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THE GENERAL PSYCHOLOGICAL CRISIS AND ITS COMPARATIVE PSYCHOLOGICAL RESOLUTION

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ABSTRACT: The crisis in general psychology is identified as one of theoretical indeterminacy. An important source of indeterminacy is the form of generalization that emphasizes classification and common characters and identifies the general with the abstract. Determinate theory requires a form of generalization that identifies the general with the concrete and emphasizes genesis and interconnection. In order to overcome its crisis, general psychology thus requires the kind of evolutionary methodology that comparative psychology is, historically speaking, best prepared to provide.

> Science must begin with that with which real history began. Logical development of theoretical definitions must therefore express the concrete historical process of the emergence and development of the object. Logical deduction is nothing but a theoretical expression of a real historical development of the concreteness under study. (Ilyenkov, 1982, p. 200)

The decade of the 1970s was a difficult period for comparative psychology. The troubles first became apparent somewhere around 1950 when Frank Beach called attention, in a deploring tone, to our "excessive concentration" on a very limited number of species, especially the ubiquitous *Rattus norvegicus*. It is now clear to me that these "troubles" did not in fact amount to a real "crisis." This, then, is as good a place as any to recant my earlier claim that they did so (Tolman, 1984).

In the same year, 1950, and for not dissimilar reasons, we were taken to task by Konrad Lorenz who found it "misleading if psychologists, who evidently are not familiar with what we (biologists) mean when we speak of the comparative method, apply the same term in a very loose sense to all behavior studies concerned with different forms of life" (p. 239). His reaction was a biting one: "I must confess that I strongly resent it, not only from the terminological viewpoint, but also in the interests of the very hard-working and honest craft of really comparative investigators, when an American journal masquerades under the title of 'comparative'

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psychology, although, to the best of my knowledge, no really comparative paper ever has been published in it" (pp. 239-240). The doubts seemed to mount until 1969 when it was announced by Hodos and Campbell that the evolutionary aspirations of comparative psychology were misplaced and there could consequently be no comparative psychological theory. Lockard (1971) followed quickly to announce the "fall of comparative psychology." And then, whatever pieces might be left, Wilson announced in 1975, would soon be "cannibalized," along with our sister discipline ethology, by sociobiology.

The responses to all this were varied (e.g., Tobach, Adler, & Adler, 1973; Wyers *et al.*, 1980), but I should hope that by now most thoughtful observers will agree with Gottlieb (1984) that comparative psychology has survived, does have theory (if not *a* theory), and that this theory is potentially and genuinely evolutionary (cf. also Ardila, 1986, and Ribes, Ibañez, and Hernandez Pozo, 1986). The onslaught of the skeptics and disbelievers has done us the great favor of awakening us from our "dogmatic slumbers" (Kant). We are probably more keenly aware now of the evolutionary-theoretical potentials of comparative psychology than ever before in our history. I shall return to this line of discourse momentarily.

THE CRISIS IN GENERAL PSYCHOLOGY

In the meantime, while comparative psychology was experiencing its own peculiar difficulties, general psychology was going through what was recognized both inside and outside the discipline as a genuine "crisis." As one psychologist put it: "Whether they (psychologists) are experiencing an identity crisis, a paradigmatic crisis, or a crisis of confidence, most seem agreed that a crisis is at hand . . ." (Elms, 1975, p. 968). In his introduction to the 1975 Nebraska Symposium on Motivation, each of the eight contributions to which was in its own way crisis-oriented, the editor, William Arnold, expressed his understanding of the matter as follows:

> To the extent . . . that psychology has been committed to an older, Newtonian conceptual system (and its interlocking philosophical assumptions), it became apparent that we, in our domain (psychology), have been guiding ourselves by a limited conception of science and, accordingly, by a restricted conception of the human being. Nonetheless, in recent years many psychologists have been acquiring a sharper perception of the mutable, evolutionary character of all theoretical conceptions—including the regnant conceptions in psychology. As a consequence psychology appears to be in a period of transition or "crisis." (Arnold, 1976, p. vii).

