

On Ersatz Teleologists and the Temptations of Rationalism: Some Reactions to Some of the Reactions

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Among the authors of the commentaries on the symposium, Professor Leahey did not refer at all to the Lamiell and Durbeck papers, while the remarks by Professors Martindale, Muscari, Tyler, and Westcott were all relatively positive. Professors Chaplin and Westerman, on the other hand, have been rather more critical. Accordingly, and in consideration of the space constraints imposed on the symposiasts' rejoinders, I will restrict myself in this article to the papers by Chaplin and by Westerman.

Are the Symposiasts Questioning "Straw Men"?

In the first section of his paper, Chaplin argues that many cognitive psychologists have long been committed to the view that persons are not merely passive beings but are constructive as well, and thus that there is already widespread acceptance of the central point being defended by the symposiasts. But it is not at all clear to me (or, apparently, to any of the other six commentators) that this is the case.

Of course, there are a great many cognitive psychologists who *profess* allegiance with the view that human beings are purposive. The point is that professing such an allegiance is one thing, while constructing a theoretical edifice that is actually—and not just apparently—capable of accommodating that allegiance is quite another. In other words, when an author uses words such as "purposive," "intentional," and the like, it is necessary to know just what that author wishes to signify by those terms before the conclusion is entitled that a genuinely teleological conception of behavior is on offer.

My point here can be illustrated with an anecdote bearing on Chaplin's contention that George Kelly and Walter Mischel are two of the many theorists among whom "it has long been recognized that individuals actively construct

their reality. Several years ago I and several colleagues contributed an article to a special issue of *Journal of Personality* organized around the theme "Personality and prediction: Nomothetic and idiographic approaches" (Lamiell, Foss, Larsen, and Hempel, 1983). In that article, empirical findings very similar to those reported by myself and Durbeck in the present issue were discussed, and a theoretical interpretation of those findings was offered along the same lines that Durbeck and I have suggested in the more recent work. As it happens, Mischel was asked to (and did) write an article critically discussing the various contributions to that special issue. In commenting on the Lamiell et al. work, he noted that he shared with those authors the view that person-centered rather than norm-centered inquiry is an important goal to retain and cultivate. He then argued as follows:

The argument for casting that goal within a philosophical teleology, however, is not convincing. George Kelly (1955) led the way to seeing our "subjects" as scientists—as active constructors and transformers of their own worlds—yet managed a philosophical constructive alternativism without the additional assumption of teleology. That assumption of teleology loses more for us as scientists than it gains for us as humanists. (Mischel, 1983, p. 591)

In Mischel's own eyes, therefore, neither he nor Kelly is the teleologist Chaplin makes them both out to be. At the very least, contradictions of this sort should give pause to one who would advise the symposiasts that they "have more friends out [t]here than they seem to think."

Regarding Chaplin's observations that "there is also substantial evidence that individuals construe events differently," I would simply say that the point is as irrelevant as it is true. There is nothing in the notion that cognitive constructions can differ from one person to the next that commits one to any particular theoretical conception of the nature of the construction process itself, and the latter is what is at issue if what we are discussing are theories of cognition. Moreover, the empirical findings generated by studies of individual differences in cognitions cannot properly be said to address this issue, because while the constructive process is an individual matter (which is *not* to say that it transpires wholly apart from social context), the findings generated by individual differences research bear no legitimate interpretations whatsoever at the level of the individual (Lamiell, 1987).

My misgivings about Chaplin's observations concerning existing accommodations for dialectical reasoning within extant theories of cognition are similar to those just expressed in reaction to the alleged widespread commitment to a teleological conception of behavior. For example, while I would certainly acknowledge that the "simulation heuristic" postulated by Kahneman and Tversky (1982) might be conceptualized *by us* as a reflection of the play of dialectic in human mentation, the question of whether or not Kahneman and Tversky think likewise is quite another matter.

Some Clarification on What Lamiell and Durbeck Have and Have Not Contended

Since I am not familiar with the details of the unpublished empirical research cited by Chaplin, I am not in a position to comment on that work. I would, however, draw attention to a point pertinent to Chaplin's concluding comments. Specifically, my objective is not and has never been to establish the "correctness" of a dialectical model of the judgment process underlying impression formation to the complete exclusion of a normative (or, for that matter, ipsative) model. In the first place, it is not logically possible to achieve that objective. In the second place, I do not deny (and have never denied) that human beings can and do reason normatively and ipsatively as well as dialectically (see, e.g., Lamiell, Foss, Trierweiler, and Leffel, 1983, p. 232). If they couldn't, there would be no such things as normative and ipsative models for psychological measurement! I do maintain, however, that normative and ipsative judgments *can* be made only insofar as dialectical judgments have previously been made, and that this is true no less in the context of "objective" personality measurement than in the context of "subjective" personality ratings (see Lamiell, 1987, chapters 5 and 6). Stated otherwise, my thesis is not that impression formation judgments are never normative (or ipsative), but that dialectical judgments have *epistemological priority*.

