Duhem, Quine and the other dogma

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1 Introduction

A resemblance[1] between positions held by Duhem and Quine has led to the conjunction of their names: one speaks of "Duhem-Quine." Whether the conjunction—amid differences[2] of period, provenance, profession, subject-matter, style and generality—is entirely justified is debatable, but not really the issue here. Quine's position is famously expressed in "Two dogmas of empiricism"; it was by disputing the second[3] (dogma2) that he came to be associated with Duhem. But there is also the first (dogma1), the "cleavage between analytic and synthetic truths."[4] Quine claims they are equivalent (dogma1 $\Leftrightarrow$ dogma2), indeed "two sides of a single dubious coin," and contests both together. Duhem on the other hand attributes the impossibility ($\neg$ dogma2) of crucial experiments to the 'cleavage,' as one might call it, between physics and mathematics. But surely the truths of physics are synthetic, those of mathematics (more or less) analytic. How then can the 'Duhem-Quine thesis' ($\neg$ dogma2) depend on the cleavage separating mathematics and physics (dogma1?), while a purportedly equivalent thesis ($\neg$ dogma1) rejects the cleavage between analytic and synthetic? We appear to have something like

$$(\text{dogma1} \underset{\text{Quine}}{\Leftrightarrow} \text{dogma2}) \wedge (\text{dogma1} \underset{\text{Duhem}}{\overset{?}{\Rightarrow}} \neg\, \text{dogma2}).$$

A kind of holism[5]—an entanglement of essences and accidents,[6] of essential experimental intention and accidental auxiliary assumptions—is the main obstacle to

[1] On this resemblance, as recognised by Quine, see the footnote on p.41 of [42], footnote 7 on p.67 of [43] and the very beginning of [45].

[2] Krips [35], Ariew [7], Quine [45] and Vuillemin [49] have pointed out several. Too many according to Needham [39], who argues that Duhem and Quine share much common ground.

[3] In other words "*reductionism*: the belief that each meaningful statement is equivalent to some logical construct upon terms which refer to immediate experience," as Quine ([42] p.20) puts it.

[4] Quine's rejection of it has met with much disapproval; see for instance [38], [46], [27], [33], [34], [6], [15].

[5] For a detailed analysis of various kinds of holism see [23].

[6] "Accident" and cognates will sometimes be used in a rather 'Galilean' way. For Galileo an *accidente* deviates from or even interferes with the ideal purity of an object or scheme; hence air resistance and friction are *accidenti*, as is an imperfection on a glass sphere or smooth plane.

crucial experiments and (empirically grounded) meanings. Using notions hinted at by Duhem and Quine, formalised using the resources of set-theoretical axiomatisation, I argue that such holism and inextricability can be largely overcome.[7] Taking Quine's association—however questionable—of essence, meaning, synonymy and analyticity for granted, I also argue that analyticity is rehabilitated to the extent that the aforementioned entanglement of essences and accidents is undermined. If this recovery of the analytic completely dissociates it from the synthetic, giving it a distinct and separate identity, we arrive at the aforementioned paradox; for a rehabilitation of crucial experiments would appear to have the opposite effect on mathematics and physics, by consolidating the cleavage between them rather than undermining it. The matter is brought up, not for definite resolution, but to shed light on the web of issues involved, including relations between the arguments of Duhem and Quine.

I begin (§2) with a scheme for overcoming holism by disentangling essences from accidents, which leads (§3) to a new characterisation of the meaning and reference of sentences, involving 'abstract tests.' After noting (§4) that Duhem and Quine themselves already adumbrated such tests I show how they can be formalised in the language of model theory, in fact of set-theoretical axiomatisation. A quantum-mechanical example is looked at in §5. In §6 I consider how Quine relates meaning, essence and analyticity, in §7 how Duhem relates the cleavage between physics and mathematics to the impossibility of crucial experiments, and whether holism really does have conflicting implications for Duhem and for Quine.

2 Essences, accidents and holism

"The Aristotelian notion of essence," writes Quine ([42] p.22), "was the forerunner, no doubt, of the modern notion of [...] meaning. [...] Things had essences, for Aristotle, but only linguistic forms have meanings. Meaning is what essence becomes when it is divorced from the object of reference and wedded to the word." Much here[8] turns on the fact that there is more to the object[9] of reference than just the essence intended—for if the essence exhausted the object why speak of an essence at all. Since there *is* more to it, we can distinguish between the essence and the rest or *accident*. Perhaps an essence is best viewed as being 'embodied' in an object, which bears the essence along with unintended accidental features.

[7] Similar claims abound in the literature, *e.g.* "A naive holism that supposes theory to confront experience as an unstructured, blockish whole will inevitably be perplexed by the power of scientific argument to distribute praise and to distribute blame among our beliefs" [25]. See also [28], [29]—Quine replies in [44], Laudan defends Duhem in [36], claiming that Grünbaum has attacked too strong a version of "the Duhemian argument"—[26] and [6].

[8] This section serves only to introduce the next, and a notation, without any pretence of contributing to the abundant literature on the subject.

[9] Here an 'object' will be a *physical* object—even if mathematical objects have been considered ever since the early days of the meaning-reference distinction; see [24] p.26 for instance.

