DUHEM, QUINE AND THE OTHER DOGMA

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Introduction

A resemblance[1] between positions held by Duhem and Quine has led to the conjunction of their names: one speaks of "Duhem-Quine"[2] (or the other way around). Whether the affinity—amid many differences[3] of period, provenance, profession, subject-matter, style and generality—is enough to justify enduring matrimony is debatable, but not the issue here.[4] Quine's position is famously expressed in "Two dogmas of empiricism"; it was by disputing the second[5] that he wound up in the company of Duhem. But there is also the *first*, the analytic-synthetic distinction;[6] Quine claims they are the same, and indeed contests both together.

With resources hinted at in different ways by both Duhem and Quine, it will be argued that some of their misgivings about empirical confirmation, or crucial experiments, may be exaggerated[7] or unfounded; and that such experiments, suitably conceived, can give good meaning to empirical sentences. With appropriate meanings one can then wonder about synonymy and analyticity.

Crucial experiments will be taken to be not only those that discriminate between theories, but also those that test a single theory or statement. *Absolute* cruciality is an unattainable Platonic limit; but the approach proposed here allows us to come close enough to speak of "crucial experiments" *tout court*—rather than just, say, "very crucial experiments." For it can be misleading not to represent significant 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).

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[1] On this resemblance, as recognized 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] The alliance has had considerable success, and produced much debate, comment and literature. And a new holistic zeal has taken views surprisingly ascribed to our authors to remarkable extremes.

[3] 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.

[4] Attention to both authors only means that, for all their differences, it is felt they can be fruitfully compared, and should not be taken as a charge of plagiarism. Quine's originality is not in question.

[5] 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.

[6] 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).

[7] Similar claims, it must be said, 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).

A general theory of meaning is not being proposed. The discussion only concerns certain empirical meanings, and has special reference to physical experiments. Generalization is possible, in various directions, but will not be attempted.

Duhem on mathematics, physics and crucial experiments

Whereas Quine's argument in "Two dogmas" is meant to undermine the analytic-synthetic distinction, Duhem's corresponding argument *turns* on a very similar distinction: over and over he emphasizes the troublesome 'synthetic' character of physics by contrasting it with the clean necessity of mathematics[8]—in which analytic truths are generally held to figure conspicuously, indeed paradigmatically.[9]

> 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.
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> 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.[10]

Needless to say Duhem then explains how mistaken they are. A few pages later:

> 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.[11]

But of course physics is not so straightforward. Further on:

> 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

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

[9] Until the difficulties and paradoxes that arose around the turn of the century (and then into the twentieth), 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 ..."

[10] Duhem (1989) p.285. 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." Quine may be in question, but not the indeterminacy of translation (Quine 1960, esp. §§12-16), in acceptance of which Duhem has been left in French.

[11] P.288.

épuisé toutes les suppositions imaginables ; la vérité d'une théorie physique ne se décide pas à croix ou pile. [11]

So Duhem's discussion of crucial experiments turns[12] on a distinction which is at least very similar to the one disputed by Quine, indeed perhaps on an acceptance of the first dogma.

Abstract tests

The distinction drawn between mathematics and physics seems to rest in large measure on the peculiarities and contingencies of the *particular* experiment; but experiment of a more abstract kind can also be considered:

> 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.[13]

An *expérience* here is not an individual real experiment, subject to the difficulties Duhem will raise later, in II.VI, 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 Platonic idea can be identified with all its realizations, or a theory with its models.

There is a similar notion in *Word and object*: "We may begin by defining the *affirmative stimulus meaning* of a sentence […] as the class of all the stimulations […] that would prompt […] assent."[14] A couple of pages on:

> Yet 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.[15]

Both Duhem and Quine have in mind an abstract test—an 'abstract' *expérience*, a universal—with many particular realizations. It is in such tests that the desired cruciality will be sought.

One can first wonder about appropriate formalization, for the notion is nebulous and of little use as it stands. What the various realizations of an abstract test have in common is *structure* of some sort; it is in that sense that they all test the same thing. But "structure"

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[12] 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*.

[13] P.224; emphasis mine.

[14] Quine (1960) p.32.

