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MEANING AND TESTABILITY IN THE STRUCTURALIST THEORY OF SCIENCE

ABSTRACT. The connection between scientific knowledge and our empirical access to reality is not well explained within the structuralist approach to scientific theories. I argue that this is due to the use of a semantics not rich enough from the philosophical point of view. My proposal is to employ Sellars–Brandom's inferential semantics to understand how can scientific terms have empirical content, and Hintikka's game-theoretical semantics to analyse how can theories be empirically tested. The main conclusions are that scientific concepts gain their meaning through 'basic theories' grounded on 'common sense', and that scientific method usually allows the pragmatic verification and falsification of scientific theories.

1. INTRODUCTION

Structuralism is probably the best account we have of the nature of scientific theories. Concepts such as 'T-theoreticity', 'intended application', 'empirical assertion', 'theory net', or 'theoretical link' have provided very deep insights on the structure of scientific knowledge, and can be counted amongst the most significant recent contributions to the philosophical study of science. Nevertheless, it is disappointing how little has structuralism to offer in order to illuminate one of the most important topics in contemporary epistemology: the problem of the empirical testing of scientific theories. Sneed's original analysis of theoreticity in *The Logical Structure of Mathematical Physics* was a very promising beginning, for it made it clear how was it possible to express the (empirical) assertion of a theory containing theoretical terms as an assertion just about systems whose description contained none of them; the empirical test of a theory would require, hence, only the measuring of its non-theoretical quantities, the values of its theoretical ones being determined afterwards by 'internal' means. The problem was that, if all scientific terms are 'theory laden', these non-theoretical concepts would presuppose other, 'more fundamental' theories, and this seemed to lead, either to an infinite regress, or to some kind of circularity, or to the existence of 'bed rock' theories (i.e., those containing no concept which is theoretical with respect to a still

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more fundamental theory). The first of these three possibilities is easily discarded since the number of actual scientific theories is always finite. Balzer, Moulines and Sneed (from now on, BMS) rejected also the last one in the last chapter of *An Architectonic for Science*, arguing that if bed rock theories did not have non-theoretical terms, then their 'partial models' would be systems with domains but without relations nor functions, and this would make their 'empirical assertions' vacuous or arbitrary. As a result, these authors opted for admitting the possible existence of 'loops' in the 'ordering' of theories by the presupposition relation, something rather appealing from the point of view of a coherentist epistemology, but which also tended to make it more obscure the connection of scientific constructions with our perceptual experience.

Two different but related problems can be pointed out. In the first place, BMS's coherentism leaves unexplained what it is for a scientific concept (or, by the way, for a scientific theory) to have 'empirical content'. Structuralist definitions of T-theoreticity explained clearly what is it, for a concept, to be dependent on a particular theory, but, if the normal case in science is that theories constitute 'holons' with no first elements in the relation of presupposition, it is not clear how to make a distinction between those 'theory holons' which are 'empirical' *in some sense or another*, and those abstract theoretical systems which 'float' without any connection whatsoever with our empirical access to reality. In the second place, from the structuralist analysis of the 'empirical assertion of a scientific theory' (with or without 'loops') one would expect to find some indications about what could a scientist *do* in order to decide whether that theory is or is not acceptable on empirical grounds, or whether it is more acceptable than other rival theories. Truly enough, BMS assert that "the aim of [our] conceptual framework is not to attack the unsolved problems of confirmation theory",[1] but one does not see any reason to be so unambitious once that conceptual framework has proved to be so powerful when applied to other philosophical problems. Some attempts of enriching the structuralist approach with a methodological flank have been made before, particularly with the help of the theory of verisimilitude,[2] but I will now try a more empiricist strategy, since the general aim of this paper is to illuminate the connection between abstract scientific theories and our perceptual experience.

An underlying hypothesis in this paper is that the difficulties indicated above are essentially due to the use of a too 'extensionalist' semantics (model theory), which has been very helpful for analysing *structural* features of theories, but much less so for illuminating the *pragmatics* of empirical concept formation and theory assessment. Without departing from a semantic approach to scientific theories, this paper suggests to complement the use of more classical formal tools with other semantic insights. In particular, I will make use of Wilfried Sellars' and Robert Brandom's *semantic inferentialism*, and of Jaakko Hintikka's *game theoretical semantics*. As it will become clear, I think that the main virtue of these approaches is that they transparently show the connection between the *semantic* and the *pragmatic* aspects of our representations of the world, something model theory tends to make abstraction of. Inferentialism will be used to throw light on the first problem stated in the previous paragraph: *what does it mean for a scientific concept to be 'empirical'?* Hintikka's approach will be used, instead, to offer a tentative answer to the second problem: *what can researchers do in order to decide whether the 'empirical assertion' of a theory is acceptable or not?*

2. OBSERVARTIONAL CONCEPTS: AN INFERENTIALIST APPROACH

2.1. *In Defence of 'Bed Rock' Theories*

Structuralist theory of science offered a new way of looking at the problem of theoreticity. According to Sneed and his followers, the 'theoretical' does not oppose to the 'observational', for it always means 'theoretical-with-respect-to-a-particular-theory', and hence, the right contrast is between 'T-theoretical' versus 'T-non-theoretical', where 'T' stands for a specific theory. Given the widespread acceptance of the 'theory-ladenness of observation', Sneed's analysis may have led to conclude that the very idea of an 'observational' term is vacuous, or at least, irrelevant for the philosophical study of empirical sciences. In structuralism, the empirical entered the content of theories through their *intended applications*, rather than through observational concepts; but these applications had to be described using some conceptual structure, which presupposes other, more 'basic' theories, which had their own intended applications, which presuppose other theories, and so on. As I said before, BMS rejected the existence of bed rock theories (those theories which do not presuppose still more basic ones) by arguing that, for a theory to have a non vacuous empirical assertion, its intended applications must be described with the help of relations and functions which are non-theoretical with respect to that theory; but the terms of a bed rock theory B can not be theoretical with respect to other theories (for in that case, the former would not be 'bed rock'), and hence, they have to be theoretical with respect to B (assuming that all concepts are 'theory laden', and, by the sake of the argument, that the presupposition relation is antisymmetric); as a result, there will be no concepts that can empirically describe B's intended applications,[3] and so, B could not have an empirical content. BMS concluded, then, that the only way of warranting that scientific theories make non trivial empirical assertions is by allowing the relation of presupposition among theories being symmetric in some cases: the empirical assertion of a theory T would employ terms which depend on T', whereas the intended applications of T' are described with terms which are proper of T.

This is perhaps a reasonable story when we interpret it as an explanation about the *meaning* of scientific terms. So understood, it would exemplify a kind of *semantic holism*, according to which one needs to understand *many* concepts in order to understand *any* concept; I have nothing to oppose to this interpretation, since I will defend here a different species of semantic holism. But we have not to forget that the notion of a theory's 'empirical assertion' is not only necessary for explaining the theory's *conceptual structure*, but also for explaining the possibility of testing the theory. When we take this into account, BMS's argument transmogrifies into a dangerous circularity: in order to test T, we have to describe its intended applications with concepts which presuppose the validity of T', but we can not test whether T' is valid if we have not described its own applications with the help of concepts which presuppose the validity of T. So, *in the order of testing* (while probably not in the order of *meaning*), the relation of presupposition can not be symmetric in general, if we want empirical testing to be possible. We can turn BMS's argument on its head and assert that, since the empirical testing of scientific theories demands that there are some loop-free ways, some bed rock theories *must* exist. The rest of this section is devoted to show that this conclusion is coherent both with the structuralist analysis of theoretical terms and with the thesis that all empirical knowledge is essentially conjectural.

2.2. *Two Structuralist Approaches to Theoreticity*

In the past section I have used the notion of 'T-theoreticity' in a very informal way. Actually, one of the main tasks of structuralists has been to explicate what does it mean exactly for a concept to 'presuppose' or to 'belong essentially' to a specific theory. Basically, two different approaches have been followed. Sneed's original account offered a *pragmatic* criterion of theoreticity (a concept is theoretical with respect to T if any *actually employed* determination procedure of that concept presupposes that some real physical systems are actually models of T); this approach was followed by Stegmüller and Moulines.[4] On the other hand, several authors, mainly Gähde and Balzer, proposed purely *formal* definitions of theoreticity, which in principle would only require to examine the theory's set-theoretical predicate (and basically its mathematical invariances) to decide which of their concepts essentially depend on the equations occurring in that predicate.[5] Both approaches seem to have some good reasons behind, and both have been subjected to several criticisms. For example, Balzer has argued against Sneed's approach that, in practice, we usually cannot examine all existing measurement procedures of a magnitude in order to decide whether it is T-theoretical or not, and also that the application of Sneed's criterion to those cases where all the terms of a theory are T-theoretical would make its empirical claim vacuous.[6] On the other hand, formal criteria are not easily applicable to theories outside fundamental physics (where mathematical invariances do not play a clear role), and they lead to some weird conclusions: for example, Gähde's approach may induce different partitions between theoretical and non-theoretical terms within a single theory, whereas Balzer's definition allows for a concept being theoretical with respect to more than one theory.

