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On the Alleged Degeneration of the Kohlbergian Research Program*

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In order for a branch of science to gauge its progress in the continuing explication of knowledge, it periodically becomes necessary to reconsider the scientific merit of its theories and the evidential warrant for holding them. Criteria for judging the adequacy of theories must be worked out on the level of metatheory according to a given reconstruction of how science actually (and rationally) proceeds in its business of solving theoretical problems. In a recent contribution to this journal, Phillips and Nicolayev attempted to provide such an analysis by evaluating the status of Kohlberg's theory of moral development in terms of methodological criteria proposed by Lakatos.[1] Although Kohlberg and his colleagues are convicted of many sins, the central complaint is that Kohlberg's is a degenerating research program, because it has failed to deflect evidential challenges to the hard core of the theory and because, importantly, it has failed to anticipate novel facts.

We believe this conclusion to be mistaken. We intend to show in this article that Phillips and Nicolayev violated both the substance and the spirit of the Lakatosian approach and that a more appropriate application of its features to Kohlberg's theory disarms much of their criticism. We will argue that their critique is compromised by significant category errors, by a failure to grasp the conceptual relation between the "hard core" of the theory and its "protective belt," by a misidentification of the features of the core and belt, and by a failure to specify the "positive heuristic" of the research program. We will proceed in the following manner. Since it is our contention that much of the problem lies in a misunderstanding of Lakatos's reconstruction of science, we will first review some of its relevant features. We will next critically examine the claims made against the moral development research program by Phillips and Nicolayev, and we will review the theoretical and empirical features of the program which vitiate their claims. We will conclude that when the proper distinctions are made, Kohlberg's moral development theory does indeed represent a progressive problemshift in our understanding of morality and that the cognitive-developmental research program, with its positive heuristic — stage — is far from degenerating, insofar as it has been successful in anticipating novel facts.

What Is the Lakatosian Reconstruction of Science?

The history of science presents a number of intriguing challenges to one interested in establishing the rational foundations of inquiry. How does one develop, for example, a model of science that permits the comparability of theories in spite of observations being theory laden, that is empiricist without being inductive, and that accounts for the

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1. D. C. Phillips and J. Nicolayev, "Kohlbergian Moral Development: A Progressing or Degenerating Research Program?" *Educational Theory* 28 (1978): 286-301; Imre Lakatos, "Falsification and the Methodology of Scientific Research Programs," in *The Methodology of Scientific Research Programs,* ed. J. Worrall and G. Currie (Cambridge: Cambridge University Press, 1978).

autonomy and tenacity of theoretical science? Further, what if it can be shown, which it can, that all scientific theories are equally unprovable, improbable, and undisprovable? This would surely seem to be an invitation to skepticism and irrationality. Lakatos's reconstruction of science, however, which he calls "sophisticated methodological falsificationism," has taken up the challenge and has provided us with a powerful model for dealing with issues such as these. Since Lakatos's is a historical and comparative approach, as we shall see, an understanding of his system must also take on a historical and comparative character.[2] Thus, we shall digress and relate here a brief history of previous metatheoretical constructions of science as developed by Lakatos. Only then can we comprehend his great advance and the appeal his model has for the evaluation of contemporary educational theories.

SOME RECONSTRUCTIONS OF SCIENCE

Lakatos picks up the history with the collapse of the traditional view of science, justificationism. Justificationism, which posits a strict demarcation between fact and theory, held that scientific knowledge must mean proven knowledge. Hard facts, fortified by a powerful inductive logic, were thought capable of establishing the truth-value of theories. Justificationism breaks down, however, on the question of inductive logic. As Lakatos points out, inductive logic cannot infallibly increase content.[3] This has been known at least since Hume's indictment of inductive logic, and it has been fortified by Popper's demonstration that inductivism leads either to infinite regress or to *apriorism.*[4]

Neojustificationists, according to Lakatos, proceeded to reconstruct the methodology of science to avoid the conclusion that, because theoretical science is unprovable, we necessarily submit to radical doubt. They argued that induction need not prove knowledge, only that we determine its *probability.* According to Lakatos, "Scientific honesty requires less than had been thought: it consists in offering only highly probable [as opposed to provable] theories; or even in merely specifying, for each scientific theory, the evidence and the probability of the theory in light of the evidence."[5] Popper, however, has demonstrated that probabilism fails to escape the twin problems of infinite regress and *apriorism* and that, in addition, under general conditions, *all theories have zero probability.*[6] Thus Lakatos concludes that not only are all theories equally unprovable, they are equally improbable as well.

Subsequent reconstruction of scientific methodology, dogmatic and (naive and sophisticated) methodological falsificationism, held that if science cannot prove theories, it can certainly disprove them via the *modus tollens.* According to the dogmatic falsificationist reconstruction, once a theory is disproved, it should be eliminated from the corpus of scientific theories. What is assumed is that there exists an infallible empirical basis (whereas scientific theories are said to be fallible). That is, a psychological demarcation is said to exist between theories and facts, so that one could unequivocally appeal to "facts" in the evaluation of "theories." The dogmatic falsificationist position

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2. There is some question as to whether Lakatos saw his own reconstruction of science as a (metatheoretical) research program in its own right, one that can be justified in comparison with rivals according to "problemshift" criteria developed by his own model. To our knowledge, Lakatos did not explicitly address this question. However, we believe that there are sufficient grounds for arguing that Lakatos at least implicitly had this in mind when he attempted to account for rational progress in the history of metatheoretical reconstructions of science. Apart from the fact that he readily calls the Popperian and Kuhnian approaches "research programs" ("Falsification," 90), he does state that "the concepts of 'progressive' and 'degenerating' problemshifts, the role of proliferation of theories can be generalized to any sort of rational discussion and thus serve as tools for a general theory of criticism" ("Falsification," 92, fn. 3).

