THE DUHEM THESIS, THE QUINE THESIS AND THE PROBLEM OF MEANING HOLISM IN SCIENTIFIC THEORIES

MPhil Thesis by

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Dedicated to the memory of Gilberto Ciarmiello (1973 - 1997),
with whom I cultivated a love for philosophy.

ABSTRACT

Through a detailed analysis of Duhem's writings some light is cast on the relations between holism, underdetermination and theory-ladenness of experimentation. The latter, which results from the need to interpret theoretically what is actually observed during an experiment, plays a key role in Duhem's analysis of the relation between observation and theory. I will argue that the theory-ladenness of experimentation on one hand provides a general argument for the holistic character of theory testing, and on the other renders problematic the thesis that theories are underdetermined by empirical evidence. A tension is found between Duhem's claim that the aim of theory is *to save the phenomena* and his analysis of the interpretative role of theory in experiments. I suggest how to overcome this difficulty by showing in what sense we can say that theory saves theory-laden phenomena.

After stressing the differences between the Duhemian and the Quinean variants of holism, I argue that Quine fails to take into account the importance of the theory-ladenness of experimentation and the implications of Duhem's thought: Quine shares with the Logical Empiricists the belief that it is possible to detach from theories their empirical content. His acceptance of holism has simply the effect of restricting the attribution of empirical content only to conjunctions of many theoretical statements. I analyse and criticise the two notions of empirical content that Quine has developed. Furthermore I argue that there is no general theory-free expression of the experiential implications of a theory, for theories are logically connected to observable events only within local contexts defined theoretically and brought about by the activities of experimenters.

Finally I suggest that, in the light of these considerations, the implications resulting from the possibility of rival incommensurable traditions of research should be discussed, rather than Quine's dilemma concerning empirically equivalent systems of the world.

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List of Abbreviations

Works by P. Duhem

ASPT: *The Aim and Structure of Physical Theory*. Princeton: Princeton University Press, 1982.

EHPS: *Essays in the History and Philosophy of Science*. Indianapolis: Hackett Publishing Company, 1996.

SP: *To Save the Phenomena*. Chicago: Chicago University Press, 1969.

Works by W. V. Quine

EESW: "On Empirically Equivalent Systems of the World" *Erkenntnis* 9, (1975): 313-28.

FLPV: *From a logical point of view*. Cambridge Massachusetts: Harvard University Press, 1980.

FSS: *From Stimulus to Science*. Cambridge, Massachusetts: Harvard University Press, 1995.

NNK: "The Nature of Natural Knowledge" In *Mind and Language*, edited by Samuel Guttenplan, pp. 67-81. Oxford: Clarendon Press, 1975.

OR: *Ontological Relativity and Other Essays.* New York: Columbia University Press, 1969.

PT: *Pursuit of Truth.* Cambridge, Massachusetts: Harvard University Press, 1992.

RIT: "On the Reasons for Indeterminacy of Translation" *The Journal of Philosophy,* 67 (1970): pp. 179-83.

RR: *The Roots of Reference.* La Salle, Ill.: Open Court, 1974.

SN: "Structure and Nature" *The Journal of Philosophy*, Vol. 89, Issue 1 (Jan., 1992), pp. 5-9.

TDR: "Two Dogmas In Retrospect" *Canadian Journal of Philosophy* Vol. 21, No. 3, September 1991, pp. 265-274.

TI: "Three Indeterminacies" In *Perspectives on Quine*, edited by Robert B. Barret and Roger F. Gibson, pp. 1-16. Cambridge, Massachusetts: Basil Blackwell, 1990.

TT: *Theories and Things.* Cambridge, Massachusetts: Harvard University Press, 1981.

WB: (with J. S. Ullian) *The Web of Belief.* New York: Random House, Inc., 1970.

WO: *Word and Object.* Cambridge, Massachusetts: Massachusetts Institute of Technology Press, 1960.

1. DUHEM'S ANALYSIS OF THEORY TESTING IN PHYSICS

Introduction

In this chapter I will expose and analyse in detail Duhem's account of the relation between theory and observation in physics. For this purpose I introduce a terminology that will be adopted throughout the thesis. I also discuss some examples that will be often referred to in subsequent chapters when dealing with Quine's philosophy of science.

The thesis that no hypothesis in physics can be tested in isolation (also referred to as the Duhem Thesis or the thesis of holism in theory testing) will be shown to be a consequence of the theory-ladenness of experimentation. Duhem illustrates the latter by underlining the contrast between common-sense observation and experiment: an experiment requires always a theoretical interpretation of what is directly perceived. I then analyse the consequences of Duhem's analysis for the doctrine of underdetermination.[1] The latter is not a logical consequence of holism, although Duhem's discussion suggests examples of pragmatic underdetermination. The logical consequence of holism is rather the multiplicity of paths that a researcher can legitimately follow when confronted with a refuting experiment.

The theory-ladenness of experimentation renders the idea of underdetermination problematic, for the latter is meaningful insofar rival theories can be said to save the same phenomena. Indeed there is a tension in Duhem's thought between his insistence that the aim of physical theory is simply to save the phenomena and his findings concerning the theory-ladenness of experimentation. In the last two sections I try to solve this tension suggesting that theory saves theory-laden phenomena and that

[1] There are many formulations of the problem of underdetermination. Often is argued that two theories can be said to be underdetermined only if they are equally supported by the available evidence. Therefore the mere fact that two theories imply the same observable consequences would not be enough to consider them as underdetermined by empirical evidence. As it will soon be clear, this thesis will deal rather with a preliminary problem, that is with the nature of the very connection between theory and observation.

therefore its function is twofold: to inform and to save the phenomena. This conclusion is reached by comparing the different relations subsisting between theory and observation respectively in ancient astronomy and in modern physics.

1.1 Common-sense facts and common-sense laws

In his most influential book, *The Aim and Structure of Physical Theory*, Duhem draws a sharp distinction between two kinds of facts, common-sense on one side, interpreted or theoretical ones on the other. Generalisation and induction applied to these two kinds of facts lead to the formulation respectively of common-sense laws and scientific laws. The recognition of a common-sense fact is for Duhem absolutely uncontroversial insofar there is no ground to doubt of the normal conditions under which the observation has been carried out. An observational report such as "there is now a white horse in Portugal Street" is indeed vague insofar it does not describe the phenomenon in a detailed way, but ought to be believed without hesitation if supported by direct experience. From such uncontroversial facts we can infer what Duhem calls common-sense laws, i.e. generalisations that are true in virtue of pure observation and inductive inference. An example of such law could be "all men are mortal" or "the thunder is heard always after the flash of lightning has been seen". As Duhem is justly ranked among the critics of inductive methodology, is it worth noticing that he does not submit to any scrutiny the validity of these simple cases of inductive inferences. They lead to statements that are true "for all time and for all men" and their degree of certainty is "fixed and absolute"[2].

A statement such as "all men are mortal" is indeed abstract and general: it expresses the relation between the abstract idea of man and the abstract idea of death. It is actually the case that any law-like statement must include general terms. What is then the source of the certainty with which we can assert such established truth of common-sense? The answer lies in the fact that in a particular man we can see the embodiment of the abstract idea of man and similarly in the case of a particular death, or thunder of lightning. The abstract concept "man" is but an abstraction obtained considering what

[2] ASPT, p. 178.

there is in common to all particular men. If we wish to verify the truth of the law in question we can proceed to observe a particular case to which the abstract notions directly apply. Adopting the usual terminology brought later into fashion by the logical empiricists, we could say that Duhem is here considering "man" and "mortal" as observational terms that are unambiguously applicable. However, it will be always important to remember that Duhem does not distinguish explicitly and systematically between facts and the linguistic expressions used to state them.

Of course, the price that we pay in order to obtain the certainty of common-sense laws is the poverty and vagueness of the cognitive content of our statements. The sentence stating that every day the sun rises in Paris in the East and sets in the West is true and certainly so, but it informs us in no way on whether the point of the horizon at which the sun appears in the morning varies from day to day or from month to month; it does not tell us in what day of the year the shortest or the longest time separates the dawn from the sunset. The knowledge that we aspire to attain requires a precise account of the object of enquiry and, along with it, detailed indications of the conditions under which experience would confirm our account. In order to achieve these goals we must replace ordinary testimony with physical experiment and ordinary language with the language of physical theories. What is the difference then between the latter and the former? What is the nature of physical experiment? Let us first analyse the notion of fact and then let us move on to explain how from this notion we can formulate general laws.

1.2 The mismatch between common-sense facts and their interpretation in physical terms

Duhem deals with the problem for the first time in the section "Physical Approximation and Mathematical Precision".[3] A physical theory is here considered as a set of postulates in algebraic form from which it is possible to derive, with the aid of logical and mathematical deductive procedures, results that can be compared with observations carried out by means of physical experiments. Thermodynamics for

[3] ASPT, p. 132.

instance tells us that a certain block of ice under a certain pressure will melt at a precise value of the temperature. Let us here notice that in order to obtain a prediction of this kind the theory will have to include besides a number of principles, laws and theorems, also a classification of the materials and an indication of their relevant coefficients (such as specific heat or density). The situation is essentially that of a normal exercise in a physics textbook: some initial conditions defining the state of a system are given and the theory is used in order to derive the value of some properties of the system under study at the initial or at a different instant. How can we relate this abstract calculation with what we would actually observe if we were to perform the experiment?

It is clear that the abstract pattern of geometrical shapes and mathematical properties that we have in mind has very different features from what we actually observe in the laboratory. If we imagine confronting step by step the realisation of the experiment in terms of concrete objects and instruments with the solution of the associated physical problem that we could write on the blackboard, in order to compare the results and hence to judge on the validity of the theory, we immediately notice that, both at the beginning and at the end of the experiment, the theoretical description can be "welded" to what is actually observed only through *acts of measurement*. The mathematical schematic description and the description carried out by the experimenter in terms of observable facts are expressed in different languages and, as Duhem points out,

> ... the method of measurement is the dictionary that makes possible the rendering of these two translations in either direction.[4]

A sort of translation occurs between what Duhem refers to as "theoretical facts" and "practical facts", the former being the mathematical data and the geometrical idealisations dealt with in the abstract reasoning of the theoretician. For instance a solid body whose thermal properties are under investigation will be considered as having a precise geometrical shape and a well-defined value of the temperature at each of its points. For the purpose of the theoretical treatment the real object is replaced by

[4] ASPT, p. 133.

an idealised one, which has perfectly defined properties. The practical fact, on the other hand, will have no precise geometrical or mathematical property. The shape will be only approximately defined and it will be possible in general only to ascribe a defined value of the temperature to a small region of it, not of course to each of its points (whatever this might mean). Moreover the result of a measurement is never a precise real number, but an interval of real numbers depending on the precision of the instrument used. Hence, whereas the theoretician will consider any two different values of a physical magnitude as incompatible results, the experimenter will consider them as coincident if their difference is not detectable by the available instruments. As a consequence *the same practical fact, being approximate, can be made to correspond with an infinity of incompatible theoretical facts.* We find here a first level of inadequacy of the translation between what is dealt with by the experimenter and what is theorised about.

In the fourth chapter of *The Aim and Structure of Physical Theory,* Duhem analyses in greater detail the nature of physical experiment and the relation between theoretical facts and practical facts. I will have occasion to notice that there is some vagueness in his terminology, which may lead to confusion. This defect in the exposition however does not affect the conclusions of Duhem's arguments on this point.

Duhem invites us to imagine a laboratory in which a certain experiment is being performed. What the direct observation will attest is the movement of some pieces of metal, mercury columns becoming level with some line, the arrow of some instrument moving and other ordinary facts. The experimenter on the other hand will record on her notebook that, say, a certain increase in temperature has produced a certain change in the resistance of a coil. Thus, we face here two distinct parts of the same experiment: on one side there is a number of plain observations of what is directly manifest to the senses, on the other hand we have the written report, which is expressed in a symbolic and abstract language and hence consists of an interpretation of the observable part. This language contains terms such as "temperature", "pressure", "electric current" which are in no way found instantiated in concrete objects of observation. We don't observe a value of the temperature, we observe the level of mercury in a glass column. In order to relate the content of direct testimony with abstract concepts *we need physical theory.*

In every experiment the interpretative role played by physical theory is visible at various levels. To start with, a physical concept is a magnitude, hence its determination needs a measurement procedure, which in turn is dependent on a number of theoretical assumptions. Even the simplest kind of measurement, the determination of a length, needs the assumptions that the ruler being used has a constant length, and that its shape is regular enough. Moreover most of physical concepts can be related to numbers only by means of instruments whose functioning is interpreted in the light of a physical principle. A thermometer works under the assumption that mercury expands when heated, and that the increase in volume is proportional to the increase in temperature; a magnetic galvanometer yields a numerical value that we can consider as the measure of an electric current, only if we assume a good deal of electromagnetic theory. The very meaning of these abstract concepts is given by the theories admitted by current physics. Theory is needed in order to translate what is actually observed into an abstract and symbolic judgement:

> Not one of the words serving to state the result of such an experiment directly represents a visible and tangible object; each of them has an abstract and symbolic meaning related to concrete realities only by long and complicated theoretical intermediaries.[5]

Mathematical properties cannot be read off from the sensible appearance of an object, as for instance hardness or colours. The decision to describe a phenomenon in mathematical terms requires the adoption of theoretical assumptions about the phenomenon and about its relations with other phenomena.[6]

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[5] ASPT, p. 148.

[6] Let us by *physical property* mean here a definite value of a physical magnitude which is attributed to an object through an act of measurement. We are so used to considering such properties as inhering in a particular object that we tend to regard them as monadic properties or predicates. An object is red, round, hard and is 3 meter long. In the light of the previous analysis it is clear that this way of thinking is wrong. This mistaken attitude is also somehow reinforced by the fact that we normally read from components such as resistors or capacitors their physical characteristics *printed on them*. Let us consider first the simplest magnitude: length. It is certainly a monadic intrinsic property of an object its having length, but not its having a certain length. In a world in which that object were the only existing one, it would be non-sense to ask how long it is. A measurement needs a unit of measurement, hence it requires at least the existence of another object. The result is then *a ratio between two lengths*. Of course the number by itself does not tell us anything about the objects; real numbers do not inhere in things as properties do (though natural numbers might be said to inhere in sets of things). We would be then tempted to say that while a ratio cannot be true of an object, it can certainly be true of a pair of objects,

The interpretation of an experiment depends on accepted theory also in another sense. All the objects involved in the experiment must be replaced in the reasoning of the physicists by idealised versions of them. An electric wire has normally many irregularities in shape and impurities in the material it is made of, but the experimenter will be able to take into account these details only up to a certain level and, in order to do so, he will have to rely on further theoretical knowledge. Hence we need theory for the calibration of an instrument, which yields a set of values and properties that will define the idealised model of it on which the abstract reasoning will be applied. The consideration of *systematic errors* in the analysis of the data is due to the impossibility of eliminating certain undesired physical effects that interfere with the correct course of the experiment; and the determination of the value of a systematic error can be a very complicated (i.e. theory-dependent) procedure. Further it requires the presupposition that some disturbing physical effect is at work. But how can we possibly be sure that we are taking into account (if only in an approximate way) all of the perturbations produced by the environment on our experimental setting? Of course there is no such a guarantee: after taking all possible precautions, after having considered all the known kinds of physical effects, we will have simply *to assume* that no other effect is at work while the experiment is carried out.[7] In conclusion the interpretation of the observed facts requires three main stages:

1. The sentences in common-sense language describing the facts really observed must be replaced by abstract and symbolic judgements.

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and conclude that to say that a certain thing is three times and a half longer than a certain other thing is to state a *relation* between them. In this sense we could say that a number inheres in the pairs object-unit of measurement. But we face a first problem when we realise that an act of measurement can yield only an approximate value, hence, what is really the case is that an infinite set of numbers contains the possible candidates. Hence, we have a relation among two objects and a set of real numbers. Moreover the relation holds only under the assumptions implied by the method of measurement. The conclusion is that any act of measurement leads to the formulation of a triadic relation that is true under certain theoretical presuppositions. Hence it presupposes always a conceptual background that goes beyond mathematics by itself. (On the fact that measures cannot be "read off" from objects see also Cassirer in Kockelmans, 1968. Cassirer quotes Duhem at length.)

[7] This assumption turns out very often to be wrong. An important example is provided by the discovery of the X-rays. Kuhn underlines that X-rays had been certainly produced in the course of many experiments, among them even routine ones, long before Röntgen's discovery, and that their presence exerted an unnoticed influence on the results of some of those experiments. Consequently, following the identification of the X-rays, physicists had to *reinterpret* some of their experimental practices (see, Kuhn, 1962, pp. 57-59).

2. The instruments used must be mentally substituted by idealisations endowed with physical properties known in an approximate way.

3. The actual spatio-temporal region in which the phenomenon under investigation occurs must be replaced in the mind of the experimenter by a physical system fully defined by all physical properties that, as far as we know, may turn out to affect in some way the result.

The third condition corresponds essentially to the *ceteris paribus* clauses of Lakatos and states, we could say, that from the theoretical description of a phenomenon the entire world is omitted, except for what our theories assert to be relevant, once a certain degree of approximation in the physical description has been set as a target. Some of the effects will be in general produced by distant objects (as in the case of gravitational fields), hence they should be included in the "reduced world" in which the experiment is mentally situated. However it will suffice to know the effect of the distant object in the region where the experiment takes place.

As Duhem points out, it would be a mistake to think that the abstract and symbolic language adopted by the experimenter is merely a technical language, which could be, so to speak, translated word by word into the language of facts, as in the case, for instance, of the sailor-men jargon. An experimenter can certainly translate into facts a statement such as "there is here a current of 3 Ampere", but he can do it in an infinity of different ways. For instance any circuit with an appropriate generator, connected with any appropriate kind of measurement device will constitute a good practical realisation of the theoretical situation reported in the sentence. In each of these cases what a naive observer would see in the laboratory is a completely different thing. In each case what is actually observed would be a visible effect (such as the position of a pointer) which is connected through a chain of events to the presence of the current, and the chain of events itself is not observed, but postulated by a theory. It is because we accept a certain description of the effects produced by electric currents on magnets, that we can interpret the movement of the pointer in a magnetic galvanometer as the clear sign that the current is on. Moreover no single observable event enables by itself the experimenter to draw a definite conclusion. It may happen that the pointer of the galvanometer stands still, while there are sparks in some breaks in the conductor or it may happen that a resistor is becoming warm. We would then say that there is something wrong with the galvanometer and that the current is indeed on. Hence the (so to speak) factual correspondent of statements such as "there is here a current of 3 Ampere" or "the current is on" is an infinite set of different observable situations, which in virtue of the accepted theories ("in virtue of constant relations among diverse experimental laws"[8]) are assumed to reveal the presence of the current and are basically *identified from the theoretical point of view*.

> The disparity between the *practical* fact really observed, and the *theoretical* fact, the symbolic, abstract formula stated by the physicist, is revealed to us when very different concrete facts interpreted by a theory fuse into one another to constitute but one and the same experiment, and are expressed by a single symbolic proposition: the same theoretical fact may correspond to an infinity of distinct practical facts.[9]

If we remember our earlier result about the mismatch between practical facts and theoretical facts drawn from the necessarily approximate character of any measurement, we can conclude with Duhem:

> A single theoretical fact may be translated into an infinity of disparate practical fact; a single practical fact corresponds to an infinity of incompatible theoretical facts.[10]

What does Duhem means here for *practical fact*? As I anticipated before Duhem does not give to this expression a very clear meaning. A practical fact is what would be observed during an experiment by a person without any knowledge of physics.[11] Hence its description could for instance include the fact that a pointer is in a certain position, not the reading of the pointer expressed in number and unit of measurement.

[8] ASPT, p. 151.
[9] ASPT, p. 151-152.
[10] ASPT, p. 152.

The naive observer could at best say that the pointer is in the position labelled by a certain symbol, but he would not be able to say that this means, for instance, that the pressure of a gas has a certain value. As we have already noticed, any judgement concerning physical magnitudes is meaningful only against a background of physical theory.[12] On the other hand, Duhem seems to refer to a practical fact as what the experimenter would report in her notebook,[13] for instance "under such and such conditions the pressure of the gas is 125,2 Pascal, with an uncertainty of 0.1 Pascal" or, in general, as the rough mathematical description of a phenomenon that experimental practices allow to attain. This fact can be translated in an infinity of incompatible theoretical facts (every value of pressure between 125.1 and 125.3 Pascal). However, it has to be noticed that in the latter case we have an abstract and symbolic judgement, even though approximate, which may be further refined on the ground of the so-called "analysis or errors". Hence the big interpretative step has been taken exactly in order to pass from the first to the second kind of "practical fact". Physical theory is what is needed to interpret a "recital of concrete and obvious facts" as an experiment yielding a result in mathematical terms. The second stage is to take into account all the possible sources of systematic errors, hence obtaining a result with a better approximation. This procedure draws heavily on physical theory, but, it must be stressed, it is applied to data already formulated in the language of physics. The final result obtained in this way is thereafter compared with the prediction of the theory. Only at this level does it become relevant the fact that mathematical physics considers different and incompatible two values that the "coarse" experimenter will identify.

