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DUHEM, QUINE, AND THE MULTIPLICITY OF SCIENTIFIC TESTS

Yuri Balashov†‡

Program in History and Philosophy of Science
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 anti-holistic arguments of Popper, Greenwood, and Lakatos significantly reinforces their joint power

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‡ Current address: Department of Philosophy, 107 Peabody Hall, University of Georgia, Athens, GA 30602; yuri@arches.uga.edu

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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 to be 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 *anti-holistic* 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, 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" 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". (Quine and Ullian 1978, 32).

To be sure, 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 actually paralyzed by the holistic considerations. Scientists do succeed in making their decisions as to 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. (Duhem [1906] 1954, 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, Ch. 6 and 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 anti-holistic "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 shall show that various anti-holistic arguments they provide can be put together 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 unit of the model of testing sketched in the below 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 $\mathbf{I_1}$,... $\mathbf{I_n}$ (figure 1).

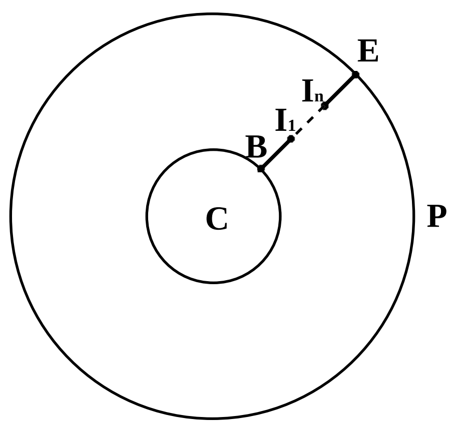

Figure 1. String model of testing.

This picture bears a family resemblance to the structure of the Lakatosian research program. Some important qualifications are, however, necessary. First of all, the nature of intermediary links $\mathbf{I_1}$,... $\mathbf{I_n}$ can be varied, depending on their location in a string. The links adjacent to the periphery (... $\mathbf{I_{n-1}}$, $\mathbf{I_n}$) are most likely to belong to the broad category of 'observational' and 'interpretative' theories. Those near the core ($\mathbf{I_1}$, $\mathbf{I_2}$, ...) tend to function as 'auxiliary theories'. Some of them may specify *ceteris paribus* clauses. Such auxiliaries are still very different from the element **B** immediately adjacent to the core, which provides the “boundary conditions”. The difference is due to the fact that $\mathbf{I_1}$, $\mathbf{I_2}$, ... can be disconnected from **C** whereas **B** cannot.

Indeed, $\mathbf{I_1}$, $\mathbf{I_2}$, ... may be totally 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*. **B**, on the other hand, is connected to the core “more intimately than by mere conjunction” (in the felicitous phrase of Lakatos (1978, 46)). It is only together with the boundary conditions **B** that the core **C** constitutes a *minimal* theoretical unit capable of being tested. Without the boundary conditions the core **C** is, in general, too abstract to give rise to any 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 *Principia*. It is only with the specification of the parameters $\mathbf{B_N}$ of the particular planetary system that the core becomes a *model* which can function as an instrument of computation, prediction and explanation. Clearly, the parameters at hand are something more than mere conjuncts. They are rather 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, interpretative, and auxiliary elements $\mathbf{I_1}$,... $\mathbf{I_n}$, on the contrary, is independent of any other elements of the system. The intermediary links $\mathbf{I_1}$,... $\mathbf{I_n}$ are not bound to conform to the model **C+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 above is heterogeneous in the following sense. Its intermediary elements $\mathbf{I_1}$,... $\mathbf{I_n}$ do not in general form a linear inferential chain. Most of them function singly, or in certain combinations, as “mere conjuncts”. The whole conjunction of them (or their combinations), which may be rather intricate, mediates the relation between **C+B** and **E**, which *is* inferential, unlike the relations between most of $\mathbf{I_1}$,... $\mathbf{I_n}$. However, some of ... $\mathbf{I_{n-1}}$, $\mathbf{I_n}$, ‘instrumental’ and ‘interpretative’ 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 2a). For the sake of simplicity, however, I shall henceforth adhere to the plain string pattern (figure 2b), bearing in mind that a typical string contains inferential, as well as non-inferential ‘conjunctive’ parts. Obviously they both contribute to the testing procedure in the same way: each of them can impair the whole string, either by blocking the inference along the inferential part of it, or by negating the entire conjunction in the “conjunctive” part.