3. Lakatos, "Falsification," 11.

4. D. Hume, *An Inquiry Concerning Human Understanding* (1748; Indianapolis: Bobbs-Merrill, 1955); K. Popper, *The Logic of Scientific Discovery* (New York: Harper Torchbooks, 1959).

5. Lakatos, "Falsification," 11.

6. Popper, *The Logic of Scientific Discovery;* A. Ritchie, "Induction and Probability," *Mind* 35 (1926): 301-17.

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is untenable, however, for the following reasons. First, what are considered facts are only acceptable to us if we believe in certain theories that describe how our measuring instruments work. Second, there is no strict demarcation between fact and theory. According to Lakatos, "There can be no sensations unimpregnated by expectations and *therefore there is no natural demarcation between observational and theoretical propositions.*"[7] Even should such a demarcation exist, the problem would still remain as to how factual propositions could be proved by an experiment. As noted previously in the justificationist case, logic dictates that propositions can be derived only from propositions, they cannot be derived from facts. Thus, if all facts are theory bound, then the clash between "theories" and "facts" cannot produce refutations but only inconsistencies.

Yet a more significant problem exists for the dogmatic falsificationist in Lakatos's view — no theory can ever forbid any possible experimental outcome. That is, disconfirming results can always be attributed to other extraneous factors thought to be influential or to factors not previously taken into account (via the *ceteris paribus* clause). According to Lakatos,

> . . . some scientific theories forbid an event occurring in some specific finite spatio-temporal region only on the condition that no other factor . . . has any influences on it. But then such theories never alone contradict a "basic" statement: they contradict at most a conjunction of a basic statement describing a spatio-temporally singular event and of a universal nonexistence statement saying that no other relevant cause is at work . . . (thus) the dogmatic falsificationist cannot possibly claim that such universal nonexistence statements belong to the empirical basis.[8]

In other words, every scientific theory contains a *ceteris paribus* clause such that it is a theory plus the *ceteris paribus* clause that is subjected to a test. Since it is always possible to replace the *ceteris paribus* clause, the test of a specific theory is inconsequential. This seems to lead to the conclusion that scientific theories are not only equally *unprovable* and *improbable,* they are all equally *undisprovable* as well![9]

Popper's (naive) methodological falsificationism attempts to rescue empirical science from skepticism by asserting that science is not only a corpus of assertions, but it is also a system of conventions. The process of testing our theoretical conjectures is impossible without making a series of methodological decisions. Because there are no pure observations, what we regard as facts must be conventionally agreed upon in light of a " 'relevant technique' such that anyone who has learned it will be able to *decide* that the statement is 'acceptable.' "[10] Since propositions regarding observations, or the empirical basis, are only a special case of theoretical propositions, which simply means that all propositions of science are theoretical, methodological conventions are required to demarcate the "theory" under test from unproblematic background knowledge representing the "facts," the empirical basis. Thus the potential falsifiers of a theory are granted "observational" status by decision. The truth-values of such observations are arrived at by a relevant experimental technique. The methodological falsificationist appreciates the fact that experimental techniques and scientific theories (not necessarily mutually exclusive) are fallible, but, by decision, she assumes that such theories constitute unproblematic background knowledge to be subsumed by the *ceteris paribus* clause. While this solves the problem of how to demarcate fact from theory, we are still left with the problem of how, given the *ceteris paribus* clause, we are able to subject a specific theory to refutation. Popper maintains that we do this by making yet another methodological decision. The researcher decides, before an ex-

7. Lakatos, "Falsification," 15.
8. Ibid., 10.
9. Ibid., 16.
10. Ibid., 22.

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periment is conducted, what state of affairs she will accept as a falsification of her theory, irrespective of *ceteris paribus.*[11]

Yet this advance is still not sufficient, for it does not yet account for the tenacity of theories and the criteria by which we compare rival theories and account for progress in scientific knowledge. It is the Lakatosian reconstruction, sophisticated methodological falsificationism, which attempts to address these issues. In short, one must distinguish between *criticism* of a theory and its *abandonment.*

One may criticize a theory (or research program) by pointing out the existence of empirical anomalies, phenomena that the program claims to account for but does not. Mere criticism, however, mere refuting evidence, *is never sufficient in itself to falsify a theory.* This is so (among other reasons) because researchers protect the "hard core" of their theory from refutation with a "protective belt" of auxiliary theories. This is the "negative heuristic" of a research program. It consists of a methodological decision to cordon off the core of a theory from the threat of refutation. That is, we forbid the *modus tollens* to be directed at the hard core and insist instead that the auxiliary theories bear the brunt of the tests. In addition to the negative heuristic, which tells us what path *not* to pursue, research programs also have a positive heuristic, which tells us the direction that research *should* take. It consists of models, or suggestions, as to how to modify the "refutable" protective belt. For the cognitive developmental approach, the positive heuristic involves, in part, the "suggestion" to employ ever more powerful stage models until empirical realities are accounted for. The positive heuristic proceeds in the face of counterevidence and "refutation," but there is no need to consider the presence of empirical anomalies as being decisive. These "refutations" are not ignored, but they are considered inconclusive until some future time, when the positive heuristic must confront the disconfirming evidence and turn it into supporting evidence. This reconstruction thus accounts for the relative autonomy and tenacity of theoretical science.