In spite of their differences, the two approaches are not really conflicting, at least in the sense that they are not mutually contradictory, although they are not equivalent either, for they do not always generate the same distinction between theoretical and non-theoretical terms.[7] Rather, they simply offer two different ways of explicating the notion of 'a scientific concept being "dependent" on a particular theory', and the differences between all the proposed criteria would then be due to the polisemy of the word 'dependence'. But not all possible specifications of that concept are necessarily on a par from the philosophical point of view. A definition of theoreticity will be more or less interesting depending on whether, in the first place, the existence of real scientific concepts satisfying that definition leads to serious epistemological or methodological problems, and in the second place, if the definition shows some fruitful strategies for solving those problems. Otherwise, devising a new definition of theoreticity would just be like 'having a solution in search of a problem'. From the point of view adopted in this paper, the fundamental epistemological question about theoreticity is how the presence of T-theoretical terms in a theory makes it more difficult its empirical testing, and I think that, in spite of its possible shortcomings, Sneed's criterion of theoreticity is the one which best captures the *specific* dependence relation between concepts and theories which makes theory testing problematic. Other types of dependence between a concept and a theory can be proposed and explicated, but it is not clear for me how they can help us in solving *this* epistemic problem, although I do not prejudge the usefulness those criteria may have for solving *other* meta-scientific problems (for example, problems regarding the role of invariances in physical theories).

In particular, purely formal criteria can hardly be employed to illuminate the relation which links scientific concepts to *observation*, i.e., to explain what makes of a concept an *empirical* one: imagine a theoretical system which is isomorphic to a real scientific theory (say, classical mechanics), but whose 'intended applications' were composed out of angels (instead of 'particles'), and of a three dimensional function describing three imaginary properties angels may have (instead of 'position'). Any conclusions the formal criteria of theoreticity allowed to draw about the *non-theoretical* character of this strange 'position' function (with respect to our imaginary 'angel mechanics' theory) would indicate absolutely nothing about the *empirical or non-empirical* character of it. The Sneedian approach is more promising, at least because the existence of certain measurement procedures for that function is relevant to determine with respect to what theories it can be theoretical or non-theoretical, *and also* to determine whether it is a function with empirical content or not.

The *methodological* problem of theoreticity relates to our ability to justify non-tautological assertions containing T-theoretical terms. For example, if mass is theoretical with respect to classical mechanics *in the Sneedian sense*, how can we reasonably *know* that the mass of a given car is, say, about 2,000 kilograms? All we have is a series of apparatuses (for example, a weigh-bridge) which produce, probably after some calculation, some numerical readings depending on the objects to which they are applied. But how do we know that the resulting numbers can be interpreted as *masses*? Only because we know that those apparatuses behave according to some laws of classical mechanics. But we can only know *this* after actually measuring *some* masses and forces. Sneed showed that, in order to avoid a circularity here, it was necessary to interpret the assertion of a scientific theory as a *global* claim about a set of physical systems ('partial models') *which can be described without using the concepts dependent on that theory* (for example, in the classical mechanics its intended applications would be purely cinematic systems); this claim is what is called the 'Ramsey-Sneed sentence' of a theory, which is the assertion that there are some theoretical functions which added to the theory's intended application, produce some abstract systems which are actually models of the theory (see Section 3 below). The precise form this sentence must have in the logical reconstruction of a scientific theory has been elaborated in detail by structuralists, in parallel with the discussions about theoreticity (for these discussions essentially related to the nature of 'partial models', those, according to Sneed, not containing theoretical concepts), and it demanded the elucidation of concepts such as 'theory-net' and 'theoretical link'.[8] Nevertheless, I will not use here all those formal complications, for my aim here is limited to the following problem: how can we reconcile the thesis that some (and probably all) scientific concepts 'depend' upon some theory (in more or less the Sneedian sense) with the idea that most scientific concepts are directly or indirectly connected to human experience?

Although Sneed's theoreticity criterion has been criticised for being too 'pragmatic' (cf. note 6), my strategy in the remainder of this section will be to argue that, in order to solve the problem just mentioned, we need to develop a still more 'pragmatic' version of that criterion. As we have seen at the beginning of the past paragraph, one virtue of the Sneedian approach is that it clearly shows that the problem of theoreticity is not a mere philosophical invention, but a real difficulty existing in scientific practice. Our question is whether the definitions offered within this approach are useful enough for illuminating the connection between scientific concepts and human experience (for purely formal approaches are not very helpful – recall the 'angel mechanics' example). In the next subsection I will comment on Balzer's criticisms to Sneed-like definitions of theoreticity, and I will also point to some additional difficulties. The remaining part of this section will be devoted to show how can inferential semantics help in solving these shortcomings.

2.3. *Why the Pragmatic Criterion of Theoreticity Is Not Pragmatic Enough.*

BMS present the following 'naïve' definition of theoreticity (p. 50):

(1) a concept (...) will be called theoretical with respect to theory T if all methods of measurement involved in its determination have to be conceived as models of T.

The 'informal' or 'pragmatic' criterion of theoreticity which BMS offer as a reconstruction of that naive definition is the following (p. 68):

(2) "t is *T-theoretical* iff (...) for all x: if x is an adequate t-determining model (...) then x (is a model of T)",

where x is a *t-determining* model if it satisfies a scientific formula which makes that, given the values of the other functions in x, the values of t are uniquely determined, and it is an *adequate* t-determining model if that formula is actually employed in scientific practice. So, what (2) basically adds to (1) is a clearer description of what a 'method of measurement/determination' is. As I stated in the previous subsection, the most important methodological problem regarding Sneed's criterion is *not* whether it is an 'adequate' definition of theoreticity (i.e., whether it fits or not somebody's 'intuitions' about what theoreticity is), but *whether important scientific concepts actually satisfy it or not* (for, *if* they do, then the empirical testing of some theories will be problematic). Obviously, the question is to determine if there are concepts which are T-theoretical according to (2). As we saw, the main difficulty for doing this is that the set of existing measuring methods for a given magnitude can be very large, and even not well defined (for example, there can be some procedures which are only used to measure some *other* magnitudes, but which *could* be employed to determine the values of the first one in an indirect way; should we take into account these methods, even if scientists do not actually use them?). The set of measurement procedures can also change, making that a T-theoretical concept ceases to be so, or viceversa; hence, the criterion seems to require a temporal index attached to it. As we have seen, Balzer and others have tried to solve this problem by eliminating all the 'pragmatic' aspects of the naive criterion of theoreticity. I assume, instead, that these aspects are necessary in order to understand what makes of a scientific concept an empirical one.

Balzer has also pointed out some other difficulties with the application of Sneed's criterion.[9] In the first place, it can be very complex due to the sophisticated mathematical form of some theories. In the second place, for theories whose terms are all T-theoretical, the criterion makes T's empirical claim almost vacuous. And in the third place, for theories whose terms are all non-T-theoretical, the testing of their empirical claims would demand the determination of the full range of all their functions. From my point of view, the first problem is not solved by moving to a purely formal criterion, for the application of this can be as difficult as that of Sneed's; instead, we will see that a more pragmatic criterion partly avoids the difficulties which are due to the formal complexity of the theories. With respect to the second point, I agree with Balzer that part of the problem is due to collapsing the T-theoretical/T-non-theoretical distinction with that between 'potential' and 'partial' models, and I will actually follow Balzer's approach to interpret the empirical assertion of theories which only contain theoretical terms (as it is the case for the 'basic theories' I will present below). Lastly, the third problem seems to have nothing to do with the possibility that every term in a theory is non-theoretical: even if only some of its terms are theoretical, the theory's claim will be about a set of structures with perhaps some infinite domains. I think this problem refers instead to the question whether the intended applications of a theory (the things the theory talks about) have to be taken as finite data structures, or as (usually infinite) real physical systems whose properties are tested by means of finite data models. We will see in Section 3 that in practice, researchers test scientific theories by making claims about *types* of empirical applications, which usually contain an infinite number of individual models; but if there is a problem here, it has to do with the possibility of induction, not with theoreticity.

