Dynamic Functionalism: An Exchange

MARION J. LEVY, JR., Princeton University
MICHAEL A. FAIA, College of William and Mary

Dynamic Functionalism: Strategy and Tactics.
By Michael A. Faia. Cambridge University Press. 187 pp. $27.95.

Reviewer: MARION J. LEVY, JR., Princeton University

Professor Faia’s book was awarded one of the most prestigious prizes the American Sociological Society can bestow, selection for the Rose Monograph Series. “The sole criterion for publication in the series is that a manuscript contribute to knowledge in the discipline of sociology in a systematic and substantial manner.”

To be fair to Faia, I give you in part his very own précis of his book, taken from his very own Preface:

The thesis of this book is that Kingsley Davis, in his famous presidential address before the American Sociological Association (1959), was mistaken in the claim that all sociologists are functionalists and that the functionalist paradigm is basically unsound. . . . I argue in the first two chapters that functionalism has unique elements that are not fully exploited by most social scientists, and that this uniqueness has to do largely with the way the functionalist model focuses on the survivability of social organizations as they age.

Chapter 3 contends that the stable population model, specifically the life table, provides a way of analyzing one of the major dependent variables of functionalism—the survivorship of social organizations—and that . . . functional analysis based on the model is inherently dynamic. . . .

Chapter 4 shows that the major prolegomenon of any functional analysis is to trace the behavior of relevant variables through time: I show that the functionalist paradigm readily comprehends a clear instance of Marxian catastrophism, and that there is therefore no strong incompatibility between functional analysis and Marxist analysis.

Chapters 5 and 6 . . . provide copious illustrations of dynamic functionalism, followed by a brief introduction to time-series analysis. . . . It becomes clear in these chapters that, for the present, dynamic functionalism is more readily illustrated by contrived data than by real data. . . .

Part Three concludes that functionalist propositions ought to be considered the summum bonum of the social scientific endeavor. . . . The Appendix, essentially a supplement to Chapter 1, argues that the celebrated shortcomings of earlier functionalists, from Malthus to Parsons, do not inhere in the functionalist paradigm itself but derive rather from the idiosyncrasies of its practitioners.

Faia is, I would judge, very bright, maybe even brilliant, extremely learned and culturally literate, and he has a sense of humor—witness the “delightful graf-
fito” (which he correctly puts in the singular) at the end of his Appendix. Nevertheless, if a book may be judged by its title, his book is a disaster as far as science is concerned. This is surprising because, judging from internal evidence in the book, Faia knows a great deal about science—far more than most sociologists. He has neglected, however, to come to grips with a peculiar characteristic of science. Science is the only language in which denotation is everything and connotation is a curse. Even “the law” does not go to this extreme. Nowhere in his book can I find Faia’s definition of the term functionalism or a forthright declaration that for his work that concept is an undefined predicate. He tells us that his undefined term is “inherently dynamic,” he often tells us bits and parts of what other people mean by the term, but he does not take his readers into his confidence on the meaning of the term on which his whole monograph rests.

Recently I told a colleague that more than 35 years ago I had tried to comb out the nonsense (e.g., one term used with several quite different meanings, undefined terms, unintentional confusion of one’s values with one’s analysis, teleology, misuses of stability assumptions, etc.) associated with the concepts of function and structure and that the remnant was simply a way of looking at any scientific analysis—not just any sociological analysis, but any analysis. My colleague asked: “If you comb the nonsense out of a given approach, can one still describe the resultant as that approach?” I had no ready answer for her. The same question may be asked of Faia’s monograph. Faia maintains in his preface “that functionalism has unique elements that are not fully exploited by most social scientists, and that this uniqueness has to do largely with the way the functionalists’ model focuses on the survivability of social organizations as they age.” One can question that assertion from two ends: (1) is that characteristic of the structural-functional approach in general, and (2) is it unique to the structural-functional approach? I insist on linking the two concepts, function and structure, because either implicitly or explicitly one never does any analysis without reference to both of them, and they fall into a peculiar category of concepts: i.e., what is a function from one point of view is a structure from another, and vice versa (cf. oxidation and reduction, anabolism and catabolism, power and responsibility, etc.). (For my definitions of all these terms, see The Structure of Society [1952].)

Faia further maintains in his Preface that “functional analysis based on [the stable population model] is inherently dynamic, that is, oriented to the study of social change.” This raises another question about this monograph. If functional analysis is “inherently dynamic,” is not its title, Dynamic Functionalism, inherently redundant? This is the first intimation we have of a general problem with this monograph: so little precision in the face of so much pretension. Finally, Faia mentions that his Part Three will show that “functionalist propositions ought to be considered the sumnum bonum of the social scientific endeavor.” (I must confess that I am rather put off by one of Faia’s tics. He, along with most other sociologists today, never fails to use the term “paradigm” when the more ordinary term “model” would better serve purposes of clarity, precision, and brevity in any ordinary scientific discourse.)