One should be prepared to abandon a research program, on the other hand, only if certain criteria are met. There must exist a rival research program that is powerful enough to account for all the facts of the former program. In addition, and importantly, the rival research program must anticipate new "facts," some of which have been corroborated. But even these criteria, while necessary grounds for inducing the abandonment of a research program, are not in themselves sufficient as long as the former program is "progressive," that is, as long as its positive heuristic is still capable of generating novel facts. Even if a research program is "degenerating," one is still entitled to embrace it so long as no rival program exists satisfying the above criteria.

Hence it is no longer feasible or appropriate to talk of a *theory* being falsified. There can be no "refutation" until the emergence of a more powerful theory. Thus a *proliferation of theories* and their critical comparison is fundamental to sophisticated falsificationism. We are concerned not with the appraisal of theory, but with a series of theories. Progress in our explication of a problem "is measured by the degree to which a problemshift is progressive, by the degree to which the series of theories leads us to the discovery of novel facts. We regard a theory as 'falsified' when it is superseded by a theory with higher corroborated content."[12]

RESEARCH PROGRAMS AND KOHLBERGIAN MORAL DEVELOPMENT

With this description of the Lakatosian approach in mind, we are now in a position critically to reexamine Phillips and Nicolayev's evaluation of Kohlbergian moral development. In the authors' opening remarks, they approvingly cite Feyerabend to the effect that evaluative standards for assessing scientific change apply to research programs and not to individual theories. Thus, according to Feyerabend, "The methodology of research programs develops standards for the evaluation of (scientific or, more generally,

11. Popper, *Logic.*
12. Lakatos, "Falsification," 34.

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conceptual) *change.* The standards apply to research programs, not to individual theories; they judge the evolution of a program over a period of time, not its shape at a particular time; and they judge this evolution in comparison with the evolution of rivals, not by itself."[13] Further, the authors point out that "a clear case can be made that the theory of moral development advanced by Lawrence Kohlberg and his associates forms a research program in the Lakatosian sense."[14] Phillips and Nicolayev note that, while Hartshorne and May and Piaget all have written on the psychology of moral behavior and thought, the moral development domain itself is traditionally associated with Kohlberg's 1958 dissertation. "Since that time," they write, "there has been a flurry of experimental work, theoretical revision, and reconstruction of experimental techniques. The question is, have these activities been content increasing?"[15] The authors will conclude, of course, that such activities have not, that the Kohlbergian research program is indeed in the process of degeneration.

But there is much in this brief account to which one may object. A clear case cannot, in fact, be made that Kohlberg's moral development theory constitutes a Lakatosian research program. One cannot, under the aegis of Lakatosian evaluative criteria, assess "problemshifts" by looking at individual theories. According to Lakatos, "Sophisticated falsificationism thus shifts the problem of how to appraise *theories* to the problem of how to appraise *series of theories.* Not an isolated *theory,* but only a series of theories can be said to be scientific or unscientific; to apply the term 'scientific' to one single theory is a category mistake."[16] Hence Kohlberg's specific individual theory of moral development, which he posed in 1958, is being conflated with a research program (a configuration of theories) in an impermissible manner. This would seem to have been clear to the authors, inasmuch as they apparently endorse the Feyerabend position that research programs involve multiple theories and not individual theories as such and that it is research programs which constitute the proper subject matter for the application of evaluative criteria.

But what is the research program associated with the Kohlberg theory? This program is clearly the "cognitive-developmental approach," which *has* generated bold (at least partly) corroborated hypotheses, of which the Kohlberg theory is but a single instance. Phillips and Nicolayev beg the question when they dismiss the Hartshorne and May and Piagetian work on moral conduct and development and begin their analysis rather arbitrarily with Kohlberg's 1958 dissertation. The proper question (for the moment) is not whether the revisions of the Kohlbergian theory since 1958 are content increasing. Rather, the proper question is whether Kohlberg's theory is content increasing vis-à-vis other psychological theories of morality.

There have been a number of attempts recently to assess the extent to which Kohlberg's theory of moral development represents an advance over Piaget's theory. Lickona, for example, maintains that "a comparison of Piaget's two-stage theory with Kohlberg's highly refined six-stage moral sequence makes it clear that an early, ground breaking cognitive-developmental theory has been subsumed and supplanted by a later more comprehensive theory in the same tradition."[17] He points out that Piaget's stage model fails to account for moral developmental phenomena at either end of the age continuum. For example, Kohlberg postulates three stages *beyond* the highest stage proposed by Piaget. In addition, Piaget's model cannot account for the egocentric but authority-independent moral reasoning characteristic of Kohlberg's Stage 2. Rather, subjects are portrayed in Piaget's theory as moving directly from an obedience

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13. P. Feyerabend, "Zahar on Einstein," *British Journal for Philosophy of Science* 25 (1974): 25.

14. Phillips and Nicolayev, "Kohlbergian Moral Development," 287.

15. Ibid., 287.

16. Lakatos, "Falsification," 34.

17. T. Lickona, "Research on Piaget's Theory of Moral Development," in *Moral Development and Behavior,* ed. T. Lickona (New York: Holt, Rinehart and Winston, 1976), 240.