[15] 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 characterization of stimulus meaning is serving only as a hint and not as a foundation for meaning.

also stands in need of elucidation, for the ordinary connotations of the word will hardly do (Duhem and Quine, who speak of *form*, do no better). Specification of a means of description can clarify: of the many available ways of describing structure, the resources of set-theoretical axiomatization, associated chiefly with Patrick Suppes,[16] will be used. In his language a set-theoretical predicate defines a *theory*, whose realizations are *models*, whereas here the predicate will identify an *abstract test* or *experimental structure*, whose particular implementations are again *models*.

Of course no recipe or algorithm is enough on its own to assign an abstract test to an empirical sentence, blindly and mechanically; judgement will be needed, no matter how comprehensive and detailed the theoretical prescriptions.[17]

Though the models of an experimental structure can disagree, to simplify *unanimity* will be required throughout, in other words that they all produce the same verdict. It will then be claimed that such a structure (in other words the class of its models) represents a crucial experiment, which can significantly diminish the gap between mathematics and physics to which Duhem repeatedly alludes. Auxiliary assumptions[18] admittedly have to be made in each particular model, indeed to connect the abstract test with the world, but they vary widely over the whole class. The unanimity of a verdict cannot reasonably be attributed to a conspiracy of the assumptions or theories peripheral to each model, extraneous to the core structure; it must be due to that common structure itself.

Grice and Strawson (1956 p.156) write that "two statements are synonymous if and only if any experiences which, *on certain assumptions*[19] *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." Their *assumptions* are replaced here by *quantification*, together with the imposition of *unanimity*: rather than making assumptions as to the truth-values of other statements, it seems preferable to quantify over all "other statements" compatible with the abstract test—in other words over all additional assumptions—and then impose unanimity. This may amount to *a posteriori* inference, rather than *a priori* assumptions, as to the truth values of the other statements.

The separation of *essential* experimental meaning from the *accidental* auxiliary assumptions (or "collateral information") that only confuse matters is analogous to the differentiation of an *essence* expressed by meaning or intension from the *accidental* features also brought in by naming, reference or extension.

> The Aristotelian notion of essence was the forerunner, no doubt, of the modern notion of intension or meaning. For Aristotle it was essential in men to be rational, accidental to be two-legged. But there is an important difference between this attitude and the doctrine of meaning. From the latter point of view it may indeed be conceded (if only for the sake of

[16] Suppes (2002), for instance.

[17] The issue is delicate; indeed Quine (1953 p.38) asks "What [...] is the nature of the relation between a statement and the experiences which contribute to or detract from its confirmation?," and has no simple answer (short of invoking all of science).

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

[19] *Cf.* Duhem (1989) p.281: "Le physicien déclare-t-il que cette erreur est précisément contenue dans la proposition qu'il voulait réfuter et non pas ailleurs ? C'est qu'il admet implicitement l'exactitude de toutes les autres propositions dont il a fait usage ; tant vaut cette confiance, tant vaut sa conclusion."

> argument) that rationality is involved in the meaning of the word 'man' while two-leggedness is not; but two-leggedness may at the same time be viewed as involved in the meaning of 'biped' while rationality is not. Thus from the point of view of the doctrine of meaning it makes no sense to say of the actual individual, who is at once a man and a biped, that his rationality is essential and his two-leggedness accidental or vice-versa. 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.[20]

Meaning is in a sense stronger than reference, and emphasizes a quality or essence in what is referred to; reference just indicates a whole object, including accidental features peripheral to what is really meant. With physical objects the separation of meaning from reference can require a mutilation or violence that would undermine even meaning, thus producing a confusing inextricability.[21] But experiments can be more amenable to the extraction of an essence or meaning from the various possible accidents of physical implementation.

An example will be useful.

The Bell test

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[22] and Bohr[23] certainly did (and still does). There have been celebrated efforts to satisfy the need; experiments to test the Bell inequality[24] by Alain Aspect and others[25] 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.*[26] But 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

[20] Quine (1953) p.22.
[21] "There is no assurance here that the extensional agreement of 'bachelor' and 'unmarried man' rests on meaning rather than merely on accidental matters of fact, as does the extensional agreement of 'creature with a heart' and 'creature with kidneys'" (Quine 1953 p.31).
[22] See for instance Einstein *et al*. (1935).
[23] See for instance Bohr (1935a, 1935b).
[24] See Bell (1965, 1987), and also Afriat and Selleri (1998).
[25] *E.g.* Aspect *et al.* (1981, 1982a, 1982b), Clauser and Horne (1974), Perrie *et al.* (1985), Walther and Fry (1997).
[26] Duhem (1989) p.286.

