questa può essere, come la storia dimostra ampiamente, tanto evolutiva quanto involutiva.[20] In queste necessità generali e genericamente umane sta l'unico universalismo possibile; *come* ciascuna cultura faccia fronte a questi compiti, poi, dipende dalla sua storia, dalle condizioni in cui si è sviluppata, dalle piste che ha scelto di esplorare e da quelle che sono rimaste in sordina.

Non c'è posto, in questo quadro, per scomuniche legate a presupposti monoteistici (ovvero, all'idea secondo cui la nostra cultura è la sola legittima e desiderabile), ma solo per vincoli etici. Riconoscere le necessità "genericamente umane" alla base di tutte le culture permette di capire l'intelligenza delle teorie e dei dispositivi specifici; e questa comprensione, a sua volta, permette di sostenere e dare forza ai processi trasformativi delle teorie e dei dispositivi stessi, rendendoli sensibili ai processi in corso, ai nuovi esistenti in trasformazione e alle possibilità reali – conflittuali anziché guerresche – che si aprono.

Si tratta quindi di favorire e accelerare il passaggio da una globalizzazione omologante e riduttiva, prodotta dalla logica delle cose, a una pluralità setacciata, diversificante e generativa, esito del lavoro e dell'intenzione umana.

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[20] Sironi (1999) nota, ad esempio, che la diffusione della tortura è connessa, almeno nell'ultimo secolo, al disfarsi dei legami sociali e all'instabilità delle forme di vita associativa (o anche, usando il vocabolario demartiniano, all'infragilirsi della presenza nel mondo degli individui).

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Book Review

Unity in Psychology: Possibility or Pipedream?

Edited by Robert J. Sternberg
American Psychological Association, Washington DC, 2005

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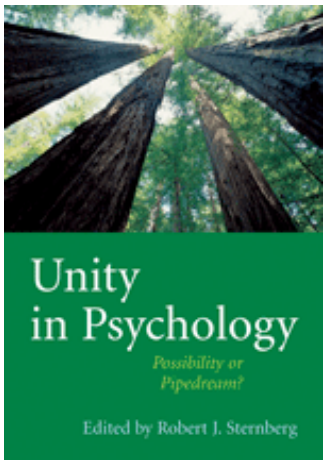

Il tema trattato nel volume edito dalla *American Psychological Association* risulta assolutamente concreto ed attuale, non solo per la psicologia americana ma anche per quella di altri paesi, anche se in dimensioni e dinamiche diverse. L'importanza dell'opera è poi riconducibile all'ampiezza degli aspetti trattati da un nutrito numero di autori che vengono qui di seguito presentati.

Nel breve spazio di una recensione non è possibile sintetizzare ogni specifico contributo. Risulta comunque indicativa l'analisi svolta dal curatore e dagli autori dei capitoli quarto e nono.

Nel contributo *Unifying the Field of Psychology*, il curatore Robert J. Sternberg, segue in modo appassionato l'ipotesi della "unità" della psicologia. Egli analizza i motivi di preoccupazione per una eccessiva frammentazione della disciplina e delinea un percorso di pensiero che sarà approfondito e sviluppato dai vari coautori del volume. Sternberg giustamente rileva come lo sviluppo della psicologia nel ventesimo secolo abbia seguito una specifica strada che è stata quella della frammentazione in aree di interessi diversi, fino a raggiungere oggi una divisione in molti sotto settori disciplinari. Sternberg ricorda come esempi di questi la psicologia cognitivista, quella dello sviluppo, quella biologica. Anche se può apparire che la distinzione in settori diversi – oltre ad essere il risultato di uno sviluppo storico – sia anche una necessità intrinseca legata agli oggetti di studio, il curatore rileva in questo problemi e pericoli legati appunto alla attuale struttura "per scuole" della psicologia. Emerge così una ipotesi di unificazione, di 'unità' della psicologia. Egli fa un esplicito riferimento a quella visione integrata, unitaria che fu propria di William James, unanimemente ritenuto uno dei fondatori della psicologia americana.

> William James era uno psicologo che adottava un'ottica unitaria. La cosa triste è che la Psicologia ha mostrato una grande lentezza ad adottare tale idea e si è mossa nella direzione della frammentazione piuttosto che dell'unità. (p. 13)

Un primo motivo di preoccupazione è rintracciabile in una dinamica di "svalutazione": percorsi operativi e formativi di estrema specializzazione portano a svalutare approcci diversi. Un altro fattore di crisi è indicato come "non conoscenza": l'impegno cogente di studi all'interno di un campo specialistico porta una "ignoranza" nei confronti di altri approcci. Infine, la settorializzazione porta con sé un alto grado di competitività tra approcci diversi e questo si risolve nell'accettare solo la propria posizione, rifiutando altre idee.

Questi aspetti critici hanno determinato effetti quali lotte interne e divisioni che hanno ridotto la credibilità verso l'esterno, verso la società. Così, Sternberg non solo dichiara la sua adesione ad una idea di unificazione della psicologia ma lancia questa proposta alla comunità scientifica.

I motivi per un processo unificante sono riconducibili a due macro-fattori. Un primo fattore rimanda alle prassi di ricerca: aprire alle conoscenze di altre "scuole", a percorsi di ricerca diversi da quelli della propria appartenenza scientifica, può permettere di "fare le giuste domande", di usare impostazioni nuove che possono generare ulteriori progressi. Il riconoscere un grado di interdipendenza fra scuole di pensiero può permettere di "lavorare insieme", di costruire percorsi interattivi, di trarre "profitto l'un l'altro". Un secondo fattore è quello che riguarda gli aspetti gestionali e di immagine. Il poter produrre progetti interrelati permetterebbe la "conservazione delle risorse", mentre, presentare la psicologia come un corpus unitario, permetterebbe di recuperare e potenziare gli aspetti di "credibilità" nei confronti della società e degli utenti. Per Sternberg la "cura" per il problema della segmentazione eccessiva nei metodi e nei percorsi di ricerca consiste nel delineare un approccio che accetti di studiare i fenomeni psicologici veramente secondo prospettive diverse.

Il contributo dell'*American Psychological Association* (APA) nel volume risulta particolarmente significativo in quanto essa è la più importante associazione di psicologi negli Stati Uniti. La distribuzione professionale degli psicologi risulta così articolata: il 25% è inserito nel settore accademico, il 60% nell'area dei servizi e la restante percentuale in settori vari del governo e degli affari. L'associazione quindi contiene al suo interno un gran numero di professionalità che coprono molte decine di aree diverse di intervento come quelle relative al lavoro, alla salute, all'educazione. L'APA può così contribuire, sia nella ricerca di base che in quella applicata, a definire linee guida a livello nazionale ed anche, con il prestigio raggiunto e consolidato, a livello internazionale. Risulta chiaro che una associazione così strutturata può promuovere anche la prospettiva dell'unità nella psicologia, offrendo la possibilità di un confronto utile per integrare conoscenze e pratiche attualmente ben differenziate. Questa funzione unificatrice è sempre stata svolta dall'APA fino dalla sua fondazione, anche se la psicologia in America è nata non come corpo di un sapere unitario ma attraverso gli apporti di ricercatori provenienti da settori di studi diversi.

In sintesi gli autori affermano che attraverso l'APA risulta effettivamente possibile sviluppare nuovi tipi di collaborazione interdisciplinare che permettono alle specializzazioni in aree ristrette di interagire e integrare le proprie competenze.

Nell'insieme delle riflessioni sul tema dei possibili percorsi per costruire una significativa unità nella psicologia, quella svolta da F. Rychlak affronta il problema in relazione alla struttura scientifica in termini di Teoria e Metodo. L'autore ricorda come il tema dell'unificazione sia già presente all'inizio della psicologia moderna, dove lo sviluppo degli studi si è caratterizzato verso una differenziazione sia nel campo della teoria che del metodo. Strade diverse per i paradigmi teorici: etichette concettuali o basate sulla percezione della scelta – i paradigmi di tipo realistico – o costruite su assunti di idee – paradigma idealistico.

L'autore ricorda che anche a livello di metodo i percorsi sono differenziati. In relazione al tema di base che riguarda la verità e la falsità di una teoria, i percorsi metodologici non sono unitari. Esistono due grandi strutture concettuali: il percorso cognitivo e il percorso di ricerca di prove oggettive. Il percorso "cognitivo" mette l'accento su aspetti procedurali che permettono di rendere evidenti le esperienze all'interno della richiesta teorica. L'autore usa come esemplificazione l'insieme di istruzioni che uno skipper può dare per condurre una barca. Il percorso della "prova" richiede invece una ricerca sperimentale, e quindi di una osservazione realizzata in modo affidabile, per individuare la "prova validante" che permetta di rendere evidente e vera una teoria.

Percorsi scientifici diversi, mentalità di fondo diverse, paradigmi diversi fra i quali ogni singolo operatore, ogni "psicologo" sceglie il suo specifico percorso. Riprendendo Sternberg,

anche l'autore di questo contributo riconosce che il "chiudersi" in un singolo paradigma impedisce di ampliare le conoscenze. Il percorso verso l'unità inizia proprio dall'attitudine a "rinunciare" ad un paradigma singolo: l'uso di «paradigmi multipli potrà così contribuire a comprendere ogni singolo fenomeno psicologico» (p. 149).

Per cogliere la filosofia di fondo di questo volume possiamo riferirci alla dedica iniziale: «Questo libro è dedicato alla memoria di William James, il primo grande americano che ha lavorato per una visione unitaria della psicologia». James, infatti, sviluppando prima gli studi psicologici nell'ottica fisiologica e successivamente lavorando nel campo della filosofia, seppe cogliere l'importanza di avere una visione ampia delle ricerche psicologiche: quella visione che gli autori di questo volume auspicano come momento epistemologico per la psicologia contemporanea.

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Book Review

Introduzione alla storia della psicologia

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Laterza, Roma-Bari, 2007

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Il testo di Carmela Morabito ripercorre le tappe principali che hanno condotto la psicologia dalle sue origini filosofiche fino ai recenti sviluppi delle scienze cognitive. L'autrice muove da una considerazione stimolante: secondo Morabito, è inadeguato e storicamente scorretto assumere che la psicologia scientifica sia *nata*, così come viene tradizionalmente scritto nei manuali, con l'apertura del Laboratorio di Psicologia Sperimentale di Lipsia nel 1879 per opera di Wilhelm Wundt. I recenti sviluppi epistemologici, come le attuali analisi storiografiche della scienza, hanno ribadito la necessità di collocare opportunamente il sapere scientifico nel contesto storico, sociale e culturale. In particolare, i punti di vista storiografico e genetico possono offrire un'angolazione privilegiata proprio per discipline controverse come la psicologia, per quanto riguarda i presupposti, i metodi e i risultati sperimentali. L'assunzione di un preciso momento di demarcazione tra "un lungo passato" e "la breve storia" della disciplina rappresentato da Wundt, è, secondo Morabito, frutto di una modalità (erronea) di concepire la ricerca psicologica tipica di una certa epistemologia del Novecento e peculiare della scuola comportamentistica che ne ha definito i parametri e i metodi di indagine scientifica. In questa nuova luce, occorre recuperare i contributi delle epistemologie post-popperiane e delle impostazioni recenti della storiografia della scienza per una corretta analisi della disciplina.

Il testo non presenta particolari differenze rispetto ad altri manuali universitari nella scelta degli argomenti dei capitoli (l'eredità filosofica, la psicologia scientifica wundtiana, la prospettiva storico-culturale, la tradizione nord-americana e quella europea, le neuroscienze cognitive) quanto, appunto, si caratterizza per un'articolata ricognizione delle principali innovazioni filosofiche e metodologiche che hanno interessato il progressivo sviluppo dello studio scientifico della mente umana. Vengono pertanto analizzati il pensiero greco e romano, proseguendo con Cartesio, Hobbes, Bacone, Mill, La Mettrie, Spencer, soltanto per ricordare alcuni degli autori presentati, fino alle rivoluzionarie concezioni darwiniane e, quindi, a quella che è considerata 'la breve storia' della disciplina, iniziata con i fondamentali contributi di Wundt, Helmotz, Titchener, Fechner, James, sino all'attuale e consolidato rapporto con le neuroscienze cognitive.

Seppur contraddistinta da un'evidente frammentazione, la storia della psicologia ha visto alternarsi costantemente una pluralità di indirizzi programmatici e metodologie: l'esigenza di un'analisi fenomenologica si è accompagnata a una rigorosa matematizzazione, così come la necessità di oggettività del 'soggetto universale' si è scontrata, e ancora oggi si scontra, con l'irriducibilità della componente individuale e storica. Il quadro complessivo che emerge dalla rassegna dell'autrice è quello di una psicologia assai diversificata internamente fin dalla sua genesi, se di effettiva nascita appare ancora oggi legittimo parlare. Tale frammentazione, inoltre, ha promosso il fiorire di una moltitudine di linee di ricerca indipendenti e, in alcuni

casi, contrastanti, che inducono gli psicologi a una seria riflessione sullo statuto epistemologico della loro disciplina.

Morabito ci ricorda, infine, la nota enigmaticità della psicologia, che, come una figura reversibile, ora si osserva dal di dentro e altrettanto facilmente tenta di farlo dall'esterno, incarnando in maniera più complessa di altri campi del sapere, l'ambiguità tipica della soggettività umana. Tale ricca e intricata natura non è soltanto sinonimo di oscurità concettuale o confusione metodologica, ma è anche il risultato di un glorioso passato ancora in evoluzione che impone alla psicologia il confronto con discipline contraddistinte da una maggiore maturità (filosofia, matematica, biologia, fisica) senza per questo perdere la propria identità o specificità programmatica.

INDICE

Book Review

Mind. A Historical and Philosophical Introduction to the Major Theories

André Kukla and Joel Walmsley

Hackett Publishing Company, Indianapolis-Cambridge, 2006

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Mind. A historical and Philosophical Introduction to the Major Theories by André Kukla e Joel Welmsley is an introduction to the mayor modern theories of mind. In the Preface, the authors explain that the book is intended to be an introduction to the science of psychology for the nonspecialist «who isn't afraid to do a little thinking» (p. VII). They claim that their book is fundamentally different from the other introductions to the subject. Indeed, while the latter focuses on the data from experimental studies and on their empirical assessment, *Mind*, focuses on theories. In fact, the authors contend that the best introduction to psychology is by reporting theories rather than mere summaries of data, and criticize those who consider theories to be superfluous and fill books with experiments. Indeed, «data gathering activities that have no bearing on any theoretical issues are of little or no scientific value» (p. VIII).This is important also because the book is for nonspecialists, and «if there's one thing the non specialist should know about psychology it's the answers, however tentative they might be, to the deepest theoretical questions that have been asked» (p. VIII).

Kukla e Welmsley start their book by presenting Descartes' dualism, according to which the body and the mind are two fundamentally different kinds of substances:

> For Descartes the mind is an immaterial entity that functions as the repository of a person's conscious experiences. […] The mind's immateriality entails that it can't be identified with the physical body or any portion thereof. Cartesians regard persons as amalgams of two substances: material body and immaterial mind. For this reason, their view is said to be a type of dualism. (p. 1)

The authors then discuss three standard arguments showing that dualism is false. The first is that the dualism conflicts with the most fundamental laws of science; the second is based on the idea that there cannot be a causal connection between material and immaterial events; finally, the third is that materialism is simpler than dualism, and it «is a basic principle of scientific method that when choosing between two rival theories that are otherwise equally good, we should prefer the simpler one» (p. 13). The last paragraph of this first chapter is about determinism and voluntarism – «the other major limb of Cartesianism» (p. 15) –, voluntarism that seems incompatible with some of our most basic deterministic beliefs about morality and rationality.

The second chapter is about Wilhelm Wundt's introspectionism. Introspective psychology adopted the Cartesian assumption of dualism: the world is a combination of physical objects and immaterial mind. The physical portion is studied by natural sciences, while the mental is studied by psychology. The authors detect the most important rejection of introspectionism:

the behaviorist critique, the psychoanalytic critique by Sigmund Freud and the phenomenological critique.

Then they analyze the theory of psychoanalysis that studies the unconscious and irrational wellsprings of action. Freud showed a lot of examples about phenomena that didn't fit into the rationalistic framework and the most important conclusion from his observations is that there are unconscious causes of behavior. After two chapters dedicated to this theory the authors describe the Behavioral revolution and then the Cognitive revolution.

Behaviorists think that there can't be a science of mind. Behaviorism gets its name from the doctrine that the only thing that can be studied from the science of psychology is the behavior of organisms, that is to say, the purely physical movements exhibited by living beings. According to the behaviorists, psychology is the science that tries to explain and predict behavior. The research method of behaviorism is completely different than introspectionism. For behaviorists the experimenter puts others persons into a certain situation and observes and records how they behave in that situation. Wundt's methodology is drastically different. For example there is no distinction between the experimenter and the people observed because the experimenter puts himself in various situation and, most of all, what's observed and recorded isn't behavior but sensations, thoughts, feelings and so on.

After the downfall of behaviorism there was a new theory of mind: the cognitive science. It's a scientific approach to studying the mind that permits the mentalist language of inner states, like beliefs and desires. The cognitive science differs from the previously discussed traditions for its interdisciplinary. It's not only psychologists who are cognitive scientist but this theory includes contributions from philosophers, computer scientists, linguistics and anthropologists. Cognitive science is an attempt to combine all the insights from different fields into a single approach.

The book illustrates very well the disputes internal to each theoretical framework in psychology by discussing the major flaws and inconsistency of each of them. Moreover, they highlight the connections between the theories, and how each theory is a reaction to the flaws of their predecessors. The authors think that earlier theories not only inspired the current ones but also anticipated their difficulties. An image of psychology as an ongoing project is the final outcome of the book.

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Book Review

Psichiatria, psicoanalisi e nuova biologia della mente

Eric R. Kandel
Raffaello Cortina Editore, Milano, 2007

Noemi Della Ratta[*]
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In *Psichiatria, psicoanalisi e nuova biologia della mente* sono stati raccolti otto saggi del premio Nobel Eric Kandel, scritti nei trent'anni in cui era membro del *College of Physicians and Surgeons* di New York e pubblicati in diverse riviste scientifiche. Vengono affrontati diversi temi che spaziano dal campo della neurobiologia, a quelli della psichiatria e della psicoanalisi.

Quest'opera ricostruisce tematicamente il percorso che ha portato Kandel a fondare un'epistemologia scientifica a partire dalle sue prime ricerche per il *National Institute of Mental Health*. È convinzione di Kandel che per progredire nelle ricerche di biologia della mente, la «principale area di ricerca del ventunesimo secolo» (p. XVII) necessaria una convergenza tra aree di ricerca eterogenee. Alla base di quest'idea vi è la propria esperienza accademica; dopo aver conseguito il tirocinio in psichiatria, infatti, Kandel si è dedicato alle ricerche nel campo della biologia, potendo affermare successivamente:

> Sono convinto che il mio tirocinio in psichiatria e il mio interesse per la psicoanalisi svolgano un ruolo centrale nel mio pensiero e mi abbiano fornito una prospettiva sul comportamento che ha influenzato ogni aspetto del mio lavoro. (p. XXVI)

La psichiatria deve, da un lato, cercare di rispondere alle domande che le sono pertinenti, dall'altro porre domande sul comportamento a cui la biologia deve cercare di dare una risposta: solo così è possibile arrivare ad una visione complessiva dei processi mentali superiori dell'uomo, come afferma nel saggio *Un nuovo contesto intellettuale per la psichiatria* (1998):

> Il futuro della psichiatria è profondamente radicato nel suo passato e nel suo rapporto con la biologia, e i programmi di internato in psichiatria e neurologia dovrebbero partire da una base comune. (p. XXVI)

È stata proprio questa doppia formazione che ha consentito al neuroscienziato di incentrare le proprie ricerche sulle basi molecolari della plasticità sinaptica, ossia sulla capacità che i neuroni hanno di modificare le proprie sinapsi in relazione all'apprendimento e alla memoria. Gli studi condotti sulla lumaca di mare *Aplysia californica*, hanno permesso a Kandel di raggiungere risultati importanti: in primo luogo, analizzando gli aspetti fisiologici, biochimici e molecolari dell'apprendimento si è potuto dimostrare come questo possa influenzare l'eccitabilità dei neuroni oltre alla trasmissione sinaptica. Inoltre, si è dato prova di come i cambiamenti riguardanti la memoria promuovono trasformazioni strutturali dei contatti sinaptici, a

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dimostrazione di come il comportamento possa essere modificato ogni volta che si acquisiscono nuove informazioni sul mondo.

Quali sono stati i motivi che hanno spinto Kandel a condurre le proprie indagini empiriche su un animale così semplice e così fisiologicamente lontano dall'uomo? E quale metodo d'indagine è stato preferito? La difficoltà del tema con cui ci si andava ad imbattere richiedeva di partire da un sistema semplice ma definibile, che potesse poi far acquisire nuove informazioni sui processi più complessi. La grande intuizione di Kandel è stata quella di vedere un punto di contatto evoluzionistico tra gli animali e l'uomo: «Si trattava [*Aplysia*] di un organismo abbastanza complesso da mostrare semplici forme di apprendimento, ma con un sistema nervoso abbastanza semplice da poter essere studiato a fondo» (p. 244), secondo Steven Hyman, *la migliore delle ragioni per un riduzionista*. Le scoperte intorno alla memoria a breve e a lungo termine rilevate nell'*Aplysia*, sono state poi ricondotte su mammiferi.

Plasticità sinaptica, apprendimento e memoria: questi i grandi temi trattati nel volume. Ma ciò che i saggi sottolineano di pari passo alle grandi scoperte e intuizioni di Kandel – affiancato da illustri colleghi nel corso delle ricerche – è la strada aperta dall'autore e i consigli per continuare su questa promettente linea di ricerca. La nuova nascita della scienza della mente, infatti, necessita di un'unificazione complessiva del sistema concettuale delle varie discipline, quali neuroscienze, biologia molecolare e psicologia cognitiva. L'obiettivo è quello di fondare su basi scientifiche la psicoterapia, e di esplorare le sue implicazioni sul piano biologico. Per far questo è necessario usufruire di indagini empiriche che si servano di nuovi strumenti e nuove tecniche, su tutte quella di *brain imaging*.

Venendo a un'analisi dei singoli capitoli, il primo saggio, *Psicoterapia e sinapsi: l'influenza delle teorie psichiatriche sulla ricerca in neurobiologia*, illustra l'importanza del ruolo svolto dall'esperienza durante lo sviluppo dei primati, specie umana inclusa. Analizzando deprivazioni infantili si sono rese manifeste le implicazioni negative sull'apprendimento e la memoria derivate dalla mancanza di *vivere l'esperienza giusta in momenti giusti*.

Nel secondo saggio, *Un nuovo contesto intellettuale per la psichiatria*, Kandel delinea ipotetici ruoli che in futuro verranno svolti dalla psicoanalisi all'interno della ricerca scientifica, auspicandosi una cooperazione tra gli ambiti di indagine della biologia e quello della psicoanalisi.

Nel terzo saggio, *La biologia e il futuro della psicoanalisi: una rilettura di 'Un nuovo contesto intellettuale per la psichiatria'*, Kandel esorta la psicoanalisi ad integrarsi con le scienze sperimentali affinché si possa realizzare un sistema scientifico completo. In tal modo, da un lato la psicoanalisi può apportare significativi contributi teorici, dall'altro la neurobiologia deve cercare di confermare sperimentalmente queste intuizioni.

Dalla metapsicologia alla biologia molecolare: uno studio sui meccanismi dell'ansia mette in luce l'importanza dei modelli animali nello studio dei disturbi psichiatrici. Dal momento che vige una certa continuità evoluzionistica gli studi sul comportamento animale possono essere applicati successivamente all'individuo.

Nel quinto saggio, *Neurobiologia e biologia molecolare: un secondo incontro*, Kandel si pronuncia in modo definitivo sull'integrazione della biologia molecolare con le neuroscienze, dimostrando come già importanti studiosi di biologia molecolare all'inizio degli anni '80 – Francis Crick, Seymour Benzer, Sidney Brenner e James Watson – fossero passati a studi neurobiologici.

Il sesto saggio *Neuroscienze: un secolo di progressi e i misteri ancora irrisolti* riassume a partire dalle origini i progressi delle neuroscienze, indicando quali prospettive si sono aperte all'indagine nel corso degli anni e quali problemi ancora non hanno trovato una soluzione compiuta.

La biologia molecolare della memoria: un dialogo tra geni e sinapsi ripropone la lezione magistrale tenutasi nel 2000 in occasione del conferimento del premio Nobel. Viene qui riassunto il percorso di ricerca seguito da Kandel e dai suoi collaboratori, presentando le scoperte fondamentali derivate dallo studio sulla lumaca di mare *Aplysia californica*.

Nell'ultimo saggio *Geni, cervello e comprensione dell'individuo: l'aspirazione della biologia a un nuovo umanesimo* vengono delineate le implicazioni mediche e sociali derivate dalle nuove conoscenze sul genoma umano.

