DUHEM, QUINE AND THE OTHER DOGMA

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ABSTRACT: By linking meaning and analyticity (through synonymy), Quine rejects both "dogmas of empiricism" together, as "two sides of a single dubious coin." His rejection of the second ("reductionism") has been associated with Duhem's argument against crucial experiments—which relies on fundamental differences, brought up again and again, between mathematics and physics. The other dogma rejected by Quine is the "cleavage between analytic and synthetic truths"; but aren't the truths of mathematics analytic, those of physics synthetic? Exploiting Quine's association of essences, meaning, synonymy and analyticity, and appealing to a 'model-theoretical' notion of *abstract test* derived from Duhem and Quine—which can be used to overcome their holism by separating essences from accidents—I reconsider the 'crucial experiment,' the aforementioned "cleavage," and the differences Duhem attributed to mathematics and physics; and propose a characterisation of the meaning and reference of sentences, which extends, in a natural way, the distinction as it applies to words.

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 many 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] that he came to be associated with Duhem. But there is also the *first*, the "cleavage between analytic and synthetic truths."[4] Quine claims they are equivalent, indeed "two sides of the same dubious coin," and contests both together. Duhem on the other hand argues that experiments cannot be crucial because physics is so *different* from mathematics. But surely the truths of physics are synthetic, those of mathematics analytic. How then can the 'Duhem-Quine thesis' depend on the difference between mathematics and physics, and at the same time blur the distinction between analytic and synthetic?

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[1] On this resemblance, as recognised by Quine, see the footnote on p.41 of Quine (1953), footnote 7 on p.67 of Quine (1960) and the very beginning of Quine (1986).

[2] Krips (1982), Ariew (1984), Quine (1986) and Vuillemin (1986) have pointed out several. Too many according to Needham (2000), 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 (1953 p.20) puts it.

[4] Quine's rejection of it has met with much disapproval; see for instance Mates (1951), Strawson (1955), Grice and Strawson (1956), Katz (1967,1974), Boghossian (1996).

A kind of holism[5]—an inextricability of essences and accidents,[6] of essential experimental 'intention' (or 'intension'[7]) and 'accidental' auxiliary assumptions—is the main obstacle to crucial experiments and (empirically grounded) meanings. Using notions hinted at by Duhem and Quine, formalised with the resources of set-theoretical axiomatisation, I argue that such holism and inextricability can be largely overcome.[8] 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 holism and entanglement of essences and accidents are 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 making them *more* and not less similar. The matter is brought up, not for definite resolution, but to shed light on the web of issues involved, including the relations between the arguments of Duhem and Quine.

I begin, in §2, with a general strategy for overcoming holism, which leads, in §3, to a new characterisation of the meaning and reference of sentences. In §4 I argue that the strategy—of 'abstract tests,' as I call them—is already hinted at by Duhem and Quine themselves, and show how such tests can be formalised in the language of model theory, in fact of set-theoretical axiomatisation; the requirement of unanimity, and the independence and prior plausibility of auxiliary assumptions, are also discussed. A quantum-mechanical example is looked at in §5. In §6 I consider how Quine relates meaning, essence and analyticity. Given those relations, analyticity is recovered to the extent that holism is overcome—whereas undermining Duhem's holism would appear to bring physics *closer* to mathematics, as I argue in §7. Final remarks on a couple of loose ends are in §8.

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[5] For a detailed analysis of various kinds of holism see Esfeld (2002).

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

[7] Or 'intension.' The two words seem, much as their spellings, to be closely related—here at any rate. Rather than draw subtle distinctions that may be hard to maintain I will always write "intention."

[8] 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" (Glymour 1975 p.426). See also Grünbaum (1960,1962)—Quine replies in (1962), Laudan defends Duhem in (1965), claiming that Grünbaum has attacked too strong a version of "the Duhemian argument"—and Glymour (1980).

2. Essences, accidents and holism

"The Aristotelian notion of essence," writes Quine (1953 p.22), "was the forerunner, no doubt, of the modern notion of intension or 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 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 of the object, and call that rest *accidental*. Perhaps an essence is best viewed as being 'embodied' in an object, which acts as a physical support by 'carrying' or 'bearing' it, along with unintended accidental features.