Faia heavily emphasizes what he refers to as “circular causation”—a term that in itself should have set off warning lights and buzzers in his mind. (Circular viewing is a vice in all empirical sciences.) At the present state of the development of the social sciences, the point of view of the actor and especially the concept of
"future states of affairs towards which action is oriented" are highly relevant. There is no scientific way to demonstrate that view of the actor to be a nonreducible element in the scientific analysis of the behavior of human beings, however fashionable such a point of view might be with sociologists and anthropologists. Nevertheless, unless we find a way around the most powerful of all generalizations on this score, that of W. I. Thomas, "If men define situations as real, they are real in their consequences," Stitchcombe's reductio ad absurdum of this point of view ("Many social phenomena tend to be causes of themselves—once established they tend to maintain themselves.") will continue to plague us as a confusion of what need not be confused. At the very least it frequently implies a sort of special social teleology: "Whatever Lola wants, Lola gets!" Empirically, what Lola wants Lola frequently does not get, although the fact that she wants it may still be highly relevant to an understanding of her behavior. In addition, the relation between the achievement or nonachievement of empirical ends sought by actors, let alone non-empirical ones, does not bear any simple relation to the maintenance of the actors' point of view in these respects. Circular causation is as much a fallacy or a tautology in social analysis as in any other scientific field. No astrophysicist or even layperson is likely to be caught dead today saying that stars are sucked in by black holes because black holes have an urge to consume them.

Faia has a peculiar methodology in his apparent assumption that what he has alleged somewhat earlier in his text is fully established and can be taken for granted as applying subsequently. Thus on page 14 one finds Davis "continuing his misdirected effort to show that functionalism is the common practice of virtually all social scientists," but Faia has not even given us his criteria for determining that Davis' effort is "misdirected."

Sometimes it is difficult to know whether Faia is teasing us. On page 56 Faia has not yet given us a definition of functionalism, although he has referred to two elements referred to by Davis (i.e., the question of intentionality and feedback on the one hand and of self-regulation on the other), yet he suddenly states that "this chapter proposes that the major goal of any functionalist inquiry is to clarify the nature, determinants and consequences of a given time series process." Since all material phenomena, whether social or nonsocial, occur in some time-space frame of reference and since the terms "nature," "determinants," and "consequences," are all undefined predicates in this book, what he describes as a major goal of any functionalist inquiry would presumably involve all the goals any scientific inquiry of any sort could have. Is there any kind of inquiry about anything that takes place in a time-space frame of reference that has no concern with the nature of the process? And whether we are concerned with old-fashioned causal statements or the very latest in equations, is not everyone in the scientific realm concerned with determinants and consequences in some sense? Faia has given us a low-level form of a high-level truism.

Faia is given to such statements as "The Club of Rome world model is nothing more than a sophisticated form of Malthusianism, and it is readily comprehended under the functionalist rubric despite the fact that it seems to lead to catastrophe." He has not demonstrated that the Club of Rome model is sophisticated, but never mind about that. What is more relevant to his book is that he has not shown that the functionalist rubric is particularly relevant or fruitful for the subject or, for that matter, that anyone ever showed that functionalism couldn't
handle catastrophes. Insofar as the various categories of structure and function—shorn of their nonsense, improper uses of teleology and stability assumptions, and the like—reduce to nothing more than a general vocabulary for science, then of course such a translation may be made, but at the sacrifice of holding that the functionalist's rubric is either peculiar or fruitful as such. Since Faia has not translated the text into functionalalese and since the people who derived the results to which he refers certainly didn't make use of that tongue on any conscious level, it is hard to know what Faia has accomplished by such examples.

Again, when Faia asserts that "The 'bottom line,' however, is that there is almost nothing in Marxian theory that cannot be incorporated into dynamic functionalism," his statement may be a triumph of simple assertion, but since "dynamic functionalism" is both redundant and undefined it is a little difficult to know how seriously one should take it, whether one is a Marxist or not. Even if one grants Faia a meaning for "dynamic functionalism," this statement remains a non sequitur from preceding statements about the Club of Rome.