This is perhaps as good a place as any to comment on Martindale's observation that "Lamiell and Durbeck also ask if we judge absolutely or relatively." While I think I can appreciate what Martindale has meant, I would phrase things a bit differently. Specifically, I do not deny that a judgment of some empirical given A must be relative. The question is: Relative to what? The most prevalent answer to this question (as reflected in the logic of normative and ipsative measurement) has been: relative to some other empirical given B. The alternative answer I have defended is: relative to not-A. This answer does not, in my view, entail the thesis that judgments are absolute. It simply asserts that judgments are relative in a way other than that which has traditionally been assumed.

Some Thoughts on the Temptations of Rationalism

Turning now to the paper by Westerman, I must emphasize first of all my conviction that constructivist thinking of the sort reflected in Westerman's paper (see also Harré, 1984) has much of consequence to contribute to contemporary theoretical discourse on human cognition (and on human psychological functioning more broadly defined). On the other hand, I think that Westerman has been a bit glib in his dismissal of an intellectual tradition spawned by the philosophies of Descartes, Spinoza, and Leibniz, and arguably traceable, in at least some respects, back through Aristotle and Plato to the ancient Pythagoreans (Robinson, 1981). Accordingly, I think that his analysis leaves certain important questions begging.

Consider, for example, the implications of Westerman's assertion that "*from the outset everything in the world takes its place in the meaningful context of shared practices*" (emphasis in original), and his subsequent claim that "the world of shared practices is always there before the individual agent enters the picture." It is, of course, on the basis of these views that Westerman embraces what he calls "involved subjectivity," and rejects what he calls "uninvolved subjectivity."

Now as one who is perhaps guilty of succumbing to at least some of the temptations of rationalism, I am given to wonder just *who*, on Westerman's account, is to be regarded as *sharing* the practices to which he refers. If what Westerman means to say is that when the infant enters the world there are already people here interacting with one another on the basis of cultural meanings *they* share, then I know of no quarter—rationalist or otherwise—from which he will get a serious argument. The question is whether or not from the outset the *infant* is to be regarded as one of the *sharers*.

If the answer to this question is "yes," then we are forced, it would seem, to conclude that, somehow, the infant enters the world already equipped with the meanings in question, for how could the infant be said to *share* something it did not have in the first place? This would in turn force the conclusion that an individual never really needs to be *taught* the meanings embedded in the practices of his/her culture, for why should it ever be necessary to *teach* a person what s/he has always known?

Since these are clearly conclusions that Westerman would not wish to endorse, it must be presumed that his answer to the question posed above would be "no." But to thus acknowledge that, at the outset, the infant is *not* properly regarded as one of the sharers of cultural meanings is, it seems to me, to inject at least a measure of "uninvolved subjectivity" into one's theorizing, simply because to embrace this view is to assert that, at the beginning, and with respect to cultural meanings, "*uninvolved*" is *precisely what the subject is*. Were it otherwise, the process of socialization, so central to constructivist thinking, would be entirely gratuitous. Perhaps, therefore, to retain something of rationalist thinking in our theoretical outlook is not such a bad thing after all.

Note that nothing in what has just been said challenges the notion that from the outset the infant is involved in *something*. At issue is whether or not that something can be said to include, from the start, the meanings embedded in the cultural practices into the midst of which the infant is thrust. Though I have argued that the answer to this question must be "no," this does not prevent me from finding merit in Westerman's claim that "all thematized understanding is grounded in prior familiarity with activities," and that "practical know-how is viewed as the primary term. 'Knowing that,' or thematized understanding, is based on it." We do well to not forget that it was that rationalist Kant who began his famous *Critique of Pure Reason* with the observation: "That all our knowledge begins with experience there can be no doubt." Certainly, this statement will

bear an interpretation quite compatible with the constructivist view, and at least one currently prominent spokesperson for that view (Harré, in press) has suggested that the whole of Kantian epistemology can be reconciled with constructivist principles. Whether or not this is the case, its very suggestion would warn us of the hazards of bifurcating rationalism and constructivism too soon and too cavalierly.

Comment on Specific Criticisms

By way of specific criticisms of the study reported by Lamiell and Durbeck, Westerman develops two major points. The first is that "the study provided [the subjects with] no context for making judgments about the targets," or, alternatively, "that by default the study constitutes an artificial context in which subjects made judgments simply because they were asked to do so as part of participating in a research project." Westerman goes on to cite recent research findings indicating that "the purposes for forming an impression (e.g., in order to make a decision about whether to play with another child at a later time) *make a big difference in the impressions formed*" (parenthesis in original, emphasis added).

Once again, since the research cited by Westerman is as yet unpublished, I am not in a position to comment on the details of the work. Whatever they may be, however, I doubt not at all the validity of Westerman's assertion that the purpose one has for forming an impression of another can exert an enormous influence *on the impression formed*. But the Lamiell and Durbeck study was not about the impressions formed. It was about *the reasoning process through which* impressions are formed. There is nothing in the study of changes in or differences between impressions as a function of anything that can inform that concern.