It is certainly better to refer to these two kinds of facts with different terms: I will call a fact as observed by the layman a *brute fact* while I will refer to the fact reported by the experimenter as a *practical fact*.[14] A practical fact can be more or less analysed and sophisticated according to the details of data-elaboration, but it will be always

[11] ASPT, p. 151.
[12] It is reasonable to assume that a layman is capable of counting and we may also allow him the ability to observe that the metal bar of a tangent galvanometer has deviated from its position *roughly* by a certain angle. However, none of his reports could ever make reference to physical units of measurement, which all imply commitment to physical theories.
[13] ASPT, p. 134.

formulated in approximate terms. It will therefore be sharply separated from the corresponding theoretical facts, which will be described in terms of pure idealisations and definite mathematical values and will be infinite in number.

Going back to the first example suggested by Duhem, let us imagine to be studying the thermal properties of a rigid body. The brute fact will be constituted by the experiences of hardness, shape and warmth and other ordinary denotations; the practical fact will consist of approximate descriptions in physico-mathematical terms; finally, the theoretical fact will be one of the many exact physico-mathematical descriptions compatible with the approximate one. According to this interpretation, the second quotation at page 15 should be read as referring to the relation between theoretical facts and brute facts.

One could suggest that physical measurement is needed to weld the brute fact with the practical facts, not, certainly, the latter with theoretical facts. But here again we face a problem. In the simple situation just described, the brute facts can be thought of as pre-existing the use of measurement instruments: we may imagine having the rigid body situated on a table and deciding that what we "see" at the moment constitutes the brute fact. We then add a thermometer and we proceed in the act of translating the brute fact into a set of approximate mathematical data. In this case such reconstruction is plausible, however it does not correspond to Duhem's intent. The analysis he develops in chapter IV clearly aims at including in the brute facts the measurement instruments as mere objects and the aspects of their workings accessible to the senses. Hence, even the act of measurement has a purely phenomenal aspect, which subsequently is interpreted in the light of theoretical knowledge. In this picture it is the *interpretative moment* that welds what is actually observed with the world of abstractions and symbols. This descriptive approach is justified by the fact that in a physical experiment of a certain complexity it may well be very hard to isolate within the experimental setting what the phenomenon under investigation is by itself, from what is added in order to measure some of its inherent physical magnitudes. Moreover in this way Duhem makes clear that a measuring device is not a sort of magic camera

[14] The ambiguity of Duhem's terminology has lead, for instance, Mary Hesse to identify without remainder the practical facts with the *raw data* of the experimenter, (see Hesse in Harding, 1976, p. 186).

which takes "mathematical pictures" of what we observe with the eyes; rather it is something causally related to the object of inquiry, in virtue of known empirical laws.

1.2.1 First Consequences

Let us quote Duhem's conclusion and then try to analyse its implications:[15]

> Between the phenomena really observed in the course of an experiment and the result formulated by the physicist, there is interpolated a very complex intellectual elaboration which substitutes for the recital of concrete facts an abstract and symbolic judgement.[16]

In this quotation we find the roots of Duhem's *holistic thesis about theory testing*. Duhem contrasts the status of purely observational sciences, such as anatomy, whose relation with experience is very much analogous to the one of common-sense knowledge, with the peculiar status of physics. The fact that to the theoretical fact "there is current of 3 Ampere" there corresponds an infinity of concrete realisations has the following major consequences:

1. The meaning of physical terms or judgements cannot be specified with any ostensive procedure; no single class of observable facts by itself can ever embody the idea of, say, electric current.

2. The abstract and symbolic judgements of physics can be linked to a direct observation only relying on theoretical presuppositions. No single immediate observation (such as: "the pointer of the manometer is in division b") has any import outside a network of assumptions (trivially, the manometer may be out of order, or there may be a distorting effect in the laboratory because of which the manometer *does not read the value of the pressure*.)

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[15] Not all of the following implications are explicitly stated by Duhem in the same form (this is true most of all of consequence number four). I believe they can be seen as deriving from Duhem's analysis.
[16] ASPT, p. 153.

3. Hence such judgements can be true only under the condition that there is no mistake in our presuppositions or in the way they have been related to the particular experimental situation. Their truth its, by definition, *conditioned* (or relative), in the sense that it does not result directly from a confrontation with given facts, being dependent upon the truth of other beliefs. Those beliefs, in turn, are themselves related to experience only indirectly.

4. If we believe that the source of meaning (if there is one) of an abstract and symbolic judgement is to be found in the concrete situation in which we could declare it true, we face the problem that the infinite set of such situations, we might well say, the equivalence class of empirical situations that is conceptually grouped by the judgement, is defined by physical theory itself.

On the other hand, that a practical fact can be translated into an infinity of theoretical facts, implies, in conjunction with what has already been established, that:

1. The abstract and symbolic judgements of physics are always approximate.

2. Although it is in principle always possible to narrow down the "indeterminacy of translation", by improving the instruments and the physical idealisation representing the concrete experimental setting, the indeterminacy will never be entirely removed. Besides, the enhanced precision will be in most cases purchased at the price of having to rely on more physical theory. The measure of the length of a rod with a ruler and the naked eye requires a minimum amount of theoretical assumptions but leads to a result with an error of, at best, half a millimetre. To attain in the same kind of measurement a level of precision one thousand times greater, we may employ optical means.[17] A huge body of scientific theorising will be therefore relied upon, both for the realisation of the required procedure and for the control over possible interference from the environment (for instance, virtually no variation of the temperature could be allowed in the laboratory).

[17] See Giannoni in Harding, 1976, p. 165.

3. Hence it is possible to conclude that a phenomenon reveals its fine structure only under the condition that a deep knowledge of its relations with other phenomena has been attained, which in turn requires a vast network of physical theories. A *deeper* knowledge requires a more *extensive* one.

1.3 The Duhem Thesis

We can now state the Duhem Thesis in its original formulation. The foregoing analysis leads to the following conclusion:

> In sum, the physicist can never subject an isolated hypothesis to experimental test, but only a whole group of hypotheses; when the experiment is in disagreement with his predictions, what he learns is that at least one of the hypotheses constituting this group is unacceptable and ought to be modified; but the experiment does not designate which one should be changed.[18]

Traditionally the claims summarised in this passage have been rephrased in two distinct theses:

The Separability Thesis (S): No single or individual theoretical hypothesis by itself has any observable consequences.[19]

The Falsifiability Thesis (F): No single theoretical hypothesis can be conclusively falsified by any observations.

Philip Quinn[20] has tried to show that F is not a logical consequence of S. In his view holism does not prohibit that the conjunction of *two* hypotheses (H_i & H_j) may have an

[18] ASPT, p. 187.
[19] A long conjunction of hypotheses should not be considered as a "single or individual hypothesis".
[20] Quinn, 1969, pp. 47-48.

observable consequence O (which will consist of the description of a brute fact). He then imagines a case in which one component of such a pair of hypotheses implying the observable consequence, say H_i, is conclusively verified. In this situation an empirical finding (– O) contradicting the prediction of the conjunction would unambiguously falsify H_j. Quinn concludes that F is not a logical consequence of S, for in order to establish F one needs to rule out the possibility that a hypothesis can be conclusively verified. He then retracted his opinion[21] following a criticism by Nancy Tuana[22], who has showed that in the above situation S cannot be true. If the hypothesis H_i has been conclusively verified, argues Tuana, there must exist a set of observation sentences O* whose truth implies the truth of H_i. But then the conjunction of – O and O* entails the falsity of H_j, from which we can derive by logical rules that H_j itself entails the disjunction of O and – O*. Hence in the situation discussed by Quinn H_j would have its own empirical consequences against thesis S.[23]

Tuana also shows that F is a logical consequence of S. If we assume that F is false, this means that a particular hypothesis H_i can be conclusively falsified by observation. Interpreting this possibility to mean that there is an observation O that entails the falsity of H_i, we can easily see that in this situation H_i would entail – O, against what stated by S.

I do agree that F is a consequence of S, but I have other reservations about Quinn's arguments that go beyond Tuana's criticism. Quinn clearly assumes that it is meaningful to consider scientific hypotheses as sharply distinguished units. In the light of Duhem's analysis this seems doubtful. The trick of considering the conjunction of *two* hypotheses in order to overcome the prohibition resulting from S is hardly acceptable when the practical difficulty of *counting* the hypotheses involved in an experiment is taken into consideration. If the experimenter assumes that the presence in the laboratory of his body and that of his assistant are not distorting an electrical field under study, how many hypotheses is he relying on? Should he simply assume that two human bodies of a certain mass have negligible electromagnetic effects, or should he be more careful and distinguish between the potential effect of the experimenters' bodies and that due to the metal watches they are wearing? Is he

[21] Quinn, 1978.
[22] Tuana, 1978, p. 458.

accepting a hypothesis concerning the sheer total mass of the bodies or does it make a difference for him if that mass is distributed in two parts that *move around* independently in the laboratory? An experimenter normally works in an ocean of implicit assumptions, he is normally not even aware of, and that would resist a precise enumeration. When something goes wrong he does not have at hand a full list of hypotheses to read through in search of the one to blame; he has to understand the cause of the failure, and this may require a good deal of imagination and further experimentation. The vagueness of the so-called *ceteris paribus* clauses should suffice to convince of this.[24] I will add later further arguments to the effect that the interpretation of an experiment is always based on a kind of *pragmatic* judgement that cannot be equated to the clear statement of a conjunction of hypotheses from which the result of the experiment can be literally deduced.

Besides, the idea that a theoretical hypothesis can be conclusively established by observation runs against the simplest analysis of inductive inference. Of course Quinn is trying to make a logical point, he is trying to understand whether F is, strictly speaking, a *logical consequence* of S. But if Quinn was so worried about formal deduction, he should have also considered the case of a conjunction of two hypotheses (let us for a moment suppose that it makes sense to talk in this way), one of which is known by the experimenter to be true *a priori*, and that jointly imply an observable consequence. In this case, if the observation runs against the prediction we would know which of our two hypotheses is to be blamed. Quinn would then want to say that F is not a logical consequence of S alone, but of the conjunction of S with a thesis such as "there exists no sentence known to be true a priori, that jointly with an empirical hypothesis entails observable consequences." [25] Note that Tuana's criticism would not

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[23] This argument is endorsed also by Roger Ariew (1984), who rightly insists on the centrality of S in Duhem's thought.

[24] The same consideration would apply if one were to reformulate Quinn's argument considering the conjunction of n hypotheses n-1 of which are conclusively confirmed by observations (as it is done for instance by Curd and Cover, 1998, p. 356).

[25] In the context of Duhem's holism, the propositions of mathematics and geometry are not considered literally as part of the backlog of assumptions the physicists work with, for according to him their truth is established independently of any speculation about the external world (as it is well-known Quine holds a contrary view on this point, see further, section 2.2). Therefore the alleged sentences known to be true a priori should amount to something like the Kantian universal laws of nature, or like the Cartesian fundamental principles of natural philosophy, such as the non-existence of vacuum (Kant, for instance, thought that the dependence of gravitational attraction from the inverse of the square of the distance constituted a logical necessity, see Kant, 1953, pp. 82-84). The possibility of a priori physical

apply to this argument, for it is based on the assumption that it is a set O* of observations that conclusively establishes the truth of a hypothesis.

I have insisted on this minor point only because I believe that Quinn's error is indicative of a tendency to reduce the problem of holism to a logical puzzle. The real import of Duhem's analysis is to be found in the discussion of the role of theory in the actual practice of experimentation, and it is born out of a detailed study of how physicists really work. Also in this sense Duhem was anticipating the general attitude of post-positivist epistemology.[26] It is the real complexity of experimentation that must be taken into account, if the nature of holism is to be understood.

Let us now summarise the consequences of the Duhem Thesis:

1. In order to test a physical hypothesis it is necessary to rely on a wide set of assumptions. The more precision is required in the experiment, the wider the set of background assumptions will be.

2. Hence a contradiction between the result of the experiment (i.e. between the theoretical interpretation of the experiment) and the consequences derived from the introduction of the hypothesis under test cannot be considered a conclusive refutation of the latter. Experience does not tell us which one of our assumptions needs to be replaced. Refutation is always ambiguous.

3. Physics is not a structure made out of hypotheses that can be compared with experience one by one. It is a holistic system that faces the judgement of experience, maybe not in its entirety, but only through the involvement of a large part of it. In general this portion increases with the level of precision required.

4. *There is no crucial experiment in physics* leading to the conclusive refutation of a hypothesis, since it is always possible to blame one of the assumptions that make

knowledge is not even considered by contemporary physicists, but the same can be said about the possibility of establishing conclusively the truth of a hypothesis with a finite set of observations.

[26] In addition to the importance attributed by Duhem to the theory-ladenness of experimentation and, from a methodological point of view, to the study of the history of science.

possible its being put under test, or its connection with the rest of our theoretical knowledge.[27]

5. Experiments and logic, whether deductive or inductive are not capable by themselves of guiding the choice of the physicist. There is a degree of freedom in the evaluation of the experimental evidence. The physicist must decide also on the ground of his "good sense" whether a hypothesis needs to be abandoned in the face of contrary experience.

1.4 The motivation for the Duhem Thesis: the theory-ladenness of experimentation.

In order to test Newton's law of gravitation we must surely rely on other purely theoretical hypotheses such as the principles of dynamics. This case of holistic entanglement has nothing to do with the particular experimental technique that is adopted in the course of a specific test. The inseparability of Newton's law from the classical principles of dynamics (or from hypothetical substitutes of them) is purely theoretical; crucially *it would be present even in a thought-experiment.* This is the situation that we are more likely to find in testing any abstract law or hypothesis.[28] However Duhem's analysis, as just exposed, besides providing examples of various kinds, yields a *general argument* for holism: the theory-ladenness of the interpretation of the observable aspect of an experiment. The *necessarily* holistic nature of theory testing in physics is a consequence of the need to translate what is actually observed into the language of physics. This translation ends up in a *scientific judgement* that is

[27] This is a logical consequence of holism. Duhem is aware of it, but he also gives another argument (independent from holism) to show that crucial experiments are impossible: the physicist can never be sure that the two theoretical alternatives tested in a so-called crucial experiment are the only conceivable ones. This in turn refutes the idea, again independently of holism, that a crucial experiment can conclusively demonstrate a hypothesis true, by refuting its competitor (see ASPT, pp. 180-190). For a detailed analysis of this twofold attack on the idea of crucial experiment, see Quinn, 1969, pp. 39-46.

[28] Adolf Grünbaum has individuated another source of holism (Grünbaum in Nagel, 1971, pp. 72-74). Some hypotheses need simply to be made *precise* before they can be tested. Grünbaum mentions the hypothesis of the spontaneous generation of life. In order to test the theory we have to add the qualification that the time in which abiogenesis should occur needs to be very short compared to man's life span. Indeed without this qualification the theory of abiogenesis, far from being refuted, is correctly accepted by those who investigate the origin of life on the Earth!

built (also) upon several common-sense judgements; the difference between these two kinds of judgement being that the latter is based on ordinary observation, while the former is dependent upon a set of theoretical assumptions.

Duhem's holism is based on the claim that there is a sharp discontinuity between common-sense knowledge and physical knowledge, between *observation* and *experiment*.[29] Holism is, in physics, a consequence of the *theory-ladenness of experimentation*.

1.5 The Duhem Thesis and underdetermination

How does the ambiguity of falsification relate to the usual formulation of underdetermination, i.e. empirical evidence underdetermines the choice of theories?

It is very important to notice that, although Duhem uses very often the notion of underdetermination in order to discredit scientific realism, he does not stress the relationship between that notion and his holistic thesis. Duhem sees underdetermination essentially as a logical consequence of the hypothetico-deductive method. Given that the theorist can do no more than suggest a hypothesis consistent with the known phenomena, he can never be sure that some other hypotheses would not do the same job just as well. Duhem, on this point, has not much more to add to what Aquinas said in his commentary on Aristotle's *De Caelo* about the cosmological systems of the time:

> The assumptions of those people (the astrologers) are not necessarily true. Although they save the appearances by suppositions constructed in this way, one ought not to say that these suppositions are true, because one might save the appearances concerning the stars equally well by means of some other method not yet understood by men.[30]

[29] Alexandre Koyré has stressed the fact that the innovative aspect of Scientific Revolution consisted not so much in the recognition of the importance of observation (astronomers and naturalists had always based their work on observation, and, up to a certain extent, this is true also of the alchemists), but rather in the adoption of a methodology based on *experimentation*. (See, for instance, Koyré, 1943, p. 401).

[30] Aquinas, Ad. Lect. XVII, book 2, ii. Quoted by Duhem in *Physics and Metaphysics*, EHPS, p. 41.

Seen in this way underdetermination has nothing to do with holism. Indeed in *The Aim and Structure* the arguments against realism based on underdetermination (a word that, by the way, Duhem never uses) precede the famous discussion of the relation between theory and experiment that constitutes the basis of the Duhem Thesis.[31]

The question is then whether holism adds further credibility to the thesis that empirical evidence underdetermines the choice of theory. The first thing to stress is that the logical possibility to retain a hypothesis in the face of a disconfirming result by blaming another hypothesis assumed in the course of the experiment, does not ensure us in any way that the hypothesis retained can be made part of a non trivial explanation of that result. Duhem never upholds such a view,[32] which lacks logical support. If this were the case, then the step from holism to underdetermination would be, up to a certain extent, a short one.

In point of logic, from the fact that scientific hypotheses apply to phenomena only collectively it does not follow that there are many empirically equivalent theories for each range of phenomena. Moreover, as I will explain later, *the theory-ladennes of experimentation renders problematic the very idea of empirical equivalence (and hence of underdetermination)*.

What can be said instead is that the holistic nature of theory testing adds new degrees of freedom to the theorist's *search* for a successful theory. This is certainly what Duhem had in mind while discussing the case in which an experiment has refuted a system of hypotheses:

> No absolute principle directs this inquiry, which different physicists may conduct in very different ways without having the right to accuse one another of illogicality. For instance, one may be obliged to safeguard certain fundamental hypotheses while he tries to reestablish harmony between the consequences of the theory and the facts by complicating the schematism in which these hypotheses are applied, by invoking various causes of error, and by multiplying corrections. The next physicist, disdainful of these complicated artificial

[31] Certainly the holistic thesis lends further support to Duhem's antirealist conception of physical theory.
[32] Contrary to what Grünbaum asserted in one of his articles on the subject (Grünbaum, in Harding, 1976, p. 117). Also Quine is explicit on this point.

procedures, may decide to change some one of the essential assumptions supporting the entire system. The first physicist does not have the right to condemn in advance the boldness of the second one, nor does the latter have the right to treat the timidity of the first physicist as absurd.[33]

Here we have the synthetic description of two different research programmes.[34] Holism increases the number of paths that the physicist can legitimately follow in the light of experimental results. In sum:

Holism enhances the possibility of the multiplication of research programmes.

What the destiny of these research programmes will be, is a different matter. Certainly these further degrees of freedom in the construction of theories render much more *plausible* that different underdetermined theories may be build; but we cannot go much further than this.[35]

1.5.1 Physical laws: underdetermination due to approximation and due to symbolic expression

In this section I will analyse the consequences of Duhem's analysis concerning the status of physical laws: they are symbolic and approximate. In doing so we will find two types of underdetermination that are peculiar to physics. We will also have the chance to see in a different way in what sense the duhemian account of experimentation *lends support* to underdetermination. In doing so I will again draw consequences that are not explicitly stated by Duhem.