A given meaning, then, breaks an object up into essential and accidental features, the latter being unintended and *dispensable*, in the sense that without them the object would remain 'what it is' and not be ontologically compromised. The idea is that since a man after a haircut undoubtedly remains a man, what's left on the barber's floor was accidental.

Suppose a word W refers to an object O characterised by certain features $F = \{F_1, F_2, \ldots\}$. Whereas reference catches all the features, essential and accidental, only the essential ones $\bar{F}$ are meant by W. Even if we know that $\bar{F}$ is a proper subset of F, it may be less clear exactly which elements make it up. Hence the following test: remove the features one by one, and see what happens; if F_1 is removed and the object with features $\{F_2, F_3, \ldots\}$ is still intended, F_1 was not essential, and so on. Of course the test cannot be conclusive since the essences $\bar{F}$ are never found on their own, without accidents, some of which will necessarily be tangled up with essences (which could otherwise exhaust the object). Suppose a physical constraint prevents F_m from being separated from F_n. We notice that W still applies when both are present, and that it no longer does once they have been removed. What then? We cannot tell the three cases (1. $F_m \in \bar{F}, F_n \notin \bar{F}$; 2. $F_m \notin \bar{F}, F_n \in \bar{F}$; 3. $F_m \in \bar{F}, F_n \in \bar{F}$) apart and must therefore wonder about *dispensability*; for if a feature F_m cannot be removed without taking something essential with it, in what sense was that feature dispensable and hence accidental? F_m and F_n may be *conceptually* separable, just by thought, but *physical* separation can be considered more trustworthy and 'empirical.' This entanglement of essence and accident already adumbrates the holisms of Duhem and Quine.

For proper names and single objects the problem is insurmountable. But even if the essential features $\bar{F}$ cannot exist on their own, without accidental ones of some kind or other, they may be found with *different* sets of accidental features: W could refer to *various* objects (which perhaps constitute a 'natural kind'). In other words $\bar{F}$ may be accompanied by the accidents $\{F_1^1, F_2^1, \ldots\}$ or by $\{F_1^2, F_2^2, \ldots\}$ or $\{F_1^3, F_2^3, \ldots\}$ *etc.*, in which case W, while *meaning* $\bar{F}$, would *refer to* object O^1 with features $F^1 = \{\bar{F}, F_1^1, F_2^1, \ldots\}$ or to object O^2 with features $F^1 = \{\bar{F}, F_1^2, F_2^2, \ldots\}$ and so on. Even without knowing the exact makeup of $\bar{F}$ beforehand, it is clearly a subset of $\hat{F} = \bigcap F^i$; and if the family of objects $O^1, O^2, \ldots$ is sufficiently large and the accidental features sufficiently varied, one can reasonably *identify* $\bar{F}$ with $\hat{F}$. The extension of W, if large and varied enough, therefore allows us to determine the intended essence. The idea being that even if that essence cannot be physically abstracted from the bearing object, with all its accidents, it can be abstracted from particular accidents (rather than others); for the distinguished features $\bar{F}$ emerge as the ones belonging to all the objects.

But of course not all linguistic forms are words. Quine seems to have been chiefly concerned with sentences, to whose meaning and reference we now turn.

3 The meaning and reference of sentences

For the empiricists an empirical procedure O was needed to give meaning to an (observation)[10] sentence W. But Quine wonders whether even that will work; for such an O cannot help entangling W with the world in a messy, complicated way, involving all sorts of *unintended* sentences, or rather 'collateral'[11] experimental features corresponding to assumptions one might even call 'accidental.' So we again have a holistic problem of entanglement: an inextricability of ideal experimental *essence* or *intention* and unavoidable experimental *accidents* needed to implement that intention in the world. This is already reminiscent of the meaning and reference of words, and indeed I will propose a parallel characterisation for sentences, emphasised by a similar notation. Whereas sets and their intersections were enough to separate essences from accidents in my treatment of words, resources from elementary model theory will be used to effect the separation for sentences and the experiments used to test them.

Frege extended his *Sinn-Bedeutung* distinction from words to statements:

> Wir fragen nun nach Sinn und Bedeutung eines ganzen Behauptungssatzes. Ein solcher Satz enthält einen Gedanken. Ist dieser Gedanke nun als dessen Sinn oder als dessen Bedeutung anzusehen?[12]

A few lines on:

> Der Gedanke kann also nicht die Bedeutung des Satzes sein, vielmehr werden wir ihn als den Sinn aufzufassen haben. Wie ist es nun aber mit der Bedeutung? Dürfen wir überhaupt danach fragen? Hat vielleicht ein Satz als Ganzes nur einen Sinn, aber keine Bedeutung?[13]

In due course he answers:

> So werden wir dahin gedrängt, den *Wahrheitswert* eines Satzes als seine Bedeutung anzuerkennen. Ich verstehe unter dem Wahrheitswerte eines Satzes den Umstand, daß es wahr oder daß er falsch ist.[14]

[10]Classification of sentences is not the issue here. Or rather it presupposes distinctions (analytic/synthetic *etc.*) that *are* the issue, and are best approached directly as such, rather than indirectly in a derivative attempt at classifying sentences.

[11]Indeed one is reminded of the "collateral information" of [43], esp. §§9,10.

