The Methodology of Positive Accounting

Charles Christenson


Stable URL: http://links.jstor.org/sici?sici=0001-4826%28198301%2958%3A1%3C1%3ATMOPA%3E2.0.CO%3B2-Z

The Accounting Review is currently published by American Accounting Association.
ABSTRACT: Jensen, Watts and Zimmerman (referred to hereafter, following Jensen [1976], as “the Rochester School of Accounting”) have charged that most accounting theories are “unscientific” because they are “normative.” They advocate the development of “positive” theories to explain actual accounting practice. The program of the Rochester School raises a number of methodological issues that are addressed in this article. First, it is argued that the Rochester School’s criticism of traditional accounting theory is off the mark because of a failure to distinguish between two different levels of phenomena. Second, it is argued that the concept of “positive” theory is based on the misconception (derived from nineteenth-century positivism) that empirical science is concerned solely with the actual, with “what is.” Empirical theories, it is shown, are negative in their import; they state what is to be taken as empirically impossible. Third, it is shown that “negative” theories of the sort described in this article are exactly what is needed in predictive, explanatory, and normative reasoning. Finally, it is argued that the standards advocated by the Rochester School for the appraisal of their own theories are so weak that those theories fail to satisfy Popper’s [1959] proposal for demarking science from metaphysics.

Heralding an “emerging Rochester School of Accounting,” Jensen [1976] charged that “research in accounting has been (with one or two notable exceptions) unscientific... [b]ecause the focus of this research has been overwhelmingly normative and definitional” [p. 11]. In accounting, he said, “‘theory’ has come to mean normative proposition. The so-called accounting theory texts are almost entirely devoted to the examination of questions of a ‘what ought to be done’ nature” [p. 11]. Jensen called for “[t]he development of a positive theory of accounting [which] will explain why accounting is what it is, why accountants do what they do, and what effects these phenomena have on people and resource utilization” [p. 13]. Without such “positive theory,” he said, “neither academics nor professionals will make significant progress in obtaining answers to the normative questions they continue to ask...” [p. 12].

Research supported by the Division of Research, Harvard University Graduate School of Business Administration, out of funds provided by the Associates of the Harvard Business School. For comments on earlier drafts of this article, I am indebted to Rashad Abdelkhalik, Robert Anthony, W. W. Cooper, Anthony Hopwood, Robert Jaedicke, Spiro J. Latsis, E. A. Lowe, Richard Mattessich, Merton Miller, Joseph G. San Miguel, Edward Stamp, Robert Sterling, Tony Tinker, Richard Vancil, H. Martin Weingartner, and two anonymous reviewers. I have also benefited from the comments of the participants in the Seminar in Financial Accounting and Financial Analysis at the Harvard Business School; the Workshop in Accounting at the Sloan School of Management, Massachusetts Institute of Technology; and the Workshop in Methodology and Accounting of the European Institute for Advanced Studies in Management.

Charles Christenson is Royal Little Professor of Business Administration, Harvard University.

Manuscript received July 1981.
Revision received April 1982.
Accepted May 1982.
Subsequently, Jensen’s colleagues Watts and Zimmerman have carried the banner of Positive Accounting forward in a series of articles which repeat several of Jensen’s themes. Watts [1977, p. 54], for example, calls the traditional financial accounting literature “unscientific” because it “concentrates on prescriptions” and gives “[v]ery little attention . . . to developing a theory . . . to explain why financial statements take their current form.” Watts and Zimmerman [1978, pp. 112–113] argue that “a positive theory of the determination of accounting standards” is necessary “to determine if prescriptions from normative theories . . . are feasible.” In a later paper, while continuing to distinguish between “positive” and “normative” theories, they make it clear that they award the honorific “theory” to the normative accounting literature only to avoid “semantick debate”; they “would prefer to reserve the term ‘theory’ for principles advanced to explain a set of phenomena” [Watts and Zimmerman, 1979, p. 273 n. 1]. Zimmerman [1980, pp. 107–108] says that “positive research seeks to develop a theory that can explain observed phenomena.” Watts and Zimmerman, then, appear to be the principal members of the “Rochester School” announced by Jensen.

Each of the two articles coauthored by Watts and Zimmerman cited above won the AICPA’s award for a Notable Contribution to the Accounting Literature in its year of publication, and an article by Zimmerman [1979] seeking to develop a “positive” explanation of cost allocation won the Competitive Manuscript Award of the AAA. That these articles are considered “top of the heap” is, I shall argue, a sad commentary on the standards used in evaluating contemporary accounting research. This is a reflection, I would say, of the fact that accounting researchers today are well trained in research methods but hardly at all in methodology.¹ Machlup [1963] distinguishes the two:

Methodology, in the sense in which literate people use the word, is a branch of philosophy or of logic. . . . Semiliterates adopt the word when they are concerned neither with philosophy nor with logic, but simply with methods. Instead of “statistical techniques” they would say “statistical methodology,” and instead of “research methods” they love to say “research methodology.”

“The methodology of a science,” as Blaug [1980, p. 47] says, “is its rationale for accepting or rejecting its theories or hypotheses.” Thus, methodology is normative, and for that reason it would presumably be called unscientific by Jensen and Watts.² Yet, obviously no science can exist without making methodological commitments. Hayek [1952, p. 37] even asserts that science as a whole is normative: “Its concern is not what men think about the world and how they consequently behave, but what they ought to think” [emphasis supplied].

Like other normative judgments, methodological ones may be made with varying degrees of self-consciousness. In

¹ Zimmerman [1980, p. 120] asserts that the time is now propitious for positive research in accounting because accounting researchers are becoming increasingly well trained in economic theory and research methods. If Blaug [1980, p. xiii] is to be believed, however, this is no cause for complacency. He says that “the average modern economist has little use for methodological inquiries” and that, “to be perfectly frank, economic methodology has little place in the training of modern economists.”

² It is not surprising that positivists such as Jensen and Watts hold methodology in low esteem. As Popper [1959, p. 51] observes, it is a characteristically positivist dogma to deride methodology as “unscientific” and “meaningless”: “The positivist dislikes the idea that there should be meaningful problems outside the field of ‘positive’ empirical science . . . He dislikes the idea that there should be a genuine theory of knowledge, an epistemology or a methodology.” This dogma does not enable positivists to avoid methodological commitments but only makes it less likely that they will be self-conscious about the commitments they make.
the more established sciences, which have a research tradition of proven fruitfulness to follow, awareness of methodological issues among what Kuhn [1970] calls "normal scientists" may be low. But as Friedman [1953, p. 40] argues, "more than other scientists, social scientists need to be self-conscious about their methodology." Samuelson [1962, p. 21] for once agrees: "Paradoxically, the soft sciences that are still akin to an art benefit more from an explicit awareness of the canons of scientific method ... than do the hard sciences, where doing what comes naturally will protect even a fool from gross methodological error."

In the interests of consciousness-raising among accounting researchers, therefore, I will examine in this article the following questions raised by the methodology of the Rochester School. What should be the domain of accounting theory? What, if anything, is a "positive" theory? How can scientific theories be used in explanation, prediction, and prescription? How should scientific theories be appraised?