Due to the one-to-many correspondence between a practical fact and its theoretical exact counterparts, any set of data is compatible with an infinity of different physical

[33] ASPT, pp. 216-217.
[34] I use this expression in a broader and more liberal sense than the strictly lakatosian one.
[35] Duhem believed that in case both of the physicists mentioned in the quotation succeeded in their search for consistency with experiments, only good sense could lead to the correct choice of which theory should be accepted (ASPT pp. 217-248).

laws, no matter how much the precision of the experiments has been improved. Hence, according to Duhem, physical laws are strictly speaking neither true nor false, they are approximate.[36] Moreover they are provisional, since an improvement in the experimental technique, or else, in the theoretical analysis might reveal that an accepted law must be corrected. We find here a first example of a kind of underdetermination. It is clear that empirical data leave us the freedom to choose among infinite alternatives. I believe this is the only non-trivial kind of underdetermination that is likely to be in action at each step of the work of a physicist. By itself it is not so much a consequence of holism, but simply of the fact that physics adopts a mathematical language (a fact which is, of course, also a major source of holism). To be sure, it is really a kind of pragmatic underdetermination, rather than one in principle, for the different laws do make slightly different predictions. However, no matter how much we improve the precision of our measurement, we will never get rid completely of the indeterminacy due to approximation.

Let us see how this kind of underdetermination can be dealt with in practice. How can we direct our choice? To look for simplicity and elegance is a normal way of going about, but following it will not always suffice (not to mention the everlasting problem of defining those concepts unambiguously). Many alternatives, Duhem points out, are naturally discarded on the ground of their incompatibility with physical theory. He argues however that, the latter being only a means of classifying known physical regularities, we will not be able to rely on it as if it were an impartial truthful tribunal. Suppose that from an accepted theory we deduce a law, which corresponds with the empirical data within the limit of error. An infinite number of other laws will indeed fit the data just as well, although they might be in contrast with the predictions of the theory. In such a case, every physicist would regard the experiment as a success for the theory and would retain the law deduced from it, unless he or she had in mind an alternative theory from which one of the rival laws could be deduced.

[36] This Duhemian tenet follows from the preceding analysis only if an antirealist conception of truth is accepted. A realist could try to argue that, although we can only know a law in an approximate way, there is in reality *the* true variant of it. Duhem is not entirely explicit about his conception of truth. However he certainly would reject a realist account of the truth of the laws of nature. The situation is somewhat complicated by Duhem's *faith* (as he would call it) that physical theory tends in the long run to approximate a *natural classification* of phenomena; but this faith is kept by Duhem outside his *logical* reconstruction of the cognitive status of physics (see ASPT, pp. 19-30, 293-298 and EHPS, pp. 68, 236-237).

Let us consider Duhem's example. We want to determine the daily path of the sun in Paris with the highest precision attainable at the moment. After using the best optical instruments and taking into account all known effects influencing the results of the observation (due for instance to the presence of the atmosphere), we will find ourselves with a bundle constituted by an infinity of different trajectories. Further, an infinite subset of this bundle will be compatible with our celestial mechanics. *To suggest that this subset should be chosen means to supplement the demand that theories be correspondent to experiments with a coherence criterion.* It should also be noticed that, in this case, there seem to be sharp boundaries to the possible theoretical choices underdetermined by empirical data: all trajectories are roughly the same: the sun rises from the east and sets every day in the west. The absolute truth of common-sense laws sets a limit to the possible divergence of acceptable experimental results.

Duhem would certainly be very pleased by this conclusion, which indeed can be considered as a direct consequence of the role he attributes to *le bon sense* in his methodology. As a painter can improve a portrait by adding or modifying its details, but not by altering it completely, similarly physics can make our common-sense knowledge more precise, but will have to preserve its essential traits.[37] It is this kind of methodological attitude that leads Duhem to regard the theory of Special Relativity as literally absurd.[38]

Physical laws are symbolic expressions because they express connections of theoretical facts. Let us contrast again physical laws with common-sense ones. It is not the case that the mere introduction of symbols in the expression of a law makes a great difference from an epistemological point of view, contrary to what Duhem suggests at some point.[39] Rather, as he later explains clearly, what matters is that the symbols of physical laws owe their meaning to physical theory. Only physical theory can relate them to actual states of affairs through their double function of providing and interpreting measurement procedures. Indeed to express the common-sense law "all men are mortal" in a symbolic way such as "(x) M(X)<Mort(x)", will not affect its meaning and truth value. On the contrary:

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[37] The metaphor of the painter is very frequent in Duhem's writings.

[38] According to Duhem it is an undeniable fact of experience that space and time are independent of one another (see EHPS, pp. 270-273).

According to whether we adopt one theory or another, the very words which figure in a physical law change their meaning, so that the law may be accepted by one physicist who admits a certain theory and rejected by another physicist who admits some other theory. (...) A physical law is a symbolic relation whose application to concrete reality requires that a whole group of laws be known and accepted.[40]

From the symbolic character of physical laws it also follows again that they are provisional, but in a different sense. No physical law has a scope of application that can be defined once and forever, for the simple reason that there is an infinity of different conditions under which it can be tested. The law of compressibility and expansion of oxygen ceases to be valid if the gas under study is placed between the plates of a strongly charged condenser: it must be substituted by a more complex expression containing the symbol representing the electric field. In what sense has the law changed? In the sense that the original law has now a more restricted field of application. We can never be sure that an analogous modification will not be needed as a consequence of new kinds of experiments. This fact is a consequence of the symbolic character of laws, because

> ... there are always cases in which the symbols related by a law are no longer capable of representing reality in a satisfactory manner.[41]

While the terms present in common-sense laws correspond to a fixed class of objects or facts, the symbols of theoretical physics are introduced in the description of a range of phenomena whose boundaries can be redefined by further experimentation.

The preceding quotations suggest the possibility of a kind of underdetermination different from the one due to approximation: *it is sometimes possible to choose different symbols with a different meaning in order to represent the same experimental situation*. A simple example, I believe, is provided by the attempts to explain capillary phenomena. In the seventeenth-century many physicists belonging to the Newtonian

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[39] ASPT, p. 168.
[40] ASPT, p. 167-168.
[41] ASPT, p. 174.

school (such as Laplace) argued that the mathematical form of Newton's equation was to be corrected in order to explain the phenomenon of capillary attraction.[42] We now interpret the latter as a consequence of electromagnetic interaction between molecules. Thus we have two theoretical pictures that invoke different theoretical concepts and would translate the same brute facts into two essentially different symbolic judgements.

Hence we find here two different sources of underdetermination: approximation and symbolism. It is clear that even if it were possible to perform measurements that lead to exact mathematical results, we could not rule out the *possibility* that more than one theory implying these results could be devised. However, the approximation necessarily involved in any empirical data enhances the likelihood of there being (from a pragmatic point of view) empirically equivalent theories, since two theories may be said, in practice, to be empirically equivalent[43] and hence underdetermined by empirical evidence, just in case their predictions differ by a (at the moment) non-detectable amount. Approximation is sufficient but not necessary to the presence of underdetermination.

1.6 To save the phenomena

Another formulation of the problem of underdetermination could be the following: there are different ways of "saving" the same phenomena. The problem is that in order to be meaningful this formulation must include a specification of what the phenomena that should be saved by a theory amount to. We assume that we are confronted with something fixed and given on one side, the phenomena, and with a set of possibilities on the other, the set of theories underdetermined by the phenomena. Here we face the most serious problem of this analysis: what do we mean by phenomena? In this section I will try to answer to this question drawing on *Duhem's philosophy of physics.* For

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[42] Curiously enough, Duhem himself still considered this the only available solution (see ASPT, p. 177), and coherently he does not use the example of capillary phenomena to illustrate underdetermination.
[43] See further for a more detailed account of the notion of empirical equivalence.

this purpose I will contrast the different roles assumed by the observable phenomena respectively in ancient astronomy and in modern physical experimentation.[44]

One of the chief examples of underdetermination arose in the context of the ancient debate over cosmological systems. The various schools of astronomers were proposing different ways to save the same phenomena,[45] that is the apparent motions of celestial bodies. It should be noticed however that the different cosmological systems were meant to provide quantitative predictions about the motions, for instance, of the planets. For an astronomical theory, to save the phenomena means to predict the mathematical translation of what is observed in the sky. Indeed an astronomical theory is a system of geometrical and mathematical postulates; hence it can be used *only* for the deduction of prediction in *mathematical form*. We thus find five different levels:

1. The sensible appearances in the sky of phenomena such as stars, phases of the moon, eclipses, etc. (i.e. brute facts).

2. The approximate description of these phenomena in geometrical and mathematical terms (i.e. practical facts).

3. The infinite sets of theoretical facts corresponding to the practical facts.

4. The corresponding predictions of the various theories.

5. The *different* theories.

This schematisation suggests the following

Definition: a theory T *saves the phenomena* if it maps past practical facts into predicted practical facts that are *compatible* with the practical facts that actually obtain.

[44] Duhem does insist that the aim of physical theory is to save the phenomena, but he does not seem to realise that his analysis of the relation between theory and experiment, an analysis that, as we have seen, emphasises the theory-ladenness of experimental practices, renders more difficult to appreciate the meaning of that claim.
[45] For simplicity, I will assume in this discussion that the determination of the celestial motions was unproblematic. This is, of course, historically false.

In order to test a theory we need (so to speak) to *feed* into it the results of some observations, that is some practical facts, and then work out a prediction. The prediction will be a practical fact, which is an infinite set of theoretical facts (typically an interval centred on a particular value). The final step consists of comparing the prediction with the result of a new observation that is with another practical fact. What is required for the theory to pass the test, is not that the predicted practical fact be identical with the observed one, but *compatible*. In practice we can often talk of compatibility when the value at the centre of the interval defining the predicted practical fact lies within the interval defining the observed practical fact.[46]

Let us analyse the consequences of this definition for physical science in general:

1. If by *observational basis* we mean what is (or should be) *actually used* to confirm or disconfirm theories, the observational basis of physics is constituted by practical facts.

2. Thus the *phenomena* saved by physical theory are the practical facts, not what is actually experienced.

3. We can say that we face a problem of underdetermination when more than one theory saves the phenomena.

4. In general those underdetermined theories would make slightly different predictions, that, however, will be compatible with the same practical facts.

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[46] This picture is very simplified and idealised, mainly for two reasons:
1. There are many different opinions about how to define the so-called *interval of confidence* associated with a measurement result, even in the context of the same *physical* analysis of the experiment. The set of theoretical facts corresponding to a practical fact has, in general, vague boundaries. In most cases physicists would assume that the agreement between theoretical predictions and experimental results comes in degree. In other words, there are many ways, according to the situation, to define the concept of *compatibility*.
2. The relation between theory and experiment is usually more complex. In real science competing theories will in general all disagree with some facts. Moreover, mainly for the problem of *ceteris paribus* clauses, the acknowledgement of anomalies can be very controversial. Finally, the lack both of computational methods and of experimental techniques may prevent physicists from actually performing various tests that could be in principle highly significant.

5. Hence what remain fixed, while we consider the various underdetermined theories, are the "observed" practical facts, not the theoretical facts.

6. In conclusion, to say that physical theory is aimed at saving the phenomena means to say that its aim is compatibility with practical facts.

7. Theories are not underdetermined by the brute facts, but by their interpretations.

8. We notice again the various competing theories can indeed be very different, but none of them could ever be in sharp (brute?) disagreement with brute facts. This is a weak sense in which one could say that physical theories save what is actually manifest to the senses.

The first of the five levels of description is (following Duhem) the only "purely" empirical one. In order to get from the first to second level we need to replace celestial bodies with geometrical idealisations and then refer to the latter the mathematical descriptions of the apparent motions. This second level is what is saved by the theories. Nevertheless, between the first and the second level there is in this case a good correspondence: on one hand the theoretical interpretation required is in this case very limited, on the other hand even at the second level of description we are adopting symbols that refer directly to the *individual* objects belonging to the first level: this circle in the map of the sky represents *the Moon,* this point represents *Mars.* This is why consequence number 8 is particularly appropriate in this case. The translation between the first and the second level is so immediate (the "linguistic" gap is so narrow) that the theories can be said, with some licence, to entail (also) what is directly perceived with the eyes.

Can a physical experiment be described in such a way that we could individuate certain aspects of the experiment that are purely empirical and contrast them with those that are theoretical? If we adopt Duhem's point of view about common-sense knowledge the answer seems to be positive. We can describe the physical apparatus, its parts and the place in which it has been situated. We can moreover describe the part of

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Therefore, "empirically equivalent" is, in practice, a vague predicate.

the apparatus that will yield a readable result. Let us restrict ourselves to the case in which a certain measurement is being performed. Any such experiment will require a certain set of objects, connected to each other in a certain way and ultimately connected to a device on which a number can be read. The entire apparatus, including the mode of presentation of the reading, will constitute part of the "phenomenal side" of the experiment. Only the conventional link between the symbols adopted and the real number concepts to which they refer has no physical character whatsoever.

According to Duhem it would be possible in principle to describe minutely each of these components adopting a common-sense language. For that purpose we should simply describe them in the same way a person without any knowledge of physics would do.[47] The word "conductor", for instance, could not figure in this description, since it is parasitic on the concept of charge and along with it on a good deal of theory about electrical phenomena; similarly all physical concepts such as "temperature", "pressure" or "electrical current" should be omitted. A word such as "metal" or "copper" on the other hand would be admissible.

It is by now time to notice that things are not quite as easy as Duhem thought. His idea that the layman-language could state common-sense facts unambiguously is beset by many difficulties. What predicates are, after all, available to our hypothetical layman? Would he be able to say that a certain column of glass is filled with mercury, or could he only mention the presence of a silver-like liquid in that column? Besides, a *fin du siecle* layman could certainly speak of metals, but with what competence? Could he tell apart iron from steel or magnetite or from a hundred different but seemingly alike metals? Not even a *fin du siecle* physicist could tell apart substances that we have now recognised/classified as different. And what kind of description would a native inhabitant of Bali make of Duhem's laboratory in *his own language*?[48] Duhem does not attempt to give an answer to these questions. His view of common-sense knowledge is rather traditional, not to say dogmatic. We will see that Quine, on the contrary, provides a sophisticated account of the relation between language and reality, which avoids the notion of unambiguous common-sense facts, and yet holds fast the distinction between the observational and the theoretical.

[47] ASPT, p. 164. Duhem stresses that the resulting translation would be extremely long and complicated and most of all obscure.

However, for the moment, I intend to follow Duhem on this point and assume that every-day-facts are obviously *given to us* in a certain way, which is in turn expressible by every-day language. The following considerations remain valid also if Quine's philosophy of language is taken into account.

Let us then suppose that we can give a "theory-free" description of an experimental setting along with its conditions throughout the execution of the experiment. What would be the relation between this description and set of phenomena (in the sense of consequence number 2) we intend to save? Could it play the same role in modern physics that the apparent motion of the planets played in the discussions among Medieval and Renaissance astronomers? The answer is *no.* Let us see why.

According to Duhem the most important part of the test consists really in the interpretation of the experiment. We compare the prediction of the theory with the interpretation of the experiment. If we find a contradiction, then we must change something in our theoretical knowledge or in the interpretation of the experiment. But we don't have a contradiction between the hypothesis under test and the empirical evidence by itself. The contradiction is not between the facts really observed and the theory, but between the latter and the idealised, theoretical interpretation of the experiment which is present in the mind of the experimenter.

We can now see the crucial difference between the execution of a normal physical experiment and the observation of the apparent motion of the planets. It lies in the fact that in the former *the actual observable appearances (reported in the layman description of the experiment) is but a representative of the equivalence class of* brute *facts that correspond to the same* practical *fact, obtained through interpretation* (the practical fact in turn will correspond to an infinity of theoretical facts, hence the relation brute-practical-theoretical fact is a many-to-one-to-many relation).

Imagine that we perform the same experiment, with the only difference that all copper wires have been replaced by silver wires endowed with the same (up to the required level of approximation) electrical characteristics as the former. The interpretation of the experiment remains unchanged. We have hence realised a unique practical fact in two different brute facts. Let us imagine that the experiment does confirm the theory, which will be in this case standard electromagnetism. Then, to say

[48] A *locus classicus* against the idea that common-sense predicates can be applied without ambiguity is:

that the theory saves the phenomena means to say that its predictions are compatible with the practical facts which a (generally) much wider body of theoretical assumptions put in correspondence with a potentially infinite class of different brute facts.

Imagine all the possible realisations of a particular oscillating circuit. It may well be that some of them have *nothing in common* from the common-sense point of view with some others; yet the physicist will consider them as instances of the same concept, the concept "oscillating circuit having such and such characteristics" (including the value of the capacity of the condenser, and so on). We could say that the *intension* of the concept is what the theory deals with; the *extension* is the set of all possible concrete realisations in terms of brute facts. The *background knowledge* is what is required in order to establish that a certain pattern of brute facts belongs to the extension of the concept, i.e. that *it constitutes an embodiment of a certain ideal-type (the concept), defined solely by abstract properties.* From now on, I will call *ideal-type* of a theory a pattern of theoretical facts defined solely in the language of the theory and complex enough to illustrate some nomological consequences of the theory. Examples of ideal-types are normally given by the physical situations described at the beginning of the exercises in standard physics textbooks. The requirement of complexity is easily understood when we think that a simple theoretical fact such as "there is a rigid rod of length 3.12 m" cannot be used by itself to illustrate the predictions of classical mechanics. In order to do this we will have to describe a more complicated situation (involving the forces acting on the rod, its degrees of freedom, etc.).

Electromagnetism enables us for instance to predict that an oscillating circuit, composed by a capacitor, an inductance and a resistor has a certain resonance frequency. Theories deal with ideal-types, which in turn represent classes of concrete situations.[49]

Hence, in an experiment of physics it can happen that nothing that is actually sensed is really necessary, because it could be replaced in virtue of background

K. Popper, 1968.

[49] Here and in the following sections I will disregard, for the sake of simplicity, the distinction that one should draw between *theoretical ideal-types* and *practical ideal-types*. This distinction obviously mirrors that between theoretical and practical facts. Strictly speaking, a concrete situation can be seen as the instantiation of an infinity of theoretical ideal-types. However I have already noticed that the biggest interpretative step is taken in relating brute facts to practical ones, not the latter to theoretical facts.

knowledge by something else, provided that this can play the same role in the experiment.

On the contrary, in the case of ancient astronomy the second and third level of description did not constitute an ideal-type, they were really representations of *something* particular. This does not seem at all surprising if we think that the theories under discussion were not properly nomological: they were descriptions of the structure of a particular set of objects (the celestial bodies). It is only with the theory of gravitation that the Sun, the Planets and the Earth got out of the picture to give way to a system of *masses* in mutual interaction (laws require abstract concepts). At the second and third level of description we now find an ideal-type consisting of a particular many-body gravitational system and the Solar System has now become just a gigantic experimental setting in which the predictions of the theory *concerning the ideal-type* are tested by means of a concrete realisation. Although Newton's theory still predicts what we actually see in the sky, even the existence of the Solar System is irrelevant for the meaning and the truth of the theory. [50]

To illustrate this fact we can consider an imaginary situation in which an ancient and a modern astronomer witness together an extraordinary event: the moon leaves its orbit and goes astray. The astonishment of the first astronomer will be due not so much to the impression that his theory has been refuted, but to the fact that the part of reality the theory was meant to describe *has changed*. Strictly speaking there is no refutation, for what has happened is that the description of a particular object, the Solar System, has been outdated by an intrinsic modification of that very object.[51] The modern astronomer would instead try to *explain* the event seeking a new ideal-type under which the Solar System can be subsumed. This attempt may or may not lead to

[50] This example explains why I prefer the term "ideal-type" to the more common "physical system". The latter is often used also to refer to particular concrete situations. The solar system is a physical system, but only an instantiation of an ideal-type, not an ideal-type. The term "model" is also used to refer to particular situations. Moreover physicists often talk about models when the resources of a theory are deemed insufficient by themselves to give a faithful account of a specific phenomenon. A model is not in this case an ideal situation to which the theory perfectly applies, but, on the contrary, an *ad hoc* description to the formulation of which the theory has only contributed.

[51] One could object to this conclusion on the ground that according to the ancient cosmology no change could occur in the heaven. However this belief was based on (meta-)physical considerations about the nature of the elements and their intrinsic properties, which astronomers where not concerned with, for their only aim was to save the actual phenomena. The imaginary situation would have upset aristotelian physics, not the methods of book-keeper-like astronomers (on the ancient separation between astronomy and physics see, for instance, Drake, 1980).

abandoning Newton's theory, depending on whether he can suggest a *theoretical interpretation* of the event consistent with Newton's theory.

The theories developed along the history of science have generally become more and more abstract. Newton's theory, or the theory of heat have still a phenomenal side that we could call in a loose way *ostensive*, for they refer to classes of objects or facts that belong also to common-sense knowledge. With electromagnetism this ostensive character has become even weaker. Finally Twentieth Century particle physics has produced theories that do not refer directly in any way to something that can be observed. Their dictionary does not contain any term that belongs to common-sense knowledge, nor that is easily related to it. With respect to them even Newtonian talk about masses seems after all referring to something open to our gaze. We cannot point at something and be sure that what we are seeing has something to do with the predictions of the standard model of particle physics, for it is only background knowledge that can tell us if and when a pattern of observable facts embodies an ideal-type of the theory. The price to be paid for enriching our knowledge with extremely abstract and general theories lies in their remoteness from every-day observation.