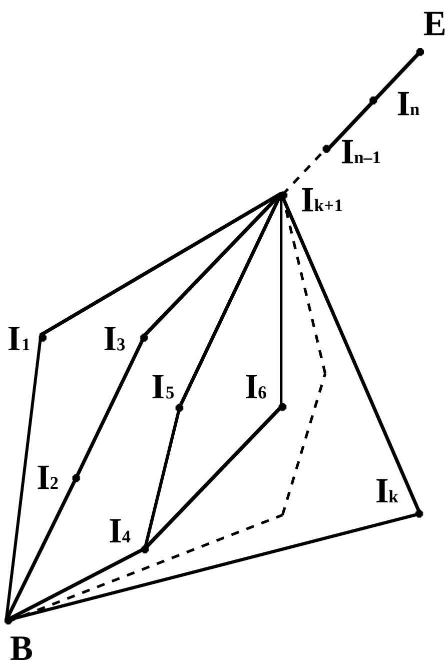

Figure 2a. Inferential and conjunctive elements of a testing procedure.

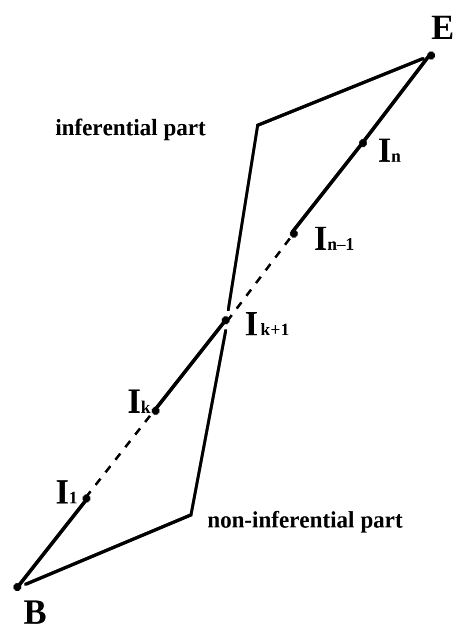

Figure 2b. An equivalent "string" representation of the test situation depicted in figure 2a.

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, what is to locate a weak element of a such a string? It is nothing else but showing that this element is truly "under strain" in the process of test, whereas other elements in a string are only *transmitting* strain to the impaired one. It was the common belief of Duhem and of Quine 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 any reason to believe that this element is likely to be false, but only a practical reason to abandon it. This is so simply because it is always the whole string, and not a particular element in it, that 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 is concerned with the refutation of this view. First, I examine the anti-holistic 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. A certain type of anti-holistic arguments, associated with Popper's falsificationism (1959, ch. 1; 1963, 238-9), 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 the number of the remaining *problematic* elements in a given negative-instance string may be more than one. 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 rather 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 simply 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 to be indispensable to any successful anti-holistic 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 anti-holistic strategy described in sections 4 and 5. I will show that the negative delineation of the probable "strain transmitters" is in fact 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.

B. Another anti-holistic argument was recently developed by Greenwood (1990). 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. The reason is that the theories under test ("explanatory theories") are themselves based on certain "exploratory theories" (... $\mathbf{I}_{\mathbf{n-1}}$, $\mathbf{I}_{\mathbf{n}}$, 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., 565-6), 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 anti-holistic claims different from, and, in a sense, stronger than, Popper's—their remarkable relation (which was strangely overlooked in the earlier literature on the "Duhem-Quine thesis") to a "core" theory's confirmation base. Because of this relation, "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" is simply wrong (Greenwood 1990, 567). Such a modification *cannot* always be made. This does not mean, however, that it can *never* be made.

Greenwood is right to note (1990, 569) 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 of all, 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, for example, North 1965, 46-47) to amend the law of gravitation in such a way that would explain Mercury's anomaly away *and* preserve all positive instances of celestial mechanics. All these attempts failed. In fact, none had looked very plausible in the first place—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 it also happens that the strain of negative evidence forces scientists to *break* a certain intermediary element in a testing string into two or more elements, one of which preserves the prior confirmation base of an "explanatory theory" and the others accommodate new recalcitrant data, in a fairly *legitimate* way. 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, for example, 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 fact in the island universe theory, it was later suggested that S Andromedae is, in fact, not an "ordinary" nova, but rather a *supernova*. This broke the "exploratory theory" of novae employed in support for a "core" theory into two different theories, one of which was left to account for the novae data and the other to explain away the recalcitrant supernovae data. However unattractive this step might seem at first sight, it proved absolutely correct.