I. THE DOMAIN OF ACCOUNTING THEORY

"To have a science of anything," according to Caws [1972, p. 72], "is first to have recognized a domain and a set of phenomena in that domain, and second to have devised a theory whose inputs and outputs are [descriptions of] phenomena in the domain (the first observations, the second predictions) and whose terms may describe the underlying reality of the domain."\(^3\)

To illustrate the distinction between "normative" and "positive" research questions, Jensen [1976, pp. 11–12] presents two lists of examples, reproduced as Figure 1. The most obvious difference between the two lists—as Jensen intended—is that each question on the "normative" list contains the word "should" and each question on the "positive" list contains the words "why," "what," or "how."

There is a second, more subtle difference between the two lists. Every question on the first list is about the description of accounting entities [Kohler, 1975, p. 14]. Every question on the second list, in contrast, is about the description and explanation of the behavior of accountants, i.e., those persons responsible in some way for the description of accounting entities.

Thus, in his attempt to illustrate the positive-normative distinction, Jensen has managed to confound it with another—that between phenomenal domains at two different levels. Jensen's confusion is of exactly the type warned against by Popper [1972, pp. 176–177], who points out that the problem of understanding the behavior of a problem-solver is on a higher level than the problem which concerns the problem-solver:

The problem of understanding is a metaproblem. ... Accordingly, the theory designed to solve the problem of understanding is a metatheory. ... We have to distinguish clearly between the metaproblems and metatheories of the historian of science ... and the problems and theories of the scientists. ... It is only too easy to mix these two up ...

To clarify matters, I propose a three-way classification of accounting problems rather than the one-way, positive-normative classification suggested by Jensen. With respect to a given problem, the first question to be asked is whether it is, in Popper's terms, a problem or a metaproblem. To use an accounting example, are we concerned with the problem faced by General Electric's management in deciding what to present in the company's financial statements for 1981? Or are we...

\(^3\) I have interpolated the two bracketed words into Caws's statement in anticipation of a point to be developed later in this article.
<table>
<thead>
<tr>
<th>&quot;Normative&quot; Questions</th>
<th>&quot;Positive&quot; Questions</th>
</tr>
</thead>
<tbody>
<tr>
<td>1) How should leases be treated on the balance sheet?</td>
<td>1) There is much discussion in the literature regarding the “needs” of those using accounting reports. Why is there little or no attention paid to the “needs” of the suppliers of accounting reports? What are the supply-side forces, and what impact do they have on accounting practices?</td>
</tr>
<tr>
<td>2) Should replacement (or liquidation) values be used in the balance sheet and income statements?</td>
<td>2) Why do most firms continue to allocate overhead charges to performance centers?</td>
</tr>
<tr>
<td>3) How should changing price levels be accounted for?</td>
<td>3) Why do firms change accounting techniques?</td>
</tr>
<tr>
<td>4) How should changes in foreign exchange rates be accounted for by firms with foreign interests?</td>
<td>4) Why do firms change auditors?</td>
</tr>
<tr>
<td>5) How should inventories be valued?</td>
<td>5) Why has the accounting profession been cursed with a strong authoritative bias—resulting in the establishment of professional bodies such as the CAP, APB, and FASB to rule on “generally accepted accounting techniques?”</td>
</tr>
<tr>
<td>6) What should be reported in annual financial statements?</td>
<td>6) How have court regulation and rulings influenced accounting practice?</td>
</tr>
<tr>
<td>7) Should interim financial statements be audited?</td>
<td>7) Why do firms continue to use historical cost depreciation for other than tax purposes?</td>
</tr>
<tr>
<td>8) How should minority interests in subsidiaries be treated in consolidated statements?</td>
<td>8) Why are public accounting firms organized as partnerships?</td>
</tr>
<tr>
<td></td>
<td>9) Why is fund accounting so different from corporate accounting?</td>
</tr>
<tr>
<td></td>
<td>10) What impact has the CPA certification procedure had on the practice of accounting and on research in accounting?</td>
</tr>
<tr>
<td></td>
<td>11) What have been the effects on the focus of research of accounting educational programs which require faculty to expend substantial effort teaching institutionally oriented material aimed at the CPA exam?</td>
</tr>
<tr>
<td></td>
<td>12) Why does the accounting field place an emphasis on “professionalism” and “professional ethics?”</td>
</tr>
</tbody>
</table>

concerned with the metaproblem of understanding why management made the choices it did?

For reasons to be stated in Section II, I wish to avoid use of the term “positive.” As an alternative to the positive-normative distinction, therefore, I will use what Popper [1966, vol. ii, p. 383] has referred to as the dualism of “propositions, which state facts, and proposals, which propose policies, including principles or standards of policy.” Questions on Jensen’s first list call for proposals in response; those on his second list call for propositions. The second question to be asked in classifying research problems, then, is whether our problem is to be resolved with a proposition or a proposal. Do we want to know what method GE uses for inventory valuation? Or are we instead concerned with what method they should use?

In Section II I will show that propositions—statements of fact—are of two distinct logical forms: observational and theoretical. If the research problem we are classifying is to be resolved by a proposition, then a third question must be asked: Is the required proposition observational or theoretical?

Figure 2 shows how this classification scheme would apply to various kinds of accounting-related problems. In each cell of the figure, I have indicated the actors who would be concerned with the ac-
counting problems falling in that cell.

The first row of the table is concerned with problems at the primary level, about the state or behavior of accounting entities. The financial statements of an accounting entity have the character of observational propositions. Therefore, practicing accountants, who are concerned with constructing (or “verifying”) these statements on the basis of analyses of the entity’s actual transactions, belong in the first cell of this row.

It has been suggested [FASB, 1978] that an objective of financial reporting for an entity is to enable the prediction of its future cash flows. In Section III, I will show that predictions require not only observational propositions, such as financial statements, but also primary-level theories. The second cell of the first row would be the concern of those who are interested in constructing predictive theories of this kind.

The managers of an accounting entity are concerned with what the transactions of the entity (as distinct from their accounting representation) ought to be. These problems fall in the rightmost cell of the first row.

The problems considered in the traditional “accounting theory” literature criticized by the Rochester School are concerned with how practicing accountants ought to describe accounting entities. They are metaproblems whose solutions are proposals, and therefore they belong in the rightmost cell of the bottom row. I have said above that financial statements of an accounting entity have the character of observational hypotheses. I would therefore call those who are concerned with the rationale for accepting or rejecting these descriptions “methodologists” rather than “theorists.”

The program of the Rochester School is concerned with describing, predicting, and explaining the behavior of accountants and managers, not that of accounting entities. Therefore it also belongs on the metalevel, but in the leftmost cell. (I have not shown the division of this cell into “observational” and “theoretical.”) The discipline of the Rochester School might be called “history of accounting” or “economics of accounting,” since it uses concepts and methods from both history and economics. I prefer to call it “sociology of accounting,” using “sociology” in the inclusive sense of Pareto [1935, p. 3]: “Human society is the subject of many researches. . . . To the synthesis of them all, which aims at
studying human society in general, we may give the name of *sociology.*”

Having sorted matters out as shown in Figure 2, I will now make a few observations. First, the problems addressed by both the traditional accounting literature and the program of the Rochester School occur at the metalevel. For this reason, I would argue that neither the traditional literature nor the Rochester School is directly concerned with what should properly be called “accounting theory.” Such theory belongs to the primary level. This is consistent with usage in the established sciences: Chemical theory consists of propositions about the behavior of chemical entities (molecules and atoms), not about the behavior of chemists.5

Second, I agree with the Rochester School that a scientific theory should be useful in predicting and explaining the phenomena that occur in its domain. However, it is “positive” theory at the primary level that is required to predict the behavior of accounting entities, not theory at the metalevel where the Rochester School has directed its attention. Moreover, as I have already argued, the development of good “positive” theory at the primary level requires sound “normative” theory—methodology—at the metalevel.