The second major criticism leveled by Westerman against the Lamiell and Durbeck study is that by virtue of the manner in which subjective weights or relevance values were determined, those values were assumed to be invariant within a given subject across the targets rated. In suggesting a possible corrective for this procedural constraint, Westerman states that

If Lamiell and Durbeck had approached their data (in accordance with the idea that the significance of a given item depends on the overall story told by the full set of activity scores for a given subject) they would have gone some distance toward incorporating recognition of the fundamental role played by context. But they did not do this. Instead, they treated each activity as if it were a context-free element. (emphasis in original)

In connection with this point, I would note first of all that the decision to ignore the question of context in the sense identified by Westerman was made in the interest of *getting at* the question of context in quite a different sense. Indeed, the central question of the research reported by Lamiell and Durbeck can be viewed as a question of context: What is the larger psychological context within which one "fixes" the location of another with respect to some underlying

attribute of behavior? Is that psychological context properly regarded as a normative one, borne of the memory traces of prior experience? Or, alternatively, is that context properly regarded as one that the subject generates dialectically through a process of negating the given? In short, there are many different facets to the question of context as it arises in impression formation research, and though the work reported by Lamiell and Durbeck does not address them all, neither is it oblivious to them all. Nor is it the exclusive province of Westerman to decide which of these facets should be given investigative priority.

All of this said, and in conclusion, it must be emphasized that Westerman has quite properly identified an important limitation of the Lamiell and Durbeck study; one which should be remedied by future research.

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Are We All Clear On What A Mediational Model Of Behavior Is?

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I would like to thank the panel of commentators. They have greatly enriched the presentation of our topic, and to do complete justice to their viewpoints would demand another symposium. In the interests of space I will confine my reactions to the questions of just what we mean when we refer to a mediational theory of behavior, particularly since this point is central to the teleologist's case—at least, to "this" teleologist's case. I will begin with some of the philosophical issues raised, and then focus more specifically on the current practices of cognitive science.

First of all, it has always seemed crucial to me to distinguish between a theory or preferred understanding of some topic of interest on the one hand, and the method or manner of putting that theory to test on the other (Rychlak, 1980, 1981). We rarely solve theoretical differences through empirical data, because so often data can make sense in more than one way (indeed, there are in principle N explanations for any one empirical fact pattern). Data can, of course, embarrass a theory and no self-respecting scientist would want to defend a theory that is completely at odds with the empirical facts. We found Chaplin, Muscari, and Westcott referring to empirical findings in support of their contentions. These researches were framed by each commentator's theoretical predications of what the findings meant. Theory plays a role "in addition to" the role played by evidence (a method is the means or manner of obtaining evidence). And when I therefore refer to a "mediational model" I am not referring to some collection of facts that pressed themselves on me. I am referring to my preferred way of understanding, construing, or accounting for *any* facts!

In my paper, I claimed that the British empiricists or associationists had as their preferred understanding of mentation that the person takes influences in from the environment and then associates them together according to the frequency and contiguity of this or that occurrence, or remembers (knows, etc.) them based upon repetitions of one sort or another. Such frequency and contiguity issues are said to direct the course of thought itself. Leahey questions

my understanding of John Locke, so I will say a bit more about this representative of the British school even though I mentioned several others in this line of theoretical descent.

The selection of Locke's *Essay Concerning Human Understanding* that Leahey focuses on (i.e., Book II, Chapter XXXIII) is, as he says, aimed at criticizing certain types of associations—namely, those associations that are brought about through unexamined or unreasonable ties of pure chance or of custom. Note in the quote Leahey gives us that Locke speaks of *two* forms of “connexion of ideas.” The “bad” kind of connexion that Locke criticizes is the sort in which, for example, darkness is tied to goblins and spirits by ignorant people, such as the housemaid who teaches such nonsense to the children of the house (Locke, 1952, p. 249). Another bad kind of connexion arises when person A has had a painful experience with person B, and, thinking of this troubled interaction “over and over” (*ibid.*, p. 250) forms such a strong connexion via these frequent repetitions that each time person B comes to mind so does pain and displeasure. A “good” connexion of ideas would be something like the idea of lightning when we hear thunder. Experience establishes such associations reliably over time and we are in line with reason when we therefore continue to expect the one to go with the other.

Because Locke is critical of such (what he considers) unreasonable and unnecessary connexions in mind, Leahey jumps to the conclusion that his concept of the “association of ideas is far from being the central machinery of the mind.” I disagree with this conclusion, because it seems to me that the entire thrust of Lockean mental philosophy is to suggest that all we human beings have in mind begins as simple ideas that are formed into increasingly complex ideas by way of (either sound or unsound) connexions formed through frequency considerations of one sort or another, such as the number of times the housemaid tells the child about goblins in dark places, the child's resultant ruminations about these connexions, and so on. Without frequency of this or that experience to direct thought we would need a homunculus to explain why it is that the Lockean mind ruminates this way as opposed to that way. The need for association begins in this requirement of the simple constituting into the complex by way of a frequency-contiguity principle.