1.6.1 To save theory-laden phenomena

Duhem defended the thesis that the aim of physics is to *save the phenomena*.[52] The Greeks coined this expression in order to define the aim of the science at that time called astronomy. Drawing on Duhem's analysis of the relation between theory and experiment in physics, it is possible to say that the meaning of that motto has now changed radically. It is striking that Duhem himself does not show awareness of this fact. At first sight, the view suggested by it seems to imply:

1. that the phenomena are "out there", waiting for us to study them,

2. that physics seeks a correspondence between its theories and the given phenomena.

[52] See SP, p. 117.

The foregoing analysis suggests that on the contrary:

1. physical theory is concerned, not with given phenomena, but with ideal-types, that is *intellectual abstractions*;

2. the relation between the ideal-types and what is actually observed is mediated by physical theory (in general, background knowledge) that tells us what observable situation counts as an instance of an ideal-type. This is a reformulation of the thesis that experimentation is theory-laden, which in turn provides an argument for the Duhem Thesis.

3. Hence theory is necessary to shape the phenomena, which in turn cannot be considered as pre-existing ready-made entities.

4. As the gap between ideal-types and immediate experience is bridged by theory, it is impossible to confront theoretical hypotheses directly with reality. Thus physical investigation cannot be based on a naïve correspondence theory of truth. This is a reformulation of the Duhem Thesis and, indeed, a consequence of conclusion 2.

Hence, physical theory does not simply save the phenomena in the old sense, it also contributes to their very structure; it saves something that, as it is, would not exist without it.

2. QUINEAN HOLISM

Introduction

The Quine Thesis results from the interplay of the verificationist theory of meaning with epistemological holism. The outcome is an all-embracing holism, both semantical and epistemological for which only clusters of sentences have empirical significance.

In the first section I briefly recall the gradual emergence of holism within Neopositivism culminating in Quine's formulation. In the second section I compare the Quine Thesis with the Duhem Thesis.

2.1 The Quine Thesis: Holism and Verificationism

Pierre Duhem is mentioned in the famous Neopositivists' Manifesto among the inspirers of the *scientific worldview* upheld by the Vienna Circle. Carnap recognises in the *Logische Syntax der Sprache* that tests in physics involve, at the bottom level, a group of hypotheses and not just a single one. He also mentions that Duhem and Poincaré had pointed out this fact. Ayer is even more explicit than Carnap:

> When one speaks of hypotheses being verified in experience, it is important to bear in mind that it is never just a single hypothesis which an observation confirms or discredits, but always a system of hypotheses.[53]

[53] Ayer, 1946. p. 94.

He even draws the conclusion that, from a logical point of view, the falsification of a hypothesis is never conclusive.

> The "facts of experience" can never compel us to abandon a hypothesis. A man can always sustain his convictions in the face of apparently hostile evidence if he is prepared to make the necessary ad hoc assumptions.[54]

Nevertheless Quine stresses the importance of holism in overthrowing the Neopositivistic conception of knowledge:

> Carnap shows his appreciation of this [holism] by speaking well of Pierre Duhem, who with Poincaré and Milhaud, founded conventionalism.[55] Carnap, like others in the Vienna Circle, didn't follow the consequences of holism sufficiently...
>
> [The renewal of the holistic perspective] marked my separation from Carnap.[56]

We are then stricken by the impression that holism has had on the logical empiricists the effect of a time bomb. Quine introduces his holism precisely as stemming from the collapse of what he calls the dogma of *reductionism*. In its radical form the dogma amounts to the belief that any meaningful statement can be translated into a statement

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[54] Ibid., p. 95. Ayer's holism is not extended to the analytic sentences. However, as we shall see, according to Quine, once holism is taken seriously in matter of verification, then the distinction itself between analytic and synthetic sentences is doomed. The evaluation of this highly controversial quinean tenet lies outside the scope of this work.

[55] To rank Duhem as a conventionalist is inaccurate and not simply for his conception of physical theory as heading toward a natural classification of phenomena. Duhem criticises Poincaré's tenet that certain physical principles have the status of conventions that physicists will never let experience to refute. On the contrary, according to Duhem, a new systematisation of physical theory may replace them with different ones (see ASPT, pp. 208-212). Vuillemin compares Duhem's criticism of Poincaré's conception of theoretical convention with Quine's attack on the carnapian analytic sentences (see Vuillemin in Hahn and Schilpp, 1986, p. 596). It should be noted that Vuillemin does not resist the temptation of defining Duhem a pragmatist (Ibid., p. 597). However Duhem himself takes the pain to reject that label (see EHPS, p. 237). See also Maiocchi, 1990, where *The Aim and Structure* is defined "a book against conventionalism".

[56] Both quotations are taken from an interview to Quine, printed in Borradori, 1994, pp. 29-39.

about immediate experience. Carnap's attempt in *Der Logische Aufbau der Welt* was precisely that of providing such a translation in a systematic way. Besides a sense-datum language, Carnap included in his attempted translation also logic, set theory and mathematics. After realising the impossibility of achieving such a translation,[57] Carnap opted for a weaker form of reductionism. He deemed possible to introduce *reduction forms*[58] that connected sentences in theoretical language with sentences describing observable circumstances. This constituted a further attempt to develop a *verificationist theory of meaning*, based on the idea, which Quine attributes already to Peirce,[59] that the meaning of a sentence amounts to the difference that its truth would make in terms of experience. Carnap had consequently abandoned the dream of rendering from a logical point of view redundant all theoretical language. However, according to Quine, Carnap is still committed to the belief that a single statement is connected, taken in isolation, to a given class of observable circumstances that add to the likelihood of its truth.[60] Precisely these circumstances could be considered as giving factual content or empirical significance to each meaningful statement. It is on this point that Quine develops his criticism:

> The dogma of reductionism survives in the supposition that each statement, taken in isolation from its fellows, can admit of confirmation or infirmation at all. My countersuggestion, issuing essentially from Carnap's doctrine of the physical world in the *Aufbau*, is that our statements about the external world face the tribunal of sense experience not individually but only as a corporate body. [Footnote: This doctrine was well argued by Duhem, pp. 303-328. Or see Lowinger, pp. 132-140.][61]

[57] See FLPV, pp. 38-41.
[58] Carnap, 1936-1937.
[59] OR, p. 78.
[60] Quine's reading of the second Carnap is, on this point, historically questionable.
[61] FLPV, p. 41.

Much more recently Quine has declared that at the time he wrote the famous article *Two Dogmas of Empiricism* (from which the preceding quotation is taken), he did not know that Duhem had reached similar conclusions so many years before.[62]

In *Epistemology Naturalised* Quine underlines that the failure of the liberalised form of reductionism is not due to the difficulty of specifying the observational implications of a statement. Even if those were very complex and ramified, it would be possible in principle to express them in a tight form, just in the way we axiomatise in a finite formulation an infinity of theorems. The problem is by no means practical, the problem is that a single statement "has no fund of experiential implications it can call its own".[63]

Quine argues that the first dogma of empiricism, the distinction between analytic and synthetic sentences falls with the dogma of reductionism. If it were possible to talk about the range of experiences confirming of infirming a single statement, then it would be possible to individuate a class of sentences that are true *come what may*, that are, as Quine says, "vacuously confirmed"[64] by any observation. This would be the class of analytic statements.[65] The idea behind the analytic-synthetic distinction is that the truth of a sentence is partly linguistic and partly factual. When the factual part were absent, we would feel entitled to talk about analytic truth. But, holism prevents us from following this path. Precisely because a single statement does not have its fund of experiential implications, we cannot separate the factual and the linguistic components of its truth. In *Two Dogmas* Quine develops this argument in an extreme form:

> Taken collectively, science has its double dependence upon language and experience; but this duality is not significantly traceable into the statements of science taken one by one. ... The unit of empirical significance is the whole of science.[66]

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[62] Quine writes: "Both Hempel and Philipp Franck subsequently brought Duhem to my attention, so I inserted the footnote when 'Two Dogmas' was reprinted in *From a Logical Point of View*." TDR, p. 269.
[63] OR, p. 79.
[64] FLPV, p. 41.
[65] In the first part of *Two Dogmas* Quine attacks the analytic-synthetic distinction adopting a different strategy (FLPV, pp. 20-37).
[66] FLPV, p. 42.

Hence holism is according to Quine at the root of the rejection of both dogmas of empiricism (and, in particular, of logical empiricism). His theory of meaning is still a verificationist one and holism is seen by Quine precisely as resulting from the only coherent development of verificationism.

> The Vienna Circle espoused a verification theory of meaning but did not take it seriously enough.[67]

What kind of empiricism are we left with, when the two dogmas are abandoned in favour of a holistic conception of truth and meaning? Quine suggests that we should look at the whole of human knowledge as a field of force, whose boundary conditions are given by experience. All beliefs can be seen as placed at different distances from the periphery. Logic and mathematics will be right at the centre of the field and observational reports at the periphery. The conditions imposed by experience leave us great freedom in the choice of which of our beliefs to adjust when our expectations are disappointed by it. This leads to Quine's famous conclusion:

> Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system. Even a statement very close to the periphery can be held true in the face of recalcitrant experience by pleading hallucination or by amending certain statements of the kind called logical laws. Conversely, by the same token, no statement is immune to revision.[68]

What in the metaphor is called the distance from the boundary of the field of force amounts in practice simply to *our degree of willingness* to revise a certain statement in the event of infirming experience.[69] We will not even consider, in general, changing a logical rule in order to save some of our ordinary beliefs; while beliefs about common-sense objects, for instance, tend to be revised very often just in response to ordinary

[67] OR, p. 80. According to Quine his thesis on the indeterminacy of translation (see, for instance, WO, chapter II), is a consequence of the conjunction of Peirce's and Duhem's ideas (OR. pp. 80-81).
[68] FLPV, p. 43.

experiences. But also beliefs that are entrenched in our worldview can be revised under special conditions. Quine mentions two historical examples: the deviations from classical logic suggested for simplifying Quantum Mechanics and the intuitionistic rejection of the law of excluded middle in order to deal properly with the concept of infinite in mathematics. Both logic and mathematics are seen by Quine as constituted by hypotheses whose cognitive status is not different in principle from that of beliefs that are less general and intuitively more related to experience. Those hypotheses are part of the body of theory that is needed in order to imply anything observable.[70] Hence the duhemian freedom to choose which hypothesis to reject if a falsifying result is obtained extends, in principle, also to logic and mathematics.[71]

On the other hand even statements about ordinary observable events can in principle be held true come what may. Quine suggests two extreme strategies: pleading hallucination and changing the meaning of the words appearing in the statement. In practice "pleading hallucination" refers to the possibility of *disqualifying* an observational report when it conflicts with a well-established theory. It is not necessary for that purpose to turn the belief into disbelief, it is not necessary, for example, to convince ourselves that the water was not boiling when we were sure that it was in fact boiling. We can more modestly turn the belief into a *non- belief.*[72] We settle for simply eschewing the belief from our knowledge and wait for further investigation. However, as we will see, the rejection of observation reports is something we should, according to Quine, rarely indulge in.

The expedient of changing the meaning of a word in order to save a theory looks at first sight far too paradoxical to be acceptable. But here again Quine is not really suggesting to trivially adopt this expedient in the everyday practice of science; he is simply clarifying the logical implications of his philosophy of language. Indeed there

[69] As a matter of fact Quine's notion of "distance from the periphery" has also a *logical* aspect. An ordinary observation report is logically linked to a smaller number of statements than a mathematical theorem.

[70] In this way the problem that had beset the logical empiricists, and Carnap in particular, of explaining the meaningfulness of mathematics and logic (which seem devoid of factual content) is in Quine's view resolved. Both mathematics and logic are meaningful for they are part of the system of beliefs that we use to describe and predict experience. Moreover the apparent necessity of their truth is due to the fact that they are the last beliefs we would normally decide to modify. This is in turn a consequence of the fact that any such modification produces a chain reaction that can affect large portions of our web of belief.

[71] It should be stressed that by considering logical rules just as mere hypotheses assumed in the course of a derivation Quine runs into Lewis Carol's problem.

is a tension between semantic and epistemic holism. According to the latter it is possible to preserve a claim by revising other statements that jointly with it imply observational consequences that fail to be true; while according to the former such a move changes the very meaning of the claim we intend to save from refutation. The stratagem of changing the meaning of a word is then an extreme consequence of the interplay between semantic and epistemic holism.

If the constraints imposed by experience on our choice are so weak, what are the criteria that can guide us? Why should we decide to sacrifice a belief in order to save another belief? Quine's answer lies in his empiricism:

> As an empiricist I continue to think of the conceptual scheme of science as a tool, ultimately, for predicting future experience in the light of past experience.[73]

Our choice of hypotheses will be aimed at improving science as an instrument of prediction. At the end of the next section I will briefly outline Quine's view on this issue.

2.2 A comparison between the Duhem Thesis and the Quine Thesis

Many commentators have compared the Duhemian and Quinean brands of holism on various grounds. I will here consider three aspects: 1) scope, 2) critical semantic mass, 3) methodology.

Scope: the most striking contrast between the Duhem and the Quine Theses lies in the different extension of the part of knowledge that they concern. This has been noted by most critics, but without paying too much attention to the reasons for it.[74] According to Duhem holism arises essentially in physics and up to a certain extent in sciences that have a similar logical structure, such as chemistry. There is no holism in sciences such

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[72] WB, chapter I.
[73] FLPV, p. 44.
[74] See, for instance, Krips, 1982; Ariew, 1984; Gochet, 1986, p. 29; Gillies, 1993.

as anatomy of zoology. Although Duhem is not very explicit on this point, we could say that in his view holism manifests itself only in sciences that are based on mathematics and geometry and hence require acts of measurement for welding their language with the descriptions of ordinary facts. My reconstruction of the motivation for the Duhem Thesis is consistent with this interpretation. Holism is for Duhem a consequence of the structurally different status of physical knowledge with respect to common-sense one. The latter is exempted from holism because the typical judgement on which it is based provides a direct mirroring of the relations existing among individual instantiations of universals (we have heard this thunder after seeing the lightning; this man is mortal): experiments are theory-laden, observations are not.

Quine's picture is very different: holism becomes a *general* theory of meaning. No sentence belonging to a language has meaning taken in isolation. There are no atomic expressions that correspond to given states of the affairs in the world; the very notion of state of affairs, or proposition that a sentence in isolation should mirror is rejected.[75] Even observation reports do not have a meaning by themselves in the intuitive sense of the term *meaning*.[76] Common-sense knowledge is not exempt from quinean holism, nor are those sciences, such as zoology or botany that are (or we should say *were* at the time of Duhem) mainly based on common-sense observations. Indeed the crucial divergence between Duhem and Quine lies in their opposite views concerning the relationship between science and common-sense knowledge: Duhem would firmly reject Quine's gradualism on this issue:

Science is a continuation of common sense[77]

We are working up our science from infancy onward.[78]

[75] "This conclusion, ..., seals the fate of any general notion of propositional meaning or, for that matter, state of affairs." OR, p.81. This does not mean that the notion of state of affairs by itself is rejected by Quine, who defends a radical version of physicalism (See Hookway, 1988, pp. 61-78, 212-216 and Gibson, 1982, pp. 161-166). The only real facts are according to Quine physical facts, but those facts cannot be put in correspondence to single statements belonging to the language of physics. My sentence "there are free electrons in a metal" does not have as *meaning* the fact that there are free electrons in a metal or a kind of fregean sense expressed by it. Simply it has no meaning by itself. When considered as a part of our system of beliefs it acquires cognitive significance insofar this system allows us to predict future experience in the light of past experience. To Quine's holism there corresponds a disquotational theory of truth, according to which saying "there are free electrons in a metal is true" is really nothing more than saying that there are free electrons in a metal (see WO, section 6).
[76] See below for further clarifications on this point.

Contra Duhem, holism extends for Quine also to logic, mathematics and geometry. According to Duhem we have a direct, intuitive access to the truths of these disciplines. Quine, as we have seen, holds instead the view that they just provide part of the bundle of hypotheses that we need in order to predict anything about experience: there is no other evidence for their claims beyond the truth of the observable consequences they contribute to entail. Consequently mathematics, geometry and logic are revisable according to Quine, but unrevisable according to Duhem.

Critical Semantic Mass: this problem is related to the previous one. In *Two Dogmas* Quine exposes an extreme holism according to which "the unit of empirical significance is the whole of science". Nearly all critics have rejected this conclusion. As a matter of fact we don't need to involve all our beliefs in the implication of an observable consequence. Conversely the repercussions of a revision at the periphery of our web of belief are generally very limited. Already by the time he wrote his most influential book *Word and Object*, Quine had changed his mind on this point. [79] In 1962 in a letter to Adolf Grünbaum he writes:

> Actually my holism is not as extreme as those brief vague paragraphs at the end of "Two dogmas of empiricism" are bound to sound.[80]

In *Two Dogmas in Retrospect* Quine confesses to regret his old "needlessly strong statement of holism".[81] In general there are clusters of sentences that are large enough to imply observable effects or, in Quine's terminology, large enough to have *critical semantic mass*.[82] Therefore the units of empirical significance are smaller than the whole of science. However, this moderate conclusion should not make us forget that logic *is* shared by all parts of our knowledge. Therefore there are tensions that propagate through the whole web of belief. As I have already noticed, according to

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[77] FLPV, p. 45.
[78] RR, p. 138.
[79] WO, sections 1-3 and 7-10.
[80] Published in Harding, 1976, p. 132.
[81] TDR, p. 268.
[82] In *Two Dogmas in Retrospect* Quine is very precise on the issue: "... a cluster of sentences has critical semantic mass if it implies an observation categorical" (TDR, p. 268). I will discuss in detail the concept of observation categorical in a later part of the thesis.

Quine it is because a change in the logical rules has such widespread consequences that we tend to deem the propositions of logic necessarily true.

Many critics have focused on Quine's first extreme formulation of holism and have contrasted it with the view resulting from the duhemian analysis of experimentation. Duhem seems to suggest that a large portion of physical theory is involved in performing an experiment and interpreting its results, not certainly the whole of physical theory. Most of it, along with all the rest of knowledge, remains outside the laboratory. This contrast is attenuated when Quine's moderate holism is considered for comparison. At the end both Duhem and Quine would say that the "number" of hypotheses involved must be *large enough* and will vary from case to case.

Methodology: to focus narrowly on *Two Dogmas* can lead to the error of reading Quine as an extreme relativist who suggests to replace scientific methodology with an almost complete freedom of theorising. Donald Gillies, for instance, contrasts Duhem with Quine on the ground that the former offers a way out of the holistic impasse of theory choice with the already mentioned conception of scientific good sense, while the latter does not go beyond the logical point that experiment cannot compel us to accept or reject a hypothesis.[83] However in a later work[84] Quine does develop a sketchy theory of scientific methodology based on *virtues* "which count toward the plausibility of a hypothesis".[85] The five virtues are 1) conservatism, 2) generality, 3) simplicity, 4) refutability, 5) modesty. He then adds a sixth one, precision.[86] In Gibson's[87] book on Quine and in an article by Krips[88] Quine's criteria for theory choice are discussed in some detail. I will here comment only on the importance of the first one, conservatism. In the revision of our beliefs motivated by recalcitrant (or simply by new) experience we have, according to Quine, to follow a maxim of *minimal mutilation*. This means that scientists should try to limit the damages to their pre-existing system of beliefs, especially when these are deemed very plausible.

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[83] Gillies, 1993, pp. 98-116.
[84] WB, chapter V and VII.
[85] Ibid. p. 42.
[86] Ibid. p. 65.
[87] Gibson, 1982, pp. 169-173.
[88] Krips, 1982. For a succinct appraisal of Quine's philosophy of science see also Smart's article "Quine's Philosophy of Science" in Davidson and Hintikka, 1969.

> The plausibility of a hypothesis varies inversely with the plausibility of the prior beliefs that it disallows.[89]

I believe that the role of conservatism in Quine's account of scientific method must be stressed, because it helps to defuse the seemingly paradoxical claims that are made in *The Two Dogmas*. According to Quine it would be anti-scientific or even irrational to conduct scientific research with the constant appeal to revolutionary and unheard-of theories in order to accommodate a new phenomenon or to save from refutation a cherished hypothesis.

