DUHEM, QUINE, AND THE MULTIPLICITY OF SCIENTIFIC TESTS*

YURI BALASHOV†‡

History and Philosophy of Science Program
and
Department of Philosophy
University of Notre Dame

Duhem's and Quine's holistic theses, when properly understood, allow methodologically responsible ways of resolving a conflict between a theoretical system and experience; they only deny the possibility of doing it in an epistemically persuasive way. By developing a "string" model of scientific tests I argue that the pattern of interaction between the elements of a theoretical system arising in response to multiple adverse data can be helpful in locating a "weak spot" in it. Combining this model with antiholistic arguments of Popper, Greenwood, and Lakatos significantly reinforces their joint power.

1. What Duhem and Quine Did Not Assert. According to the "Duhem-Quine thesis", the holistic nature of scientific tests makes the rejection of a particular hypothesis, as a result of an adverse experience, essentially inconclusive. One can always retain the hypothesis at hand by making appropriate adjustments "elsewhere in the system". However, in light of many divergences between Duhem's and Quine's statements of the problem of holism revealed by recent discussions (Harding 1976, Vuillemin 1987, Krips 1982, Ariew 1984), the term "Duhem-Quine thesis" seems misleading. Both authors would definitely be distressed to see their statements detached from the particular contexts in which they introduced them. Moreover, certain reservations made by Duhem and by Quine with respect to their holist theses can be regarded as *antiholistic* arguments.

Neither Duhem nor Quine argued that one can stick to a false hypothesis indefinitely, or make it irrefutable at will. Such a hypothesis, according to Duhem, is supposed to crumble finally with the whole system in which it is embedded, "under the weight of the contradictions inflicted by reality on the consequences of this system taken as a whole" (Duhem [1906] 1954, 216). Quine concurs that even though any single statement,

*Received August 1993; revised January 1994.

†I am indebted to J. T. Cushing, G. Gale, J. Leslie, E. McMullin, and P. Quinn for many helpful comments, and to an anonymous referee for insightful criticism and suggestions. Special thanks are due to S. V. Illarionov for stimulating discussions of the D-thesis.

‡Send reprint requests to the author, History and Philosophy of Science Program, 309 O'Shaughnessy Hall, University of Notre Dame, Notre Dame, IN 46556, USA.

Philosophy of Science, 61 (1994) pp. 608–628

or belief, can in principle be retained "come what may", the whole body of beliefs containing it can be rationally rejected in some circumstances when a theory can be sustained "only at the cost of systematic waiving" (Quine and Ullian 1978, 32) of auxiliary hypotheses involved in interpreting recalcitrant observations. As a result of such a strategy, the whole system would eventually become "an undependable instrument of prediction and not a good example of scientific method" (ibid.).

This would not enable one to locate "the weak spot that impairs the whole system" (Duhem [1906] 1954, 216). It does not mean, though, that scientific progress can be paralyzed by the holistic considerations. Scientists do succeed in deciding which hypothesis to abandon in light of adverse data. Although such decisions, based as they are, according to Duhem, on the "good sense" of physicists

> do not impose themselves with the same implacable rigor that the prescriptions of logic do . . . we may find it childish and unreasonable for the . . . physicist to maintain obstinately at any cost, at the price of continual repairs and many tangled-up stays, the worm-eaten columns of a building tottering in every part, when by razing these columns it would be possible to construct a simple, elegant, and solid system. (Ibid., 217)

The same goes for Quine. What are the "considerations of equilibrium" mediating, though indirectly, the relations between particular experiences and particular statements of a theoretical system? "Conservatism" figures in them, as does "the quest for simplicity" (Quine 1953, 46). Elsewhere Quine introduced, besides conservatism and simplicity, other criteria for theory appraisal: modesty, generality, refutability, and precision (Quine and Ullian 1978, chaps. 6, 8).

Thus both Duhem and Quine appreciate the ability of scientists to decide in practice which way to proceed in each particular case. What makes such decisions epistemically inconclusive is their manifestly pragmatic character. This character, however, can be reduced, and the antiholistic "counterpoints" occasionally audible through the main holistic themes of Duhem and Quine can be elaborated into the leitmotif of scientific rationality. Some proposals as to how this can be done have been put forward by Popper (1959, 1963), Lakatos (1978), and, most recently, Greenwood (1990). I show that various antiholistic arguments can be combined and mutually reinforced in the framework of the model of scientific tests I develop in the next section.

2. The String Model of Scientific Tests. A typical theoretical system comprises a "core", an area of "intermediary elements", and a "periphery" receptive to the input from the outer world. The main constitutive

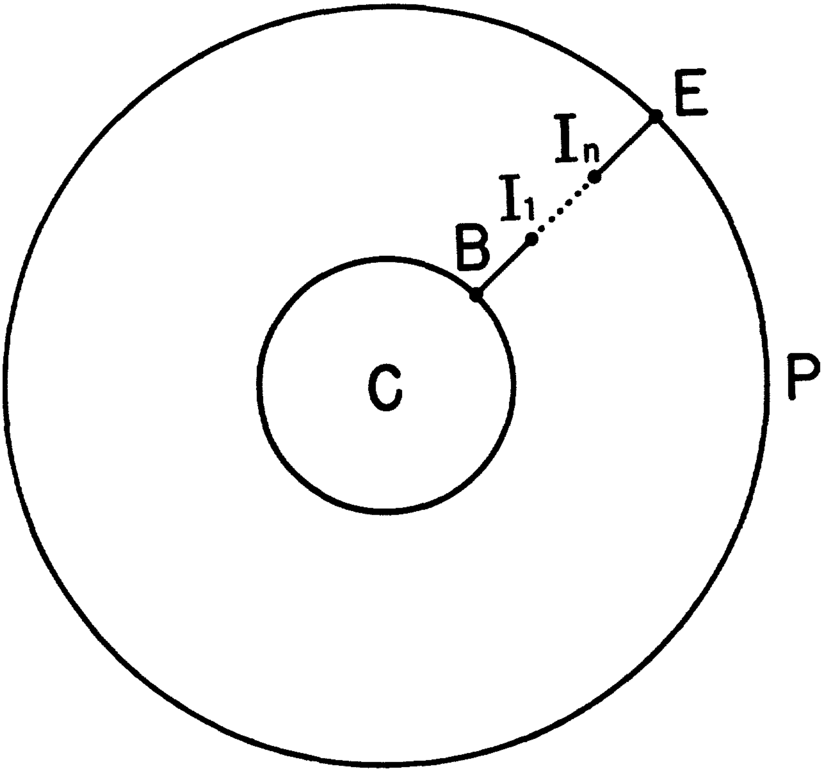

Figure 2.1. String model of testing.

unit of the model of testing sketched in figure 2.1 is a string of elements connecting a certain point **B** of the theoretical core **C** and a particular "port of entry" **E** on the periphery **P** established by deducing a consequence from a system. A string normally contains a number of intermediary elements $\mathbf{I}_1, \ldots, \mathbf{I}_n$.