[12][24] p.32. Quine may be in question, but not the indeterminacy of translation ([43], esp. §§12-16), in acceptance of which quotations have been left in the original. Translation: "We now wonder about the meaning and reference of a whole affirmative sentence. Such a sentence contains a thought. Is this thought to be viewed as its meaning or as its reference?" (The translations are mine.)

[13]Translation: "So the thought cannot be the reference of the sentence, rather we will have to take it as the meaning. What about the reference? Should we wonder about it at all? Does an entire sentence only have a meaning, but no reference?"

[14]P.34. Translation: "We will thus be obliged to recognise the *truth-value* of a sentence as its reference. By the truth-value of a sentence I mean the circumstance, that it is true or that it is false."

But since the leap from an object to a truth-value[15] is considerable, this seems an unnatural extension—however justified within his scheme—of the nomenclature first adopted for words.

Attempting, then, a natural extension of the meaning-reference distinction from words to sentences, I suggest that a single experiment O provides not the *meaning* of a sentence—for the reasons urged by Quine—but something more like its 'reference.' With the analogy between experiments and physical objects in mind I propose, then, to say that a sentence W *refers* to a specific experiment—to experiment O^1 with features $F^1 = \{\bar{F}, F_1^1, F_2^1, \ldots\}$ or to O^2 with features $F^2 = \{\bar{F}, F_1^2, F_2^2, \ldots\}$ or to O^3 *etc.*—and that its *meaning* is given by the subset $\bar{F}$ of $\hat{F} = \bigcap F^i$ that corresponds to W by expressing an ideal experimental intention, an abstract logical core. It is up to the ingenuity of the experimenters to reduce $\hat{F}$ to $\bar{F}$ by producing enough experiments, with sufficiently varied auxiliary assumptions. Or rather the experimenters begin with the experimental intention $\bar{F}$ expressing W, and then go about finding many different ways to implement it physically. The trouble is that $\bar{F}$ is a tenuous, ideal object, which cannot be performed on its own; auxiliary features[16] of some kind or other are needed to realise it, to bring it about. Quine's point is roughly that W cannot be determined empirically because its counterpart $\bar{F}$ cannot be carried out alone, without accidental auxiliary features, which then confuse the logic of the experiment by unavoidable entanglement with $\bar{F}$.

The various experiments could agree or disagree. Disagreement complicates matters; for then which are to be trusted? Would the majority, or perhaps some privileged experiment or subclass of them, necessarily be right? To avoid such complications unanimity will be required: the experiments must all yield the same verdict.[17] It will then be claimed that, taken together, they are crucial. Such 'cruciality' rests on the variety and prior plausibility of the auxiliary assumptions. Variety guarantees independence—for if the assumptions resemble each other too much, agreement will be no surprise[18]—and prior plausibility is inherited from other contexts. So I will assume that the validity of every auxiliary assumption F_b^a made in each experiment O^a was established in several other experimental contexts $\{O_{b1}, O_{b2}, \ldots\}$; and furthermore that validity so established is maintained in the particular experiment O^a; $a, b = 1, 2, \ldots$. The unanimity of the verdict cannot then be reasonably attributed to a conspiracy of the auxiliary assumptions $\{F_1^1, F_2^1, \ldots\}$, $\{F_1^2, F_2^2, \ldots\}, \ldots$; it must be due to the experimental intention $\bar{F}$.

Another approach, adopted by Grice and Strawson ([27] p.156) in response to

[15] Admittedly an object of sorts too, but a very particular one.

[16] Auxiliary *features* and *assumptions* seem closely related enough to justify conflation.

[17] Perhaps disagreement is more common or likely than agreement; but unanimous agreement remains possible nonetheless.

[18] As has been pointed out to me by John Earman and John Norton. The standard resources of confirmation theory, such as *probabilities*, have been deliberately avoided here.

Quine, is to deal with the troublesome auxiliary statements F_b^a by making "certain assumptions about the[ir] truth-values":

> [. . .] two statements are synonymous if and only if any experiences which, *on certain assumptions about the truth-values of other statements*, confirm or disconfirm one of the pair, also, *on the same assumptions*, confirm or disconfirm the other to the same degree.

But surely the truth-values of statements are subject to the same holistic entanglement as their meanings. Why should truth-values be less empirical, less susceptible to the intricacies of empirical determination, than meanings? Of course one could, while obliging meanings to maintain the *empirical* grounding that's causing all the trouble, arbitrarily adopt an 'ontological' notion of truth and truth-values, admitting the very kind of purely conceptual disentanglement that holism precludes for empirical meanings—but surely the problems at issue here would thereby be left untouched. So one can wonder about the legitimacy of a fine-grained, detailed (ontological) assignment of individual *truth-values*, within and alongside the messy tangle of (empirical) *meanings*, when the 'atoms' of meaning are so much larger than the 'atoms' of truth.

In the approach I propose, the unanimity of the verdict provides *a posteriori* support for the prior plausibility of the auxiliary assumptions.

