

reason alone they are apt to seem to support those theories in the light of which they were interpreted. But real support can be obtained only from observations undertaken as tests (by 'attempted refutations'); and for this purpose *criteria of refutation* have to be laid down beforehand: it must be agreed which observable situations, if actually observed, mean that the theory is refuted. But what kind of clinical responses would refute to the satisfaction of the analyst not merely a particular analytic diagnosis but psycho-analysis itself? And have such criteria ever been discussed or agreed upon by analysts? Is there not, on the contrary, a whole family of analytic concepts, such as 'ambivalence' (I do not suggest that there is no such thing as ambivalence), which would make it difficult, if not impossible, to agree upon such criteria? Moreover, how much headway has been made in investigating the question of the extent to which the (conscious or unconscious) expectations and theories held by the analyst influence the 'clinical responses' of the patient? (To say nothing about the conscious attempts to influence the patient by proposing interpretations to him, etc.) Years ago I introduced the term '*Oedipus effect*' to describe the influence of a theory or expectation or prediction upon the event which it predicts or describes: it will be remembered that the causal chain leading to Oedipus' parricide was started by the oracle's prediction

of this event. This is a characteristic and recurrent theme of such myths, but one which seems to have failed to attract the interest of the analysts, perhaps not accidentally. (The problem of confirmatory dreams suggested by the analyst is discussed by Freud, for example in *Gesammelte Schriften*, III, 1925, where he says on p. 315: 'If anybody asserts that most of the dreams which can be utilized in an analysis . . . owe their origin to [the analyst's] suggestion, then no objection can be made from the point of view of analytic theory. Yet there is nothing in this fact', he surprisingly adds, 'which would detract from the reliability of our results.')

4. The case of astrology, nowadays a typical pseudoscience, may illustrate this point. It was attacked, by Aristotelians and other rationalists, down to Newton's day, for the wrong reason—for its now accepted assertion that the planets had an 'influence' upon terrestrial ('sublunar') events. In fact Newton's theory of gravity, and especially the lunar theory of the tides, was historically speaking an offspring of astrological lore. Newton, it seems, was most reluctant to adopt a theory which came from the same stable as for example the theory that 'influenza' epidemics are due to an astral 'influence'. And Galileo, no doubt for the same reason, actually rejected the lunar theory of the tides; and his misgivings about Kepler may easily be explained by his misgivings about astrology.



3

THOMAS S. KUHN

Logic of Discovery or Psychology of Research?

My object in these pages is to juxtapose the view of scientific development outlined in my book, *The Structure of Scientific Revolutions*, with the better-

known views of our chairman, Sir Karl Popper.¹ Ordinarily I should decline such an undertaking, for I am not so sanguine as Sir Karl about the utility of confrontations. Besides, I have admired his work for too long to turn critic easily at this date. Nevertheless, I am persuaded that for this occasion the attempt must be made. Even before my book was

Criticism and the Growth of Knowledge, Imre Lakatos and Alan Musgrave, eds. (New York: Cambridge University Press, 1970), pp. 1-23. Reprinted with permission of the publisher.

published two and a half years ago, I had begun to discover special and often puzzling characteristics of the relation between my views and his. That relation and the divergent reactions I have encountered to it suggest that a disciplined comparison of the two may produce peculiar enlightenment. Let me say why I think this could occur.

On almost all the occasions when we turn explicitly to the same problems, Sir Karl's view of science and my own are very nearly identical.² We are both concerned with the dynamic process by which scientific knowledge is acquired rather than with the logical structure of the products of scientific research. Given that concern, both of us emphasize, as legitimate data, the facts and also the spirit of actual scientific life, and both of us turn often to history to find them. From this pool of shared data, we draw many of the same conclusions. Both of us reject the view that science progresses by accretion; both emphasize instead the revolutionary process by which an older theory is rejected and replaced by an incompatible new one³; and both deeply underscore the role played in this process by the older theory's occasional failure to meet challenges posed by logic, experiment, or observation. Finally, Sir Karl and I are united in opposition to a number of classical positivism's most characteristic theses. We both emphasize, for example, the intimate and inevitable entanglement of scientific observation with scientific theory; we are correspondingly skeptical of efforts to produce any neutral observation language; and we both insist that scientists may properly aim to invent theories that *explain* observed phenomena and that do so in terms of *real* objects, whatever the latter phrase may mean.