Nella postfazione, Kandel delinea i risultati più importanti che, con probabilità, verranno conseguiti negli studi futuri e le priorità di ricerca sulle quali è necessario investire, affermando in conclusione:

> La biologia svolgerà il ruolo, che le è proprio, di ponte naturale tra le scienze dell'uomo, interessate ai temi dell'esistenza umana, e le scienze della natura, che si occupano del mondo fisico. […] Possiamo augurarci che la psichiatria e la psicoanalisi siano protagoniste in quest'impresa storica di comprendere la mente in termini biologici, così da catturare nuovamente l'interesse dei membri più validi e promettenti delle nuove generazioni. (p. 456)

Book Review

Unified Social Cognition

Norman H. Anderson
Psychology Press, New York, 2008

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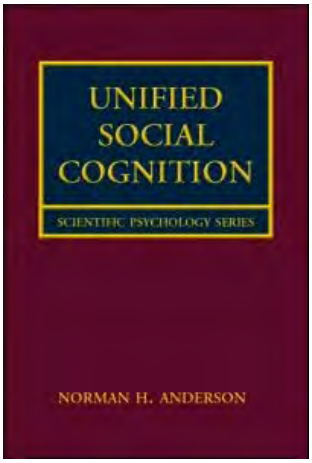

This book is the fifth one of a series dedicated to Information Integration Theory (IIT); a theory about the way living organisms and humans in particular, integrate the information from the environment for the purpose of adapting to its changing character. In 1981, Norman Anderson published *Foundations of Information Integration Theory* in which he established the basis of IIT, and synthesized the empirical findings gathered since the early 60s that led him to suggest that information integration in the human mind (but not only in the human mind) obeys three fundamental laws: averaging, adding, and multiplication. He considered these laws as a foundation for a functional theory of purposive perception, thought, and action. In 1982, Anderson published *Methods of Information Integration Theory* in which he offered a set of well-designed tools allowing for the characterization of information integration in any setting. In *Contributions to Information Integration Theory*, published in 1991, Anderson discussed and synthesized the many contributions by members of his team in the fields of cognitive, developmental and social psychology. Finally, in *A Functional Theory of Cognition*, published in 1996, Anderson strongly restated the everyday life character of his theory.

In the present book, Anderson presents an ultimate synthesis of his theory, and offers his personal views on what he believes should be the scientific psychology of the current century. Psychological science rests on two axioms: the Axiom of Purposiveness and the Axiom of Integration. The Axiom of Purposiveness states that any perception, thought or action is goal-oriented; «Life is purpose, purpose is life» (p. 399). It follows that psychological theories that don't consider purposiveness as a central concept are bound to miss the essence of the phenomena they aim to describe and explain. The Axiom of Integration states that perception, thought, and action depend on the integrated action of multiple informers. It follows that psychological theories that don't consider integration as a central concept or that don't have adequate tools for characterizing integration processes are bound not to understand thought and action, because understanding them primarily supposes understanding how informers have been combined. Most psychological theories have, until now, not fully recognized the centrality of purposiveness and integration in human conduct. As a result, present day psychological science appears as a very fragmented field, a field that is populated by mini theories, each one explaining a very narrow segment of the psychological reality, each one having often been developed in complete ignorance of the others. Unification appears largely elusive, and as a consequence, progress is (at best) very slow: Mini theories reign for a time in their respective fields until they are replaced by more fashionable others.

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The book is aimed at demonstrating how cumulative progress has been made within IIT owing to the efforts of many investigators whose work has established the three integration laws on strong empirical ground in many areas of everyday life psychology. This includes person cognition, functional theory of attitudes, moral algebra, group dynamics, developmental integration theory, consciousness analysis, person science, integration learning theory, functional memory, cognitive judgment-decision, language integration, integration psychophysics, psychological measurement, and analytic gestalt theory.

Chapter I (Unified Theory of Cognition) fully develops the two axioms. It presents what is called "the information diagram", a chain of three operators: Valuation, Integration, and Action. Physical stimuli that impinge on the organism are transmuted into psychological representations by the valuation operator. The valuation operation depends on the content of the stimuli and on the goal that is pursed at the time of the operation. In other words, physical stimuli don't have any fixed psychological value. Then, the psychological representations (not the physical stimuli) are integrated by the integration operator to produce an internal response. The integration process depends on the psychological representations, and also on the goal that is pursed at the time of the operation. The three integration laws mentioned before refer to the integration process. Two identical psychological representations may be integrated in an additive way under some circumstances and in a multiplicative way under other circumstances. Finally, the internal response is externalized by the action operator to become the observable response. This externalization also depends on the goal assigned at the time of the operation. Psychological representations, internal responses and actions are constructed during the process; they are in no way predetermined. IIT is, unambiguously, a constructionist theory.

Chapter II (Psychological Laws) shows the way to solve the problem of estimating, from the physical values of the stimuli impinging on the organism (inputs) and the observed responses of this organism (outputs), the three unknowns in the integration diagram. In other words, this chapter demonstrates the way for measuring the psychological representations, determining the internal responses, and characterizing the integration rule. The basic idea consists in relying on factorial designs, observing patterns of external responses (not just single responses), and deriving from them the three sets of unknowns. This is made possible owing to the existence and ubiquity of the psychological laws of information integration. These mathematical laws predict different patterns of responses. From the patterns observed, it is possible to determine which law has been implemented and, as a result, estimating all the psychological values involved becomes possible. This is why these laws are considered as the foundation of unified psychological science. «They are Nature's solution to the Axioms of Purposiveness and Integration» (p. 48). Demonstrations of the Parallelism Theorem (the pattern predicted by the adding law), and of the Linear Fan Theorem (the pattern predicted by the multiplication law), are also presented, and the averaging law is discussed at length.

Chapter III (Foundations of person cognition) concretely illustrates the way in which the principles developed in Chapter II can help disentangle most of the unresolved issues in person cognition. It argues that person cognition is the basic area of social-personality psychology, although it is fragmented across many sub-domains: attitudes, close relationships, moral cognition, emotion, and personality, to name a few. Once unified, the area of personal cognition should be the basis of social-personality psychology.

Chapter IV (Functional Theory of Attitudes), V (Attitude integration theory), and VI (Comparison of attitudes theories) illustrate how the same principles can allow for a radical shift in attitude research. Although most social attitudes are strongly endorsed (attitudes towards religion, government, gender roles), the attitude field has been largely focused on

"nonattitudes"; that is, on attitudes towards issues on which people have no strong involvement. This is because the field was centered on persuasion, and only weakly endorsed "attitudes" can be changed during a laboratory session. This exclusive focus on "nonattitudes" has been responsible for the extreme fragmentation of the attitude field (no less than eight different theories are compared in chapter V). How well long term attitudes work in everyday cognition – that is, how they regulate the valuation operator – how they are integrated with current, short term information, and how they impact on the action operator should be interesting future research objectives.

Chapter VII (Moral Algebra) shows that moral cognition obeys algebraic laws (e.g., Hommers' Culpa theory). This finding may appear as puzzling (or simplistic) to modern moral psychologists' eyes, but it would not have been a surprise to ancient philosophers (e.g., Aristotle's unfairness model). The psychology of morality has for a long time been confined to peripheral issues owing to inappropriate methodology (e.g., Goldberg's dilemma technique). The chapter shows how IIT can enrich the field by driving the researcher's attention to the wealth of moral phenomena in everyday life:

> helping and not helping, kindness and unkindness, fairness and unfairness, deserving, praise and blame, obligation and duty, resentment, getting even, envy, lying, cheating, honesty, extenuation, apology, mitigation, atonement, and forgiveness. (p. 199)

Chapter VIII (Group Dynamics) focuses on marriage and spouse relationships, group discussion, group affiliation, and group decision. In particular, the Social Averaging Theorem, and the evidence that supports it is discussed at length.

Chapter IX (Cognitive theory of judgment-decision) synthesizes studies on judgment and decision that totally deviate from the normative framework of optimal decision that has reigned in the field since the mid-70s. The criticisms that address studies conducted in the normative framework are especially interesting. First, most judgments of everyday life lack normative standards. As a result, confining psychological research to situations in which standards exist amounts to turning our backs on most everyday life situations. Second, psychological values are essential to really understand people's judgments and decisions. However, values are, unfortunately, outside normative frameworks. Third, what is examined and "explained" in these studies are not the persons' responses but the deviations between these responses and the standard response. These deviations are called bias. Biases are unlikely to reveal true cognitive processes. In addition, the invalidity of these prescriptive models for the description of cognitive processes is evident on a priori grounds for the simple reason that these models (e.g., Bayes' theorem), as with any human-made material or intellectual tool (e.g., the flint biface, the computer), have been created throughout human history for complementing the human innate capacities in areas in which they have already been shown to be insufficient for solving specific problems (e.g., cutting meat, combining uncertainties, running simulations).

Chapter X (General Theory) insists on the duality of the two worlds: the external, physical world and the internal, psychological world. The three laws of information integration are laws describing the functioning of the internal world. In other terms, this world exhibits mathematical laws, which allows study it on its own terms, without appealing to the structure of the physical world. Examples taken from studies on language, hedonic theory, intuitive physics, social stereotypes, to name a few, show how these laws allow for the measurement of non-conscious sensations. These laws are probably at the origin of the development of mathematics.

Chapter XI (Experimental Methods) shows how the development of methodology has gone hand in hand with the uncovering of the basic psychological laws. Special importance is given in this chapter to single person designs and to personal designs (single person design embedded into the person's environment): as «cognition occurs in individual organisms; that is where it can be most effectively studied» (p. 374). Finally Chapter XII, the concluding chapter, suggests new avenues for intrepid researchers.

Norman Anderson's book is not suitable for the faint of heart and timid researchers. It is full of original (sometimes extreme) views on practically every field in psychology. Moreover, the book is unique in that its main objective is not to merely criticize existing theories and methods, but rather to present the reader with a unified framework (IIT) that is able to respond to all the criticisms that have been raised. In particular, this framework, which has already been applied in virtually every domain that deals with human judgment, provides an alternative for micro-theories. This is what makes this book a good recommendation for psychologists who are not irresistibly attracted to current fashions and reigning consensus.

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Book Review

Psychiatry as a Cognitive Neuroscience

Edited by Matthew R. Broome and Lisa Bortolotti
Oxford University Press, Oxford, 2009

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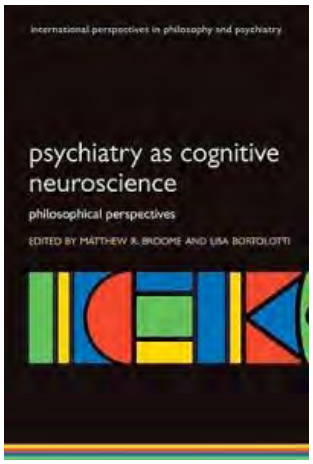

The first thing one might think about a book bearing the title *Psychiatry as cognitive neuroscience* is that it looks unbelievably promising. Authors of this collection of papers, which is in the new series for the OUP devoted to the analytical philosophy of psychiatry, agree to the recent suggestion in the psychiatric debate: re-conceiving psychiatry in cognitive terms (see Marshall and Halligan 1996, Murphy 2006). The book is written by major experts of this new field but could hardly act as an introduction for beginners: all papers presuppose a certain level of knowledge.

Actually the title could somewhat deceive future inexpert readers. At present psychiatry is neither a cognitive nor a scientific discipline. Compared to etiology-based medical disciplines, psychiatry has a purely descriptive nosography. Namely, it is stuck at a scientifically primitive stage. Thus, as being etiologically *pre*-paradigmatic in Kuhn's (1962) sense (see Aragona 2006), psychiatry is roughly equivalent to the pre-seventeenth century general medicine. Besides, the field explored (psychiatry as a cognitive discipline) is much more at dawn than well-developed, and it is not founded on some shared theoretical manifest. As a matter of fact, cognitive neuropsychiatry is no more than a hybrid label coming from cognitive neuropsychology, which provided models and tools for new research on psychiatric symptoms (in particular delusions). Firstly, the *deficit* paradigm comes from cognitive neuropsychology for psychopathology, as an alternative to the *conflict* paradigm from psychoanalysis and to its assumption of *subtractivity* from the normal cognitive functioning. Secondly, the reductionist view suggested by the 'neuro' prefix, which links psychiatry to neural substrates granted by the new techniques of functional neuroimaging. Nevertheless even cognitive science, except for some background constraints, is still hesitant about the paradigm to embrace and confined by two different conceptions of mind, namely the classical (i.e., functionalism) and post-classical cognitivism (i.e., connectionism, dynamicism and situated cognition). One could wonder which kind of cognitivism psychiatry should look at. Sure, «cognitive neuroscience is not cognitive science in the broad generic sense» (p. 201, ft. 1). But that is the first remarkable omitted question in this philosophical book: what is cognitive neuroscience?

Apart from a short introduction, the 17 chapters of the book are divided into 7 main sections, to which the editors' paper (Chapter 18) follows as an epilogue. The first three sections show a broader philosophical purpose: discussing psychiatry as a science (Section 1), as dealing with natural kinds (Section 2), and as either a personal or sub-personal level discipline (Section 3). Section 4 introduces to the issue of neuroscience and links to the following more specific sections on phenomenology (Section 5), delusions as cognition (Section

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6), and moral psychology. Each contribution is conclusive in its own purpose and can be read apart from others.

Concerning the question of the scientific nature of psychiatry (Section 1), three contributions are given. Rachel Cooper's Chapter 1 claims that the question itself is a non-sense referred to any discipline, as the history of philosophy of science (from Popper to Feyerabend) shows that the search for a demarcation criterion for science failed. The solution defended conceives science as a "family resemblance term"' in the Wittgensteinian sense, and it replaces the core question about the scientific nature with the question on the reliability of methods employed. Chapter 2, written by Bill Fulford and Norman Sartorius, reveals a secret detail about the historical genesis of the current descriptive classifications (OCD and DSM) of mental disorders. Against the received history, authors deny the recurring statement of C. G. Hempel and E. Stengel's contribution towards less mentioned psychiatrist Sir. Aubrey Lewis' fatherhood. Richard Samuel's Chapter 3 faces the question whether mental illnesses can be or not object of scientific enquiry in the shape of a philosophical defense of the NK (i.e., Natural Kind) Thesis for delusions from common objections.

Chapter 2 and 3 arouse more interest than Cooper's one. It's true that philosophy of science debate stranded on the conception of irrationality of scientific discovery in Feyerabend's style. Nevertheless science, even if not infallible and objective, can be conceived not as completely arbitrary. Science is in fact conceivable as an enquiry benefiting from the inter-subjective controllability of objective theories (see Brown 1977). So the question of the scientific status of a discipline is still significantly relevant in the debate. Moreover, the suggested meaning shift could hardly stop calling into question psychiatry. Current psychiatry methods are still not reliable if the here undefined reliability is somehow related to the capability of providing explanations and treatments for mental illnesses. The idea of method centrality stands not because the question on psychiatry as a science is inappropriate, as it is not, but because the question on a definite choice on methods is more impellent.

Fulford and Sartorius's contribution is original in the debate. They bring up in discussion standard historical reconstruction on Carl Hempel's theoretical responsibility for current psychiatric taxonomy. Just a little negligence is that authors, although recognizing Hempel as associated to the Vienna Circle (p. 30), identify him as an «American philosopher» (twice in p. 29). However, even though emigrated to the United States after anti-Semitic repression and involved in the American debate, Hempel was German-born.

In line with other Samuels' works, Chapter 3 is brilliant and well-articulated. Delusions are conceived as natural kinds even if possibly multiply realized. Responses to objections appear motivated but, compared to the debate exposition, they are shorter, thus further developable. Delusion is a fundamental psychiatric symptom and became recently matter of debate for normativity in psychiatry. Nevertheless Constructivist objections against natural kinds are dedicated to mental illness in general and Samuel's discussion could have worked for it too. The explicit reference to delusions in the title of the paper contrasts the generality of the section as well as the topic that seems much more pertaining to the next section.

Section 2 in fact is dedicated to the nature of mental illness and includes Hanna Pickard's Chapter 4 and Dominic Murphy's Chapter 5. Contrary to Samuels' opinion, Pickard stands for a defender of the old-fashioned Thomas Szasz' thesis of the myth of mental illness, in particular for schizophrenia (hence psychoses and again delusions) and personality disorders, while she states that their symptoms can be scientifically explained and thus psychiatric symptoms are not a myth. The paper is right in conclusions but misleading. At present most of psychiatrists, perhaps all of them, agree upon the invalid scientific status of these two diagnostic categories. Nevertheless I have doubts that this means sharing generally the Szasz' view, which inspired

anti-psychiatrist movement in the 70s and appears much more metaphysically radical than Pickard thinks.

More accurate is Dominic Murphy's chapter on the medical model in psychiatric classification. He reports new references and considerations, but his paper coincides essentially with his recent book, where stated conception of multilevel cognitive reductionism is well-developed. That is, a strong (etiological) version of the medical model for psychiatry does not entail necessary the new-fangled micro-reductionist approach from molecular psychiatry. Even in this case, though, the topic (classification) seem not completely relevant to the section purpose and the paper might have been better included in the previous section, instead of Samuels' one that might have appeared here.

Section 3 proposes to reconcile paradigms, namely personal narratives with sub-personal scientific explanations. Tim Thornton's Chapter 6 deals with paradigms explicitly in these philosophical terms; John Campbell's Chapter 7 talks of rational attitudes and neuroscientific explanation; and Philip Gerrans' Chapter 8 compares inferential accounts to neurobiological explanations for delusion. Samuel's question of the normativity of delusions returns here as the philosophical macro-tension of the metaphysical interface between realms of speech.

Admittedly, paradigms are all but reconciled by Thornton and Campbell. Thornton's negative answer gives little hope to the naturalist. No place for neither reductionism nor supervenience antireductionism nor stance strategies middle way. And the pragmatic suggestion is to pursue local accommodations neglecting the idea of a philosophical global resolution. Campbell's solution on propositional attitudes is that they express causal relations not rational ones, in spite of the well-known Davidson-Dennett tradition that states the link between rationality and mental state ascription. Again examples come up from delusions. Campbell appears agnostic, though, about the discovery of a biological mechanism linking cause and effect and then maintains personal level descriptions.

On the contrary, Gerrans' neurocomputational theory on the role of prefrontal cortex for delusion is the only reconciling. He brilliantly links the two apparently incommensurable paradigms on delusion as either a belief or a neurotransmitter problem. He originally provides a bridge among three level hypotheses: dopamine from neurobiology, mental time travel process from cognitive psychology and autobiographical narratives from phenomenology. Thus he challenges the standard confirmation bias conception (Stone and Young 1997) in favor of a narrative bias account and finds evidence of it from cognitive behavioral therapy. In all that, the paper is of great scientific interest.

Section 4 originally deals with psychiatry and the neurosciences. All over the section, neuroscientific findings are integrated by conclusions coming from philosophy. While Chapter 9 looks at higher brain size levels (cognitive function and brain areas), Chapter 10 is at a lower (neurogenetics). Dan Loyd's Chapter 9 integrates cognitive accounts on schizophrenia with the phenomenological concept of temporality. He then reports recent findings in fMRI on schizophrenia symptoms involving multiple brain areas, and he proposes to use fMRI for a unifying account of schizophrenia as a disruption in temporal domain. Dan J. Stein's Chapter 10 considers gene polymorphism contribution to mental illness and its implications for some philosophical questions. He concludes with irreducibility of psychological phenomena to a particular gene variant. The chapter is, though, a bit rushed and shallow in argumentations. Finally, Stephens and Graham's Chapter 11 explicitly states the impossibility to trace a neuroscientific account for compulsion in addictions without referring to norms coming from philosophical analysis. That is to say, the onlooker (philosophy) gets the best of the fight.

Section 5, on phenomenology and scientific explanation, concludes again in favor of philosophy. Matthew Ratcliffe's Chapter 12 defends phenomenological subjective explanation as object of enquiry. In short, no neuroscience research without phenomenology, in particular

for depression. Shaun Gallagher's Chapter 13 on the phenomenology of delusion shows the same purpose. He reviews delusion debate, namely the most recent versions of top-down and bottom-up accounts. He then shows an alternative model, not predicated as the formers on the standard view of delusion as dysfunction in the brain. This model is based on the relativistic phenomenological hypothesis of multiple realities. It is however conceived as an essential descriptive integration to the scientific explanatory account (see p. 260).

Section 6 is explicitly devoted to delusion and cognition. It consists of Keith Frankish's Chapter 14 on a two-level framework for delusion, and the remarkable Anne and Martin Davies' Chapter 15 on how to explain pathologies of belief. Frankish's effort to interpret the doxastic conception of delusion by the recent dual-system theory on belief is appreciable. He argues that beliefs can be located at two different levels (one unconscious and dispositional, the other conscious and functional), and that delusions, whatever doxastic or non-doxastic, belong to the second as acceptances. His arguments, although supported by experimental data on reasoning, are strictly folk psychological, but they suggest an attractive path that Mr. and Mrs. Davies tread better. Their extended and painstaking contribution is top-grade. They suggest different explanations of delusion can be parametric variation within the two-factor framework. However, the great merit of the paper is connecting for the first time in the debate belief evaluation to cognitive and brain functions through experimental neuroimaging studies on reasoning. That makes the paper an indispensable reading and an extraordinary advance in the debate on delusion, where the naturalization of reasoning processes is usually denied (see Bermúdez 2001, Murphy 2006).

Last section, Section 7, is on moral psychology. It includes Jeanette Kennett and Steve Matthews' Chapter 16 on frontal lobe damages and lack of agent responsibility, and Iain Law's Chapter 17 on the interference of depression with normal moral motivation. While the former relates normative concepts as autonomous agency to the neural basis of cognitive function (mental time travel), the latter deals with depression and concepts as motivation and virtue at the personal level of philosophy and folk psychology.

Conclusive chapter, by editors Lisa Bortolotti and Matthew Broome, keeps in line with many other papers. Authors again try to show why neurosciences cannot do all the explanatory work in psychiatry by the two examples of normativity in the definition of deviancy (in particular the normative concept of 'authorship' for delusion) and environmental etiology or externalism in schizophrenia. Again they tip the scales too much in favor of philosophy and folk psychology. Here authors seem to forget, however, two important things. On the one hand, cognitive neuroscience does not correspond necessarily to eliminativism. But stating the importance of psychology or other higher-level science does not need to take the form of denying the possibility of reduction of higher levels (see, for example, Bechtel 2007). On the other hand, assuming the internalist view according to which the *locus of control* is the mind-brain does not mean that the quest to understand how a cognitive agent is situated in its environment is not well motivated (see Bechtel 2009).

In conclusion, such a long book hardly calls for a conclusive verdict. As we have seen, each paper has a different degree of scientific value compared to others: some are very interesting, others are less winning. By the way, the book as a whole is affected by a list of problems. Firstly, the book is not well-organized. Section titles are often misleading and included papers are sometimes off the topic (see what said about Sections 1-2-3-4). Even the book title is inappropriate. As a matter of fact, the subtitle *Philosophical Perspectives* translates much more the book purposes. This is not a text of reference for the new cognitive neuropsychiatry, as the title seems to suggest. This is much more a philosophical text that critically explores the real possibility of such a research. The book thus appears much more destructive than constructive.

The general view arising from many papers is in fact that this discipline inevitably resists to naturalization. Therefore, why talking about "psychiatry as a cognitive *neuroscience*" if in many cases the neuroscientific approach encounters insuperable limits (personal explanation, phenomenology, normativity, and so on)? Here future psychiatry seems rather mainly conceived as an anti-reductionist discipline. Namely, each epistemological level maintains complete autonomy and has no hope to be linked to the lower. To tell the truth, the book remains ambiguous on the subject (perhaps because not all authors would have shared the same opinion – see, as examples, Chapter 8, 15 and 16 for a contrary approach).

Secondly, a problem recurring in the philosophical debate on psychiatry affects this book as well. The book gives too much weight to delusion. Certainly delusion is a core psychiatric symptom, but it is not all the story. Contemporary philosophy of mind have centered the debate on delusion since hypotheses on delusion (in particular, the Capgras delusion) became paradigmatic of the new research in cognitive neuropsychiatry. In due time this restricted philosophical interest might appear a caricature of the discipline. What about other psychiatric phenomena? A feeble interest for them is showed in the book (only 5 of 18 chapters mention one of the other psychiatric conditions).

Thirdly, the book often seems to come to a standstill. According to philosophers' wish, the first concern doesn't seem to be how to better construct a cognitive neuropsychiatry to resolve the puzzling problem of mental illness, but reserving a secure place for philosophy in this new field of research. The book nearly ignores the literature on Mechanist account for general cognitive neuroscience (see Bechtel 2008, Craver 2007), which would have overcome the fear of the death of philosophy and psychology because of reduction. And in worrying too much about philosophical issues, it sometimes appears to miss the point of psychiatry. That is, the point of a medical practice with concrete needs. Reduction of higher levels would be the way to put philosophical ideas in the concrete form of clinical intervention.

In conclusion, this text is a good guide to keep expert scholars abreast of philosophical developments on psychiatry. Nevertheless, because of few considerable papers, it is only a partial tool if it aims to clarify to psychiatrists what espousing a cognitive neuroscientific direction might really mean for themselves. So, apart from some remarkable contribution, the book as a whole does not come up to expectations. Unfortunately, we are still a long way from a tangible cognitive revolution in psychiatry.