4 Abstract tests

Before attempting a characterisation of abstract tests we note that a similar idea can already be found in *La théorie physique*:

> Pour apprécier la variation de la force électromotrice, il pourra employer successivement tous les types connus d'électromètres, de galvanomètres, d'électrodynamomètres, de voltmètres [. . .]. *Cependant, toutes ces manipulations, si diverses qu'un profane n'apercevrait entre elles aucune analogie, ne sont pas vraiment des expériences différentes ; ce sont seulement des formes différentes d'une même expérience* ; les faits qui se sont réellement produits ont été aussi dissemblables que possible ; cependant la constatation de ces faits s'exprime par cet unique énoncé : La force électromotrice de telle pile augmente de tant de volts lorsque la pression augmente de tant d'atmosphères.[19]

[19]P.224; emphasis mine. Translation: "To appreciate the variation of electromotive force, he can employ in succession all the known kinds of eletrometers, galvanometers, eletrodynamometers, voltmeters [. . .]. *However, all these manipulations, so different that a layman would see no analogy among them, are not really different experiments; they are only different forms of a single experiment*; the facts that really occurred were as different as possible, but can nonetheless be expressed in the same way: The electromotive force of such and such a battery increases by so many volts when the pressure increases by so many atmospheres."

An *expérience* here is not a particular real experiment, subject to the difficulties Duhem will raise later, in Ch.VI §§II,III, but a class of equivalent experiments that all test or measure the same thing. Such an abstract experiment can be associated with the class of its *formes différentes* in the same way a *theory* (in the logical, Tarskian sense) can be identified with all its *models*. The accidental and logically confusing peculiarities of particular implementation are thus transcended.

There is something similar in *Word and object* (p.32) too: "We may begin by defining the affirmative stimulus meaning of a sentence [. . .] as the class of all the stimulations [. . .] that would prompt [. . .] assent." A couple of pages on:

> [. . .] a stimulation must be conceived for these purposes not as a dated particular event but as a universal, a repeatable event form. We are to say not that two like stimulations have occurred, but that the same stimulation has recurred. Such an attitude is implied the moment we speak of sameness of stimulus meaning for two speakers.[20]

Here the models are the 'repetitions' of the "repeatable event form."

So both Duhem and Quine have in mind an abstract test—an abstract *expérience*, a universal, a repeatable form—with many particular realisations. It is in such tests that the desired cruciality will be sought.

One can wonder about appropriate formalisation, for the notion is nebulous and of little use as it stands. What the various realisations of an abstract test have in common is *structure*[21] of some sort; it is in that sense that they all test the same thing. But there remains the matter of what exactly "structure" is. The ordinary connotations of the word will hardly do; Duhem and Quine, who speak of *form*, provide little help. Specification of a means of description can clarify: of the many available ways of characterizing structure, the resources of set-theoretical axiomatisation, associated chiefly with Patrick Suppes (*e.g.* [48]) seem appropriate and will be used. In his language a set-theoretical predicate defines a *theory*, satisfied by *models*, whereas here the predicate will characterise an *abstract test*, again satisfied by *models*. It is the abstract test, rather than any particular model, that represents a crucial experiment. Auxiliary assumptions have admittedly to be made in each individual implementation, but again, they can be required to vary widely over the class, and to have a plausibility derived from other contexts.

The idea can be formalised by spelling out a set-theoretical predicate, after the manner of Suppes: a string $(A, B, \dots)$ of primitive notions 'is an X,' for instance, if certain axioms, say

[20] P.34. Quine argues, especially in [43] §§11,12, that stimulus meaning does not fix meaning well enough for all purposes and criteria. But his reservations, which regard behavioural linguistics, need not concern us here, especially as his characterisation of stimulus meaning is being taken only as a hint or rough ancestor.

[21] In the logical literature "structure" is often a synonym of "model," whereas here its meaning is closer to that of "theory."

1. A is a nonempty finite set.
2. The function $B : A \to \mathbb{R}^+$ is differentiable and ...
3. ...

$$\vdots$$

are satisfied. Any such particular $O^a = (A^a, B^a, \dots)$ satisfying the axioms is a *model*. The extension of the predicate 'is an X' is the set $\{O^1, O^2, \dots\}$ of models.

We are again dealing with essences and accidents, in the sense that a set-theoretical predicate defines the 'essence' $\bar{F}$ common to all the models. Essential and accidental features are entangled, and indeed can be hard to tell apart, in any particular model O^a, which has its own contingent peculiarities $\{F_1^a, F_2^a, \dots\}$ in addition to the common, essential core $\bar{F}$ determined by the axioms. But once that model is considered alongside others, essences can be made out as what is common to all of them. The abstract test $\bar{F}$, in other words the set-theoretical predicate 'is an $\bar{F}$-test,' therefore gives the *meaning* of the sentence W, which *refers* to any model O^a of the test.

The cleavage between mathematics and physics is largely overcome by such abstract tests, which, being mathematical objects in themselves (despite having physical models), give physics much of the rigid necessity of mathematics. In §7 I consider the differences Duhem attributes to mathematics and physics, in §6 the way Quine links analyticity and 'reductionist' meanings, after a much-needed example.

5 Example: Bell's inequality

If ever a scientific controversy stood sorely in need of experimental arbitration, the dispute over the foundations of quantum mechanics that developed around the positions of Einstein (*e.g.* [22]) and Bohr (*e.g.* [16] or [17]) certainly did (and still does). There have been celebrated efforts to satisfy the need; experiments to test Bell's inequality ([12], [14], and also [5]) by Alain Aspect and others (*e.g.* [8], [9], [10], [11], [18], [40], [50]) have been among the most spectacular and controversial attempts at empirical discrimination. But far from settling the debate they have given it new life and vigour ...