This picture resembles the structure of the Lakatosian research program. Some important qualifications are, however, necessary. First, the nature of intermediary links $\mathbf{I}_1, \ldots, \mathbf{I}_n$ can be varied, depending on their location in a string. The links adjacent to the periphery $(\ldots \mathbf{I}_{n-1}, \mathbf{I}_n)$ most likely belong to the broad category of "observational" and "interpretive" theories. Those near the core $(\mathbf{I}_1, \mathbf{I}_2, \ldots)$ function as "auxiliary theories". Some may specify ceteris paribus clauses. Such auxiliaries still differ from the element **B** immediately adjacent to the core, which provides the "boundary conditions". The difference occurs since $\mathbf{I}_1, \mathbf{I}_2, \ldots$ can be disconnected from **C** whereas **B** cannot.

Indeed, $\mathbf{I}_1, \mathbf{I}_2, \ldots$ may be alien to the core theory. They are called up to play a role in the testing process, but they enter the "testing string" as mere conjuncts. Element **B**, on the other hand, is connected to the core "more intimately than by mere conjunction" (Lakatos 1978, 46). Only together with the boundary conditions **B** does core **C** constitute a minimal theoretical unit capable of being tested. Without the boundary conditions the core **C** is, in general, too abstract to give rise to concrete results that could be used evidentially for theory appraisal.

The core $\mathbf{C}_N$ of classical celestial mechanics, for example, comprises the general laws of motion and the relevant metaphysics associated with Newton's *Mathematical Principles* ([1729] 1962). Only with the specification of the parameters $\mathbf{B}_N$ of the particular planetary system does the core become a model that functions as an instrument of computation, prediction and explanation. Clearly, the parameters are more than mere conjuncts. They are the theory's contact points with reality whose cognitive significance fully derives from that of a theory. They can be appropriately adjusted and readjusted, but they cannot be separated from the core theory and form an independent unit of cognitive significance. The status of observational, interpretive, and auxiliary elements $\mathbf{I}_1, \ldots, \mathbf{I}_n$, on the contrary, is independent of any other elements of the system. The intermediary links $\mathbf{I}_1, \ldots, \mathbf{I}_n$ are not bound to conform to the model $\mathbf{C} + \mathbf{B}$ and to each other in any other way except conceptual compatibility.

Testing a mechanical model of the solar system by means of astronomical observation would thus involve, at the minimum, a theory of optics and that of gases which are necessary to describe the propagation of light from planets and other celestial bodies to the observer. Both optics and a theory of gases serve here as independent auxiliary assumptions mediating the connection between $\mathbf{C}_N + \mathbf{B}_N$ and $\mathbf{E}_N$.

The string picture outlined is heterogeneous in the sense that its intermediary elements $\mathbf{I}_1, \ldots, \mathbf{I}_n$ do not in general form a linear inferential chain. Most of them function singly, or in certain combinations, as "mere conjuncts". The whole—perhaps intricate—conjunction of them (or their combinations) mediates the relation between $\mathbf{C} + \mathbf{B}$ and **E**, which *is* inferential, unlike the relations between most of $\mathbf{I}_1, \ldots, \mathbf{I}_n$. However, some of $\ldots \mathbf{I}_{n-1}, \mathbf{I}_n$, "instrumental" and "interpretive" theories, may form a short linear inferential "channel" flowing into a port of entry. A more precise picture should display this difference between inferential and conjunctive elements of a testing procedure (figure 2.2a). For the sake of simplicity, however, I henceforth adhere to the plain string pattern (figure 2.2b), bearing in mind that a typical string contains inferential, as well as noninferential, "conjunctive" parts. Obviously both contribute to the testing procedure in the same way: Each can impair the whole string,

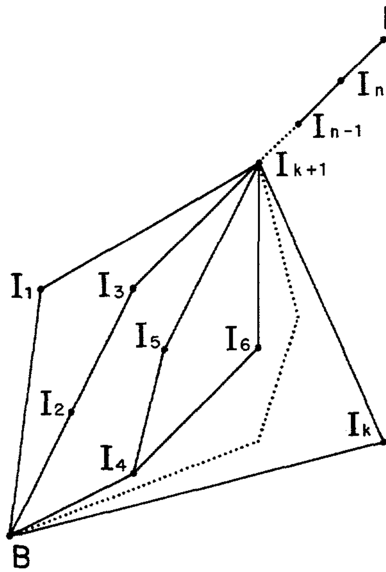

Figure 2.2a. Inferential and conjunctive elements of a testing procedure.

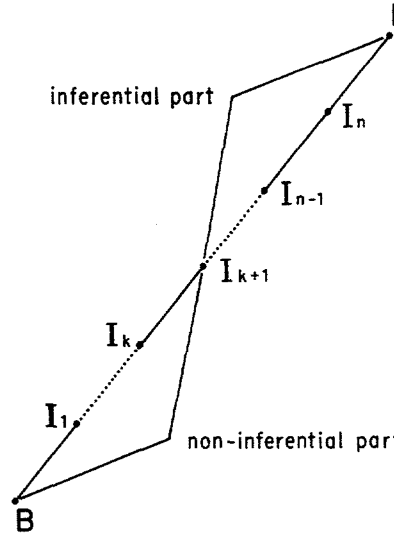

Figure 2.2b. An equivalent "string" representation of the test situation depicted in figure 2.2a.

either by blocking the inference along the inferential part, or by negating the entire conjunction in the "conjunctive" part.

The problem of holism restated in terms of our string model can be reduced to that of *identification of roles* played by different elements of a testing string. Indeed, to locate a weak element of such a string is merely to show that this element is truly "under strain" in the process of test, whereas other elements in a string only transmit strain to the impaired one. Duhem and Quine shared the belief that no element can have the epistemic privilege of being "truly" under strain and the whole task of identification or, rather, distribution of roles has a distinctly pragmatic character. In other words, the decision as to which element in a string is under strain does not give reason to believe that this element is likely to be false, but only a practical reason to abandon it. This is so simply because the whole string, and not a particular element in it, is "objectively" under tension. The discrimination of elements into "weak" and "strong" ones has, on this view, nothing to do with the distinction between true and false. The rest of this essay concerns the refutation of this view. First, I examine the antiholistic arguments of Popper, Greenwood and Lakatos.