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Book Review

The Cambridge Handbook of Sociocultural Psychology

Edited by Jaan Valsiner and Alberto Rosa
Cambridge University Press, New York, 2007

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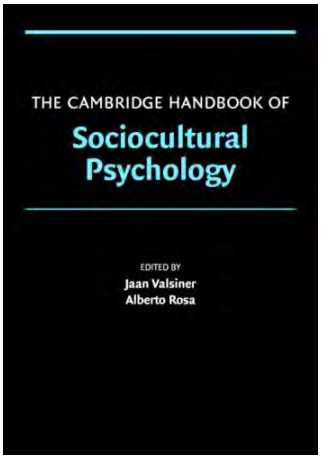

It is a very difficult and exciting task to account for the content of a book such as the *Cambridge Handbook of Sociocultural Psychology*. Indeed, the challenge of a handbook is generally that of addressing scholars and students by representing the state-of-the-art of a research field. However, this *Handbook* seems to have a higher target. It contains 35 chapters by 59 different authors, covering almost all the topics of contemporary psychology – such as language, cognition, the relationship between theory and methodology, the concepts of identity, culture, representation, and Self, only to mention some – by the light of sociocultural psychology, whose main concern is to understand the relationship between mind and culture, between individual psychology and social functioning.

Beyond all different versions of social, psychological or physiological reductionism, sociocultural psychology, as drawn by the editors and the authors of the *Handbook*, is arguing today that «social and cultural life are indissociable from the threads which make up the fabric of the human Psyche» (p. ix). This sounds like an old-fashioned statement, and from some point of view it re-proposes the way in which «the very forefathers of Psychology» (p. ix) conceived the basic intertwine between social and psychological phenomena against reductionism. Thus, to understand the editors' claim we can easily go back to 1860 to find an example in Carlo Cattaneo's statement that «the most social act of human beings is thinking» (Cattaneo 2000, p. 89, *my translation*). Later on, Wundt recognized that «the material aspects of the world culture exerted an influence upon the mental aspects, whose direct expressions are speech and writing» (Wundt 1916, p. 486). Reductionism was a sworn enemy also for Lurja, claiming for a «romantic science» able to grasp the richness and the complexity of the «living totality» (Lurja 1979).

However, the failure of social sciences in pursuing the very knowledge of human nature was already clear during the first half of the 20th Century (Jahoda 2007). As Murchinson complained:

> The social sciences at the present moment stand naked and feeble in the midst of the political uncertainty in the world […] And at the end of all these centuries, no one knows what is wrong with the world or what is likely to happen in the world. (Murchinson 1935, p. ix)

The answer to this malaise, passed through the decades, is the «result of various historical dialogues within psychology, sociology, and anthropology» (p. 3).

The common features of the scientific threads converging towards a sociocultural view of human Psyche are the focus on interaction, language and semiosis, the holistic concept of

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human beings, the role of human activity in shaping both social and individual world, and the attention to historical and processual nature of human facts.

The *Handbook* is structured in seven main sections providing the point of view of sociocultural psychology on the above mentioned dimensions. The authors draw a network of pathways crossing the psychological landmark of human beings. The first section (*Theoretical and methodological issue*) presents some major theoretical and methodological issues, such as explanation, time and causality direction in social sciences and how it affects the delimitation of phenomena, and the link between theory formulation and empirical research. The second section (*From nature to culture*) presents several reflections on the biological and phylogenetic roots of human Psyche with respect to the culture. The third section (*From orientation to meaning*) moves towards the specificity of human nature, that is, the ability to use signs to build the cultural world from perception, motor exploration and action. This ability to manage symbolic resources to build the Self and the identity within a given social contexts is the focus of the forth section (*From orientation to meaning*). The fifth (*From society to the person through culture*) and sixth (*From social culture to personal culture*) sections widen the perspective by taking into account the relationship between the individual and the society: a movement going backwards and forwards from society to the person and from social culture to personal culture. Finally, the seventh section (*Making sense of the past for the future: memory and Self-reflection*) deals with the diachronic dimension of mind and culture describing how «collective and personal memory functions are intricately linked» (p. 16).

The overall impression provided by the *Handbook* is a rich and interesting endeavour to reread the whole of psychological knowledge by challenging some assumptions that the mainstream psychology has too quickly taken for granted. As the editors claim: «researchers are explorers, not caravaneers» (p. x).

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Book Review

Psychology's Interpretive Turn: The Search for Truth and Agency in Theoretical and Philosophical Psychology

Barbara S. Held

American Psychological Association, Washington DC, 2007

William Meehan[*]

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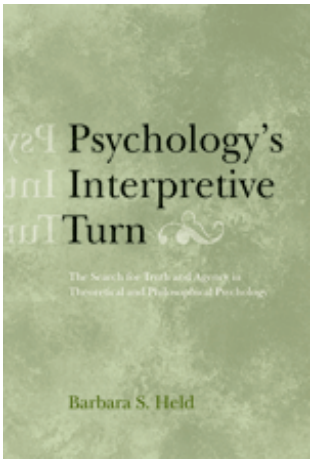

Much of the history of philosophical psychology over the past three quarters of a century has been occupied with the postmodern critique of conventional scientific psychology, which the critics have decried as mechanistic, reductionist, and unable to account for the essential human experience of agency. But the fundamental epistemic relativism of postmodernism, or social constructionism, has itself been criticized, even by those sympathetic to its central insights, and in the past few decades a variety of theorists and empirical researchers have tried to preserve essential elements of both traditions (for a broad sampling of such approaches, see Erneling & Johnson 2005). Barbara Held's most recent book, *Psychology's Interpretive Turn: the Search for Truth and Agency in Theoretical and Philosophical Psychology* is a study and critique of one segment of the literature that has arisen from this search for a *via media*.

Though united by this search for a more moderate critique of conventional psychology, the group of thinkers, to whom Held refers collectively as *middle ground-theorists*, do not constitute a unified school. Her subjects represent a number of traditions including hermeneutics, neo-positivism, and discursive constructionism. What they do have in common, however, is a commitment to modifying the profound relativism of the radical postmodernists and social constructionists in ways that preserve the postmodern idea that, «[o]ur psychological existence is constituted by, constrained by, and thus dependent on the historical/cultural/linguistic context of our location» (p. 88).

The plurality of traditions to which Held's subjects owe allegiance makes her task a difficult one. In order to do justice to this diversity, it is necessary for her to make multiple theory-specific analyses, which tend to break the flow of her own discussion and give the book a mosaic quality. A reader who has paid close attention to her introductory chapter should not have trouble following the thread of her argument, but the need for specificity complicates what is already a relatively complex analysis of the interplay of ontological and epistemological issues discussed by all her subjects.

Held's most fundamental critique of the middle-ground theorists is that they fail to differentiate themselves from the basic postmodern belief that human nature is self-constructed to such an extent that there can be no objective psychological universals. Such an ontology, she argues, implies that reason itself is socially constructed, which, if true, would necessitate a fundamentally relativist epistemology. This ontological-epistemological question demands a sophisticated philosophical discussion of both of its elements, which is reflected in

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the book's organization into a series of sections in which Held first considers the ontological and epistemological issues separately, and then combines them in a final section where she analyzes the interdependent ontological-epistemological unity.

In the sphere of ontology, Held sees the middle-ground theorists as seeking to carve out positions that avoid both relativism and mechanism, combining the realism of conventional psychology with the agency that postmodernists say is missing from 'mainstream' approaches. Held emphatically approves of the general intent of this project but argues that these theorists fail to achieve their goal, in part, because they accept the postmodernists' mistaken definition of agency with self-fashioning (cf. Greenblatt 1980). Middle-ground theorists argue that, in their models, the constraints on self-construction imposed by a given society, culture and language ground the psychological individual in ways that allow for ontological status, without having to see human action as the result of a string of external causes. Pointing to passages in the writings of middle-ground theorists in which they assert that human beings are «simply» or «just» what they interpret themselves as being (p. 99), Held argues that interpretation cannot confer such ontological status, even when that interpretation is constrained by social and cultural context. What is needed, she proposes, is a conception of human agency as rooted, not in freedom of interpretation, but in the human ability to rationally evaluate one's circumstances and act accordingly.

The agency-as-self-fashioning position of the middle-ground theorists also stems, Held suggests, from their acceptance of the postmodern characterization of conventional scientific realism. According to her subjects, the conventional research program's focus on the study of mind-independent objects carries with it the notion that psychological properties and behaviors are reducible to those of fundamental physical particles. So defined, an objective approach could only lead to a reductionism that is unsuitable for human sciences; but Held rejects this identification of realism with reductionism, citing the work of the realist philosopher, Amie Thomasson (2008), who argues that human-scale objects are as real as the fundamental particles of which they are composed. Such an ontology, Held suggests, makes it possible to consider human behavior and psychological states without reducing them either to brain processes or to the local cultural and linguistic forces that shape the psychological subject's phenomenological experience. The middle-ground theorists' inability to envision such non-reductive, objectivist realism, Held argues, results in an ontology that, whatever its claims about social and cultural constraints, is hardly less relativist than that of the explicitly radical postmodernists.

Such an ontology, in turn, restricts the range of epistemological positions open to the middle-ground theorists. Specifically, it provides no basis from which to criticize the postmodernist dichotomy that limits epistemological options to a choice between pretensions to a "view from nowhere" and "situated knowing" in which both knower and known are so relativized that the subjective and the objective become indistinguishable. As an alternative to such a dichotomy, Held suggests the more complex formulation of Edward Pols (1992), who understands human experience as incorporating both situated and universal elements.

The conflict between the conventional and the interpretive approaches are indicative of one of the most serious of the problems that contemporary psychology needs to address. Elsewhere (Meehan 2009), I have argued that the solution cannot be found in compromise between the two, limited and thus flawed approaches, and Held's critique makes it clear that modification of the postmodern approach is not the answer, either. What is also clear is that both research traditions embody crucial insights. Conventional psychology has, without question, often mistaken situated findings for universal psychological attributes, but it has also produced some extremely valuable work on human cognition (e.g., Miller, Galanter & Pribram

1960), social interaction (e.g., Haney at al. 1973), and psychotherapeutic process (e.g., Weiss at al. 1986). Likewise, the essential corrections to traditional narratives about the history of science that have followed from Thomas Kuhn's (1962) seminal work in the sociology of science have made it quite clear that, in many of its aspects, science, including scientific psychology, is socially constructed. And this insight remains important even though some have used Kuhn's work to advance philosophical agendas that are too far from realism to be compatible with scientific investigation.

The essential point, of course, is that any valid philosophy of psychology would have to be open both to the legitimate findings of conventional research and to the sociology of science. The development of such a program lies beyond the scope of *Psychology's interpretive turn*, which is primarily a critical work. But no critique can be advanced without reference to affirmative tenets, and the constructive aspect of Held's critique suggests some grounds upon which such a task might be attempted. The constructive thrust of Held's argument can be found primarily in the use she makes of the philosophical resources she marshals in the course of her argument. One particularly salient example is her adaptation of Thomasson's work, which defends the ontological status of human-scale *objects* against assertions of the priority of fundamental physical particles. In being used to assert the objectivity of *psychological structures*, these arguments do not lose their importance for the philosophy of physics, and Held, by adapting them to the philosophy of psychology provides a true middle ground: one from which both physicalism and cultural relativism can be subjected to reasoned criticism.

Ultimately, the most important aspect of the philosophical position from which Held confronts the hermeneutic and neo-pragmatic assumptions of both postmodern and middle-ground theorists is her assertion of the validity of reason. Here again, her approach allows for a critique of both physicalists, who mistrust the reasoning that connects observable phenomena to unobservable posits, and postmoderns, who believe reason to be socially constructed: clearly a more legitimate middle ground between the two extremes than could arise from either compromising between the two or from modifying either. And, once again, the strategy that allows her to establish this ground is the application of philosophic approaches that are different from those of either the positivists or of the postmoderns – in this case Pols' insight that human understanding is not to be reducible to either universals or purely existential situated elements.

As noted above, Held's book is not an easy read. No book that poses a fundamental challenge to such an important an intellectual project as that of her subjects could be easy and Held's task is complicated by the variety of different approaches taken by her middle-ground theorists. Her treatment of this diversity is extremely generous in its attention to specific arguments of the various authors and in the profusion of extended quotations which allow the reader to encounter the opinions she discusses in their author's own words. Held's own argument is profound and richly suggestive of alternative solutions to a long-standing problem in the theory of psychology. Though complex, the book is well worth the effort, as both the arguments she addresses, and the position she develops, are important ones.

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Book Review

Phenomenology and Psychological Science. Historical and Philosophical Perspectives

Edited by Peter D. Ashworth and Man C. Chung
Springer, Heidelberg, 2006

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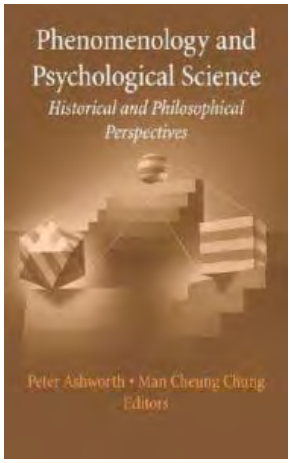

The relations between the phenomenological tradition and the psychological sciences have a long and articulated history. In some cases the two have been said to represent alternative and incompatible approaches to the understanding of human nature. In others the impact of phenomenological thinking on psychology has been fully acknowledged both from a thematic and from a methodological point of view. The contributions collected in *Phenomenology and Psychological Science. Historical and Philosophical Perspectives* (edited by Peter D. Ashworth and Man C. Chung for Springer Verlag) aim to shed light on such complex relations both from a historical and from a philosophical point of view.

The questions animating the volume are: What should we consider to be the initial contact between phenomenology and psychology? What is the place of phenomenological theories in psychiatry? In which cases did research in psychology consider at least some aspects of the phenomenological tradition? What is the value of phenomenology for psychology? How is phenomenological psychology carried out? How did Husserl's thought influence psychology? What are the differences between Sartre, Heidegger and Merleau-Ponty with respect to how philosophy should treat psychological sciences?

The several contributions can be divided up into five main groups. The first copes with the initial contact between phenomenology and psychology; the second focuses on methodological issues of both phenomenology and psychological sciences; the third addresses Husserl's idea of transcendental subjectivity; the fourth is about the critiques moved to the Husserlian phenomenology by his followers; finally, the fifth group of essays tackles conceptual issues coming from the contemporary existential approaches to psychological problems.

In his contribution (*The Meeting between Phenomenology and Psychology*) Ashworth sets the scene for the analysis of the initial contact between phenomenological philosophy and psychological sciences, the common conceptual element between the two being the focus on what it means to have experience of something. Despite their main differences, regarding mostly the role of phenomenological reductions as methodological tools and the role of the world in the understanding of human nature, Ashworth describes both the Husserlian and the Heideggerian approaches as being transcendental in nature. He finally discusses the relationships between phenomenology and some developments in psychology both in American and in German contexts.

Amedeo Giorgi (*The value of Phenomenology for Psychology*) draws a comparison between Husserl's conceptualization of consciousness as essentially intentional and the way early psychologists treated the same topic. He focuses in particular on structural psychologists as Wundt and Titchener, on functionalism, and on James' psychology.

Barbro Giorgi (*Can an Empirical Psychology be drawn from Husserl's Phenomenology?*) discusses how a phenomenological psychology should be carried out and addresses the methodological problems of such an enterprise. The main aim, she argues, should be making explicit the participant's lived psychological meanings. This should be accomplished not through the experimental testing of causal hypothesis but rather through the elucidation of experience. Giorgi finally shows how the main concepts of phenomenological philosophy (i.e., intersubjectivity, intentionality, lifeworld) and the practical research methods applied by phenomenological psychology can be dealt with together in a unique analytical framework.

Dahlberg's contribution (*Did Husserl Change His Mind?*) addresses one of the most controversial themes in the Husserlian phenomenology, namely, the way Husserl conceived of transcendental subjectivity. The focus is here on how we can have access to consciousness through phenomenological reduction. This is a fundamental topic for phenomenology because, through phenomenological reduction, consciousness establishes a distance both between itself and the world and between itself and the natural attitude.

MacDonald (*Husserl against Heidegger against Husserl*) addresses some interesting criticisms Heidegger moves to Husserlian phenomenology. Heidegger, according to MacDonald, pinpoints the following four flaws in Husserl's thought: (i) its over-theorization; (ii) its over-intellectualization; (iii) the splitting of the ego between a transcendental and an empirical ego; (iv) and the separation of consciousness and world by an abyss. Heidegger's criticisms, it is argued, contributed to strengthen the relationship between phenomenological philosophy and psychological sciences. In other words, they provided the phenomenological approach with conceptual and analytical tools that make it available for psychological use.

The two main questions that Groth addresses in her article (*The Influence of Heidegger on Sartre's Existential Psychoanalysis*) are the following: What was Sartre's contribution to psychology? What is the influence that Heidegger exercised on Sartre's thought? As to the first one, Groth focuses above all on the fact that, according to Sartre, human beings are not a cluster of functions and that they constitute rather an entire whole, which we cannot grasp because of our going through a continuous change. As to the second, notwithstanding the big influence of Heidegger on Sartre, there is a rather important difference between the two thinkers. Groth argues, in fact, that the main difference can be traced back to different ways of conceiving the historicity of human beings. While the Heideggerian *Dasein* is fully historical in nature, the same cannot be said about Sartre's humanistic notion of human being.

The two last contributions by Jenner (*Medard Boss' Phenomenologically based Psychopathology*) and Comb (*Contemporary Existentialist Tendencies in Psychology*) are dedicated to a discussion of *Daseinanalysis*. The first addresses the development of Boss' existential psychology. In fact, Boss nurtured a view of psychiatry that goes beyond simplistic medical axioms. The second, instead, focuses on practical issues concerning how both existential psychology and psychiatry clarify the structure of personal experience.

Phenomenology and Psychological Science. Historical and Philosophical Perspectives finally succeeds in conveying to the reader both the complexity and the intricacy of the relations between the phenomenological tradition and psychological sciences. In fact, it makes clear that, if we want to grasp the relevance of the phenomenological tradition to psychology, we have to work through carefully the texts and debates animating the phenomenological movement. The general take of the book, though, does not seem to encourage a constructive dialogue between phenomenologically oriented ways of investigating human nature and the complex epistemic landscape representing the current status of psychological research. On the

contrary, the editors tend to stress the differences between the two approaches and to prevent the achievement of a more comprehensive and fine-grained dialogue among them.

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Book Review

Psychodynamic Therapy: Conceptual and Empirical Foundations

Steven K. Huprich
Routledge, New York, 2009

V. Barry Dauphin[*]
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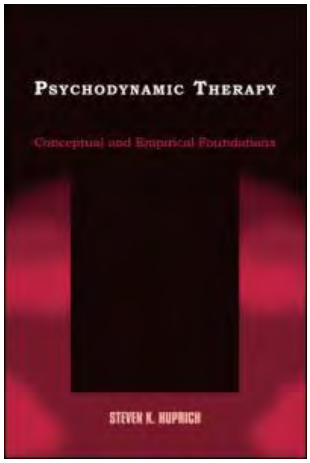

The climate in American universities is very conflicted with respect to the teaching of psychoanalytic principles. Over the last several decades the influence of psychoanalysis has shrunk in psychology departments, psychiatry training programs and schools of social work, and it has been replaced with cognitive behavioral therapy principles and biological-medical models of pathology. At the same time psychoanalysis has flourished in the humanities (e.g., in Departments of English, History, Cultural Studies, etc.), and, as such, it is viewed with suspicion by the insurance industry and by empirical researchers. *Psychodynamic Therapy: Conceptual and Empirical Foundations* by Steven K. Huprich, Ph.D. is thus a needed antidote, to borrow a biological metaphor, to the waning influence of psychoanalysis in the teaching of the helping professions.

Huprich's book is a valuable text for its ability to a) discuss and integrate a number of theoretical differences among various psychoanalytic theories in a concise and clear way, b) provide a broad based review of empirical literature that not only supports psychoanalytic principles but shows that psychoanalysis is actually more likely to serve as a comprehensive theory to bridge basic research in the neurosciences to theories of personality, the unconscious, and therapeutic action. This text is intended for beginning therapists and is an extremely useful text for those studying to become therapists because it as well written, does not assume prior familiarity with psychoanalytic principles, and nonetheless is comprehensive without being pedantic.

The book is divided into three main sections: Theoretical Underpinnings, Treatment Principles and Empirical Support, Therapeutic Process, and it begins with the presentation of two cases from Huprich's work. These clinical encounters are used to illustrate the concepts discussed throughout the book. They are interesting cases because they could easily be viewed through an overly medicalized lens and be seen as little more than a collection of symptoms to be eradicated except that Huprich provides the reader with the stories of their lives as heard by a psychoanalytic listener. That which a medical-biological point of view would understand and treat as a passive disease process, Huprich makes intelligible via a psychoanalytic understanding of the irrational.

For example, one case involves a 19 yr. old high school dropout who had developed «severe bouts of panic that had come upon him about 8 months earlier for no apparent reason» (p. 3).

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Under many contemporary treatment guidelines in the United States, the assessment of the individual would largely involve a combination of the history of symptoms and use of questionnaires designed to measure quantitatively the anxiety level of the individual. It would be standard practice to initiate medical intervention in the form of anti-anxiety medication as well as Cognitive Behavioral Therapy (CBT) focused on dispelling his irrational fears as quickly as possible. Rapid symptom reduction would be of paramount concern. Treatment would likely be quite didactic and directive. Instead, Huprich provides a context for listening to what might otherwise be thought of as irrelevant background noise. Via a listening process that does not direct the patient into the preferred, predetermined areas of interest to the clinician, we learn that this young man has suppressed a great deal of anger at his parents who were pushing him to become more independent and yet he was simultaneously fearful of losing them (as his initial panic attack was unconsciously associated with the death of his favorite uncle). A disease of anxiety instead is revealed to be a story of covert rage and a struggle to become a responsible adult. Huprich illustrates that his symptoms hold meaning and that understanding the meanings of his symptoms could provide insight into his experiences and relief for him. Huprich holds open the possibility that the young man could think of himself as an agent and not as a victim.

Following the presentation of cases, which illustrates a psychoanalytic method to understanding individuals, Huprich discusses theoretical underpinnings of psychoanalytic work. Beginning with Freud and continuing through ego psychology, object relations, and self psychology, major concepts in psychoanalysis are discussed in a way that beginning therapists could understand. For those more familiar with psychoanalysis, this brief review of theory reads in a very progressive manner over the history of psychoanalytic thinking. Substantial attention is given to Heinz Hartmann, Jacob Arlow and Charles Brenner, Margaret Mahler, Edith Jacobsen, Melanie Klein, Michael Balint, Donald W. Winnicott, William R. D. Fairbairn, Harry Guntrip, Harry Stack Sullivan, and Heinz Kohut.

The book provides a sequence of discussing issues that have the potential to be understood as part of a whole, rather than as disconnected or merely contradictory theoretical viewpoints in psychoanalysis. The book makes areas of conflict quite clear, but the effect of reading through concise descriptions of the theories is that it helps articulate commonalities more clearly. Because this text is aimed at beginning level therapists, this approach is effective. Students can and should delve into more detail in subsequent portions of their training. In fact those who well versed with psychoanalysis would benefit from this refresher of historical debates in psychoanalysis.

Huprich continues to display the evolution of psychoanalysis through attempts at integration, such as via Kernberg, and further extensions of psychoanalytic thinking with discussion of analytic/introjective configurations in the development of pathology, sadomasochism, attachment theory, mentalization and reflective functioning, intersubjectivity and co-constructed reality in psychotherapy, and cognitive experiential theory. Thus, the book captures the influence of relational work in contemporary American psychoanalysis. As such, it is best to understand this book as grounded on the work of theorists who have been most influential in American psychoanalysis. The book does not cover Lacan, Kristeva, Matte-Blanco and others who have been influential outside of America. In the United States many contemporary European and South American thinkers would be considered part of advanced or specialized studies in psychoanalysis. Lacan, for example, has been quite influential in American humanities departments compared to education directed at training mental health professionals.

In the second section Huprich provides an overview of basic principles involved in psychodynamic therapy. He addresses a variety of modalities and views psychoanalytic work as part of a continuum. He explicitly discusses that for purposes of this introductory text he uses the terms *psychoanalytic* and *psychodynamic* interchangeably, while noting the nature of conflicts in the field about the usages of these terms. He discusses the goals of psychodynamic work and the nature of the therapeutic alliance, free association, transference/ countertransference, interpretation and resistance. Following this he explores empirical studies in psychoanalytic work, especially focused on the therapeutic aspects he had just discussed.

Thus, a beginning student can see an organized presentation of basic psychoanalytic principles followed by a discussion of empirical research relevant to those principles. Research on various elements is described (e.g., on transference, countertransference, etc.) followed by a chapter concerning research into the global process and outcome of psychoanalytic work. Huprich concludes the section on treatment principles and empirical support with a chapter on cognitive neuroscience, with particular attention to research involved in unconscious processes.

In order to make this work intelligible, he discusses some basic concepts of cognitive neuroscience. At first blush a discussion of a concept such as attention appears far removed from the work of psychoanalytic therapists, but Huprich is able to explicate the relevance of basic research in information processing and neuroscience to psychoanalytic theory. Academic research into implicit and explicit memory and connectionist models of memory might provide empirical grounding for unconscious mental processes. In fact it is not possible to understand human information processing without models of unconscious processing. Huprich summarizes research exploring such links. For example, the work of Westen and Gabbard (2002) suggests a relationship between transference and the findings of cognitive neuroscience, especially that most representations are multimodal and exist as potentials for activation. Representations exist in multiple networks along with different affects and motives. Unconscious procedures manage emotions, which could be thought of as analogous to defenses. While conscious representations are some of the representations that are activated, other representations are activated, which are not conscious. Consciousness operates via serial processing, while the parallel processing system is important to understand behavior does not operate at a conscious level.