A given meaning, then, breaks an object up into essential and accidental features, the accidental ones 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 not essential.

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, say $\overline{F}$, are really meant by W. Even if it is clear that $\overline{F}$ is a proper subset of F, it may be less clear exactly which elements of F make it up. Hence the following test: remove the features F 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. Sometimes the test would work smoothly, and be conclusive in the end, by determining which features are meant. But 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 distinguish between the three possible cases $(1.\ F_m \in \overline{F}, F_n \notin \overline{F};\quad 2.\ F_m \notin \overline{F}, F_n \in \overline{F};\ 3.\ F_m \in \overline{F}, F_n \in \overline{F})$ 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 in the

[9] Here an 'object' will be taken to be a *physical* object—even if mathematical objects have been considered ever since the early days of the meaning-reference distinction; see Frege (1892).

operationalist spirit of empiricism. This inextricability of essence and accident already adumbrates the holisms of Duhem and Quine.

For single objects the problem can be insurmountable. But even if the essential features $\overline{F}$ cannot exist on their own, without accidental features of some kind or other, they may be found with *different* sets of accidental features; in other words $\overline{F}$ may be accompanied by the accidents $F_1^1, F_2^1, \ldots$ or by $F_1^2, F_2^2, \ldots$ or $F_1^3, F_2^3, \ldots$ and so on, in which case W, while *meaning* $\overline{F}$, would *refer* to object O^1 with features $F^1 = \{\overline{F}, F_1^1, F_2^1, \ldots\}$ or to object O^2 with features $F^2 = \{\overline{F}, F_1^2, F_2^2, \ldots\}$ *etc*. Even without knowing the exact makeup of $\overline{F}$ beforehand, it is clearly a subset of $\hat{F} = \bigcap_i F^i$; and if the family of objects $O^1, O^2, \ldots$ is sufficiently large and the accidental features sufficiently varied, one can reasonably *identify* $\overline{F}$ 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 the essences cannot be physically abstracted from the bearing object, with all its accidents, they can be abstracted from *particular* accidents (rather than others); for the distinguished features $\overline{F}$ 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 a 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 auxiliary, 'collateral'[10] experimental features corresponding to assumptions one may even call 'accidental.' So we again have a holistic problem, of entanglement; an entanglement of ideal experimental *essence* or *intention* with unavoidable experimental *accidents* needed to connect that intention with 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 our treatment of physical objects, resources from model theory will be used to effect the separation for experiments.

[10] Indeed one is reminded of the "collateral information" of Quine (1960), esp. §§9,10.

Frege (1879) famously extended his *Sinn-Bedeutung* distinction from words to *Sätze*: "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?"[11] 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?"[12] 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."[13] But this seems an unnatural extension—however justified within his scheme—of the nomenclature (*Sinn, Bedeutung*) first adopted for words.

Attempting, then, a natural extension of the meaning-reference distinction from words to sentences, I claim that a single experiment O provides not the *meaning* of a sentence W—for the reasons Quine gives—but something more like its 'reference.' I propose to say that a sentence *refers* to a particular experiment O^1 with features $F^1 = \{\overline{F}, F_1^1, F_2^1, \ldots\}$ or to an experiment O^2 with features $F^2 = \{\overline{F}, F_1^2, F_2^2, \ldots\}$ or to O^3 *etc*., and that its *meaning* is given by the subset $\overline{F}$ of $\hat{F} = \bigcap_i F^i$ that corresponds to W by expressing an ideal experimental *intention*, an abstract logical core—where it is up to the ingenuity of the experimenters to reduce $\hat{F}$ to $\overline{F}$ by producing enough experiments, with sufficiently varied auxiliary assumptions. Or rather the experimenters begin with an idea of the experimental intention $\overline{F}$ expressing W, and then go about finding different ways to implement it physically. The trouble is that $\overline{F}$ is a tenuous, ideal object, which cannot be carried out on its own; its implementation requires auxiliary features of some kind or other to connect it with the world. Quine's point is roughly that W cannot be determined empirically because $\overline{F}$ cannot be performed alone, without 'accidental' auxiliary features.