Finally, it should be noted that theories may sometimes preserve themselves by *abandoning* some of their prior positive instances, together with a certain "exploratory theory", if this leads to *overwhelmingly* new support. In other words, the weight of the original support may finally 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 1840–50s (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 was made dependent on further resolutions. William Huggins put an end to these expectations in 1864, when he showed, with a new spectroscopic technique, that some nebulae (including Orion) are in fact glowing gases and, hence, could not be resolved. Together with some other counterevidence, this new discovery effectively marginalized the island universe theory for many years. Its advocates, however, after reconsidering its confirmation base, demonstrated that they can live with gaseous nebulae and still insist that other "nebulae" are true "island universes". The theory gained completely *new* and overwhelming support in the 1910–20s and proved able to handle other "anomalies" remaining from the past.

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

C. Lakatos's anti-holistic strategy is very different from Popper's and Greenwood's approaches. He endorsed 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 and should be abandoned or replaced in light of some counterinstance, until one tries several options in order to see which of them brings about content-increasing "problemshifts" and which of the latter advances us most. The option that actually 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 itself serves for separating the elements of a system into "weak" and "strong" ones. An element is *defined* as "weak" if its preservation at the expense of other elements leads to the external regress of the whole system, whereas rejecting or replacing this element can result in the empirical progress. Conversely, an element is *defined* as "strong" if its rejection more often than not leads to empirical degeneration. Obviously, the idea here 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 employed it, is that it *alone* was 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", see Lakatos 1978, 23-31). The whole 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 (that is, a research program) was 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 there is no real possibility of doing it! Furthermore, even if it were possible to try *everything* and if scientists in fact did it, the results of the trials would give them 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 may be rather willful and is not bound to represent any long-term tendency which alone is significant for epistemic evaluation.

In fact, the initial suggestions for role identification are never based on mere guesswork. In making these suggestions, scientists are guided by various considerations that, in general, include Popper's "independent corroboration", as well as Greenwood's "no-go" arguments prohibiting a modification of certain "exploratory theories", but also some other considerations that I will present in the next section. If these other considerations are put to work, 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 entirely independent of matters of empirical adequacy. But it is quite possible for it to 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 certain 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 fact has somehow to be explained. A reasonable explanation one can give of such a "correlation" is that the system at hand possesses certain epistemic merits. A successful anti-holistic 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.

4. Locating Weak Spots: The Multiplicity of Tests vs. the Wholeness of Theory. To take the bearing of a spy's radio station one needs at least two scannings made from two different vantage points. The intersection of the two straight lines will then give a position of the station. One scanning would be insufficient to do the job. 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 singles out a particular testing string in a theoretical system as containing a "weak spot", but it is insufficient to "locate" this element. However, two or more instances of testing can bring about the intersection of different strings at a certain point. In figure 3 this point is identified with the element **I** common to both testing strings.

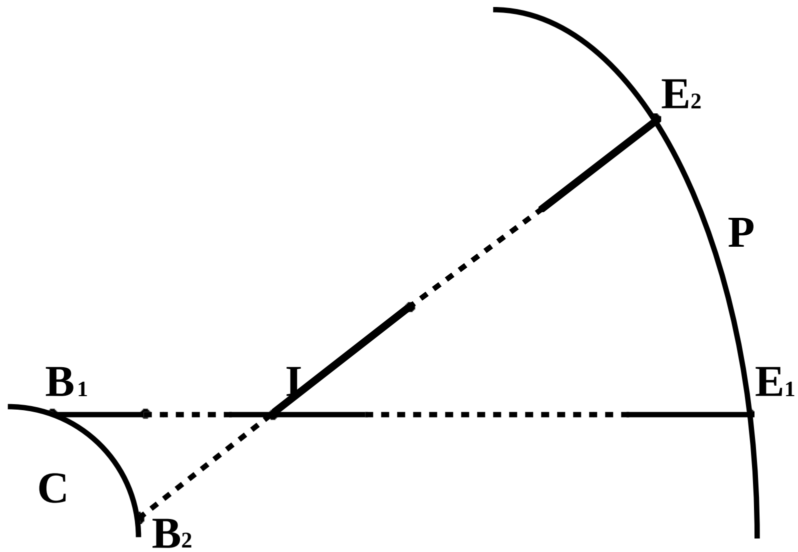

Figure 3. Intersetction of testing strings at an intermediary element **I**.