In conclusion Quine makes at least an attempt to make explicit and as objective as possible the criteria for theory choice, contrary to Duhem, who limits himself to a declaration of faith in the wisdom and intuition of researchers.[90] Therefore, on this issue, Gillies' picture is reversed.[91]

Let us now go back to the central theme of this thesis: the empirical basis of science.

[89] WB, p. 44.
[90] See Krips, 1982, pp. 258-263.
[91] Quine's attempt does not prevent him from thinking that some freedom is present in theory choice. He does not hope to find a set of rules that *dictate* the choice to be made in any circumstances. Whence his talk of *virtues*, not of *rules*.

3. QUINE ON OBSERVATION

Introduction

In this chapter I will expose Quine's views concerning observation. First of all I stress the vital need in his philosophy for a clarification of this concept. Quine's solution comes through the naturalisation of the theory of knowledge. A naturalistic explicans for the intuitive notion of meaning of an observation sentence is provided by the concept of stimulus meaning. In the last section I give an account of how, in Quine's view, his notion of observation sentence solves the problems related to the theory-ladenness of *observation*.

Although I suspect that Quine's account of observation could be criticised in many respects, in this thesis I will not question it as far as common-sense knowledge is concerned.

3.1 The tension between Holism and Empiricism

As we have seen Quine's holism is not restricted to a part of our knowledge, namely physics, but it concerns the whole of our talk about the world. Such an extreme thesis demands that a clarification between holism, theory-ladenness and underdetermination be given. These three concepts are often vaguely associated when the credentials of observation and experiment as sources of reliable knowledge are put into question. However it will become clear from the following considerations that the possibility of their pacific coexistence is far from being obvious.[92]

[92] This difficulty is sensed also by Michael Dickson: "If observation is 'theory-laden', how can there be 'observationally equivalent theories'?" (Dickson, 1999, p. S47).

There is certainly a line of reasoning that ties together a form of empiricism, and the doctrine of underdetermination. If we can identify a fundamental level of experience, that is if we can make sense of the notion of the given, then a first step is taken toward the idea that science is underdetermined by empirical evidence. A purely empirical level of description could be seen as a screen, which separates the domain of subjectivity from the external world. The latter would become an object of cognition only insofar it could manifest itself, directly or indirectly at the level of phenomena. The objects of the external world would project, so to speak, their shadows on the screen, and we would be left with nothing else, apart from those shadows, to understand the inner structure of reality. The metaphor is well suited to our purpose precisely because an infinite number of radically different objects can project exactly the same shadow. Any theory of the world that would enable us to predict the behaviour of the shadowy appearances could be seen as good as any other such theory. After all, one may try to argue, we could in principle know everything about the shadows and be dispensed with theory. We could, again in principle, accept the empirical content common to all empirically equivalent theories as the only real knowledge we can attain, and treat the theories as mere instruments of predictions.

This argument is one of the many that underlie Berkeley's denial of the existence of matter and his instrumentalist interpretation of Newton's physics.[93] The metaphor can be used to illustrate also the more recent empiricist position resulting from having left behind the first and second of what Quine calls the five milestones of empiricism,[94] that is after the shift from ideas to word and, in turn, from words to sentences. Instead of the shadows themselves we have now a purely observational language (such as a sense-datum language) in which it is possible to formulate the theory-free empirical content of scientific theories.

This last remark suggests that a traditional (although updated to the linguistic turn) empiricist account of scientific knowledge needs to overcome two difficulties in order to give a clear meaning to the doctrine of radical underdetermination (which in turn lends support to an instrumentalist conception of science): 1) to individuate a part of language which can be called observational; 2) to develop a notion of empirical content of scientific theory which can be tested directly against reports in observation

[93] See Berkeley's *De Motu* in Berkeley, 1980, and Popper, 1963, pp. 166-174.

language, that is to clarify the relation between theory and observation. I will in the next two sections expose and analyse the way in which Quine meets the first challenge. Quine's attempt to solve the second problem will be dealt with in the next chapter.

The quinean picture is from the outset very problematic, in that it allows the scientist to reject also the observational reports if he believes it needed. In the face of recalcitrant experience we can sometimes reject a statement close to the periphery in order to avoid a disruptive effect on the rest of our web of belief. This consequence of Quine's holism has prompted a severe criticism from Dummett, who sees no way to reconcile it with an empiricist view of knowledge. Moreover, argues Dummett, it is no longer clear in what sense we can speak of the periphery and the centre of the web of belief:

> If alternative revisions are always possible, and, in particular, ones which leave the periphery intact, there is no content to saying that the total theory makes contact with experience only at the periphery. Rather, the total theory confronts experience as a whole: as a whole, revision is or is not required in it by the occurrence of an experience; but there is not any one point or region in the total theory where the impact is made.[95]

To hold that an alternative revision that leaves the periphery intact is in principle always possible, means to say that even statements at the periphery do not have meaning in isolation, they are theory-laden in the sense that their acceptance or their rebuttal is dependent upon our overall theory of the world. In what sense then, asks Dummett, can we save empiricism and maintain that there is a periphery on which experience directly impinges? To go back to our metaphor there seem to be no screen and no shadows for nothing is really given in any sense. In some sense everything becomes theory.

In order to introduce Quine's solution to these problems, let us start quoting a crucial passage from the article *On Empirically Equivalent Systems of the World*:

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[94] TT, chapter 7.

> Now the Duhem thesis still holds in a somewhat literalistic way, even for these observation statements. For the scientist does occasionally revoke even an observation statement, when it conflicts with a well attested body of theories and when he has tried in vain to reproduce the experiment. But the Duhem thesis would be wrong if understood as imposing an equal status on all the statements in a scientific theory and thus denying the strong presumption in favor of the observation statements. *It is this bias that makes science empirical.*[96]

There is therefore a methodological criterion that dictates as part of scientific rationality the bias in favour of observation sentences. However the preceding considerations suggest that what is needed is, first of all, a clear account of what observation statements are. How can they be demarcated from theoretical statements? To begin to answer to this question is necessary to recall the naturalistic behaviouristic conception of language and knowledge that informs all of Quine's thought.[97]

3.2 Quine on observation sentences

In the framework of Quine's naturalistic-behaviouristic theory of knowledge the notion of experience as something given to the subject, something that we can describe in the first person, is rejected from the outset. Quine's surrogate for experience (I owe this expression to Hookway) is provided by the triggering of receptors at our nerves' endings. From the naturalistic point of view empiricism is not a thesis which can be established prior to science by means of a kind of first philosophy, it is a scientific hypothesis concerning how humans actually acquire information about their environment. And it is precisely science that tells us that the only way we get in touch with the world is via the stimulation of our nerves' endings. Empiricism is thus formulated as a thesis concerning man as a part of the objective reality which science investigates. Coherently enough Quine admits the possibility for future scientific

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[95] Dummett, 1973, p. 593. Quoted in Gochet, 1986, p. 31.
[96] EESW, p. 314 (my emphasis).

investigation to refute his empiricism, for it might be the case that men are found to be endowed with some kind of extra-sensorial perception. Paradoxical though it may seem, Quine's empiricism, being an empirical thesis, could be in principle refuted by experience. However, as we shall see in the next chapter, there are other, I would say more serious problems that beset Quine's empiricism from within his philosophy.

Once we accept to ask science for an account of what science itself is, we can, according to Quine, say that

> ... science is a conceptual bridge of our own making, linking sensory stimulation to sensory stimulation; there is no extrasensory perception.[98]

This insight is also the starting point for any acceptable theory of meaning,[99] a theory that must be based upon a coherent empirical semantics. We must investigate language analysing the behaviour of speakers under the presence of verbal and non-verbal stimulation.

For this purpose Quine divides all sentences into two main categories: *standing sentences* and *occasion sentences*. Occasion sentences are sentences true in some occasions and false in others that prompt the assent of a competent speaker in the presence of the appropriate range of stimulation (for example, "It's raining", "There is a rabbit", "This is blue", but also "This is a bachelor"). Standing sentences are sentences that can be assented to also in the absence of the appropriate kind of stimulation that motivated the original assent (for example: "the Times has come"). Among them there is the important subclass of the *eternal sentences*, which are true or false for all members of a speech community at any time, independently of the stimulatory conditions (obvious example "2 + 2 = 4", but also "It rains in London on the 10th of October 2000"). Among the occasion sentences we must further distinguish between two subgroups: the set of *observation sentences* and its complement, that we might call the set of *non-observational occasion sentences*.

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[97] The logically fundamental role of the naturalistic-behaviouristic conception of knowledge and language in Quine's philosophy is stressed by R. F. Gibson. See Gibson, 1982, pp. XIX, 62, 63-65, 173.
[98] TT, p. 2.
[99] Quine believes that as to the theory of linguistic meaning "one has no choice but to be an empiricist", OR, p. 81.

Let us now focus on observation sentences. For a given speaker there is a difference in degree in the amount of collateral information that is needed beyond the presence of the appropriate stimulation to assent to a certain statement. In order to say "this man is a bachelor", I must have gathered some information about his past. Indeed the range of stimulation associated with the presence of that man in my surroundings is by no means sufficient to prompt the assent to the sentence in question. Another competent speaker may lack of the relevant information necessary for it. As a matter of fact some stored information will be required for assenting to any sentence, for the speaker will have to able at least to remember the words that compose them. Consequently Quine defines the observation sentences, as those occasion sentences that can be assented to without any collateral information beyond what is required to the recognition of the linguistic expression itself.[100] Hence an observation sentence will be assented to by any member of a given speech community in the relevant stimulatory situations. Moreover, the occasion of utterance must be intersubjective: "I am in pain" cannot count as an observation sentence, for, as it fails to be related to a range of external stimulations, the occasion that prompts assent to it cannot be public. In short an observation sentence is defined as

> an occasion sentence whose occasion is not only intersubjectively observable but is generally adequate, moreover, to elicit assent to the sentence from any present witness conversant with the language.[101]

Observation sentences are according to Quine the repository of empirical knowledge and play a key role in the construction of the edifice of science. In defining the notion of observation sentence I have earlier referred to their relation with the triggering of receptors. How is this relation to be understood more precisely? Quine introduces the notion of *stimulus meaning*:

> The range of stimulations associated with an observation sentence, affirmatively or negatively, I call its affirmative or negative stimulus meaning for the given speaker.[102]

[100] OR, p. 86.

This definition links any observation sentence to a class of physical events occurring in the external world. Indeed if the intuitive idea of observation sentence was that of a sentence closely related to perception, we can appreciate how Quine's definition gives a suitable explicans of it in naturalistic, behaviouristic terms: as a matter of fact some bits of our language are closely related to the triggering of our receptors.

> The problem of relating theory to sensory stimulation may now be put less forbiddingly as that of relating theory formulations to observation sentences.[103]

How can we in practice identify the class of observation sentences? Quine gives two answers to this question. In vague terms the identification can be made on behavioural ground, by studying the linguistic reactions of a given speech community under appropriate stimulations. The reference to the whole community is here crucial, for it is well known that members of some groups of specialists share such a rich storage of background knowledge, that they can assent, under the appropriate conditions, to many occasion sentences most of speakers would not even be able to understand. What would be classified as observational for a group of trained experimental physicists would be considered by most of people as highly theoretical: Duhem already pointed it out very clearly. The behavioural criterion prescribes that the matter be settled by taking as paradigmatic the linguistic responses of the untrained observer for

> Specialists rest content with the level of evidence that commands their expert agreement, but in principle they usually could reduce this recondite evidence to observation terms at the layman's level.[104]

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[101] NNK, p. 73.
[102] PT p. 3. For a more detailed definition of stimulus meaning, involving also the duration of the stimulation, see WO, section 8.
[103] TT, p. 25.
[104] EESW p. 316-317.

Let us note how much here Quine is echoing Duhem's reference to the layman's language. The behavioural criterion for identifying the observation sentences has however two major shortcomings.

1) We would like to rank as observational those statements that belong to speakers trained to recognise particular smells, tastes and sounds, such as chemists, wine tasters and musicians, and that are certainly not translatable into common-sense language.

2) In general even the simplest observation terms have a certain kind of vagueness that can make agreement between speakers difficult and inconstant.

In order to overcome these weaknesses of the behavioural criterion Quine proposes to individuate the observation sentences as those that *can* be learned ostensively[105] (there is no necessity that each single speaker has actually learned them all by ostention).[106]

3.3 Observation sentences and Holism

The notion of observation sentence has been now clarified enough to see how it is used by Quine in order to provide an account of the empirical basis of science. Thanks to it we can avoid direct reference to the triggering of receptors and talk instead of the relation between theory and observation sentences. Holism can be phrased as the thesis that a statement taken in isolation has no separate fund of confirmatory of disconfirmatory experiences. Quine's empirical semantics provides a surrogate for such fund of experience with the notion of stimulus meaning *for the observation sentences*. As I have said, what is wanting for an explanation is how Quine can consistently maintain his version of holism and commit himself to empiricism. The first step toward this explanation has been taken. Yet many problems still have to be solved. Have we found a bedrock of sentences that can be equated with Duhem's brute facts? Certainly not. Although Quine has defined in a clear way the observation sentences, and has somehow managed to give them a special status in virtue of the notion of

[105] EESW p. 316.

stimulus meaning, the problem of their theory-ladennes seems still to be tackled. How can we make intelligible the bias in favour of observation sentences that renders science empirical? Let us see Quine's solution to the problem.

> My definition distinguishes observation sentences from others, whether relative to a special community or to the general one, without reference to theory-freedom. There is a sense, as we shall now see, in which they are all theory-laden, even the most primitive ones, and there is a sense in which none are, even the most professional ones.[107]

In order to clarify this passage it is necessary to briefly recall Quine's theory of language acquisition. The first expressions that a child learns are simple words such as "mamma" or "water". They are learned ostensively and through normal mechanisms of imitation and reinforcement. These words, along with simple sequences of words such as "dad is here" are learnt as undivided wholes or, in technical terms *holophrastically*. The child does not strictly speaking learn any word; he learns only whole sentences by becoming accustomed to the stimulatory condition under which they should be uttered. "Water", at this stage, is a sentence just as much as "dad is here". The ostensive learning leads inevitably to a kind of conditioning of the linguistic behaviour, for which the sentence is simply associated to a range of stimulations. Only much later will the child acquire the ability to master the language in its full complexity. Only at that time will the child be able to analyse word by word the sentences that he has learned holophrastically.

It is clear that the first sentences learned are in general observation sentences, while the theoretical sentences, loosely related to the sensory stimulations, are mastered at a later stage. The crucial point is that *observation sentences, seen holophrastically, are theory-free*, as they are assented to or denied, somehow mechanically in the presence of the relevant conditions. We don't need to think or to recall any kind of auxiliary information in order to say "it is raining" while walking out of the door. We utter that sentence because we have been conditioned to associate it to a certain pattern of stimulation. However the words that go into the observation

[106] Further qualifications on the concept of observation sentence are added by Quine in TI, pp. 1-4.

sentences appear also in the theoretical ones. It is this sharing of words that makes possible the logical connection between observation and theoretical sentences, and these logical connections are responsible for the holistic character of language and knowledge. Their effect is twofold: on one side they make observation relevant for theoretical language, (speaking with licence, they let empirical meaning edge itself through observation into theory); on the other side they inevitably load observation sentences with a theoretical burden. To illustrate this point let us consider the simple statement "this is water". The term "water" appearing in it is also to be found in a statement such as "water is H_2O". Therefore whereas the holophrastic expression "water" was simply a non-committal linguistic reaction to certain stimulations, the same expression seen word by word becomes extremely theory-laden. For the speaker by saying "this is water" can be led to saying "this is H_2O", which is an expression that underlies a great deal of physical and chemical theory and that is connected to a virtually infinite set of possible confirming or disconfirming experiences (imagine all possible tests aimed at finding out whether a liquid is water). I would say that by "becoming" theory-laden the statement has lost its direct connection to the original stimulus meaning, it falls prey to the lack of a separate fund of relating experiences that affect almost all sentences.[108] I believe that Quine is hinting at the possibility that the speaker might revoke his early observation sentence as a consequence of careful chemical analyses, hence as a consequence of stimulations that have nothing to do with the stimulus meaning of the sentence "this is water".

The conclusion is that given an observation sentence

> Seen holophrastically, as conditioned to stimulatory situations, the sentence is theory-free; seen analytically, word by word, it is theory-laden.[109]

Note that this is true for any object whatsoever to which a term in an observation sentence may refer. The example just mentioned involved a term like water, that is in

[107] PT, p. 7.
[108] Here we can appreciate that for Quine theory and language are inseparably intertwined.
[109] PT p. 7. Gochet refers to this solution as following from his own reading of Quine (Gochet, 1986, p. 33). However already by the time Gochet published his book, Quine had been very clear on this issue (see for instance TT p. 30).

clear relationship with scientific theory; but the same considerations would apply to terms such as "table" or "hand". Any *posit* is by itself theoretical.[110] Ontology does not appear at all at the level of holophrastic expressions. Quine makes clear in several writings that the full shift from holophrastic expressions to the appreciation of their term by term structure requires a complex apparatus of prediction that leads to objective reference through quantification.[111] Any talk about objects requires the full development of the theoretical part of language. The holophrastic expression "there is water" does not by itself commit the speaker uttering it to any particular kind of ontology.

Quine individuates three advantages of his definition of observation sentence:

1) as their link with theory is given simply by the sharing of vocabulary, there is no need to postulate bridge principles à la Reichenbach in order to connect observations with theory.

2) The definition itself of observation sentence has nothing to do with its being theory-laden or theory-free. Hence most of the debate sprung among neopositivists on the so-called "protocol sentences" and their relation with experience and theory[112] is side-stepped.

3) We can investigate the acquisition and the use of observation sentences "without prejudging what objects, if any, the component words are meant to refer to"[113] whereas "Taking terms as starting point would have meant finessing reification and conceding objective reference out of hand, without considering what it is for and what goes into it"[114]

[110] It should be recalled that according to Quine "to call a posit a posit is not to patronise it" (WO, p. 22) Any entity is a posit from the point of view of naturalistic epistemology, that studies how we develop our theoretical talk as a response to the sensory input. From the point of view of the related theory a posit is *real*.
[111] See, for instance, OR, pp. 1-25 and WO, chapter 3.
[112] For an account of the debate on protocol sentences between Schlick, Carnap and Neurath, see Hanfling, 1981. Some of the original articles can be found in Ayer, 1959.
[113] PT, p. 8.
[114] PT, p. 8-9.

In conclusion, Quine develops a notion of observation sentence aimed at dissolving the tension between his commitment to both empiricism and all-embracing (although moderate) holism *as far as the concept of observation is concerned.* We can now take a step forward and see how he tries to give, within his theoretical framework, an account of the empirical basis of physical theories, of the concept of empirical content and of the thesis of underdetermination of theories by empirical evidence. We will see that it is precisely in relation with these issues that Quine's empiricism shows its weaknesses.

4. QUINE ON THE RELATION BEWEEN OBSERVATION AND SCIENTIFIC THEORY: EXPOSITION AND CRITICISM

Introduction

The first four sections of this chapter deal mainly with the concept of empirical content of theories. I will first of all comment on the importance of this concept especially in relation with the problem of underdetermination. The difference between Quine's and van Fraassen's treatments of the latter will be underlined.

Quine has developed two different accounts of the concept of empirical content. He has first tried to specify it by means of the notion of observation conditional. This attempt is dealt with in sections 4.1 and 4.1.1. The intuitive idea behind it is roughly that it would be possible in principle to describe all the observable events of the history of the universe and *then* ask scientists to formulate theories that encompass such description in a finite formulation. Problems related mainly to the unavoidably theoretical character of the specification of space and time led Quine to a different strategy for defining the empirical content of theories, which is based on the concept of observation categorical. In section 4.2.1 I will criticise this strategy.

In general Quine's view concerning the role of observation in experiments and its relation with theory will be the object of many objections. I will argue that it is impossible to individuate a purely empirical content of a theory. In the last two sections I will deal with two criticisms that may be brought against the point of view defended in this chapter.

4.1 Quine on empirical content, empirical equivalence and underdetermination

The first time Quine deals systematically with the issue of underdetermination is in the famous article *On Empirically Equivalent Systems of the World*, in which many important claims concerning empirical content and empirical equivalence are stated very clearly.

After distinguishing holism from the thesis of underdetermination, Quine maintains that the former lends credence to the latter. We have already underlined this fact. However Quine is aware that holism by itself does not logically guarantee that the thesis of underdetermination is correct. Furthermore the latter is simply not very clear. The main purpose of the article lies in the attempt to define precisely the notion of underdetermination and then to assess its plausibility.