That list, though it by no means exhausts the issues about which Sir Karl and I agree,⁴ is already extensive enough to place us in the same minority among contemporary philosophers of science. Presumably that is why Sir Karl's followers have with some regularity provided my most sympathetic philosophical audience, one for which I continue to be grateful. But my gratitude is not unmixed. The same agreement that evokes the sympathy of this group too often misdirects its interest. Apparently Sir Karl's followers can often read much of my book as chapters from a late (and, for some, a drastic) revision of his classic, *The Logic of Scientific Discovery*.

One of them asks whether the view of science outlined in my *Scientific Revolutions* has not long been common knowledge. A second, more charitably, isolates my originality as the demonstration that discoveries-of-fact have a life cycle very like that displayed by innovations-of-theory. Still others express general pleasure in the book but will discuss only the two comparatively secondary issues about which my disagreement with Sir Karl is most nearly explicit: my emphasis on the importance of deep commitment to tradition and my discontent with the implications of the term 'falsification'. All these men, in short, read my book through a quite special pair of spectacles, and there is another way to read it. The view through those spectacles is not wrong—my agreement with Sir Karl is real and substantial. Yet readers outside of the Popperian circle almost invariably fail even to notice that the agreement exists, and it is these readers who most often recognize (not necessarily with sympathy) what seem to me the central issues. I conclude that a gestalt switch divides readers of my book into two or more groups. What one of these sees as striking parallelism is virtually invisible to the others. The desire to understand how this can be so motivates the present comparison of my view with Sir Karl's.

The comparison must not, however, be a mere point by point juxtaposition. What demands attention is not so much the peripheral area in which our occasional secondary disagreements are to be isolated but the central region in which we appear to agree. Sir Karl and I do appeal to the same data; to an uncommon extent we are seeing the same lines on the same paper; asked about those lines and those data, we often give virtually identical responses, or at least responses that inevitably seem identical in the isolation enforced by the question-and-answer mode. Nevertheless, experiences like those mentioned above convince me that our intentions are often quite different when we say the same things. Though the lines are the same, the figures which emerge from them are not. That is why I call what separates us a gestalt switch rather than a disagreement and also why I am at once perplexed and intrigued about how best to explore the separation. How am I to persuade Sir Karl, who knows everything I know about scientific development and who has somewhere or other said it, that what he calls a

duck can be seen as a rabbit? How am I to show him what it would be like to wear my spectacles when he has already learned to look at everything I can point to through his own?

In this situation a change in strategy is called for, and the following suggests itself. Reading over once more a number of Sir Karl's principal books and essays, I encounter again a series of recurrent phrases which, though I understand them and do not quite disagree, are locutions that I could never have used in the same places. Undoubtedly they are most often intended as metaphors applied rhetorically to situations for which Sir Karl has elsewhere provided unexceptionable descriptions. Nevertheless, for present purposes these metaphors, which strike me as patently inappropriate, may prove more useful than straightforward descriptions. They may, that is, be symptomatic of contextual differences that a careful literal expression hides. If that is so, then these locutions may function not as the lines-on-paper but as the rabbit-ear, the shawl, or the ribbon-at-the-throat which one isolates when teaching a friend to transform his way of seeing a gestalt diagram. That, at least, is my hope for them. I have four such differences of locutions in mind and shall treat them *seriatim*.

Among the most fundamental issues on which Sir Karl and I agree is our insistence that an analysis of the development of scientific knowledge must take account of the way science has actually been practiced. That being so, a few of his recurrent generalizations startle me. One of these provides the opening sentences of the first chapter of the *Logic of Scientific Discovery*: 'A scientist', writes Sir Karl, 'whether theorist or experimenter, puts forward statements, or systems of statements, and tests them step by step. In the field of the empirical sciences, more particularly, he constructs hypotheses, or systems of theories, and tests them against experience by observation and experiment.'⁵ The statement is virtually a cliché, yet in application it presents three problems. It is ambiguous in its failure to specify which of two sorts of 'statements' or 'theories' are being tested. That ambiguity can, it is true, be eliminated by reference to other passages in Sir Karl's writings, but the generalization that results is historically mistaken. Furthermore, the mistake proves important, for the unambiguous form of the description misses just that characteristic of scientific

practice which most nearly distinguishes the sciences from other creative pursuits.