On the other hand, the pragmatic criterion (2) was an attempt to partially 'de-pragmatise' (1), not only by clearly determining what a 'measurement procedure' is, but also by substituting in (1) the expression "if all methods of measurement (...) *have to be conceived* as models of T" for the expression "if all methods of measurement (...) *are* models of T". My strategy will be, instead, to interpret 'have to be conceived' in a more pragmatic way. This is in part due to other two difficulties with (2): in the first place, if we take into account that (as Popper suggested) probably no interesting scientific theory is literally true, then it will turn out that no existing method of determination of any concept will be an actual model of any relevant theory; in a more general way, *no scientific concept could ever be theoretical with respect to a false theory* (one whose intended applications cannot be appropriately expanded to actual models).[10]

In the second place, if we take (2) as asserting that the accepted descriptions of the real t-determining methods satisfy a formula A which makes them to be models of T (if the formula were true, which is perhaps the case only to a degree of approximation), then, these descriptions will also satisfy any logical consequence of T; for example, they will satisfy any tautological proposition (*Taut*). Will t have to be taken as *Taut*-theoretical as well as T-theoretical? On the other hand, the accepted descriptions of those determination procedures will probably entail more than just the axioms of T; let T^* be the conjunction of *all* the propositions which are accepted about these procedures (including, for example, the proposition that the measurement of t – if it stands for a magnitude – has always a margin of error bigger than e, where e is the minimum margin of error of all the existing measurement processes, or a very small fraction thereof). According to this reading of (2), f should also be taken as T^*-theoretical; in fact, t would be theoretical with respect of any proposition S lying 'between' *Taut* and T^* (in the order of increasing logical content), even if some of these S's stand in no logical connection with T. The question is, hence, why should T have some special *epistemic or semantic 'privilege'* over these other propositions? In the next sections I will argue that reading Sneed's criterion through the light of semantic inferentialism allows to give a natural answer to this question, an answer which, I guess, was already contained (more or less implicitly) in that criterion.

2.4. *An Inferentialist Definition of Theoreticity*

According to semantic inferentialism,[11] understanding the meaning of an expression amounts to being able of offering *reasons* which (would) justify the acceptance of the sentences where this expression occurs, as well as of drawing appropriate *conclusions* from those sentences. Stated differently, understanding an expression amounts to mastering its role 'in the game of giving and asking for reasons', to use Wilfried Sellars' metaphor. Concepts are identified, hence, by their *inferential* role, by the 'right' inferences they allow to make. Robert Brandom has recently developed this idea, insisting in its *pragmatic and normative* aspects (e.g., those lying behind the notion of 'right' inference). I will not attempt to offer here a summary of Brandom's impressive work, whose full application to the philosophy of science would demand much more work; my aim is simply to explain how it allows to understand what does it mean for a scientific concept to be 'empirical'.

According to Brandom's approach, making an assertion (as opposed to merely producing a series of noises, for example) amounts to allowing other speakers to attribute to you a *commitment* to some consequences of that assertion (these 'consequences' may be other assertions, or they may be actions), as well as a commitment to providing reasons that serve to *justify* your assertion. Only if one fulfils these commitments (or otherwise manifests his acceptance of them) will he be *entitled* to that assertion; Brandom's theory demands that certain entitlements are gained 'by default', i.e., depending on the circumstances, you will be entitled to certain assertions unless this entitlement is *appropriately* challenged by others. The links between an assertion and its possible reasons, and between it an its possible consequences, are determined by the set of *materially correct inferences* which are accepted by the members of the relevant community. For example, you will be entitled to assert that 'city X is to the north of city Y' only if you provide a reason when you are asked for it (for example, showing it on a map), and only if you also show that you accept that 'city Y is to the south of city X' when asked about it. The 'inference' from seeing something on a map to expressing it, and the inference from 'A is to the north of B' to 'B is to the north of A' are 'materially correct' in the sense that they are accepted as right moves in the linguistic game the speakers are playing.[12]

My suggestion is that (to employ Brandom's jargon) we can try to make it explicit the normative aspects which are implicit in (1) and (2). The basic idea is to interpret the expression '*have* to be conceived' in (1) as referring to a *linguistic norm*, *i. e.*, to an *obligation* established by a scientific community about the use of a particular concept. The criterion would state thus something like the following:

(3) t is T-theoretical iff: (a) if you are using t in a system x, and if you do not take x to be a model of T (either explicitly or implicitly), then you will be sanctioned for not understanding 'correctly' the meaning of t; and (b) T is the strongest theory for which (a) is true.

After explaining how (3) solves the problems indicated in the past section, I have to justify why it can be taken as an 'inferentialist' criterion. The reason is not only that it refers to the normative attitudes associated to the language game one is playing when using the term t; the fundamental point is that, in order to publicly show that you master the use of that term, you have to draw the 'appropriate' *inferences* from its use in the description of system x, and these inferences are just the ones which are commanded or allowed by theory T (they can either be inferences from one part of x to another part – due to x being a model of T, – or from x to another structure – due to x belonging to a set which satisfies T's constraints and links[13]). Furthermore, T may contain both 'analytic' statements and 'factual' propositions. We know from Quine that no clear division can be made between both types; according to Brandom, the difference which is important is the one between those inferences that you *must* commit to, and those that you *do not need* to make. In particular, T can usually contain *empirical regularities*. For example, in the case of colour terms, if you assert that something is red, and also recognise implicitly or explicitly that it is green, the other speakers may conclude that you have not understood properly the use of colour terms. So, until you have not learned things like 'if one spot is red, it is not blue', or 'if you see two things with the same colour, nobody will see them with different colours', you will not be really entitled to commit to assertions which employ colour terms. All this entails two important things; in the first place, *you have to learn some empirical regularities in order to become a player of any language game*; in the second place, *these regularities must be 'public', in order that all the players accept the same rules.* Unless there is a set of regularities on which the speakers of a language can rest to establish what are the inferential connections between some assertions and others, the game of language can not even begin.[14] On the other hand, since the essential element in (3) are the *social obligations* which lie behind the use of a term, it is clear that not all of these obligations will have the same force; this makes it still more difficult to draw any clear distinction between 'analytic' and 'factual' propositions.

With respect to the problems indicated about definition (2), in the first place (3) does not demand to inspect all t-determining methods prior to deciding whether t is T-theoretical or not, for what is relevant is to notice the *normative attitude* of a scientific community towards the use of t; this attitude must be fully accessible to the community members, for they learn it by learning to use the language game associated to T; thus, discovering that attitude will not be more difficult for the meta-scientist than discovering any other aspect of the scientific community he is studying. On the other hand, (3) needs to make no reference to special sets of models, such as 'measuring procedures' or 't-determining models', for a misunderstanding on the 'right' meaning of t can appear while one is using any potential model of the theory.

Second, the complexity of a theory is not an obstacle to knowing what of its terms are theoretical (save for the difficulty of learning the theory in the first place), although it may make it very difficult the logical reconstruction of the theory and its claim. But determining whether a term is T-theoretical – according to (3) – is something you can perfectly do once you become a 'native speaker' of the relevant scientific community's language, and *before* engaging in any structuralist reconstruction.

Third, some terms can be theoretical with respect to false theories, if these theories are needed to derive consequences from propositions containing those terms. The only difference with the terms which are theoretical with respect to valid theories is that the former will mostly lead to *counterfactual* consequences.

Lastly, t is T-theoretical (and not with respect to other theories, either logically stronger or logically weaker than T) just because of condition (3.b): if t is T-theoretical, then T is the *strongest* theory you *have* to accept in order to be entitled to use the concept t in the description of a physical system.

In a nutshell: the theoreticity of a concept points to the rules of the linguistic game associated to that concept. From this it follows very clearly that *every concept will be theoretical with respect to some theory*, namely, the theory that resumes all the inferential links to which that concept is normatively attached within a linguistic community. Our next question is how does all this help us with the problems about the *empirical* content of scientific theories.