Two implications of this statement should be examined a little more closely before going on to specify the exact nature of this crisis. First, Newton is getting here a little more of the blame than he deserves. The problem was not the "Newtonian conceptual system" as such, but rather the 19th-century positivist version of that system. The importance of this distinction will become more evident as we see the specific nature of the crisis more clearly. Second, it is worth noting that the alleged insight regarding the "evolutionary" character of psychological concepts is not elaborated by Arnold. It turns out, however, by the present analysis, to be highly prophetic.

So, what is the precise nature of the crisis? I will present here quotations from several participants in the crisis debates in order to show that a consensus on the focus does in fact emerge. First, Amedeo Giorgi, a contributor to the 1975 Nebraska Symposium:

> Clearly, there is a lack of unity in psychology with respect to theories, methods, importance of data, definition of subject matter and almost every other important dimension. Moreover, the same critiques concerning the science of psychology perseverate. Why? Because somehow the organization of the field of psychology is not meeting the pervasive needs of all of its practitioners. And this, in turn, is because its organization has not yet found the central viewpoint for integrating the various facets of its field. So one meaning of lack of unity in psychology is the fact that the point of view or central perspective for integrating the disparate aspects of psychology has not yet been achieved or elaborated. This means that the perspective adopted is exhausted before one comes to the end of commonly accepted psychological phenomena. Or it means that one can comprehend the totality of psychological phenomena only by excessive abstraction or ambiguous labeling (Giorgi, 1976, p. 285).

Although his main concern is with social psychology, the assessment of the crisis by Moscovici (1972) is similar:

> The respect of common sense, the proliferation of experimental studies lacking theoretical preoccupations, and the isolation of various areas of research in social psychology combine to explain the accumulation of facts and notions which do not amount to real progress since they are not conceptually integrated and since no theory is, in any real sense, disconfirmed or replaced by another. The concepts employed have their origin in other fields; theoretical models exist side by side in a relationship which neither constitutes real dialogue nor fertile contradiction. It is therefore not surprising that the empirically established facts are nothing but a heterogeneous collection, as are the theories on which they are supposed to depend. The experiments and empirical studies are not really capable of confrontation in a common framework . . . . (Moscovici, 1972, pp. 43–44)

Catania (1973) was obviously seeing the same state of affairs when he wrote:

> Students of psychology still are asked to choose theoretical sides. They see functional accounts of operant behavior pitted against ethological accounts of behavioral structure, analyses of reinforcement contingencies pitted against theories of cognitive processing, and descriptions of language as verbal behavior pitted against psycholinguistic formulations of language competence . . . psychologists are not yet even agreed on whether theirs is a science of behavior or science of mental life. (Catania, 1973, p. 434)

The focus that emerges in these and many other assessments is succinctly captured in the title of Arthur Staats' 1983 book, *The Crisis of Disunity in Psychology.* It appears that psychologists suddenly found themselves without any real basis for deciding among theoretical alternatives. Even the traditional basis in "facts" or data appeared to have abandoned them. This was recognized already in 1966 by Hilgard and Bower who observed then that: "One of the most perplexing problems within psychological research is how to make the gains cumulative, that is to build a firm foundation from past research on which to plan towards the future" (1966, p. 582). After rehearsing a litany of problems, such as latent learning and peripheral versus central mediation, that had not been resolved by "crucial" experiments, they concluded that: "Accumulation of knowledge means neither mere fact-gathering nor isolated hypothesis-testing, but thoughtful systematic approaches to meaningful questions leading to *conclusive thinking*" (1966, p. 583).