An intersection of testing strings of the kind shown in figure 3 is by no means a general feature of scientific tests. It is not bound to occur every time the tests are conducted. However, it can be expected in a theoretical system dealing with multiple types of evidence. For example, before Maxwell, certain theories regarded light as a wave process of unspecified nature. An auxiliary hypothesis that this process is in fact *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 center of intersection of testing strings corresponding to these two kinds of *positive* evidence. In Maxwell's theory, of course, a similar proposition (*viz*., that light is a transverse wave process) served not as an auxiliary assumption but as an element integrated into the core. The case of intersection corresponding to Maxwell's theory is depicted in figure 4. Figures 3 and 4 exhaust all possible kinds of intersection of the testing strings. Such an intersection can occur either at some intermediary element **I** (as in figure 3) or at a "boundary" element **B** (as in figure 4).

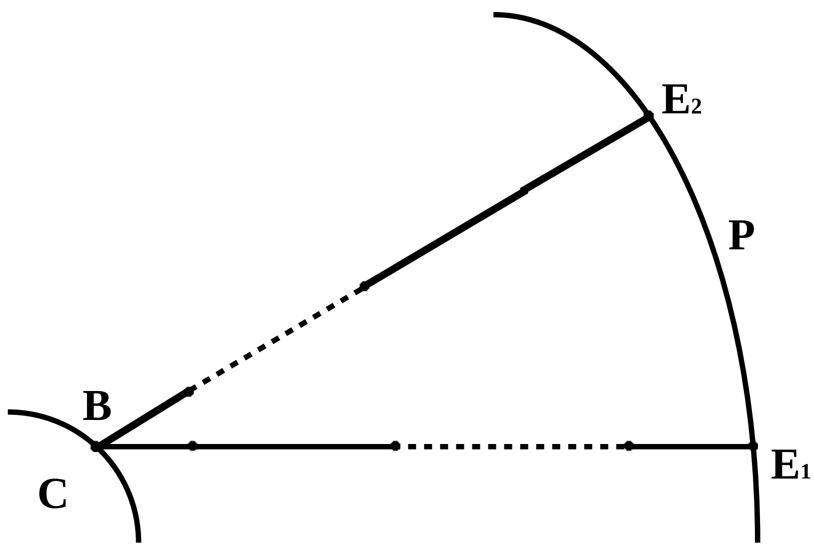

Figure 4. Intersection of testing strings at a "boundary" element **B**.

It should be emphasized that an intersection of the two strings implies nothing remarkable by itself. It only means that one and the same element is involved in two instances of testing, which is only natural. The situation is different if both instances turn out to be *adverse* to the system and each of them forces an adjustment somewhere in the corresponding string. Obviously, such an adjustment can be carried out in a variety of 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 3) 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 still other 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.

What makes this argument different from the Lakatosian approach? It is precisely 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 give initial suggestions as to the appropriate role identification. Such suggestions, then, have to be verified by checking empirical behavior of the system. The thrust of the combined anti-holistic strategy derives from a *correlation* that can be established between the convergence of multiple negative evidence toward one particular element, manifested in its being the center of intersection of the corresponding testing strings, and empirical regress of the whole system modified in the way that leaves 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 non-circularly. These are two different criteria: one of them 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 in precisely what way these criteria are different and not reducible to one another. To be sure, the "convergence" criterion is not entirely independent of matters of empirical adequacy, for strings intersecting at a certain point relate to instances of empirical predictions that proved wrong. But the fact that they intersected at one particular point, rather than at another, within the system, is a consequence of the particular relations 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 counterinstances leading in their multiplicity to a convergent pattern can be suggestive of role identification, and it is such counterinstances that are the premises of the holist problem. Positive instances are of no interest at this point. They cannot be suggestive of 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 were able to accommodate the recalcitrant results of the Michelson-Morley experiment in many ways, for example, by invoking an auxiliary hypothesis (**I**) of a partial drag. This, however, could only be done at the cost of 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 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 3 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 5).