2.5. *Basic Theories and Observational Terms*

I will now defend that our inferentialist criterion of theoreticity allows to introduce a natural definition of the idea of an *observational concept.* In order to do that, I still need to introduce a couple of notions, one of them formal, the other epistemic. The first notion is Balzer's idea[15] of defining the *expansion* of a set-theoretic structure $s = \langle D, R_1, \ldots, R_n \rangle$, not only as the addition of further *relations* $R_{n+1}, \ldots$, but also as the inclusion of new *elements*, either into the model's domain, or into its relations. Hence, s' is an expansion of s if and only if $s' = \langle D', R'_1, \ldots, R'_n \rangle$, $D \subseteq D$, and for all i $(1 \leq i \leq n)$, $R_i \subseteq R'_i$;[16] this is equivalent to saying that s is a *substructure* of s'. According to Sneed, instead, s' is an extension of s if and only if for some $m < n$, $R_i = R'_i$ if $i \leq m$, and $R_i = \emptyset$ if $i > m$, i.e., if s' simply adds 'new' relations to those of s.

The epistemic notion I need to introduce is that of a '*perceptual scenario*', which is a state of affairs where the players of a linguistic game accept that the inference from some perceptions to the making of a certain assertion is appropriate.[17] Obviously, this appropriateness can be revised for particular cases if some inconsistencies are found in the assertions and commitments made by one or several players. Furthermore, the rules defining which perceptual inferences are right can be eventually changed. But it is nevertheless essential to note that *a notion like that of a 'perceptual scenario' is necessary in order to give experience a chance of playing some role in a linguistic game*.

With the help of these concepts, we can introduce the following definitions:

(4) T is a *basic theory* iff all relations in the models of T are T-theoretical according to (3).

(5) T is an *observational basic theory* iff it is a basic theory, and the relations of its intended applications are filled in through the working of some perceptual scenarios.

(6) If R is a relation within an observational basic theory, then R is an *observational concept*. If R can be explicitly defined by means of other observational concepts, it is also an observational concept. Nothing is an observational concept save those relations fulfilling one of the former conditions.

(7) T is an *empirical theory* iff, when it is conjoined with some observational basic theories, the resulting empirical claim of all these theories is stricter than the content of the observational basic theories alone.

Basic theories are, hence, those ones whose concepts do not presuppose any other theory. The 'claim' of a basic theory is the assertion that its intended applications can be expanded (in Balzer's sense, not in Sneed's) into full models; this simply means that, in order to decide whether a given item (for example, '*a* is warmer than *b*') can be introduced into a relation of an intended application, one has to justify *first* that one knows what that item 'means', and this entails that one has to accept all the consequences that follow from the assumption that this application fulfils all the axioms of the theory (for example, that the relation of 'being warmer than' is transitive). If expressed only in epistemic terms, this has the appearance of a dangerous circularity: one only knows that *something* is warmer than something 'after' knowing that the relation of 'being warmer than' is transitive for *everything*; but this would just be to miss the point of the past section, for what is relevant here are the pragmatic, language-game aspects of theoreticity: one is only *allowed* to use a concept to represent a perception of him, if one masters the inferential practices associated with that concept. Obviously, this inferential practices are grounded (though not infallibly) upon some publicly perceived regularities, which depend also on our perceptual capabilities; but having these capabilities is not *sufficient* for mastering those practices nor for understanding empirical concepts: for example, although my ears work rather well, I am not able at all of arguing about the relations between different musical tones, until I learn some sol-fa and harmony. We must also take into account that the capability of perceiving regularities in our environment and of acting according to them dates from hundreds of million years ago, although the ability of reasoning about them is much more recent in evolutionary terms.

According to (5), an observational basic theory is simply a basic theory whose intended applications are given through some perceptual scenarios; these applications are just 'data models', but formed by data of the most primitive kind. There can be 'basic theories' which are not observational; for example, I think that (at least some) mathematical theories can be of this type, and probably some speculative, theological or metaphysic theories can also be 'basic' in our sense; but I prefer not to discuss these points here, since our current concern are *empirical* theories. (6) recursively defines observational terms simply as those belonging to an observational theory, or those that can be explicitly defined through them; conversely, non-observational concepts are those whose associated inferential patterns (the axioms of the theories to which they essentially belong) can not be reduced to the items of perceptual scenarios, i.e., those concepts which have 'extra content'. Lastly, (7) defines empirical theories as those which *add* something to the empirical claim of some observational basic theories, i.e., those which allow to make more empirical predictions than observational theories alone. It is likely that many 'isolated' non-basic theories are not empirical in this sense, but only become so when joined to other non-basic theories. For example, the core element of classical particle mechanics (Newton's second law) may have no empirical content, but some big portions of its theory net may have it. Note that most (non merely logico-mathematical) concepts in scientific theories can be conceived as empirical, though they are not observational.

2.6. *The Fallibility and Reality of Basic Theories*

Some post-Kuhnian readers may have found the previous paragraph unbearably positivistic, and in a sense it is intended as a restatement of some positivist insights about the empirical basis of scientific knowledge, particularly the conception that scientific knowledge has to be based on 'neutral' observations. Obviously, positivism was strongly criticised from philosophical, sociological and historical quarters, and I need take into account these criticisms in order to defend the notion of 'basic theories' presented above. Two critical questions are especially relevant:

(a) One of the few things known from certain about science after post-positivist epistemology is that no knowledge claim is unrevisable; so, are observational basic theories *fallible*?
(b) Another common point of the new philosophy of science is that philosophical constructions have to be grounded on the real working of science; so, can we argue that observational theories *exist*, and that they have the appropriate connection with real scientific *practice*?

With respect to the fallibility problem, I think it is enough to put a historical example. Thousands of years ago, people could take as (part of) an observational basic theory the assumption that 'x is to the west of y' (Wxy) was an asymmetric, antireflexive relation (i.e., Wab entailed $\neg Wba$, and for all a, we would have $\neg Waa$). A person who asserted that 'there is a place who is to the west of us, and we are to the west of it' would have been taken as not having properly understood the meaning of 'west' (or who was simply joking). But, obviously, when it became known that the Earth is round, this assumption proved to be wrong. So, *observational basic theories can be empirically refuted, and this can be done by showing that the inferences they command to make lead to contradictions.* When a basic theory is refuted, it is usually because these inferences were valid for a limited empirical context, and they had been unadvertedly extended to other contexts where they ceased to be valid. In our example, the primitive concept of 'west' was valid for small regions, but not in the context of the full planet. It is also clear that there can be alternative, mutually contradictory observational basic theories, for example in different speech communities. When this is the case, some philosophers might conclude that a 'neutral' observational basis is impossible, but this conclusion depends essentially on the assumption that observational basic theories are uneliminable, or eliminable only by convention. Rather on the contrary: when different speech communities have different basic theories, they can work together in order to look for the sources of their disagreements, and this research may lead to discover what assumptions were flawed. The fact that the different observational basic theories of a single community may be mutually contradictory shows that this contradiction has not to be confused with some type of cognitive incommensurability. The 'neutrality' of observation means simply that there is an *objective* way of criticising basic theories: looking whether they lead us to admit some contradictions.[18]

With respect to the second question, structuralists tend to criticise the notion of a bed rock theory on the basis (among other things) of their non existence as *scientific* theories, i.e., as theories formally presented in textbooks, taught in university classrooms, and so on. After all, one of the virtues of structuralism is its being developed as an empirical theory of science, whose object of study are actual scientific theories, and not just abstract philosophical constructions. Nonetheless, observational basic theories can not be expected to be 'scientific' in the sense in which classical mechanics or plate tectonics are; instead, they are usually networks of assumptions belonging to our use of *ordinary language*, fragments of 'common sense', and so they are 'pre-scientific'. The identification of specific basic theories is a job, hence, for the philosophers of (ordinary) language, rather than for philosophers of science, although strong collaboration is needed in order to understand the specific uses scientists do of common sense concepts.

Scientists are able of using observational concepts (as these have been defined above) not only because they have passed a specific training as scientists, but mainly because they are normal people whose mother tongue is tuned to their natural perceptual capabilities. As practising scientists, they must also learn to understand and perceive things in radically different ways, but this would be impossible if they had to disregard all their natural language and background common sense assumptions. It would be absurd to pretend that the full building of scientific knowledge were completely self-contained, in the sense that research had to be carried out *completely* in the language of 'scientific' theories, including the descriptions of all laboratory operations or field observations. This was perhaps a positivist dream, which all studies in the history and sociology of scientific practice help us to disconfirm. So, *my notion of 'observational basic theories' attempts to capture the unavoidable connection between scientific concepts (those* *which are theoretical with respect to some 'scientific' theory) and ordinary linguistic practices*. If this notion is right, scientific knowledge is built on common sense, although the former can serve to correct the latter when it proves that some common sense assumptions may lead to contradictions, as we have seen in the previous paragraph.