Although he may not have referred specifically to the “association of ideas” in the first edition of his *Essay*, Locke did express this same notion in other ways. For example, in Book II, Chapter XXIII, Locke tells us that “a certain number of these simple ideas go constantly together” and that a complex idea can be “a complication of many ideas together” (*ibid.*, p. 204). It is in elaborating on such complications (connexions, associations) that Locke advances what I think is a good definition of a mediation model: “When the understanding is once stored with these simple ideas, it has the power to repeat, compare, and unite them, even to an almost infinite variety, and so can make at pleasure new complex ideas” (*ibid.*, p. 128).

This is my understanding of what a mediational model "is." Something comes indirectly to play a role in a process that it was not initially a part of, acting as a medium, conveyance, or conduit following its introduction to that process. Locke then adds: "But it is not in the power of the most exalted wit, or enlarged understanding, by any quickness or variety of thought, to *invent* or *frame* one new simple idea in the mind, not taken in by the ways before mentioned; nor can any force of the understanding *destroy* those that are there" (ibid.). This formulation is unacceptable to a dialectical theorist. Locke is construing ideas as unipolar singularities, akin to singular objects that one might place in a cabinet (a metaphor he actually used to describe the mind and its ideas; see Cranston, 1957, p. 266). But to a dialectician, simple ideas are never this singularly simple; they are, in effect, *dimensional*—much in the way that Lamiell's research suggests. Tyler finds it difficult to accept the bipolarity of dialectic, suggesting instead that alternatives can spring forth in any of a number of directions. The dialectician would expect that her reference to a "creative advance to novelty" would be framed by some sense of going from "this" event to "that" possibility. Here is where we can turn to research for a clue, and Westcott's interesting study in which subjects found over 200 different ways to describe a feeling in opposition to the feeling of being free supports the view that dialectical machinations promote novelty of conceptualization.

So, the dialectical theorist would answer Locke by saying that many of these simple ideas would delimit and hence imply their opposite in some way—by negation, contradiction, contrariety, and so on. Some of these opposite ideas would be construed as playful and even "wild" speculations. This would mean that people could indeed create or negate simple ideas, begin with empty cabinets but be capable of framing dimensions of opposition/negation for each singularity placed within them. Leahey believes that Locke was dialectical in theoretical formulation because he spoke of a person doing or not doing something, or agreeing or disagreeing with some point through an exercise of reason. Leahey is impressed by the fact that Locke referred to a person's "will." But the fact is, the will was conceived as under the direction of desire, of so-called uneasinesses that arise in the course of living (Locke, 1952, p. 186). When desire is active, the mind has the power to suspend the execution of what the will is being impelled to do (ibid., p. 190). Here is where reason can direct the person. Locke goes on to observe: "This [suspension of will] seems to me the source of all liberty; in this seems to consist that which is (as I think improperly) called *free-will*" (ibid., p. 190; italics in original). The problem with this formulation, as axiologists have since pointed out (e.g., Rickaby, 1906, p. vii), is that Locke fails to explain why such suspension of desire takes place in some instances but not in others? He begs the question needing explanation. Once again, the homunculus seems indicated to make such choices.

If all the "person" amounts to is a receptive cabinet, a mediating conduit that takes in from the environment and then uses *only* what is put there, then the

grounds for hanging-fire on a decision to act or not to act must itself be dependent upon such external inputs (placings into the empty cabinet, or etchings upon the tabula-rasa intellect). It was easy for subsequent theorists like Hume (whom some experts believe took his conception of mental ideas from Locke; e.g., see Cranston, 1957, p. 274) to add that what determined the suspending or not suspending was the ongoing rewards and punishments of the external environment. As Leahey notes, Hume (1952) did indeed underwrite his mediational conception of mind with Newtonian mechanics, and as a result suggested the "law" that behavior was moved solely by rewards and punishments (p. 485). What this comes down to is that efficiently-caused singularities, viewed as moving "over there" (i.e., extraspectively), connect and summate into complexities based exclusively upon whether or not they result in satisfactions. The way is now set for traditional reinforcement explanations of learning.

I do not find dialectical reasoning under formal presentation in Lockean philosophy. Locke's view of reasoning is entirely demonstrative. Just because we can oppose two concepts of a theory oppositionally does not mean that the theorist who framed these concepts is relying upon a dialectical formulation. Sometimes the theoretician qua human being may be reasoning dialectically, and aligning his or her theoretical presentation through a series of oppositionally-framed conceptions. But the *formal* theory under espousal cannot therefore be said to be a dialectical presentation unless it is shown precisely how this is the case.