It is as if Jensen had advised his fellow financial economists to cease their “unscientific” interest in “normative and definitional” questions such as “How should market efficiency be defined?”, and to turn their attention instead to trying to explain the behavior of other financial economists. The latter would be a fascinating investigation, and useful for some purposes, but it would hardly have improved our understanding of financial markets.

II. THE CONCEPT OF “POSITIVE” THEORY

The reader will have observed that, except in direct quotation from the Rochester School, I have generally enclosed the word “positive” in quotation marks. In this usage, I am following the example of Einstein [1944, p. 281], who wrote that “just as on the part of a real philosopher, quotation-marks are used here to introduce an illegitimate concept, which the reader is asked to permit for the moment, although the concept is suspect in the eyes of the philosophical police.”

In this section, I will present the grounds the “philosophical police” have for suspecting the concept of “‘positive’ theory.” As is appropriate when legitimacy has been questioned, I will begin by discussing ancestry. Then I will show the deficiencies of the concept from a methodological perspective.

Ancestry of the Concept

The Rochester School has drawn its concept of “‘positive’ theory” from that guru of the Chicago School of Economics, Milton Friedman.6 Zimmerman [1980, p. 107] cites a well-known essay in which Friedman [1953] argued “for distinguishing positive economics sharply from normative economics” [pp. 6–7]. It was Friedman’s judgment that “a con-

4 Jensen [1976, p. 15] disdains “sociological” explanation as too narrow, advocating instead a model of “resourceful, evaluative, maximizing man (REMM).” See also Meckling [1976]. Pareto [1935] argues, on the other hand, that the standard economic assumption that people act in accordance with their perceived self-interest is inadequate to explain observed social phenomena and he therefore advocates augmenting this assumption with sociological premises. Jensen [1976, p. 14] advises accounting researchers to become more familiar with Pareto’s ideas. Perhaps he should follow his own advice!

5 A special problem in the social sciences is that the social scientist, as a human being, is also a social entity—and every human being is at least an amateur social scientist. This makes it much more difficult to distinguish between primary and meta levels, but it does not make it any less important.

6 Significantly, both Jensen and Watts earned their Ph.D.s at the University of Chicago, and, of 17 persons acknowledged by Watts and Zimmerman [1979, p. 273], at least eight (including Jensen) did graduate work at Chicago and three others have taught there.
sensus on 'correct' economic policy depends much less on the progress of normative economics proper than on the progress of a positive economics yielding conclusions that are, and deserve to be, widely accepted” [p. 6]. Friedman does not use the term “positive theory,” but he does say that “the ultimate goal of a positive science is the development of a ‘theory’ or ‘hypothesis’ that yields valid and meaningful (i.e., not truistic) predictions about phenomena not yet observed” [p. 7]. The echoes of Friedman in the program of the Rochester School are clear.

Friedman credits his distinction between “positive” and “normative” science to J. N. Keynes, who wrote [1891, pp. 34–35]:

![Image]

The concept of “positive science” was popular throughout the nineteenth century. It was associated with a philosophical school called “positivism,” which held that only the methods of the natural sciences provide “positive knowledge” of “what is.”

As a philosophy of science, positivism is no longer taken seriously. Passmore [1967, p. 56], for example, says that “logical positivism [the last vestige of positivism] is dead, or as dead as a philosophical movement ever becomes.”

Part of what killed logical positivism was the failure of its program to establish the traditional positivist dogma that the propositions of the established sciences such as physics, chemistry, and biology refer only to the actual, i.e., to “what is.” These sciences use theories. Theoretical propositions, as I will show, are neither positive nor normative in Keynes’s sense, neither statements of the actual nor of the ideal. Rather, they are statements of the possible.

The Rochester School’s concept of “‘positive’ theory” is philosophically suspect, then, because it reflects the erroneous belief that a scientific theory constitutes “systematized knowledge concerning what is.” A theory is no such thing. Moreover, the Keynes-Friedman-Rochester concept of “‘positive’ science” is also philosophically suspect, since, to the extent that science is theoretical, science is not concerned solely with “what is.”

Both terms—“‘positive’ science” and “‘positive’ theory”—are misleading and ought to be abandoned. There is a perfectly good substitute for the term “positive” to refer to sciences that are concerned with propositions—matters of fact—rather than with proposals—questions of value. It is the term “empirical,” which I will use henceforth.

Besides, why should anyone want to stand next to a dead philosophical movement?

Product vs. Process Views of Science

Empirical science can be viewed either as a product (a body of systematized knowledge, in Keynes’s terms) or as a process (the human activity producing the knowledge). Positivists have emphasized the product view of science, as exemplified by Keynes’s definition and by the logical positivists’ preoccupation with the formal structure of empirical propositions. The newer philosophy of science

---

7 From the final clause of this quotation I infer that Keynes believed that “positive science” is concerned with “the actual.” I shall criticize this notion later in this section. Friedman, incidentally, does not quote this clause. Nor does Zimmerman [1980, p. 107], who apparently did not consult the original source. Perhaps that is why he misattributes the quotation to “J. M. Keynes,” confusing Neville Keynes with his son Maynard.
emphasizes the process view. An early exponent of this view, Popper, proposed that “empirical science should be characterized by its methods: by our manner of dealing with scientific systems: by what we do with them and what we do to them,” and not “merely by the formal or logical structure of its statements” [Popper, 1959, p. 50].

Popper argues, on the other hand, that the first step in understanding any process should be an examination of its product [Popper, 1972, p. 114]. This should be the case whether our interest is empirical, in which case we start with an actual product and seek to explain it in terms of the process that produced it; or normative, in which case we start with an ideal product and seek to design a process that will produce it.8

Since Popper’s concern is with methodology—the normative theory of science—he is led to analyze what a body of empirical knowledge ideally ought to be, including its logical structure.9 He starts from the premise that the aim of science is to explain observed phenomena, although he observes that a body of knowledge that is explanatory will also be useful as an instrument for prediction and for technological application. So far, it will be seen, the Rochester School’s concept of science is consistent with Popper’s, although some differences will appear later.

I will summarize Popper’s concept of an empirical theory in the remainder of this section. Then, in Section III, I will show what we may do with such a theory, by way of explanation, prediction, and prescription. Finally, in Section IV, I will discuss what we should do to an empirical theory before we accept it, and show that these norms are violated by the Rochester School.

Empirical Propositions

Popper agrees with the logical positivists in considering empirical science, as a body of knowledge, to be a collection of propositions. He also agrees with the positivists in accepting “the fundamental thesis of empiricism”—the thesis that experience alone can decide upon the truth or falsity of scientific statements . . . ” [Popper, 1959, p. 42]. On the other hand, Popper also accepts Hume’s proof [1739 (1888), p. 139] that experience can never conclusively establish the truth of any statement.10 He is thus led to ask whether it is possible to save the fundamental thesis by demanding only one-sided decidability; by requiring, that is, that only the falsity of scientific statements be decidable by experience.