3. SEMANTIC GAMES AND RESEARCH GAMES

3.1. *Semantic Games for Ramsey–Sneed Sentences*

In this section, Jaakko Hintikka's game theoretical semantics will be put in use to illuminate the process of theory testing.[19] According to this approach, a *semantic* game for a proposition is a game played between two players (V, the verifier, and F, the falsifier) who try to *find out*, respectively, an example or a counterexample of that proposition. The proposition will be true if V has a winning strategy for the game, i.e., a way of playing the game which assures her victory independently of the moves made by F; and conversely, it will be false if F has a winning strategy. The structure of the game is inspired in that of semantic tableaux, and consists of the following rules:

(a) if P is an atomic proposition, V wins if P is true and F wins if it is false;
(b) if P has the form $Q \vee R$, V chooses either Q or R, and the game continues with respect to that proposition;
(c) if P has the form $Q \& R$, F chooses either Q or R, and the game continues with respect to that proposition;
(d) if P has the form $\neg Q$, the game continues with respect to Q, but changing the roles of V and F;
(e) if P has the form $\forall x Qx$, V chooses some object a and the game is continued with respect to the proposition Qa;
(f) if P has the form $\exists x Qx$, F chooses some object a and the game is continued with respect to the proposition Qa.

Game theoretical semantics was mainly developed to analyse several aspects of natural languages which were difficult to explain with other formal semantic tools; in fact, the 'players' are only abstract constructions which do not represent actual beings, and, as far as I know, the theory has not been systematically used as a means to analyse scientific method, in spite of Hintikka's other works on this subject.[20] Nevertheless, Hintikka's idea of connecting the semantic analysis of propositions with the *activity* of searching for certain objects allows to think that it might be possible to reach some relevant conclusions about the testability of scientific theories through a game theoretical analysis of them. In particular, my proposal is simply to deploy the game associated to the 'empirical claim' of a theory (its 'Ramsey–Sneed sentence', in structuralist terms), and look for interesting consequences thereof.

There are several versions of the 'empirical claim' notion in the structuralist literature, and I will use a particularly simple one, which is apt to present the basic ideas of the game-theoretical semantic approach. Further studies may be devoted to analyse the games associated to more complex versions of 'Ramsey–Sneed sentences', as well as the empirical claims of specific theories. In what follows, 'I' will represent the set of intended applications of a theory (which is a subset of M_{pp}, the set of partial potential models of the theory), 'M_p' will be the set of its potential models, and 'M' that of its actual models; 'C' ($\subseteq Po(M_p)$) will be the set of those subsets of M_p which obey the theory's constraints; lastly, $\mathcal{F}$ is the set of all possible functions from M_{pp} into M_p; each one of these functions induces a corresponding function from $Po(M_{pp})$ into $Po(M_p)$.[21] Now we can describe the empirical claim of theory T as the proposition:

$$(8) \qquad \exists f \in \mathcal{F}((\forall x \in I, f(x) \in M) \,\&\, (\forall X \in Po(I), f(X) \in C))$$

(8) asserts that there is a way of completing the intended applications into structures which obey both the laws and the constraints of the theory. Figure 1 depicts the 'normal form' game associated to this assertion; numbered cells represent decisions by the 'verifier' or the 'falsifier'; the bottom cells are the possible endings of the game, which is won by V if the chosen cell is true, and by F if it is false. Figure 2 shows the 'strategic form' of the game; columns represent the strategies available to V (each possible function f), and rows the strategies of F (which are pairs of the form '$\langle$left, $a\rangle$' or '$\langle$right, A$\rangle$', where a is an element of I and A a subset of I). Each cell is to be replaced by the truth value of the sentence included into it; if there is at least a column all whose cells are true, then proposition (8) will be true, and if there is at least a row all whose cells are false, then (8) will be false. In the case of classical logic at least, it is warranted that one of these possibilities must take place.

Figures 1 and 2 have an obvious methodological reading. The falsification of a theory amounts to finding out one or more intended applications which do not fit, either the theory's laws, or its constraints, whatever the values the 'verifier' assigns (through the g-functions) to their applications' theoretical magnitudes. Its verification amounts to finding out a way of completing all theoretical applications, such that no counterexample can be actually presented. From this point of view, scientific method consists

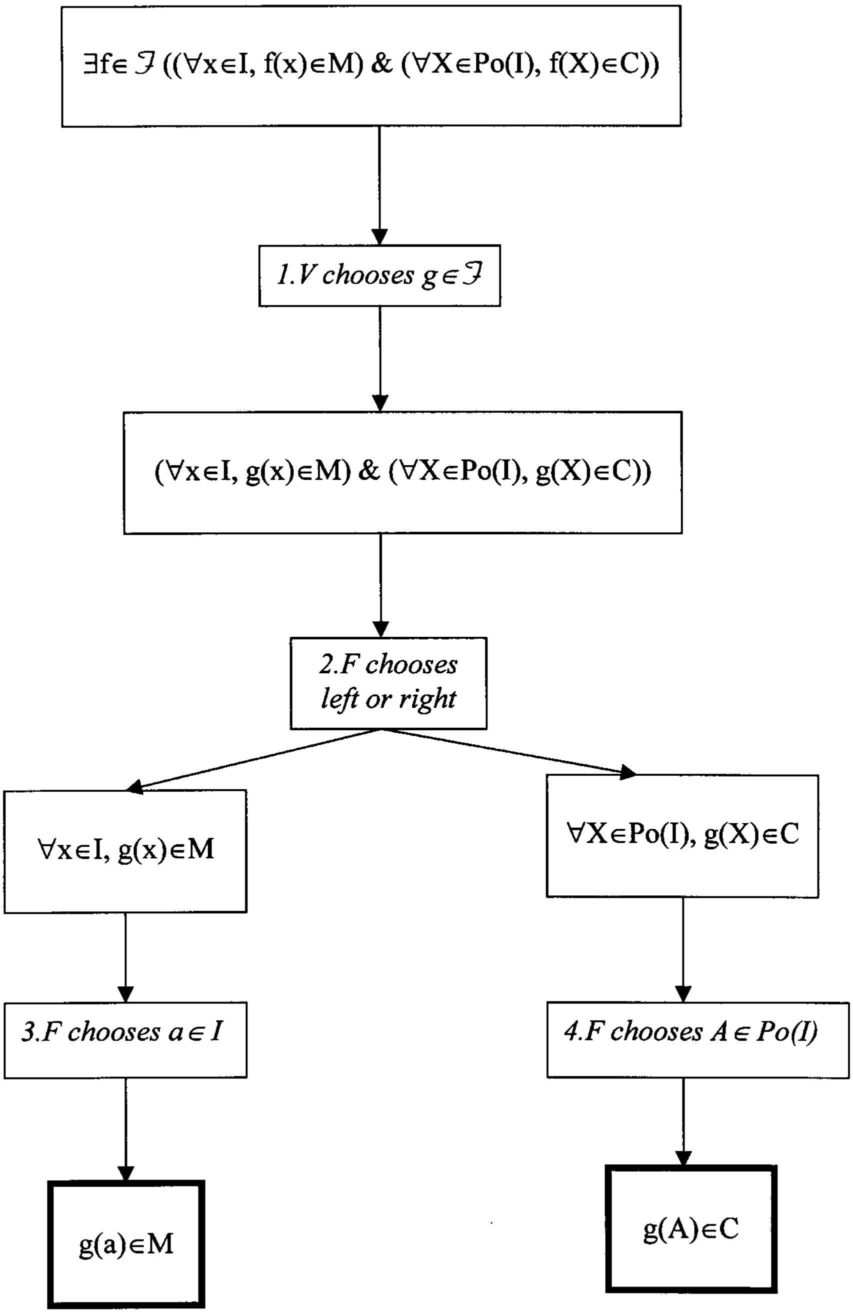
∃f∈ℑ ((∀x∈I, f(x)∈M) & (∀X∈Po(I), f(X)∈C))
1.V chooses g∈ℑ
(∀x∈I, g(x)∈M) & (∀X∈Po(I), g(X)∈C))
2.F chooses left or right
∀x∈I, g(x)∈M
∀X∈Po(I), g(X)∈C
3.F chooses a∈I
4.F chooses A∈Po(I)
g(a)∈M
g(A)∈C

Figure 1.