There is another aspect of this business of considering all functional analysis as dynamic. One of the most popular statements Talcott Parsons made, after he had begun writing (ca. 1945) in a way that gave all of those who are anti-Parsonian for the wrong reasons a tenable place to stand, was that we had in sociology some reasonably adequate and acceptable static theories, but we lacked dynamic ones. If one does not mean by this distinction that dynamic is good and static is bad, but rather uses these terms as they are used in the physical sciences from which they have been taken, to refer to whether a given proposition includes time as a variable or not, then the statement that we have static theories but no dynamic theories constitutes a curious self-deception. There are not static theories that cannot be translated into dynamic theories and vice versa. The statement, "x is a requisite of y," is a static theory (proposition). Time is not a variable in it. If that statement is tenable, however, then it will follow as the night the day that, if one has a y and removes the x from that y, the y will go out of existence. That is a dynamic proposition. Correspondingly, one can reverse the process.

In the social sciences, the term dynamic often implies "good." Everywhere one sees titles like "The Dynamics of Child Development." Has one ever seen a title such as "The Statics of Child Development"? Indeed, what would such a title mean? Those who hold Freudian theory to be dynamic and Adlerian theory to be static surely mean that they prefer Freudian theory to Adlerian, not that one includes time as a variable and the other not. In the social realm where most attention is firmly focused on therapeutic relevance, time as a variable is likely to be relevant a good part of the time. But to believe that dynamics is somehow in essence better science than statics (or that dynamic functionalism is preferable to static functionalism, which Faia has told us can't exist anyway) is to misunderstand and perhaps even to interfere with the development of dynamic propositions.

Chapters 4 and 5 are interesting substantively, but Faia never makes clear how these chapters are related to his central subject except that in both time is a relevant variable and expectations have something to do with the cases. Much the same can be said for Chapter 6. In the introduction to that chapter, Faia tantalizes us: "It is concluded that powerful forms of functional analysis will be forthcoming when survivorship variables are incorporated into adaptation hypotheses tested by
means of modern methods of time series analysis.” Even here, he does not make clear what he means by powerful. He can’t mean highly generalized, as one might in physics, because later he asserts that “nothing kills a science more quickly than the dead level abstraction.”

Finally, Faia ends his book with a fascinating discussion of how research on sexual discrimination in university pay scales might be better studied than it has been to date. All aye to that! That is penultimate to a specification of the rules for writing Petrarchan sonnets and the final expression “Let us hope that the integrated methodological paradigm encourages graduate students to do social theory as well as the preceding recipe encourages undergraduates to write Petrarchan sonnets.” Amen to that! But is that hope—how well have undergraduates been encouraged along these lines?—even if forlorn, not independent of a discussion of functionalism, whether dynamic or not? In any case Faia has not told us with any precision what he means by dynamic functionalism. He has not shown us how anyone relates that undefined method to the most interesting studies he cites, except that time and points of view are relevant to both. He has not shown us that functionalist propositions are or ought to be the summum bonum of social scientific endeavor. He has shown us what wins prizes in sociology!

Faia Responds:

At first blush, I feared that Levy was correct in claiming that I had not provided a clear definition of functionalism. But then I reexamined the pages in which I discuss Kingsley Davis (4–5), and I soon realized that Davis’ provisional definition should have sufficed—for functionalism, dynamic functionalism, and Marxian functionalism: Functionalism is a form of analysis that attempts to relate parts of society to the whole; it attempts to explain social structures with reference to their consequences; it relies heavily on the notion of feedback and circular causation. Admittedly a little vague, this definition is nonetheless an excellent basis on which to start a book. In my case, by the time I got deeply into this volume (pp. 128 ff.) I was prepared to offer a highly detailed definition of functionalism: Functionalism is a form of analysis that scores 4 on my “hypothetical Guttman scale of sociological propositions” in that it involves multivariate causal analysis with time-series data and makes use of feedback and circular causation. I agree with Levy’s remark that scientific concepts must have clear denotation; I also maintain that nothing is more precise than a Guttman scale. It was the sole purpose of my book to take the prevailing definitions of functional analysis and transform them from a gooey muddle into a Guttman scale. To me, the Guttman scale is our best attempt, so far, to specify denotation with precision.

There are several instances in which Levy does not summarize my arguments accurately. The most serious of these occurs when he raises a pair of seemingly rhetorical questions about survivability of social organizations, failing all the while to note that I have answered both questions in detail. First, given that functionalism concentrates on i-functions (interest-functions) at least as often as it concentrates on s-functions (survivability functions), survivorship analysis is not “characteristic of the structural functional approach in general.” Second, any variable, including survivorship, can be studied from non-functionalist points of view,
so that survivorship analysis is clearly not "unique to the structural functional approach." To return to my Guttman formulation, a social scientist could undertake a lovely investigation of organizational survivorship that would be ordered (i.e., it would have time as a variable), multivariate, and causal; but such an investigation would not immediately be functionalistic. It could become functionalistic, however, if the investigator were to follow my admonition—one which Levy quotes but does not grasp—that "powerful forms" of functional analysis would occur if survival were treated as an adaptation process involving feedback. In other words, when the survival of an organizational type is threatened, what sorts of structural changes take place that may (or may not) lead to enhanced survival prospects? Note that I do not use tempting phrases about changes that might occur *in order* to enhance survivorship, leaving open a question that Levy wrongly seems to think I have closed. On Levy's question whether Lola gets what she wants, I reserve judgment: Let Mephistopheles bring her front and center, and I'll be delighted to ascertain, empirically, whether she achieves her objectives.