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orientation to a (Kohlbergian) Stage 3 orientation, which stresses a concern for social approval.[18]

Weinreich also maintains that the more precise specification of stages in Kohlberg's model is evidence of greater theoretical power.[19] Kohlberg's theory is more powerful in the sense that it permits new questions to be addressed concerning fundamental psychological processes in moral development, such as the nature and form of stage transition. This, of course, satisfies the Lakatosian criteria that a theory must anticipate novel facts, (only) some of which are corroborated, before it can be said to represent a progressive problemshift.

The greater stage specificity in Kohlberg's theory also permits more points of contact with (Piagetian) logical development. Because Piaget's final stage of moral development, the stage of equity, is characteristic of early or preadolescence, for example, it cannot be directly related to the cognitive abilities permitted by advanced stages of formal operations. Thus, according to Weinreich, "It is difficult to draw any conclusions about the state of equity because [Piaget's] subjects would not have been old enough to use the full formal operations, even in intellectual functioning. Certainly the evidence from other research, especially Kohlberg's, indicates that when a distinction is made between various stages of equitable thinking, the 'formal' properties may be more fully explored."[20] Again, novel facts are anticipated by Kohlberg's theory, since it alone can postulate and test the relation between formal logical thinking and principled moral reasoning. In addition, Kohlberg's theory postulates two sublevels of moral reasoning within concrete and formal operations and is able to associate moral stages with the logical prerequisites represented by the preoperational-concrete-formal operations sequence, whereas Piaget's theory of moral development cannot.[21]

Finally, Siegal, too, has noted that the stage gradation specified by Kohlberg's theory, particularly Stages 4 and 5, offers a "modest improvement on Piaget's [theory] for it characterizes types of moral reasoning in adolescence and adulthood."[22] Siegal maintains, however, that evidence regarding, for example, the "structured whole" aspect of the Kohlbergian stage (said by Kohlberg[23] to represent an advance over the Piagetian "stage" of moral development), as well as evidence regarding invariant sequence and regression, is equivocal at best. For this reason, then, Siegal is not prepared to say that Kohlberg's theory has "eclipsed" Piaget's theory of moral development.[24] We hasten to add, however, that a theory that is a candidate for supersession of a rival theory need not be corroborated in all its aspects, only that it anticipate novel facts, *some* of which are corroborated. It would be unreasonable indeed to expect a theory, particularly one which postulates bold, content-increasing hypotheses concerning the course of moral development, to be free of its share of anomalies. All theories, indeed, all research programs, develóp, as Lakatos points out, in a "permanent ocean of anomalies. What really counts are dramatic, unexpected, stunning, predictions; a few of them are enough to tilt the balance."[25] It is our contention that apart from the assumptions attending Kohlberg's particular stage model, the existence of at least four

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18. Ibid., 240.

19. H. Weinreich, "Kohlberg and Piaget: Aspects of Their Relationship," *Journal of Moral Education* 4 (1975): 201-13.

20. Ibid., 210. See also L. Walker, "Cognitive and Perspective-taking Prerequisites for Moral Development," *Child Development* 51 (1980): 131-39.

21. Weinreich, "Kohlberg and Piaget," 210.

22. M. Siegal, "Kohlberg versus Piaget: To What Extent Has One Theory Eclipsed the Other?" *Journal of Moral Education* 4 (1975): 291.

23. L. Kohlberg and R. Kramer, "Continuities and Discontinuities in Childhood and Adult Moral Development," *Human Development* 12 (1969): 93-120.

24. Siegal, "Kohlberg versus Piaget," 291.

25. I. Lakatos, "Science and Pseudoscience," in *The Methodology of Scientific Research Programs.*

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stages is sufficiently corroborated[26] to allow the conclusion that Kohlberg's theory is a significant advance over, and extension of, Piaget's theory and that a notable problemshift has indeed occurred. We shall have occasion below to discuss the stage question in Kohlberg's theory and the role it plays in the assessment of progress in a research program.

Hence, we conclude that Phillips and Nicolayev should have granted the fact that Kohlberg's theory does indeed represent progress, that it is content increasing when the proper comparisons are made. This comparison is to a series of precedent theories, of which Piaget's (and Hartshorne and May's) is an obvious example. So our objection here can be put simply. One cannot apply the Lakatosian model to an isolated theory but only to a series of theories, and when this is done, Kohlberg's theory of moral development, and the cognitive-developmental approach from which it devolves, constitutes a progressive problemshift in our understanding of morality.

THE KOHLBERGIAN HARD CORE

Phillips and Nicolayev next review what they consider to be aspects of the Kohlbergian hard core. Under this category, the authors include (1) stages of development, (2) invariance, and (3) logical necessity. Since there are significant muddles in their treatment of these issues, we will need to consider each of their claims point by point.