V F	g_1	g_2	...	g_i	...
Left, a_1	$g_1(a_1) \in M$	$g_2(a_1) \in M$	...	$g_i(a_1) \in M$	...
...	...	...	...	...	...
Left, a_j	$g_1(a_j) \in M$	$g_2(a_j) \in M$	...	$g_i(a_j) \in M$	...
...	...	...	...	...	...
Right, A_1	$g_1(A_1) \in C$	$g_2(A_1) \in C$	...	$g_i(A_1) \in C$	...
...	...	...	...	...	...
Right, A_k	$g_1(A_k) \in C$	$g_2(A_k) \in C$	...	$g_i(A_k) \in C$	...
...	...	...	...	...	...

Figure 2.

basically in a double set of strategies: *theoretisation* looks for embedding the known empirical structures into bigger, more complex ones, such that certain laws are met; *observation and experimentation* look for finding out empirical structures which might fail to obey those laws.[22] This vision is clearly a Popperian one, although, contrarily to the falsificationist slogan, Figure 2 allows to see that in general, *scientific theories*, *besides being unverifiable*, *can be unfalsifiable as well.* The reason is that a theory is verifiable (alternatively, falsifiable) if and only if its game's strategic form has a finite number of rows (columns), i.e., if its columns (rows) are finite in length; for only in this case are human beings able of confirming that *all* the cells of a certain column (raw) are true (false). So, if the set I is finite, the theory is verifiable, and if the set $\mathcal{F}$ is finite, the theory is falsifiable. The problem is, of course, that both sets are usually infinite. For example, a theory's intended applications include not only *actual* empirical structures, but also *physically possible* ones (this is specially clear when we think of the a's as *individual* realisations of experiments that the falsifier *might* do, rather than as *types* of experiments). The transfinite nature of $\mathcal{F}$ is still clearer, since it comprises all the possible functions from M_{pp} into M_p, which are even non-denumerable sets.

3.2. *Games Scientists Play*

Although game theoretical semantics has the virtue of explaining semantic categories (e.g., truth) through pragmatic ones (e.g., the activities of searching and finding), a game for the sentence associated to the claim of a theory is so immense that human beings can not actually play it at all. Nevertheless, it is challenging to look at the competitive activities real scientists perform in their daily work as something essentially related to the game depicted in the previous section. My suggestion is that we can understand the rules of scientific method as a set of *mutual constraints* that players V and F put to each other in order to have a chance of playing that game with a limited amount of time and resources.[23] According to this vision, each scientist adopts one of those roles (not necessarily the same every time) only under the proviso that the set of strategies available to each participant in the game has been dramatically reduced. The structuralist conception of theories provides again some insights about how this reduction can be performed; in particular, I will make use of the ideas that scientific theories are organised through a net of *special laws*, and that the set of intended applications is itself organised into *types of applications*. This 'human-faced' description of the semantic game for a scientific theory can show more transparently than usual structuralist expositions some methodological aspects of the construction and testing of theory nets. A semantic game for a scientific theory, whose strategies have been reduced in order to give each player a realistic chance of winning, can be called a '*research game*'.

Before analysing such a game with reduced strategies, we have to introduce some new terminology. Let $L_1, \ldots, L_i, \ldots,$ be a series of subsets of $Po(M) \cap C$, i.e., collections of sets of models of the theory satisfying its global constraint, and which actually obey some additional condition (a special law or constraint, or both), and let us assume that this condition can be expressed through a finite formula. $\mathcal{L}$ will be the set of all these L_i's. On the other hand, let $I_1, \ldots, I_j, \ldots,$ be a series of subsets of I, i.e., types of intended applications, whose empirical identification is assumed not to be questioned by the players of the game. $\mathcal{I}$ will be the set of all these I_j's. It is important to take into account that neither the L_i's nor the I_j's are necessarily disjoint. Let $\mathcal{H}$ be the set of all possible functions from $\mathcal{I}$ into $\mathcal{L}$, i.e., the set of all possible 'theory-nets' which may be constructed for the set $\mathcal{I}$ using some of the laws contained into $\mathcal{L}$. Lastly, if x and y are structures, let 'Eyx' represent that y is an extension of x. We can then reconstruct the empirical claim of a scientific theory as follows:

(9) $\exists h \in \mathcal{H}\ \forall I_i \in \mathcal{I}\ \forall x \in I_i\ \exists y \in h(I_i)Eyx$

(9) asserts that there is a way of assigning a special law or constraint $(h(I_i))$ to each type of application (I_i), such that for each one of its individual application's (x), the result of applying to it the corresponding special law ends with an actual model of the theory. Stated differently, to each type of application can be successfully associated a theoretical formula (i.e., a formula which logically entails the theory's fundamental laws

and constraints), where 'successfully' means that this formula is logically consistent with the data included into any particular application of that type. The normal form of the 'research game' associated to (9) is depicted in Figure 3.

For the strategic form, account must be taken that, if theoretical laws are well defined, then the last movement is no 'choice' at all, for the only thing V has to do is to apply the formula $g(I^*)$ to the data contained in the application a; if the formula is not consistent with those data, the proposition of the last cell will be false due to the non existence of b, whereas if it is consistent with them, b will be uniquely determined by that formula. That is, the point of special laws is to allow to *construct* 'actual models' out of 'empirical applications' in a non arbitrary fashion.[24] Hence, the strategies of V are the elements of $\mathcal{H}$ (she has to choose a theoretical law or constraint for each type of application), while the strategies of F consist of pairs of the form '$\langle I_i, a^i_j \rangle$', where a^j_i is an element of I_j (she has to choose a type of application first, and later a particular application thereof). The 'real' game actually ends after the third movement (at the cell marked with dotted lines), for usually the last cell is fully determined by the previous choices of the players, as I have argued. The strategic form of the game is, hence, as shown in Figure 4. Each cell '$\exists y \in g_i(I_j)Eya^j_m$' amounts to the assertion that a theoretical model (y) can be constructed out of the chosen empirical system (a^j_m) with the help of the special laws determined by the combined choice of g_i and I_j (i.e., by $g_i(I_j)$).

3.3. *Methodological Strategies*

With respect to the verifiability and falsifiability of a scientific theory, it is clear from the last figure that the theory will be falsifiable (i.e., a row of false statements can be found) if and only if there is only a limited number of functions g_i's, and this occurs if and only if both the sets $\mathcal{I}$ and $\mathcal{L}$ are finite. On the other hand, the theory will be verifiable (i.e., a column of true statements can be found) if and only if both the set $\mathcal{I}$ and all of its elements (which are sets of applications) are finite; the first one of these two last conditions is more reasonable than the second: there can be a limited number of *types* of applications, but, as we saw in the case of (8), each type includes an indefinite, probably non-denumerable amount of concrete systems. So, for scientific research being carried out as a game in which each player has a reasonable chance of winning, the following three conditions must obtain: (a) researchers admit to consider only a limited number of possible theoretical laws; (b) they also accept to consider only a limited number of possible types of empirical systems, and (c) the applicability of those laws to these empirical systems can be decided. From