Popper concludes that one-sided decidability—falsifiability—is possible, but only if scientists follow certain methodological norms. What preserves the empirical character of science as a body of knowledge are these norms, and not the logical form of its propositions. It is this conclusion, of course, that drives Popper to the process view of science.

From a strictly logical point of view, a proposition is falsified not by experience but only by the acceptance of another proposition with which it is logically inconsistent.11 Accordingly, Popper de-

---

8 As Popper says [1972, p. 115], “In all sciences, the ordinary approach is from the effects [products] to the causes [processes].”

9 This has led many of Popper’s critics to miss the point that his ultimate concern is with the process of science rather than with its product, and to assume that he has proposed only some minor improvements to the logical positivist program, such as replacing its verificationist criterion of meaning with a falsificationist one. For one such critic who eventually saw the light—“it came to me as a revelation”—see Bar-Hillel [1974, p. 333].

10 Russell [1946, p. 673], an empiricist himself, somewhat despairingly refers to this as “Hume’s destruction of empiricism” and says that its natural sequel has been “the growth of unreason throughout the nineteenth century and what has passed of the twentieth.”

11 Cf. Einstein [1944, p. 287], who refers to “the gulf—logically unbridgeable—which separates the world of
fines a proposition as falsifiable (and hence potentially belonging to the body of empirical knowledge) if and only if there is at least one observational proposition (or basic statement, in Popper's own terminology) with which it is logically inconsistent.

An observational proposition asserts that an observable event is occurring in a specified individual region of space and time. The requirements that the event be "observable" in a "specified individual" region are necessary to insure that observational propositions are themselves falsifiable

Whether a proposition is an observational proposition is partly a matter of fact. Whether certain events are observable may depend, for example, on the state of the art of scientific instrumentation and may thus change over time, whereas logical form is immutable.

Falsification must be distinguished from falsifiability. A proposition is falsified if and only if an observational proposition is accepted with which it is logically inconsistent. Falsification involves not only a logical element, the inconsistency of two propositions, but also a nonlogical element, the decision to accept the falsifying observational proposition. Logic alone cannot compel one to accept a falsifying observational proposition. It is the nonlogical element which enables theories to evade even falsification by experience unless methodological safeguards are adopted.

Thus, Popper [1959, p. 54] is led to lay down a "supreme rule... which says that the other rules of scientific procedure must be designed in such a way that they do not protect any statement in science against falsification." An important subsidiary rule is that a proposition should be accepted into the body of empirical knowledge only if it has been corroborated, meaning that it has survived serious attempts to refute it. Corroboration, like falsifiability and observability, is not a matter of strict logic; a proposition once corroborated may be falsified by later evidence. Thus the acceptance of a proposition into the body of empirical science is always tentative.

**Logical Form of Observational Propositions and Theoretical Propositions**

As noted above, an observational proposition asserts that an observable event is occurring in a specified individual region of space and time. The logical form of an observational proposition is exemplified by the paradigm:

There is an occurrence of the event $S$ in the region $K$.  

The region $K$ may also be interpreted as an event. It is, however, a singular event since it contains at most one occurrence. The event $S$, on the other hand, has no restriction on the number of occurrences it might have and so is called universal.

The joint event $AB$ is defined as the event that includes all those occurrences that are occurrences of both the event $A$ and the event $B$. Using this concept, we can rewrite (1) as:

There is an occurrence of the event $SK$.  

The event $SK$ contains at most one occurrence and is therefore singular.

The two preceding paragraphs may be summarized by saying that the logical form of an observational proposition is that of a singular existential proposition:

sensory experiences from the world of concepts and propositions."

12 Popper, whose concern is primarily with the physical sciences, uses only the method of spacetime coordination to individuate occurrences. In the biological and social sciences other methods of individuation may be required; e.g., the numerical tagging of biological specimens.

13 As stated at note 12, the singularity is necessary to make an observational sentence falsifiable.
singular, because it refers to a singular event; and existential, because it asserts that an occurrence of that event exists.

The complementary event $\bar{A}$ is defined as the event that includes all those occurrences that are not occurrences of $A$. As an example of the use of this concept, consider the proposition:

There is an occurrence of the event $SK$. \(3\)

This is a singular existential proposition and therefore an observational proposition. Moreover, its acceptance would falsify (2), since $SK$ and $\bar{SK}$ are logically incompatible events; if any event is occurring in the region $K$, it is either $S$ or $\bar{S}$, not both.

Next consider the proposition:

There is no occurrence of the event $S$. \(4\)

This proposition is neither singular (it refers only to the universal event $S$) nor existential (it denies, rather than asserts, the existence of occurrences of a certain kind). Yet it is falsifiable! \(^{14}\) It is falsified by the acceptance of (2), which asserts the existence of an occurrence of $S$ in a singular region. Therefore, according to Popper's proposal, (4) must be counted as an empirical proposition—although it certainly is not a statement of "what is."

Proposition (4) can be called a law since, like the laws passed by legislative bodies, it prohibits the occurrence of certain events. \(^{15}\) It is also the paradigm of a theoretical proposition. Thus, we can conclude that the logical form of a theoretical proposition, or law, is that of a strictly universal negative-existential proposition.

As shown in Figure 3, theoretical propositions, or laws, are distinguished from observational propositions in two ways. First, theoretical propositions are negative existential; observational propositions are positive existential. Second, theoretical propositions are strictly universal; observational propositions are singular. Observational propositions assert what is. Theoretical propositions assert what is not, anywhere in spacetime.

To have an empirical theory of a phenomenal domain, then, means to have:

(1) A collection of logically possible events, including some elementary events plus all the self-consistent compound events that can be constructed from the elementary ones by the operations of forming complements and intersections; and

(2) One or more empirical laws, each of which prohibits at least one observable event.

Thus, of the class of logically possible events recognized by a theory, the theory asserts that only members of a proper subclass are empirically possible.

\(^{14}\) It is not effectively verifiable, since all of spacetime would have to be searched to prove that there is no counterexample. Nor is it "confirmable" in the sense of being rendered more probable by favorable evidence. The essence of Hume's anti-inductivist argument is that no amount of favorable evidence to a proposition such as (4) can preclude the possibility of a counterexample.

\(^{15}\) "Not for nothing do we call the laws of nature 'laws,'" as Popper [1959, p. 41] says. "The more they prohibit the more they say."
The Universal Conditional Forms of Laws

Some empirical laws have, besides the negative existential form already discussed, two logically-equivalent alternate forms. Most methodologists have focused their attention on these alternate forms, or even on only one of them. This has been a source of a considerable amount of confusion about the relationship of laws to empirical evidence.¹⁶

The laws in question are those that prohibit the joint occurrence of two or more events. An example might be:

There is no occurrence of the event $RS$. (5)

This asserts that the events $R$ and $S$ never occur in conjunction with each other.

From the definition of the complementary event, it is obviously equivalent to assert:

All occurrences of the event $R$ are occurrences of the event $S$. (6)

To use a familiar example, “All men are mortal” is logically equivalent to “There is no immortal man.”