3. The Ways of Dealing With Holism. All such arguments, including the one suggested in this essay, can be looked upon as attempts to establish methodologically compelling strategies that would enable one to distinguish "strain conductors" from "strain recipients". The former will then be worth retaining in light of recalcitrant experience falling on the port of entry, whereas the latter will be candidates for discarding or replacement.

A certain type of antiholistic arguments, associated with Popper's falsificationism (1959, chap. 1; 1963, 238–239), is based on negatively delineating those elements of a theoretical system that can rarely be identified as "strain recipients". In terms of our string model, their strength is due to independent corroboration, which they enjoy by participating in a number of positive-instance strings of the system being tested and, in general, in the positive instances of other theoretical systems as well.

Isolating the particular "basic statements" and the relevant pieces of background knowledge that can be rendered, in the face of a particular recalcitrant evidence, unproblematic narrows down the search for "weak spots". By itself, however, this may be insufficient to make the tests conclusive, for more than one problematic element in a given negative-instance string may remain. In other words, the negative separation of those elements that can hardly be "strain recipients" is only a part, albeit an important one, of the task. It should, in general, be completed with the positive identification of the roles played by the remaining elements.

Such an identification, strictly speaking, was not a part of Popper's original program. In this program, the "Duhem-Quine thesis" was regarded not so much as a distinct problem in itself, but as a concept responsible for a certain type of behavior in science, which is incompatible with the falsificationist code of rationality and should thus be avoided. On this view, a scientist should normally be concerned not with the identification of the roles played by different elements of a theoretical system, but rather with the possibility of such a distribution of these roles that can direct *modus tollens* to a chosen element. This policy was intended to accomplish the task of demarcation. Its validity derives from the basic requirement that the distribution of roles be made before the test is carried out, not after its results become known, thus effectively excluding "precisely those ways of evading falsification which . . . are logically admissible" (Popper 1959, 16). The independent support obtained by certain elements in a system under test helps to meet this requirement.

Though by itself insufficient to defeat the holistic account of tests, allowing for such a support seems indispensable to any successful antiholistic strategy. What matters is the degree of independent corroboration enjoyed by the corresponding elements of a theory. One possible way to evaluate this is to examine the set of positive-instance strings in which the elements at hand are involved. Such an evaluation will be an important part of the antiholistic strategy described in sections 4 and 5. I will show that the negative delineation of the probable "strain transmitters" is not separate from the positive search for "strain recipients". Just as the strength of the former derives from their participation in the positive-instance strings, the weakness of the latter can likewise be measured by their involvement in the negative-instance strings.

Greenwood (1990) recently developed another antiholistic argument. He argued that the particular role played by certain elements in the prior support of a theory often disqualifies them from being strain recipients in subsequent instances of testing because the theories under test ("explanatory theories") were initially based on certain "exploratory theories" (. . . $\mathbf{I}_{n-1}$, $\mathbf{I}_n$, in our notation). If the latter are allowed to be modified to accommodate recalcitrant observations, then the theories under test will thereby be deprived of their prior support. For example (ibid., 565–566), the anomalous precession of Mercury's perihelion first calculated by LeVerrier in 1849 could not be accommodated by questioning the reliability of contemporary telescopic observations, for this would undermine a significant part of the prior support for the Newtonian mechanics, which was largely based on careful observations of planetary motions.

What prevents the particular elements of a theoretical system from being strain recipients is not only the "independent corroboration" they enjoy, but also—and this makes Greenwood's antiholistic claims different from, and, in a sense, stronger than, Popper's—their remarkable relationship (which was strangely overlooked in the earlier literature on the "Duhem-Quine thesis") to a "core" theory's confirmation base. Because of this relationship, "the common assumption that exploratory theories can always be modified in the face of anomalies to preserve the evidential equivalence of an explanatory theory with respect to its rivals" (ibid., 567) is simply wrong. Such a modification cannot always be made. This does not mean, however, that it can never be made.

Greenwood (ibid., 569) correctly notes that it is a contingent matter whether one can modify or replace certain "auxiliary assumptions" in such a way that would both accommodate recalcitrant data and preserve prior support for an explanatory theory. Since the present analysis is intended to cover such situations, it is important to see that they are indeed possible both logically and historically.

First, a particular negative-instance string, corresponding to a recalcitrant evidence to be accommodated, may include several intermediary elements, not all of which are equally involved in a core theory's confirmation base. An inventive holist may thus find some ingenious and the least costly way of deflecting the blow of negative evidence from an explanatory theory. For example, to account for the anomalous secular motion of Mercury's perihelion, one could, in principle, question not the reliability of astronomical observations, but the exact form of Newton's gravitation law, without rejecting Newton's mechanics on the whole. In 1870–1890, several attempts were made (see, e.g., North 1965, 46–47) to amend the law of gravitation in such a way that would explain Mercury's anomaly *and* preserve all positive instances of celestial mechanics. All these attempts failed. In fact, none had looked initially plausible—not because they deprived an explanatory theory of its prior support, but for other reasons, such as ad hocness and inconsistency with higher order theoretical assumptions favoring the exact inverse square law of gravitation.

Sometimes the strain of negative evidence forces scientists to break an intermediary element in a testing string into two or more elements, one of which preserves the prior confirmation base of an explanatory theory while the others *legitimately* accommodate new recalcitrant data. For example, the photometry of novae in spiral "nebulae" constituted an important part of the confirmation base of the "island universe" theory in the late 1910s, for it testified to the great distances of spirals (see, e.g., Smith 1982). A striking anomaly was an extremely bright nova S discovered in 1885 in the Andromeda. Placing the Andromeda "nebula" far outside the Milky Way would make the luminosity of S Andromedae incredibly high. To accommodate this high luminosity in the island universe theory, it was later suggested that S Andromedae is a supernova. This broke the exploratory theory of novae employed in support for a core theory in two. One theory was left to account for the novae data and the other to explain the recalcitrant supernovae data. However unattractive this step seemed initially, it proved absolutely correct.

Finally, theories may sometimes be preserved by abandoning some of their prior positive instances, together with an exploratory theory, if this leads to overwhelmingly new support. In other words, the weight of the original support may turn out to be negligible in comparison to the whole balance of subsequent corroborative evidence not involving the original exploratory theory. In this case the latter can be easily sacrificed to save the hypothesis under test. Thus, the successful resolution of more and more "nebulae" into stars in 1840 to the 1850s (including Lord Rosse's and William Bond's "resolutions" of the Orion nebula) was widely regarded as a strong support for the island universe hypothesis to the extent that the whole case for this hypothesis became dependent on further resolutions. W. Huggins ended these expectations in 1864 when he showed, with a new spectroscopic technique, that some nebulae (including Orion) are glowing gases and, hence, could not be resolved. Together with other counterevidence, this new discovery effectively marginalized the island universe theory for many years. Its advocates, however, after reconsidering its confirmation base, accepted gaseous nebulae but still insisted that other "nebulae" are true "island universes". The theory gained completely new and overwhelming support in the 1910s to the 1920s and proved able to handle other "anomalies" remaining from the past.