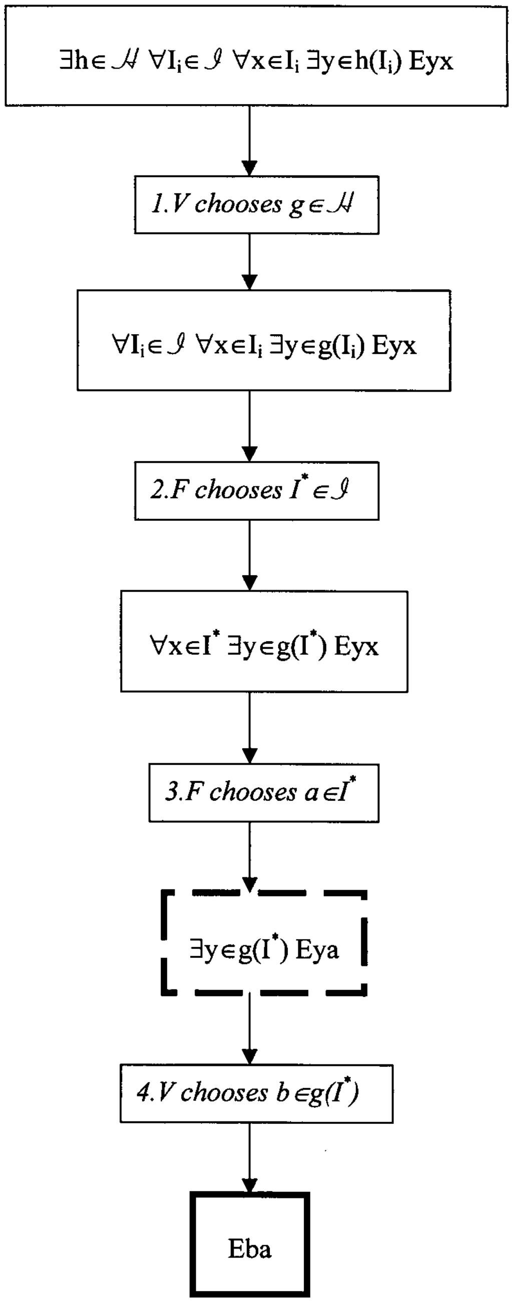
$\exists h \in \mathcal{H}\ \forall I_i \in \mathcal{I}\ \forall x \in I_i\ \exists y \in h(I_i)$ Eyx
1. V chooses $g \in \mathcal{H}$
$\forall I_i \in \mathcal{I}\ \forall x \in I_i\ \exists y \in g(I_i)$ Eyx
2. F chooses $I^* \in \mathcal{I}$
$\forall x \in I^*\ \exists y \in g(I^*)$ Eyx
3. F chooses $a \in I^*$
$\exists y \in g(I^*)$ Eya
4. V chooses $b \in g(I^*)$
Eba

Figure 3.

V / F	g_1	...	g_i	...
I_1, a_1^1	$\exists y \in g_1(I_1)\, Eya_1^1$	...	$\exists y \in g_i(I_1)\, Eya_1^1$	...
...	...	...	...	...
I_1, a_n^1	$\exists y \in g_1(I_1)\, Eya_n^1$	...	$\exists y \in g_i(I_1)\, Eya_n^1$	...
...	...	...	...	...
I_j, a_1^j	$\exists y \in g_1(I_j)\, Eya_1^j$	...	$\exists y \in g_i(I_j)\, Eya_1^j$	...
...	...	...	...	...
I_j, a_m^j	$\exists y \in g_1(I_j)\, Eya_m^j$	...	$\exists y \in g_i(I_j)\, Eya_m^j$	...
...	...	...	...	...

Figure 4.

a contractarian point of view, this can be seen as the result of the following 'negotiation': the falsifiers might make any theory *unverifiable* just by insisting that they have to examine *all* the theory's empirical applications, and the verifiers might make it *unfalsifiable* just by leaving open the set of special laws they can employ; besides this, both the falsifiers and the verifiers might make the theory both unverifiable and unfalsifiable by insisting in applying the theory to an open set of types of empirical situations; hence, *the theory becomes both verifiable and falsifiable just by the mutual agreement of not using these 'defensive' strategies*. In a nutshell, *V* accepts that the theory can in principle be falsified *in exchange* of *F*'s acceptance that it can in principle be verified. So, in contrast to Popper's thesis that theories are unverifiable by their logical form and falsifiable by convention (i.e., by the conventional decision of accepting a 'basic statement'),[25] our approach suggests that theories are *both* unverifiable and unfalsifiable by their logical form (as it is clear from Figure 2), but can become verifiable and falsifiable by agreement.

Besides this reasoning, it can also be argued that the outcomes of experiments and systematic observations are not usually singular statements (e.g., of the type of the fourth and fifth propositions in Figure 3), but regularities about kinds of empirical situations (e.g., of the type of the third proposition: '$\forall x \in I^* \, \exists y \in g(I^*)Eyx$'). This has been cogently defended by Hintikka, who asserts that what is known as 'induction' in scientific practice is not the *inference* 'from the particular to the universal', but rather the *extension* of a regularity from a limited domain to a wider one. It is important to notice, as well, that it is the third proposition in Figure 3 what has the logical form Hintikka ascribes to the outcomes of controlled experiments.[26] On the other hand, the very idea of establishing *kinds* of empirical applications presupposes that *some* regularities have been found about them: those serving to *define* that type. After all, we employ *concepts* to identify those kinds of systems, and, as we have seen in Section 2, *concepts only have an empirical meaning if they are 'parasitic' of some publicly perceived regularities.* Hence, if some regularities must have been found in order to construct a classification of empirical systems, then there is no reason why *further* regularities concerning these systems might not be empirically established as well. Of course, all these regularities are fallible (recall Section 2.6).

On the other hand, some comments can be made about the restriction of the size of sets $\mathcal{L}$ and $\mathcal{I}$. In the first place, this restriction is a *desideratum* rather than a logical constraint, and probably there are many scientific controversies where no limits are established *a priori* to the types of laws or applications; what I want to stress is that the verification or falsification of scientific theories can only take place when this restriction is agreed upon by competing researchers.

In the second place, $\mathcal{L}$ can be organised in the form of a coherent classification tree of *types* of laws, containing 'at the top' the most general types of symmetries that theoretical models can obey (or fail to do it) within a given theoretical framework. In this case, scientific research can be strongly furthered at both the theoretical and the empirical level, because some empirical regularities may serve to falsify or verify very wide ranges of possible theoretical laws, and not only particular hypotheses.

In the third place, $\mathcal{I}$ can also have the form of a classification tree, which allows to organise empirical research systematically, beginning by establishing empirical regularities for very restricted, 'low level' types of applications, and ending (with a little bit of luck) with much more abstract laws which are applicable to a wide range of systems. Nevertheless, in many cases no such 'unification' is reached, and scientists end simply with a compilation of more or less general regularities, having only quasi-tautological 'laws' at the top.

In the fourth and last place, and perhaps more importantly, the strategy of restricting $\mathcal{L}$ and $\mathcal{I}$ can be seen as the game-theoretical counterpart of two common methodological strategies, usually known as 'eliminative' and 'enumerative' induction. *Eliminative induction* is possible just if there is only a limited number of alternative combinations of special laws, and so empirical research can lead either to the rejection of all of them (in which case the full theory becomes falsified) or to the rejection of all combinations save one (which becomes confirmed); traditional expositions presented this methodological strategy as if the relevant combinations of laws were all the *conceivable* ones, but under this paper's approach it suffices that the players have agreed on *any* limitation of them, no matter the criteria employed to do it. *Enumerative induction* amounts to examining all the possible types of empirical systems, and this allows to verify whether the theory is applicable to all of them or not. The other classical sense of 'induction' (say, *Baconian induction*)[27] is that of making a generalisation from the observation of singular events to a regularity about a certain type of situation; in Figure 4 this would correspond to 'collapsing' the information obtained from a number of systems like $a_i^1, a_i^2, \ldots, a_i^n, \ldots$, into an empirical law of the form '$\forall x \in I^* \; \exists y \in g(I^*)Eyx$'. Figure 5 resumes these methodological readings of the research game;[28] 'theory building and eliminative induction' consists in identifying all the possible alternative systems of hypotheses (i.e., possible theory-nets), and using later the results of the cells to decide whether some of these systems is true, or if none is; 'enumerative induction' consists in studying all the possible types of empirical applications, in order to test whether they obey the laws assigned to them by a particular theory-net; lastly, 'Baconian induction' amounts to the production of the statements contained in each cell, which assert the applicability of some concrete laws to all the individual systems contained within some concrete type of empirical applications.