The hope at any rate was this: *Supposez* (to follow Duhem) *que deux hypothèses seulement soient en présence ;*—local realism is either valid or not—*cherchez des conditions expérimentales telles que l'une des hypothèses annonce la production d'un phénomène et l'autre la production d'un phénomène tout différent ;*—Bell's inequality is either satisfied or violated—*réalisez ces conditions et observez ce qui se passe ; selon que vous observerez le premier des phénomènes prévu ou le second, vous condamnerez la seconde hypothèse ou la première ; celle qui ne sera* *pas condamnée sera désormais incontestable ; le débat sera tranché, une vérité nouvelle sera acquise à la Science.*[22] Of course such conclusions are unwarranted, resting on assumptions that may be no less questionable than the principles supposedly refuted. Bell (1986) for instance "always emphasize[d] that the Aspect experiment is too far from the ideal in many ways—counter efficiency is only one of them," and "that there is therefore a big extrapolation from practical present-day experiments to the conclusion that nonlocality holds."

Most attempts to test Bell's inequality, such as those of Aspect *et al.*, have involved photons, but these are seldom detected; this is the issue of "counter efficiency" referred to by Bell. To violate a Bell inequality with photons, assumptions (*i.e.* accidental features F_b^a) like

> Given a pair of photons emerging from two regions of space where two polarizers can be located, the probability of their joint detection from two photomultipliers [. . .] does not depend on the presence and the orientation of the polarizers. [19]

or

> The set of detected pairs with a given orientation of the polarizers is an undistorted representative sample of the set of pairs emitted by the source. [8]

have to be made. For our purposes they are equivalent, and give rise to the same consequences: they multiply the interval figuring in the inequality by the product of the efficiencies of the counters. The assumptions turn an interval running from -1 to 1, for instance, into one running from $-\eta_1\eta_2$ to $\eta_1\eta_2$ where η_1 and η_2 are the efficiencies. If the counters are relatively efficient, and each detect, say, a photon in four, the assumptions make the inequality sixteen times easier to violate.[23] This is the idea: Averaging involves adding up N terms, then dividing by N. But what if most of the terms are 'duds,' and do not contribute to the sum? Surely dividing by N is excessive; does it not make more sense to divide by the number of valid terms instead? In other words only a small fraction of the pairs get detected, so why not take that same fraction of the interval? After all, why should the sample not be representative of the whole population? Surely the photomultipliers act randomly and indiscriminately . . .

[22] [21] p.286. Translation: "Suppose only two hypotheses are at issue; seek experimental conditions such that one of the hypotheses leads to the production of one phenomenon and the other to the production of a completely different phenomenon; realise these conditions and observe what happens; according to whether you observe the first of the predicted phenomena or the second, you will condemn the second hypothesis or the first; the one that will not be condemned will be incontestable; the issue will be settled, and Science will have a new truth."

[23] Franco Selleri expresses this by distinguishing between *strong* and *weak* inequalities, described in [37], [5] and [2].

A sample that is almost the size of the whole population will clearly be very representative, whereas a much smaller sample may or may not be. Consider the assumption:

> For every photon in the state λ the probability of detection with a polarizer placed on its trajectory is less than or equal to the detection probability with the polarizer removed. [18]

The trouble is that the polarizer might *increase* the probability of detection, especially if that probability depends on the state λ, which could be altered by the polarizer. Suppose 'detector' denotes both a vertically aligned polarizer π and a photomultiplier φ behind it. So a 'detection' involves both objects that make up the detector $\pi + \varphi$: a photon is detected when it gets through π *and* makes φ click. As horizontally polarized light will never get detected by $\pi + \varphi$—its probability of detection vanishes—an oblique polarizer placed in front of π *increases* the probability of detection.

So if the experiment produces a number lying outside the narrow interval running from $-\eta_1\eta_2$ to $\eta_1\eta_2$, what is to be concluded?

Uncertainties concerning the particular additional assumptions made vitiate comprehensive statements an experiment may inspire, like "Bell's inequality is violated in nature." Who knows if the outcome really means that—and not the unfoundedness of this or that additional assumption instead. If kaons are used rather than photons, probability of detection, being very high, is no longer the issue; but their instability leads to other assumptions (see [1], [2]) of a completely different sort; and so on. Hence the abstract test, and the corresponding class of structurally equivalent experiments, with a whole range of different auxiliary assumptions: surely they cannot *all* be wrong.