Someday, I hope, it will be an obvious redundancy to refer to *dynamic* functionalism. For now, however, I feel toward this phrase the same way that Herman Wouk must have felt when he entitled one of his books *Aurora Dawn*. The redundancy, he said, will probably assail your ears; but on the other hand, it might help to impress upon you the simple beauty of the newborn day. I hope the warming rays eventually reach Marion Levy.

When Levy says that my use of the term paradigm is a "tic," I wish he would look up the word. Tic, not paradigm. My dictionary says that a tic is a spasmodic muscular contraction not subject to intentional control. If Lola were nothing more than an uncontrollable motley of sweet and tender tics, we would probably hold her blameless. Why can't I receive the same consideration as she?

Returning to the more fundamental issues—Levy's tic is his instinct not for the jugular, but for the capillaries—I agree completely with Levy's wish that "warning lights and buzzers" be set to scream wildly whenever social scientists invoke the idea of circular causation. In large part my book deals with the costs and benefits of trying to assess causal interactions through time. There are many pitfalls, but there is also a possibility of placing functional analysis—our best established and, with Marxism, our most durable theoretical tradition—on a firm methodological foundation that would force us to eliminate the traditional mumbo jumbo about prerequisites, teleology, stability, and consensus while testing falsifiable hypotheses about system interaction through time in which survival functions and/or interest functions may or may not be "fulfilled"—this last word has the taint of the old mumbo jumbo. Incidentally, I am mystified by the fact that in the same paragraph where Levy warns us about circular causation he expresses his worries about possible neglect or abuse of the "point of view of the actor." The latter is just another set of variables that we introduce into an analysis whenever they appear to be relevant. At the moment, I see no connection between circular causation issues and the matter of the actor's point of view. And far be it from me to accuse black holes of deliberately gobbling up little stars, for whatever nefarious and polymorphously perverse purposes these Sultans of Suction may have. On the other hand, natural scientists and engineers could not live without circular causation.
Anybody who believes with Levy that causal analysis, circular or otherwise, can be taken for granted in the social sciences should thumb through a few recent issues of the ASA's new journal *Sociological Theory*. Causal analysis as a central concern of sociology is not a "truisms," as Levy alleges.

I am still prepared to argue, as I was when I wrote this book, that the functionalist paradigm—er, model—encourages us to focus on time as a variable more than do alternative models. In short, circular causation hypotheses encourage us to use lagged variables. Until such time as Levy demonstrates the irrelevance of my best examples—the Malthusian theory of population, which evolved into a true adaptation theory between 1798 and 1803, the Club of Rome world model, Zerubavel's work on the social definition and social organization of time, the sex preselection of children, sex discrimination in academic salaries, the delict-sanction model as applied to capital punishment, the interaction of military expenditures and economic stagnation at the state or nation-state level—I will continue to believe that each of these research traditions has achieved strength (or could be further strengthened) insofar as scholars have been willing to examine the reciprocal interaction of variables through time. I will also continue to believe that, even though the scholars in question may not realize it, every time they examine interactions through time in a way that emphasizes organizational survival and/or the degree to which interests (i.e., manifest functions) are served, they are practicing functional analysis. Finally, because scholars do not automatically make Levy's transition from the statement that "x is a requisite (i.e., a cause) of y" to the admonition "remove the x from that y" and see what happens (as Stinchcombe used to say), they need to be encouraged to do so.

**Levy Replies:**

Let the reader read Professor Faia's book and my review and his reply and make up his or her own mind about the volume. Let both also consult *Webster's New International Dictionary*, 2d edition, where a "tic" is defined as "involuntary or habitual."

**And Faia Responds:**

To Levy my tic may seem habitual, a mere ritual, an expression of a Durkheimian norm, and as regular as clockwork. To me, however, this particular tic is a highly functionalistic, etic tic. Now, we're too sophisticated to keep statistics on this dialectic, but it keeps sticking in my mind that there are even tics in my title. And this ticks me off.

**The Book Review Editor Intervenes:**

Enough. If there's a dead horse here, I suspect it died of Rocky Mountain spotted fever.