Concerning the role of stages, the authors point out that "it has always been part of their irrefutable hard core that an individual's moral reasoning progressively passes through a series of stages."[27] They further indicate that because Kohlberg and his collaborators insist on the empirical demonstrability of their theory, this leads one to believe "that if refuting evidence were found, the Kohlbergian would be prepared to abandon the hard core. Clearly, however, in the twenty-years that the program [sic] has existed, the assumption that moral development occurs in fixed stages has not been given up."[28]

It is one thing indeed to ascribe a stage concept to the hard core of the cognitive-development approach; it is quite another to say what stage *model* best conceptualizes developmental change. As such, stage models are part of the positive heuristic, not the hard core. The positive heuristic, as it has been pointed out, proceeds in the face of disconfirming evidence. It is thus entirely rational for Kohlbergians to cling to their stage model, since disconfirming evidence never refutes a theory anyway. One could not and should not expect them to give up a stage model until the *ceteris paribus* clause is well tested. To do otherwise would be to ignore the Lakatosian criteria regarding refutablity, that there can be no refutation until the emergence of a better theory (or model). If a given stage model (e.g., "fixed" stage), is unsuccessful in accounting for empirical evidence, other stage models can, and have been, substituted in its stead. Indeed, as Lakatos points out,

> A *"model"* is a set of initial conditions (possibly together with some of the observational theories) which one knows is *bound* to be replaced during the further development of the program, and one even knows, more or less, how. This shows once more how irrelevant "refutations" of any specific variant are in a research program: their existence is fully expected, the positive heuristic is there as a strategy both for predicting (producing) and digesting them.[29]

Rest, for example, has severely faulted the traditional Kohlbergian stage model in

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26. See, for example, B. Parikh, "Development of Moral Judgment and Its Relation to Family Environmental Factors in Indian and American Families," *Child Development* 51 (1980): 1030-39; and A. Colby, L. Kohlberg, J. Gibbs, and M. Lieberman, "A Longitudinal Study of Moral Judgment," *Monographs of the Society for Research in Child Development* 48 (1983): 1-93.

27. Phillips and Nicolayev, "Kohlbergian Moral Development," 288.

28. Ibid.

29. Lakatos, "Falsification," 51, his emphasis.

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favor of one which he proposes that reflects empirical realities (for example, stage mixture, regression, continuity).[30] Siegal, too, urges a revision of the Kohlbergian stage model to account for response heterogeneity and (apparent) developmental regression.[31] Actual revisions of the standard stage approach, in addition to Rest's, are indeed available. Bickhard, for example, proposes a stage model from which the vertical decalage phenomenon can be derived as a consequence.[32] Levine introduces a "non-displacement" perspective which attempts to explain variation in moral reasoning due to stage acquisition and stage use by conceiving moral development to be an "additive-inclusion" sequence.[33] Additive-cumulative sequences and qualitative models are discussed by van den Daele, and at least three models of the developmental course are suggested by Flavell.[34] Finally, useful conceptual distinctions for the defense of stage models have been recently proposed by Glaserfield and Kelley.[35]

But just as the characterization of stage progressions is being constantly reevaluated, the Kohlbergian theory itself is undergoing revision. The most notable change has been the omission of Stage 6 from the most current scoring method.[36] But there have been more substantive revisions. Gibbs, for example, believes that the complexities of stage development in moral reasoning, particularly in light of Kohlberg's attempt to frame an integrative *life-span* approach, can better be accounted for by a two-phase model.[37] The first phase represents the "standard" development sequence, which begins with the centrations of early childhood and culminates in the second-order systems perspective of formal operations. The second, "existential" phase would account for the metaethical theory-defining discourse of late adolescent and adult moral development, with its characteristic emphasis on relativistic subjectivism and an existential concern for meaning. Murphy and Gilligan propose a reconceptualization that distinguishes between the absolutism of adolescent logic and adult principled reasoning.[38]

The point is that reconceptualizations of Kohlbergian theory and the development of alternative stage models have taken place well within the cognitive-developmental approach. This research program is actively solving problems and digesting anomalies. Phillips and Nicolayev are thus mistaken when they claim that an enormous price must be paid for abandoning the (standard) stage hypothesis. This conclusion is only possible when the structuralist requirement for stagelike development — the hard core of the cognitive-developmental approach — is conflated with a given model of how "stages" should be conceived — the refutable instantiation of the positive heuristic.

One further comment is in order. It reflects a significant misunderstanding of the Lakatosian model to surmise that "if refuting evidence were found, the Kohlbergians would be prepared to abandon the hard core."[39] This is so for three reasons. First, one cannot admit a falsifying instance in the absence of a well-tested *ceteris paribus* clause. There is no instant rationality in the appraisal of a theory. Second, and more importantly, no research program ever abandons the hard core upon observing

30. J. Rest, "The Stage Concept in Moral Judgment Research" (Unpublished manuscript).

31. Siegal, "Kohlberg versus Piaget," 291.

32. M. Bickhard, "The Nature of Developmental Stages," *Human Development* 21 (1978): 217-33.

33. C. Levine, "Stage Acquisition and Stage Use: An Appraisal of State Displacement Explanations of Variation in Moral Reasoning," *Human Development* 22 (1979): 145-64.

34. L. van den Daele, "Qualitative Models in Developmental Analysis," *Developmental Psychology* 1 (1969): 303-10; J. Flavell, "Stage Related Properties of Cognitive Development," *Cognitive Psychology* 2 (1971): 421-53.

35. E. Glaserfield and M. Kelley, "On the Concepts of Period, Phase, Stage, and Level," *Human Development* 25 (1982): 152-60.

36. Colby et al., "Longitudinal Study of Moral Judgment."

37. J. Gibbs, "Kohlberg's Moral Stage Theory: A Piagetian Revision," *Human Development* 22 (1979): 89-112.

38. J. Murphy and C. Gilligan, "Moral Development in Late Adolescence and Adulthood: A Critique and Reconstruction of Kohlberg's Theory," *Human Development* 23 (1980): 77-104.