There is one sort of 'statement' or 'hypothesis' that scientists do repeatedly subject to systematic test. I have in mind statements of an individual's best guesses about the proper way to connect his own research problem with the corpus of accepted scientific knowledge. He may, for example, conjecture that a given chemical unknown contains the salt of a rare earth, that the obesity of his experimental rats is due to a specified component in their diet, or that a newly discovered spectral pattern is to be understood as an effect of nuclear spin. In each case, the next steps in his research are intended to try out or test the conjecture or hypothesis. If it passes enough or stringent enough tests, the scientist has made a discovery or has at least resolved the puzzle he had been set. If not, he must either abandon the puzzle entirely or attempt to solve it with the aid of some other hypothesis. Many research problems, though by no means all, take this form. Tests of this sort are a standard component of what I have elsewhere labeled 'normal science' or 'normal research', an enterprise which accounts for the overwhelming majority of the work done in basic science. In no usual sense, however, are such tests directed to current theory. On the contrary, when engaged with a normal research problem, the scientist must *premise* current theory as the rules of his game. His object is to solve a puzzle, preferably one at which others have failed, and current theory is required to define that puzzle and to guarantee that, given sufficient brilliance, it can be solved.⁶ Of course the practitioner of such an enterprise must often test the conjectural puzzle solution that his ingenuity suggests. But only his personal conjecture is tested. If it fails the test, only his own ability, not the corpus of current science, is impugned. In short, though tests occur frequently in normal science, these tests are of a peculiar sort, for in the final analysis it is the individual scientist rather than current theory which is tested.

This is not, however, the sort of test Sir Karl has in mind. He is above all concerned with the procedures through which science grows, and he is convinced that 'growth' occurs not primarily by accretion but by the revolutionary overthrow of an accepted theory and its replacement by a better one.⁷ (The subsumption under 'growth' of 'repeated overthrow'

is itself a linguistic oddity whose *raison d'être* may become more visible as we proceed.) Taking this view, the tests which Sir Karl emphasizes are those which were performed to explore the limitations of accepted theory or to subject a current theory to maximum strain. Among his favorite examples, all of them startling and destructive in their outcome, are Lavoisier's experiments on calcination, the eclipse expedition of 1919, and the recent experiments on parity conservation.⁸ All, of course, are classic tests, but in using them to characterize scientific activity Sir Karl misses something terribly important about them. Episodes like these are very rare in the development of science. When they occur, they are generally called forth either by a prior crisis in the relevant field (Lavoisier's experiments or Lee and Yang's⁹) or by the existence of a theory which competes with the existing canons of research (Einstein's general relativity). These are, however, aspects of or occasions for what I have elsewhere called 'extraordinary research', an enterprise in which scientists do display very many of the characteristics Sir Karl emphasizes, but one which, at least in the past, has arisen only intermittently and under quite special circumstances in any scientific speciality.¹⁰

I suggest then that Sir Karl has characterized the entire scientific enterprise in terms that apply only to its occasional revolutionary parts. His emphasis is natural and common: the exploits of a Copernicus or Einstein make better reading than those of a Brahe or Lorentz; Sir Karl would not be the first if he mistook what I call normal science for an intrinsically uninteresting enterprise. Nevertheless, neither science nor the development of knowledge is likely to be understood if research is viewed exclusively through the revolutions it occasionally produces. For example, though testing of basic commitments occurs only in extraordinary science, it is normal science that discloses both the points to test and the manner of testing. Or again, it is for the normal, not the extraordinary practice of science that professionals are trained; if they are nevertheless eminently successful in displacing and replacing the theories on which normal practice depends, that is an oddity which must be explained. Finally, and this is for now my main point, a careful look at the scientific enterprise suggests that it is normal science, in which Sir Karl's sort of testing does not occur, rather than extraordi-

nary science which most nearly distinguishes science from other enterprises. If a demarcation criterion exists (we must not, I think, seek a sharp or decisive one), it may lie just in that part of science which Sir Karl ignores.