This need for an adequate theoretical solution to the crisis was echoed in 1972 by Harré and Secord, whose remarks, though directed at social psychology, apply just as well to general psychology:

> The need for a comprehensive theoretical treatment of social psychology and for a reformed methodology we feel to be pressing, and to be evident from the increasing dissatisfaction with the state of social psychology, even within the citadels of the profession. The underlying reason for this state we believe to be a continued adherence to a positivist methodology, long after the theoretical justification for it, in naive behaviourism, has been repudiated. At present there is scarcely any coherent body of theory. In such a vacuum it is still possible to carry on empirical studies which make sense only if people are conceived of in the mechanical tradition as passive entities whose behaviour is the product of 'impressed forces,' and whose own contribution to social action is the latent product of earlier impressed experience. A methodology of experiment survives in which the typical investigation is recommended to be a manipulation of 'variables,' and the typical result a correlation in the manner of Boyle's Law. (Harre & Secord, 1972, p. 1)

There is remarkable agreement among the assessments sampled that the general nature of the crisis is one of lack of unity. I understand this to mean a lack of coherence, a lack of agreement among psychologists about what counts. The principal symptom has been designated as "theoretical

indeterminacy" or as the "Beliebigkeit psychologischer Theorien," as it has become known in a particular segment of the German psychological literature (Holzkamp, 1978). We are faced with numerous theories of personality, motivation, learning, social influence, development, etc., with no apparent rational grounds for choosing from among the alternatives or resolving their differences, and therefore with no basis for the progressive accumulation of coherent psychological knowledge.

It should not be imagined here that all psychologists have been equally distressed by this situation. A small, but influential number of psychologists has taken it as the necessary state of affairs, and has even welcomed it openly as a relief from what Sigmund Koch called "single-principle imperialism" (Koch, 1981). Other examples of such advocacy of pluralism are found in the work of Dixon (1983), Gergen (1981), and Royce (1982). Even Hilgard appears to have retreated toward some kind of pluralism (Hilgard, 1987, p. 803). The major difficulty with all these versions of metaphysical pluralism is that they lead logically, and therefore necessarily, to nihilism and solipsism. As I shall attempt to show in a moment, we need not accept this state of affairs as necessary; there *is* a solution to the problem of theoretical indeterminacy.

A second important conclusion that can be drawn from the crisis debates is that the solution is not a strictly empirical one. Perhaps the most important lesson to be learned from this historical development is that the accumulation of coherent knowledge is not achieved through mere induction from facts or data. The "crucial experiment" (i.e. an appeal to more data—presumably of the "right" kind—for an automatic resolution to theoretical differences) is a myth. While the right data are indeed important, even indispensable, they are not sufficient in themselves to resolve theoretical indeterminacy, thereby overcoming the crisis. As Hilgard and Bower put it in 1966, we need "conclusive thinking," whatever that is. This is the point to which we shall return in a moment.

A third conclusion is one that is essential to our understanding, though it will not preoccupy us here. This is that the philosophical root of the problem is not Newtonian materialism, but the so-called "logical" form of 19th-century positivism. It is well known that one of the main goals of modern positivism was the unification of science under some all-embracing theoretical framework. It is equally well known that all attempts at this ended in miserable failure. The reason was positivism's inability to resolve, within the framework of its own assumptions, the problem of verification (Passmore, 1967). Now the problem of verification is precisely the problem of theoretical determinacy. At the bottom of this difficulty with verification is the separation of the thing-as-known from the thing-in-itself inherited from Humean empiricism via Kantian idealism. The lesson from all of this is that an adequate metaphysical foundation for scientific thinking is, at the very least, one that grants the

access of cognition to objects themselves. The only way in which we can comprehend our agreement that the grass is green is by appeal to the fact that the grass *is* green, with the latter state of affairs having priority over the former. But it is now clear from our second conclusion that this kind of "direct realist" empiricism, while essential, will not alone provide the solution we are seeking. What, then, is the additional theoretical/methodological requirement for an acceptable solution?

TOWARD A RESOLUTION OF THE CRISIS

It is important to note at the outset that the solution I will describe is not new. It is an understanding of scientific activity that is evident in the behavior of scientists beginning at least with Galileo. The fact that crises like psychology's have not occurred in physics or biology is, I believe, largely a reflection of this. It is an understanding however, that has been neither recognized—at least as such—nor articulately developed in English-language philosophy of science. In short, there has come about a discrepancy between the practice and theory of science that has effected more mischief in psychology than in any other science (although deleterious effects are noticeable to one degree or another in all of the social sciences, for which there are good historical reasons, which we are unable to develop here).