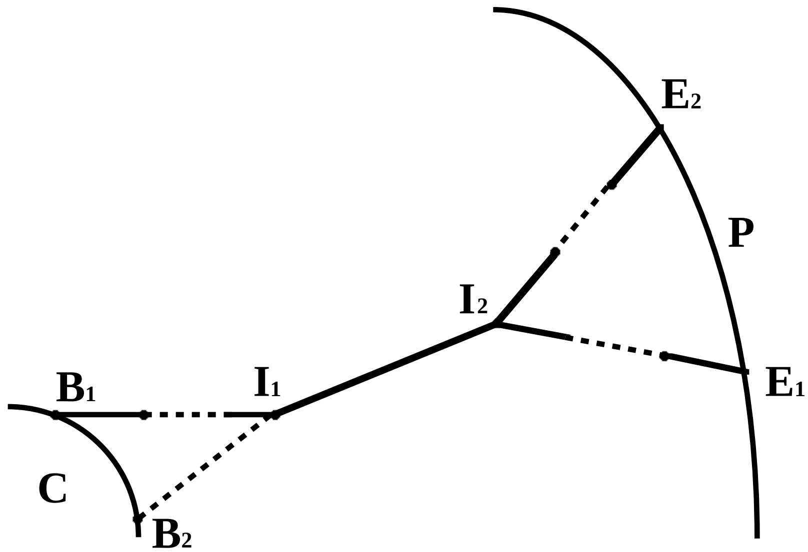

Figure 5. Intersection of strings B_1–E_1 and B_2–E_2 comprises two elements I_1 and I_2. In case of paradoxes of the Newtonian cosmology these elements are:

I_1: The universe is infinite in space;
I_2: The universe is infinite in time.

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 of them 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 I_1 and I_2, or rather their conjunction I_1 & I_2, a "weak spot". In reality, scientists chose a different route. They attempted to remove the tension by adjustment of 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 I_1 & 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 have a clearer understanding of how this can happen, it should be noted that, although positive instances of testing cannot help in locating a weak spot in the first place, they can become important later on, in deciding 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, it is necessary to examine how the system can respond to tests. Because of the various connections between the elements of the system, such a response would, in general, 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 3. The pressure of counterinstances typically force an adjustment of a certain parameter in **I**, though not always in a coherent way. For example, different adverse data may pull in opposite directions. If the parameter is adjustable, it could 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 would have to give in favor of some alternative theory **I*** capable of coherent strain removal.

In deciding what exactly should be done with **I**, due account must be taken of the positive instances involving **I**. Here Greenwood's and Popper's considerations become very 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 at this stage.

Suppose that the whole procedure of mutual 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. On the other hand, if the system becomes degenerating 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 one's initial decision to tamper with **I**. Without ingenuity, even in the presence of progress, there would be a deadlock. The only way out would be to acknowledge the *anomalies* generated in other parts of knowledge by the adjustment of **I** in the first place and to decide to tolerate these in the hope that ultimate understanding will come later on (cf.: Lakatos 1978, 49-52). Obviously, this *pragmatic* decision cannot repeal the epistemic sentence passed on **I**. It has been rightly declared 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.

One can try another option and replace the old compromised element **I** with a new one **I***, instead of simply adjusting some parameter in the former. **I*** may 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 taken care of in a way similar to the one just outlined. In some cases, coherent strain removal will only be achieved 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 again the original problem more carefully, before making a final decision. It might be that the element **I** that has been at first considered (with due reason) a weak spot is in fact 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 completely outweighed by these new counterarguments. This means that the convergence of adverse evidence toward **I** is presently insufficient as compared with its degree of entrenchment. Perhaps one could get round the difficulty more successfully, by making a suitable adjustment elsewhere in the system, the same accounting technique being applicable in this case as in the former one.

It might also be that the entrenchment of **I** is slight, whereas the convergence of recalcitrant data toward it is, on the contrary, impressive. Then one has every reason to discard **I**, but one is simply unable, at the moment, to provide a new and ingenious alternative. One's inability to cope with the problem has nothing to do with the role identification. The latter is beyond suspicion, and it is clear that one has got a typical *anomaly* now associated with **I** itself. One's only choice will be to decide to live with it for the moment.

Notice that all the options mentioned above essentially include arguments that were originally based on intratheoretical considerations and are 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 is, of course, the system's empirical dynamics. The mutual application of both criteria endows the original judgments with the power required to mollify a holist.