The experiments $O^1, O^2, \ldots$ could agree or disagree. Disagreement complicates matters; for then which experiments are to be trusted? Would the majority, or perhaps some privileged experiment or subclass, necessarily be right? To avoid such

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[11] P.32. Translation: "We now wonder about the meaning and reference of a whole statement. Such a sentence contains a thought. Is this thought to be viewed as its meaning or as its reference?"

[12] 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?"

[13] 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."

complications unanimity will be required: the experiments must all yield the same verdict.[14] 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[15]—and prior plausibility is inherited from other contexts. I will assume in other words that the validity of the auxiliary assumptions $\{F_1^1, F_2^1, \ldots\}$, $\{F_1^2, F_2^2, \ldots\}, \ldots$ made in the (unanimous) experiments $O^1, O^2, \ldots$ was established in many other experimental contexts; and furthermore that validity so established is maintained in the particular experimental context O^a. The unanimity of the verdict cannot then be reasonably attributed to a conspiracy of the assumptions or theories peripheral to each experiment, extraneous to the core structure $\overline{F}$; it must be due to that common structure itself.

Another approach, adopted by Grice and Strawson (1956) in response to Quine, is to deal with the troublesome auxiliary statements F_b^a by assigning truth-values to them: "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 the statements can be entangled in such a way as to make it hard, perhaps impossible, to test and determine them independently—this is precisely the holism at issue; so one can wonder about the legitimacy of a prior assignment of *individual* truth-values. 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

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

[15] As has been pointed out to me by John Earman and John Norton. I hesitate to reformulate the issues here explicitly in terms of *probabilities*, which I have deliberately avoided.

> 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.[16]

An *expérience* here is not an individual 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 implementations in the same way a *theory* (in the Tarskian sense) can be identified with all its *models*. The accidental 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.[17]

Both Duhem and Quine have in mind an abstract test—an abstract *expérience*, a universal—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*[18] of some sort; it is in that sense that they all test the same thing. But then what "structure" is has to be elucidated. The ordinary connotations of the word are hardly enough; 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,[19] seem appropriate and will be used. In his language a set-theoretical predicate

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[16] P.224; emphasis mine. Quine may be in dispute, but not the indeterminacy of translation (Quine 1960, esp. §§12-16), in acceptance of which Duhem has been left in French. 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." (The translations are mine.)

[17] P.34. Quine argues, especially in *Word and object* §§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.

[18] In the logical literature "structure" is often a synonym of "model." Here its meaning is closer to that of "theory."

[19] Suppes (2002), for instance.

defines a *theory*, whose realisations are *models*, whereas here the predicate will characterise an *abstract test*, whose particular implementations are again *models*. It is the abstract test, rather than this or that 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 of models, and to have a validity acquired in other contexts.

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

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

$\vdots$

are satisfied. Any scheme $(A, B, \ldots)$ satisfying the axioms is a *model*. The extension of the predicate 'is an X' is the set of models.

We are again dealing with essences and accidents, in the sense that a theory or abstract test abstracts an essence out of the set of its models. Essential and accidental features are entangled, and indeed can be hard to tell apart, in any particular model, which will have its own accidental peculiarities in addition to the common, essential features singled out by the axioms. But once that model is considered alongside others, essences can be discerned as what is common to all of them (or alternatively can be explicitly identified with the axiomatic expression of the theory or abstract test satisfied by those models).[20]

Duhem's misgivings are largely answered 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 we will return to the differences Duhem attributed to physics and mathematics, and in §6 to Quine's association of holism, meaning and analyticity, after a much-needed example.

[20] For another account of relations between a fundamental ideal scheme (here the abstract test, there the fundamental laws) and particular ways (here the various models, there the phenomenological laws) of adapting it, or rather implementing it in the world, see Cartwright (1983). Here the ideal scheme serves to discount the accidents and is primary, there the accidents are.