The problem, and its solution, lies in our understanding of the process of generalization that forms the basis of all theory construction. What our theory of scientific practice has failed to recognize is that there are two distinct, broad forms of generalization: one which emphasizes classification and common characters, and identifies the general with the abstract; and a second which emphasizes the discovery of genesis and interconnection, and identifies the general as concrete (actually "concrete universal"). It is the first that has almost exclusively dominated theory-building efforts in psychology since the early part of this century. This empirical generalization to the abstract seeks to identify traits (or principles) that are common to the largest number of instances, aiming of course at universality. Davydov (1984, pp. 20-21) cites six characteristics of "empirical knowledge," which is his term for the product of this form of generalization:

> 1. Empirical knowledge is produced by comparing objects and their representations which makes it possible to discern in them the same general traits.
>
> 2. Comparison discerns the formally common trait which makes it possible to classify separate objects under a certain formal class irrespective of their being related to each other.
>
> 3. Empirical knowledge, which is based on observation, reflects only external traits of objects and for this reason completely relies on perceptual conceptions.

4. The formally common trait is equal to the individual traits of objects.
5. The concretization of empirical knowledge consists in gathering of illustrations or examples which belong to a formally derived category.
6. The necessary means of crystallizing empirical knowledge is the word or the term.

We will not take the time to work through examples here. It should be fairly evident that these characteristics apply to such categories as "reinforcement." Just consider all the concrete detail that must be ignored in order to achieve the claimed universality of this notion. Note also that this category refers not only to a trait or character of learning, but has been advanced as a *theory* of learning. (It is instructive to note here that while the "law of reinforcement" is an empirical generalization, the "law of falling bodies" is not.) The psychology of personality and motivation are replete with examples of this sort. Whole theories of personality have been advanced on the basis of "self-esteem," while whole theories of motivation have been advanced on the basis of "achievement" or "social comparison."

There is no denying that this kind of abstraction and generalization has its place in scientific work. The difficulties arise when it is taken as the principal or only form of generalization. When this occurs, it becomes the main source of theoretical indeterminacy, which, it will be recalled, I have identified as the cause of the crisis in general psychology. How does this happen? There are at least two ways. First, there are no constraints within the abstraction process by which to decide what is to be abstracted. Any class of objects is likely to yield any number of equally "good" abstractions. This is undoubtedly the case in personality theory with its endless lists of traits. What's more, the formal criteria can lead to results that are clearly misleading. For example, we might abstract from the class "mankind" both the trait of soft earlobes and toolmaking. In fact the former is far more general, indeed it is universal, yet hardly anyone with a modern scientific understanding would be willing to conclude that soft earlobes are more essential (because universal) to being human than toolmaking. At any rate, it is clear that any number of competing theories can exist on this basis about just about anything. They may all be equally "true," and there appears to be no criterion by which to select among them. One seeming solution is the eclectic one, but this cannot be a real solution as it does not overcome the basic difficulty.

The second source of indeterminacy in this form of generalization is a circularity that, given no additional guides, can only be resolved arbitrarily. This method of generalization cannot actually discover the characters of any particular category because it must first define the category in order to decide what is general to it. The abstraction and generalization process cannot, in short, define the category. If, therefore, initial definitions cannot be determined by the scientific investigative process (generalization), then they are necessarily arbitrary, and noth-

ing prevents different investigators from beginning with different definitions. Then, owing to the different definitions, disparate lists of general traits will perforce be found. Again, there is nothing in the process of generalization to which one can appeal in order to find a rational resolution of these differences.