In one of his most evocative essays, Sir Karl traces the origin of 'the tradition of critical discussion [which] represents the only practicable way of expanding our knowledge' to the Greek philosophers between Thales and Plato, the men who, as he sees it, encouraged critical discussion both between schools and within individual schools.¹¹ The accompanying description of Presocratic discourse is most apt, but what is described does not at all resemble science. Rather it is the tradition of claims, counterclaims, and debates over fundamentals which, except perhaps during the Middle Ages, have characterized philosophy and much of social science ever since. Already by the Hellenistic period mathematics, astronomy, statics, and the geometric parts of optics had abandoned this mode of discourse in favor of puzzle solving. Other sciences, in increasing numbers, have undergone the same transition since. In a sense, to turn Sir Karl's view on its head, it is precisely the abandonment of critical discourse that marks the transition to a science. Once a field has made that transition, critical discourse recurs only at moments of crisis when the bases of the field are again in jeopardy.¹² Only when they must choose between competing theories do scientists behave like philosophers. That, I think, is why Sir Karl's brilliant description of the reasons for the choice between metaphysical systems so closely resembles my description of the reasons for choosing between scientific theories.¹³ In neither choice, as I shall shortly try to show, can testing play a quite decisive role.

There is, however, good reason why testing has seemed to do so, and in exploring it Sir Karl's duck may at last become my rabbit. No puzzle-solving enterprise can exist unless its practitioners share criteria which, for that group and for that time, determine when a particular puzzle has been solved. The same criteria necessarily determine failure to achieve a solution, and anyone who chooses may view that failure as the failure of a theory to pass a test. Normally, as I have already insisted, it is not viewed that way. Only the practitioner is blamed, not his tools. But under the special circumstances which

induce a crisis in the profession (e.g., gross failure, or repeated failure by the most brilliant professionals) the group's opinion may change. A failure that had previously been personal may then come to seem the failure of a theory under test. Thereafter, because the test arose from a puzzle and thus carried settled criteria of solution, it proves both more severe and harder to evade than the tests available within a tradition whose normal mode is critical discourse rather than puzzle solving.

In a sense, therefore, severity of test-criteria is simply one side of the coin whose other face is a puzzle-solving tradition. That is why Sir Karl's line of demarcation and my own so frequently coincide. That coincidence is, however, only in their *outcome*; the *process* of applying them is very different, and it isolates distinct aspects of the activity about which the decision—science or non-science—is to be made. Examining the vexing cases, for example, psychoanalysis or Marxist historiography, for which Sir Karl tells us his criterion was initially designed,¹⁴ I concur that they cannot now properly be labeled 'science'. But I reach that conclusion by a route far surer and more direct than his. One brief example may suggest that of the two criteria, testing and puzzle solving, the latter is at once the less equivocal and the more fundamental.

To avoid irrelevant contemporary controversies, I consider astrology rather than, say, psychoanalysis. Astrology is Sir Karl's most frequently cited example of a 'pseudo-science'.¹⁵ He says: 'By making their interpretations and prophecies sufficiently vague they [astrologers] were able to explain away anything that might have been a refutation of the theory had the theory and the prophecies been more precise. In order to escape falsification they destroyed the testability of the theory.'¹⁶ Those generalizations catch something of the spirit of the astrological enterprise. But taken at all literally, as they must be if they are to provide a demarcation criterion, they are impossible to support. The history of astrology during the centuries when it was intellectually reputable records many predictions that categorically failed.¹⁷ Not even astrology's most convinced and vehement exponents doubted the recurrence of such failures. Astrology cannot be barred from the sciences because of the form in which its predictions were cast.

Nor can it be barred because of the way its practitioners explained failure. Astrologers pointed out, for example, that, unlike general predictions about, say, an individual's propensities or a natural calamity, the forecast of an individual's future was an immensely complex task, demanding the utmost skill, and extremely sensitive to minor errors in relevant data. The configuration of the stars and eight planets was constantly changing; the astronomical tables used to compute the configuration at an individual's birth were notoriously imperfect; few men knew the instant of their birth with the requisite precision.¹⁸ No wonder, then, that forecasts often failed. Only after astrology itself became implausible did these arguments come to seem question-begging.¹⁹ Similar arguments are regularly used today when explaining, for example, failures in medicine or meteorology. In times of trouble they are also deployed in the exact sciences, fields like physics, chemistry, and astronomy.²⁰ There was nothing unscientific about the astrologer's explanation of failure.