In modern mathematical logic, (6) is further analyzed into:

For all $x$: If $x$ is an occurrence of the event $R$ then $x$ is an occurrence of the event $S$. (7)

The logical form of (7) is that of a universal conditional proposition: universal because of the prefixed phrase “For all $x$” (called the “universal quantifier” in logic), and conditional because of the “if . . . then . . .” clause. The sentence following the “if,” “$x$ is an occurrence of the event $R$,” is called the antecedent of the conditional; and the sentence following the “then,” “$x$ is an occurrence of the event $S$,” is called its consequent.

Thus, every empirical law that prohibits the joint occurrence of two or more events can be stated in the form of a universal conditional proposition.¹⁷

By a kind of optical illusion, the universal conditional form has misled many methodologists into thinking that a law such as (7) has positive import and is “confirmed” by an observation of the event $RS$. (“The evidence is consistent with the theory,” they would say.) Hempel [1946] has pointed out that this view leads to a paradox.¹⁸ For, it can be seen that (5) is also logically equivalent to:

For all $x$: If $x$ is an occurrence of the event $S$ then $x$ is an occurrence of the event $R$. (8)

The logical form of (8) is called the contrapositive, but it is obviously just another version of the universal conditional form.

Here is the paradox: If observation of an instance of $RS$ “confirms” (7), then observation of an instance of $RS$ “confirms” (8) and therefore, by virtue of the logical equivalence, it also “confirms” (7). To be concrete, the law “All crows are black” is equally confirmed by observing a black crow and a nonblack caw.

¹⁶ For example, Nagel [1963, p. 215] has suggested that Friedman’s [1953, p. 141] argument that the “assumptions” of a theory need not be “realistic” is based on the erroneous notion that the antecedent of a universal conditional proposition is an assumption of a theory. Another example of a confusion, to be discussed shortly, is Hempel’s paradox of confirmation.

¹⁷ Zimmerman [1980, p. 108] says, “A positive theory is of the form, ‘If $A$ then $B$’, that is capable of being refuted.” This represents some progress over the statement by Watts and Zimmerman [1979, p. 273]: “We would prefer to reserve the term ‘theory’ . . . for sets of hypotheses which have been confirmed [sic].” Yet Zimmerman commits three technical errors in his short sentence: (1) he omits reference to universal quantification over a free variable, (2) he omits the restriction of the conditional form to those laws which prohibit a joint event, and (3) he calls the conditional “positive” when in fact it has only negative import.

¹⁸ Hempel’s “paradox of confirmation” is discussed by Ijiri [1975, pp. 165–167]. Since I consider Hempel’s paradox to be a reductio ad absurdum of the concept of confirmation, I do not agree with Ijiri’s analysis.
FIGURE 4
HOW SEVERAL KINDS OF INFORMATION ARE COMBINED IN PREDICTION
(Shaded areas represent events excluded in region K)

(a) Law  
(b) Initial Conditions  
(c) Prediction

noncrow—such as a green vase. In fact, almost anything one might observe is consistent with a law and therefore "confirms" it!

The paradox arises because the universal conditional forms (7) and (8) make it appear that a law says something positive, i.e., that it asserts that something is the case. It is then natural to think that positive evidence supports or "confirms" the law. The paradox disappears with the recognition that both (7) and (8) reduce to the canonical form (9), which does not assert anything positive. The law "There are no nonblack crows" is refuted by the observation of a nonblack crow, but it is not "confirmed" by anything [Quine, 1974].

III. PREDICTIVE, EXPLANATORY, AND NORMATIVE REASONING

The Rochester School demands that what they call a "positive" theory ought "to predict" [Watts and Zimmerman, 1979, p. 274], "to explain a set of phenomena" [Watts and Zimmerman, 1979, p. 273, n. 1], and to enable one "to determine if prescriptions from normative theories . . . are feasible" [Watts and Zimmerman, 1978, pp. 112–113].

I will now show how an empirical theory of the "negative" form discussed in the preceding section can be used in predictive, explanatory and normative reasoning. (Since exactly the same kind of theory is used in all three kinds of reasoning, I find it more appropriate to distinguish kinds of reasoning rather than kinds of theories.)

Predictive Reasoning

A prediction is, for present purposes, simply one or more observational propositions that refer to as yet unobserved occurrences, whether these occurrences are in the future or in the past.

Since a theory states only what is empirically possible, not what is actually the case, in general no positive prediction can be derived from a theory alone. Suppose, for example, that we are given a theory consisting of a single law, "There is no occurrence of the event $R \bar{S}$." From this law we can deduce "negative" predictions of the form, "The event $R \bar{S}$ is not occurring in the spacetime region $K$." Except for $R \bar{S}$, however, any event might be occurring in region $K$. Each of the atomic events $R$, $\bar{R}$, $S$, and $\bar{S}$ is empirically possible, for example, as well as all the joint events that can be constructed from them except $R \bar{S}$ (see Figure 4(a)).

A definite prediction can be derived with the right kind of additional information. Suppose we are given the observa-
tional proposition, "The event $R$ is occurring in the spacetime region $K."$ As shown in Figure 4(b), this sentence rules out the occurrence in that region of the event $\overline{R}.$ Combining this information with that contained in the law, we can conclude (see Figure 4(c)) that all possibilities are excluded except $RS,$ the joint occurrence of $R$ and $S.$ Thus, we can derive the prediction, "The event $S$ is occurring in the spacetime region $K."$

An observational proposition used in deriving a prediction is called a statement of initial conditions [Popper, 1959, p. 59]. Thus, in order to deduce a positive prediction, one needs in general both an empirical theory (one or more laws) and one or more statements of initial conditions. In terms of Caws's characterization of a theory (quoted at the beginning of

Explanatory Reasoning

From a purely logical perspective, the explanation of a singular occurrence is the mirror image of prediction. In the case of prediction, we are given an empirical theory and some inputs (statements of initial conditions): we seek to derive a proposition describing an as-yet-unobserved occurrence. In the case of explanation, we are given an observed occurrence; we seek both an explanatory theory and some statements of initial conditions (the explicans) from which we can derive a sentence (the explicandum) describing the observed occurrence. This process may be represented by the following schema:

\[
\begin{align*}
\text{To Find:} & \quad \{L_1, L_2, \ldots, L_n \} \quad \{C_1, C_2, \ldots, C_m \} \\
\text{Given:} & \quad \because E
\end{align*}
\]

In some cases, characteristic of what Kuhn [1970] calls "normal science," we may already have a set of well-corroborated laws, so that the process of finding an explanation reduces to finding a set of initial conditions. Where we do not have such laws but must discover them, finding an appropriate description of the explicandum becomes a necessary part of the process of explaining it. Since, in a scientific system, occurrences are described in terms of events, finding an appropriate description of the explicandum means finding an appropriate event structure (or "conceptual framework") for the phenomenal domain.
Normative Reasoning

Empirical theories can be used as guides to action in two different ways, corresponding respectively to predictive and explanatory reasoning. To distinguish the two ways, Popper [1957, p. 43] has called the predictions yielded by predictive reasoning prophecies and has pointed out that predictions of a completely different kind, which he calls technological predictions, can be derived by a process analogous to explanatory reasoning. In the case of a prophecy, according to Popper, "we are told about an event which we can do nothing to prevent. Its practical value lies in one being warned of the predicted event, so that we can side-step it or meet it prepared." Technological predictions, in contrast, "form a basis of engineering. They are, so to speak, constructive, intimating the steps open to us if we want to achieve certain results." I will refer to the derivation of technological predictions as "normative reasoning."