The second form of generalization, called "substantial generalization" by Davydov (1984), overcomes these difficulties by allowing the object or category to define itself. That is, it contains within it the means by which initial definitions can be corrected in the process of investigation. According to Davydov:

> Substantial generalization reveals the essence of objects in the form of developmental laws showing what defines their development. The object of this type of generalization is to reveal a law which is the necessary connection of specific phenomena within a whole, the law of the origin of this whole. The revelation of the general nature of a real relation takes place in the analysis of such specific features which allow it to be a genetic basis of a developed system. The beginning of the concretization of these particular phenomena is the essence of the discovery of the universality of the relation in question (Davydov, 1984, pp. 12-13).

The word "genetic" is used here in its original sense as "pertaining to the origin, history, and development of an organism" (English & English, 1958, p. 223).

But by what criterion do we recognize that we have achieved such generalization? The answer is straightforward: it is the answer to the question "whether the particular phenomenon directly expressed in it is at the same time the universal genetic basis from the development of which all other, just as particular, phenomena of the given concrete system may be understood in their necessity" (Ilyenkov, 1982, p. 76). It is thus only through this form of generalization that we can claim that toolmaking is more essential to human nature than are soft earlobes. "Toolmaking" is an induction that guarantees deduction of many specifically human traits, from the form of the human jaw to a complex division of labor (see Woolfson, 1982). "Toolmaking" helps us to understand all the important ways in which humans are different from other animals. Soft earlobes certainly do not.

But this points up a very special feature of the concrete universal which is the result of substantial generalization. This is that it need not be empirically, statistically universal. There are humans who do not make tools. What makes toolmaking universal for the human species is that even what is done by those who do not make tools is understandable in terms of our toolmaking origins. It is its function as a "genetic basis" that makes toolmaking universal. And it is precisely this that makes the abstraction "toolmaking" concrete: it aids in revealing the "necessary connection of specific phenomena within a whole" (Davydov, 1984, p. 13).

Davydov offers the following characteristics of the "theoretical knowledge" that results from substantial generalization to the concrete universal, as contrasted with merely empirical knowledge:

> 1. Theoretical knowledge arises on the basis of an analysis of the role and function of a certain relation of things inside a structure system.
> 2. By means of the analysis, this real, particular relation of things is searched for which at the same time is the genetic basis of all other manifestations of the system. This relation appears as a universal form or the essence of mentally reproduced totality.
> 3. Theoretical knowledge which is based on the transformation of objects reflects their internal relations and interconnections. In the reproduction of an object, in the form of theoretical knowledge, thinking exceeds the limits of perceptual presentations.
> 4. In theoretical knowledge, the connection between the real universal relation and its various manifestations, i.e., the connection of the general and the individual, is crystallized.
> 5. The concretization of theoretical knowledge presupposes its conversion into a developed theory by the deduction and explanation of the specific manifestations of the systems relying on its universal basis.
> 6. Theoretical knowledge appears primarily as methods of intellectual activity and subsequently in different symbolic sign systems in which for example artificial and natural language is used (a theoretical concept may appear as a method of deducing the specific from the universal, but still lack a corresponding terminological formulation). (Davydov, 1984, pp. 20–21)

Research on evolutionary origins is the prime example of substantial generalization. Making this identification allows us to see that evolutionary research is not just another form of research, it is the model for all good research. It is only by revealing the evolutionary or other genetic origins of an object that a determinate theory about it can be constructed. Only in this way can we decide whether earlobes or hand structure are essential to our theory of human nature.

IMPLICATIONS FOR COMPARATIVE PSYCHOLOGY

The general implications for psychology are obvious. The crisis of indeterminacy exists because of a one-sided reliance on empirical generalization to the abstract. The solution is therefore to develop a methodology based upon substantial generalization to the concrete. This methodology, at least in its broad strokes, already exists in evolutionary and other genuinely genetic research. The psychological subdiscipline most prepared to carry out such a project is comparative psychology. In order for comparative psychology to meet the objective which I am proposing, two broad reforms will be necessary: first, it will have to adopt a stronger commitment than it has shown in recent history to a truly evolutionary, genetic methodology; and, second, it will have to expand its

conception of its mission to include a central commitment to providing the foundation for a determinate (and in this sense unified) general psychological theory. This is, as I have pointed out elsewhere (Tolman 1987a, b), much more in line with the original intentions of our discipline's founders, namely Romanes, Morgan, and Hobhouse, than with the arid psychologies of Thorndike and Watson.