The above examples show that scientists normally can recognize strain recipients and strain conductors in a theoretical system by striking a balance between positive and negative evidence, gains and losses associated with different strategies. The antiholistic account of scientific tests developed in subsequent sections attempts to show that the decisions involved in a typical test situation have some epistemic, as well as pragmatic, merits. This account builds upon, but is not reducible to, the antiholistic arguments of Popper and Greenwood discussed above. To outline its essential features, I use the "string" model introduced in section 2. First, however, I examine another conceptual source of the proposed account, the Lakatosian methodology of scientific research programs.

Lakatos's antiholistic strategy differs significantly from Popper's and Greenwood's approaches. He endorses the view of Duhem and Quine that the grip of holism with respect to confirmation/falsification procedures in science is strong enough to be taken seriously (see Lakatos 1978, 98). The only way to avoid the devastating consequences of holism for scientific rationality is to counterbalance them by recognizing certain progressive patterns of theoretical growth as evidential. The decisions as to which element of the whole theoretical system to replace, in the face of recalcitrant data, should be evaluated according to the criterion of progress, which is the ability of the modified system to predict novel facts and to get credit for such predictions via their subsequent empirical corroboration.

In other words, it is not possible to fashion any reasonable idea about which element in a string is a strain recipient that should be abandoned or replaced in light of some counterinstance without trying several options to see which produces content-increasing "problemshifts" and which of the latter advances us most. The option that achieves this can then be declared progressive—with all the advantages of hindsight.

Now the Lakatosian criterion of progress can be regarded either as *role identifier* or as *justifier* of such an identification. In the first case, this criterion separates the elements of a system into "weak" and "strong". An element is defined as "weak" if preserving it at the expense of other elements leads to the external regress of the whole system, whereas rejecting or replacing it results in empirical progress. Conversely, an element is defined as "strong" if its rejection more often than not leads to empirical degeneration. Obviously, the point is to establish the relative weakness/strongness of the corresponding elements in terms of the empirical adequacy of the system modified in accordance with the proposed role identification.

The problem with this approach, at least in the form in which Lakatos employs it, is that it alone is supposed to take care of all methodological quandaries involved in the test situation. According to Lakatos, other criteria, such as Popper's "independent corroboration", cannot be utilized for role identification because they are potentially misleading, especially when used to justify a hard-line policy (i.e., once-and-for-all rejection of the falsified theories and hypotheses required by the "methodological falsificationism"; ibid., 23–31). The point of introducing the criterion of progress was to supersede other criteria, not to complement them. On Lakatos's view, the only rational way of identifying the roles played by different elements of a theoretical system (i.e., a research program) is to try *all* kinds of modifications and check the trials by the empirical behavior of the modified system (ibid., 40–41, 45, 99). But clearly this is impossible! Furthermore, even if it were possible to try *everything* and if scientists in fact did, the results of the trials would offer suggestions for the role identifications that would reflect, at each stage, only the ever-changing short-term dynamics of the system (i.e., the research program). Surely such a dynamics is not bound to represent any long-term tendency required for epistemic evaluation.

In fact, scientists never base their initial suggestions for role identification on mere guesswork. They are guided by considerations that, in general, include Popper's "independent corroboration", as well as Greenwood's "no-go" arguments prohibiting a modification of certain exploratory theories, and other considerations that I present in the next section. With these other considerations, the criterion of progress can be effectively employed not as the primary and sole role-identifier, but as a justifier of the role identification already made, if only tentatively, on the basis of some other criterion. Such a criterion can hardly be independent of matters of empirical adequacy. But it can be significantly different from, and not reducible to, these matters in the context of a particular test situation.

Suppose such a criterion is found. It could then be argued that the joint application of both criteria will carry more epistemic import if only because the familiar "no-miracle" argument immediately comes into play. If a role identification suggested by the first criterion also happens to satisfy the second one (i.e., the criterion of progress), different from the first, this must be explained. A reasonable explanation is that the system at hand possesses certain epistemic merits. A successful antiholistic strategy has to be based on the joint application of the criterion of empirical adequacy (including the dynamics of successful and unsuccessful predictions) and some other criterion pertinent to the test situation.

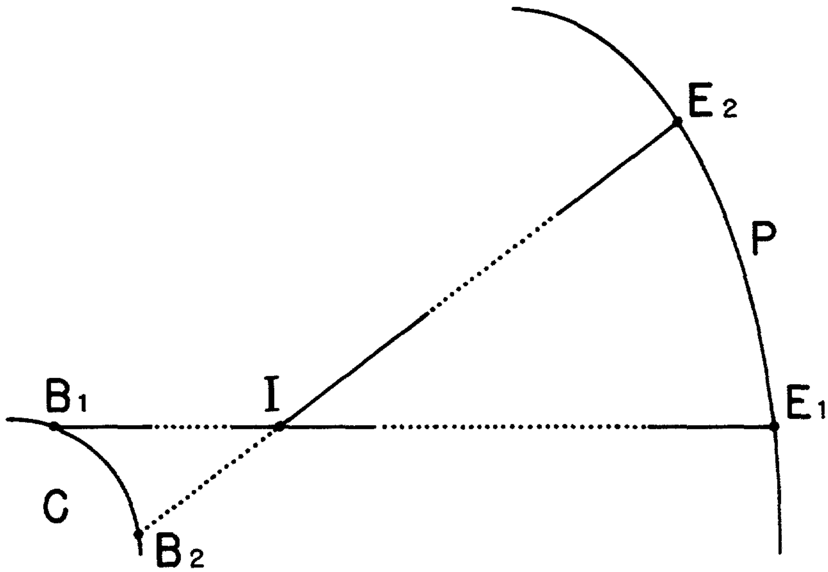

Figure 4.1. Intersection of testing strings at an intermediary element **I**.

4. Locating Weak Spots: The Multiplicity of Tests versus the Wholeness of Theory. Taking the bearing of a spy's radio station requires scannings made from at least two vantage points. The intersection of the two straight lines locates the station. One scanning would be insufficient because it could only ascertain that the station is situated somewhere on the line corresponding to the angle of maximum signal intensity.