It is time now to stress the difference between two notions of underdetermination, *local* and *global*.[115] When we say that two theories are underdetermined by empirical evidence, do we mean that the evidence so far available is not enough for settling which of the two theory is true (local underdetermination) or do we mean that the indecision would survive all possible evidence that we could even in principle gather (global underdetermination)? It is clear that two theories can be locally underdetermined even if they are not globally underdetermined. Perhaps the presence of new auxiliary assumptions may extend the range of the observable consequences of the two theories and may lead to an experiment that settles which one of the two, if any, is really empirically adequate. This and other similar arguments have motivated Laudan's and Leplin's[116] claim that underdetermination, being relative to a particular state of science, is never absolute and always refutable. However, as is stressed by Hoefer and Rosenberg, their arguments would not apply if a global theory or system of the world were at hand. In that case the presence of a rival underdetermined theory could not be dealt with by looking for new possible crucial experiments. Therefore, they conclude, if we want to take seriously the issue of underdetermination, we will have to discuss it as applied to global rival theories, for or against which no further decisive experience can be conceived.

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[115] See, for similar considerations, Hoefer and Rosenberg, 1994, pp. 592-593.
[116] Laudan and Leplin, 1991.

I believe that a similar train of thought must have motivated Quine's choice in his article to treat the problem of underdetermination as a problem concerning alternative systems of the world. In this way we separate the practical issue of having to look for new experimental results in order to opt for one of many rival theories, from the logico-epistemological issue of whether observations are by themselves enough to fix the content of our theories. In practice, as Quine says,[117] there is a trivial sense in which all theories are underdetermined by past observations, for future ones may always surprise us somehow, and even by all past and future observations that have been or will be carried out, for many other observable events may simply go unnoticed.

Let us now get back to the problem of making sense of underdetermination. First of all we need to find a way to relate theories to empirical evidence. Intuitively we might think that a theory is empirically adequate if it is confirmed by all true observation sentences. However there is no way to make a theory imply an observation sentence, for a theory can be seen as a conjunction of standing (even eternal) sentences, while an observation sentence is an occasion sentence, true in some cases, false in others. Quine attempts to solve this problem by introducing the notion of *pegged observation sentence*. If we choose an arbitrary system of co-ordinates we can then associate each observation sentence expressible in our language to each combination of spatio-temporal co-ordinates. In this way we obtain a set of standing sentences, some true and some false; these are the pegged observation sentences.[118] The subset of the true ones can be considered as the description of all observable events of the history of the universe, whether observed by man or not. In *The Nature of Natural Knowledge* Quine adopts, in order to describe the situation, the vivid image of an observational oracle

> ... capable of assigning a truth value to every standing observational report expressible in our language.[119]

[117] RIT, p. 178.
[118] I would say that some combinations of co-ordinate would correspond to places at which it is physically impossible to use our sense-organs (such as the centre of the sun!). This is not the main problem however.
[119] NNK p. 79.

According to Quine these pegged observation sentences can be deduced by a theory in conjunction with other pegged observation sentences that describe the initial conditions. The resulting sentence is called by Quine an *observation conditional.* A theory implies observation conditionals. An empirically adequate global theory is one that implies all true observation conditionals. In general "The class of observation conditionals implied by a theory formulation is said to comprise *the empirical content of a theory*"[120]. Consequently two theories are empirically equivalent when they imply the same observation conditionals.

It is, I believe, very important to explain why what we are after is specifying the empirical content of scientific theories. As I have already pointed out this notion is needed in order to identify the common observational ground that many underdermined theories may share. The importance of this analysis can be highlighted by contrasting it with the way underdetermination is usually discussed.

A standard example of underdetermination has been put forward by van Fraassen in his book *The Scientific Image.*[121] Let NT stand for Newtonian theory (i.e. Newton's mechanics plus the theory of gravitation) and R be the further hypothesis that the centre of gravity of the Solar System is at rest with respect to absolute space. Let instead be V the hypothesis that the centre of gravity of the Solar System has absolute constant velocity *v*. We can now formulate two empirically equivalent theories by considering the conjunctions NT & R and NT & V. Indeed by changing the value of *v* we can formulate an infinity of empirically equivalent theories i.e. an infinity of theories that are underdetermined by empirical evidence and yet logically incompatible, for they make contradictory claims on the absolute velocity of the centre of gravity of the Solar System.

What I want to stress is that this example, while suggesting the plausibility in principle of the thesis of underdetermination, is of very little help in understanding its real scope. After all the theories just considered differ very little in what they say about the world. If we were indecisive about which one of them we should adopt, our overall set of beliefs about the universe would be fairly stabilised. There would be little room left for doubt and indecision. As a matter of fact, those variants of the Newtonian

[120] Gibson, 1982, p. 86.
[121] Van Fraassen. 1980, chapter three. See also van Fraassen, 1976.

theory share one and the same ontology, for the all posit masses, forces, energy, momentum and even absolute space and time. Moreover they rely on the same laws and principles and, in virtue of their unanimous acceptance of concepts such as inertial frame, inertial forces etc..., they dictate the same strategies for carrying out measurements about the observable motions. All of them would teach us, for instance, that in order to improve our determination of the orbit of a planet we have to take into account several corrections due to to the complex motion of the Earth. Even the most theory-laden experimental results would be treated by those theories in the same way. In conclusion, we can say that in this example we deal with theories that, so to speak, branch away from the same trunk only at the top of the trunk. It would be clearly inappropriate to say that what they share is something we can interpret as a *purely* empirical content; for they share complicated and theory-dependent claims that are by no means uncontroversial. Duhem's analysis of the theory dependence of the use of symbols and approximations should suffice to convince of this (see section 1.2). Van Fraassen does speak about the empirical content of theories, or about the appearances that theory must save, but he is explicit in considering them as completely theory-laden. When he upholds the motto "to save the phenomena" he must be subscribing to a view similar to the one I exposed in section 1.6.1.[122]

However if we want to understand what the real scope and significance of underdetermination can be, we must investigate whether different theories are possible that differ much more radically. Maybe it is possible to eschew reference to forces and masses, maybe it is possible to develop a complicated variant of a geocentric system of the world, maybe there are other expressions of the law of gravity that could work as well, provided that we modify in some way the principles of dynamics themselves; or, again, we may try to reform the theories that allow us to measure the movement of celestial bodies, such as optics. In short, the question is: can the branching occur at a more fundamental level of description? What would be, in this case, the evidential

[122] See van Fraassen, 1980, pp. 44-47. The fact that according to van Fraassen it is necessary to distinguish between observational and non-observational parts of a theory does not conflict with this interpretation of his notion of empirical content. According to van Fraassen the belief in the existence of, say, electrons and protons should be sharply distinguished from the belief in the existence of observable objects such as planets and stars. Nevertheless what he calls the empirical content of a theory does not simply amount to a set of claims that can be tested through *unaided vision* as it results clear from his treatment of Newton's theory.

ground that the various imcompatible theories would share? And most of all, is there a really fundamental level of descriptions at which the possibility in principle of the branching first arises?

The definition of empirical content just illustrated is designed to solve these problems. Quine, just like Duhem and van Fraassen, believes that the aim of science is to save the phenomena,[123] but according to him the phenomena consist simply in the stimulations of our nerves' endings. Therefore it is at a level as close as possible to the sensory stimulations that he tries to locate the purely empirical content of scientific theories. However, as I will argue, there are many serious objections to the meaningfulness of Quine's solution.

Having defined the concept of empirical content, Quine goes on to deal with the problem of underdetermination. I will skip the details of this discussion,[124] in order to focus on what is relevant for clarifying the relationship between theory and observation. Quine summarises his discussion in this way

> ... the thesis of under-determination would assert merely that our system of the world is bound to have empirically equivalent alternatives which, if we were to discover them, we would see no way of reconciling by reconstrual of predicates. This vague and modest thesis I do believe.[125]

The consequences of this thesis are judged by Quine "vitally important to one's attitude toward science"[126]. What stance would we take toward our knowledge, if we were confronted with two logically incompatible systems of the world, that resist reduction to one another by reconstrual of predicates? Quine himself has, in various writings, oscillated between two opposite views on the matter, that he has subsequentely named the *sectarian view* and the *ecumenical view*. According to the sectarian view we are committed to believe true our system of the world and false the other. It would be impossible to step outside of the arena and contemplate both

[123] The *Pursuit of Truth* begins with Plato's famous dictum σώζειν τα φαινόμενα.
[124] A synthetic exposition of the article EESW can be found in Gibson, 1982, pp. 84-90.
[125] EESW, p. 327.
[126] Ibid.

systems.[127] Viceversa, according to the ecumenical view, we must give up our "illusion of there being only a solution to the riddle of the universe"[128], and accept the possibility of having to describe the world alternatively within one system or the other, "using distinctive signs to indicate which game we are playing"[129].

The dilemma between these two views has prompted a huge debate over the internal consistency of Quine's philosophy. The coherence of its simultaneous commitment to empiricim and naturalism has been called into question.[130] As will be clear in the remaining part of the thesis I indend to concentrate on the presuppositions shared by those who take part in the debate, regardless of the side they take.

Let us first of all appreciate how much Quine's treatment of underdetermination differs from van Fraassen's. In this case what is considered is the alternative between two incompatible systems of the world that share solely a common fund of observation conditionals. The effort is to contemplate the possibility of rival systems that differ wildly as to any theoretical aspect, that is as to anything that goes beyond the scope of observation sentences *intended holophrastically.*[131] I use the word "effort", for Quine intends to describe this possibility by means of the notion of empirical content, and I will argue below that this cannot be done. However, compared to van Fraassen's treatment, underdetermination has already now assumed a much more dramatic aspect. And, not surprisingly, the radicality of Quine's version of underdetermination is accompanied by a no less radical empiricist faith. Indeed in many passages of EESW, Quine shows a strong tendency toward the idea that theory is only practically necessary, for science, but in principle superfluous. It seems really to find in Quine's thought an echo of the old-fashioned empiricist positions that I mentioned earlier. Again the possibility of radical underdetermination raises the temptation to stick to the empirical content of a theory and jettison all the "trumped-up matter, or stuffing [sic], whose only service is to round out the formulation"[132]. Often Quine's maintains that this elimination of theory is impossible for practical or contingent reasons. A detailed

[127] TT, pp. 21-22.
[128] NNK, p. 81.
[129] EESW, p. 328.
[130] See Gibson, 1986, Genova, 1988, Gibson, 1991. On the tension between realism and Quine's doctrine of underdetermination, see Bergström, 1984, 1990, and 1993.
[131] If two theories are in complete disagreement about ontology, but are supported by the same set of observation sentences, then it follows that these have to be considered as pre-ontological.
[132] EESW, p. 324.

criticism of the notion of empirical content can show that, on the contrary, observation is irrelevant *in science* without theory. A different account of empirical equivalence and underdetermination will be therefore necessary.

4.1.1 Some objections to Quine's first notion of empirical content

The first difficulty is related to the obvious fact that theories cannot imply observation sentences. The solution, as we have seen, is to turn to the pegged observation sentences. However this is no innocent move. The process of establishing a reference frame is extremely theory-dependent. In practice we will need to choose some objects or phenomena and an instant of time as origin of the system of co-ordinates. But in order to do this we need to commit ourselves to a particular ontology. It is clear from the outset that the pegged observation sentences cannot be intended as holophrastic expression, in the first place because they are not occasion sentences. They have a term by term structure at least insofar they make reference to the origin of the system of co-ordinates. This means that their formulation requires that reification and ontology have already slipped into the picture. Different theories implying the same observation conditionals may differ wildly in what they mean by "water", but they must agree on the individuation, that is on the identity of the object taken as origin of the frame of reference.[133] Besides, they must all share some mathematics and geometry and imply techniques aimed at determining space and time intervals that lead to equal results at any level of approximation. For if there were among two theories even a small discrepancy in the determination of such intervals, we could no longer say that they imply the same pegged observation sentences. But the worst is yet to come, for the mere assumption that there is a global co-ordinate system would rule out a class of models of the theory of General Relativity! Problems arise already at the level of Special Relativity according to which there is no matter of fact about which of two events separated by a space-like interval happens first. Suppose that one system of the world is based on a classical notion of space-time and another on the theory of Special Relativity; then a change of co-ordinates to a uniformly shifting inertial system would be completely harmless for the first theory, but would imply a different spatio-temporal ordering of events according to the second.

That Quine sensed at least part of these difficulties can be understood by this remark:

> Our move from occasional observation sentences to these pegged observation sentences is already an abrupt ascent from observation into theory. We need to know not only a bit of mathematics but also quite a lot about the physical world in order to establish a system of co-ordinates. I shall suppose this much achieved, in order to get on better with further questions; but let us not lose sight of the magnitude of our assumptions. The doctrine of under-determination says there is a certain slack between observation and theory; and we have already lost some of that slack by granting the system of co-ordinates. Just in order to define the slack, we are having to take some of it up.[134]

4.2 Quine's second thought on the notion of empirical content

It is Quine's awareness of these problems that explains, in my view, why in later writings his account of the relationship between theory and observation is different. In *Theories and Things* he considers another problem related to the concept of observation conditional.[135] When a scientist tries to verify the truth of an observation conditional, he faces the problem that the antecedent conditions constituting the premise of the conditional may have occurred at a place-time which is "at some remove" from that at which those constituting the consequent of the conditional should take place. The scientist cannot be sure that the facts described in the antecedent really took place, for his senses cannot in that moment attest it to him. He will have to rely on indirect evidence in order to infer that the premise of the conditional has been fulfilled. This evidence will usually comprise memory, written records and the testimony of

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[133] As it is well known, according to Quine the individuation of objects is a major theoretical step that is taken within a language-theory of the world.
[134] EESW, p. 317.

others. Again this may seem an innocent objection. Why is Quine so keen on pointing it out? The reason is that any kind of inference depending on indirect evidence is based on some theory, primitive and unquestioned though it may be. The experimenter must assume that his memory or his assistants are reliable and that the records contain faithful description of the facts. As I have already pointed out, what Quine is trying to identify here is a level of pure observation, which is used to test theories. In order to achieve this he must distil what is nothing but direct experience expressible in observation sentences from the network of theoretical assumptions that surround any utterance. If he allows the act of observation to include the reliance on some theory his aim will not be entirely achieved.

> Clearly, then, our observation conditions were too liberal. We should limit our attention to conditional sentences "if A then B" where A and B stand for eternalized observation sentences referring to one and the same place-time.[136]

In this way Quine believes that the experimenter can see before him all that is needed to test a prediction of a theory. I don't find this solution convincing. An experiment in physics can involve the simultaneous check of such a tremendous amount of experimental conditions, that there is no way for a single person to carry it out. Whether the antecedent and the consequent of a conditional refer to the same or to different place-time the reliance on memory and most of all on testimony is unavoidable. Is it possible to imagine a verification at the CERN of a prediction based on the standard model carried out without the recourse to testimony? I think we should conclude that an observation carried out during an experiment differs from ordinary observation in that the former is always interrelated with testimony and memory in a way that the latter needs not be. We can perhaps be just a bit more daring and suggest that in an experiment (at least in one of some complexity) the concept of observation should be construed as referring to a collective activity. Or else, if we were to stick to

[135] TT, p. 26-27.
[136] TT, p. 27.

Quine's strict concept of observation, we should say that when a theory is tested observation must always be supplemented by testimony.[137]

Let us now get back to Quine's attempt to solve the problem of having to specify a system of co-ordinates for the observational basis of science. As both clauses of observation conditionals are now required to refer to the same place-time, it is possible to drop altogether the mention of space and time and turn to what Quine calls *observation categoricals*. Observation categoricals are eternal sentences formed by the conjunction of observation sentences such as "When there is smoke, there is fire"; "when the sun rises, the birds sing"; "when lightning, thunder". Their common structure is the following: "Whenever this, that". They generalise over places and time without reference to any system of co-ordinates and hence to any ontology of space and time. With this notion Quine believes he has solved the problem of relating theory to observation, for, although the observation categoricals are general statements, he maintains that they can be tested by anyone without further theoretical assumptions. Quine draws a this point the following conclusions:

1) In order to test an observation categorical an experimenter need not rely on memory or in any kind of testimony.

2) Observation is now disentangled from theory, for "Specifications of place-times are still indispensable to science, but we have kicked them upstairs: we have consigned them to the network of theoretical concepts where they belong, at a comfortable remove from observation"[138].

3) In order to test a theory it suffices to derive from it (of course in conjunction with some auxiliary hypotheses) an observation categorical. This being a compound of observation sentences, the experimenter will simply need to be in the stimulatory situation in which he will be ready to assent to the first clause of the observation

[137] The theory-dependence of the reliance on testimony is stressed by Quine and Ullian in The Web of Belief, chapter IV, where the relation between testimony and observation is equated to that between the latter and the use of instruments such as telescopes, radar, and microscopes (whose reliability is based on physical theory). Both testimony and the use of such instruments are called by the authors "in a certain figure of speech" *vicarious observation* (WB, p. 34).
[138] TT, p. 28.

categorical and to affirm or deny the second clause. Thus the observation categorical "When the sun rises, the birds sing" is refuted by observing the sun rising and noticing that the birds are silent.

4) The empirical content of a theory is the set of observation categoricals that it implies (hence we have a new definition of empirical content[139]). Consequently two theories implying the same observation categoricals are said empirically equivalent.

4.2.1 A criticism of Quine's second thought on the notion of empirical content

I have already argued that claim 1) is not convincing. I will now discuss the validity of claims 2), 3), 4). To start with it will be advisable to consider an example taken from science proper, for all instances of observation categoricals thus mentioned have very little to do with the prediction of, say, physical theory (and I cannot quite get myself to accept, as Quine does, that there is no difference in principle between the muttering of a child and a scientific observational report). The only "decent" example of experiment that, at least to my knowledge, Quine has discussed is to be found in the *Pursuit of Truth.*

Let us suppose that a group of mineralogists have identified a new crystalline mineral of pinkish colour and have called it *litholite*. Subsequently a member of the group puts forward a conjecture about the chemical structure of the mineral. This conjecture, together with a well-confirmed body of theories, implies that any specimen of litholite will emit hydrogen sulphide when heated above 180°. Quine concludes:

> These last provisions are the observables; for our mineralogist and his colleagues know litholite when they see it and hydrogen sulfide when they smell it, and they can read a thermometer.
>
> The test of a hypothesis thus hinges on a logical relation of implication. *On one side, the theoretical,* we have the backlog of accepted theory plus the hypothesis. This combination does the

[139] In later writings the notion of observation conditional is abandoned. See in PT the footnote at p. 10, where it is defined "a less fruitful notion".

implying. *On the other side, the observational, we have an implied generality that the experimenter can directly test, directly challenge* - in this case by heating some of the pink stuff and sniffing.[140]

The generality referred to should be, precisely, an observation categorical. But how are we to formulate it? Quine is not explicit on this point. However one might suggest that the observation categorical tested in this experiment is

(1) Whenever* the temperature reaches 180°, litholite smells in such a such way[141]

Now, it is easy to see that (1) cannot be an observation categorical, for its first clause is not at all an observation sentence. We can't utter values of temperature simply in the presence of some stimulatory conditions: sentences about a physical magnitude cannot be learned ostensively. Sentence (1) can be considered as a *theoretical* generalisation, which is corollary of a body of physical theory. It will be derivable only within the language of this body of theory with the further proviso that some *ceteris paribus* clauses must hold. For these reasons I have labelled the "whenever" with a "*", in order to remark that it is to be intended as a quantification over space and time and hence, following Quine's own criterion of ontological commitment, as referring to an ontology of space and time adopted in the framework of a particular group of theories. From now on I will use "whenever" without "*" only when it introduces an observation categorical à la Quine, that is a generality prior to any objective reference.

Having discarded sentence (1) we can now turn to Quine's words "... and they can read a thermometer" for a suggestion to the effect that the observation categorical may be

(2) Whenever a thermometer reads 180°, litholite smells in such a such way

Sentence (2) looks at first sight a bit odd, because, as it stands, it seems to suggest (absurdly) that the position of the thermometer is irrelevant for the truth of the

[140] PT, p. 9-10. My emphasis.