The final section explicates the therapeutic process of psychoanalytic work and uses a specific case in depth to address the various principles discussed in the book. This section begins with some discussion of the Psychodynamic Diagnostic Manual (PDM), which was published in 2006. An alternative to the DSM-IV-TR, the PDM offers a more comprehensive format for assessing individuals than the DSM-IV-TR. The assessment process via the PDM is rich and grounded in psychoanalytic principles, including descriptions of various disorders and a far more extensive and nuanced assessment approach to understanding the problems of children than the DSM series has ever offered. Huprich illustrates how the PDM can serve as a starting point for assessing individuals for psychoanalytic work and covers various aspects of an assessment process, including biological and temperament factors, life situation, personality organization, defenses, ego functioning, object relations, self representations, esteem & agency, insight and reflective functioning and sociocultural factors. An understanding of personality is considered essential in any psychoanalytic assessment, which is divergent from the DSM approach of de-contextualizing symptoms.

The book concludes with a rich case illustration from Huprich's work. Huprich discusses a young man who presented with depression and relationship complaints. He weaves this man's life story together with psychoanalytic principles discussed throughout the book. He illustrates

how a psychodynamic understanding provided him with coherence to the case that would not have been possible when thinking in a more medical framework.

This book provides beginning therapists and those who teach psychotherapy a clear, concise and thoughtful introduction to psychoanalytic principles. It would be helpful reading for any who have little familiarity with psychoanalysis to begin learning to become acquainted with psychoanalytic principles. This would also be a useful text for students who are in training programs that are not psychoanalytically oriented as well, because of the integration of theory, research and practice.

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Book Review

Doing Without Concepts

Edouard Machery

Oxford University Press, Oxford, 2009

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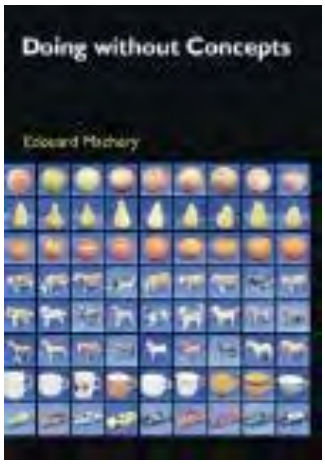

Edouard Machery's book is a book-length discussion of the thesis that «the notion of concepts ought to be eliminated from the theoretical vocabulary of psychology» (p. 4). This thesis is the conclusion of a five-step argument, which Machery calls the *Heterogeneity Hypothesis* (HH). The book is structured as follows: the central chapters (4-7) are a justification of the four main assumptions made by HH, which are introduced in the third chapter; the first two chapters are introductory to concepts, while the last one discusses the eliminativist conclusion.

The first two chapters explain what the notion of concept means respectively in psychology and philosophy. In Chapter 1, Machery firstly distinguishes between higher (categorization, deduction, induction, analogy-making, linguistic understanding, planning) and lower (perception, syntactic parsing motor planning) cognitive processes. Then, he provides a (psychologically interesting) notion of knowledge as «any contentful state that can be used in cognitive processes» (p. 8). Finally, he defines the notion discussed through the whole book:

> A concept of *x* is a body of knowledge about *x* that is stored in long-term memory and that is used by default in processes underlying most, if not all, higher cognitive competences when these processes result in judgements about *x*.[1] (p. 12)

Psychologists usually assume that «the processes underlying different cognitive competences […] use the same bodies of knowledge» (p. 16), and try to find out the general properties common to every concept. These properties concern: (i) the kind of knowledge stored in concepts (i.e., whether they store knowledge for individuals, categories or other kinds of causal relations); (ii) the representational format of concepts (i.e., whether they are amodal symbols, images or something else); (iii) the cognitive processes in which concepts occur; (iv) the possibility to acquire them; and (v) their neural localization.

Machery concludes the chapter by challenging two different definitions – i.e., concepts as temporary bodies of knowledge in working memory (Barsalou 1993), and concepts as representations under organismic control (Prinz 2004, Dennett 1993, 1996). He also discusses two alternatives consistent with his proposal, but less appealing. Namely, the definition of concepts as the constituents of thought is obscure and plays little role in the experimental psychology of concepts. Instead, the definition of concepts as categorization devices is too narrow, and leaves outside other important cognitive competences.

[1] Reference to the *default* conditions allows distinguishing knowledge stored in concepts from «background knowledge» (p. 11) associated with those concepts. Gradual factors such as the frequency of the association and explicit teaching determine the difference between the two kinds of knowledge.

In Chapter 2, Machery distinguishes psychological from philosophical theories of concepts, according to which «having a concept of *x* is being able to have propositional attitudes about *x* as *x*» (p. 32). He notes that, given the diversity of the two notions, psychological and philosophical theories of concepts have entirely different goals, and «there is little point in evaluating psychological theories of concepts according to the criteria used to evaluate philosophical theories» (p. 37), or vice versa. Philosophical and psychological theories of concepts have little in common, and must be evaluated independently one from each other.

Therefore, Machery challenges two different accounts that might be advanced to fill the gap between philosophical and psychological theories. According to the first proposal, explored by Peacocke (1992), philosophers should define the possession conditions for a concept while psychologists should explain how people can meet those conditions. Machery notes against it that Peacocke does not suggest any clear method to investigate how to spell out the possessing conditions of a concept. The second proposal, the "Foundationalist Account", states that philosophers should identify the conditions (presupposed by psychologists) that «people must meet in order to have propositional attitudes about the object of their attitudes» (p. 48). In this case, Machery notes that it is dubious whether there are constants in the attribution of propositional attitudes. Indeed, a study by Hewson (1994) showed that people disagree about the beliefs to attribute in front of the same situation.

In Chapter 3, Machery proposes the Heterogeneity Hypothesis (HH) against the commonly shared view that «concepts share many scientifically relevant properties» (p. 54). In detail, HH affirms that:

1. for each category an individual typically has several concepts[2];
2. coreferential concepts have very few properties in common;
3. prototypes, exemplars, and theories are among these heterogeneous kinds of concepts;
4. prototypes, exemplars, and theories are typically used in distinct cognitive processes;
5. the notion of concepts ought to be eliminated from the theoretical vocabulary of psychology (p. 52).

The chapter is then devolved to pave the way to the first two tenets. Machery contrasts HH against weaker proposals, namely Scope Pluralism, according to which different kinds of concepts (prototypes, exemplars, theories) are associated with different types of entities, and Competence Pluralism, according to which different kinds of concepts are specific to different cognitive processes. Against the former, HH claims that most categories are represented at the same time by different kinds of concepts while against the latter it claims that concepts do not vary across cognitive competences. Then, Machery criticizes hybrid theories of concepts, according to which concepts are composed from different parts necessarily linked together in a non-contradictory way. He contends that hybrid theories have a hard work in explaining «what is meant by claiming that several bodies of knowledge are the parts of a single concept, in contrast to being distinct concepts» (p. 64). Moreover, they make the empirically disconfirmed assumption that different parts of concepts cannot lead to inconsistent categorization judgements. HH, instead, does not negate that bodies of knowledge can be connected, but affirms that this can happen only as a contingent matter of fact. Moreover, it

[2] That is, several bodies of knowledge stored in long-term memory and used by default in higher-level cognitive processes.

predicts the empirically tested hypothesis that these different parts will lead sometimes to conflicting outcomes.

In Chapter 4, Machery describes in depth well-known paradigms of concepts, that is, the prototype, the exemplar and the theory paradigm. He also critically assesses the Neo-Empiricist view of concepts (Barsalou 1999, Prinz 2002). According to it, the knowledge stored in a concept is encoded in several perceptual – therefore, modally determined – representational formats; moreover, conceptual processing involves re-enacting and manipulating some perceptual states (p. 109). Against the former assumption, Machery points out that empirical results often provided by neo-empiricist theorists are inconsistent with some, but not all, amodal models of cognitive processes. Therefore, those results do not deny the general claim that concepts are amodal symbols. With respect to the latter claim, Machery notes that evidence that perceptual simulation is used to solve cognitive tasks does not support neo-empiricist theories over amodal theories, because even amodal theories recognize the importance of modal imagery. Therefore, «there is no strong evidence that concepts (or some concepts) are in fact similar to perceptual representations» (p. 116). Neo-empiricist theories of concepts are consequently set aside.

Then, Machery argues that prototypes, exemplars and theories store different kinds of knowledge so that, «consistent with the Heterogeneity Hypothesis, most categories could be represented by a prototype, a theory and a set of exemplars» (p. 119). Since the processes involving prototypes and exemplars rely on the computation of similarity between bodies of knowledge, while the processes involving theories rely on inference to the best explanation, his conclusion is that «when we reason, categorize, and draw analogies, we use three different kinds of cognitive processes» (p. 119).

In Chapter 5, Machery paves the way to his explanation of why prototypes, exemplars, and theories are typically used in distinct cognitive processes by contrasting what he calls the Unified View of Cognition with multi-process theories. According to the former, each cognitive competence (e.g., categorisation) is the outcome of a single cognitive process, while multi-process theories assume that «a cognitive competence is underwritten by several cognitive processes, each of which accesses a specific kind of concept» (p. 121). While the Unified View of Cognition focuses on cognitive competences, multi-process theories consider also the cognitive processes underlying a competence. Consequently, they are more interesting in order to understand the computational properties of the mind. Multi-process theories may be differentiated according to (i) whether the processes that they presuppose always run simultaneously as opposed to in particular situations, and to (ii) whether the outputs of different processes in the case of parallel running are merged or not. They can call on three kinds of evidence. The first kind is based on specific performance profiles in different experimental tasks. The second kind of evidence reinforces such results by showing that different processes produce outputs that reinforce each other in some situations and conflict in others. Finally, functional dissociations of cognitive processes might provide a third kind of evidence for multi-process theories. The chapter ends with a presentation of some examples of multi-process theories: the model of explicit and implicit cognition of Ashby et al. (1998), the dual-process theories of cognition (e.g., Stanovich and West 2000), and the fast and frugal heuristics research program (Gigerenzer et al. 1999).

In Chapter 6, Machery provides empirical results to show that categorization and concept learning are underwritten by distinct cognitive processes, each of which involves its own kind of concepts. He firstly defines categorization and concept learning, and discusses critically the methodology of experiments in psychology about these two different competences. Then, he reviews the empirical evidence that supports the prototype, the exemplar and the theory paradigms of concepts. Showed that we have different mechanisms for categorizing objects,

events, and substances that rely on different kinds of concepts, he surveys the empirical literature and founds that there is little evidence that these three processes are organized together. Indeed, «in experimental conditions, the three categorizations processes can be triggered selectively» (p. 194), depending on differences in the presentation of the input stimuli, and in the kind of output requested. Moreover, empirical studies (Allen & Brooks 1991) suggest that categorization tasks sometimes lead to conflicting categorization judgments. Therefore, not only we possess several kinds of concepts, but these kinds also involve distinct processes for learning and for categorizing.

Chapter 7 discusses other two important cognitive abilities, namely, induction and concept combination, and suggests that empirical research supports the conclusion that distinct cognitive processes underwrite also these cognitive competences. With respect to induction, there is emerging consensus between psychologists that people rely on several non-integrated induction processes. In the case of concept combination, Machery refers to Hampton's (1987) model to claim that prototypes, exemplars, and theories «are retrieved from long-term memory to create complex concepts» (p. 212). He also notes that Hampton's (1987) model does not assume a multi-process theory, but considers instead a single process to be responsible for the combination of different kinds of concepts. Finally, Machery scans neuropsychological data in search of dissociations attesting distinct processes for induction and concept combination. However, he does not find decisive results supporting his thesis.

Provided that the processes responsible for categorization, induction, concept learning and concept combination rely on different kinds of concepts, Machery dedicates Chapter 8 to the eliminativist conclusion of HH. He firstly challenges two kinds of eliminativist arguments for concepts: the anti-representationalist argument coming from the dynamical-systems approach (Thelen & Smith 1994) and situated robotic (Brooks 1999), and the argument from context-sensitivity (Smith & Samuelson 1997). These two arguments share the same structure: they firstly provide a theoretical definition for concepts, then they claim that as a matter of fact the best empirical evidence suggests that nothing satisfies the previously proposed definition. Therefore, they conclude that concepts do not exist. Machery notes that such a conclusion depends on a descriptivist theory of reference, according to which, if a term stands for a definition that is not referring to anything real, the term is empty and the generalizations made with it are false. However, if one supports a different theory of reference (e.g., a historical causal theory) the eliminativist argument loses much of its appeal.

Given that «concept eliminativism should not be hostage to the debate about reference» (p. 230), Machery hence introduces a kind of "scientific" eliminativism, which does not dispute whether concepts refer to an empty class of entities, but whether they «possess the properties that characterize the classes that matter for the empirical research» (p. 230). With this respect, Machery claims to have showed that «very few generalizations are true of all (or most) concepts besides the properties that are used to identify them, while many generalizations are true of some subset of concepts» (p. 241), namely prototypes, exemplars and theories. His conclusion, then, is that «the class of concepts is not a natural kind, while these subsets are natural kinds» (p. 241). Machery argues that eliminating concepts from the vocabulary of cognitive science will positively affect empirical research by eliminating the controversies between theorists of different paradigms. Moreover, it will drive researcher to focus on open questions such as the best models for prototypes, exemplars, and theories, how multi-processes run in different cognitive competences, and how their specific outputs are eventually processed.

Machery's book shows a remarkable mastering of the huge literature about concepts coupled with the capacity to summarize it in a reasonable number of pages. Much effort is put

in explaining different theories, therefore in developing a common framework where the strengths and flaws of each of them can be compared. Moreover, since Machery does not aim to support some theories over the others, his analysis is at the same time critic and impartial. For all these reasons, Machery's book is a very good introduction to the topic.

Furthermore, Machery conveys a particular eliminativist conclusion about concepts, which denotes a genuine philosophical attitude away from armchair philosophy. He never forgets to remind the reader the empirical nature of his argument. Concepts should be eliminated from the vocabulary of psychology not because they refer to an empty category but because the very term is misleading with respect to its use in the empirical literature. Machery is able to select and to report the results relevant to his discussion, always showing the specific contribution of each to his main thesis. Therefore, even though his conclusion might be rejected by the development of our knowledge about cognition (he recognises, for example, that the development of neo-empiricist views of concepts might provide good reasons why to revise his conclusion), his argumentation remains a succeeded example of how philosophy should apply to the scientific practice. That is, Machery's book is a bright example of clear analysis applied to empirical investigation, and it is strongly recommended to every one – philosopher or psychologist – interested in cognitive science.

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Commentary

Modelli epistemologici in psicologia

a cura di Marco Castiglioni e Antonella Corradini
Carocci, Roma, 2008

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Il saggio da rileggere, *Modelli epistemologici in psicologia. Dalla psicoanalisi al costruzionismo* di Marco Castiglioni e Antonella Corradini, Carocci editore, del 2003, è in realtà un manuale universitario con finalità essenzialmente didattiche, quindi pensato ed organizzato per offrire allo studente un quadro esauriente dello stato dell'arte dei fondamenti epistemologici della psicologia. Opera di due esperti del settore, uno psicologo clinico ed una filosofa delle scienze umane, si presenta come un agile libriccino di poche pagine. Ma l'aspetto non deve ingannare: si tratta di un saggio denso ed esauriente, scritto in uno stile limpido ed estremamente scorrevole, semplice e preciso allo stesso tempo. La struttura dei cinque brevi capitoli in cui vengono esposti i principali modelli generali si articola in due parti distinte: una prima parte dedicata ad una breve "esposizione della teoria" e una seconda parte dove gli autori presentano le loro "considerazioni epistemologiche". Il testo si chiude con un sesto capitolo con le conclusioni ed è arricchito da una bibliografia aggiornata e da due indici, uno analitico e l'altro dei nomi, ambedue molto utili.

La chiarezza e la sintesi, come già detto, sono le caratteristiche più appariscenti dell'opera ma non è da sottovalutare neppure la notevole ampiezza del panorama offerto al lettore: in circa 150 pagine tutt'altro che pesanti vediamo scorrere un secolo abbondante di storia della ricerca psicologica e tutte le sue principali connessioni con la filosofia, l'antropologia, la sociologia, la ricerca medico-neurologica fino alle recenti neuroscienze.

I cinque capitoli suddetti sono dedicati rispettivamente ai grandi paradigmi che hanno dominato la scena della cultura mondiale e che ancora si contendono, alcuni in particolare, un ruolo egemone nella ricerca contemporanea. Essi sono, nell'ordine, il *comportamentismo*, il *cognitivismo*, il *costruzionismo sociale*, l'approccio *sistemico relazionale* e la *psicoanalisi*. I primi tre vengono presentati in un ordine che potremmo considerare anche genericamente storico, negli ultimi due l'aspetto che viene più evidenziato è quello clinico-applicativo. Non possiamo certo qui entrare nei dettagli e seguire criticamente l'esposizione che i due autori fanno degli aspetti teorici e delle problematiche connesse con ciascun paradigma; quello che possiamo dire è che, a parte qualche piccola imprecisione, il testo è in genere esauriente per lo scopo per cui è stato pensato ed è sufficiente ad illustrare gli aspetti su cui gli autori si soffermano poi nelle considerazioni epistemologiche.

Il fatto che questa ampia panoramica coinvolga un gran numero di altre discipline e le loro mutue relazioni, non fa che aggiungere pregevolezza al saggio e ne testimonia l'importanza come base di partenza per una ricognizione dello stato attuale dell'arte. Gli aspetti filosofici, antropologici, scientifici e storico-sociali delle problematiche affrontate si intrecciano sia sul versante teorico che su quello clinico-pratico, offrendo un quadro davvero completo della

situazione al momento della pubblicazione A distanza di sei anni, questo volume è comunque ancora meritevole di essere riletto, al di là dei suoi pregi intrinseci, come appena detto.

Cominciamo col notare che dal 2003 ad oggi la situazione generale della ricerca psicologica in tutte le sue articolazioni è notevolmente cambiata. Ricerche che già allora avevano raggiunto importanti risultati sperimentali (ma che non potevano, ovviamente, comparire in un manuale universitario di orientamento generale) sono state confermate e altre hanno aperto strade ancor più nuove ed interessanti. Le neuroscienze in particolare e, ancor più in particolare, le tecniche di indagine strumentale dell'attività cerebrale a livello sia macro che micro, stanno offrendo un'immagine completamente nuova di fenomeni che fino a solo pochi anni fa sembravano o totalmente misteriosi o comunque fuori della portata dei nostri laboratori. Voglio qui ricordare, per dare solo qualche esempio, le ricerche di Kandel sui processi della memoria, quelle di Libet sullo scarto temporale nei processi decisionali, quelle del gruppo di Parma di Rizzolatti sui neuroni specchio, quelle di Baron-Cohen sull'autismo, quelle infine sempre più numerose e profonde sullo statuto ontologico e sulla genesi della coscienza.

Questi ultimi anni hanno visto rinascere ed estendersi, per ragioni molteplici e non solo di carattere scientifico, il dibattito (e le conseguenti polemiche) sulla teoria dell'evoluzione, sul suo significato, sulla sua applicazione ad altri campi della ricerca e sulle conseguenze che ne derivano, sia dal punto di vista scientifico che da quello più squisitamente filosofico e generale. In un clima culturale sempre più incandescente per le vivaci discussioni, soprattutto politico-ideologiche, ci siamo resi conto che l'inizio del terzo millennio sarà, con molta probabilità, caratterizzato dalla doverosa presa di coscienza che molte teorie, concetti e nozioni a cui eravamo un po' affezionati, dovranno essere riviste se non del tutto abbandonate.

Qualcuno comincia anche a suggerire che sia necessario rimediare al pericoloso ritardo con cui certe conquiste del pensiero novecentesco ancora non sono patrimonio della cultura comune, neppure a livello scolastico: la relatività einsteiniana, la meccanica quantistica, la ricerca matematica contemporanea, la linguistica formale o la biologia molecolare, tanto per fare solo qualche esempio tra i più rilevanti.

Sostiene G. Jervis in uno dei suoi ultimi saggi:

> In questi anni la psicologia è esplosa in una miriade di problemi, di temi, di specializzazioni e non ci fornisce dati univoci. Le carte sono state rimescolate, al punto che molte acquisizioni recenti non sono merito di psicologi ma di zoologi, biologi e genetisti, studiosi del cervello, linguisti, sociologi, economisti. La definizione stessa della disciplina "psicologia" è stata messa in discussione. (Jervis 2007, p. 73)

E prosegue:

> In pratica, il moltiplicarsi delle ricerche e l'evoluzione delle idee sono talmente intensi e rapidi che nessuna scuola di pensiero fa in tempo a consolidarsi. Le acquisizioni degli ultimi decenni sono notevoli. (Jervis 2007, p. 73)

E ancora:

> Da qualche tempo tutto è cambiato: la ricerca di laboratorio si è impadronita di campi che in precedenza non sembravano di sua competenza, e ne sono emersi dati sorprendenti. (Jervis 2007, p. 73)

Jervis, da studioso raffinato e sensibile qual era, aperto alle novità e alle contaminazioni che si vanno moltiplicando nel panorama della ricerca internazionale, aveva colto, in questi anni a

cavallo tra i due millenni, il senso profondo di un modo nuovo di fare scienza e cultura: tentare di capire meglio attingendo a tutte le fonti disponibili e usando ogni metodo o strumento senza preclusioni o pregiudizi.

> Abbiamo finalmente imparato ad essere meno schematici – prosegue Jervis – […] abbandonando l'alternativa rigida *nature versus culture*, oggi siamo meno interessati a chiederci quale sia il peso rispettivo dei condizionamenti biologici in contrapposizione alle influenze storico-ambientali; siamo invece più interessati ad esaminare le loro interazioni, cioè la sintesi di natura e cultura. (Jervis 2007, pp. 73-74)

Come molte altre discipline, la psicologia sta dunque rifacendo "i conti con se stessa" e in questo nuovo clima generale non solo i metodi, non solo le applicazioni, non solo la clinica hanno bisogno di essere aggiornate o totalmente ripensate ma anche le basi concettuali, i principi ispiratori e i modelli di riferimento debbono essere riesaminati criticamente. La ricerca scientifica, la ricerca in generale, anche quella artistica, per esempio, vivono momenti di passaggio a volte rapidi e caotici, situazioni di crisi profonda che sembrano sommergere o distruggere gran parte dell'esistente, sono i famosi periodi di "scienza straordinaria" di Khun. La psicologia insieme a molte discipline afferenti, in questo momento, sta vivendo probabilmente una di queste situazioni di crisi. Ma io sarei ottimista, discutere e dubitare fa bene alla ricerca, evita irrigidimenti e incrostazioni, produce sempre qualcosa di nuovo, riduce di molto la presunzione dei dogmatici.

Curiosamente, sembrerebbe quasi di ritornare indietro nel tempo e rivivere il solito dramma delle origini: una disciplina alla ricerca della sua identità, il bisogno di una rinnovata legittimazione "scientifica", la scelta di un riferimento sicuro, l'individuazione di nuovi metodi, un nuovo approccio ai "vecchi" problemi, etc. In breve, una storia già vista.

Mi sembra però che questa sensazione di dejà-vu possa essere superata dalla constatazione, per altro evidente, che molte proposte sono state abbandonate proprio perché si sono dimostrate inadeguate o incapaci di produrre i risultati attesi, o hanno fallito rispetto alle loro stesse previsioni. Come caso esemplare vorrei citare soltanto il grande progetto della Intelligenza Artificiale, quella cosiddetta "forte", che si prefiggeva, nei sogni dei suoi entusiastici sostenitori, non solo di simulare ma di riprodurre "comportamenti" umani ritenuti "intelligenti". Ricordo, in particolare, il progetto giapponese dei computer di "5° generazione", che avrebbero dovuto elaborare, traducendolo automaticamente, il linguaggio umano e funzionare con comandi trasmessi direttamente con la voce umana: già nel 1985 fu dichiarato essere irrealizzabile.

Dunque non è vero che non c'è niente di nuovo sotto il sole, che la storia "si ripete", come a qualcuno piace ripetere! Ritengo invece, se è vero che la ricerca scientifica consiste nella "esplorazione dei vicoli per vedere se sono ciechi", che la situazione attuale si presenta molto stimolante. In effetti molti vicoli si sono dimostrati ciechi e di questo dobbiamo far tesoro, senza ipocrisie o pietosi autoinganni. E questo è avvenuto in tanti settori della conoscenza, in tante discipline, fino al punto di suggerire l'idea che forse è tramontata per sempre la pretesa di scoprire un piccolo numero di leggi e principi generali che ci permettano una rappresentazione unificata del mondo: la realtà e la natura sono maledettamente complesse e "rifiutano" di adattarsi ai nostri comodi contenitori concettuali. Ciò non significa però che non sia possibile ridurre al minimo e semplificare questo armamentario teorico: una delle strade possibili può essere proprio quella della sintesi tra proposte diverse, incrociando conoscenze e metodi.

Nel campo della ricerca psicologica le ultime acquisizioni delle neuroscienze hanno ormai reso evidente che si può parlare sensatamente di una sorta di "natura umana", cioè di un insieme di caratteristiche bio-genetiche specifiche, stabilizzatesi nel corso dell'evoluzione, di

carattere universale, cioè tipiche di ogni essere umano in quanto tale, al di là delle differenze, molto meno marcate, dovute all'ambiente sociale e alla cultura.