Similarly, one instance of adverse evidence isolates a particular testing string in a theoretical system as containing a "weak spot", but is insufficient to "locate" this element. However, two or more instances of testing can bring about the intersection of different strings. In figure 4.1 this point of intersection is identified with the element **I** common to both testing strings.

Such an intersection of testing string is by no means a necessary general feature of scientific tests. However, it can be expected in a theoretical system dealing with multiple types of evidence. For example, before Maxwell, some theories regarded light as a wave process of unspecified nature. An auxiliary hypothesis that this process is transverse rather than longitudinal was involved in at least two (not entirely unrelated but still different) kinds of evidential support for such theories associated with polarization effects and double refraction. This hypothesis was thus a point of intersection of testing strings corresponding to these two kinds of positive evidence. In Maxwell's theory a similar proposition (viz., that light

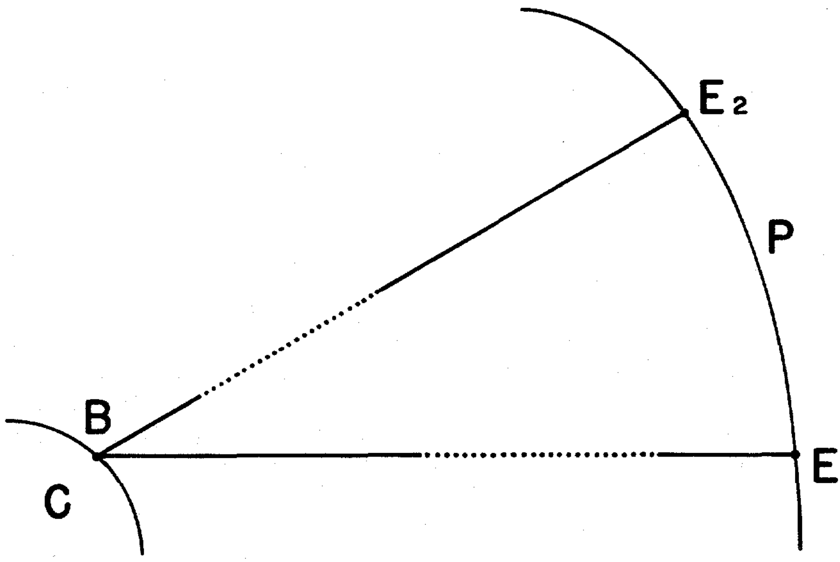

Figure 4.2. Intersection of testing strings at a "boundary" element **B**.

is a transverse wave process) was not an auxiliary assumption but a core element. The case of intersection corresponding to Maxwell's theory is depicted in figure 4.2. Figures 4.1 and 4.2 exhaust all possible kinds of intersection of the testing strings. Such intersections can occur either at some intermediary element **I** (as in figure 4.1) or at a "boundary" element **B** (as in figure 4.2).

An intersection of the two strings implies nothing remarkable. It only means that one and the same element is involved in two instances of testing, which is only natural. The situation differs if both instances turn out to be adverse to the system and each forces an adjustment in the corresponding string. Such adjustments can be carried out in various ways. For example, to eliminate the conflict with the data falling on the port of entry $\mathbf{E}_1$, some element in the string $\mathbf{B}_1$–$\mathbf{E}_1$ *other than* **I** (see figure 4.1) can be adjusted and the same can be done in the other testing string. The phenomena really can be saved in this way.

Suppose that additional recalcitrant data result in the intersection of strings at the same point **I** or **B**, whereas continually saving this element is accompanied by a growing degeneration of the whole system, as measured by a suitable empirical criterion. I claim that this constitutes a good prima facie epistemic reason to consider **I** or **B** a strain recipient.

This argument differs from Lakatos's approach because of the decision to regard the internal pattern of interaction between the elements of a system as indicative of the roles being played by them in the multiple process of test. One should not "try everything" and then fully rely on the criterion of progress. One should first carefully attend to the pattern arising under the pressure of recalcitrant data. The pattern itself can suggest the appropriate role identification. Such suggestions, then, have to be verified by checking empirical behavior of the system. The thrust of the combined antiholistic strategy derives from a correlation that can be established between the convergence of multiple negative evidence toward one particular element, manifested as the center of intersection of the corresponding testing strings, and empirical regress of the whole system modified to leave the center of intersection intact. Indeed, it is highly improbable that the convergence at hand, if accompanied by empirical regress, is purely accidental, and this improbability grows with the number of negative-instance strings that keep intersecting at one point. The relation between the appearance of the convergent pattern and the increase of empirical regress can be established noncircularly. These are two different criteria: One can be employed for the identification of roles, and the other for a subsequent verification of it in a way that is not question-begging.

It is important to see precisely how these criteria differ and cannot be reduced to one another. To be sure, the "convergence" criterion depends on matters of empirical adequacy, for strings intersecting at a certain point relate to instances of falsified empirical predictions. But that they intersected at one point rather than at another, within the system, follows from relationships of elements in this system and cannot be reduced to empirical matters alone. In other words, the *intersection* of the negative- (or positive-) instance strings *adds* something to these strings' being negative (positive).

Only multiple counterinstances that lead to a convergent pattern can suggest role identification, and such counterinstances are the premises of the holist problem. Positive instances are not interesting. They cannot suggest anything except that the system is working perfectly, even if they also bring about a convergent pattern of string intersection, as in the case of pre-Maxwellian theories of light. Let us consider some examples.

1. The ether theories of the 1890s accommodated the recalcitrant results of the Michelson-Morley experiment in several ways, for example, by invoking an auxiliary hypothesis (**I**) of a partial drag. This, however, required creating the intersection of at least two new negative-instance strings at **I**, namely, those corresponding to aberration of stellar light and to the absence of resistance that bodies should have experienced when partially dragging the ether in their motion.