I find Locke doing what psychologists do, and that is to "account for" what might be construed as dialectical reasoning in a demonstrative fashion. For example, in his discussion of what he calls "contrary" observations, circumstances, reports, tempers, designs, and so on, Locke notes that it is sometimes difficult to decide whether "this" side or "that" side of an issue is the correct one. Falling back on what I would call a frequency thesis of past inputs and ruminations based on such inputs summing to a rough equality of influence, Locke then opines: "That as the arguments and proofs *pro* and *con*, upon due examination, nicely weighing every particular circumstance, shall to any one appear, upon the whole matter, in a greater or less degree to preponderate on either side; so they are fitted to produce in the mind such different entertainments, as we call *belief, conjecture, guess, doubt, wavering, distrust, disbelief, &c.*" (Locke, 1952, p. 369; italics in original). Dialectical machinations of contrariety, questioning, and negation might be placed here. But the formal treatment is *not* dialectical in any sense. It is like the normative strategy that Lamiell discredited.

When we come to Kant the theory of mentation is completely different. I would call Kant's model of the mind a *predicational* model (Rychlak, 1987a). Predication is involved with immediacy in the aligning of meanings, so that only that is known which has been framed from the very outset by some broader expanse of meaning to conceptualize it. The Greeks referred to this as reasoning from *universals* (broader meanings) to *particulars* (targeted items that take meaning from the universals). We can affirm the relationship of the broader

meaning to the targeted meaning, deny it, or qualify it in some way, as in the case of "Patricia is intelligent" or "Patricia is not intelligent" or "Patricia sometimes seems quite bright, but at other times seems dull." "Intelligence" is the broader meaning here, so that if we employed Euler circles we could place the smaller circle labeled "Patricia" totally within the wider circle labeled "intelligence" (*is*), or keep the two circles separated (*is not*), or partially overlap them (*is sometimes*) to symbolize the various possibilities of predicating the young woman's mental capacities. We are obviously presenting here a different process of mentation than the mediation model postulates. We are more in the realm of formal-final causation than material-efficient causation.

Kant views the mind as a conceptualization process in which broad-ranging categories of the understanding (akin to our "intelligence" circle) frame experience from the very outset of cognitive processing. The mind is not first pump-primed with items "taken in" from the environment that later function *intermediately* to influence the course of thought. The mind works *immediately* to frame certain items within its aegis, lending them meaning from the moment at which cognition may be said to be underway. Kant's interpretation of an idea is completely different from Locke's interpretation. For Kant (1952), an idea is always conceptual, it frames things that are known, and can even transcend known experience (p. 115). Ideas are never "put into" mind, but function as the organizers of experience, lending a predicated meaning to experience that it would not have had otherwise. Ideas grow into complexity, but this is always by way of moving from what is already broadly conceived (understood, grasped, known, etc.) to what can then be brought within this conceptual realm. Past experience influences what we know hence conceptualize today. But that past experience as today's experience was *also* subject to predication. Westerman seems to be describing a process of this sort, but I am not sure because he is critical of the implicit rationalism of Kantian philosophy.

I was very pleased to see Martindale say that all Ph.D. candidates in psychology should be made to understand Kant's *The Critique of Pure Reason*. I agree, but I also wonder how well cognitive enthusiasts really understand what Kant is saying? Kant's name is increasingly bandied about in the research literature as a supposed precursor of modern cognitive theory. But every time I look carefully, it is clear that a mediation model is being employed to "account for" the predicational model that Kant actually employed. Martindale draws some questionable parallels between the Kantian view of mentation and the computer's processing of information. He believes that because the computer processes information according to a binary logic it is reflecting dialectical reasoning. Actually, the Boolean logic underwriting the digital computer is *non*-dialectical because it interprets disjunction as "either . . . or (but not both)" [Reese, 1980, p. 64].

This means that if we reason as a computing machine does, we presume that something is either "true" or "the case" or it is "false" or "not the case." Actually, as Westcott points out, in the strictly engineering sense of a computer, no

meaning need be attached to the concept of information ("on/off" takes the place of "true/false"). But, when theorists in artificial intelligence employ the binary logic of a computer they are relying upon Boolean algebra. And since there is no "both" in this logic it violates the "one and many" principle of dialectical reasoning (where, e.g., left is left and right is right, but there is *also* a "both" in the sense of a left-right commonality of meaning). Binary logic follows the "law of contradiction" (i.e., A is not non-A), which underwrites demonstrative reasoning.

A human being who actually reasoned the way a computer "reasons" would see no difference between "on/off" and "tall/fast," or between "true/false" and "deep/cold." Computers never "realize" that they cannot get from "tall" to "fast" through a common ground uniting these meanings, but that they *can* get from "true" to "false" thanks to the uniting bond of oppositional meaning ("both"). Writing a program to simulate such dialectical reasonings would not solve the problem because the way in which the computer actually would carry out the program would still be exclusively demonstrative, lacking a "sense" of the bonding oppositionality that all humans grasp implicitly.