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"[27] 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"[28] 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.[29]

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 following 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."[30] 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. Take the following example. The term 'detector' will denote both a vertically aligned polarizer π and a photomultiplier φ behind it. A 'detection' therefore 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 experiment really means that—and not the unfoundedness of

[27] Clauser *et al*. (1969).
[28] Aspect (1983).
[29] Franco Selleri expresses this by distinguishing between *strong* and *weak* inequalities, which are described in Selleri and Lepore (1990), Afriat and Selleri (1998) and Afriat (2001).
[30] 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[31] 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 experimental structure[32] itself, a *Bell test* will be a scheme

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

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 $\underline{\sigma}^s_n(k) = \pm 1$ for every value of $n \in \mathbb{R}$.

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

4. Ξ is accurately described by the quantum state vector

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

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

5. $B = \sigma^1_\alpha \otimes \sigma^2_\beta - \sigma^1_\alpha \otimes \sigma^2_{\beta'} + \sigma^1_{\alpha'} \otimes \sigma^2_\beta + \sigma^1_{\alpha'} \otimes \sigma^2_{\beta'}$, 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.

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 $\underline{\sigma}^s_n(k)$ 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

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

whose modulus cannot exceed 2, now for purely arithmetical reasons. But it follows from axioms 4 and 5 (and little else) that $\max\left(\langle\Sigma|\, B\,|\Sigma\rangle\right) = 2\sqrt{2}$; 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 $-2 \le \langle\Sigma|\, B\,|\Sigma\rangle \le 2$. So we have all sorts of contradictions.

One approach would be to view the inconsistency as expressing the tension at issue, and perhaps as representing a corresponding inconsistency of nature itself. Of course if a model

[31] See Afriat (2000, 2001).
[32] *Cf.* Afriat (2003a, 2003b).

is a scheme *satisfying* the axioms, both 'model' and 'satisfaction' have to be understood in appropriately weakened, generalized 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.

Duhem's misgivings are at least partially answered by such abstract tests, which, being mathematical objects in themselves (despite having physical models), allow physics to partake of the rigid necessity of mathematics, of much of it at any rate.

Empirical meaning, synonymy and analyticity

If contingent assumptions, peripheral to the core test, were the principal obstacle to empirical confirmation—and hence to the meanings, synonymy and analyticity[33] that would depend on it—we have proposed a framework that reduces their incidence, and are now in a position to countenance sweeping statements like "nature violates Bell's inequality, no matter how the experiment is performed." An empirical sentence can admittedly base only a limited and precarious meaning on a particular experiment; but an abstract test gives, say, "Bell's inequality is violated in nature," the kind and definiteness of meaning hoped for by the empiricists. And meanings so solidly founded in experiment give rise to an acceptable empirical notion of synonymy, requiring no appeal to semantical rules or linguistic convention: two sentences are synonymous if both base their meanings on the same abstract test.[34] Even if sentences S_1 and S_2 look very different, and appear completely heteronymous at first sight, it could be that they ought both to be associated with the same abstract test T, indeed independently of any *direct linguistic* perception of a synonymy between the sentences; in which case synonymy is perceived only indirectly, *a posteriori*, once it is seen that both sentences have the same test. The assignment of the common test to the two different sentences can be psychologically independent of any prior linguistic intuition regarding the sentences; there is no reason why the common association of S_1 and S_2 with T should be possible only where it is preceded by a linguistic association of the sentences.

With synonymy we have a corresponding analyticity:

> […] 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

[33] "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 […]" (Quine 1953 p.22).

[34] "Then 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" (Quine 1953 p.37). Here something like the "if" is claimed; the "only if" would require further argument.

> analytic, and a sentence is analytic if and only if synonymous with self-conditionals ('If *p* then *p*').[35]

With good meanings, and hence synonymy, Quine would presumably be prepared to concede analyticity.[36]

In "Two dogmas" Quine characterizes analyticity in the following terms as well: "The verification theory of meaning […] is that the meaning of a statement is the method of empirically confirming or infirming it. An analytic statement is that limiting case which is confirmed no matter what."[37] What is to be made of *empirical* confirmation *no matter what* is unclear. "Whatever the world may tell us" or "whatever the experimental verdict" seem a little extreme, no matter how drastic the adjustments Quine claims he is prepared to make. In the scheme proposed here "no matter what" has a natural interpretation, and can without undue distortion be taken to mean *however the experimental structure gets realized*, or *whatever the auxiliary assumptions*. An abstract test is meaningful, and gives rise to synonymy and analyticity, if it produces the same verdict *come what may*, in other words in all its physical implementations.