2. Newtonian cosmology, in an attempt to construct a model of the infinite static universe within the framework of classical mechanics, was

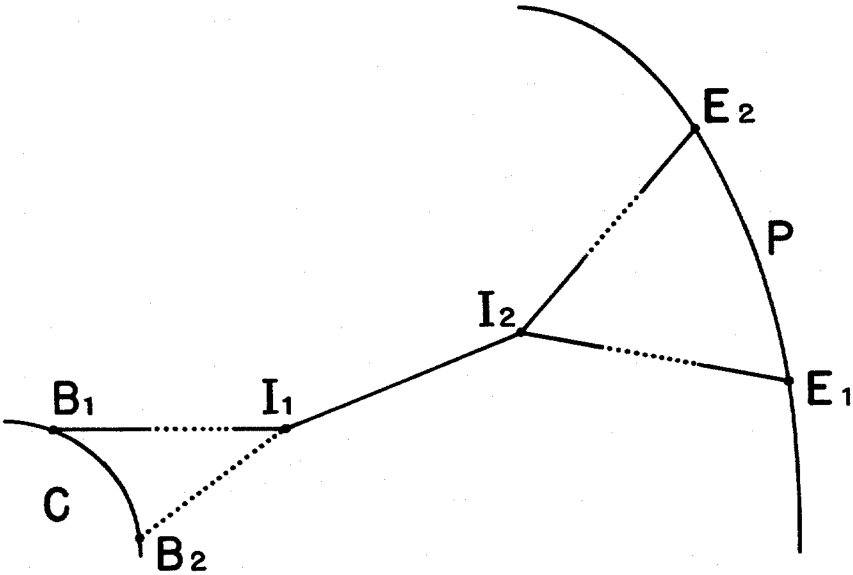

Figure 4.3. Intersection of strings $\mathbf{B}_1$–$\mathbf{E}_1$ and $\mathbf{B}_2$–$\mathbf{E}_2$ comprises two elements $\mathbf{I}_1$ and $\mathbf{I}_2$. In case of paradoxes of the Newtonian cosmology these elements are:
$\mathbf{I}_1$: The universe is infinite in space;
$\mathbf{I}_2$: The universe is infinite in time.

led to the well-known paradoxes of de Cheseaux-Olbers and of Seeliger (see, e.g., North 1965, 16–23). According to the first, the brightness of any segment of the sky should equal that of the sun's disc; according to the second, the gravitational potential at any point of the infinite universe becomes indefinite. To represent this situation in terms of the string model, figure 4.1 must be slightly modified. As it turns out, the intersection of the negative-instance strings corresponding to these paradoxes comprise two elements, the assumption of spatial infinity of the universe and that of its infinite duration in time (see figure 4.3).

The tension impairing both strings could, in principle, be removed, even in the context of the Newtonian cosmology, by dropping either of these assumptions. In actuality, neither was even recognized, before the relativistic era, as a distinct theoretical statement. That the universe is infinite both in space and time was simply taken for granted. Thus the convergence of negative evidence on these assumptions was, in a sense, implicit. Had it been explicitly established, there could have been a prima facie reason (stemming from this intersection correlated with the lack of empirical progress of the Newtonian world model) to consider $\mathbf{I}_1$ and $\mathbf{I}_2$, or rather their conjunction $\mathbf{I}_1$ & $\mathbf{I}_2$, a "weak spot". Scientists instead attempted to remove the tension by adjusting other elements in the intersecting strings, for example, by proposing hierarchical world models (à la Charlier) and by modifying the Newtonian law of gravitation. Had they succeeded in identifying the "weak spot" as $\mathbf{I}_1$ & $\mathbf{I}_2$, they would still have had to decide whether $\mathbf{I}_1$, or $\mathbf{I}_2$, or both, should have been abandoned. The holistic dilemma with respect to the conjunction $\mathbf{I}_1$ & $\mathbf{I}_2$ would still threaten the hypothetical progress of the Newtonian cosmology. However, new instances of testing could have ultimately separated $\mathbf{I}_1$ and $\mathbf{I}_2$.

To clarify how this can happen, note that, although positive instances of testing cannot help to locate a weak spot in the first place, they can later help to decide what to do with the element already located. To locate a weak spot in a system is only a part of the problem. To declare it a strain recipient requires examining how the system can respond to tests. Because of various connections between the elements of the system, such a response would usually affect more than one element (a "ripple effect", in Quine's wording) though the one that has been spotted would be the first to be dealt with. Let us consider some basic scenarios.

5. Identifying the Strain Recipients: How Theoretical Systems React to Tests. Suppose the center of intersection of several negative-instance strings is some intermediary element **I**, as shown in figure 4.1. The pressure of counterinstances typically force an adjustment of a parameter in **I**, though not always coherently. For example, different adverse data may pull in opposite directions. If the parameter is adjustable, it can be fine-tuned so that the strain spread over the intersecting strings can be eliminated or at least reduced. If the parameter at hand is fixed in the theory **I** and cannot be fine-tuned, then the whole theory must yield to some alternative theory $\mathbf{I}^*$ capable of coherent strain removal.

In deciding what exactly to do with **I**, account must be taken of the positive instances involving **I**. Here Greenwood's and Popper's considerations become important. If **I** is involved in a number of strings corresponding to such instances, besides those of counterevidence, its adjustment or replacement will disturb the former strings. It will also leave under strain those negative-instance strings (if any) that pulled the parameter being considered in another direction. It may also disturb certain positive-instance strings in other theoretical systems in which **I** may be involved. In any event, some other elements in the corresponding strings would have to be fine-tuned to make up for such a disturbance and to eliminate the remaining strain. But these elements too might be members of still other positive-instance strings, and so the latter might also have to be adjusted. Depending on how deeply **I** is entrenched in relations with other elements of a system, or of other systems, several options arise.

Suppose that the whole procedure of tuning can be carried out coherently within the system at hand, that this does not bring significant disturbing effects to other parts of knowledge, and that, as a result of the adjustments made, the system as a whole displays empirical progress. One's initial belief in the incorrectness of **I**, that was based on the examination of the internal pattern, is thereby supported. Alternately, if the system degenerates after the fine-tuning, one has a good occasion to remember the Duhemian "tangled-up stays" and "worm-eaten columns" and to abandon the adjustment tactic altogether.

The same conclusion would be justified if the adjustment can be coherently accomplished within the system, but brings irreparable damage to other fragments of knowledge. Only an ingenious and successful attempt to completely repair it, accompanied by pronounced empirical progress, can support the initial decision to tamper with **I**. Without ingenuity, even in the presence of progress, a deadlock would occur. The only way out requires acknowledging the anomalies generated in other parts of knowledge by the adjustment of **I** in the first place and tolerating these in the hope that ultimate understanding will follow (see Lakatos 1978, 49–52). Obviously, this pragmatic decision cannot repeal the epistemic sentence passed on **I**, which has been declared correctly a strain recipient. How successfully it is "broken" depends on whether the "ultimate understanding" mentioned above really comes. It may not. Then the whole adjustment tactic should be terminated.