As an example of a technological prediction, one can calculate with the aid of Newton's theory of gravitation the force that would have to be applied to a given mass on the earth's surface in order to place it in orbit. It is interesting that Newton himself anticipated this technological application of his theory [Newton, 1729, p. 551]. Of course, a test of this particular prediction was not technologically feasible for nearly a quarter of a millennium! But it could not be derived at all from earlier theories which lacked the dynamical concepts of mass and force.

Two points may be observed from this example. First, technological predictions require the explanatory concepts of the theory ("mass" and "force" in the example). Second, they require that at least some of these explanatory concepts correspond to controllable events, since it is through manipulation of these events that the desired outcome is obtained.

Normative reasoning, the derivation of technological predictions, follows this schema: 19

<table>
<thead>
<tr>
<th>To Find:</th>
<th>( {L_1, L_2, \ldots, L_n} ) (Empirical Laws)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Given: ( {C_1, C_2, \ldots, C_m} ) (Controllable Initial Conditions)</td>
<td>( \therefore S ) (Desired Final State)</td>
</tr>
</tbody>
</table>

When there is a well-established empirical science applying to his problem, the technological predictor may find the relevant laws within that body of knowledge rather than having to develop them from scratch. The modern mechanical engineer, for example, applies the laws of the science of mechanics. Before there is such a science, however, the practitioner faces exactly the same logical problem as a scientist searching for an explanatory theory [Christenson, 1976].

Analysis and Synthesis

Note that there is a close formal resemblance between the schema for normative reasoning and that for explanatory reasoning. In each case we are given the conclusion of a deductive argument, and we seek to find the premises.

19 As Simon [1965] points out, the initial conditions and the description of the desired final state in this schema will not be observational propositions but rather what he calls commands and what I have earlier called proposals. With a couple of restrictions, however, these commands can be treated for logical purposes as if they were observational propositions. The following analogy may be enlightening. In a linear programming problem, the constraint set and the objective function usually define what is empirically possible, and thus correspond to the set of empirical laws. The command "maximize the objective function" characterizes the desired final state. The values of the variables appearing in the constraints and the objective function are the controllable initial conditions.
from which it can be deduced. In other words, we reason in the reverse of the deductive direction. There is this difference: In explanatory reasoning, we accept the explicandum as true on the basis of observation; we terminate reverse reasoning when we have found laws and initial conditions that we likewise accept as true on the basis of observation. In normative reasoning, we desire that the description of a certain final state be true; we terminate reverse reasoning when we have found laws that we accept as true and controllable initial conditions that we can make true by our actions.20

In ancient Greek geometry, forward or deductive reasoning, which was used in "proofs," was known as the "method of synthesis," since in it a number of conditions are combined or synthesized to produce a single result. Reverse reasoning, which seeks to discover the necessary conditions, was known as the "method of analysis." The two methods are discussed by Polya [1945, pp. 141–154]. See also Hintikka and Remes [1974, Chapter IX].

IV. THE APPRAISAL OF THEORIES

My purpose in this section is not to appraise the theories of the Rochester School but rather to criticize the standards by which they think a theory ought to be appraised, that is to say, their methodology. It is not possible, of course, to draw a clean line between these two purposes. The essence of my criticism of the Rochester School's methodology is that they use it to defend their theories against what would be appropriate criticism.

Instrumentalism vs. Realism

In Section III it was noted that, from a purely logical point of view, explanation is prediction in reverse. There are two divergent methodological interpretations of this state of affairs, and they lead to different conclusions regarding the appraisal of theories.

According to one methodological view, called instrumentalism, explanation is nothing but prediction in reverse, or, to put it a little differently, a theory is nothing but an instrument for prediction. The credo of instrumentalism has been characterized as follows: In explanatory reasoning, both the statements of initial conditions and the explicandum describe aspects of reality, to-wit, occurrences in the phenomenal domain. In contrast, the theory itself does not describe any aspect of reality. Reality, according to the instrumentalist, consists of nothing but occurrences of events [Popper, 1965, p. 108].

Instrumentalism, it may be noted, is a slightly liberalized version of positivism. Positivism awards scientific status only to statements of "what is." Instrumentalism differs from strict positivism in admitting that theories, although they cannot be reduced to statements of "what is," are nevertheless needed in science. It claims, however, that their utility is only as instruments for prediction, and not as descriptions of reality. Positivism and instrumentalism both agree that only observational propositions describe reality.

An alternative methodological view, called realism, agrees that explanation is (logically speaking) prediction in reverse and that, therefore, a theory that explains can also be used as an instrument for prediction. Realism also admits that some theories may be nothing but instruments for prediction. Realism holds, however, that for a theory to be considered explanatory, it must be more than an instrument for prediction. It must also

20 Reverse reasoning is, of course, the method used in reasoning from a product to the process that produces it, as discussed in Section III of this article.
be interpretable as a description of a deeper reality that underlies the surface reality of the phenomenal domain of occurrences.

A theory known to be false can still yield predictions that are highly accurate, or at least sufficiently accurate for many practical purposes. Newton’s gravitational theory, and even the Ptolemaic theory, are still adequate for many astronomical predictions. On the other hand, an explanation that is false is no explanation at all. An explanatory theory is supposed to answer the question “why,” and a false answer to the question is certainly unsatisfactory. For this reason, Einstein’s theory has replaced Newton’s as an explanation. An explanatory theory, in short, ought to be true (or at least not known to be false), a merely predictive theory need not be true (Popper, 1972, p. 192).

Although the Rochester School claims to base its positivist methodology on Friedman [1953], they diverge from him on the issue of instrumentalism vs. realism. Friedman puts great stress on the predictive function of theory and downplays the explanatory function. By my count, he uses the words “explain” or “explanation” only four times in a 40-page essay. Each time, he encloses the word in quotation marks (as if to signify he is using an illegitimate concept) and makes clear that what he really means by the terms is correct prediction.

Friedman’s position is avowedly instrumentalist [Boland, 1979]. He says, for example, that “the only [my emphasis] relevant test of the validity [his emphasis] of a hypothesis is comparison of its predictions with experience” [1953, pp. 8–9]. It is notable that, in addition to insisting that its instrumental value is the only relevant test of a theory, Friedman also speaks of the validity of a theory rather than its truth. Friedman goes so far as to say [1953, p. 14] that “truly important and significant hypotheses will be found to have ‘assumptions’ that are wildly inaccurate descriptive representations of reality [i.e., are false], and, in general, the more significant the theory, the more unrealistic the assumptions (in this sense).”

In my opinion, instrumentalism is a mistaken philosophy of science. It results, I think, from confusing an empirical law, in its universal conditional form, with a rule of logical inference. Rules of inference may be said to be valid or invalid, but empirical laws are true or false. Also, instrumentalism has the effect (and, in Friedman’s case, the intent) of shielding some of the propositions of a theoretical system from falsification. As I indicated in Section III, this robs these propositions of any claim to belong to empirical science. (Perhaps that’s why Friedman prefers the term “positive” science.”) Finally, instrumentalism is incompatible with the method of analysis since, in that method, we terminate reverse reasoning only when we have discovered premises that are both sufficient to predict observed occurrences and are “already known or admittedly true,” or at least not known to be false. For essentially the same reason, theories that have only an instrumental interpretation are useless in normative reasoning as described in Section

21 Friedman’s instrumentalism may be contrasted with the realism of Galileo [1967, p. 341], who said approvingly that Copernicus “very well understood that although the celestial appearances might be saved by means of assumptions essentially false in nature, it would be very much better if he could derive them from true suppositions.” Galileo could have avoided trial by the Inquisition by accepting Cardinal Bellarmino’s suggestion that he adopt an instrumentalist interpretation of the Copernican Theory.