Martindale tells us that "everything Kant said about human understanding applies just as well to computer 'understanding.'" I disagree with this reading of the *Critique*. As we have just seen, computers have no understanding of alternative meanings by way of dialectical (non-Boolean) logic. Neither can the computer transcend its executive program, thanks to a transcendental reasoning capacity, and frame thereby (often erroneous) alternatives to this script in the way that Kant said a person could (*ibid.*, pp. 59, 109, 203, 229). This is why computers do not make mistakes; they cannot question and distort things through dialectical machinations. This is also why computers cannot be said to be teleological mechanisms. They cannot in principle fashion the grounds for the sake of which they are demonstratively determined. They are Lockean creatures, bound by their programs which they cannot transcend and reflexively alter in the way Westcott has indicated that he can alter his behavioral direction through self-examination.

Martindale cannot really be committed to Kantian formulations if he believes, as he tells us, that dialectic is not a *natural* (much less useful or important) mode of reasoning. Here are some quotes from Kant that state precisely the opposite: ". . . human reason, which naturally pursues a dialectical course" (*ibid.*, p. 248) and "Pure reason always has its dialectic, whether it is considered in its speculative or its practical employment" (*ibid.*, p. 337). It should also be recalled that the Kantian categories of the understanding are framed dialectically (e.g., unity-plurality, possibility-impossibility, necessity-contingency, etc.). And please, Dr. Martindale, let us try to remember in the future that Hegel is *not* the father of dialectical reasoning—which can be seen active in primitive languages and writings stretching back to 2000 BC. No one has to be a Hegelian in order to be a dialectician. Kierkegaard, for one, was a dialectician who ridiculed Hegelian pretensions.

I would like briefly to address a point raised by Muscari. He seems to think that dialectical thought has to be conscious, and that somehow if we pursue a dialectical analysis of mentation we will be missing out on the unconscious side of things. Actually, as I have tried to show in many of my writings, dialectical reasoning is very friendly to a theory of the unconscious mind. Jung was openly supportive of dialectical formulations, and although Freud disparaged "dialectic" per se his theories are fraught with such explanations. I wonder if Muscari is familiar with Freud's *very first* theoretical effort, *A Case of Successful Treatment by Hypnotism* (1966; written in 1892/93). In this formulation he makes use of clearly dialectical concepts such as the antithetical idea and counter-will. And as for the unconscious, it was always construed by Freud as a predicational model, a conceptualizing process that was underway at birth and did not have to be "given" unpredicated inputs to get it something to think about. The unconscious is underway (*immediately*) at birth, framing contents through a Kantian form of idea rather than taking them in (*mediately*) on the basis of a Lockean idea. Concepts like defense, repression, denial, reaction-formation, and so on, are completely in line with a dialectical intelligence. It is *not* "illogical" for a dialectical mode of thought to know (*consciously*) and not know (*unconsciously*) something at the same time. Freudian thought does not rely upon the Boolean interpretation of disjunction, as do the cognitive models of today.

I would like next to take up some of the interesting beliefs and attitudes held to by certain of the commentators regarding science, and in particular the role that psychology is to play in science. Chaplin, Leahey, and Martindale find the efforts of this symposium's contributors to be irrelevant, or beside the point. Chaplin is not bothered by the fact that cognitive psychology is just as much an efficient-cause account of behavior as traditional behaviorism. He feels that the broader reaches of theory and research in cognitive psychology are accounting for the telic aspects of behavior in any case. Martindale attempts to redefine final causality into efficient causality, essentially dismissing the distinction drawn and adhered to in Western philosophy virtually from its inception. Leahey asks us to pursue the neo-Newtonian aspirations of trying to explain human behavior mechanistically, or else consider ourselves to be non-scientists.

As to Martindale's renaming of the final cause: just because the premised intention (encompassing a predication) comes before the instrumental act carrying out this intention does *not* make this an efficient-cause sequence. All four of the causes (material, efficient, formal, final) can be shown to have causes *and* effects, in that order. In the case of final causation, the intended premise—the "that for the sake of which"—appears first in *logical* order, which may on occasion parallel the flow of efficiently caused antecedents-to-consequents across time. However, in a statement such as we used above—"Patricia is intelligent"—the predication, the intended meaning ("cause") being extended to the target lies to the right or comes after the item on which it will have an "effect." In this case the intended meaning being extended is *not* preceding the target, even though logically it "comes first" as a precedent. The point is, in the realm of

final-cause description, time's passage is irrelevant; a final "cause-effect" sequence can parallel time's supposed passage from earlier to later *or not*. I have in my logical learning theory broken from traditional, efficient-cause terminology such as stimulus-response or input-output by speaking of the "telosponse," which involves a precedent-sequacious flow of meaning without regard for time's passage (Rychlak, 1987b). I did this precisely for the reason reflected in Martindale's efforts to make one and only one "cause-effect" sequencing relevant to behavioral description. It is impossible to express final causation through efficient causation. You can repress or dismiss the former by limiting discussion to the latter, but this restricts the account, and, in the case of human beings, distorts it unnaturally.