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[21] and Bohr[22] certainly did (and still does). There have been celebrated efforts to satisfy the need; experiments to test Bell's inequality[23] by Alain Aspect and others[24] 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 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.*[25] 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 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"[26] or "The set of detected pairs with a

[21] See for instance Einstein *et al*. (1935).
[22] See for instance Bohr (1935a, 1935b).
[23] See Bell (1965, 1987), and also Afriat and Selleri (1998).
[24] *E.g.* Aspect *et al.* (1981, 1982a, 1982b), Clauser and Horne (1974), Perrie *et al.* (1985), Walther and Fry (1997).
[25] Duhem (1989) 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."
[26] Clauser *et al*. (1969).

given orientation of the polarizers is an undistorted representative sample of the set of pairs emitted by the source"[27] 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.[28] 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…

A sample that is almost the size of the whole population will clearly be very representative; 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."[29] 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

[27] Aspect (1983).

[28] Franco Selleri expresses this by distinguishing between *strong* and *weak* inequalities, described in Lepore and Selleri (1990), Afriat and Selleri (1998) and Afriat (2001).

[29] Clauser and Horne (1974).

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[30] 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[31] itself, a *Bell test* will be a scheme

$$\left(\Xi, \mathfrak{O}^s(k), \underline{\sigma}_n^s(k), \underline{B}; |\Sigma\rangle, \sigma_n^s, B\right)$$

satisfying the following axioms:

1. $\Xi = \left\{\left(\mathfrak{O}^1(1), \mathfrak{O}^2(1)\right), \ldots, \left(\mathfrak{O}^1(N), \mathfrak{O}^2(N)\right)\right\}$ is a large ensemble of pairs of objects.
2. Object $\mathfrak{O}^s(k)$ has an intrinsic property $\underline{\sigma}_n^s(k) = \pm 1$ for every value of $n \in \mathbb{R}$.
3. $\underline{B} = \dfrac{1}{N}\sum_{k=1}^{N}\left[\underline{\sigma}_\alpha^1(k)\underline{\sigma}_\beta^2(k) - \underline{\sigma}_\alpha^1(k)\underline{\sigma}_{\beta'}^2(k) + \underline{\sigma}_{\alpha'}^1(k)\underline{\sigma}_\beta^2(k) + \underline{\sigma}_{\alpha'}^1(k)\underline{\sigma}_{\beta'}^2(k)\right].$
4. Ξ is accurately described by the quantum state vector[32]
$$|\Sigma\rangle = \frac{1}{\sqrt{2}}\left(|+-\rangle - |-+\rangle\right) \in \mathbb{C}^{2(1)} \otimes \mathbb{C}^{2(2)},$$
where the $|\pm\rangle$ are orthonormal, and both Hilbert spaces $\mathbb{C}^{2(s)}$ are two-dimensional.
5. $B = \sigma_\alpha^1 \otimes \sigma_\beta^2 - \sigma_\alpha^1 \otimes \sigma_{\beta'}^2 + \sigma_{\alpha'}^1 \otimes \sigma_\beta^2 + \sigma_{\alpha'}^1 \otimes \sigma_{\beta'}^2,$ where $\sigma_n^s : \mathbb{C}^{2(s)} \to \mathbb{C}^{2(s)}$ is self-adjoint and unitary, with vanishing trace.
6. Measurement of σ_n^s faithfully reveals property $\underline{\sigma}_n^s(k),$ for all $k, n.$

The models of the axioms make up the extension of the predicate ‘is a Bell test.’

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 $\underline{\sigma}_n^s(k)$ 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

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

whose modulus cannot exceed 2, for purely arithmetical reasons. But it follows from axioms 4 and 5 that $\max\left(\langle\Sigma|B|\Sigma\rangle\right) = 2\sqrt{2};$ from axioms 3, 5, 6 (& 1, 2, 4) that

[30] See Afriat (2000, 2001).
[31] *Cf.* Afriat (2003a, 2003b).
[32] The phase difference (here π) could be different, but is not really the point.

$\langle \Sigma | B | \Sigma \rangle = \underline{B};$ 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 empirically grounded meaning, and will now consider, with little comment, what Quine has to say about the association of 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 […].