Ma allora, se è stato possibile ridefinire questa certa idea di universalità naturale, possiamo anche chiederci quali siano le condizioni dell'ambiente fisico effettivamente necessarie e sufficienti perché questo si sia potuto verificare. Dobbiamo cioè cominciare a guardare al "mondo naturale" con occhi diversi, raffinando i nostri strumenti di indagine per vedere se, sotto il disordine apparente degli eventi, non si nasconda invece una sorta di tessitura, magari anche finemente strutturata. E questa, infatti, è un'altra grande acquisizione recente: la realtà del mondo naturale non è amorfa, caotica o infinitamente malleabile ma si presenta ricchissima di informazione e di struttura, non è un dato immutabile e indipendente ma interagisce col soggetto conoscente in modo dinamico secondo regole e parametri vincolati solo da leggi naturali universali.

Per studiare a fondo questa nuova immagine, quella che emerge dalle nostre esplorazioni, servono allora strumenti concettuali diversi, strumenti più adatti di quelli disponibili a un dominio così variegato e mobile com'è quello del mondo della vita, quello in cui sistemi altamente complessi e parzialmente stabili interagiscono continuamente tra loro in modo talora caotico. Allora, così come l'invenzione della matematica degli infinitesimi (l'analisi) consentì la crescita e lo sviluppo della fisica newtoniana, così oggi è giunto forse il momento di provare ad "inventare" un linguaggio nuovo per la dinamica delle strutture viventi e auto-organizzate in continua trasformazione. Al momento, il candidato più promettente sembra rappresentato dalla cosiddetta "teoria della complessità" che ha come oggetto lo studio del comportamento dei sistemi dinamici, lineari e non-lineari, in particolare quelli definiti "caotici" o altamente instabili. In effetti, come già accennato, una delle caratteristiche salienti del vivente è proprio quella di presentarsi come sistemi complessi auto-organizzati, emergenti da un ambiente fisico caotico ad alto tasso di instabilità.[*]

Se poi dal generico mondo del vivente passiamo a considerare l'uomo e il suo comportamento, ci troviamo subito davanti un ostacolo formidabile: spiegare "scientificamente" l'intenzionalità, la coscienza e il libero volere, il famoso "libero arbitrio" del millenario dibattito dei filosofi. Credo di poter dire che sono state avanzate in merito a questo problema delle proposte interessanti proprio in questi ultimi anni. Si va da considerazioni che si ispirano di volta in volta alla meccanica quantistica, al fenomeno dell'incompletezza della fisica e della matematica, ai risultati logici sulla non descrivibilità univoca delle entità astratte, ai vincoli cosmologici sulla dinamica del movimento e sulla corporeità, etc.[†]

Ma sono proposte, ovviamente, molto complesse che fanno uso di strumenti concettuali diversi, generalmente inusuali e, per molti aspetti, raffinati, certamente assenti nel bagaglio culturale e professionale della gran parte degli psicologi in attività. È un problema questo che per ora si presenta a livello di ricerca ma che ben presto si tradurrà in un problema di formazione professionale e culturale generale, come capita sempre più spesso. Molti temi infatti appaiono più problematici di quanto non siano in realtà proprio perché l'informazione e soprattutto, la formazione scientifica sono insufficienti o non aggiornate (in certi casi il ritardo

[*] Per una prima introduzione potremmo suggerire la nuova edizione (2007) presso Bruno Mondadori del volume di Gianluca Bocchi e Mauro Ceruti, *La sfida della complessità*, oppure il recente *Sulle Orme del caos* di Gian-Italo Bischi, Rosa Carini, Laura Gardini e Paolo Tenti sempre dello stesso editore (2004); per maggiori dettagli va ancora bene il vecchio Grégoire Nicolis e Ilya Prigogine, *La complessità*, Einaudi, Torino, 1991.

[†] Per tutta la problematica mi permetto di suggerire Francis Bailly, Giuseppe Longo, *Mathématiques et sciences de la nature. La singularité physique du vivant*, Hermann, Parigi, 2006.

è impressionante e interessa purtroppo anche coloro che "fanno scuola"). Ma questa è un'altra questione, molto importante, di cui non possiamo occuparci qui!

Altri aspetti interessanti sarebbero anch'essi pertinenti alle nostre osservazioni sullo stato della ricerca psicologica attuale: l'intreccio sempre più stretto con la filosofia della mente e le conseguenze che certe acquisizioni stanno avendo sulle ipotesi circa l'esistenza e la natura degli "eventi mentali" è solo un esempio, la rinascita di un forte interesse verso l'ontologia e il recupero di alcuni importanti risultati della logica novecentesca, altri due.

Vorrei concludere questo breve commento tornando alla domanda iniziale: perché rileggere questo testo universitario a distanza di sei anni dalla sua pubblicazione? Qual è la sua utilità, qualità oggi molto apprezzata in una scuola attenta alle "competenze"? La mia risposta è, in ultima analisi, molto semplice e discende da quanto sopra argomentato: ci troviamo in un contesto storico e culturale particolarmente vivace nel quale le istanze innovative si fanno più pressanti a fronte di una situazione che presenta molti aspetti di crisi, ossia in cui interi settori della conoscenza, intere aree disciplinari, interi paradigmi sembrano aver esaurito la loro funzione. In poche semplici parole, si sente il bisogno di aprire nuove strade, essendo che molte di quelle vecchie sono giunte al termine: dobbiamo cercare di guardare più lontano.

Un famoso motto, attribuito a Newton, dichiarava che, per vedere più lontano era stato necessario salire sulle spalle di giganti. Ecco, io credo che il testo di Castiglioni e Corradini sia necessario per avere un'idea abbastanza precisa di quali siano stati questi giganti sulle cui spalle bisogna salire se vogliamo provare a vedere davvero più lontano.

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Commentary

L'aroma del cervello. Commento a *le Osservazioni sulla filosofia della psicologia*

Ludwig Wittgenstein
Adelphi, Milano, 1990

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[Q]uello che io voglio dire è, invece:
è tutto qui.
Ludwig Wittgenstein

I due volumi intitolati *Remarks on the Philosophy of Psychology/Bemerkungen über die Philosophie der Psychologie* furono pubblicati da Basil Blackwell a Oxford nel 1980, ben 27 anni dopo la pubblicazione delle *Philosophical Investigations/ Philosophische Untersuchungen*, la prima delle "opere" postume di Wittgenstein, la quale era uscita sempre da Basil Blackwell nel 1953, due anni dopo la sua morte, a cura dei suoi tre esecutori letterari: G. M. Anscombe, R. Rhees, Georg H. von Wright.[1] La cura del primo volume delle *Bemerkungen* è della Anscombe, la quale ne è anche la traduttrice inglese, e di von Wright; la cura del secondo è nuovamente di von Wright, affiancato questa volta da H. Nyman, mentre la traduzione inglese è di C. G. Luckardt e M. A. E. Aune. Si tratta della penultima delle opere tra i cui curatori compaiono uno o più degli esecutori letterari inizialmente indicati da Wittgenstein, l'ultima essendo costituita dai due volumi dei *Last Writings on the Philosophy of Psychology/Lezte Schriften über die Philosophie der Psychologie*, usciti, rispettivamente, nel 1982 e nel 1992, ancora da Basil Blackwell a cura di Von Wright e H. Nyman.[2] La traduzione italiana dei due volumi delle *Bemerkungen* uscirà, a cura di R. De Monticelli, dieci anni dopo l'edizione originale, nel 1990, presso Adelphi di Milano in un unico volume dal titolo *Osservazioni sulla filosofia della psicologia*.[3]

Nel 1980, l'anno della pubblicazione delle *Bememrkungen*, gli esecutori letterari di Wittgenstein erano ben consapevoli del disaccordo che molti studiosi avevano manifestato nei riguardi delle forme, modi e tempi con cui essi stavano facendo conoscere il lascito wittgensteiniano. In particolare, erano stati fortemente criticate (a) la decisione di rinunciare a

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[1] Tradotte in italiano da R. Piovesan e M. Trinchero con il titolo *Ricerche filosofiche*, Einaudi, Torino, 1967 (con diverse ristampe successive).
[2] Tradotti in italiano da B. Agnese e A. G. Gargani in un unico volume dal titolo *Ultimi scritti 1948-1951. La filosofia della psicologia*, Laterza, Roma-Bari, 1998.
[3] Nel seguito citerò (nel testo principale) da questa traduzione: la cifra romana, che indica il numero del volume, sarà seguita dal numero del paragrafo; il numero della pagina si riferirà invece alla premessa dei curatori.

pubblicare il cosiddetto *Big Typescript*[4] e di confezionare al suo posto un'"opera", la *Philosophische Grammatik*[5], che, dal punto di vista filologico, appariva, come aveva dimostrato nel dettaglio A. Kenny[6], a dir poco discutibile; (b) la scelta di aggiungere senza alcuna chiara ed esplicita motivazione alla cosiddetta "Parte I" delle *Philosophical Investigations* una Parte II costituita da un dattiloscritto, andato in seguito perduto, risalente probabilmente al 1949. Più in generale, ciò che con sempre più forza e da più parti si criticava era la pretesa di ritagliare dall'insieme dei manoscritti e dattiloscritti di Wittgenstein delle vere e proprie "opere" a cui si potesse attribuire una qualche significativa, anche se non definitiva, compiutezza. A parere di diversi studiosi ciò che così si produceva era però solo una immagine fortemente deformata del lavoro filosofico di Wittgenstein; molto meglio sarebbe stato pubblicare l'intero lascito wittgensteiniano, senza censure e senza quegli arbitri filologici da cui era nata la *Philosophical Grammar*.[7]

L'edizione delle *Bemerkungen* mostra che gli esecutori del lascito wittgensteinano non erano insensibili a queste critiche. Nei due volumi vengono infatti pubblicati *in toto* (p. 4), ossia senza selezioni e riorganizzazioni, due dattiloscritti, rispettivamente i dattiloscritti 229 (dettato nel tardo autunno 1947) e 232 (dettato agli inizi dell'autunno 1948) che si basano su una serie di manoscritti (i manoscritti 130-138 secondo la ormai classica numerazione di von Wright) stesi a cominciare dal maggio 1946. In questo senso questa edizione si avvicina più di altre a quel rigore filologico da molti studiosi auspicato, anche se, come negli altri casi, il resoconto dei criteri editoriali datoci dai curatori ci appare, se paragonato alle «tormentate revisioni e ricopiature» di Wittgenstein, «impassibile e breve».[8] Va qui tra l'altro ricordato come queste annotazioni non fossero, alla data della loro pubblicazione, interamente sconosciute. Un numero non indifferente di esse era già comparso nel 1967 in *Zettel* [9] (369 dei frammenti qui raccolti erano stati infatti ritagliati da Wittgenstein proprio da dattiloscritti 229 e 232). Inoltre diverse altre annotazioni erano state pubblicate nel 1953 nella già ricordata Parte II delle *Philosophical Investigations*, ossia di quella parte in vista della quale, secondo la Anscombe e von Wright, Wittgenstein aveva per l'appunto steso i manoscritti 130-138, i quali possono essere così considerati, «non senza giustificazione [ma senza che la evocata giustificazione venga resa esplicita dai curatori], come un insieme di studi preparatori per la Parte II delle *Ricerche filosofiche*» (p. 3).

Come leggere le *Osservazioni*? Di solito di fronte a un testo la domanda che ci si pone per prima riguarda il suo tema, contenuto o argomento: "Di che cosa tratta?". Uno dei compiti di un titolo, forse il suo compito principale, è darci una prima indicazione al riguardo. Nel caso delle *Osservazioni* sappiamo però che il titolo non è dell'autore, bensì dei curatori. Di più: mentre il termine "osservazioni" (*Bemerkungen*) è di evidente derivazione wittgensteiniana (è Wittgenstein infatti che nella *Prefazione* alle *Ricerche filosofiche*, ma non solo qui, parla, a

[4] Ora pubblicato a cura di M. Nedo come II volume della *Wiener Ausgabe* (Springer, Wien 2000); la traduzione italiana di A. De Palma è stata pubblicata nel 2002 presso Einaudi (Torino).

[5] Pubblicata nel 1969 a cura di R. Rhees, presso Basil Blackwell (Oxford); la traduzione inglese di A. Kenny (*Philosophical Grammar*) fu pubblicata nel 1974; la traduzione italiana di M. Trinchero (*Grammatica filosofica*) nel 1990 presso La Nuova Italia (Firenze).

[6] A. Kenny, From the *Big Typescript* to the *Philosophical Grammar*. *Acta Philosophica Fennica, 28* (1976).

[7] Da qualche anno gli studiosi hanno finalmente a disposizione l'edizione elettronica del lascito di Wittgenstein, curata dai Wittgenstein Archives dell'Università di Bergen: *Wittgenstein's Nachlass: The Bergen Electronic Edition*, Oxford Univesity Press, Oxford, 2000.

[8] Così scrive Roberta De Monticelli nella sua postfazione alle *Osservazioni*, cit., p. 511.

[9] A cura di G. E. M. Anscombe e G. H. von Wright con traduzione inglese a fronte della Anscombe, Basil Blackwell, Oxford, 1967 (seconda edizione 1981); traduzione italiana di M. Trinchero, Einaudi, Torino, 1986.

proposito delle sue annotazioni, di «osservazioni filosofiche»[10]), l'espressione "filosofia della psicologia" non solo si trova raramente[11] in Wittgenstein, ma non è nemmeno immediatamente perspicua. Nella loro breve *Premessa* la Anscombe e von Wright sembrano giustificare la scelta del titolo allorché ci informano che i quaderni manoscritti 130-138, che stanno alla base dei dattiloscritti 229 e 232, contengono «osservazioni quasi esclusivamente dedicate alla natura dei concetti psicologici» (p. 3). Sono comunque subito evidenti i diversi interrogativi che questa succinta indicazione solleva. Per esempio: (1) se i concetti psicologici a cui sono dedicate le annotazioni di Wittgenstein siano gli stessi concetti di cui tratta la psicologia intesa come scienza; (2) quale rapporto vi sia tra la filosofia e la psicologia, ossia se tra la prima e la seconda vi siano o meno sovrapposizioni o addirittura interferenze; (3) come e in che senso la filosofia (cioè, la filosofia come la intende e pratica Wittgenstein; l'ovvia precisazione sarà d'ora in poi sottintesa) si misuri con la *natura* dei concetti, nel nostro caso con la natura dei concetti *psicologici*; (4) perché mai i concetti psicologici siano di così grande interesse e richiamo per la filosofia; insomma perché Wittgenstein abbia loro dedicato tanto tempo e spazio negli ultimi intensi cinque anni della sua vita.

La risposta di Wittgenstein al primo dei nostri interrogativi è certamente affermativa. In II, §.20, per esempio, egli osserva, in riferimento al concetto di 'pensare', che è «[d]al linguaggio quotidiano», dall'uso che facciamo del verbo "pensare", che «prendiamo quel concetto» (vedi anche II, §.194: «'pensare' è una parola del linguaggio quotidiano»); in II, §.62 e nell'appena citato II, §.194, l'osservazione viene, per così dire, estesa a tutti i termini e i concetti della psicologia; i concetti psicologici (e i relativi termini), annota infatti Wittgenstein, «sono davvero concetti della vita quotidiana», concetti della «vita umana» (II, §.16), e niente affatto «concetti creati *ex novo* dalla scienza per i propri scopi, come, invece, quelli della fisica e della chimica» (II, §.62). È appunto per questo, ossia perché i concetti psicologici non sono né creati *ex novo* né introdotti in vista di scopi interni alla scienza, che l'impiego dei termini psicologici risulta «ingarbugliato», ossia «non […] così chiaro, e così facile da afferrare con un colpo d'occhio, come quello, per esempio, dei termini della meccanica» (II, §.20); ed è sempre per questo che non dobbiamo aspettarci che essi abbiano «un impiego unitario» (II, §.194). «I concetti psicologici – precisa infatti Wittgenstein con un certo compiacimento – hanno con quelli delle scienze rigorose la stessa relazione che hanno i concetti della medicina scientifica con quelli delle vecchie donne che si dedicano alla cura dei malati» (II, §.62).

L'obiezione di chi aspirava (il riferimento è ovviamente agli anni in cui scriveva Wittgenstein) a intendere e a praticare la psicologia come una scienza può essere qui facilmente prevista. Se la psicologia può così apparire – costui potrà osservare – è perché essa non è ancora pervenuta a quella maturità che da tempo è stata raggiunta dalla fisica o dalla chimica. Per fortuna, comunque, essa sta ormai smettendo i neri grembiali delle vecchie donne per indossare i bianchi camici dei medici. Wittgenstein conosceva questa reazione, la quale trovava espressa, per esempio, in W. Köhler[12], il quale riteneva che la «confusione in psicologia» andrebbe «spiegata con il fatto che essa è una 'scienza giovane'» (I, §.1039), ossia

[10] «[…] mi accorsi che il meglio che potessi scrivere sarebbe sempre rimasto allo stato di osservazioni filosofiche […] Le osservazioni filosofiche contenute in questo libro sono, per così dire, una raccolta di schizzi paesistici, nate da queste lunghe e complicate scorribande [in "una vasta regione di pensiero"]" (*Ricerche filosofiche*, cit., p. 3).

[11] "Filosofia della psicologia" sarebbe stata, per esempio, l'espressione usata da Wittgenstein per indicare delle sue lezioni a Cambridge nel 1946-1947; vedi, al riguardo, *Wittgenstein's Lectures on Philosophical Psychology*, 1946-1947, a cura di P. T. Geach, Harvester, London, 1988, p. 235.

[12] La traduttrice italiana rinvia al riguardo al secondo capitolo di W. Köhler, *La psicologia della Gestalt*, tr.it. di G. De Toni, Feltrinelli, Milano 1984, il cui titolo è, per l'appunto, "La psicologia è una scienza giovane" (edizione originale: *Gestalt Psychology*, New York 1947).

una scienza dal cui metodo sperimentale non abbiamo ancora ottenuto tutto ciò che esso può darci per quanto riguarda, per esempio, la comprensione del pensiero. Se continueremo a fare esperimenti, prima o poi il pensiero cesserà tuttavia di essere quel "processo enigmatico" che è ancora per noi.

Wittgenstein ritiene però che sia fuorviante imputare alla circostanza che il metodo sperimentale è ancora agli inizi le insufficienze della psicologia. In questo modo si rischia di far assomigliare lo psicologo a qualcuno che «volesse stabilire per mezzo di esperimenti chimici che cosa sia materia e che cosa sia spirito» (I, §.1093). A differenza di quanto credeva Köhler, la confusione che si ha nella psicologia (quella confusione che ci fa apparire enigmatico il pensiero) è, secondo Wittgenstein, innanzitutto una «confusione concettuale» (I, §.1039), per cui cercare di farvi fronte con degli esperimenti significa confondere concetti e fenomeni; ricerche empiriche e ricerche concettuali. Ciò che qui Wittgenstein critica non è allora la psicologia in quanto scienza sperimentale, bensì lo psicologo che pretende di trovare negli esperimenti e nel metodo sperimentale la risposta a un problema che è «di natura concettuale». Nella prospettiva delle *Osservazioni* costui è propriamente un metafisico, se si considera che per Wittgenstein «[l]'essenziale della metafisica» consiste, per l'appunto, nel non aver «chiara la differenza fra ricerche concettuali e ricerche riguardo ai fatti» (I, §.949).

Come si ricorderà ci eravamo domandati (era il secondo dei nostri interrogativi) se la filosofia possa in qualche modo o senso interferire con la psicologia. La risposta è per Wittgenstein senz'altro negativa: la filosofia non può né deve indirizzare o condizionare l'indagine psicologica in quanto tale; essa non ha nulla da dirci sulla correttezza metodologica di un esperimento né spetta a essa valutare la fondatezza di un'ipotesi fisiologica. In questo senso il suo rapporto con la psicologia non è differente da quello che essa intrattiene con qualsiasi altra scienza. Questa non è però tutta la storia. La filosofia può infatti indurre lo psicologo a domandarsi se alcuni di quelli che gli si presentano come problemi da risolvere sperimentalmente non siano piuttosto delle confusioni concettuali che occorre primariamente chiarire e sbrogliare. In questo senso è possibile parlare, come filosofo, della psicologia o, come psicologo, dedicare una parte del proprio impegno ai malintesi e rompicapi e fraintendimenti che l'impiego dei concetti psicologici e dei relativi termini produce. Come Wittgenstein precisa altrove, si tratta, in particolare (ma non solo), di quel «genere di malintesi […] che sono prodotti dalla tendenza ad assimilare tra loro espressioni che svolgono nel linguaggio funzioni assai differenti» o che sorgono allorché «[c]erchiamo di parlare di cose molto diverse mediante un medesimo schema» (Diamond, 1982, p.15).[13]

«Ma che cosa è una ricerca concettuale?» si domanda lo stesso Wittgenstein in I, §.950. Wittgenstein esclude innanzitutto che una ricerca concettuale abbia come suo compito quello di spiegare perché abbiamo proprio i concetti che abbiamo; ossia che il suo scopo sia quello di scoprire come si formino, ossia su quali «fatti di natura (psicologici e fisici)» si fondino, i nostri concetti. In questo senso, la filosofia in quanto ricerca concettuale non è affatto «una scienza naturale travestita», la quale farebbe allora bene a interessarsi esplicitamente, senza travestimenti per l'appunto, e in maniera scientifica (ossia, facendo ipotesi e mettendole alla prova) di ciò che davvero importa, ossia, per l'appunto, «di ciò che in natura sta alla […] base» delle nostre costruzioni concettuali.

Nelle *Osservazioni* Wittgenstein ritorna più volte sul punto a testimonianza della sua importanza. Nell'annotazione appena ricordata, egli ammette che vi è indubbiamente una

[13] Wittgenstein mette comunque in guardia dall'assolutizzazione del proprio atteggiamento: «io sottolineerò le differenze tra le cose, laddove di solito se ne sottolineano le somiglianze» (Diamond, 1982, p.15), benché anche questo possa far nascere malintesi.

«corrispondenza fra la nostra grammatica [tra i nostri concetti][14] e fatti molto generali della natura (raramente espressi in parole)» (I, §.46). Per esempio, «se il rosso apparisse soltanto […] sulla punta delle foglie di certi alberi, che in autunno gradualmente si trasformano da verdi in rosse, niente verrebbe più naturale che chiamare il rosso un verde degenerato» (I, §.47; vedi anche I, §.626). In questo modo: "Il rosso è un verde degenerato" cesserebbe di essere un nonsenso e sarebbe proprio esso a dirci che tipo di colore il rosso sia.[15] Questo però non significa che nelle circostanze immaginate il rosso sarebbe stato necessariamente chiamato un verde degenerato o che il concetto di 'rosso' sarebbe stato necessariamente diverso dal nostro. Come annota scrupolosamente Wittgenstein, ciò che ci appare naturale, non per questo è necessario (vedi II, §.49).

La corrispondenza tra concetti e fatti molto generali della natura non va in ogni caso intesa come un'ipotesi sulle «cause *possibili*» (I, §.46) dei concetti, come se parlando di corrispondenza si volesse avanzare l'ipotesi che, «[s]e i fatti di natura stessero diversamente, avremmo concetti diversi» (I, §.48). Ciò che a Wittgenstein preme è qualcosa di ben diverso: mutare l'atteggiamento nei confronti dei nostri concetti sconfiggendo la tentazione di imprimere su di essi il suggello della necessità.

> Quello che dico – annota infatti Wittgenstein – è solo: Se credi che i nostri concetti siano quelli giusti, quelli che si confanno a essere umani intelligenti, […] allora prova a rappresentarti certi fatti generali di natura diversamente da come sono, e costruzioni concettuali diverse dalle nostre ti appariranno *naturali*. (I, §.48; vedi anche I, §.643)

Per esempio, a chi ritenesse la distinzione tra forma e colore come qualcosa di necessario, al punto che un sistema concettuale privo di essa potrebbe solo apparirgli come qualcosa di strano e di ancora primitivo, Wittgenstein chiede di rappresentarsi un mondo in cui determinate forme siano sempre vincolate a determinati colori (per esempio, il verde alla forma circolare; il rosso a quella quadrata) e di domandarsi se in quel caso un sistema concettuale senza la distinzione tra forma e colore non cesserebbe di colpirlo come strano e incompleto (vedi I, §.47).

Anche se può richiamarsi ad alcuni fatti molto generali della natura, una ricerca concettuale è dunque essenzialmente differente da una ricerca fattuale (scientifica) che mette alla prova ipotesi, ricerca spiegazioni causali, elabora teorie, fa predizioni. È questo un punto più volte ribadito nelle *Osservazioni*, del resto in stretta sintonia con tutto ciò che sulla filosofia Wittgenstein ha sempre continuato a sostenere. Non che questa rinuncia alla teoria e alla spiegazione sia qualcosa di facile; al contrario, la rinuncia è particolarmente difficile e, per così dire, dolorosa. Sembra infatti che rinunciarvi equivalga a rinunciare alla filosofia in quanto tale, finendo con l'accontentarsi di ciò che ci è «più vicino e abituale» (I, §.361), di semplici esempi e dei «casi speciali» (I, §.633) e incompleti (I, §.723), abbandonando ciò che sembra sia sempre stato lo scopo della filosofia (e della scienza): «scendere in profondità» (I, §.361), ridurre i diversi casi e esempi «al minor numero di leggi […] primitive […] unificare, mediante una generalizzazione, la trattazione di differenti argomenti» (Wittgenstein, 1983, p. 28).