I see the prospects for all of this as quite auspicious. There are at least three grounds, besides the theoretical one discussed above, for my optimism. The first is the "state of the art" of comparative psychology. As I have already indicated, we have survived our troubled times and have even come out of them a good deal wiser. We have been forced to take a close look at our theories and methods, to become more explicit about them, and to pay closer attention to their evolutionary nature. The discussions on anagenesis and levels (e.g., Aronson, 1984; Yarczower, 1984) have been, in my view, an important part of our reexamination; and the post-neoDarwinian thinking on evolutionary process may prove to be even more significant (Ho, 1987), especially when combined with a recognition of the leading role of behavior in evolution (e.g., Napier, 1976, p. 4). It is conceptions like these that are enabling us better to formulate our evolutionary, genetic, and therefore "substantial," mission.

An emphasis on continuity in evolution served a very important historical purpose in the late 19th and early 20th centuries, but it goes awry when we allow it to back us into the reductionist corner from which it appears that animal species are not really different and that nothing psychological has actually evolved. The concepts of anagenesis and levels help us to restore discontinuity, now scientifically conceived, to its rightful place. It is, after all, discontinuity that evolutionary theory must explain, not continuity. Armed with such methodological notions, comparative psychology is better than ever prepared to face up to the challenge of explaining why humans are different from apes, why apes are different from rats, why vertebrates are different from invertebrates, and so on, without fear of falling into the error of the *Scala naturae*, rather than spending all its energy on abstract notions that supposedly unite all the species, yet in fact explain nothing.

A second ground for my optimism is that we are primates. According to Napier:

> Primates are unique among the living mammals in displaying the 'staircase' phenomenon. In other orders the evolutionary sequence shows the successive replacement of archaic forms by new and improved ones. Take for example the horse. There is only one genus of horse today; there is no *Eohippus*, *Merychippus*, or *Pliohippus* to provide the milestones on the road to *Equus*. As each 'new' horse evolved, the 'old' horses went out of business; not so with the primates. As each new grade of primates evolved, the old grades, which had established themselves in their own particular environmental niches, continued to flourish." (Napier, 1972, p. 6).

In other words, the anagenetic grades of most immediate concern for human evolution are represented by living species. Add to this the rapidly expanding information now available from paleoanthropology, especially about *Australopithicus*, and one could hardly imagine a more advantageous position from which to develop a comparative psychology of the human species. I hasten to add, however, that this in no way disparages the important work on preprimate grades of psychological evolution. Primates, as an order, are quite advanced and that, too, must be explained.

The third ground for my optimism is the fact that a comprehensive scheme of psychological evolution has already been developed, which, it appears to me, holds much promise as a guiding thread for an evolutionarily reconstructed comparative psychology. This is the scheme developed in the 1940s and 50s by A. N. Leontyev (1981) and his colleagues at Moscow University as a foundation for current "activity theory" and recently developed further by Holzkamp (1973) and Schurig (1975a, 1975b, and 1976). In anagenetic terms, the scheme outlines the major grades of what Leontyev chose to call "psychic" development. It is a scheme that has the enormous advantage of being thoroughly ecological in outlook (i.e. it does not deal with the evolution of merely psychic qualities independent of biological qualities or of the environment, but is guided by a clear idea of essential subject-object interrelations). It also contains hypotheses about the causes of intergrade transitions. In short, it is a scheme that is totally in keeping with the demands of substantial generalization. As I have outlined the details of the scheme elsewhere (Tolman, 1987c), I shall not say more about it here.