I understand Chaplin's desire to demonstrate that we are well on the way to explaining behavior teleologically in cognitive science. I too believe that the cognitive movement in psychology augurs well for teleology, though much work (as is presently underway in our exchanges) must be done to clarify things. When Chaplin (as Westcott) cites George Kelly (1955) as an example of this move to teleology he is on the right track. Kelly, who was my teacher and influenced my thinking, definitely *did* try to present a rigorous humanism, relying on a predicational model. His ideas are frequently twisted into mediational models today. I cannot comment on all of the papers Chaplin included as examples of worthwhile tendencies toward teleology, but when he tosses in Mischel and Bandura I must take exception. I have had exchanges in print with both of these theorists concerning just how well they are "accounting for" telic capacities such as reflexive thought and personal choice (Rychlak, 1976, 1979). Both Mischel and Bandura use language having the "sound" of teleology, but when the chips are down it is clear that they are mediation-model thinkers. Bandura (1979) once criticized my efforts to explain behavior teleologically, as follows: "There is a difference between analyzing cognition as a contributing factor in the reciprocal determination of events and conceptualizing cognition as a psychic agent that orchestrates behavior. Understanding of how people exert some influence over their actions is more likely to be advanced by delineating and exploring the nature of self-regulatory *mechanisms* [italics added] than by simply ascribing behavior to a psychic agent" (p. 440).

A mechanism is, by definition, an efficiently caused sequence of events. So far as I am concerned, Bandura's "reciprocal determinism" is a direct descendant of Lockean "complex" ideas—where three items (behavior, person, environment) somehow "add up to" something more than they are individually in an efficient-cause fashion. The same Lockean formulation is to be found in Minsky's (1986) *Society of Mind*, which Martindale believes is doing a suitable job of accounting for purpose and intention. We have to be careful about the use of words. Though Minsky builds his entire case on so-called agents, the resulting agency has nothing to do with teleology. He clearly states that "Minds are simply what brains do" (*ibid.*, p. 287), that brains are "machines with enormous numbers of

parts that work in perfect accord with physical laws" (p. 288), and that agency in the sense of free-will is a myth (p. 307). So, let us not be misled by the use of telic-sounding words. When mechanists "account for" teleology they merely give us mechanism in new clothing.

The same is true of Tolman (1967), whom Leahey tells us "always respected purpose as a vital, fundamental aspect of behavior." Yes, but what did he *mean* by purpose? He meant an observable improvability (docility) in behavior, as when an animal does some task better over trials (*ibid.*, p. 14). He did *not* mean what a teleologist means by purpose. He was thinking in terms of efficient causes, functioning as "intervening variables" (mediation model) coming between the environment/biological substrate and the animal's observable behavior. Indeed, he specifically ridicules McDougall—a true teleologist—for believing in an introspectively framed "something" called purpose that is not to be seen overtly in behavior (*ibid.*, p. 16). The upshot is that if a person intends to solve a problem but shows *no* improvement over trials we cannot say that he or she has a purpose in mind. Since we cannot "see" the purpose it does not exist unless it reflects docility. If this satisfies Leahey's sensibilities as a psychological account of purpose, well and good. But it surely does not satisfy mine.

I found Westerman's reactions to be rather presumptive, in that he seems to have a fixed idea of what "a" teleological approach represents, and he worked mightily to disengage himself from it. It seems to me that he was assigning all kinds of views to the members of this panel that we do not really hold. I do not like to use "subjectivity" in referring to the person's influence on his or her experience, or to people as "subjects" excepting only in a research report. We could all—as individual persons—be agents of our experience quite "objectively," agreeing on the nature of reality. I do not think that a teleological position requires that we defend only or even primarily a subjective view of cognition. Westerman seems to believe this. He tells us that "the subject always comes to know an object against the background of the subject's prior involvement in the social world." This is where he wants to place the dialectical interaction, rather than within cognition itself as I am claiming. Supposedly, I am therefore defending the position of an "uninvolved subject," who sits off to the side and construes the world as a passing scene "over there" in some fashion.

I hope it is clear now that as a predicational theorist I certainly do not think of the person as uninvolved with ongoing experience, social or otherwise. How can you be uninvolved if the very meaning of your experience depends upon framing it predicationally to begin with? I would paraphrase Westerman's statement above and say "the subject [to be read in my terminology as "the person"] comes to know the social world against the subject's prior involvement in the non-social world, trailing back to predications framed in the crib." Westerman criticizes the members of the symposium for not going far enough, for not giving an alternative way in which the initial cognition—the framing of A regardless of non-A—takes place. This is simply not true in my case, as my

concept of telosponsivity (accounting for predication) should now make clear. We did not go into every aspect of cognition in this symposium because we were hoping to raise the awareness level of our colleagues concerning dialectical reasoning—something that now, for all practical purposes, does not exist in the psychological literature! But I for one certainly *do* have an alternative understanding of what cognition is all about at the level of the A “input” (in my terms, the framing of a premise encompassing predication in the telosponsive act).