Nevertheless, astrology was not a science. Instead it was a craft, one of the practical arts, with close resemblances to engineering, meteorology, and medicine as these fields were practiced until little more than a century ago. The parallels to an older medicine and to contemporary psychoanalysis are, I think, particularly close. In each of these fields shared theory was adequate only to establish the plausibility of the discipline and to provide a rationale for the various craft-rules which governed practice. These rules had proved their use in the past, but no practitioner supposed they were sufficient to prevent recurrent failure. A more articulated theory and more powerful rules were desired, but it would have been absurd to abandon a plausible and badly needed discipline with a tradition of limited success simply because these desiderata were not yet at hand. In their absence, however, neither the astrologer nor the doctor could do research. Though they had rules to apply, they had no puzzles to solve and therefore no science to practice.²¹

Compare the situations of the astronomer and the astrologer. If an astronomer's prediction failed and his calculations checked, he could hope to set the situation right. Perhaps the data were at fault: old observations could be re-examined and new measurements made, tasks which posed a host of calculational and

instrumental puzzles. Or perhaps theory needed adjustment, either by the manipulation of epicycles, eccentrics, equants, etc., or by more fundamental reforms of astronomical technique. For more than a millennium these were the theoretical and mathematical puzzles around which, together with their instrumental counterparts, the astronomical research tradition was constituted. The astrologer, by contrast, had no such puzzles. The occurrence of failures could be explained, but particular failures did not give rise to research puzzles, for no man, however skilled, could make use of them in a constructive attempt to revise the astrological tradition. There were too many possible sources of difficulty, most of them beyond the astrologer's knowledge, control, or responsibility. Individual failures were correspondingly uninformative, and they did not reflect on the competence of the prognosticator in the eyes of his professional competitors.²² Though astronomy and astrology were regularly practiced by the same people, including Ptolemy, Kepler, and Tycho Brahe, there was never an astrological equivalent of the puzzle-solving astronomical tradition. And without puzzles, able first to challenge and then to attest the ingenuity of the individual practitioner, astrology could not have become a science even if the stars had, in fact, controlled human destiny.

In short, though astrologers made testable predictions and recognized that these predictions sometimes failed, they did not and could not engage in the sorts of activities that normally characterize all recognized sciences. Sir Karl is right to exclude astrology from the sciences, but his over-concentration on science's occasional revolutions prevents his seeing the surest reason for doing so.

That fact, in turn, may explain another oddity of Sir Karl's historiography. Though he repeatedly underlines the role of tests in the replacement of scientific theories, he is also constrained to recognize that many theories, for example the Ptolemaic, were replaced before they had in fact been tested.²³ On some occasions, at least, tests are not requisite to the revolutions through which science advances. But that is not true of puzzles. Though the theories Sir Karl cites had not been put to the test before their displacement, none of these was replaced before it had ceased adequately to support a puzzle-solving tradition. The state of astronomy was a scandal in the early sixteenth century. Most astronomers neverthe-

less felt that normal adjustments of a basically Ptolemaic model would set the situation right. In this sense the theory had not failed a test. But a few astronomers, Copernicus among them, felt that the difficulties must lie in the Ptolemaic approach itself rather than in the particular versions of Ptolemaic theory so far developed, and the results of that conviction are already recorded. The situation is typical.²⁴ With or without tests, a puzzle-solving tradition can prepare the way for its own displacement. To rely on testing as the mark of a science is to miss what scientists mostly do and, with it, the most characteristic feature of their enterprise.

NOTES

1. For purposes of the following discussion I have reviewed Sir Karl Popper's [1959], his [1963], and his [1957]. I have also occasionally referred to his original [1935] and his [1945]. My own [1962] provides a more extended account of many of the issues discussed below.
2. More than coincidence is presumably responsible for this extensive overlap. Though I had read none of Sir Karl's work before the appearance in 1959 of the English translation of his [1935] (by which time my book was in draft), I had repeatedly heard a number of his main ideas discussed. In particular, I had heard him discuss some of them as William James Lecturer at Harvard in the spring of 1950. These circumstances do not permit me to specify an intellectual debt to Sir Karl, but there must be one.
3. Elsewhere I use the term 'paradigm' rather than 'theory' to denote what is rejected and replaced during scientific revolutions. Some reasons for the change of term will emerge below.
4. Underlining one additional area of agreement about which there has been much misunderstanding may further highlight what I take to be the real differences between Sir Karl's views and mine. We both insist that adherence to a tradition has an essential role in scientific development. He has written, for example, 'Quantitatively and qualitatively by far the most important source of our knowledge—apart from inborn knowledge—is tradition' (Popper [1963], p. 27). Even more to the point, as early as 1948 Sir Karl wrote, 'I do not think that we could ever free ourselves entirely from the bonds of tradition. The so-called freeing is really only a change from one tradition to another' ([1963], p. 122).