39. Phillips and Nicolayev, "Kohlbergian Moral Development," 288.

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disconfirming evidence. What *could* be abandoned is an auxiliary theory or model of the positive heuristic, but again, only after *ceteris paribus* is well tested. It is precisely because an aspect of a theory *is* the hard core which makes it irrefutable. Why should Kohlbergians be expected to give up an "irrefutable" hard core given certain empirical demonstrations? To insist that they should, as Phillips and Nicolayev do, is to disbelieve that the hard core *is* the hard core, i.e., is irrefutable. But this would not be an accurate application of the Lakatosian model. Finally, under the Lakatos model, it would be quite irrational anyway for a Kohlbergian to abandon the theory in the absence of a superseding one, since there can be no talk of refutation before the emergence of a better theory.

Let us now consider the problem of invariance and logical necessity, both claimed by Phillips and Nicolayev to be part of the Kohlbergian hard core. Concerning the former, the authors make the point that "it would take only one case of variation — one case in which B did not follow A — to refute such a claim."[40] This assertion is not countenanced by the Lakatosian approach, since there is no refutation until *ceteris paribus* options are exhausted or prior to the emergence of a better theory. Further, a concept of invariance is not independent of a particular stage model. Hence, invariance is allied with the refutable model generated by the positive heuristic (e.g., stage) and is therefore not part of the hard core in the first place. If empirical realities vitiate a radical claim for invariance, then a permissible option, as noted before, is simply to replace the stage model with one that accounts better for the data. But it is far from certain that empirical realities do indeed require Kohlbergians to abandon their stage model,[41] since measurement error represents a plausible appeal to *ceteris paribus*.[42]

Concerning logical necessity, the authors maintain that to posit such a claim for the moral development sequence "brilliantly" protects the theory from counterevidence, since "matters of logical necessity are immune from empirical refutation."[43] It is not *logically* impossible, they point out, for the sequence to be arranged differently, for Stage 1 to emerge after Stage 3, for example. Hence, "It is not a logical contradiction, however empirically implausible it may be, to claim that first an individual believes good behavior pleases or helps others, and later the individual moves to the view that the physical consequences of an action determine its goodness or badness."[44]

We will not conduct an exegetical analysis as to whether Kohlberg does or does not believe the moral development sequence (or any other cognitive-developmental stage sequence) to be logically necessary in the sense indicated by Phillips and Nicolayev, though the point is discussed by others.[45] Rather, it is our intention to unravel the semantic muddles attending the use of the words "logic" and "logical necessity" and to offer a methodological clarification. There are two senses of the term "logic" which can be distinguished. The first, which we will call logic_1, is classical logic, the study of reasoning and the structure of propositions apart from their content. It is not logically impossible, in the classic sense of the term, for bolts to unscrew themselves, for the sun not to rise tomorrow, for a billiard ball to resist the propelling impact of another ball, and for "pigs to fly." It is this sense of logic_1 that Phillips and Nicolayev have in mind, but we doubt this is the sense intended by Kohlberg or other researchers.

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40. Ibid., 296.

41. M. Davison, S. Robbins, and D. Swanson, "Stage Structure in Objective Moral Judgments," *Developmental Psychology* 14 (1978): 137-46; see also J. Broughton, "The Cognitive-Developmental Approach to Morality: A Reply to Kurtines and Grief," *Journal of Moral Education* 7 (1978): 81-96.

42. B. Puka, "A Kohlbergian Reply," in *The Domain of Moral Education,* ed. D. Cochrane, C. Hamm, and A. Kazepides (New York: Paulist Press, 1979); Colby et al., "Longitudinal Study of Moral Judgment"; R. Enright and D. Lapsley, "Social Role-Taking: A Review of the Constructs, Measures and Measurement Properties," *Review of Educational Research* 50 (1980): 647-74.

43. Phillips and Nicolayev, "Kohlbergian Moral Development," 290.

44. Ibid.

45. J. Flavell, "An Analysis of Cognitive Developmental Sequences," *Genetic Psychology Monographs* 86 (1972): 279-350; see also Puka, "A Kohlbergian Reply," 289; and R. S. Peters, "Moral Developments: A Plea for Pluralism," in *Cognitive Development and Epistemology,* ed. T. Mischel (New York: Academic Press, 1971).

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Indeed, it is plainly disingenuous of Phillips and Nicolayev to insist, on the one hand, on Kohlberg's theory satisfying formal empirical criteria and, on the other, on an interpretation of "logic" that is patently metaphysical. While there are an infinite number of logically possible worlds, only one is the domain of science — the world of experience. Any empirical-theoretical system must satisfy three requirements, according to Popper: "First, it must be synthetic, so that it may represent a noncontradictory, a *possible* world. Secondly, it must satisfy the criterion of demarcation, i.e., it must not be metaphysical, but must represent a world of possible *experience*. Thirdly, it must be a system distinguished in some way from other such systems as the one which represents *our* world of experience."[46]

Now we doubt that one can find in any of Kohlberg's writing any indication that he is interested in any but the logically possible, synthetic world of human experience. Hence, when he introduces a notion of "logical necessity," it must surely mean theoretical necessity, which we will call $logic_2$. When one examines the formal properties of structural development in relation to a theory of moral judgment, one finds that only one arrangement of stages is theoretically plausible. While other arrangements may be logically possible in a metaphysical sense ($logic_1$), they would violate theoretico-empirical expectations ($logic_2$) regarding the emergence of structures and are hence theoretically inconsistent in the synthetic world of experience.