Turning to the abstract test (*cf.* [3], [4]) itself, a *Bell test* will be a scheme

$$(\Xi, \Omega^s(k), \underline{\sigma}^s_n(k), \underline{B}; |\Sigma\rangle, \sigma^s_n, B)$$

satisfying the following axioms:

1. $\Xi = \{(\Omega^1(1), \Omega^2(1)), \ldots, (\Omega^1(N), \Omega^2(N))\}$ is a large ensemble of pairs of objects.
2. Object $\Omega^s(k)$ has an intrinsic property $\underline{\sigma}^s_n(k) = \pm 1$ for every value of $n \in \mathbb{R}$.
3. $\underline{B} = \sum_{k=1}^N \{\underline{\sigma}^1_\alpha(k)\,\underline{\sigma}^2_\beta(k) - \underline{\sigma}^1_\alpha(k)\,\underline{\sigma}^2_{\beta'}(k) + \underline{\sigma}^1_{\alpha'}(k)\,\underline{\sigma}^2_\beta(k) + \underline{\sigma}^1_{\alpha'}(k)\,\underline{\sigma}^2_{\beta'}(k)\}/N$.
4. Ξ is accurately described by the quantum state vector[24]

$$|\Sigma\rangle = \frac{1}{\sqrt{2}}(|+-\rangle - |-+\rangle) \in \mathbb{C}^{2(1)} \otimes \mathbb{C}^{2(2)},$$

[24] The phase difference of π, which may seem an unduly strong requirement, is not the point here.

where the $|\pm\rangle$ are orthonormal, and both Hilbert spaces $\mathbb{C}^{2(s)}$ are two-dimensional.

5. $B = \sigma^1_\alpha \otimes \sigma^2_\beta - \sigma^1_\alpha \otimes \sigma^2_{\beta'} + \sigma^1_{\alpha'} \otimes \sigma^2_{\beta'} + \sigma^1_{\alpha'} \otimes \sigma^1_{\beta'}$, where $\sigma^s_n : \mathbb{C}^{2(s)} \to \mathbb{C}^{2(s)}$ is self-adjoint and unitary, with vanishing trace.
6. Measurement of σ^s_n faithfully reveals property $\underline{\sigma}^s_n(k)$, for all k, n (and both values of s).

The models of the axioms make up the extension of the predicate 'is a Bell test.' Here the *essence*, the *experimental intention* $\bar{F}$ is the abstract Bell test, and a fair sampling assumption like "The set of detected pairs with a given *etc.*" above would be one of the accidents $\{F^a_1, F^a_2, \ldots\}$ of a model O^a.

Leaving aside other difficulties—like the precarious counterfactual thinking required by axiom 6—which would lead us too far astray, the axioms are inconsistent. The notation adopted in axioms 2 and 3, with just a single subscript, tacitly expresses a further axiom, say 7, by suggesting that property $\underline{\sigma}^s_n(k)$ only depends (once k and s have been fixed) on its subscript n, *and not on the subscript of the neighbouring factor*. This allows us to write

$$\underline{B} = \frac{1}{N} \sum_{k=1}^{N} \left[\underline{\sigma}^1_\alpha(k) \{ \underline{\sigma}^2_\beta(k) - \underline{\sigma}^2_{\beta'}(k) \} + \underline{\sigma}^1_{\alpha'}(k) \{ \underline{\sigma}^2_\beta(k) + \underline{\sigma}^2_{\beta'}(k) \} \right],$$

whose modulus cannot exceed 2, for purely arithmetical reasons. But it follows from axioms 4 and 5 that $\max(\langle \Sigma | B | \Sigma \rangle) = 2\sqrt{2}$; from axioms 3, 5, 6 (& 1, 2, 4) that $\langle \Sigma | B | \Sigma \rangle = \underline{B}$; from 4, 5, 6 (& 1, 2, 3) that $\max(\underline{B}) = 2\sqrt{2}$; and from 3, 5, 6, 7 (& 1, 2) that $-2 \leq \langle \Sigma | B | \Sigma \rangle \leq 2$. So we have all sorts of contradictions.

One approach would be to view the inconsistency as expressing the tension at issue, perhaps as representing a corresponding 'inconsistency' of nature itself. Of course if a model is a scheme *satisfying* the axioms, both 'model' and 'satisfaction' have to be understood in appropriately weakened, generalised senses.

The contradictory set has the advantage of allowing us to choose which axiom(s)—2, 4, 6 or 7—to blame, but it nevertheless remains simplest to make the axioms consistent by abandoning an axiom, say 4 or 6. Once consistent the axioms admit normal, classical models, in fact quite a variety of them, involving angles, polarizers and photons; or times and precessions generated by appropriate fields; or kaons and strangeness; and so forth—each with its own peculiar additional assumptions.

6 Quine on meaning, synonymy and analyticity

Let us now return to Quine, who by linking meaning, synonymy and analyticity argues that holism undermines analyticity along with meaning. We have already seen what holism has to do with meaning, and will now consider, with little comment, how Quine associates meaning, synonymy and analyticity. In "Two dogmas" (p.22) he explicitly connects all three:

> Once the theory of meaning is sharply separated from the theory of reference, it is a short step to recognizing as the primary business of the theory of meaning simply the synonymy of linguistic forms and the analyticity of statements [...].

Fifteen pages on: "The verification theory of meaning [...] is that the meaning of a statement is the method of empirically confirming or infirming it," so that "[...] what the verification theory says is that statements are synonymous if and only if they are alike in point of method of empirical confirmation or infirmation"; meaning and synonymy are thus brought together through verificationist "reductionism." Reductionism also yields analyticity: "So, if the verification theory can be accepted as an adequate account of statement synonymy, the notion of analyticity is saved after all" ([42] p.38). Analyticity and synonymy are again linked in *Word and object* (p.65):

> [...] synonymy [...] is interdefinable with another elusive notion of intuitive philosophical semantics: that of an *analytic* sentence. [...] The interdefinitions run thus: sentences are synonymous if and only if their biconditional (formed by joining them with 'if and only if') is analytic, and a sentence is analytic if and only if synonymous with self-conditionals ('If p then p').