[141] This is one of the cases, already discussed, in which observation sentences used by specialists resist reduction to the layman language. This is not, however, the problem I intend to discuss here.

consequent. This objection is up to a certain extent unfair, for, by definition, observation categoricals[142] refer to pairs of observable events that are present, as Quine says, one the same scene and the same time. Sentence (1) does not say that if a thermometer put in an oven in York reads 180°, then a specimen of litholite in London (or, even worse, any sample of litholite in the universe) will start smelling. A valid objection would rather concern the need to specify precisely the distance between the thermometer and the sample under study. This objection introduces the real criticism of taking (2) to be part of the empirical content of a theory, that is: even granted that the first clause of (2) is built upon on observation sentence, *no theory can by itself imply such an observation categorical*. The observable fact that a mercury column is level with a sign labelled by a number has *by itself* nothing to do with anything physical theory can talk about; it has (again by itself) nothing do to with whatever we mean by "the value of the temperature". In order to connect with the theory the observable event that the experimenter can witness, that is, in order to interpret the observation sentences uttered by the experimenter as a test of a the theory, we need first to check that the thermometer is reliable. The reliability of an instrument amounts in turn to its correct functioning and its correct use according to our present-state physical knowledge. In order to accept the reading as correct, we must first find out (or assume), that each of its components has been calibrated, whether the liquid used is pure mercury and many other things (it seems that we can hear Duhem saying this). For instance we will have to check whether there is a strong magnetic field at the place-time where the experiment will be performed (it is well known that a mercury thermometer is not reliable in the presence of magnetic fields). In order to do this we need both our background knowledge and a host of observation reports. It is only the conjunction of this further auxiliary hypotheses and, crucially, of further observation reports that we can connect theory with observation. Once this preliminary work has been done, the experimenter will subscribe to

(3) When *the* (or this) thermometer reads 180°, litholite smells in such and such way

[142] To be more precise *free* observation categoricals, as opposed to *focal* ones (see, PT, pp. 10-11 and FSS, p. 27).

The problem now is that (3) is not an observation categorical, for it is not a general statement about the conjunction of observable events. Sentence (3) is an occasion sentence that an experimenter accepts once he has persuaded himself that some conditions are verified at the moment and at the time in which the experiment is performed. I have replaced "whenever" by "when" to stress that the confidence of the experimenter is limited to a short period of time. He will subscribe to (3) immediately after having carried out the necessary checks, and probably just before performing the experiment. If the experiment needs to be repeated, the decision on whether (3) is still to be relied upon will be left to the experimenter.

The foregoing analysis shows also that (3) *cannot be said to be part of the empirical content of the theory even if the latter is defined in a way that is different from Quine's*, for it would be senseless to regard as empirical content of an abstract theory something that can derived from it only by means of auxiliary observations and assumptions and that refers to particular objects.

A further attempt to save the generality of the implied sentence is to resort to

(4) Whenever* a *reliable* thermometer reads 180°, then litholite smells in such and such way

or else we could stipulate that the word "thermometer" means "reliable thermometer", but then the observationality of the first clause is lost (as the "*" indicates). Note, further, that the reliability of the thermometer is not sufficient to guarantee that (4) can be implied by the theory. It is conceivable that the temperature is in fact 180° and that still there is no gaseous emission due to the presence of disturbing factors. In case these factors are present, it is also possible, in principle, that the emission does take place and that the temperature is the predicted one, but the cause of the emission is a different one.

The necessary switch from (2) to (3), that is from the indeterminate to the determinate article corresponds to the need for some empirical investigation *in loco*, with the purpose of establishing that for that thermometer, in that place and at that time, the conditions are such that the conjunction of their description with the theory will imply a conditional involving observable events. Consequently this conditional will be labelled with indications of space and time. We will be able to say, staying with Quine's example, that, given that such and such conditions hold here and now, the simultaneous reading of the thermometer and smelly emission will amount to a confirmation of the theory.

I suppose that the example itself is a bit misleading. We are so used to reading thermometers in every day life, that we take those objects as interfaces between theory and observation: we seem to read something that so, to speak, *is* a temperature. It is possible to modify the example in such a way that this illusion vanishes. Imagine that our mineralogists feel such a fierce antipathy toward experimental physics, that they have asked a technician to build thermometers without any display. All they need for their experiments is a thermometer (based on whatever physical principle) closed in a box on top of which a bulb has been situated. The bulb lights when the thermometer reaches 180°. Then the sentences potentially belonging to the empirical content of the theory would be

(5) whenever a bulb lights, litholite smells in such and such way.

(6) when the (or this) bulb lights, litholite smells in such and such way.

I don't think anybody would consider (5) or (6) as part of the empirical content of a theory. It must be emphasised that there is no difference in principle between (5) and (2) on one hand, and (6) and (3) on the other. What is actually seen by the experimenter as the result is the least important thing in the all procedure, for it is, at least to some extent, conventional. We can choose any observable event whatsoever to stand for any value of a physical magnitude; insofar we are sure that the correct causal chain between the observable event and the phenomenon under investigation is actually taking place. Displays, pointers or other similar devices are simply *symbolic interfaces*. There is though an important difference between the latter and ordinary symbols: we are free to use any object as symbol of any other object, but, when we interpret an observable event (what Duhem would call a brute fact) as expressing the result of an experiment, we are *judging* that our theoretical interpretation is correct, and of course, nothing assures that we are not going astray.[143]

What is then, in the light of these considerations, the role of observation sentences in the testing of theories? Let us summarise the essential features of an experiment in physics. We start with a theory, for instance with Maxwell's theory of electromagnetism. From the theory we derive a sentence expressing that whenever X occurs, also Y occurs. For instance we derive

(7) If in a region of space there is an electrical current of intensity I along a line (X), there will be a magnetic field of intensity B with cylindrical symmetry centred on the line (Y)

The sentence (7) is general and cannot be applied to a particular place-time, without the specification of *ceteris paribus* clauses.[144] Once we have taken into account these first, abstract and general *ceteris paribus* clauses we can turn to a sentence like

(8) If in this region now, there is an X, then there is an Y, *ceteris paribus*

Now most of the work is still to be done. We must realise X in a concrete system and verify that we have done it properly with the aid of measurement devices. At this stage the observation sentences come into play: we must describe the apparatus (in this case an electrical circuit, a generator and so on) and our instruments (a galvanometer and a magnetometer). As I have already said the reliability of these instruments requires the trust in some physical theory, in some observation and in other *ceteris paribus* clauses. The experimental setting will typically involve symbolic interfaces, hence we will make sure, that under the given circumstances the following sentences are true "if the pointer of the galvanometer is in position A (for short, if A), then at this space-time (in

[143] The fact that the second clause of the sentences (1)-(6) does not refer to a reading should not lead us into thinking that the same considerations would not apply to it. The fact that we smell a certain odour will count as a valid piece of information about the theory only under certain conditions. Anyway, most of experiments in physics generally involve only collections of numerical data as results, which means that the observable fact corresponding to them are pattern of liquid crystals, positions of pointers and so on.

[144] Clearly if in a certain region of space there are other sources of magnetic fields, the resulting field may have a different shape, or may be simply absent.

our laboratory) there is an X" (a similar statement must be derived for the magnetometer, involving Y and an observation sentence B). When this procedure has been accomplished we can say that Maxwell's theory, in conjunction with *ceteris paribus* clauses and with many observations and auxiliary assumption predicts: "here and now, when A, B." Finally we perform the experiment and we verify whether indeed we would utter both sentences A and B. *It follows from this analysis that the observation sentences A and B cannot be regarded as holophrastic expressions.* They are relevant for the test of the theory only because we recognise that the terms occurring in them refer to objects that are part of the causal chain we have brought into existence in the laboratory at the time in which the experiment takes place. They make implicit reference to space-time co-ordinates and they have a term by term structure that renders them completely theory-laden in Quine's terms.

Take for instance A. If A is to have anything at all to do with Maxwell theory, its utterance must implicitly include reference to our notion of what a galvanometer is and so on. In practice, A is valuable for scientific reports only if intended as an abbreviation for a sentence like "at such and such place time, at which many *ceteris paribus* conditions hold, where this and that object are present, there is a galvanometer, which is an instrument of such and such characteristics, and which in this case is casually connected in this way to... and the pointer of the galvanometer is in position A". Therefore:

We also need the complete apparatus of predication and objective reference for uttering an observation sentence that counts for a scientific report.

From the theory we cannot derive any observation categorical to be tested against observation sentences. On the contrary an observation sentence can count toward the testing of a theory only insofar we have, by means of theory and observation, decided that the *context* in which the observation is carried out is the appropriate one.

Nothing that is theory-free, such as a holophrastic expression, can constitute the checkpoint of physical theory. Theory dictates the conditions for an observation sentence to bear relevance to a theory.

It is easy to see that there is no end to the possible controls that a scrupulous experimenter may require. If, for instance, one suspects that there is something wrong with the shape of the glass column of a thermometer, one can resort to a technique devised for the purpose of detecting small irregularities on a surface. The experimenter may then doubt the reliability of his technique and call for further tests. At some point he will simply *judge* that the experimental setting is correspondent to his theoretical interpretation of it. This judgement will always imply the reliance on theory, on observation and on testimony, for certainly it is necessary in practice to believe, at least tentatively, that some instruments implemented have been manufactured and calibrated according to their finality. It is this theoretical judgement that stops the infinite process of checking the verification of the appropriate conditions and that allows an observable result to become epistemically significant for the test of a theory.

We can now evaluate Quine's claims 2) 3) 4).

2) From what has been said so far in an experiment there is no disentanglement between theory and observation that is achievable even in principle. Furthermore no sentence referring to observable events can be used in a test of a theory without specification of space and time.

3) The testing of theory cannot be done by means of holophrastic sentences, because a theory never implies observation categoricals. This does justice to the fact that a physical theory is never abandoned in the same way a common-sense belief sometimes is. My observing some A, not followed by B does not count as a disproof of any theory unless I specify when and where the observation was done and what conditions and hypotheses made me believe that the theory in question would have implied the occurrence of B given A in that moment. Indeed in official reports of experiments what Quine calls observation sentences are usually omitted, for the description of the context (including the specification of space and time) of the experiment and the interpretation of the data are much more important for the significance of the result (which will usually consist of a set of values). What an experimental physicist wants to make sure, when he publishes a report, is not so much that his colleagues will believe

in what he has seen, but that they will understand how the experiment can, up to certain extent, be repeated. Indeed:

Physics is a collective enterprise, and a theory is buried by a literature, not by scattered episodes of impingement on receptors by whatever it is that activates them.

On the contrary, as we have seen, the role of sensory stimulation, although indispensable for the scientific enterprise, is, in the practice of experimentation, subordinate to the role of theory. Furthermore, observation sentences used by scientists during an experiment must be seen as having internal structure, hence, according to Quine's terminology, they must be considered theory-laden.

4) Quine's definitions of empirical content and empirical equivalence are unacceptable, for no physical theory implies observation categoricals.

4.2.1.1 Observation sentences and prediction

In this and in the following subsection I will deal with two objections that might be raised to defend Quine's account of the relation between theory and observation from my criticism. The first concerns the role of observation in the everyday circumstances in which we rely on the predictions of physical theory.

It might be argued that in these situations we do utter observation sentences as unstructured wholes to describe events that are predicted by scientists. When people gather in a place to observe a solar eclipse, they are usually entirely unaware of the long labour that has been necessary for predicting that phenomenon. They look up to the sky and think: "there is an eclipse!" Some of the occasional observers may be children, or people ignorant of any physical theory. Some know that the dark circle obscuring the sun is *the moon*;[145] others, short of any scientific knowledge, may wonder just like children what is happening *to* the sun itself. There are fragments of rival systems of the world surrounding the same observation sentence; but the disagreement remains unnoticed. Everyone is ready to affirm "there is an eclipse" for having being conditioned to assent to that expression under relevant receptive episodes; and this is the only thing that matters for that occasion. What is of crucial importance, however, is that, in cases like the one just described, well confirmed theories are used simply as tools to make predictions at the level of common-sense facts. Observation has here no epistemic role. It would suddenly acquire scientific relevance in case some unforeseen event occurred; but then ordinary testimony would immediately give way to careful investigation.

The situation is reversed when we perform an experiment, for *in an experiment observations are used as tools to test theories*, and it is in the context of an experiment that the foregoing considerations apply. We must know how to master tools, we cannot use them unreflectively.

4.2.1.2 More on the relations between theories and observation categoricals

A second possible objection may concern the fact that it would seem questionable that no physical theory can imply an observation categorical. After all, the objection would run, a simple sentence such as

(9) "whenever lightning, thunder"

can be deduced by a consistent body of physical knowledge. We may be tempted to conclude that the criticism to Quine account's of the relationship between theory and observation can work only when the predictions of a physical theory concern the results of measurement devices. If this were the conclusion, the criticism would still be very damaging for Quine, given that theoretical physics deals virtually only with predictions concerning the results of measurement devices. However, a more careful discussion will show that the considerations developed in the preceding sections can be applied also to the lightning and thunder case, and to similar ones.

[145] They have made a step forward into theory by identifying the moon as normally seen with the dark circle visible during a solar eclipse. On identity and ostention, see FLPV, pp. 65-79.

To start with it needs to be stressed that we cannot give a rigorously nomological account (that is in terms of laws and initial conditions) of phenomena as complex as a thunder. Physics is able only to provide *explanations* of them, through the aid of simplified models that have little predictive power.[146] However for the purpose of this discussion I will assume that a rigorous nomological interpretation could be given, in principle, also for such irregular and multifarious phenomena. This assumption simplifies the discussion and constitutes, after all, a concession to Quine's overall conception of science.

As we have seen Quine is trying to specify the empirical content of a theory. In order to achieve this he wants to reach a stage of analysis in which he can proclaim to have "the theory on one side" and "the observation categorical on the other". Between these two levels he sees a relation of logical implication. I believe that this simple picture does not apply even to the trivial empirical generalisation we are discussing now, for, again, no body of physical theory however complicated will imply a sentence like (9) without being supplemented by an enormous amount of information concerning *the atmosphere of the Earth*. Indeed no matter how many theories we avail ourselves of (electromagnetism, chemistry, mechanics, acoustics), sentence (9) will never be implied, for it is true only under particular conditions. We may be inclined to consider (9) as a primitive example of law of nature, but indeed, from the point of view of physical theory, it is not even the caricature of a law of nature.

Being used to verify (9) in everyday life we can believe that it states a generality, so that, following Quine, we can omit reference to space and time, but (9), seen as an empty generality cannot be derived from theory, that, referring only to theoretical ideal-types[147] cannot say anything by itself about what happens in the atmosphere of the Earth. Indeed there is no reason to think that there cannot be a planet whose atmosphere gives rise to similar electrical phenomena, but is not very dense. In such an atmosphere a lightning may not be followed by a thunder. There would be of course a propagation of acoustic waves, but it would not be detectable by a human ear. Similarly we would be inclined to think that "whenever a stone is in the water, it sinks"[148] can be seen as a consequence of Newton's theory, but to convince us of the

[146] For a model of the electric phenomena in the atmosphere, see Feynman, 1964, Vol. II, chapter 9.
[147] See section 1.6.
[148] An example of *focal* observation categorical.

contrary it will suffice to think about what would happen if we tested this observation categorical in an orbiting laboratory, or if the stone were one of volcanic origin, whose specific weight is less than that of water. Generalisations of common-sense observations are normally false, if taken without further qualifications, besides a theory will imply them only when many such qualifications are added to it, and these qualifications must be expressed in the language of theory. Indeed these two facts are one and the same, for it is precisely because those generalisations are true only in some circumstances, that theory can imply them only under certain assumptions. In conclusion:

It is a model taking into account the specific conditions of the Earth's atmosphere that must be added to theory in order to deduce a sentence similar to (9).

This model will be built with the aid of both observation and theory, and finally will be judged to be a faithful theoretical representation of what actually happens in the Earth's atmosphere. Hence we find again that there is no purely theoretical *side* implying an observation conditional and hence (9) is not part of the empirical content of any theory.

The model I have just referred to is the analogue of the theoretical interpretation with which, as it is shown by Duhem (see section 1.2), the experimenter must mentally replace the actual experimental setting that he has before his eyes. Finally, even in conjunction with empirical information concerning the Earth, plus *ceteris paribus* clauses, plus an overall judgement of the applicability of the theories in the case at issue, (where the "case" must be defined also in terms of spatio-temporal co-ordinates), our body of theories will not imply (9), intended as an unrestricted generality, but something like:

> On the Earth, in the period of (geological!) time during which the density of the atmosphere exceeds a certain threshold value, a normal person placed within a certain distance from the area where a lightning involving an amount of energy that exceeds a given value is produced

will, if there is no obstacle to the propagation of electromagnetic and acoustic waves, hear thunder after seeing the lightning.

An observation will be of some relevance to the refutation of the body of theoretical and empirical knowledge that does the implying, only if circumstantiated with all information that our knowledge of these phenomena renders pertinent.

The apparent difference between this case and that of the thermometer is due to the fact that whenever measurement instruments are involved, it is easier to realise that the assumptions concerning the correctness of the conditions setting the stage for a *scientifically* relevant observation must be checked continuously and with a high level of precision. This is why "the here and now" came into place. Normally, when an experimenter performs routine operations in his laboratory, he will simply forget about all the assumptions that he is making, for his experience tells him what he can be confident about at least tentatively. It is only from a logical point of view that the assumptions are reiterated at each step. This explains also why there is in practice an end to the potentially infinite regress of tests to which I referred before. The "here and now" in which the judgement to decreeing the relevance of the observations for the theory is expressed, can expand both in time and in space in proportion with the simplicity of the experiment, with the level of vagueness tolerated in the results and, most of all, in proportion with the confidence that the determining conditions are stable. If I am measuring the temperature of my body, I will not worry about which room of my apartment I am sitting in, or whether I am close to the magnetic field produced by a freezer.[149]

Sentence (9) seems detached by theory essentially for its lack of mathematical precision and its dependence on highly stable conditions. The laboratory in this case is the entire atmosphere of the Earth. The duration of the experiment is at least as long as mankind's history. Furthermore when we talk about lightning followed by thunders we are not fussy about the correlation between the energy involved in the former and the intensity of the latter. In this laboratory, during this long time, we have become accustomed to witnessing the constant conjunction of these events. Hence we tend to

[149] We have seen, commenting on Duhem's analysis of the role of approximation in physics, that the more precise a measurement must be, the more conditions will become relevant for the correctness of the measurement (see end of section 1.2.1).

believe that these conjunction is something like a law of nature, *and that a theory of the world should imply it.* But from the lofty point of view of the theories of electromagnetism and mechanics, (whose language does not include terms like "Earth" or "atmosphere"), the atmosphere of the Earth is but one of the infinite possible instantiations of one of the ideal-types they can jointly define, just like the electric system of a house. We might consider (9) as a *common-sense law*, just like Duhem does (see section 1.1), but by doing so we would simply highlight the cleavage between scientific and common-sense knowledge.

There is no difference in principle between my confidence that when I press a switch a lamp will light, and my confidence that after seeing a lightning I will hear a thunder. The apparent differences are due to the fact that the conditions presupposed in one case are extremely more variable than those presupposed in the other. Will we require a theory of the world to imply observational generalisations concerning our chandeliers?

5. AN ALTERNATIVE ACCOUNT OF THE RELATION BETWEEN THEORY AND OBSERVATION

Introduction

In this chapter I will sum up the results of the thesis and add further comments in order to sketch an account of the relation between theory and observation which is alternative to that of Quine. The impossibility to detach from theories their experiential implications is the starting point of these considerations.

In the first section the consequences of this impossibility for the concepts of empirical equivalence and underdetermination are illustrated. In the following section I analyse once more the link between theories and empirical evidence stressing the crucial role of practical activity in establishing it. In the third section I argue that Quine's empiricist philosophy of science is still too close to Neopositivism and does not take into account the most important consequences of Duhem's analysis. Finally, in the last section, the implications of the theory-ladenness of experimentation for the dilemma of empirically equivalent systems of the world are discussed.

5.1 Empirical equivalence (and underdetermination) without empirical content

Let us now consider, in the light of the preceding considerations, what sense can be made of the concepts of empirical equivalence and underdetermination. Following Quine, I will assume that two global theories are stated in the same language and that the proponents of both refer to observable events through the same observation sentences. As we have seen even in this case it would be impossible to detach from the two theories something like an empirical content, not even if we were able to encompass in a tight formulation an infinity of observation categoricals. The problem is by no means practical; the problem is that we lack a strategy for specifying the empirical content of any theory. From a theory we can derive only conditional sentences of the form

(9) Whenever* conditions C hold, then when A, B

(where A and B are two observation sentences, or more plausibly two long conjunctions of observation sentences). The reason why this sentence cannot be regarded as belonging to the empirical content of a theory is that its premise is not observational (that is, it cannot be verified observationally). As we have seen the explanation of this fact is twofold. On one hand the expression "whenever*" quantifies over space and time, and hence is committed to a particular ontology of space and time; on the other hand C consists of general of observation sentences such that "there is a small container full of water", but also of sentences containing theoretical terms such as "accumulator", or "inductance" (some of which refer to measurement devices) and describing *ceteris paribus* conditions in theoretical language.