CONCLUSION

I have shown that the crisis in general psychology is such that it can only be resolved by resorting to an alternate form of generalization that leads to concrete theoretical knowledge rather than abstract generalities. This alternative form of generalization turns out to be one that is evolutionary (genetic or developmental) in nature. In a broad sense, general psychology, if it is to overcome its difficulties, must become a truly evolutionary (genetic or developmental) psychology. Although comparative psychology has had historical growing pains of its own, we now see it moving toward the development of precisely the kind of methodology that is lacking in general psychology. It is in this sense, then, that the *resolution* of the general psychological crisis is *comparative* psychological. Its evolutionary methodology is the only possible basis for a theoretically healthy general psychology. One can easily imagine that as general psychology becomes more and more informed by comparative psychological methodology, the distinction between the two psycholo-

gies will diminish: the discipline will rightly become more like its sister, biology, in which comparative biology is the only kind that exists.

REFERENCES

Ardila, R. (1986). Significado y necesidad de la psicologia comparada. *Revista Latinoamericana de la Psychologia, 18,* 157-169.

Arnold, W. J. (1976). Introduction. In W. J. Arnold (Ed.), *Nebraska Symposium on Motivation 1975.* Lincoln, NE: University of Nebraska.

Aronson, L. R. (1984). Levels of integration and organization: a reevaluation of the evolutionary scale. In G. Greenberg & E. Tobach (eds.), *Behavioral evolution and integrative levels,* (pp. 57-81). Hillsdale, NJ: Erlbaum.

Beach, F. A. (1950). The snark was a boojum. *American Psychologist, 5,* 115-124.

Catania, A. C. (1973). The psychologies of structure, function and development. *American Psychologist, 28,* 434-443.

Davydov, V. V. (1984). Substantial generalization and the dialectical-materialist theory of thinking. In M. Hedegaard, P. Hakkarainen, & Y. Engeström (Eds.), *Learning and teaching on a scientific basis.* Aarhus, DK: Aarhus Universitet.

Dixon, R. A. (1983). Theoretical proliferation in psychology: a plea for sustained disunity. *The Psychological Record, 33,* 337-340.

Elms, A. C. (1975). The crisis of confidence in social psychology. *American Psychologist, 30,* 967-976.

English, H. B., & English, A. C. (1958). *A comprehensive dictionary of psychological and psychoanalytic terms.* New York: Longmans, Green & Co.

Gergen, K. J. (1981). The meager voice of empiricist affirmation. *Personality and Social Psychology Bulletin, 7,* 333-337.

Giorgi, A. (1976). Phenomenology and the foundations of psychology. In W. J. Arnold (Ed.), *Nebraska Symposium on Motivation 1975,* (pp. 281-348). Lincoln, NE: University of Nebraska.

Gottlieb, G. (1984). Evolutionary Trends and evolutionary origins: relevance to theory in comparative psychology. *Psychological Review, 91,* 448-456.

Harré, R., & Secord, P. F. (1972). *The explanation of social behaviour.* Oxford: Basil Blackwell.

Hilgard, E. R. (1987). *Psychology in America: a historical survey.* New York: Harcourt Brace Janovich.

Hilgard, E. R., & Bower, G. H. (1966). *Theories of learning.* New York: Appleton-Century-Crofts.

Ho, M. W. (1987). Evolution by process, not by consequence: implications of the new molecular genetics on development and evolution. *International Journal of Comparative Psychology, 1,* 3-27.

Hodos, W., & Campbell, C. B. G. (1969). *Scala naturae:* why there is no theory in comparative psychology. *Psychological Review, 76,* 337-350.

Holzkamp, K. (1973). *Sinnliche Erkenntnis—Historischer Ursprung und gesellschaftliche Funktion der Wahrnehmung.* Frankfurt a. M.: Athenäum Verlag.

Holzkamp, K. (1978). Die Überwindung der wissenschaftlichen Beliebigkeit psychologischer Theorien durch die Kritische Psychologie. In Holzkamp, K., *Gesellschaftlichkeit des Individuum.* Köln: Pahl-Rugenstein.