I have studied Westerman’s comments concerning agency, and if he has accounted for this in some way the explanation eludes me. He completely overlooked the fact that when I defined agency I said the agent could also “comply” with what the environment or biological prompting indicated. He seems to think that teleologists claim a person’s goals come from the subject’s (i.e., person’s) side of things, and that “agents are free to make choices from a vantage outside the flow of events.” Now, there may be some teleologists who believe this, but I surely do not. What the person “knows” as the “flow of events” is what the person predicates as the “flow of events,” and this is based on the same dialogue and interaction with the environment that Westerman alludes to in describing so-called involved subjectivity. In fact, everything he says about context fits my understanding of predication.

A context is a wider expanse of meaning than the particular item or circumstance being framed by that context. I agree that the context (predicate) is the “that [meaning] for the sake of which” ongoing experience is made meaningful. Westerman accuses me of defending “radical freedom,” but at no point have I ever claimed that the person can literally do everything that he or she can freely concoct mentally. Seeing the elevator entrance from the tenth floor next to the window I can see myself leaving by *either* “way down.” I can imagine myself, as in a dream, leaping out the window and floating with waving hands to a gentle landing on the street below. But, because I have this clear picture in mind and the obvious freedom to leap out the window, does that mean I am really free to do the gentle landing? The facts of reality tell me “no.” But sometimes it is also the case that by reasoning in opposition to common sense, to the “facts” of reality, a person like Einstein can indeed create empirically established outcomes as spectacular as arm-waving oneself down ten stories to the pavement below.

There does seem to be an issue separating Westerman’s view of the context from my own. He stresses the fact that the background context is always a “shared” practice, by which I take it he means some kind of mutual, interpersonal definition of reality. I guess his concept of involvement hinges in some large degree on social involvement. On the other hand, I say that preliminary to social involvement is the process of cognizing, and this process is predicational “by nature.” Hence, when the infant relates to others for the very first time this predicational process—this context-endowing process—is already underway. And, as an aspect of this predicational process we have dialectical reasoning. Here is where I see Westerman losing his predicational model to what may be

another one of those “back door” mediational models so common in psychology, for he tells us: “. . . the notion of involved subjectivity leads to the view that the context defines for us what is salient.” I, on the other hand, would contend that salience issues from the person (Westerman’s “subject”), who can affectively assess (itself, a form of predication) and align experience predicationally one way or another *from birth*. And it is the dialectical reasoning capacity which makes such alternatives possible. (I might mention in passing that none of the commentators presented any empirical evidence to contradict the likelihood of dialectical reasoning in human behavior. The distaste and/or reservations expressed for this concept are apparently totally theoretical. I find this almost amusing).

The final point I would address is one that we see again and again in psychology. It mystifies me why this view is so prevalent, considering the immense changes that have come about in the philosophy of the physical sciences over this century. Leahey enunciates this view when he says that “Since the time of Newton, science has sought to explain what appears to be purposive in mechanistic terms.” I can think of leading scientists like Mach, Einstein, Bohr, Heisenberg, and others who would not agree that human purpose is a mechanism. Leahey goes on to suggest the following: “If psychology is to be a natural science like the other natural sciences, then it must *explain* purpose and meaning, as the other sciences have, with principles that themselves contain neither teleology nor semantics.” I read this and wonder why we psychologists persist in such outdated Newtonianism? To give but one recent example of an eminent scientist who would disagree with Leahey, I cite the following view of Ilya Prigogine, a Nobel laureate: “. . . the reality studied by physics is also a mental construct; it is not merely given. . . . One of the reasons for the opposition between the ‘two cultures’ [i.e., Art vs. Science] may have been the belief that literature corresponds to a conceptualization of reality to ‘fiction,’ while science seems to express ‘reality.’ Quantum mechanics teaches us that the situation is not so simple. On all levels reality implies an essential element of conceptualization” (Prigogine and Stengers, 1984, p. 225).

Prigogine is not relying on a mediational model in his comments here. Conceptualization in his view—as was true in the views of the scientists listed above—amounts to an act of predication, a framing of “that, for the sake of which” (final cause) something can be known. For Leahey to ask me as a psychologist to “accept purpose and meaning as human givens, as ultimate bases for understanding [and] not themselves to be explained by anything more basic” is in effect to ask me to forego my professional commitments, my very “calling” as a scientist interested in human nature. But now, why does Leahey tie my hands? The mechanists are free to prove their theory empirically, but I should not busy myself doing so regarding my agential theory. I should not propose concepts to explain human agency and seek their validation through typical experimental methods. Why in the world do so many psychologists who

are—or, are potentially—friendly to teleology continually reject the scientist's role? We are past Newtonianism! Let's get on with proving what we readily sense ourselves to be—telic organisms, agents of our behavior. Maybe then psychology will attain the relevance in the family of sciences that it now lacks.

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