> Limitarsi a descrivere [rinunciando a spiegare] – annota infatti Wittgenstein – è così difficile perché si crede che per comprendere i fatti sia necessario integrarli. È come se uno vedesse uno

[14] Gli enunciati grammaticali sono quegli enunciati di cui ci serviamo per determinare (circoscrivere o delimitare) i nostri concetti. Impiegato grammaticalmente, un enunciato come "I libri non spariscono da soli" non ci dà informazioni sui libri, ma determina il nostro concetto di 'oggetto materiale'. In questo senso, l'espressione «la nostra grammatica» che compare in I, §.46 può essere sostituita dall'espressione "i nostri concetti".

[15] «Che tipo di oggetto una cosa sia: questo dice la grammatica» (Wittgenstein 1967, I, §.373).

> schermo su cui sono sparse delle macchie di colore e dicesse: così come sono, sono inintelligibili; acquisteranno senso solo se le si integra in una figura. Mentre quello che io voglio dire è, invece: è tutto qui. (Se lo integri, lo snaturi). (I, §.257)

E ancora: «La difficoltà della rinuncia a ogni teoria: bisogna concepire questo e quello, che pare così chiaramente incompleto, come qualcosa di completo» (I, §.723).

Proviamo a chiarire il punto considerando brevemente quelle osservazioni in cui Wittgenstein si confronta con chi sostiene non solo che la psicologia debba spiegare (formulare ipotesi, metterle sperimentalmente alla prova, elaborare teorie), ma anche che solo le spiegazioni *fisiologiche* siano da considerare vere e proprie spiegazioni. È evidente che se Wittgenstein non ha nulla da obiettare alle spiegazioni, non ha nemmeno nulla da obiettare alle spiegazioni fisiologiche in quanto tali; ciò che contesta è ancora una volta l'assunto che da esse, o addirittura solo da esse, possa venire un «chiarimento dei problemi concettuali in psicologia» (I, §.1063). Un esempio di problema concettuale discusso a lungo nelle *Osservazioni* riguarda la tristezza: se la tristezza sia una sensazione; se essa vada infilata, per così dire, nel cassetto delle sensazioni. Il punto è per Wittgenstein di grande importanza perché infilare qualcosa «nel cassetto sbagliato», ossia classificarlo malamente, significa farlo apparire «misterioso, inafferrabile, sorprendente», mentre «se lo consideriamo nel modo giusto, la sua 'inafferrabilità' si affaccia alla nostra coscienza non più di quella del tempo quando sentiamo dire: 'È tempo di pranzare'» (I, §.380). Ora, è del tutto legittimo condurre una ricerca sperimentale sulla tristezza; potremmo, per esempio, scoprire «che le ghiandole della persona che è triste producono una secrezione diversa da quelle di una che è allegra» e potremmo anche concludere, su base sperimentale, «che questa secrezione è la causa, o una delle cause, della tristezza». Gli effetti di questa scoperta sperimentale sarebbero, com'è facile immaginare, molti e alcuni di questi di grande importanza (in campo medico, per esempio). Questa scoperta però – è questo per Wittgenstein il punto rilevante – non servirebbe affatto a provare «che la tristezza è una *sensazione* prodotta da questa secrezione» (I, §.802). Con questa scoperta non avremmo insomma risposto al problema di natura concettuale "Che cos'è la tristezza?" ("Che cosa significa 'essere triste'?"). Immaginiamo, per esempio, che intervenendo per via farmacologica si blocchi in qualcuno la secrezione che è causa della tristezza. Ebbene, di fronte, per esempio, alla malattia di un amico, costui potrebbe ancora dire: "Non provo nessuna delle sensazioni che provavo quando mi sentivo triste, anche se so bene che dovrei essere triste".

Nel secondo volume delle *Osservazioni*, Wittgenstein abbozza un «[p]iano della trattazione dei concetti psicologi» (vedi II, §.63 e §.148) dal quale, tra le altre cose, si ricava che la tristezza va infilata nel cassetto delle emozioni piuttosto che in quello delle sensazioni. Nel suo piano Wittgenstein distingue infatti le emozioni (collera, gioia, depressione, paura, eccetera) dalle sensazioni (per esempio, sensazioni di pressione, temperatura, sapore, dolore). Come le sensazioni anche le emozioni hanno una durata autentica (per esempio, «[l]a collera divampa, si placa, sparisce; così fanno anche la gioia, la depressione, la paura»), ma, a differenza delle sensazioni, esse «non sono localizzate (e nemmeno diffuse!)». Le emozioni sono inoltre accompagnate da espressioni comportamentali e sensazioni caratteristiche (per esempio, «la tristezza spesso si accompagna al pianto, e alle sue sensazioni caratteristiche»), «ma queste sensazioni non sono le emozioni». Alcune emozioni poi sono «dotate di una direzione […] Paura *di* qualcosa; gioia *per* qualcosa», anche se «[q]uesto qualcosa non è la causa, bensì l'oggetto dell'emozione»: che si provi piacere per qualcosa, aveva già annotato Wittgenstein nel primo dattiloscritto, «non vuol dire che questo qualcosa causi una in noi una sensazione»; pensarla altrimenti significherebbe non distinguere tra ragione e causa (I, §.800). Un'ulteriore caratteristica dell'emozione è che essa ha un contenuto che ci rappresentiamo «sempre come

un'immagine»; per esempio, «[l]e tenebre della depressione, che calano su una persona, le fiamme dell'ira». Infine, le emozioni, a differenza delle sensazioni, «non ci istruiscono sul mondo esterno» (II, §.148).

La costruzione di un piano siffatto, il quale non aspira all'esattezza, ma alla perspicuità (I, §.895), è uno dei modi con cui Wittgenstein cerca di fare ciò che dovrebbe fare una ricerca concettuale: «*padroneggiare* le affinità e le differenze fra i concetti» (I, §.1054); andare in cerca delle analogie e connessioni tra i concetti (I, §.923 e II, §.59). Due punti vanno qui sottolineati, i quali ci permetteranno anche di abbozzare una risposta al terzo degli interrogativi che ci hanno guidato in questa rilettura delle *Osservazioni*: prima di tutto, nel tracciare una mappa dei nostri concetti (vedi I, §.303 e I, §.556), ogni materiale va, per così dire, bene: ciò che abitualmente diciamo; ciò che possiamo immaginare che diremmo se…; come gli uomini hanno raccontato la tristezza, la gioia, la sofferenza, la collera o la paura in romanzi e novelle; come le hanno dipinte o disegnate; come si recita la paura o si imita la gioia; come si reagisce alla sofferenza o come si condivide la gioia. È in questo senso che dovremmo dire che la psicologia ha a che fare propriamente con la vita umana o con determinati suoi aspetti (vedi II, §.35). Il secondo punto è altrettanto rilevante: analogie e connessioni non se ne stanno lì, impresse nelle cose: tutto può infatti essere connesso a tutto; ogni cosa può essere divisa da ogni altra. Per esempio, tra il vedere, l'udire, eccetera, «sussistono analogie e connessioni le quali costituiscono la giustificazione del nostro metterli assieme» (II, §.59). Ma se proviamo a domandarci che tipo di connessioni e analogie vi siano tra vedere e udire, vedere e prendere, vedere e annusare, «subito i sensi, per così dire, si distanziano l'uno dall'altro più di quanto non sembrasse a prima vista» (II, §§.60-61). Che cosa mai vi può essere – verrebbe infatti da dire – di più diverso del vedere e dell'annusare? Ma allora sbagliavamo prima quando li mettevamo assieme o sbagliamo adesso? La risposta è che sbagliamo solo se scambiamo il nostro ordine, il quale è «solo uno dei molti ordini possibili», per «*l*'ordine» (Wiitgenstein 1967, I, §.132); se prendiamo la nostra mappa, disegnata per uno scopo determinato, per la «idea preconcetta a cui la realtà *deve* corrispondere» (Wiitgenstein 1967, I, §.131).[16]

Ma – si potrebbe a questo punto obiettare – non è altrettanto dogmatica l'insistenza con cui Wittgenstein insiste sulla irrilevanza per la filosofia di ogni spiegazione in psicologia, in particolare delle spiegazioni fisiologiche? Qui mi riferisco, in particolare, ad alcune annotazioni delle *Osservazioni* che sono state sovente rigettate o addirittura irrise come l'espressione di un oscurantismo antiscientifico. Ciò vale, per esempio, per la dichiarazione che apre I, §.903:

> Nessuna supposizione mi sembra più naturale di quella che al fare associazioni, o al pensiero, non sia coordinato nessun processo nel cervello; così che sarebbe impossibile leggere nei processi del cervello dei processi di pensiero. (I, §.903; vedi anche I, §.1063)

O per l'annotazione: «Dunque è assolutamente possibile che certi fenomeni psicologici non possano essere indagati fisiologicamente, perché niente di fisiologico corrisponde loro» (I, §.904).

Vi sono diversi modi di intendere il ruolo e il peso di questa e altre controverse annotazioni. Ovviamente, Wittgenstein sa bene che pensiero e cervello sono, diciamo così, coordinati. Lo sa come lo sappiamo tutti noi, anche quando le nostre informazioni sugli stati e i processi cerebrali sono del tutto vaghe e imprecise. Né è sua intenzione sottovalutare o denigrare le indagini fisiologiche in ambito psicologico. Le considerazioni wittgensteiniane vanno piuttosto ricondotte a uno specifico contesto caratterizzato dalla sua resistenza a quelli che egli chiama

[16] In questo consiste, precisa Wittgenstein tra parentesi, quel «dogmatismo in cui si cade così facilmente facendo filosofia» (Wiitgenstein 1967, I, §.131).

«pregiudizi fisiologici» (I, §.1101). Significativo è al riguardo il gruppo di annotazioni I, §§.1100-1101. Qui Wittgenstein osserva come noi non vediamo l'occhio umano «come un ricettore»; infatti, «l'occhio non sembra lasciar entrare qualcosa, bensì sembra che lo emetta. L'orecchio riceve; l'occhio guarda. (Lancia sguardi, s'accende di lampi, è raggiante, risplende)». Insomma: «[s]e vedi l'occhio, […] vedi lo sguardo dell'occhio» (I, §.1100). Come reagire a tutto questo? Forse qualcuno potrebbe obiettare che lo sguardo noi non lo vediamo in senso proprio. Ma questa sarebbe per Wittgenstein «una stupidaggine» dettata e condizionata, per l'appunto, dai suddetti pregiudizi fisiologici. Lo sguardo dell'occhio io lo vedo, come, per esempio, «vedo lo sguardo che lanci a un altro» (I, §.1101). Non per questo, tuttavia, sono costretto ad ammettere che vedo «lo sguardo 'esattamente' come vedo la forma e il colore dell'occhio» (I, §.1101). Il nostro concetto di 'vedere' può raccogliere senza assimilarli entrambi i casi.

Ma che cosa accade quando qualcuno, Wittgenstein lo chiama qui «il purista», afferma che «[i]n realtà io non vedo lo sguardo, ma solo forme e colori»? Sta forse con ciò dicendo «che aveva torto chi sosteneva di aver visto bene il mio sguardo» (I, §.1102)? La risposta alla domanda sarebbe affermativa se "Ho visto bene il tuo sguardo" contenesse «una teoria della visione». Ma le cose non stanno assolutamente così (I, §.1101). In effetti, la sola maniera di contraddire chi dice di aver visto bene il mio sguardo è di obiettargli che egli si inganna o mente perché non ha veramente visto quello che dice di aver visto così bene. Come intendere allora il purista? Il suggerimento di Wittgenstein è che costui stia «soltanto richiamando l'attenzione su un confine fra concetti», ossia sul modo in cui la parola 'vedere' classifica le percezioni. Certo, il confine può essere diversamente ridisegnato, forse in relazione allo sguardo «sarebbe più giusto usare un'altra parola in luogo di 'vedere'» (I, §.1102) al fine di dare rilievo a questa o a quella distinzione, ma in ogni caso nessuna indagine e spiegazione fisiologica può, in quanto tale, stabilire che la linea concettuale che è stata tracciata sia esattamente la linea che doveva essere tracciata.

Le ultime considerazioni ci consentono anche di dare una risposta esplicita, anche se breve e non definitiva, all'ultimo dei nostri interrogativi. I concetti psicologici sono – lo sappiamo – concetti ingarbugliati, così come vario e variegato è l'impiego dei relativi termini psicologici. Non può allora stupire che da essi sorgano «naturalmente» (Geach 1988, p. 235) quei malintesi, rompicapi e fraintendimenti che spetta alla filosofia sciogliere e sbrogliare. Ma il filosofo li scioglie e sbroglia solo riconoscendo che quei concetti e termini sono parte della nostra vita; che sono i concetti e i termini con cui gli uomini parlano di sé, si raccontano, si espongono gli uni agli altri o si nascondono, mentono o si confessano, si esprimono e si travestono. In questo senso è l'interesse positivo per la vita degli uomini che motiva, nelle *Osservazioni*, il lavoro filosofico, in apparenza solo negativo, di Wittgenstein.[17]

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Commentary

Psychiatry in the Scientific Image

Dominic Murphy
MIT Press, Cambridge, MA, 2006

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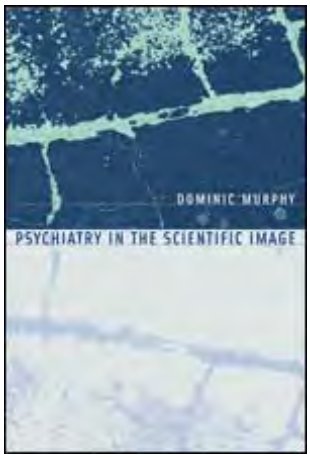

Dominic Murphy's first book is a contribution to the debate on psychiatry within analytical philosophy, in spite of the well-known philosophical production on the subject in the continental tradition. Even clearer than the few available textbooks on analytical philosophy of psychiatry, the author remarkably presents and discusses in detail the most considerable psychiatric issues on which contemporary philosophers of mind are interested: the concept of mental disorder (Chapter 2), folk psychology (Chapter 3), the medical model in psychiatry (Chapter 4), mechanistic explanation (Chapter 5), the problem of validation (Chapter 6), both social constructionist (Chapter 7) and evolutionary approaches (Chapter 8), and finally taxonomy (Chapters 8-9).

Its title, *Psychiatry in the Scientific Image*, reminds that of the Wilfrid Sellars' paper *Philosophy and the Scientific image of man* that inspired also the popular Bas Van Fraassen's work *The Scientific Image*. Murphy states openly this linguistic fatherhood but reports only Sellars' text, ignoring Van Fraassen's one among bibliographical references. Sellars' expression contrasts the *manifest* image of the world, roughly the world as it appears to human observation, with its *scientific* one. Murphy shows that present psychiatry, although intended to be discussed within the latter, conforms much more to the former. As a matter of fact, he discredits the current psychiatric nosology, that of the DSM, as *incoherent-heterogeneous-provincial*, being reliable on manifest symptoms but unsatisfying about causal explanations of mental illnesses (Chapter 9). Yet that constructive empiricism suggested by Van Fraassen, and apparently ignored by an explicit reference, seems to be somewhat a theoretical inspiration. According to Van Fraassen (1980), science does not aim to state the truth, but empirical adequacy, therefore it should explain correctly what is observable, and should construct *models* suitable to phenomena. Though adverse to a purely descriptivist approach, conceived as just commonsense based, Murphy states that wondering whether mental disorders are natural kinds is an inconclusive question as it obscures the more important issue of the availability of categorical taxonomies. What matters, he says, is whether we can come up with a basis for classification and he suggests we can do it through the idealized notion of *exemplar*, causally explained representations of symptoms (Chapter 7 and 10). However, while, on the one hand, he creates exemplars on the basis of the DSM categories (e.g., OCD and schizophrenia), on the other hand he seems to forget the limits of the DSM whose commonsense clusters hardly fit for identifying a shared cause.

Concerning the scientific image of psychiatry, Dominic Murphy explores an attractive possibility: making psychiatry part of the most important contemporary scientific enterprise on mind-brain, cognitive science. On the one hand, his aim testifies the worrying isolation of

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psychiatry from the other sciences of the mind. On the other hand, cognitive theories appear only as one direction among a range of etiological and taxonomic approaches to psychiatry. In fact, certain skepticism about such a project in the psychiatric debate is often motivated by the fact that cognitivism would involve a definite theoretical choice for a pre-paradigmatic discipline like psychiatry, a choice that many psychiatrists would feel not inclined to. However, Murphy's work has the remarkable merit of beginning to build a bridge between cognitive sciences and psychiatry as accurately and widely as no one before. The general goal of his text is offering the image that psychiatry should acquire, concerning the main related philosophical problems, if it came to be reformulated as a "clinical cognitive neuroscience".

With regard to the former paper *Darwin in the Madhouse*, co-authored with Stephen Stich in 1998, the conception of psychiatry in cognitive terms shows different features. As a matter of fact Murphy raises objections to the view he previously defended that such a reformulation should be completely modeled on Evolutionary Psychology and on its main constraints, the *Massive Modularity Hypothesis* and the *Adaptation Hypothesis*. This could disappoint whoever expected this book were an extension of those ideas. In fact, two central chapters are dedicated to arguments against the possibility of fully explaining mental illnesses either as a modular deficit (Chapter 5) or as an adaptive problem (Chapter 8). However, while Murphy claims that Evolutionary Explanation simply fail to stand up, Modularity is partly maintained for deficits of peripheral cognitive systems. Therefore, Murphy seems to have agreed to José Louis Bermúdez' objections (Bermúdez 2001), according to which modularity could not face the problem of explaining supposed deficits of central systems (i.e., delusions and other problems in rationality) in neurocomputational terms.

Nevertheless, Bermúdez referred to a different conception of Modularity from the Massive one, namely, Fodorian Modularity (Fodor 1983). Hence, by taking Bermúdez' arguments in consideration, Murphy does not seem to adopt a definite position regarding the matter. It is not clear whether and why he became Fodorian in matter of architecture of mind since the problem of explaining central cognitive systems largely results from Fodorian conceptions about modules and cognitive reasoning mechanism that Massive Modularists elude. Massive modules do not include strong informational encapsulation and can intend reasoning not to be executed by a single and isolated module, which manifests the same properties - holism and isotropy - the phenomenon of reasoning shows from the outside (see Sperber 1996). Furthermore, those properties seem not to be in human reasoning as sharp as Fodor thinks they are. Murphy considers neither these counter-argumentations nor those coming from the Connectionist area against two main ideas: the idea of the lack of informational encapsulation even in peripheral systems (Churchland 1988) and the idea of sub-symbolic computation for modules (Waskan & Bechtel 1997). Connectionists state that their approach poses fewer problems to the holism of representations. On the contrary, Murphy prefers to share what he defines a "two-stage picture" about psychiatry, according to which the foundation of psychiatry in positive facts (or naturalization project) is not always wholly attainable. In case of deficits of central systems, normativity side would resist to naturalization as well as personal explanation.

Against the current Micro-reductionist approach in biological psychiatry (that of genes and neurotransmitters), Murphy espouses a kind of non-eliminative epistemological Reductionism (Chapters 2-3), which places psychiatry within the medical model (i.e., mental illnesses as deficits of brain mechanisms) but maintains multilevel explanations (i.e., personal-intentional plus computational and neurological levels). He challenges the idea of a *fundamental* explanation, which corresponds for Micro-reductionists with the smallest level in nature. However reasons he offers against Molecular Research Strategies are not as decisive as he

thinks. He states that Molecular Reductionism cannot play the unifying explanatory role its theorists expect for it. As a reason, he claims that Genetic Reductionism is dependent on animal models. He then argues that even in very simple organism like small worms the causal pathways between genes and phenotype are of great complexity, and require multilevel causal models. Nevertheless the very same dependence occurs in models of brain mechanisms at highest levels. Moreover, he does not clarify what he means when talking of 'levels' except for mentioning David Marr. As a matter of fact, he confuses size or aggregation levels (molecules, cells, organ areas, organs, bodies, etc.) with realization levels (mind-brain) (see Kim 1998). Therefore, his argumentations save at most highest size levels. However, if they are intended to save psychological level too, they fail.

He identifies his kind of reduction with the Mechanist approach and with its decomposition assumption (Chapter 3). However, the offered characterization of Mechanistic approach seems to be misleading. He refers to the mentioned Fodorian objections against Massive Modularity as "The Limits of Mechanistic Explanation", as he entitles Chapter 5. Nevertheless William Bechtel and colleagues insist that Mechanistic theory, although a functional decomposition theory, has little to do with Modularity (see, for example, Bechtel 2003, 2009). Among several differences between the two approaches, it is worth saying that Mechanism does not retain the idea of relatively autonomous components because it states such an idea hardly squares the organization of the brain. According to Mechanisms, it is no longer appropriate to think in terms of a dichotomy between modular accounts and holistic ones, but a continuum in which the middle is occupied by various designs of mechanisms. Assuming decomposability in scientific investigations is just a heuristic assumption that is only partially true of any given mechanism. Natural systems, especially biological systems, are conceived *nearly decomposable* systems, or better still independent only to a first approximation. Accordingly, even when mechanistic theorists identify a particular system as a locus of control for a particular function, they need not impute full responsibility to that component. They think natural systems are complex systems whose main characteristic is interaction.

Anyhow Murphy's concept of disorder is objectivist (Chapter 2). That is, mental illnesses are brain diseases and they can be explained in factual biological terms. He takes place against the Social Constructivist view that mental illnesses are violations of social norms but his arguments against Social Constructivism are clearly inadequate. Firstly, he argues that Constructivism cannot explain why we distinguish between psychological and behavioral phenomena like racism, boorishness, hypocrisy and so on, conceived only as disapproved, and mental disorders. Secondly, he says that Constructivism cannot avoid Relativism. However, on the one hand, Constructivists think that social deviances and mental disorders have much more in common than one may think at first sight. For Social Constructivists, they are both social constructs which vary from society to society and relate on the different norms established by each of them. For example, racism wasn't a deviance from social norms up to the latest century as well as religious delusions. A Constructivist might argue that our different judgments depend on the way each society considers them more or less dangerous. Michel Foucault, for example, consecrated his work to both the phenomena of criminality and madness, and conceived them as two form of the same purpose of segregation of people violating social norms, which originate detentions as clinics (Foucault 1966) and prison (Foucault 1975). On the other hand, Murphy is espousing a *petitio principii.* Social Constructivism is exactly a kind of Truth Relativism (see Boghossian 2006), a thesis according to which there are not true statements but statements true for X and not for Y - where X and Y are people, historical eras or social communities. Constructivism is a Relativist thesis and thus cannot avoid Relativism, Murphy says. The inference is not strictly invalid, but clearly deceptive. Why should we avoid relativism? He has no arguments for that. Nevertheless,

except for this fault, his way of integrating social in natural causation (Chapter 7) is interesting and worthy, as well as his integrative conception (Chapter 3) of the medical (Guze 1992) and the biopsychosocial model (Engel 1977), usually regarded as alternative solutions. Social forces are natural ones.

Even though commonsense intuitions are seen relevant to science, the book refuses traditional conceptual analysis. Harmful-dysfunction analysis (Wakefield 1992) for the concept of disorder is rejected as a Folk Psychology *a priori* product (Chapter 2), exactly as DSM constructs (Chapter 9). As involving empirical assumption, they both need to be defined on the basis of empirical enquiry, and Murphy has a liking particularly for the former approach about disorder definition. With regard to the concept of 'function' involved in the dysfunction component of the definition of disorder, between historical-teleological and designed, he chooses an eclectic view that reflects current debate in philosophy of biology (see Godfrey-Smith 1993) according to which the term vary depending of what the theory is trying to explain. It is unusual however that he continues to use the scientifically temporary term 'disorder', which literally denotes a behavioral condition with unknown etiopathogenesis, even though he thinks mental disorders hide underling causal disruptions. Thus they are pathologies, not disorders.

Admittedly some argumentations in the book are not very accurate and do not meet analytical philosopher's standards of clarity and method. Reasons to accept some theories are sometimes proposed in a rhetorical style. Nevertheless main suggestions are both well-described and attractive. This is not only the first monographic book that explores systematically the whole project of discussing the theoretical foundations of a future cognitive neuropsychiatry. It is one of the most brilliant in the current philosophy of psychiatry. Written by a philosopher alone, it is a remarkable and hard work of research. It offers painstaking knowledge for main issues in both huge philosophical and psychiatric debates. That what makes it a fundamental and indispensable reference for philosophers of mind, of science and of biology and for psychiatrists. Moreover this book is an excellent guide to philosophy of pathological mind, also for beginners. Its conclusions support a promising new direction of scientific research for psychiatry. They leave interesting open questions regarding the role of normativity and the possibility of a complete naturalization. Prolific matter of debate for future philosophical work.

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Commentary

The Philosophical Legacy of Behaviorism

Bruce A. Thyer
Kluwer Academic, Dordrecht, 1999

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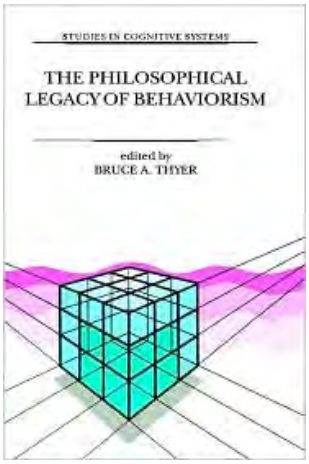

This volume contains a collection of papers concerning the philosophical implications of behaviorism, "in the era of the Cognitive Revolution". The contributions of behaviorism to the study of mind are manifold: the analysis of verbal behavior, the nature of inner states and private events, the examination of concepts like free will, determinism and self control, the development and the application of educational technologies and psychotherapic techniques. But behaviorism is more than an interesting philosophy; it is also synonymous with rigour and scientific approach to the science of mind. It involves important philosophical antecedents like the contributions of Mach, Comte, Poincaré, logical empiricism and positivism. It is more than just Skinner's thinking, thanks, mainly, to the work of Pavlov, Tolman, Hull, Watson, Spence, Guthrie. Besides, as James Dinsmoor says in the foreword, behaviorism highlights «a great deal of continuity» with cognitive psychology and cognitive science (p. 2). What is, then, its inheritance to current psychology and philosophy of science?