22 Popper himself acknowledges [1959, p. 76] that at the time he wrote the original German version of his book he was confused about the distinction between a conditional proposition and a rule of logical inference.
III: only theories that are at least a good approximation to the truth are acceptable for this purpose.

The Rochester School, however, is immune to any criticism of instrumentalism, since it abandons (perhaps unself-consciously) that part of Friedman’s methodology. It places the greater stress on theories that describe and explain, and it mentions prediction relatively infrequently, usually in a context where the testing of a theory through its predictions rather than its instrumental use is the issue. Jensen’s list of “positive” questions (Figure 1) illustrates this emphasis on explanation rather than prediction. And Watts and Zimmerman [1979, p. 274] proudly claim that their theory not only “predicts that accounting theory will be used to ‘buttress preconceived notions’” but also that “it explains why.”

If someone questions the realism of part of Friedman’s theories, he can fall back behind his instrumentalist ramparts and argue that the criticism is irrelevant; the theories with which he is concerned do not purport to explain but only to predict, and so long as they predict correctly they are adequate. The same defense is not available to the Rochester School. The Rochester School’s theories must either be realistic or they must be rejected as explanations.

Appraisal of an Explanatory Theory

The Rochester School appears to believe that the only way one can test the truth of a theory is to derive predictions from it. They also appear to believe that correct predictions make a theory more acceptable. [Watts and Zimmerman, 1978, p. 125; 1979, p. 283; Zimmerman, 1980, p. 122]. (As a matter of fact, as I will show later, Watts and Zimmerman claim that their theories should be accepted even though predictions derived from them are false.)

Popper [1972, p. 353], however, calls the notion that an explicans is corroborated23 by drawing correct predictions from it “incorrect and grossly misleading.” He points out that “A true prediction may easily have been validly deduced from an explicans that is false.” As we have seen in Section II, a theory as a collection of laws has no positive import, and almost all observations will be consistent with (i.e., will not contradict) even a false theory. Therefore, we cannot infer the truth of a theory from the truth of predictions drawn from it. In short, the Rochester School’s notion that an explanatory theory is made acceptable merely by successful prediction is logically fallacious.

On the other hand, a false conclusion to an argument does entail the falsity of its premises. If, therefore, a prediction derived from an explanatory argument turns out to be false, we can be sure that the explicans is also false. “This means,” as Popper [1972, p. 353] says, “that a prediction can be used to corroborate a theory only if its comparison with observations might be regarded as a serious attempt at testing the explicans—a serious attempt at refuting it. A ‘risky’ prediction of this kind may be called ‘relevant to a test of the theory.’”

Since an explicans consists of at least two premises—a law and a statement of initial conditions—the fact that the falsity of the conclusion implies the falsity of the explicans does not determine how this falsity is to be distributed over the terms of the explicans. That is, the falsity of the explicans may mean that the law(s) are true and the initial conditions false; that the law(s) are false and the

23 As indicated in Section II above, Popper considers a theory to be corroborated only if it has survived serious attempts to refute it. His reasons for preferring the term “corroboration” to the term “confirmation” are given in [1959, pp. 251–252, n.*1].
initial conditions true; or that both are false.

The fact that a prediction permits a test only of the explicans as a whole leads to the methodological norm that an explicans should be considered satisfactory prior to testing only if its terms are independently testable. That is to say, if a prediction intended as a test of an explicans fails, we can determine which of the premises is (are) responsible for the failure only if we can test each of them independently of the others.

But, as Zimmerman [1980, p. 122] has acknowledged, he and Watts in their several articles have made “simplifying assumptions regarding (unobservable) relative costs,” assumptions that at times “appear arbitrary and ex post.” Zimmerman attempts to excuse the use of untestable assumptions by saying that “if they yield testable implications that are consistent with the evidence in replications, then progress towards a positive theory of accounting is achieved.” This flies in the face of the logical fact that the truth of an explicandum does not entail the truth of its explicans.

There is a way in which a theory can be tested without introducing initial conditions. Although initial conditions are required to make a positive prediction, a theory negatively predicts, without initial conditions, that certain logically possible events will not occur. If one is seriously interested in testing a theory, then, rather than an entire explicans, one could specify what these prohibited events are and where, in the light perhaps of background knowledge, they would be most likely to occur; and one would then do one’s damndest to find these prohibited occurrences. That would be the kind of severe test Popper calls “relevant.” The search for correct predictions engaged in by the Rochester School is simply not “relevant.”

The Issue of “Exceptions”

The fact that the Rochester School only weakly tests its theories becomes less significant, perhaps, when we observe that their theories fail even these weak tests. That is, their theories yield false predictions, which are then excused away.

Watts and Zimmerman [1978, p. 118, n. 24] tell us, for example, that their theory of corporate lobbying on accounting standards “is developed for firms in the same industry that only differ by size.” In testing the theory, therefore, a control by industry should clearly be used. Instead, Watts and Zimmerman pool their data across industries. This tends to obscure the fact that the two firms in the retail industry behave in exactly the opposite way to that predicted. We also observe a contrary-to-prediction rank reversal in the responses of the oil industry. Finally, as the authors themselves note, at least three firms who were predicted to submit to the FASB did not submit.

Since this article is a critique of the Rochester School’s methodology rather than of their theories, my concern is less with the fact that the Rochester School’s “explanations” were falsified at the first attempt to test them, than with the rationalizations used by Watts and Zimmerman to “explain” these falsifications. We are told, for example, that “we can only expect a positive theory to hold on average” [Watts and Zimmerman, 1978, p. 127, n. 37]. We are also advised “to remember that as in all empirical theories we are concerned with general trends” [Watts and Zimmerman, 1979, pp. 288–289], where “general” is used in the weak

24 The three firms are identified at one point as IBM, U. S. Steel, and Chrysler; later in the same paragraph two of the three are identified as IBM and General Telephone. No explanation is given of this obvious inconsistency [Watts and Zimmerman, 1978, p. 127].
sense of “true or applicable in most instances but not all” rather than in the strong sense of “relating to, concerned with, or applicable to every member of a class” [American Heritage Dictionary, 1969, p. 548]. In other words, we are told without any evidence or argument that since all empirical theories admit exceptions we needn’t worry about the exceptions to the Rochester School’s theory.

In responding to this argument, I will again follow Jensen’s advice and consult Pareto, who tells us:

A ‘law’ that has exceptions, that is to say, a uniformity which is not uniform, is an expression devoid of meaning.... If one grants to a person who is stating a law that his law may have its exceptions, he can always meet every fact that is adduced against him with the excuse that it is an “exception,” and he will never be caught in the wrong. And that is exactly what literary economists, moralists, and metaphysicists do [Pareto, 1935, Section 1689, no. 31.

And that, of course, is also exactly what Watts and Zimmerman do!