One could object that a decision to retain an anomaly, mentioned above among the alternatives, 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. Doing so would reveal a serious underestimation of the power inherent in the "string analysis". The scope of the trial, the sequence of steps in it and, what is more important, its starting point, can reliably be determined by such an analysis.

The situation depicted in figure 4, 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** (figure 4), differs from that considered above in one significant respect. 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 it is only through the boundary condition **B** that **C** can 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 it does not, or if the adjustment tactic leads to no success, one would badly want to replace **B**, or break it into $\mathbf{B_1}^*$, $\mathbf{B_2}^*$, ..., by analogy with **I**.

However, one cannot do it, for **B** is not a detachable theory or hypothesis that can exist separately from the core as an independent unit of cognitive significance.

Yet the effect of a replacement can be attained by tampering with the whole complex **C+B** constituting the model. As noted by Lakatos, "any theory can be saved from counterinstances either by some auxiliary hypothesis or by a suitable reinterpretation of its terms" (Lakatos 1978, 32). It is precisely by the ingenious and heuristically progressive reinterpretation of **B** *within* the model **C+B**, that **B** can be successfully turned into $\mathbf{B^*}$ or broken into $\mathbf{B_1^*}$, $\mathbf{B_2^*}$, ... Again, any particular decision with respect to **B** suggested by the pattern of string intersection will have to be tested against the progress of the modified system.

6. Discussion. To illustrate these points, let me 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, $\mathbf{t_U} \approx 2x10^9$, in the standard relativistic model. This was in drastic contradiction with the estimated age of the Earth $\mathbf{t_E} \geq 3x10^9$ years (the "time-scale problem"). Owing to the same misidentification, our galaxy also turned out to be far larger than any other observable "nebula".[1] This conclusion ran counter to 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 (the time-scale discrepancy and the improper size of our Galaxy) thus intersected at a common auxiliary element **I** (see figure 3) 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 $\mathbf{t_U \sim H^{-1}}$ (**H** being 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 that the actually observable Hubble parameter **H** did not relate in any simple way to the age of the universe.

The lack of progress of relativistic cosmology 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 call Hubble's data into question (though the intratheoretical considerations, as we noted, clearly suggested it). Why? The "external" circumstances definitely played a role here. First, it was not possible to check Hubble's data independently, for no instrument comparable to the Mount Wilson 100-inch reflector existed elsewhere in the world. Second, Hubble's authority was indisputable. The *prima facie* suggestions that could, in principle, have been read off the string pattern of the situation ca. 1930 were acted upon only in the 1950s, when Hubble's distance scale was explicitly shown to be severely underestimated.

One can look at the same situation from another viewpoint. A reasonable estimation of the age of the oldest stars in the universe, $\mathbf{t_S} \geq 5x10^9$ 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 all the data available at that time, there was no doubt that $\mathbf{B_{BB}}$, and not any other elements in the testing strings related to geophysical and astrophysical data, was a strain-recipient, despite the fact that a great deal of theoretical baggage was involved in interpreting the relevant geological and astronomical observations. No part of this baggage was above suspicion. However, it was $\mathbf{B_{BB}}$ that was actually called into question. Why? One definite reason was the convergence of recalcitrant evidence toward it.

This situation gave rise to very different responses. One of them, considered above, was to invoke special evolutionary models with the Λ-term. Another way to proceed was to recognize the current value of $\mathbf{t_U}$ as anomalous but to continue to adhere to the simple model in the hope that the anomaly would somehow be resolved in the future. Such a decision was, in fact, implicitly adopted by the majority of the cosmological community. It was purely pragmatic at that time 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 ($\mathbf{t_U}=\infty$) was in accordance with both kinds of data that gave the big bang model trouble. 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, which prompted 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 some clarification. The violation of exact conservation of energy required by the model was extremely small and did not run counter to any available experimental evidence. In other words, all positive-instance strings involving the conservation law as an *intermediary* element remained intact, and it was Bondi's intention to downgrade this law to the status of a purely empirical regularity that is not bound to be exact (Bondi 1957, 198). 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 did adopt a “reconstruction” strategy in his (1948). 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 anti-holistic strategy developed here is an attempt to provide a non-pragmatist 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.

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Footnotes

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.