4. CONCLUSIONS

In this paper I have tried to defend a couple of traditional views about scientific method which in the last decades had been rather discredited. These ideas are that scientific knowledge is grounded (though not infallibly) on perceptual experience and common sense, and that scientific theories can actually be (pragmatically) verified as well as falsified by means of empirical research. In order to defend these thesis, I have resorted to semantic approaches which are grounded on pragmatic intuitions: the inferential semantics employed in Section 2 brings to sight the normative aspects of conceptual and empirical content, and the game-theoretic semantics deployed in Section 3 points to the competitive activities of scientific researchers. Thus, the 'traditionalist' flavour of my conclusions is won thanks to the recognition of some basic anti-positivist tenets: that linguistic practices are always collectively regulated and dependent on cultural contexts, and that scientific research is a field where competition is one determinant factor. Nevertheless, this does not entail that 'all knowl-

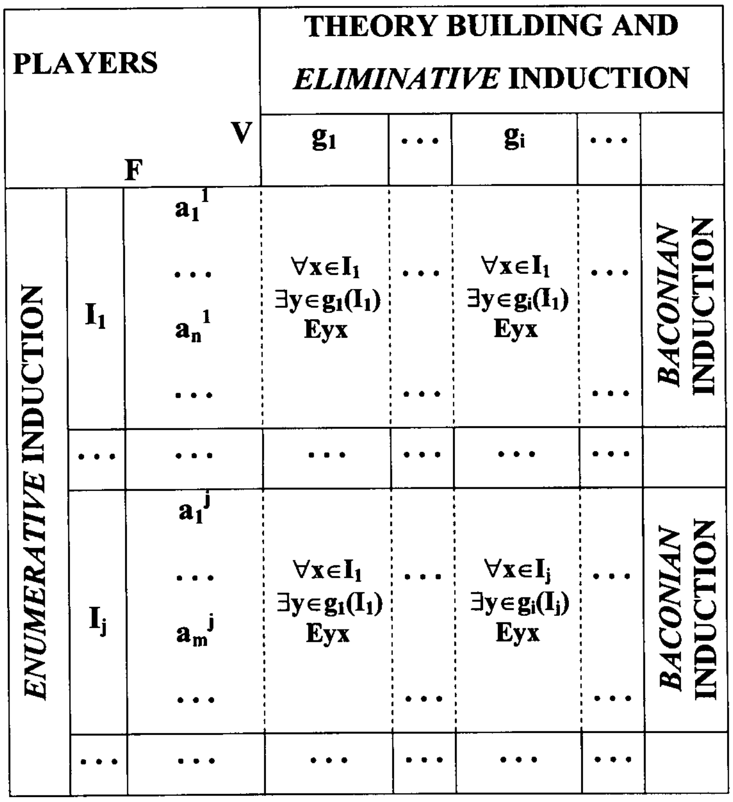

Figure 5.

edge is relative', because those social practices can be seen, not as an obstacle in the searching for objectivity, but as an unavoidable precondition of its success.

ACKNOWLEDGEMENTS

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NOTES

[1] Balzer–Moulines–Sneed (1987, p. 221). In the introduction to a special issue of *Synthese*, Moulines (2002) has recently recognised that structuralism "really do not address the BIG PHILOSOPHICAL ISSUES about science" (upper case in the original). The papers contributed to that issue represent an interesting step towards the discussion of those problems; in particular, questions about the meaning of scientific concepts and their relation to observation are studied in Díez Calzada (2002), and methodological problems are studied in Balzer (2002).

[2] See especially Kuipers (1996) and Zamora Bonilla (1996).

[3] The only possible claim about those intended applications is on the cardinalities of their base sets. This is analogous to the case of a Ramsey sentence of a theory all whose terms are 'theoretical' in the positivist sense. Cf. Hintikka (1999, ch. 13).

[4] See Sneed (1971), Stegmüller (1979), and Moulines (1985).

[5] See Gähde (1983), and Balzer (1985) and (1996).

[6] Balzer (1996, p. 154).

[7] Cf. Balzer–Moulines–Sneed (1987, pp. 47–78), where both approaches are presented. In a nutshell, Sneed's definition of theoreticity asserts that a function f is T-theoretical if T is needed in order to make a determination of f, whereas Balzer's asserts that f is T-*non*-theoretical if other theories besides T are needed to make such a determination. So, according to Sneed, f is T-non-theoretical if assuming T is not necessary for measuring f, while according to Balzer, f is T-non-theoretical if assuming T is not sufficient. What makes of Sneed's a 'pragmatic' criterion and of Balzer's a 'formal' one is that, in order to discover that f is T-non-theoretical in Sneed's sense, we need to find out in scientific practice an empirical f-measuring process which does not presuppose the validity of T, while in order to determine that f is T-non-theoretical in Balzer's sense, it is enough to examine the mathematical invariances entailed by the laws of T.

[8] See Balzer–Moulines–Sneed (1987, ch. 2), Gähde (1996), and Moulines and Polanski (1996).

[9] Balzer (1986, p. 154.)

[10] It is still worse: if we do not include in (2) – after the 'iff' – the condition 'there is some adequate t-determining method, and for all x...', then *all* terms for which there is no adequate determination method will be theoretical with respect to *all* theories, for in that case, the condition 'x is an adequate t-determining model' will be vacuously satisfied.

[11] See particularly Sellars (1997) and Brandom (1994).

[12] I personally think that Brandom's own account of how this set of 'correct' inferences is *established* is not complete, but this point is not essential for my current argument. On the other hand, as it is clear, the term 'inference' is used in this approach to cover any move from a 'position' in a language game to another 'position', although these 'positions' can be non-linguistic ones: they can also be 'inputs' in the game (basically, perceptions), as well as 'outputs' (basically, actions).

[13] Though not all constraints and links of T need to be essential for 'understanding' t; in that case, t will be theoretical only with respect to that part of T which are really required for the relevant community. On the other hand, when some constraints and links are essential for the understanding of t, this makes it necessary to modify (3) in the following way: '(*a*) if you are using t in a system $x \in S$, and if you do not take S to be included in $Po(Mod(T)) \cap C \cap L$ (either explicitly or implicitly), then ...', where C and L are, respectively, the relevant constraints and links.

[14] By the way, this offers a pragmatic 'solution' to the problem of induction: it is not that inductive arguments must be valid in order that the learning of language can begin, but that speakers must act *as if* certain empirical regularities were 'well established' if linguistic expressions are to have an inferential content at all for them. Of course, this does not warrant that all those regularities are actually true (see Sections 2.6 and 3.3).

[15] Balzer (1985).

[16] Additionally, each R_i must be of the same logical type that R'_i. Obviously, some R_i's can be unary, i.e., they can be 'properties', and they can also be functions.

[17] This idea is related to the notion of 'observational scene', as used by Díez Calzada (2002, pp. 32 and ff). One main difference between his approach and mine is that he uses 'observational scenes' more or less like a reconstruction of classical 'protocol sentences', whereas my concept of 'perceptual scenario' is more naturalistic, in the sense that an external observer could (tentatively) accept that another individual, whose observational concepts the former does not posses, is being acting in (what for the latter is) a perceptual scenario; e.g., a blind person could agree that I am seeing a red box, and I might agree that bats ear the walls of a room.

[18] Díez Calzada (2002) defends also the 'neutrality' of observation, but on the ground of the universality of human *perceptual* capabilities. My approach insists also on the role of our *inferential* capabilities.

[19] See Hintikka (1973).

[20] Methodological writings by Hintikka are more inspired by his 'interrogative model' of scientific research. See, e.g., the papers collected in Hintikka (1999).

[21] That is, if X is a subset of M_{pp}, then $f(X)$ is $\{y \in M_p / \exists z \in X, y = f(z)\}$.

[22] Obviously, the utility of theoretisation, observation and experimentation is not limited to their roles in these semantic games: it is important to take into account also what the point *of the game of science* is. Perhaps it is to 'discover the underlying truth', or perhaps it is to help us to 'control our environment'. In this paper, nevertheless, I shall be agnostic about this ultimate question.

[23] I have defended this view of scientific method in Zamora Bonilla (2002).

[24] In some cases, nevertheless, the special laws do not determine *uniquely* an extension of the intended application (several theoretical models can be possible extensions of a, all of them consistent with the special laws which correspond to $g(I^*)$), and in these cases V has still certainly a choice. However, I will restrict my discussion to the case where $g(I^*)$ determines a unique theoretical extension.

[25] See Popper (1959, section 29).

[26] See again Hintikka (1999), esp. chapters 7 and 8. He calls 'the atomistic postulate' the assumption that the basis of all knowledge are propositions without quantifiers.

[27] This is simply a quick way of speaking. I do not want to enter a discussion about whether Francis Bacon would actually defend this type of induction or not.

[28] Similar methodological conclusions are reached in Balzer (2002), where, besides 'enumerative induction', a method called 'hypothesis construction induction' is suggested, which is closely related to the 'theory building and eliminative induction' method of Figure 5. Nevertheless, I think that the use Balzer makes there of structuralist categories is basically unrequired by the rest of his arguments.

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