If astronomers were interested only in general trends, the assertion that the heavenly bodies move uniformly from east to west would be a satisfactory theory, since only a handful of pesky bodies (planets and comets) behave exceptionally. Almost the entire history of astronomy, however, consists of attempts to explain these exceptions rather than to excuse them.25 Even the Ptolemaic astronomers had the good sense to add another epicycle or equant when needed!

Pareto concluded the statement quoted above by saying, “A law [that admits exceptions] has no significance, and knowledge of it is not of the slightest use.” By arguing that their theories admit exceptions, Watts and Zimmerman condemn them as insignificant and useless. I agree.

Watts and Zimmerman [1979, p. 288] also attempt to deal with the problem of exceptions by drawing an irrelevant analogy with the toss of a coin: They say that “one or two ‘heads’ is not sufficient to reject the hypothesis that a given coin is ‘fair’.” The analogy is irrelevant because, in the case of a fair coin, we are dealing with a stochastic law, which asserts that events occur with specifically stated numerical probabilities derived from symmetry considerations. There is nothing remotely comparable to such a law in the theories of the Rochester School.26

As Blaug [1976, p. 173] has written, “Much empirical work in economics is like ‘playing tennis with the net down’: instead of attempting to refute testable predictions, economists spend much of their time showing that the real world bears out their predictions, thus replacing falsification, which is difficult, with confirmation, which is easy.” The work of the Rochester School is certainly no exception to this “general trend.”

VI. SUMMARY AND CONCLUSION

The main criticisms of the methodology of Positive Accounting advanced in this article are the following:

1. The Rochester School’s assertion that the kind of “positive” research they are undertaking is a prerequisite for normative accounting theory is based on a confusion of phenomenal domains at two different levels (accounting entities vs. accountants), and is mistaken.

2. The concept of “‘positive’ theory” is drawn from an obsolete philosophy of science and is in any case a misnomer, because the theories of

26 Because their theories are not formulated in stochastic terms, I have not discussed in this article the nature of stochastic laws and the particular problems involved in testing them. Popper [1959, Chapter VIII] discusses these problems in detail.
empirical science make no positive statement of “what is.”

3. Although a theory may be used merely for prediction even if it is known to be false, an explanatory theory of the type sought by the Rochester School, or one that is to be used to test normative proposals, ought not to be known to be false. The method of analysis, which reasons backward from the phenomena to premises which are acceptable on the basis of independent evidence, is the appropriate method for constructing explanatory theories.

4. Contrary to the empirical method of subjecting theories to severe attempts to falsify them, the Rochester School introduces ad hoc arguments to excuse the failures of their theories. This tactic is a violation of the norms which, according to Popper [1959, Section 20], must be followed if a system of propositions is to be considered “scientific.”

Of course, Watts and Zimmerman [1979, p. 290] do say, “We do not contend that all issues are settled, but rather encourage others to pursue, correct, and extend our analysis.”

This encouragement is a trap for the unwary. As the authors point out earlier in the same article [p. 286], “Researchers have non-pecuniary incentives to be well-known, and this reputation is rewarded by a higher salary and a plenitude of research funds.” A footnote adds that “Higher compensation will accrue to the most prolific, articulate, and creative advocates—to those who are able to establish early property rights in a topic and thus must be cited by later theorists” [p. 286, n. 46].

In other words, the Rochester School is inviting other researchers to repair the deficiencies in their program, while acknowledging that they will be the main beneficiaries of this activity.

The Rochester School ought to put its own program in order before it asks others to take that program seriously. A useful first step would be for them to stick with one set of phenomena until they have understood it well and satisfactorily explained it, rather than leaping to a different phenomenal domain in each new article in an effort to establish squatters’ rights. They should follow the advice of Newton, who said (quoted by Guerlac [1973, p. 385]):

> But if without deriving the properties of things from Phaenomena you feign Hypotheses and think by them to explain all nature you may make a plausible systeme of Philosophy for getting your self a name, but your systeme will be little better than a Romance. To explain all nature is too difficult a task for any one man or even for any one age. Tis much better to do a little with certainty and leave the rest for others that come after you than to explain all things by conjecture without making sure of any thing.

Newton himself set an appropriate example, refraining from publishing his lunar theory for nearly twenty years while he attempted to resolve discrepancies between its predictions and observational data. As Lakatos [1978, vol. i, p. 216] observes, “The first dozen [versions of Newton’s theory] ended up in Newton’s wastepaper-basket.” In a footnote, Lakatos explains:

> ‘Wastepaper-baskets’ were containers used in the seventeenth century for the disposal of some first versions of manuscripts which self-criticism—or private criticism of learned friends—ruled out on the first reading. In our age of publication explosion most people have

27 Watts and Zimmerman [1978], for example, deals with the behavior of practicing financial accountants and managers; Watts and Zimmerman [1979], with that of academic accountants; and Zimmerman [1979], with that of cost accountants.
Christenson

no time to read their manuscripts, and the function of wastepaper-baskets has now been taken over by scientific journals.

The old ways still have much to recommend them.

REFERENCES


Newton, Isaac (1729), *Principia* (University of California Press, 1934; reprinted from the English translation of 1729).

Polya, George (1945), How to Solve It (Princeton University Press, 1945).
Russell, Bertrand (1946), A History of Western Philosophy (Simon & Shuster, 1946).
The Methodology of Positive Accounting
Charles Christenson
Stable URL: http://links.jstor.org/sici?sici=0001-4826%28198301%2958%3A1%3C1%3ATMOPA%3E2.0.CO%3B2-Z

This article references the following linked citations. If you are trying to access articles from an off-campus location, you may be required to first logon via your library web site to access JSTOR. Please visit your library's website or contact a librarian to learn about options for remote access to JSTOR.

[Footnotes]

16 Introductory Remarks
Fritz Machlup
Stable URL: http://links.jstor.org/sici?sici=0002-8282%28196305%2953%3A2%3C204%3AIR%3E2.0.CO%3B2-A

19 The Logic of Rational Decision
Herbert A. Simon
Stable URL: http://links.jstor.org/sici?sici=0007-0882%28196511%2916%3A63%3C169%3ATLORD%3E2.0.CO%3B2-I

References

A Critique of Friedman's Critics
Lawrence A. Boland
Stable URL: http://links.jstor.org/sici?sici=0022-0515%28197906%2917%3A2%3C503%3AACOF%3E2.0.CO%3B2-H

NOTE: The reference numbering from the original has been maintained in this citation list.
A Note on the Paradoxes of Confirmation
Carl G. Hempel
Stable URL: http://links.jstor.org/sici?sici=0026-4423%28194601%292%3A55%3A217%3C79%3AANOTPO%3E2.0.CO%3B2-0

Introductory Remarks
Fritz Machlup
Stable URL: http://links.jstor.org/sici?sici=0002-8282%28196305%2953%3A2%3C204%3AIR%3E2.0.CO%3B2-A

Assumptions in Economic Theory
Ernest Nagel
Stable URL: http://links.jstor.org/sici?sici=0002-8282%28196305%2953%3A2%3C211%3AATET%3E2.0.CO%3B2-I

The Logic of Rational Decision
Herbert A. Simon
Stable URL: http://links.jstor.org/sici?sici=0007-0882%28196511%2916%3A63%3C169%3ATLORD%3E2.0.CO%3B2-I

NOTE: The reference numbering from the original has been maintained in this citation list.