5. Popper [1959], p. 27.
6. For an extended discussion of normal science, the activity which practitioners are trained to carry on, see my [1962], pp. 23–42, and 135–42. It is important to notice that when I describe the scientist as a puzzle solver and Sir Karl describes him as a problem solver (e.g., in his [1963], pp. 67, 222), the similarity of our terms disguises a fundamental divergence. Sir Karl writes (the italics are his), 'Admittedly, our expectations, and thus our theories, may precede, historically, even our problems. *Yet science starts only with problems.* Problems crop up especially when we are disappointed in our expectations, or when our theories involve us in difficulties, in contradictions'. I use the term 'puzzle' in order to emphasize that the difficulties which *ordinarily* confront even the very best scientists are, like crossword puzzles or chess puzzles, challenges only to his ingenuity. *He* is in difficulty, not current theory. My point is almost the converse of Sir Karl's.
7. Cf. Popper [1963], pp. 129, 215, and 221, for particularly forceful statements of this position.
8. For example, Popper [1963], p. 220.
9. For the work on calcination see, Guerlac [1961]. For the background of the parity experiments see, Hafner and Presswood [1965].
10. The point is argued at length in my [1962], pp. 52–97.
11. Popper [1963], Chapter 5, especially pp. 148–52.
12. Though I was not then seeking a demarcation criterion, just these points are argued at length in my [1962], pp. 10–22 and 87–90.
13. Cf. Popper [1963], pp. 192–200, with my [1962], pp. 143–58.
14. Popper [1963], p. 34.
15. The index to Popper [1963] has eight entries under the heading 'astrology as a typical pseudo science'.
16. Popper [1963], p. 37.
17. For examples see, Thorndike [1923–58], 5, pp. 225 ff., 6, pp. 71, 101, 114.
18. For reiterated explanations of failure see, *ibid.* I, pp. 11 and 514 f.; 4, 368; 5, 279.
19. A perceptive account of some reasons for astrology's loss of plausibility is included in Stahlman [1956]. For an explanation of astrology's previous appeal see, Thorndike [1955].
20. Cf. my [1962], pp. 66–76.
21. This formulation suggests that Sir Karl's criterion of demarcation might be saved by a minor restatement entirely in keeping with his apparent intent. For a field to be a science its conclusions must be

logically derivable from shared premises. On this view astrology is to be barred not because its forecasts were not testable but because only the most general and least testable ones could be derived from accepted theory. Since any field that did satisfy this condition might support a puzzle-solving tradition, the suggestion is clearly helpful. It comes close to supplying a sufficient condition for a field's being a science. But in this form, at least, it is not even quite a sufficient condition, and it is surely not a necessary one. It would, for example, admit surveying and navigation as sciences, and it would bar taxonomy, historical geology, and the theory of evolution. The conclusions of a science may be both precise and binding without being fully derivable by logic from accepted premises. Cf. my [1962], pp. 35–51.

22. This is not to suggest that astrologers did not criticize each other. On the contrary, like practitioners of philosophy and some social sciences, they belonged to a variety of different schools, and the inter-school strife was sometimes bitter. But these debates ordinarily revolved about the *implausibility* of the particular theory employed by one or another school. Failures of individual predictions played very little role. Compare Thorndike [1923–58], 5, p. 233.
23. Cf. Popper [1963], p. 246.
24. Cf. my [1962], pp. 77–87.

REFERENCES

- Guerlac [1961]: *Lavoisier—The Crucial Year*, 1961.
 Hafner and Presswood [1965]: "Strong Interference and Weak Interactions," *Science*, 149, pp. 503–10.
 Kuhn [1962]: *The Structure of Scientific Revolutions*, 1962.
 Popper [1935]: *Logik der Forschung*, 1935.
 Popper [1945]: *The Open Society and Its Enemies*, 2 vols., 1945.
 Popper [1957]: *The Poverty of Historicism*, 1957.
 Popper [1959]: *Logic of Scientific Discovery*, 1959.
 Popper [1963]: *Conjectures and Refutations*, 1963.
 Stahlman [1956]: "Astrology in Colonial America: An Extended Query," *William and Mary Quarterly*, 13, pp. 551–63.
 Thorndike [1923–58]: *A History of Magic and Experimental Science*, 8 vols, 1923–58.
 Thorndike [1955]: "The True Place of Astrology in the History of Science," *Isis*, 46, pp. 273–8.