The program of behaviorism concerns three principal points (two philosophical and one methodological): the rejection of the dualism between physical events and states of mind, the refusal of the introspective method, and, third, its particular goal, which is limited to the prediction and control of behavior rather than to the analysis of conscious states (p. 1). What could be the implications – acknowledging these requirements – to ethics and psychotherapy? How is it possible, in fact, to ascribe the adjective 'ethic' to human actions without introducing a form of agencies that is compatible with a quantitative analysis of the properties of contingency conditions (p. 90)? What does thinking of behaviorism imply not exclusively in terms of Skinner's precepts? Is a behavioral analysis aspiring to become «an [effective] analytic and scientific epistemology» (p. 71)? Are we able to find a consistent system of belief for cognitive psychology or does «the word *cognitive* serve primarily as a magic wand that transforms mice into horses» (p. 5)? And is it correct to talk about the cognitive *revolution*?

Contemporary cognitive psychology, in fact, is full of theories assuming, without any empirical evidence, the existence of cognitive structures like inner models, frames, scripts, schemas, mental representations, resources, modules etc. With respect to them, the analysis given by the legacy of behaviorism appears still actual: how could one find, in fact, the proof of the existence of these 'mental objects'? Probably not empirically, because we would lose the inner access, essential, by means of reasoning, to postulate these same structures. And how is it possible to refuse an inner access and, at the same time, deny something that is psychologically *not* existent? The problem is well known, but we think that leaving out inner causes and including *states* and mental *processes* among natural phenomena does not mean solving the Cartesian dilemma. How Hayes, Wilson and Gifford claim, «in a sense, methodological behaviorism was overtly dualistic». It postulated «a word composed by two

kinds of stuff», and science can only take part in one of the two directly (p. 154). In addition, mental states and consciousness are not – as some cognitivists argue – epiphenomena or illusions created by the evolution: we need theories to explain them.

Honestly, we must say that the problem of the existence of inner states has been denied only by one part of the behavioristic school: Skinner, for example, claimed that consciousness is the product of inner stimulations and that it has an important role for the behavior and the economy of the organism (Skinner 1974, p. 221). But it is also a social product expressed by a verbal community. For Skinner, private events are the result of environmental causes: only in a second time the verbal community can ask questions about these events, not before. Mental properties, according to Skinner, are not necessary: both processes (biological and socio-linguistic) are physical. Such approach does not imply leaving out the problem of the self, or giving up the mind's causal role, but it needs to refuse the philosophers' metaphysical pretension of conceiving consciousness as a form of *self*-consciousness: intuitions, these, still pertinent in the debate on mind-body problem.

According to the authors, behavioral science can give important contributions to the major topics of philosophy, as we can read from the titles of the chapters: epistemology, ethics, consciousness, language, free will and determinism. The authors' intent to underline the confusions about radical behaviorism and other behavioral approaches deserves consideration. In fact, we must remember that the stereotypical image of behaviorism as a science of stimulus-response is totally wrong: as Skinner claimed, if it were so, an organism would look very much like to an automaton, or a puppet. Stimuli do not elicit operational reactions, but they change the *probability* of emission of specific reactions (Skinner 1974, p. 225). Paradoxically, the image of the automaton suits better the conception of mind suggested by some persistent claimers of the view of 'computationalism'. In fact, the metaphor of cognition as symbol manipulation and its analysis of condition-action rules defined over data structures conceived by particular research programs have minimized the active role of the body and the social context for the development of mental processes. And it is partially as a reaction to this view of the human mind that some cognitive scientists have developed the idea of human cognition coupled with the environment and the body of the organism called embodied or situated cognition.

In addition to the important contributions for philosophy of mind and science's methodology, behaviorism had an important role in the genesis of cognitivism. In fact, from the behaviorists, the cognitivists have inherited the attention for the experimental methodology and the selection of the problems to be faced (Peruzzi 2004, p. 175). It is important to remember, for example, that Watson himself did not refuse verbal reports of internal processes, and that he was one of the first researchers to use talking-aloud procedures – procedures that are now widely used by many cognitive psychologists. The link between these two schools is well represented by Edward Tolman, who introduced the term 'cognitive maps' for rats performing in absence of reinforcement (Mecacci 1998, p. 208).

Besides, the cognitivists have recovered mentalist arguments by means of the metaphor of computer programs. And, at this time, computers do not have mental states, inner access or consciousness' processes! Connectionism, for example, represents, among contemporary developments of AI, an important result of the philosophical legacy of behaviorism. Neural nets, programs trained algebraically by 'reward and punishment', represent the concrete answer to the dilemma between symbolic and sub-symbolic neurologically plausible representations. One can debate if neural nets are, in fact, representation-free: Luccio, for example, claimed that the representations of neural nets are, simply, the matrices of synaptic weights, and that the evolution of the representations is *symbolized* by the evolution of the

matrices (Luccio 1998, p. 113). But the philosophical legacy of behaviorism is also expressed by those scholars who claimed a neuroscientifically-inspired form of eliminativism about mental content, or by some proponents of dynamic approaches to cognition who criticized the postulation of mental representations, even though both approaches do not imply either the refusal of *all* forms of mental representations, or the refusal of internalistic solutions to mental content.

Behavioral analysis, finally, has still an important role towards classic problems of philosophy of mind: the ontology of *qualia*, the opposition between reductionists and antireductionists, the role of folk psychology, the relation between biological and social influences in the development of mind and problems concerning language and intentionality. These arguments deserve a deeper scrutiny than that possible here. Finally, we agree with the authors that behaviorism is a 'philosophy alive' and that it represents a school of thinking more complex than a rough and incomplete picture of the human being.

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Commentary

Alternative to Cognition. A New Look at Explaining Human Social Behavior

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Alternative to Cognition. A New Look At Explaining Human Social Behavior arises from the need to criticize the cognitive dominance and centrality of cognitive explanation for human behavior. Lee argues that after the "cognitive revolution" psychology appeared to be the only efficient explanation for understanding the complexity of human behavior. However, the focus of psychology on individual thought leads to ignore other important realities for human beings, such as the biological, the social, and the economic realities. Therefore, if psychology wants to become a really scientific explanation, a more critical and flexible approach to our subjectivity is essential. The author does not suggest only a single alternative to the dominance of the cognitive view, but proposes the development of a more flexible and open-minded approach to human social behavior considering the interactions between people and their environment.

In the first chapter, the author investigates why there is a primacy of cognition in the explanation of human experience, and whether the only alternative to behaviorism is the *cognitive* explanation. The primacy of cognitivism is showed by a survey of the long history of consciousness studies from Descartes' dualism to the contemporary cognitive models, which are also dualistic (for example when they use the metaphor of the *mind as a computer*). For what concerns explanation in psychology, Lee notes that all contemporary psychological theories are based on the assumption that conscious thought is a direct cause of behavior, which is the only cause that psychology ought to study. Instead, economic as well as social and environmental influences on choice are completely ignored by contemporary cognitive theories.

According to Lee, all cognitive theories of psychology suffer from a big flaw, namely, they rely on undefined and unobservable variables to explain behavior. However, they are not able to explain how those variables arise from external experiences and events. Moreover, it is unclear how these cognitive variables interact to produce composite cognitions.

In the second chapter, Lee makes her argument more specific by analyzing the theory of self-efficacy and the social-cognitive models introduced by Albert Bandura (1977). Bandura has defined self-efficacy as one's belief in one's ability to succeed in specific situations. One's sense of self-efficacy can play a major role in how one approaches goals, tasks, and challenges. Against Bandura, Lees argues that at the level of theoretical explanation the statement that people behave in certain ways because of their expectations cannot be tested, and that at the level of practical techniques this hypothesis is not useful. In fact, the most important weakness of self-efficacy and similar theories is their inability to make precise predictions. For example,

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there have been many demonstrations that correlations between self-efficacy and behavior (tasks, goals) can be high if self-efficacy is measured specifically and immediately prior to a straightforward, unambiguous task performed in a restricted environment. However, other researches have failed to find any relation between self-efficacy and behavior. Indeed, over the longer term and in less controlled settings, the predictive accuracy of self–efficacy is generally very low. A further problem with the self-efficacy theory and similar models is that data consistent with one's chosen theory are often assumed to support it, although they could equally support alternative models. Lee concludes that Bandura's self-efficacy theory, like other similar models, is a vague descriptive model, and cannot be considered an explanatory theory.

In the third chapter, the author argues that cognitive theories are less than optimal because the theoretical basis of the work is poorly articulated. According to Lee, if the theories that relate cognitive processes and overt behaviors were refined in order to make accurate predictions, it might be possible to identify the circumstances in which particular cognitive variables are useful predictors of behavior and those in which they are not. Finally, it might be possible to find a limited, but useful, role for these theories. For example, cognitive processes are considered to be influenced by the environment and also by stable or temporary biological variables, such as the state of health, hunger and thirst. Furthermore, vicarious experiences affect levels of self-efficacy in a new situation or habit. However, without any clue about how these influences should be measured, we do not have any information that allows predictions beyond those suggested by commonsense.

Lee proposes that cognitive models should be able to define their limits. Indeed, if cognitive-behavioral models only apply to circumscribed situations, within a limited range of behaviors, then one might provide empirical support for them by stating in advance the limits of their applicability. On the other hand, if it is impossible, even within quite narrow constraints, to develop an accurate cognitively based model for predicting behavior, the definition according to which cognitive processes underlie our actions may be less central for understanding behavior than we believe.

In the fourth chapter, the author examines evidence that challenges the assumption that all human behavior is caused by some underlying cognitive processes. Indeed, evidence from a number of areas of psychology shows that cognition is not central for understanding human behavior. According to Lee, one of the major problems with the cognition-centered models of human action is that they are based on confusion between the way human beings appear from the inside and the way in which they actually work.

With this assumption Lee examines critically the view that conscious cognition controls human emotional experience. Although contemporary clinical psychological theories have emphasized the role of cognition in the development or maintenance of negative emotions, evidence from other research areas suggests that this is by no means the only legitimate view. For example, the James-Lange model, according to which emotions arise from a generalized arousal, is no longer tenable. Instead, theories of emotions as grounded in different physiological processes have gained widespread consensus within physiological psychology. In this framework, the understanding of the underlying neural basis must constrain and inform models of emotion. Therefore, Lees concludes that a cognition-centered view of dysfunctional emotion is not a very explanatory model, and that perhaps cognitive models that focus exclusively on understanding emotions are missing something important.

Lee suggests that the use of physiological techniques is useful in order to explore the relationships between emotion and cognition. Recent developments in the measurement and recording of both physiological processes and microbehaviors are now allowing objective

measurements in areas that were previously not available for empirical observation. Therefore, psychophysiology may allow us to understand emotional events. It may also be that future developments in behavioral theory and technique will require a stronger focus on the assessment of physiological events. For example, there are studies conducted by Chabrol, Barrere, Guell, and Moron (1986) showing that the blood flow in the prefrontal cerebral region is lower in clinically depressed than in no-depressed subjects.

In the fifth chapter, Lee examines the concept of unconscious cognition, referring to which one might expect to resolve some problems of the general assumption that all human behavior is the result of conscious or reportable thought. For example, researchers in the fields of memory and perception have made an extensive use of the notion of unconscious thought to explain those behaviors that have been learned without the subject being aware of it. Concepts that fall into this category include procedural knowledge and implicit memory. The author thinks that unconscious thought provides only an illusory explanation. In fact, most psychological theories of human behavior assume that any behavior not determined by an immediate conscious thought is necessarily determined by unconscious cognitive processes. However, Lee argues that, even though it is not possible to assume that people act without an underlying cause (e.g., as a response to an environmental stimulus), some habitual behavior does not involve any cognitive activity.

In the sixth chapter, the author presents the argument for the possibility that at least some human behavior occurs in the absence of anything that can be referred to as cognition. Her main assumption is that we do not «behave in the way we do because of the way we think but […] we think the way we do because of the way we see ourselves behave» (p. 69).

The idea that our cognitive processes arise as a direct result of the observation of our own behavior has a long history in social psychology. For example, the theory of cognitive dissonance strongly supports this hypothesis. Self-perception theories have also provided extensive evidence that individuals do not have privileged access to internal information about themselves. Thus, reports of cognitive variables, such as attributions and attitudes, can be seen as *post hoc* explanations of observed behaviors, instead of the causes of those actions.

There are experimental findings supporting the idea that a development of preferences without cognitive involvement is possible. Kunst-Wilson and Zajonc (1980) demonstrated that affective reactions to neutral stimuli become more positive with repeated exposure to those stimuli. Moreover, subjects were unable to state with any degree of accuracy whether they had seen the stimuli before. This suggests that the exposure had an effect on preference without any conscious awareness. Moreover, Lee reports increasing evidence suggesting that complex and dynamic behaviors involved in social interactions may also be explained without involving cognitive processes. For example, the ability to coordinate verbal interactions and non-verbal gestures during conversation is a complex activity that occurs without conscious effort or control. Finally, models that emphasize cognition support the idea that human beings are different from the other animals. Despite this, detailed works on primates, as well as on humans, showed that vicarious learning occurs in animals and may be explained in classical conditioning terms.

The seventh chapter deals with the position of rational thought in contemporary social and clinical psychology. The author examines some current approaches to rationality and the difficulties that arise from the mutual incompatibility of these approaches. The assumption that the capacity of rational thought is critical in healthy human behavior is also discussed.

The concept of rationality is central to many contemporary psychological theories. For example, the cognitive-behavioral approaches to therapy are based on the assumption that dysfunctional emotional states necessarily arise from irrational thought processes. However the cognitive models of depression and optimism show that a healthy thought and a rational

thought are not necessarily the same thing. Furthermore, other researches suggest that our ability to think rationally may actually be diminished by positive emotion.

There is an agreement about criticizing rational thought. Indeed, Argyle (1991) criticized the hard-line version of cognitive social psychology that assumes social behavior and judgments as a kind of rational problem-solving, performed by computer-like processes in the head. He argued that social behavior in particular could not be understood without considering emotional and contextual variables. Lee notes that arguments like this are largely ignored, because psychologists have a culturally acquired tendency to find mentalistic explanations of human behavior.

In the eighth chapter, Lee argues that there are many reasons why an individualistic and mentalistic approach persists in psychology despite the availability of contrasting arguments and evidence. As Hickey (1994) claimed, a pervasive social hostility to behaviorism has arisen because behaviorism argues against the folk intuition that human action is determined by soul, spirit, or mind, so that the mechanism of human agency lies outside the natural world. Lee argues that another reason why psychology is focused only on the cognitive view is that this represents an unprecedented unity of purposes within academic psychology. However, the author argues that unity has no intrinsic value, but it is the adequacy of a united approach to be important.

Lee adds that a professional and scientific culture that places more importance on cognitive events than on objective reality is likely to encourage psychologists to focus interventions on changing cognition rather than altering objective circumstances. According to Braginsky (1985), by ignoring culture and politics, psychologists have failed to recognize the ideological assumptions underlying their work. Therefore, they have upheld these ideologies rather than examined their impact on the lives of others. That is, the role of clinical, educational, organizational or experimental psychologists has become that of helping individuals adjust to existing social order, rather than of empowering them to make changes to their environment. Lee's point is not that social and cognitive psychology is part of a deliberate conspiracy to maintain social inequalities, but that psychology is often unaware of the cultural basis and the cultural implications of its focus.

In the last chapter, Lee argues that the identification of cognitivism as a unified theory produces a false and premature excitement, and that psychology needs to explore many other alternative explanatory models. Lee points out that she is not advocating a style of eclectic psychology in which all theories are equally right, but that she is distinguishing eclecticism from a relativistic approach that encourages different theoretical developments and supports the idea that each theory identifies its own boundaries and justifies itself in its own terms.

The author claims that the cognitive bias in modern psychology has obscured the progresses that characterize the behavioral theory. Indeed, behavioral models explain a range of human behaviors generally regarded as cognitive, from specific instances such as Eisenberger's (1992) learned industriousness concept, which provides a learning theory explanation for the work ethic, to general models that try to explain essential human characteristics such as language in behavioral terms. The alternative that Lee proposes to cognition is the contextualist behaviorist approach, «a development that aims to combine a non-cognitive approach to human behavior with an increased emphasis on the influences of physical, social, and cultural context on that behavior» (p. 117).

Contextualism is not only a conception of behaviorism because, for example, social psychophysiology is based on the premise that physiological events can only be fully understood within their specific social and physical context. Contextualist behaviorism is a perspective that has arisen, at least in part, as a response to a frustration with an individual-

focused psychology. Lee's conclusions concern the fact that there is a considerable social hostility to a behaviorist and scientific understanding of human behavior. It arises from a discomfort with models that run counter to our dominant dualistic philosophy, from an uneasiness concerning the implications of such models for potentially radical social changes, and from the desire to maintain a comfortable view of psychological distress as something different from ordinary behavior.

This book is not an introductory work. Instead, it is for researchers already involved in the debate, because the author does not characterize cognitivism and behaviorism in a precise way, and she does not explain many topics that she mentions (e.g., self-efficacy theory, theory of reasoned action, theory of planned behavior, protection motivation theory, the model of behavioral choice, attribution theory, and dissonance theory).

The aim of the book is to propose alternative models to the primacy of cognitive theories in order to understand human behavior, but the most part of the book is dedicated only to criticize psychological theory focused on cognition instead of proposing alternative models. Only in the last pages of the last chapter, does Lee propose contextualist behaviorism as a possible alternative.

Therefore, even though I agree with her criticism about psychological theories – especially when she claims that cognitive models are vague and inaccurate, and consequently not able to predict human behavior –, I do not think that contextualist behaviorism is the best model for understanding human action. I believe that in some arguments the book lacks in continuity, for example, in the fourth chapter, when talking about the causes of depression, the author advocates the importance of physiological data coming from neurosciences, but then these are not considered in contextualist behavioral models.

It is clear that Lee's aim is to propose a different perspective on psychology, in particular by giving importance to context and environment as causes of psychological problems, and by criticizing the individual centered models. However, even though the book is useful to focus on environmental conditions, I do not agree with contextualist behaviorism, which is centered only on external factors, because a good model for understanding human behavior has to consider both components, cognitive and behavioral.

We can appreciate Lee's efforts to change a very well established psychological model centered on individual, conscious and rational thought, and arise a critical view among psychologists, who sometimes are too much focused on the subject.

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Commentary

Critical Thinking about Psychology

Edited by Brent D. Slife, Jeffrey S. Reber and Frank C. Richardson
APA Books, Washington DC, 2004

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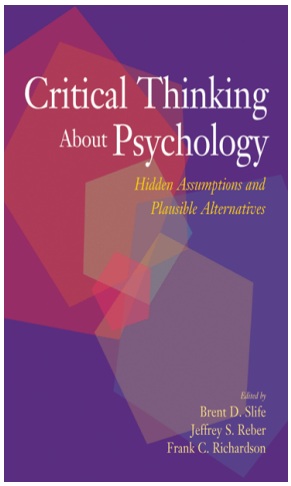

Critical thinking has often been considered to be a cornerstone for many disciplines, and an essential skill for academics, especially within psychology. Indeed, psychologists are taught to use their research and findings to examine common myths and debunk false beliefs. However, do they subject psychology itself to any evaluation? This volume intends to show that, despite the emphasis on critical analysis, rarely psychologists can give a positive answer to the question.

The book is organised into six parts corresponding to some of the major subdisciplines of psychology. Each part is composed of two chapters: the first examines and critiques many hidden assumptions of specific subdisciplines, while the second explores alternative points of view to better understand the human mind. Part I is focused on the discussion of clinical psychology in comparison with virtue ethics. Part II investigates social psychology in the light of constructionist psychology, hermeneutic philosophy and ethical phenomenology. Part III is concerned with the concept of mental disorder – in particular, depression – in experimental psychology and neuroscience. Part IV undertakes a critical analysis of memory and reasoning in cognitive psychology. Part V considers the biases imposed to theories of development by the Western culture. Finally, Part VI concerns statistics methods used in psychology.

In Chapter 1, Frank Richardson identifies and shows the limits of some of the crucial assumptions underlying both the theory and the practice of modern counselling, which he says triumphed in our time after the Freudian revolution in psychology. The first assumption is value-freedom. Indeed, due to its objectivity, psychotherapy thinks to be independent in the same way as other natural sciences from ethics and virtues. That is, moral and cultural values are irrelevant to understand human behaviour and do not matter for psychotherapy. This leads to the contradiction that therapists «ought value being value-free» (Slife et al. 2003). Another strong assumption in modern psychotherapy is that the individual is the basic unit of human reality, which inevitably constitutes a slide toward moral relativism. We can distinguish a utilitarian and an expressive form of such individualism. The first considers human life to be an effort by individuals to maximize their self-interest to reach given ends such as survival, security and satisfaction. The latter is guided by the belief that each person has unique feelings and intuitions that should be pursued and realized. The last assumption is instrumentalism: people think in terms of means-ends and cost-benefit, and are comfortable with such a language. However, instrumentalism leads to social fragmentation, and it fosters emotional problems to people grew under this principle.

Chapter 2 contends that psychotherapy should respond to the dilemmas generated by these assumptions. Blaine Fowers reformulates psychotherapy as a process «of helping clients

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to cultivate the best in themselves and to live the best kind of life available to them» (p. 39). He refers to the Aristotelian Nicomachean Ethics and maintains that virtues like generosity and honesty are the traits that enable us to pursue a good life. Virtue ethics singles out that human "flourishing" – as Aristotle said – is the outgrowth of becoming one's best self. This account is in contrast with instrumental individualism, which sees the social context as an adversary, and not as the arena in which we learn what is good.

Virtue ethics helps people – and therapists – to go beyond instrumental reasons and to focus on what is most important to them. Fowers examines six ways in which virtues and human activities are related, and that therefore should be considered in the therapy. First, virtue ethics portrays human activities as pursuing what we see as a worthwhile goal, because humans are goal-seeking beings who act to accomplish meaningful aims. This suggests to the therapist to help the person to consider what is possible, what is in her interest, and what the consequences of seeking a particular goal are. Second, virtue always shows itself in action, and there is no virtue without concrete activity. In fact, also for a therapist excellence is reflected in the way he acts. Third, to make an action virtuous, we have to act for the right reason. Therefore, the therapist should stimulate the understanding of the praiseworthy end we seek to guide our action and help us to better enact it. Fourth, our actions depend also on our heartfelt emotions, thus we should be aware of what these are. Fifth, virtue is a character, that is, a settled disposition, and does not depend on acting well on an occasional basis. Sixth, since virtue ethic is a practical wisdom, there is no training to recognize what is most important for a client. This imposes to the therapist to learn to make wise choices at the moment.

Part II is about social psychology. In Chapter 3, Jeffrey Reber and Lisa Osbeck consider the implicit assumptions in three of the main topics of social psychology: human sociality, love and helping behaviour. The assumption beyond human sociality is atomism, that is, the idea that the self-properties, although developed in social circumstances, are inherent to the individual and not to the group. Atomism allows studying complex social phenomena in a systematic way both by isolating variables and by maximizing experimental control. However, it restricts the understanding of human sociality. Moreover, the proper subject of social psychology is not only the person, but also the social environment, which exists for and in relation to that person. Since the social whole is different from the sum of its individualistic parts, and atomism is concerned only with the latter, atomism is rejected as a candidate principle for a real social psychology. The assumption beyond love is naturalism, that is «the idea that human beings are fundamentally the same as other objects of nature and as such are subject to the same laws, that determine the behaviour of those objects» (p. 168). According to a naturalistic perspective, love is an adaptive mechanism that facilitates the proliferation of genes. In this way, psychologists have a material referent to investigate it. However, this approach hinders a deep understanding of love. For example, it is inadequate to explain why people attach to love and their experience in intimate relationships. Moreover, if love is a function of natural selection, it would be a natural phenomenon, and should not be studied by social psychology. Finally, the authors analyze the assumption beyond helping behaviour, namely, egoism, that is, the idea that human beings are intrinsically selfish. Egoism has the advantage of explaining human behaviour because there is an endless list of egoistic reasons for either helping or not helping people. Nevertheless, it does not explain the range of altruistic behaviours. Indeed, even though we can always find an egoistic reason for an act, such a reason does not prove that any behaviour is egoistic. The conclusion is that human sociality, love and helping behaviour easily reduce subjective experiences to biological mechanisms.

In Chapter 4, Edwin Gantt examines alternative perspectives on human social life: social constructionism, hermeneutics, and the ethical phenomenology of Emmanuel Levinas. Each of

these three approaches forces social psychology to wide the range of the considered phenomena in order to understand the moral nature of human social relations. According to social constructionism, the reality of human social existence occurs only by considering the complex social relationships between people. Truth and knowledge are the products of the social negotiation and consensus building that constantly take place between people and cultures. Moreover, hermeneutic social psychology aims at understanding human behaviour, and not at explaining it in terms of natural forces. It shares with social constructionism the aversion to atomism and determinism. Finally, Levinas argued that human existence is based on "alterity": that is, we are who we are because we are with others. Our life has a meaning in that we are first relational beings. To be a human being is to exist in obligation to others, and only this allows the possibility for genuine altruism in human action.