A few pages on (p.37): "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"[33]; 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."[34] 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

[33] *ibid.* p.37
[34] *ibid.* p.38

> '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 go into Quine's association of meaning, synonymy and analyticity in any detail; it will be taken for granted.

7. Duhem on mathematics, physics and crucial experiments

Whereas Quine questions the analytic-synthetic distinction, Duhem's corresponding argument *turns* on a very similar distinction: over and over he emphasises the troublesome 'synthetic' character of physics by contrasting it with the clean necessity of mathematics[35]—in which analytic truths are held to figure conspicuously, indeed paradigmatically.[36]

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

> 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.[37]

A few pages later 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

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[35] *Cf.* Needham (2000) p.109-11.

[36] Until the difficulties and paradoxes that arose around the beginning of the twentieth century, mathematics was a paradigm of necessity. See Helmholtz (1870), 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 …"

[37] Duhem (1989) 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."

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 ?[38]

Not only does *tertium non datur* not hold in physics, the possibilities of negation are limitless: $\neg H$ can suggest, say, some H'; but it could also mean H'' or H''' or who knows what else. So even if it were possible to refute a hypothesis in physics, its refutation would certainly not bring with it the confirmation of some other hypothesis—whereas the rejection of a hypothesis in mathematics typically allows a very precise 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.[39]

So Duhem's discussion of crucial experiments turns[40] on a distinction which is at least very similar to the one disputed by Quine, indeed perhaps on an acceptance of the first dogma.

Since the holism Duhem describes 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 differences between mathematics and physics that are so central to the following section, §III (*L'*experimentum crucis *est impossible en physique*), it would seem that overcoming holism would diminish those differences. This brings us to the difficulty raised at the beginning: that holism appears

[38] 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?"

[39] Translation: "Experimental contradiction does not, unlike the *reductio ad absurdum* employed by geometers, have the power to turn a physical hypothesis into an incontestable truth; to do so, one would have to enumerate completely the various hypotheses to which a certain group of phenomena can give rise; but the physicist is never sure he has exhausted all imaginable suppositions; the truth of a physical theory is not settled by heads or tails."

[40] Only because Duhem was unaware that mathematics may not be so certain and 'analytic' after all, according to Crowe (1990), who argues that it shares many of the difficulties attributed to physics in *La théorie physique*.

to have very different, perhaps opposite, implications for Duhem and for Quine. So let us briefly turn to relations between Duhem's §II and §III (Ch. VI).

One relation is immediate proximity—§III clearly comes right after §II; another is that both sections are about crucial experiments. §II explains how holism prevents experiments from being crucial, the next section directly relates the impossibility of crucial experiments to differences between mathematics and physics. 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; $\neg(\neg H) = H$,[41] for instance, which typically holds in mathematics but not in physics, has little to do with holism.

8. Final remarks

Of course mathematics may not be as analytic as I have taken it to be; Kant and others have regarded much or all of it as synthetic. And physics, by becoming more and more detached from the world, may be losing its synthetic character. So the association of mathematics with the analytic, physics with the synthetic, may not be as straightforward as I have made it out to be. But this is not the place to elaborate, nor even to propose a definition of the analytic and the synthetic.

A final matter is the prevalence in my arguments of shades of grey over black and white; I have often spoken of degree, of more and less, rather than of *sic et non*, of true and false: holism is undermined, meaning acquires much definiteness, analyticity is recovered to the extent that holism is overcome and so on. The gains in 'cruciality,' definiteness of meaning, analyticity *etc*. with respect to the concerns of Duhem and Quine are admittedly a matter of degree, but that degree seems considerable, perhaps considerable enough to warrant representation as differences in kind.[42] So I claim it is not wrong, if essences are separated from accidents in the way I propose, to

[41] The possibility of 'multi-valued' or 'non-invertible' negation in physics is closely related, perhaps even equivalent, to this.

[42] It can be misleading not to represent certain differences in degree as differences in kind—and hence, for instance, not to call the unlikeliest events 'impossible,' to differentiate clearly from those that are only moderately unlikely.

countenance *crucial experiments*, certain empirically grounded *meanings*, and perhaps even *analyticity*.

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