But again, this is not the place to dispute Quine's association of meaning, synonymy and analyticity, which will be taken for granted.

To understand whether holism really has conflicting implications for Duhem and for Quine, let us now see how Duhem relates the impossibility of crucial experiments to the 'cleavage' separating mathematics and physics.

7 Duhem on mathematics, physics and crucial experiments

Whereas Quine rejects the "cleavage between analytic and synthetic truths" (dogma1) along with "reductionism" (dogma2), Duhem's argument (against dogma2) *turns* (dogma1 $\Rightarrow \neg$ dogma2 ?) on a similar cleavage (dogma1?): over and over he emphasises the troublesome 'synthetic' character of physics by contrasting it with the clean necessity of mathematics (*cf.* [39] p.109-11)—in which analytic truths can be claimed to figure conspicuously, indeed paradigmatically.[25]

Experimental refutation is often taken to be just like *reductio ad absurdum*:

[25] Until the difficulties and paradoxes that arose around the beginning of the twentieth century, mathematics was a paradigm of necessity. See [31], for instance, on the certainties of geometry: "Unter allen Zweigen menschlicher Wissenschaft gibt es keine [...] von deren vernichtender Aegis Widerspruch und Zweifel so wenig ihre Augen aufzuschlagen wagten. Dabei fällt ihr in keiner Weise die mühsame und langwierige Aufgabe zu, Erfahrungsthatsachen sammeln zu müssen, wie es die Naturwissenschaften im engeren Sinne zu thun haben, sondern die ausschliessliche Form ihres wissenschaftlichen Verfahrens ist die Deduktion. Schluss wird aus Schluss entwickelt ..."

> La réduction à l'absurde, qui semble n'être qu'un moyen de réfutation, peut devenir une méthode de démonstration ; pour démontrer qu'une proposition est vraie, il suffit d'acculer à une conséquence absurde celui qui admettrait la proposition contradictoire de celle-là ; on sait quel parti les géomètres grecs ont tiré de ce mode de démonstration. Ceux qui assimilent la contradiction expérimentale à la réduction à l'absurde pensent qu'on peut, en Physique, user d'un argument semblable à celui dont Euclide a fait un si fréquent usage en Géométrie.[26]

A few pages on Duhem points out that—quite apart from the rôles and validity of other assumptions—the *tertium non datur* usually assumed in mathematics does not hold in physics, where statements can be negated in many different ways:

> Mais admettons, pour un instant, que, dans chacun de ces systèmes, tout soit forcé, tout soit nécessaire de nécessité logique, sauf une seule hypothèse ; admettons, par conséquent, que les faits, en condamnant l'un des deux systèmes, condamnent à coup sûr la seule supposition douteuse qu'il renferme. En résulte-t-il qu'on puisse trouver dans l'*experimentum crucis* un procédé irréfutable pour transformer en vérité démontrée l'une des deux hypothèses en présence, de même que la réduction à l'absurde d'une proposition géométrique confère la certitude à la proposition contradictoire ? Entre deux théorèmes de Géométrie qui sont contradictoires entre eux, il n'y a pas place pour un troisième jugement ; si l'un est faux, l'autre est nécessairement vrai. Deux hypothèses de Physique constituent-elles jamais un dilemme aussi rigoureux ? Oserons-nous jamais affirmer qu'aucune autre hypothèse n'est imaginable ?[27]

[26][21] p.285. Translation: "*Reductio ad absurdum*, which only appears to be a way of refuting, can become a method of demonstration; to demonstrate that a proposition is true, it is enough to push him who would assume the contrary proposition back to an absurd consequence; one knows what use the Greek geometers made of this mode of demonstration. Those who associate experimental contradiction with *reductio ad absurdum* think that one can, in physics, use an argument similar to the one Euclid used so often in geometry." Also p.280: "Un pareil mode de démonstration semble aussi convaincant, aussi irréfutable que la réduction à l'absurde usuelle aux géomètres ; c'est, du reste, sur la réduction à l'absurde que cette démonstration est calquée, la contradiction expérimentale jouant dans l'une le rôle que la contradiction logique joue dans l'autre."

[27]P.288. Translation : "But let us assume, for a moment, that, in each of these systems, all is forced, all is necessary of logical necessity, except a single hypothesis ; let us assume, as a consequence, that the facts, by condemning one of the two systems, condemn with certainty the only doubtful supposition it contains. Does it follow that one can find in the *experimentum crucis* an irrefutable procedure to transform one of the two hypotheses at issue into a demonstrated truth, in the same way that the *reductio ad absurdum* of a geometrical proposition confers certainty on the contradictory proposition ? Between two theorems of geometry that contradict one another, there is no room for a third judgement ; if one is false, the other is necessarily true. Do two hypotheses of physics ever constitute so rigorous a dilemma ? Would we ever dare to claim that no other hypothesis can be imagined ?"