Part III regards neuroscience. In Chapter 5, Dawson Hedges and Colin Burchfield consider depression as an example of the current trend to rely on neurobiological explanations in psychology. Materialism, that is, the idea that at some point biological explanation will be able to clarify psychological phenomena, is the assumption that guides neuroscience. Despite that many advances have been made thanks to the materialistic investigation of depression, some assumptions beyond materialism need to be considered: material causal determinism, atomism and objectivism. Material causal determinism is the idea that biological forces determine behaviour. It implies that any psychological change in the individual requires a causal force. For example, even though there is a gap between neurochemistry and the subjective experience of depression, the treatment for depression have to be pharmacological. Atomism asserts that objects are composed by single cells or atoms, and that the properties of an individual can be explained on the ground of their mere inner structure. This means that depression origins from within individuals, not from their interaction with others, and that individual's own neurobiology is responsible for depression. Finally, objectivism assumes that materialistic methods are sufficient to investigate materialistic phenomena, and to provide observable, transparent truth. Unfortunately, if a research is based on unevaluated assumptions, its conclusion will be limited. Therefore, objectivism limits the possibility to extend the results of every research. The authors also consider another consequence of materialism: by eliminating the immaterial side of the brain as a reaction to Cartesian dualism, materialism joins a one-sided dualism, because the immaterial remains a part of the methods for investigating psychological phenomena.

In Chapter 6, Brent Slife and Ramona Hopkins explore the possibility of a true monism, postulating the oneness of mind and body without giving up the notion of agentic responsibility. They propose a holistic perspective, where mind and body are viewed as parts of a larger system in which they mutually constitute one another. Materialism alone is not sufficient for understanding our minds and behaviour. Therefore, we need at least three categories to explain human behaviour: the mind, the body and the context. The authors think that holistic monism is a better framework for neuroscience because it takes into account the limits of current methods, and it is more open to other empirical possibilities. This proposal will facilitate research because it admits also agentic and contextual factors.

Part IV is concerned with cognitive psychology. In Chapter 7, Robert Bishop criticizes four assumptions that make this approach so appealing: efficient causation, instrumental reason, individualism and atomism. Efficient causation is the most widely accepted form of causation in the sciences, and also cognitive psychology wants to explain all questions about human behaviour in terms of it by proposing the "input-processing-output" model. The instrumental view of reason means that agents are immersed in the causal flow of events, and in some way are able to manipulate it. This notion fits well within a physical world of efficient causation, and with the idea that all information is representational. However, it does not consider that

we use other forms of non-representational knowledge (e.g., to understand how to comfort a small child). Individualism and atomism remove psychology from its purpose, that is, the understanding of the human mind and behaviour into his most intimate sphere, which involves also a social and shared context.

In Chapter 8, Stephen Yanchar proposes an alternative contextualist theory based on holism and on the concepts of "lifeworld" and narrative structure. By endorsing holism, contextualism assumes that information is not fragmentised, but that the meaning of one part of a system is constituted and codetermined by the other parts. The parts are already connected and meaningful because of their shared existence within the whole context. The concept of lifeworld refers to the «world of meaningful human activity as experienced prior to detached contemplations and scientific analysis» (p. 176). Lifeworld removes the separation between the inner mind and the external world, because it is presupposed that the mind has unmediated access to objects and people. Finally, the assumption of narrative structures stresses that people experience their lives in narrative meaningful sequences, while for the mainstream cognitive psychology life is based on the metaphor of the machine, so that it is separated in bits and information. Yanchar contends that contextualism does not deny the importance of the results of cognitive psychology, and makes an example of how the concepts of holism, lifeworld and narrative structure can explain empirical findings, such as reconstructive memory and mental imagery.

Part V regards developmental psychology and ethical issues. Brian Vandenberg and Shawn O'Connor want to demonstrate how historical understanding can help critical thinking about contemporary psychology. They use as an example Piaget's theory about the change from theological explanation to material cause in children cognition. Piaget's work constituted an important step toward a new conception of human development. In fact, Piaget interpreted human intelligence as adaption to environment. Thanks to the cognitive functions of assimilation and accommodation, the organism incorporates the world into existing structures and adjusts its structures to the environment. In order to reduce the rift between science and ethical issues, Piaget tried to empirically establish that the universal laws of the progress of knowledge, according to which explanation passes from religious and teleological to material causes in the history of the humanity, applies to individual development. However, his attempt is problematic. Indeed, facts and values, *is* and *ought*, cannot be separated. Moreover, science is not free of moral values and ethical implications. Therefore, we do not need psychological theories incorporating religious beliefs, but theories providing understanding of the meaning of ethical issues in human life.

In Chapter 10, John Chambers Christopher weaves together insights from interactivism (an emerging proposal developed by Mark Bickhard) and the hermeneutic philosophy of Heidegger and Gadamer to explain, in a different way from the universalism of Piaget's developmental psychology, both the universals of human being and the cultural variations. Christopher makes a deep critique to the individualistic cultural roots of current psychology, and to the Western notion of individual. Indeed this notion arises from one cultural tradition and it is not objectively true through reason and science. Cristopher claims that the perspectives he examines maintain that different traditions – though identifying some constants across people – cultivate a variety of moral vision. Developmental psychology should examine the moral vision of other cultural groups, and other accounts of good life, in order to understand also why these accounts are so compelling to those who embrace them.

The sixth part of the book is about methodology. In Chapter 11, Richard Williams examines the assumptions underlying the application of traditional scientific methods to psychological phenomena, and he contends that we should reassess the pretension that psychology is a

science. He claims that the methods of empirical science, namely verification and falsification, are the product of human reason, and they are not unproblematic. With respect to the case of psychological explanations, they do not have the power to support causal inferences. Indeed, the isolation of a cause requires the complete control of other causal influences, and this is in principle not possible. Hence, empiricism fails as a basis for a noncontingent knowledge. Williams invites the reader to search for alternative approaches, which enable psychology to be a more relevant discipline for the understanding of human condition.

In Chapter 12, Jeff Sugarman and Jack Martin follow these suggestions and propose an alternative based on hermeneutics, which is concerned with interpretation of what is to be human and how human understanding is possible. Indeed, hermeneutics starts from an analysis of human nature and its conditions to understand how people think, act and experience their lives. A hermeneutical psychology opposes naturalism and reductionism by assuming that human motivations, thoughts, and actions are immersed in a context of historical traditions and social practices. Moreover, unlike natural phenomena, psychological phenomena require interpretative practices because interpretation is strictly involved with the subject of psychology, and does not concern only its interpretation.

The book ends with a reflection by Suzanne Kirschner, who resumes the main merits of every section and wishes that the volume will open the mind of students by making them understand that one's "truth" can always be imperfect and partial.

This volume is very rich and accurately organised. The reader is accompanied in the lecture by a well defined and clear structure. Even if each chapter has a different author, all of them share the same approach. The book critiques the most common beliefs in psychology, but owing to its arrangement it is quite easy also to individuate some of the "alternatives" proposed. Therefore, it is a very good instrument for students who want to take a hard look at some fundamental themes in psychology and go beyond traditional assumptions.

All the chapters provide good cues and give the chance to understand the topics from different points of view. I want to highlight, in particular, the first chapter about clinical psychology, which, by opposing the great diffusion of psychotherapy and psycho medicine in our time, stresses the importance either of values that we have lost or of facets of our life that we often forget. Despite the number of scholars who contributed to the volume, all of them focus on the same issues: the conviction that the human beings should be analysed in their context, as a "social animal", the importance of assuming a holistic perspective and opposing individualism, the wish to abandon a perspective grounded on materialism and determinism, the need to avoid to give importance only to "scientific data", as if psychology were a science. The authors share the idea that psychology cannot be considered a science just solely for the fact that interpretation and hermeneutic are at the basis of its work and investigation.

This approach is meaningful and of great interest for it attempts to develop a critical thinking in people studying psychology, but runs the risk of taking an explicit direction that might involve some others limitations.

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Commentary

La costruzione del soggetto. Le origini storiche della ricerca psicologica

Kurt Danziger

Laterza, Roma-Bari, 1995

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Credo sia utile cominciare il percorso di riflessione sul testo di Kurt Danziger con qualche punto di domanda: che ruolo riveste oggi la storia nel campo dell'istruzione scientifica? Che importanza e quale funzione può avere la storia della scienza nella formazione universitaria? E ancora, visto che in questo fascicolo di *Humana.Mente* si tratta proprio di pluralità: quale storia serve a cosa?

Sono domande certo non nuove. Accompagnano – e spesso precedono – il processo stesso di istituzionalizzazione della storia della scienza nelle università. Per esempio, per quel che riguarda l'Italia, facendo un riferimento specifico al caso della medicina, va ricordato come nel corso dell'Ottocento, quando già risultavano attive nelle università di Napoli e Bologna le cattedre di Storia della medicina (rispettivamente tenute da Salvatore De Renzi e da Angelo Camillo De Meis), le prospettive sulle finalità e sul ruolo assegnato a questa disciplina fossero tutt'altro che argomento di concordia; da una parte, la visione di chi ravvisava nella storia della medicina al più lo spunto per un accessorio arricchimento culturale – quasi uno svago, nel quadro della severa formazione scientifica: era questa la posizione di Salvatore Tommasi, esponente di spicco della medicina sperimentale italiana nonché autore, nel 1865, di uno dei 'manifesti' del positivismo italiano. Dall'altra, la prospettiva del già citato De Meis, convinto che la storia della medicina potesse e dovesse servire a coltivare generazioni di medici epistemologicamente consapevoli, dotati di cautela e umiltà gnoseologica, di uno spirito critico che solo lo sguardo retrospettivo sulla propria disciplina avrebbe potuto saldamente fondare.

Sono due posizioni che esemplificano agli estremi i termini di una questione in realtà assai complessa. Ciò non di meno, nella loro esemplare contrapposizione, sono posizioni che restano vive e che offrono spunti di dibattito e/o scontro anche ai giorni nostri. Da un lato, la visione accessoria di una storia considerata come predicato della scienza attuale, per la quale il passato viene descritto come la cronaca di una successione di figure e scoperte, ordinate sulla base della loro rilevanza e rispondenza agli attuali canoni di scientificità (che, va detto, sono sempre canoni di scientificità specifica, relativa cioè a determinati indirizzi, particolari scuole e correnti). In questo caso la storia della scienza riflette l'assetto, distinto per competenze e approcci, di una rigida specializzazione settoriale. Questo elemento rafforza l'appartenenza più che il confronto, o presta i suoi argomenti più alla rivendicazione e allo scontro che al dibattito e al dialogo. Sul fronte opposto, si pone una visione formativa più ampia: la storia come veicolo per una riflessione critica e articolata, non assolutistica, sul proprio sapere. Essa diventa fattore non di mera istruzione (chi ha inventato o detto cosa e quando), ma fonte di energia viva del pensiero e della pratica professionali. In questa seconda direzione sembra oggi

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procedere – per rimanere nel campo della medicina – la storia coltivata nel vasto arcipelago disciplinare a cui è stato dato il nome di *Medical Humanities*.[1]

E la psicologia? E la storia della psicologia? Quanto fin qui osservato per la storia della medicina costituisce un esempio in un certo senso paradigmatico anche per la storia di altre discipline scientifiche. Si tratta di un nodo cruciale: quello dei rapporti fra scienza e storia. E su questo nodo il libro di Danziger ci porta a riflettere proprio per quello che concerne lo specifico ambito della psicologia.

Per comprendere alcuni degli spunti di riflessione più interessanti offerti da *La costruzione del soggetto* conviene forse soffermarsi un momento su alcuni aspetti relativi alla figura e alla formazione del suo autore.[2] Nato nel 1926 a Breslau, Kurt Danziger cresce e studia in Sud Africa, dove si trasferisce con la famiglia quando è ancora un bambino. Terminata la formazione universitaria – si laurea con una tesi di psicologia sociale a Cape Town – parte per Oxford; qui svolge il dottorato presso il Dipartimento di psicologia, sotto la guida di George Humphrey. L'orientamento delle sue ricerche, in questa fase, è di stampo prettamente empirico e sperimentale: studia in laboratorio il comportamento animale seguendo i modelli classici delle ricerche sui topi.

In questo stesso periodo, tuttavia, comincia a nutrire una certa insofferenza e insoddisfazione per l'orientamento di ricerca intrapreso. Complice l'incontro con Niko Tinbergen – futuro premio Nobel e padre dell'etologia, in quegli anni in visita a Oxford – Danziger comincia a maturare un cambiamento profondo: l'uso di modelli sperimentali standard per lo studio del comportamento animale gli sembra non portare da nessuna parte. Le condizioni artificiali dell'osservazione in laboratorio gli sembrano imporre all'oggetto di studio una limitazione falsante.

Ciò nondimeno, Kurt Danziger è ormai coinvolto in un lavoro che non può abbandonare, costituendo il suo programma di ricerca per il conseguimento del dottorato. Finisce perciò quel che ha cominciato, ma contemporaneamente decide che la fine di quel progetto segnerà per lui la fine di una stagione: dopo il conseguimento del Ph.D si dedicherà ad altro.

E così è. Nel 1951, ottenuto il dottorato, decide di cambiare pagina. Parte per l'Australia, dove lavora nel dipartimento di psicologia dell'Università di Melbourne. Qui svolge nuove ricerche sui processi di socializzazione nei bambini. Il passaggio dalla psicologia sperimentale alla psicologia dello sviluppo rappresenta un momento cruciale: segna il progressivo riavvicinamento di Danziger all'alveo originario degli studi di psicologia sociale da cui, ancora studente, aveva preso le mosse.

A metà degli anni Cinquanta, dopo la parentesi australiana, Danziger torna in Sud Africa, presso l'Università di Natal, dove conosce Gordon Allport, psicologo sociale statunitense noto per le sue ricerche sul pregiudizio. L'incontro con Allport è uno stimolo a riflettere sui rapporti fra psicologia e contesto sociale, e più in particolare sull'influenza esercitata dalle condizioni sociali sui contenuti della ricerca psicologica. Gli studi di Allport sul pregiudizio, infatti, trovano difficilmente modo di inserirsi in un contesto scientifico come quello sud africano, dove gli aspetti soggettivi, *psicologici,* del pregiudizio risultano un elemento tutto sommato secondario di fronte alla drammatica realtà *oggettiva* della discriminazione dell'apartheid.

[1] Cfr. M. Conforti, G. Corbellini, *Storia della medicina e medical humanities,* in R. Bucci (a cura di), *Manuale di Medical Humanities,* Zadig, Roma, 2006.

[2] Cfr. A. Brock, *An interview with Kurt Danziger,* reperibile all'indirizzo: http://psychology.dur.ac.uk/eshhs/newsletter/interview.htm

Le riflessioni maturate in questa direzione a Natal, con Allport, hanno modo di approfondirsi e di articolarsi ulteriormente negli anni successivi, con il trasferimento di Danziger in Indonesia. Il primo contatto con un mondo totalmente 'altro', con una cultura profondamente diversa da quella occidentale, è un'esperienza importantissima. A Jakarta non si usa nemmeno la parola 'psicologia': si preferisce l'espressione 'scienza dell'anima'. Tutto è diverso: metodi, concetti, teorie – tutto ciò che nella quotidiana pratica di ricerca sembra essere dato per acquisito e scontato, qui non lo è. Ci sono altre parole, altre prospettive. Un mondo altro, un'altra scienza.

Dall'Indonesia Danziger farà ritorno in Sud Africa – questa volta a Cape Town – con una visione molto più marcatamente culturologica e sociologica di prima. Siamo negli anni Sessanta. Lo studioso assume la direzione del Dipartimento di psicologia dell'Università. Manterrà l'incarico fino a quando non sarà costretto a lasciare il paese, questa volta a causa dell'evidente ostilità del Governo nei confronti del suo attivismo politico anti-apartheid.

È la volta del Canada. Danziger approda alla York University di Toronto e vi si stabilisce definitivamente. Qui allarga ulteriormente i suoi campi di indagine: alla psicologia sociale e dello sviluppo affianca la storia della psicologia.

Quello per la storia della propria disciplina è un interesse che Danziger coltiva già da tempo. Adesso, però, il suo impegno su questo fronte diviene sempre maggiore, sistematico. Un interesse che incarna un'ulteriore faccia del nomadismo scientifico – oltre che geografico – di questo studioso, e che forse nasce anche dall'esigenza di rintracciare, di quel nomadismo, ragioni e radici: apparente paradosso delle avventure intellettuali non disgiunte dalla *vita*. Non a caso, partendo dalle origini della psicologia sperimentale wundtiana, Danziger allarga progressivamente la sua prospettiva all'analisi dello sviluppo storico dei diversi metodi della ricerca psicologica.

Frutto maturo di questo percorso è *La costruzione del soggetto*. Edito nel 1990 e tradotto in italiano nel 1995, il libro prende in esame l'evoluzione e la varietà delle pratiche di ricerca psicologica in un arco di tempo che va dalla fondazione del laboratorio di Wundt a Lipsia, nel 1879, fino all'inizio della II guerra mondiale. Fra questi estremi temporali, che circoscrivono "il periodo formativo della psicologia moderna" (Danziger 1990), possiamo assistere al graduale cambiamento di concetti, teorie, metodi, oggetti e obiettivi della ricerca. Danziger prende, infatti, in esame la trasformazione delle pratiche psicologiche, sottolineandone la diversa collocazione istituzionale e sociale.

Il laboratorio di Lipsia rappresenta il punto di partenza poiché la sua fondazione segna un momento in cui «la psicologia scientifica fu intesa per la prima volta come un'attività organizzata e autoconsapevole di una comunità di ricercatori» (Danziger 1990, p. 25). Con Wundt la sperimentazione psicologica diviene pratica sociale condivisa da un gruppo di 'addetti ai lavori'. Vengono definiti i suoi metodi – l'analisi sperimentale di sensazioni e percezioni, coniugata a una severa pratica introspettiva 'ripulita' da ogni soggettivismo – e il suo oggetto – lo studio della causalità psichica.

Sullo scorcio della fine del XIX secolo, tuttavia, il modello di sperimentazione di Lipsia non è l'unico ad imporsi. La varietà dei 'canoni' sperimentali viene presentata da Danziger come un fattore originario e caratteristico della ricerca psicologica; così, di fianco al nome di Wundt ne troviamo altri, come quello di Binet in Francia, o di Galton in Inghilterra. Si tratta di modelli profondamente diversi da quello tedesco, che attingono a specifiche matrici scientifiche e che rispecchiano l'inserimento delle comunità di specialisti in diversi contesti culturali e nazionali. Così come sono diversi gli oggetti e gli scopi, oltre che i metodi, della ricerca: dallo studio della causalità psichica, condotto con la sperimentazione su soggetti adulti, normali e istruiti alla tecnica sperimentale, si passa con Binet (modello clinico) e Galton (modello antropometrico) allo studio delle facoltà mentali, a scopo educativo e/o selettivo. In breve: da una ricerca –

come quella tedesca – rivolta al chiarimento dei processi mentali 'universali', cioè non legati alle caratteristiche individuali, si passa a una ricerca che mira proprio allo studio di queste ultime.

La prospettiva adottata da Danziger nel suo testo è di tipo marcatamente sociologico. Che si concepisca come studio delle leggi dell'accadere psichico, o come indagine clinica, o come selezione antropometrica, la psicologia è sempre un prodotto umano i cui contenuti risultano strettamente legati alle società in cui si produce la ricerca. Il termine società va peraltro qui inteso in diverse accezioni, poste ovviamente fra loro in rapporti d'interazione: in senso 'ristretto', cioè riferito alla comunità scientifica, e in senso più ampio, relativo al contesto sociale nel quale gli stessi gruppi di addetti ai lavori si ritrovano ad operare.

Nella scienza così intesa, appunto come attività sociale, una questione cruciale è quella del consenso. Anche in questo caso, il termine va inteso in due direzioni: come consenso interno alla comunità scientifica – problema su cui pure si è cimentato un classico della sociologia della scienza, come *La struttura delle rivoluzioni scientifiche* (Kuhn 1962); e come consenso esterno, spesso traducibile nei termini di un problema di spendibilità sociale della ricerca scientifica. Quel che è certo è che in entrambe le accezioni, il consenso a un determinato modello di ricerca non viene accordato sulla base di valutazioni meramente razionali e logiche, quanto piuttosto sulla base della sua rispondenza a domande poste dai potenziali consumatori dei frutti della ricerca. Non esiste dunque un modello 'più vero'; semmai, esiste un modello più opportuno. In questo senso si può inquadrare, per esempio, l'eclisse del modello wundtiano nel quadro della psicologia statunitense:

> Dopo il 1890 – scrive Danziger – lo stile originario di Lipsia […] non dominò più realmente la disciplina. Nel migliore dei casi, era un modello tra gli altri. Mai dominante, questo modello perse d'importanza fino quasi a scomparire nella psicologia americana durante il periodo precedente la II guerra mondiale. (Danziger 1990, p. 87)

Da questo momento in poi diventa preminente una psicologia in grado di prestarsi ai fini applicativi:

> Ciò che si perseguiva era una conoscenza che potesse essere utilizzata rapidamente dalle agenzie di controllo sociale in modo da rendere il loro lavoro più efficiente e difendibile in modo razionale. La conoscenza che permetteva di fare delle predizioni comportamentali serviva a questo scopo. (*Ibidem*)

Non era certo questo il tipo di conoscenza prodotto dagli studi di stampo wundtiano.

Sottolineando la molteplicità delle prospettive di ricerca psicologica nonché il loro vario destino in relazione alla struttura sociale, Kurt Danziger si muove nel solco di un'accesa attitudine critica verso la rigidità degli imperativi metodologici legati alla assolutizzazione di approcci e prospettive. L'analisi storica, in questa direzione, dà uno strumento all'ex sperimentalista insoddisfatto per mostrare come l'espressione psicologia scientifica non possa essere intesa in modo univoco. La storia, armata del suo grimaldello sociologico, diviene così per Danziger uno strumento critico essenziale e potenzialmente 'sovversivo', poiché, mostrandone le radici e lo sviluppo, rivela che i metodi di ricerca sono prodotti umani questionabili e non assunti indiscutibili calati dal cielo.

Non esiste quindi una sola psicologia, un solo criterio valido di scientificità legittimato sulla base di una pura coerenza logica. Esistono invece molte psicologie, diversamente articolate in base a diverse impronte sociali. La psicologia scientifica non è *una*, ma plurima. Meglio allora è parlare di psicologi*e* scientifich*e*. E ricordare come queste siano socialmente determinate: «nel

parlare di un campo come la psicologia scientifica, parliamo di un dominio di costruzioni» (Danziger 1990, p. 4).

La distinzione (e contrapposizione) fra una psicologia intesa come scienza naturale e una psicologia intesa come scienza umana rappresenta dunque solo l'estrema semplificazione di una pluralità di approcci che contraddistingue la psicologia sperimentale stessa. La storia, svelandolo, può essere perciò anche intesa, non troppo indirettamente, come strumento di possibile dialogo fra le diverse anime della disciplina.

Certo è che dalla lettura proposta da Danziger il concetto di scienza viene a perdere un po' della sua aurea, guadagnata obbedendo a un modello mutuato dalle scienze fisiche, secondo cui la conoscenza viene rappresentata come un processo cumulativo di verità. Si tratta di un modello che ha influenzato a lungo – e ancora influenza – anche il lavoro dello storico:

> A molti psicologi è stato insegnato a caratterizzare la propria disciplina entro uno schema concettuale che proviene dalla fisica dell'Ottocento. Questi psicologi vedono se stessi come singoli ricercatori che cercano di accumulare dei fatti su alcuni aspetti della natura attraverso l'uso di ipotesi e tecniche appropriate. Quando descrivono lo sviluppo storico del loro campo, tendono a farlo pressappoco nello stesso modo, rappresentandolo come una successione di singoli studiosi che accumulano 'scoperte' sulla base di ipotesi progressivamente sempre più raffinate e con l'aiuto di strumenti sempre più sofisticati. (Danziger 1990, p. 4)

Nella prefazione a *La costruzione del soggetto* la presa di distanza da questo orientamento, tipico di una storiografia internista, è esplicita. Kurt Danziger torna poi anche in seguito a riflettere sull'argomento, a cui dedica un articolo dal titolo significativo: *La storia della psicologia ha un futuro?*. La questione qui affrontata è soprattutto relativa a quale futuro si voglia riservare alla disciplina. La posizione di Danziger è chiara: se la storia della psicologia viene asservita al modello delle scienze sperimentali, il suo futuro si esprimerà, al meglio, in una forma di buon antiquariato. La sua sarà dunque una prospettiva molto limitata. Se, invece, gli storici non si lasceranno sedurre dalle tentazioni delle certezze interniste, si potrà allora produrre una storia di più alto livello. Una storia critica che mentre si pone fuori dal modello sperimentale, ne mostra la variabilità, la componente soggettiva e sociale, offrendosi sia come veicolo di dialogo fra le diverse anime della psicologia, sia come strumento per la coltivazione di una maggiore consapevolezza professionale.

Si può decidere, allora, da quale parte stare, quale storia fare. Ma che nel farlo si stia operando una scelta, e dunque un'operazione che non ammette ingenuità. Socialmente e istituzionalmente parlando, del resto, anche qui, come altrove, si tratta pur sempre di una questione di consenso.

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