Following this argument our search for the bottom level of the descent from theory to observation we will have to turn to

(10) If here and now conditions C hold, then (here and now) when A, B

If we distribute the "if" over the many component sentences of the conjunction C, we will obtain a long conjunction of conditionals such as "if here and now there is a specimen of gold...", "if here and now there is a conductor with a resistance of 3 Ohm". Each proponent of one of the two alternative theories of the world may indeed state a proposition like (10). Given our premises the two variants will be very different form each other. They will share only the observational part of the description. Both physicists will utter the conditional "if there is a specimen of gold", for under our assumption they share a common store of observation reports and, precisely for this reason they would both utter the second clause of (10). However the two conditions (let us call them C' and C") will radically differ one from the other, for they will share only some observation sentences. Theoretical terms would be present, for instance, whenever a certain value of a physical magnitude appears in the description of the conditions.

To illustrate this situation, suppose that a person interested in physics and with a good deal of practical skills has built a complicated machine with a bulb at the top and a switch on a side. The machine need not be useful at all, it can be seen as a big toy or as a kind of technological puzzle. This apparatus, however complicated, serves to a simple purpose: when you switch it on, the bulb lights. The ingenious craftsman invites the two physicists belonging to the two rival schools and challenges them to explain the functioning of the machine in terms of their own theories (note that we need not suppose that the craftsman defends a third theory of the world, he may well be accustomed to accept one of the two). They can open the apparatus, take away each component and perform any kind of test they may believe useful, but at the end of the analysis they must be able to deduce from their theoretical description of it that the bulb lights, when the switch has been pressed.

After long and careful investigation the two physicists have both drawn on a blackboard their interpretation of the experimental setting. In short they have both written a sentence of the form of (10). Actually, as they have before their eyes a given experimental setting, the two sentences will be

(11) Here and now conditions C' hold, hence when A, B

(12) Here and now conditions C'' hold, hence when A, B

(where A and B mean respectively "the switch is in position on" and "the bulb lights"). I believe we have now got as far as it is possible in the direction of defining a theory-free relation of empirical equivalence.[150] The ingenious craftsman is able to say (by believing that none of the physicists has made a mistake), that the two systems of the world are empirically equivalent at this place-time and as far as the phenomenon investigated (the machine) is concerned. To be sure the real appreciation of the empirical equivalence of two theories, even in such a weak sense, would require the

[150] For what has been said in section 4.2.1, sentences A and B, being uttered in the context on an experiment, must be interpreted by the two physicists according to the different internal structure that their respective language-theories prescribe.

intellectual ability to master both theories of the world and the practical ability to perform all tests needed in order to describe the experimental setting according to those theories. After all, one of the two physicists could have made many mistakes, from the point of view of his own theory, that led fortuitously the a correct prediction.

The conclusion seems disappointing, for all we have is so to speak *an indexical definition of empirical equivalence*; but this was certainly to be expected from the moment in which we realised, together with Quine, that there is no way to compare two different global theories of the world in terms of a theory-free notion of space-time, and that we can't first fix a certain theory of space time and then go on comparing the two theories on that ground. Quine's solution was to resort to observation categoricals, which are time-free and space-free generalities. But we have seen that those categoricals are never implied by physical theories.[151] Therefore we must be content with comparing the predictions made by the two rival schools case by case.

Strictly speaking there is no notion of empirical equivalence, but a local one, local in the sense that it refers to a particular phenomenon defined ostensively, and to the particular place-time at which the phenomenon takes place.

I obviously sense some uneasiness here, for it seems that with a normal inductive step we could jump from (11) and (12) to a more general statement of empirical equivalence, because we would expect that "whenever" we used the big toy again, or "whenever" we were to build a similar one, the two theories would always make the same prediction. But this impression is motivated by the fact that we normally accept a basic theory of the world as more or less unquestionable, along with a well-established ontology of space-time. As we have seen, there is no theory-free way to express a generalisation of the experimental result just described that could be shared by both physicists.

In practice physicists will talk about empirical equivalence in a more straightforward way. They will compare different theories that are related to a common background of physical knowledge, but differ in the interpretation of the results of

[151] Independently of the problem of having to specify a system of co-ordinates, it can be shown with arguments similar to the ones just illustrated, that a physical theory cannot imply even observation conditionals.

some experiments, i.e. they differ in the interpretation of results that are themselves highly theoretical. One extreme case of this situation is provided by the already discussed example of van Fraassen, in which two theories branch away from each other only at the most theoretical level. However, as I have already pointed out, in cases like this we don't speak of a purely empirical content that two theories share.

In conclusion, the concept of empirical equivalence can be construed in a narrow or in a broad way. If we require that it be applicable also to theories that are as different as they can be (given that they are related to the same observation sentences), then the notion of empirical equivalence must be indexical. If we are content with comparing theories that have all grown apart on top of some shared background knowledge, then the required notion of "empirical" equivalence can be a more liberal one, referring to a body of theory-laden facts, which is provisionally considered unquestionable (such is also the situation described in the examples of section 1.5.1). In both cases we talk about empirical equivalence, without reference to a purely empirical content.

The move from empirical equivalence to underdetermination is now more complicated. If we consider the hypothetical case of radically different rival *systems of the world*[152], then we will have to talk only of an indexical notion of underdetermination, corresponding to indexical empirical equivalence. If instead we deal with the more pragmatic notion of empirical equivalence between two partial theories, then, the concept of local underdetermination will be at hand.[153]

5.2 The pragmatic dimension of the relationship between theory and observation

In the article *Three Indeterminacies* Quine seems to have in mind some of the objections just discussed to his account of the testing of theories:

> Scientists are apt to feel that my account of test and refutation is still vastly over-simplified, despite its commendable holism. In practice a failed prediction is often excused, and a hypothesis saved, by finding

[152] But see section 5.4 for some comments on the possibility of a system of the world.
[153] In the case of partial theories, the step from empirical equivalence to underdetermination has been questioned by Laudan and Leplin (1991).

> or assuming some interference. Such an excuse, however, can be analyzed in my terms as a discovery of a logical error in the deduction of the observation categorical that was tested. Some tacit qualification of ceteris paribus needed to be sorted out. Some of its tacit clauses would have been needed *among the experimental conditions – hence in the antecedent of the observation categorical* – for a rigorous clinching of the implication.[154]

Quine then goes on to say that in practice it is never possible to state all hypotheses that are needed to imply the observation categorical of an experiment. However his analysis, as he underlines, is intended to grasp the logical "essence" of empirical tests, to understand "how, in principle, empirical science might proceed if, heaven forbid, all were explicitly set down."[155] My intent has been precisely that of showing that Quine's account is flawed in principle and fails to grasp the logical structure of scientific testing, not its multifarious and unmanageable details. What is striking is that while dealing with the problem of *ceteris paribus* clauses Quine realises that some of them will have to be about the experimental conditions, hence, as he says, in the antecedent of the observation categorical. But how then can this antecedent consist of a purely observable situation? These *ceteris paribus* clauses will be stated in theoretical language and will resist any translation into observation sentences for the reasons already discussed. Moreover, it is not only about these "other things being equal" clauses that one should worry about. The reliability of the instruments involved is again a theoretical concept that cannot be unpacked in observation language.

Indeed from my analysis as well as from the last long quotation it results that there are two logically different kinds of beliefs that a physicist must have in mind during the test of a theory. There are those belonging to the body of abstract theories that is under test; and there are those that concern narrowly the particular experimental setting he has before him. The conjunction of these two sets will then clearly not imply an unrestricted empirical generality, but only a *contextual conjunction* of particular observable events in the laboratory where the experiment is being performed. The abstract and general hypotheses (among which are the axioms of the theory and some

[154] TI, p. 12. Emphasis added.

ceteris paribus clauses) are needed in order to get down to the level of an ideal-type. At that level, and only at that level, we can have an unrestricted generality, stated in theoretical language. The second set of assumptions, the local and particular ones, is required so to speak to move in the opposite direction, from the concrete to the abstract. They are needed to subsume the observable situation under the ideal-type. No experimenter can proceed without them, for it is not the case that "theory tells him that if the hypothesis under consideration [the abstract and general one to be tested] is true, then, whenever a certain *observable* situation is set up, a certain effect should be observed."[156] The set up *is* observable, but no theory will imply a statement having as antecedent its description in observation sentences, even if this were possible in principle.

The two sets of hypotheses build up their own part of a bridge from the opposite banks of a river, and the bridge is not so much between theory and observation (although some preliminary observations will be needed on one side to describe the experimental setting), but between the *universal* and the *particular*, between the ideal-type and the realisation of the ideal-type. The final act of observation, the result, stands, so to speak, on top of the bridge; it consists of the observational description of a particular event or group of events that is logically linked to the realm of abstraction by the bridge, but only insofar other hypotheses support the bridge *on the other side*. If the bridge is wobbling, then the cognitive import of the observation, that is of the sensory stimulation, becomes suspect. Without bridge there is no experiment, but only bird watching.

The Duhemian character of these considerations should be evident. What I called, while discussing Duhem, the "scientific judgement" needed in order to subsume a concrete situation under an ideal-type amounts precisely to the move from the particular to the abstract that allows the logical connection of theories not with unrestricted empirical regularities (for they are never connected to them), but with local conjunctions of observable events. This move is a "very complex intellectual elaboration which substitutes for the recital of concrete facts an abstract and symbolic judgement."[157]

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[155] Ibid.
[156] Ibid., p. 8. Emphasis added.
[157] ASPT, p. 153.

Duhem talks about "intellectual elaboration" while describing how the physicist states the result of his experiment in abstract and symbolic language. Such an elaboration though is needed also at the beginning of the experiment, when the setting is judged to be correct. I believe that here Duhem's analysis is somehow incomplete, for it does not underline enough the distinctive role of the experimenters' practical activity. In order to judge that some observable conditions can be described in a certain way in the language of theory, a physicist cannot simply observe and recall his theoretical knowledge, *he must perform some operations*, namely, tests and measurements. An experiment does not simply amount to the interplay of theory and observation, it is based on the latter along with practical operations. If we intend to know whether what we have before us is a reliable galvanometer, or whether a conductor has really a certain resistance, we will have to act in some way *on* those objects. *The scientific judgement is therefore based on a form of subsumption that has also a pragmatic dimension.*

We can see the contrast with the case in which we want to understand if a given vector space is an Hilbert space, or if a function is differentiable. In those cases we simply find out whether some abstract objects have or fail to have the properties relevant for membership in a class of abstract objects. There is usually a routine for doing this and a computer is able to carry out the task. Also the classification of ordinary objects can be in some cases reproduced approximately by pattern-recognising systems. The activity of the experimenter is a different matter. The judgement stating that the experimental setting is correct relies on a complex activity, which besides being theory-dependent is also practice-dependent.

The real starting point in an experiment is, of course, the very theory that one intends to test. So much would be granted even by Quine. The next step consists of the realisation of an experiment. In general, for this purpose, a concrete device has to be constructed. Each of its components needs to be carefully manufactured according to our aim, relying on tools and measurement instruments. Then the device needs to be checked and finally prepared (calibrated) for the particular experiment we have in mind: *in an experiment the hand is no less necessary than the eye*. Just before actually carrying on with the test, the experimenter tentatively commits himself to a scientific judgement, which presupposes all the background knowledge and all the activities involved in the preliminary stage (therefore it will rely also on memory and testimony, see section 4.2). Note that any complicated experiment requires a careful discussion of the various approximations tolerated in the preparation as well as in the execution. Moreover it requires stopping at some point the infinite regress of controls to which I have already referred. There is no given routine that the experimenter can follow to accomplish his aim. The first thing a student is told in a laboratory is that it is experience, intuition and autonomous thinking that matter, not the commandments found in scholarly primers to experimental physics.

This scientific-pragmatic judgement creates the logical connection between the theory and the phenomena that will be observed. In what sense is this connection *logical*? If we consider a very simple experiment, in which we just expect to see a pointer in a certain position after an apparatus has been switched on, then we can say that the conjunction of some theories (among which the one to be tested) with the judgement about the experimental setting literally implies the expected result. This derivation, however, differs in a distinctive trait from those of, say, mathematics and pure geometry. The difference does not lie so much in the fact that a derivation in mathematics and in geometry leads unavoidably to true conclusions if no logical error is made, while the predictions of physics can turn out be wrong for reasons that have nothing to do with logic. In my view the most striking difference lies in the fact that among the premises of the experimenter's derivation there is one, the final judgement about the experimental setting, which has a kind of indexical nature. A theory is logically connected to experience only in virtue of a judgement, which is uttered by *a person* at a place-time about a particular context. Once stated, the judgement allows the implying, the logical connection; but this logical connection exists solely in virtue of human activity, and is established only within its spatial and temporal bounds.

Empiricism is often seen as a philosophy that enhances the role of subjectivity. The phenomenalists' attempt to translate science into a sense datum language thus rendering theoretical talk in principle redundant, can be seen as a move toward the identification of knowledge with what is given to the subject. Quine, as we have seen, is still to a certain extent close to that point of view. He is still inclined to think, in some rather unclear sense, that if we knew everything about the observable world, we would be dispensed with theory. However Quine believes that there is a purely logical link between theory and observation, a time-less link which connects theory to generalisations expressed in observational language, which in turn correspond to constant conjunctions of specific patterns of stimulation. In this way the relationship between theory and evidence is abstracted from the conscious activity of the subject. Observation is given priority in knowledge, but it is encoded in observation conditionals that, so to speak, *stand on their own*. Indeed Quine sees also their relationship with abstract theories as standing on its own, in the realm of ideas (forgetting for a moment Quine's opposition to any kind of Platonism). This view is coherent with the conception according to which science is fundamentally a set of sentences, or a part of our language. Some sentences, the theoretical, stand in logical relation of implication with some others, the observation conditionals. Here we find the roots of Quine's belief that the empirical content of a theory can be detached from the theory itself.

I believe, on the contrary that science can be better portrayed as an *activity* based on theorising, manipulation and observation. The evidential link between theory and observation can only exist pending a scientific-pragmatic judgement that mediates *locally* between the realm of abstractions and the world of tangible objects. In physics the divine Demiurge who shapes matter in accordance with the ideal forms is the very physicist who builds the experimental setting and verifies that it is has the required characteristics.

In conclusion, it is impossible to abstract science and, in particular, the evidential link between science and observation from the active role played by the human subject, that, besides being a recipient of stimulations, is also a conscious agent who chooses and deliberates according to pre-established aims and beliefs.

5.3 The theory-ladenness of experimentation versus Quine's empiricism

The contrast between Carnap and Quine is, after all, not very sharp. According to Carnap it makes sense to speak about the experiential consequences of the truth of a single statement; according to Quine instead only a conjunction of statements can have empirical meaning. Such a conjunction must have a critical semantic mass in order to imply anything observable. As a conjunction of statements is nothing but *a* statement, Quine's divergence from Carnap amounts simply to the restriction of the number of statements that are empirically meaningful. Quine's holistic turn boils down to this restriction; the rejection of the analytic-synthetic distinction being, as we have seen, a consequence of it. Verificationism is there and so is the belief that the empirical content of theories can be individuated and separated from the theories.

We have seen that Quine's rejection of the intuitive notion of meaning leads to a problem for the coexistence of empiricism and holism. In what sense can language be welded to experience if there is no sentence that has by itself empirical meaning? The notions of observation sentence and of stimulus meaning are designed to solve this problem. The solution comes through the acceptance of a naturalistic theory of knowledge, in the framework of which empiricism becomes a scientific thesis and semantics itself becomes empirical. Some sequences of sounds are related through conditioning to given receptive situations: here we have the constraints that experience imposes on language. Quine is then a holist, for he claims that only conjunctions of hypotheses can be tested, and an empiricist, for he believes that the cognitive significance of such conjunctions is limited to their experiential implications.

I have not questioned whether Quine's empiricism might be successful as an account of common-sense knowledge. I have instead argued that it cannot be applied to the case of physics. In point of logic, holism alone does not suffice to support my criticism. If holism is taken narrowly as the thesis that only a conjunction of hypotheses can be empirically tested, then we cannot derive from it that it is impossible to separate from a theory its (purely) empirical content. This impossibility I take to be rather a consequence of a thesis that is logically stronger than holism, for it implies it, namely that physical experiments are necessarily based on the interpretation in theoretical terms of what is directly observed. I have above described this thesis as the motivation for Duhem's holism or as the thesis of the theory-ladenness of experimentation, for it provides an argument to the effect that tests in physics have necessarily a holistic character (see section 1.4).

We can now reconsider the question posed in sections 1.6 and 1.6.1: what does it mean to say that the aim of theory is to save the phenomena? In those two sections I have argued that the theory-ladenness of experiments forces us to give a new meaning to this time-honoured motto, for theory saves phenomena that are shaped by it. I stressed that background knowledge is what is needed in order to subsume a pattern of observable facts under an ideal-type, which in turn constitutes what theory deals with. In the last section I have remarked on the pragmatic component of this kind of subsumption. Quine has followed a different direction. Instead of accepting that the phenomena scientists investigate are theory-laden and practice-laden, he tries to define a theory-free empirical content of scientific theories consisting of empirical regularities that can be tested against observation sentences. After all, Quine could argue, if theory predicts unrestricted regularities at the level of the ideal-types, and if background knowledge (that is other theory) allows us to subsume observable situations under ideal-type, it should be possible in principle to formulate all the various observable regularities that correspond to the same theoretical regularity. The criticisms contained in chapter 4 are intended to show that this strategy is not available: theory cannot imply unrestricted generalities concerning the conjunction of observable situations.

Science is not a conceptual bridge linking sensory stimulation to sensory stimulation, for in a scientific experiment of some complexity theory is always on both side of sensory stimulation. I believe, therefore, that Quine's account of the empirical basis of science is vitiated by the failure to appreciate the different status of experimentation with respect to common-sense observation. In this sense my position is fully Duhemian. We can say that experiments present a peculiar holistic character that has remained unnoticed by Quine, in spite of his acquaintance with Duhem's writings.

5.4 Incommensurable scientific traditions versus empirically equivalent systems of the world

Let us go back to the problem of empirically equivalent systems of the world. What I have being arguing for is that if we follow Duhem in thinking that some of the bundle of hypotheses needed for implying an observable consequence will bestow to it its physical meaning, then we realise that there is no theoretically neutral evidence for physical theories. Observable events count as evidence for or against a theory when they occur within a context that can be defined only in theoretical terms. Therefore there is no general purely empirical ground on which to compare two radically different systems of the world, even if they share a common store of observation sentences. Indeed it becomes problematic to formulate the very idea of a system of the world. For, in what sense can a physical theory be complete? The idea of a complete physics can perhaps make sense if we accept the existence of given range of phenomena, which are ready-made, waiting for us, just like the Quinean observation categoricals. Without this assumption, the concept of "system of the world" is in the need of clarification.

I believe Quine's thought-experiment should be modified, by giving up reference to empirically equivalent systems of the world and by considering instead the logical possibility of two entirely independent, highly developed *traditions of research*. We can still assume that the members of the two rival schools adopt the same set of observation sentences.[158] As we have seen, the only conceivable comparison would be based on indexical empirical equivalence. However for most purposes even this minimal form of direct empirical comparison would not be possible. Most of the experiments in an advanced science yield results that resist reduction to a simple conjunction of observable events. This is due both to the already stressed fact (see section 1.6) that many physical theories developed in the Twentieth Century do not have any "ostensive" character, and to the complication often involved in the analysis of data. An experiment at the CERN, for instance, requires a sophisticated elaboration of a huge amount of data, which needs to be carried out case by case (not to mention

[158] I refer to traditions or schools *of physics*, without presupposing that they would be in agreement as to the rest of knowledge. Like Quine, I assume that their agreement may not go beyond observation sentences intended holophrastically.

the complexity of the preparation of the experiment). Without such theoretical elaboration there is no result, but only noise. If we take in consideration also the directive role of theories in suggesting what experimental setting to realise, we might *expect* (although this would be by no means necessary from logical point of view) that the proponents of the two rival traditions would look for different things in different directions, would carry out radically different experiments, interpreting observable results each according to their own theories, and would perform in their laboratories operations that have no meaning for the scientists trained in the other tradition. In short, it would be even likely (but, at any rate, certainly possible) that the two traditions could turn out to be essentially *incommensurable*.

The mere logical possibility of this eventuality is enough to suggest, in my view, that the discussion over ecumenism and sectarianism in epistemology should be recast in a framework in which underdetermination has given way to incommensurability. Furthermore the latter and not so much the former, should be seen, in general, as deeply related to the Duhemian findings concerning the holistic character of physics.

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