Not only does *tertium non datur* not hold in physics, the possibilities of negation are limitless: the negation $\neg H$ of hypothesis H can suggest, say, another hypothesis $H' = \neg H$; but why not some other $H'' = \neg H$ or $H''' = \neg H$ or who knows what else. So even if it were possible to refute a hypothesis in physics, its refutation would certainly not lead to the confirmation of another hypothesis—whereas the rejection of a hypothesis in mathematics typically allows a single, definite conclusion to be reached.

> La contradiction expérimentale n'a pas, comme la réduction à l'absurde employée par les géomètres, le pouvoir de transformer une hypothèse physique en une vérité incontestable ; pour le lui conférer, il faudrait énumerer complètement les diverses hypothèses auxquelles un groupe déterminé de phénomènes peut donner lieu ; or, le physicien n'est jamais sur d'avoir épuisé toutes les suppositions imaginables ; la vérité d'une théorie physique ne se décide pas à croix ou pile.

So Duhem's rejection ($\neg$ dogma2) of crucial experiments turns on a 'cleavage' which resembles the one (dogma1) repudiated in "Two dogmas," where it is claimed the dogmas are "two sides of a single dubious coin" (dogma1 $\Leftrightarrow$ dogma2).

Since the holism Duhem dwells on in Ch. VI §II (*Qu'une expérience en Physique ne peut jamais condamner une hypothèse isolée, mais seulement tout un ensemble théorique*) appears to be largely responsible for the cleavage invoked repeatedly in the following section, §III (*L'*experimentum crucis *est impossible en physique*), it could seem that overcoming holism would undermine that cleavage. This brings us to the difficulty raised at the beginning: that holism appears to have conflicting implications for Duhem and for Quine. In this connection let us briefly consider relations between Duhem's §II and §III (Ch. VI).

One relation is immediate succession—§III comes right after §II; another is that both are about crucial experiments. §II explains how holism prevents experiments from being crucial, the next section directly relates the impossibility of crucial experiments to the cleavage dividing physics and mathematics; one almost sees a simple syllogism:

II *Holism prevents experiments from being crucial.*

III *The impossibility of crucial experiments makes physics unlike mathematics.*

$\therefore$ *Holism makes physics unlike mathematics.*

The trouble is that the differences between physics and mathematics are only partly due to holism; single-valued, invertible negation,[28] for instance, which holds in mathematics but not in physics according to Duhem, has little to do with holism.

[28] One can write $\neg(\neg H) = H$.

Holism, which for Quine undermines meaning and hence analyticity, is therefore not entirely responsible for the cleavage repeatedly invoked by Duhem in his rejection of crucial experiments.

It must also be said that mathematics may not be as analytic as I have taken it to be; Kant and others have regarded much of it as synthetic. Kant (1787 B190) defines the synthetic in terms of the principle of contradiction:

> Der Satz nun: Keinem Dinge kommt ein Prädikat zu, welches ihm widerspricht, heißt der Satz des Widerspruchs [...]. Denn, wenn *das Urteil analytisch ist*, es mag nun verneinend oder bejahend sein, so muß dessen Wahrheit jederzeit nach dem Satze des Widerspruchs hinreichend können erkannt werden.[29]

And Poincaré ([41] Ch. I) writes that the *règle du raisonnement par récurrence*—which he considers the *raisonnement mathématique par excellence*—is *irréductible* (involving infinitely many syllogisms) *au principe de contradiction*, and hence is the *véritable type du jugement synthétique a priori*. Crowe [20] argues that mathematics shares many of the difficulties attributed to physics in *La théorie physique*, and that Duhem attaches such weight to the distinctions of §III out of ignorance that mathematics is not so certain and 'analytic' after all. Physics, by becoming more and more detached from the world, seems moreover to be losing its synthetic character, and may have begun decades ago. The association of mathematics with the analytic, physics with the synthetic, could therefore be less straightforward than I have made it out to be. But again, definite resolution has not been my purpose; I have rather tried to explore the web of issues involved, and view in a fresh—perhaps questionable—light.

8 Final remarks

Troubling shades of grey have prevailed in these pages over the reassuring certainties of black and white. I have often spoken of degree and nuance, of more and less, rather than of *sic et non*, of true and false: holism is undermined but not completely eradicated, meaning acquires much definiteness, analyticity is recovered to the extent that holism is overcome and so on. But isn't Quine's point that analytic and synthetic differ in degree and not in kind?

The gains in cruciality and analyticity with respect to the concerns of Duhem and Quine may be a matter of degree, but that degree seems considerable, perhaps considerable enough to warrant representation as promotions 'in kind.' Indeed it can be misleading not to view certain differences in degree as differences in kind—and hence, for instance, not to call the unlikeliest events 'impossible,' to

[29]Translation: "Now the statement that nothing can have a predicate which contradicts it, is called the principle of contradiction [...]. For if *the judgement is analytic*—be it negative or affirmative—its truth must always be adequately recognised by means of the principle of contradiction."

distinguish clearly from those that are only moderately unlikely. Nuances within a small enough 'margin of discrimination' can be safely ignored.

As the distances from the ideal Platonic limits of absolute cruciality and analyticity can be made logically, conceptually negligible by a proper extrication of essences from accidents, the contested notions can reasonably be countenanced.

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