Another option is to replace the old compromised element **I** with a new one, **I***, instead of simply adjusting some parameter in the former. Element **I*** might enter both kinds of strings corresponding to the positive and negative evidence in a qualitatively new and ingenious way. It might, for example, eliminate the original strain in the negative-instance strings without disturbing the positive-evidence ones, or disturbing them to such a minimal degree that the remaining minor difficulties can easily be resolved as in the previous example. In some cases, coherent strain removal will be achieved only by breaking **I** into two or more new chunks of theory $\mathbf{I}_1^*$, $\mathbf{I}_2^*$, . . . entering different strings (recall the novae/supernovae case). Such "multiplication of essences", as well as all other options can then be validated by empirical success or condemned by the lack of it. In the latter case, and also in the case of one's inability to invent a "new and ingenious" theory **I***, one is back to the starting point.

At this stage it may be a good idea to scrutinize the original problem more carefully. Element **I** that has been at first considered (with due reason) a weak spot may be so deeply and multifariously entrenched in the overwhelming body of the positive evidence that one's initial reasons to regard it a strain recipient are now outweighed by these new counterarguments. This means that the convergence of adverse evidence toward **I** is presently insufficient to overthrow it. Perhaps one could avoid the difficulty more successfully by making a suitable adjustment elsewhere in the system, applying the same accounting technique as in the former case.

If the entrenchment of **I** is slight, whereas the convergence of recalcitrant data toward it is, on the contrary, impressive, then discarding **I** is reasonable except that, at the moment, no new and ingenious alternative is available. Inability to cope with the problem has nothing to do with the role identification. The latter is beyond suspicion, and clearly a typical anomaly is now associated with **I**. Accepting **I** for the moment may be necessary.

Notice that all the options mentioned include arguments originally based on intratheoretical considerations and later evaluated in terms of the criterion of progress/regress. The balance of the negative and positive evidence and the impact of possible alternative changes in the system on its degree of coherence can be read off the string pattern. What cannot be so read off are the system's empirical dynamics. The joint application of both criteria endows the original judgements with the power required to mollify a holist.

One could object that a decision to retain an anomaly runs counter to the original suggestions. But this pragmatic decision to retain the element that is likely to be wrong is in no way related to the epistemic arguments for its being wrong.

In order to estimate the value of the alternatives, it is not necessary to try everything, which would reveal a serious underestimation of the power inherent in the "string analysis". The scope of the trial, the sequence of its steps and, more importantly, its starting point, can be determined reliably by such an analysis.

The situation depicted in figure 4.2, where the center of intersection of the negative-instance strings is not an intermediary element **I**, but a point **B** on the boundary of the core **C**, differs significantly from the other. We recall that, unlike any **I**, **B** is connected with the core "more intimately than by mere conjunction". Together with **C** it forms a model of the particular class of phenomena, and only through the boundary condition **B** can **C** gain access to the phenomenal quantifiable world. In response to the multiple impact of adverse evidence, **B** can only be adjusted if it contains adjustable parameters. If not, or if the adjustment is unsuccessful, one would want to replace **B**, or break it into $\mathbf{B}_1^*$, $\mathbf{B}_2^*$, . . ., by analogy with **I**. However, one cannot, for **B** is not detachable from the core as an independent unit of cognitive significance.

Yet the effect of a replacement can be attained by tampering with the whole model complex **C** + **B**. As noted by Lakatos, "any theory can be saved from counterinstances either by some auxiliary hypothesis or by a suitable reinterpretation of its terms" (1978, 32). Precisely by the ingenious and heuristically progressive reinterpretation of **B** *within* the model **C** + **B**, **B** can be successfully turned into **B*** or broken into $\mathbf{B}_1^*$, $\mathbf{B}_2^*$, ...

Again, a decision with respect to **B** suggested by the pattern of string intersection must be tested against the progress of the modified system.

6. Discussion. To illustrate these points, let us finally consider some examples from the history of modern cosmology. Hubble's original misidentification of the relevant type of Cepheids led to a mistaken estimate of the age of the universe, $\mathbf{t}_U \approx 2 \times 10^9$ years, in the standard relativistic model. This drastically contradicted the estimated age of the earth as $\mathbf{t}_E \geq 3 \times 10^9$ years (the "time-scale problem"). According to the same misidentification, our galaxy turned out to be far larger than any other observable "nebula".[1] This conclusion counters the cosmological principle requiring that no large enough region of the cosmos be distinguished, in terms of physical properties of its population, from any other such region.

The strings corresponding to these two instances of negative evidence thus intersected at a common auxiliary element **I** (see figure 4.1) representing Hubble's scale of extragalactic distances derived from the wrong period-luminosity relation for Cepheids (and also from Hubble's mistaking HII regions in the distant galaxies for the brightest stars). The convergence of the negative evidence toward **I**, however, was ignored by astrophysicists. To remove the strain in the intersecting strings they decided to adjust other elements in them. The size of our galaxy was attributed to a huge fluctuation, whereas the time-scale discrepancy was eliminated by *changing the meaning* of $\mathbf{B}_{BB}$, the parameter specifying the age of the universe $\mathbf{t}_U$ in what was later called big bang cosmology. The simple relation $\mathbf{t}_U \sim \mathbf{H}^{-1}$ (**H** is the Hubble parameter) was not valid in the Eddington-Lemaître and the later Lemaître models with the Λ-term. The evolution of the universe was supposed to proceed very slowly in the past, so the observable Hubble parameter **H** did not relate in any simple way to the age of the universe.

Relativistic cosmology's lack of progress in the 1930s was manifest. And Hubble's methods of establishing the distances of galaxies were not entrenched in any other positive-instance string. Yet nobody dared to question Hubble's data (despite the intratheoretical considerations noted above). Why? The "external" circumstances played a role here. First, Hubble's data could not be checked independently, for no other instrument was comparable to the Mount Wilson 100-inch reflector. Second, Hubble's authority was indisputable. The prima facie suggestions that could, in principle, have been read off the string pattern of the situation circa 1930 were acted upon only in the 1950s when Hubble's distance scale was explicitly shown to be severely underestimated.

[1]"Whereas the other galaxies were 'islands', ours was a continent" (Bondi 1990, 194). Hubble's scale of distances created at least one more observable anomaly: The globular clusters in the Andromeda galaxy seemed fainter than those in our galaxy.

From another viewpoint, a reasonable estimation of the age of the oldest stars in the universe, $\mathbf{t}_S \geq 5 \times 10^9$ years, became available in the 1940s. Together with the age of the earth, this counterevidence exerted pressure on $\mathbf{B}_{BB}$ (the "boundary" parameter specifying the age of the universe in a simple big bang model without the Λ-term) that pushed it in the same "direction", forcing an increase of $\mathbf{t}_U$. In light of the data available at that time, $\mathbf{B}_{BB}$, and no other elements in the testing strings related to geophysical and astrophysical data, was undoubtedly a strain recipient, despite a great deal of theoretical baggage involved in interpreting the relevant geological and astronomical observations. No part of this baggage was above suspicion. However, $\mathbf{B}_{BB}$ was actually called into question because of the convergence of recalcitrant evidence.