Admittedly, the situation is not clarified when defenders of Kohlberg maintain that "it is not matters of logical truth that Kohlbergians wish to demonstrate empirically. Rather they wish to show that a particular set of stages which logically presuppose each other actually occurs universally in human cognitive development."[47] We interpret this passage as indicating a rejection of $logic_1$ and an endorsement of $logic_2$. But it is far better to say that the sequence of stages constitutes a research hypothesis, that it results from a structural analysis whose ordering criteria devolve from cognitive-developmental theory. This permits one to jettison the apparently troubling notion of logical necessity in favor of one involving the empirical demonstrability of a theoretically consistent stage sequence. This clarification is at least implicitly endorsed by Puka when, in accounting for the fact that stages could not be skipped, he asserts that it is because "the structural capacities at the higher stage involve and transcend the capacities of the stage which logically might have been [$logic_1$], but psychologically, for structural reasons [$logic_2$], could not be skipped."[48]

THE PROTECTIVE BELT

We next consider Phillips and Nicolayev's exposition of the Kohlbergian "protective belt." Under this rubric are included (1) variation in the scoring system; (2) *ad hoc* and *ad hominem* argumentation; (3) the conflation of universal and tendency claims; (4) the inadequate presentation of data; (5) the inaccessibility of key manuscripts; and (6) the tendency to seek confirmation and avoid refutation.

It is Phillips and Nicolayev's contention that these six devices enable Kohlbergians to deflect the *modus tollens* from the core of the moral development theory, which they believe to be at risk anyway. Thus they write: "There can be no doubt that the Kohlbergian hard core is at risk. The claim of logical necessity has been shown to be untenable, the assertion of invariance is faced with considerable counter-evidence, and even the stage-assumption — which is basic to the whole program — has been found unintelligible."[49]

Nothing in the above passage indicates a proper conceptualization of the Lakatosian model. A research program is not endangered when its protective belt collapses. Rather, the program is endangered only when the collapse occurs in conjunction with the failure of the positive heuristic of the research program to uncover new facts, to

46. Popper, *Logic*, 39.
47. Puka, "A Kohlbergian Reply," 289.
48. Ibid.
49. Phillips and Nicolayev, "Kohlbergian Moral Development," 295.

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open up new domains of inquiry. We will have more to say about this below. First we must address a further misconception of the relationship between the hard core and the protective belt.

Phillips and Nicolayev seem to believe that the hard core first sustains evidential attacks and subsequently is rescued by chimerical gimmickry or sleight-of-hand manipulations of devices collectively known as the protective belt. However, it is not correct to assert that the hard core is at risk, that its features are "untenable," "unintelligible," and faced with "considerable counterevidence" and then to argue that this is somehow acceptable only if the protective belt is a strong redoubt against counterevidence. Since the core of the theory, by methodological decision, is *protected* from the *modus tollens,* it can never be under test, it can never be at risk or be faced with counterevidence. This is the definition of the hard core. It is the protective belt which must bear the brunt of tests. It is quite impossible for the hard core to be at risk prior to the collapse of the protective belt, prior to the thorough testing of appeals to *ceteris paribus,* prior to the inability of the positive heuristic to anticipate novel facts, and, importantly, prior to the emergence of a better theory.

Incredibly, Phillips and Nicolayev believe that a protective belt involves methodological chicanery (point 1, above), obfuscation and deception (points 2, 4, and 5), and intellectual dishonesty (points 3 and 6). This reflects a serious misunderstanding of the Lakatosian approach. The protective belt is not a collection of disingenuous tricks and mirrors. It is instead a substantive series of auxiliary theories and a positive heuristic. These must face the *modus tollens* (in conjunction with *ceteris paribus*). If such tests are failed, the hard core faces "demise" only if two conditions are simultaneously met: that there exists a more powerful rival theory and that the positive heuristic is incapable of generating novel facts (or is incapable of turning recalcitrant evidence into supporting evidence).

Cognitive developmentalists must use their ingenuity to articulate or invent auxiliary hypotheses that serve to protect the hard core of the program from *prima facie* refutations via the *modus tollens.* Protective-belt auxiliary hypotheses often emerge from the *ceteris paribus* clause.[50] For example, a Kohlbergian could appeal a disconfirming experimental outcome by questioning the theories underlying the assessment of moral reasoning (e.g., scoring procedure, *methode clinique* assessment, procedures to boost test reliability, models to assess simplex structuring, etc.), theories previously relegated to unproblematic background knowledge.

A more graphic illustration of protective belt hypotheses, however, involves the "revisions" of Kohlberg's theory to account for the apparent structural regression of advanced moral reasoners to Stage 2, reported in Kohlberg and Kramer.[51] Kohlberg argued that while the college student responses resembled the naive hedonism of Stage 2 reasoners, the college student thought was actually more abstract and philosophical than would be possible if their responses truly represented functional regression.[52] He subsequently hypothesized a transitional stage 4½ from conventional to principled moral thought and introduced a "level of discourse" concept to account for their distinction. That is, one must analyze moral discourse for levels of abstraction and reflection. Stage 2 moral reasoners resolve *dilemmas* by an appeal to the hedonism of selfish actors. College student discourse, on the other hand, justifies a moral *theory* from a standpoint outside the prescriptions of individuals or societies. According to Gibbs, "This level-of-discourse distinction provided Kohlberg with a tool for solving the regression problem. The discourse analysis had eliminated the notion that subjects

50. Lakatos, "Falsification," 49.

51. Kohlberg and Kramer, "Continuities and Discontinuities."

52. L. Kohlberg, "The Claim to Moral Adequacy of a Highest Stage of Moral Judgment," *Journal of Philosophy* 70 (1973): 630-46; and Kohlberg, "Continuities in Childhood and Adult Moral Development Revisited," in *Lifespan Developmental Psychology,* ed. P. Baltes and K. W. Schaie (New York: Academic Press, 1973).