Ilyenkov, E. V. (1982). *The dialectics of the abstract and the concrete in Marx's Capital.* Moscow: Progress.

Koch, S. (1981). The nature and limits of psychological knowledge. *American Psychologist, 36,* 257-269.

Leontyev, A. N. (1981). *The problems of the development of the mind.* Moscow: Progress.

Lockard, R. B. (1971). Reflections on the fall of comparative psychology: is there a message for us all? *American Psychologist, 26,* 168-179.

Lorenz, K. Z. (1950). The comparative method in studying innate behaviour patterns. In *Physiological mechanisms in animal behaviour,* (pp. 221-268). Cambridge: University Press.

Moscovici, S. (1972). Society and theory in social psychology. In J. Israel & H. Tajfel (Eds.), *The context of social psychology,* (pp. 17-68). New York: Academic Press.
Napier, J. R. (1972). *Primates and their adaptations.* Oxford: Oxford University Press.
Napier, J. R. (1976). *Primate Locomotion.* Oxford: Oxford University Press.
Passmore, J. (1967). Logical positivism. In P. Edwards (Ed.), *The encyclopedia of philosophy.* New York: Macmillan.
Ribes, E., Ibañez, C., & Hernandez Pozo, R. (1986). Hacia una psicologia comparativa: algunas consideraciones conceptuales y metodologicas. *Revista Latinoamericana de Psicologia, 18,* 263-276.
Royce, J. R. (1982). Philosophic issues, Division 24 and the future. *American Psychologist, 37,* 258-266.
Schurig, V. (1975a). *Naturgeschichte des Psychischen 1: Psychogenese und elementare Formen der Tierkommunikation.* Frankfurt a. M.: Campus Verlag.
Schurig, V. (1975b). *Naturgeschichte des Psychischen 2: Lernen und Abstraktionsleistungen bei Tieren.* Frankfurt a. M.: Campus Verlag.
Schurig, V. (1976). *Die Entstehung des Bewusstseins.* Frankfurt a. M: Campus Verlag.
Staats, A. W. (1983). *Psychology's crisis of disunity: philosophy and method for unified science.* New York: Praeger.
Tobach, E., Adler, E., & Adler, L. L. (1973). Comparative psychology at issue. *Annals of the New York Academy of Sciences, 223.*
Tolman, C. W. (1984). Krize srovnávaci psychologie, pojem úrovni a nutnost dialektiky. *Ceskoslovenská* Psychologies, 28, 1-6.
Tolman, C. W. (1987a). Comparative psychology: is there any other kind? *Journal of Comparative Psychology, 101,* 287-291.
Tolman, C. W. (1987b). Zur Vorgeschichte der historischen Heransgehensweise in der bürgerlichen Psychologie. In W. Maiers & M. Markard (Eds.), *Kritische Psychologie als Subjecktwissenschaft,* (pp. 228-240). Frankfurt: Campus Verlag.
Tolman, C. W. (1987c). The comparative psychology of A. N. Leontyev (U.S.S.R.). In E. Tobach (Ed.), *Historical perspectives and the international status of comparative psychology,* (pp. 203-209). Hillsdale, NJ: Erlbaum.
Wilson, E. O. (1975). *Sociobiology.* Cambridge MA: Harvard.
Woolfson, C. (1982). *The labour theory of culture.* London: Routledge & Kegan Paul.
Wyers, E. J., Adler, H. E., Carpen, K., Chizsar, D., Demerest, J., Flanagan, O. J., von Glasersfeld, E., Glickman, S. E., Mason, W. A., Menzel, E. W., & Tobach, E. (1980). The sociobiological challenge to psychology: on the proposal to "cannibalize" comparative psychology. *American Psychologist, 35,* 955-979.
Yarczower, M. (1984). Behavior and evolutionary progress: anagenesis, grades, and evolutionary scales. In G. Greenberg & E. Tobach (Eds.), *Behavioral evolution and integrative levels,* (pp. 105-119). Hillsdale, NJ: Erlbaum.

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