This situation provoked different responses. One, already considered, was to invoke special evolutionary models with the Λ-term. Another was to recognize the current value of $\mathbf{t}_U$ as anomalous but to continue to adhere to the simple model while hoping that the anomaly would be resolved in the future. Such a pragmatic decision was implicitly adopted by the majority of the cosmological community, and had nothing to do with the reasons condemning $\mathbf{t}_U$ as wrong. This strategy proved successful in the long run after it had found evidential support in the 1950s.

In the 1940s, however, the situation was ambiguous. Another strategy adopted by a cosmological minority in 1948 was to abandon the big bang model altogether and propose a completely different theoretical system, the steady state model. The corresponding boundary condition $\mathbf{B}_{SS}$ specifying the age of the universe in the steady state model ($\mathbf{t}_U = \infty$) accorded with both kinds of data that troubled the big bang model. In addition, the new model based on the "perfect cosmological principle" displayed at first a higher degree of heuristic coherence and even theoretical progress than its rival, prompting more advocates to support it, despite the apparent conflict with the rest of physics of the *creatio ex nihilo* hypothesis inherent in the steady state model. The true nature of this conflict needs clarification. The violation of exact conservation of energy required by the model was extremely small and did not counter any available experimental evidence. In other words, all positive-instance strings involving the conservation law as an intermediary element remained intact, and Bondi's (1957, 198) intention was to downgrade this law to the status of a purely empirical regularity that is not bound to be exact. Yet the strict conservation of energy is entailed by very general physical principles, and it is highly improbable that physics as a whole can be coherently reconstructed to satisfy the assumptions of the steady state model. Any attempt to do so would force radical changes of meaning of many physical concepts involved in a large number of positive-instance strings in which the conservation laws serve as boundary conditions or even as parts of the cores. Yet Hoyle (1948) adopted a "reconstruction" strategy. But neither he, nor anybody else, succeeded in developing it beyond a very restricted area of the original cosmological problems. It is doubtful that one could realistically have expected success here. In the 1950s, the steady state model represented only slight progress in overcoming the many hidden anomalies.

In the early 1960s, however, the model ran into overtly empirical problems. Again, the adverse evidence started to converge at $\mathbf{B}_{SS}$. The infinite age of the universe did not conform to the spatial distribution of radio sources, testifying to their systematic evolution with time, nor to the relative abundance of elements, nor finally to the presence of the microwave background radiation discovered in 1964. All attempts to make suitable adjustments elsewhere in the relevant testing strings, for example, by questioning the identification of radio sources, or the technique of their counts, by inventing alternative mechanisms to account for the microwave background, and also (Hoyle's last desperate step) by changing the meaning of $\mathbf{B}_{SS}$ had no success (for a brief account see Kragh 1993). The steady state model was by now degenerating both empirically and heuristically, especially in comparison with the simultaneous marked progress of the rival big bang model, and finally had to be given up. It was the multiple convergence of recalcitrant data toward one particular element of the system, namely $\mathbf{B}_{SS}$, that allowed it to be identified as a strain recipient at an early stage. Breaking that element under the pressure of still more adverse evidence later on was fatal to the whole system.

7. Conclusion. The antiholistic strategy developed here is an attempt to provide a nonpragmatist account of scientific testing. Based on the string model of tests, I have argued that there are objective grounds for selecting a particular element of a theoretical system that should be modified or replaced in face of recalcitrant data. The convergence of multiple negative evidence on such an element, correlated with the empirical degeneration of the whole system modified in ways that leave this element intact, gives a reason to believe that this element is likely to be false, and not only a practical reason to discard it.

REFERENCES

Ariew, R. (1984), "The Duhem Thesis", *British Journal for the Philosophy of Science 35*: 313–325.

Bondi, H. (1957), "Some Philosophical Problems in Cosmology", in C. A. Mace, (ed.), *British Philosophy in the Mid-Century*. London: Allen Unwin, pp. 195–200.

———. (1990), "The Cosmological Scene 1945–1952", in B. Bertotti, R. Balbinot, S. Bergia, and A. Messina, (eds.), *Modern Cosmology in Retrospect*. Cambridge, England: Cambridge University Press, pp. 189–196.

Duhem, P. ([1906] 1954), *The Aim and Structure of Physical Theory*. Reprint. Translated by P. P. Wiener. Originally published as *La Théorie Physique: Son Objet, et sa Structure* (Paris: Marcel Rivière & Cie). Princeton: Princeton University Press.

Greenwood, J. D. (1990), "Two Dogmas of Neo-Empiricism: The 'Theory-Informity' of Observation and the Duhem-Quine Thesis", *Philosophy of Science 57*: 553–574.

Harding, S., (ed.), (1976), *Can Theories Be Refuted? Essays on the Duhem-Quine Thesis*. Dordrecht: Reidel.

Hoyle, F. (1948), "A New Model for the Expanding Universe", *Monthly Notices of the Royal Astronomical Society 108*: 372–382.

Kragh, H. (1993), "Steady State Theory", in N. S. Hetherington, (ed.), *Encyclopedia of Cosmology*. London: Garland Publishing, pp. 629–636.

Krips, H. (1982), "Epistemological Holism: Duhem or Quine?", *Studies in History and Philosophy of Science 13*: 251–264.

Lakatos, I. (1978), *Philosophical Papers*. Vol. 1, *The Methodology of Scientific Research Programmes*. Cambridge, England: Cambridge University Press.

Newton, I. ([1729] 1962), *Mathematical Principles of Natural Philosophy*. Translated by A. Motte and F. Cajori. Berkeley and Los Angeles: University of California Press.

North, J. D. (1965), *The Measure of the Universe: A History of Modern Cosmology*. Oxford: Clarendon Press.

Popper, K. R. (1959), *The Logic of Scientific Discovery*. New York: Basic Books.

———. (1963), *Conjectures and Refutations*. London: Routledge & Kegan Paul.

Quine, W. V. O. (1953), *From A Logical Point of View*. Cambridge, MA: Harvard University Press.

Quine, W. V. and J. S. Ullian (1978), *The Web of Belief*. New York: Random House.

Smith, R. W. (1982), *The Expanding Universe: Astronomy's "Great Debate" 1900–1931*. Cambridge, England: Cambridge University Press.

Vuillemin, J. (1987), "On Duhem's and Quine's Theses", in L. E. Hahn and P. A. Schilpp, (eds.), *The Philosophy of W. V. Quine*. La Salle, IL: Open Court, pp. 595–622.