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principled in high school had really regressed to stage 2 by identifying a new transitional phase of sophisticated, if self-serving, [metaethical subjectivist] relativism."[53]

Yet, as Gibbs points out, there is still a regression problem to deal with, since formerly principled reasoners were now found to be at the transitional stage 4½.[54] New hypotheses were required to address this regression. Kohlberg hypothesized that the philosophical discourse characteristic of stage 4½ was not truly theory defining, since there was no evidence of *a priori* principles by which to organize the discourse.[55] Further, Kohlberg argued that the level of discourse actually employed represented advanced "conventional" reasoning. Thus, according to Gibbs:

> Kohlberg . . . refined his post-conventional stages into more philosophical constructs, and expanded his conventional-level stage definitions to include the formerly "principled" idealizations as new "B" substages. Correspondingly, the previous definitions of stage 3 and stage 4 became merely "A" substage versions of the stages. Alongside concerns with others' approval and role-stereotypical good conduct (3A) were placed concerns for *mutual* good faith or understanding and for *universalized* caring (3B); Alongside concerns with fixed responsibilities or authority and the givens of laws (4A) were placed concerns with *ideal* responsibility to contribute to a *better* society and with *moral* law (4B).[56]

Thus, we see how the formulation of auxiliary hypotheses protects the hard core of the theory from refutation. Rather than insist that evidence of regression vitiates the hard core development-by-stage requirement of structuralism, Kohlberg insists instead that conjectures regarding transitional stages, levels of discourse, and stage typing bear the brunt of future tests. It should be pointed out that other (Kohlbergian) theorists have proposed their own protective belt hypotheses, such as Gibbs's two-phase approach. Since, as we have pointed out, there is no instant rationality in the appraisal of a theory, we must await the further testing of these theoretical adjustments.

It is compelling to note that, while Phillips and Nicolayev are willing to conclude that the Kohlbergian research program has failed to anticipate novel facts, they do not indicate what constitutes the positive heuristic which would have allowed the program to uncover such facts (not all of which need to be corroborated). This oversight, however, is not surprising; it is instead the unintended and inevitable result of their conflating Kohlberg's moral development theory with the cognitive-developmental approach research program. It is further the result of their conflating a particular stage model (generated by the positive heuristic) with the hard core structuralist prerequisite that development have stagelike features. When one unsorts the category errors and allows the proper distinctions, one discovers that "stage," the instantiation of positive heuristic of the cognitive-developmental approach, has been extraordinarily successful in generating novel facts, even in the moral development domain.

For example, in the moral development domain, stage models have been used to identify corroborated sequences in distributive justice and, more recently, in retributive justice.[57] Stage concepts are also involved in our understanding of other substantive domains within the cognitive-developmental approach, such as role taking, interpersonal conceptions, and political development.[58] Stages have been used recently to assimilate

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53. Gibbs, "Kohlberg's Moral Stage Theory," 92-93.
54. Ibid.
55. Kohlberg, "Continuities Revisited."
56. Gibbs, "Kohlberg's Moral Stage Theory," 93.
57. W. Damon, "Early Conception of Positive Justice as Related to the Development of Logical Operations," *Child Development* 46 (1975): 301-12; and D. Lapsley, "The Development of Retributive Justice Reasoning in Children" (Ph.D. diss., University of Wisconsin-Madison, 1982).
58. R. Selman, "Toward a Structural Analysis of Developing in Interpersonal Relations," in *Tenth Minnesota Symposium on Child Psychology,* ed. A. Pick (Minneapolis: University of Minnesota Press, 1976); J. Adelson and R. O'Neil, "Growth of Political Ideas in Adolescence," *Journal of Personality and Social Psychology* 4 (1966): 295-306.

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the rigidity construct within the cognitive-developmental approach[59] and were invoked to provide a social-cognitive developmental reconceptualization of intolerance and dogmatism.[60] These examples, and they could be multiplied, are testimony to the fact that stage models are busy extending the parameters of the cognitive-developmental approach. The conclusion is inescapable, then, that the cognitive-developmental approach constitutes a *progressive research program* and is not in present danger of degeneration.

The purpose of this article was to reconsider a recent application of evaluative criteria, worked out on a level of metatheory by Lakatos, to Kohlberg's theory of moral development. While the attempt by Phillips and Nicolayev to apply the Lakatosian model to an actual developmental theory is strikingly commendable, we disagree with the details of the application. We argue that their effort was compromised by two significant category errors, by a failure properly to relate the hard core with the protective belt, by misidentifying the features of the core and belt, and by failing to identify the positive heuristic of the research program. By making the proper distinctions, we subsequently conclude, in opposition to the Phillips and Nicolayev position, that Kohlberg's theory represents a progressive problemshift in our understanding of morality and that the cognitive-development approach is a progressive and not a degenerating research program.

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59. D. Lapsley and R. Enright, "A Cognitive Developmental Model of Ridigity in Senescence," *Journal of Aging and Human Development,* in press.

60. R. Enright and D. Lapsley, "Judging Others Who Hold Opposite Beliefs: The Development of Belief Discrepancy Reasoning," *Child Development* 52 (1981): 1053-63.

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