Final remarks

A sentence, for Quine, can only be as analytic as the meanings involved are determinate. But he would presumably allow that it can be *just as* analytic as the meanings are determinate. And we have proposed an empirical analyticity based on very determinate meanings founded in abstract test.

Analytic sentences are, for some philosophers, those that are uncontaminated by the world and all its contingencies. Quine tells us that *no* sentence is altogether immune to such contamination. Perhaps; but nature's pronoucements can be purged of their contingent impurities by an appropriate abstraction and quantification, leaving empirical verdicts with much of the necessity and certainty of those of mathematics.

References

Afriat, A. (2000) "Kaons and Bell's inequality" in *The foundations of quantum mechanics – historical analysis and open questions*, edited by C. Garola and A. Rossi, Singapore, World Scientific

[35] Quine (1960) p.65. And Quine (1953): "The characteristic of [an analytic statement of the 'second class'] is that it can be turned into a logical truth by putting synonyms for synonyms […]" p.23; "Singular terms may be said to be cognitively synonymous when the statement of identity formed by putting '=' between them is analytic. Statements may be said simply to be cognitively synonymous when their biconditional (the result of joining them by 'if and only if') is analytic. […] we can describe any two linguistic forms as cognitively synonymous when the two forms are interchangeable […] *salva* (no longer *veritate* but) *analyticitate*" p.32. The omissions are not entirely innocuous but certainly simplify.

[36] "So, if the verification theory can be accepted as an adequate account of statement synonymy, the notion of analyticity is saved after all" (Quine 1953 p.38).

[37] P.37. The idea reappears on p.41: "as long as it is taken to be significant in general to speak of the confirmation and infirmation of a statement, it seems significant to speak also of a limiting kind of statement which is vacuously confirmed, *ipso facto*, come what may; and such a statement is analytic" and p.43: "Furthermore it becomes folly to seek a boundary between synthetic statements, which hold contingently on experience, and analytic statements, which hold come what may."

—— (2001) *Waves, particles and configuration space*, Modena, Accademia Nazionale delle Scienze, Lettere ed Arti

—— (2003a) "Bell's inequality with times rather than angles" *Journal of modern optics* **50**, 1063-9

—— (2003b) "Altering the remote past" *Foundations of physics letters* **16**, 293-301

—— and F. Selleri (1998) *The Einstein, Podolsky and Rosen paradox*, New York, Plenum

Ariew, R. (1984) "The Duhem thesis" *British journal for the philosophy of science* **35**, 313-25

Aspect, A. (1983) "Trois tests expérimentaux des inégalités de Bell par mesures de correlation de polarisation de photons" *Thèse*, Université de Paris-Sud, Centre d'Orsay

——, P. Grangier and G. Roger (1981) "Experimental tests of realistic local theories via Bell's theorem" *Physical review letters* **47**, 460-3

——, P. Grangier and G. Roger (1982a) "Experimental realization of Einstein-Podolsky-Rosen-Bohm Gedankenexperiment: a new violation of Bell's inequalities" *Physical review letters* **49**, 91-4

——, J. Dalibard and G. Roger (1982b) "Experimental test of Bell's inequalities using time-varying analyzers" *Physical review letters* **49**, 1804-7

Bell, J.S. (1965) "On the Einstein-Podolsky-Rosen paradox" *Physics* **1**, 195-200

—— (1986) letter to E. Santos dated 22 December

—— (1987) *Speakable and unspeakable in quantum mechanics*, Cambridge University Press

Boghossian, P. A. (1996) "Analyticity reconsidered" *Noûs* **30**, 360-91

Bohr, N. (1935a) "Quantum mechanics and physical reality" *Nature* **136**, 65

—— (1935b) "Can quantum-mechanical description of physical reality be considered complete?" *Physical Review* **48**, 696-702

Clauser, J. F. and M. A. Horne (1974) "Experimental consequences of objective local theories" *Physical review D* **10**, 526-35

Clauser, J. F., M. A. Horne, A. Shimony and R. A. Holt (1969) "Proposed experiment to test hidden variable theories" *Physical review letters* **23**, 880-4

Crowe, M. J. (1990) "Duhem and history and philosophy of mathematics" *Synthese* **83**, 431-7

Duhem, P. (1989) *La théorie physique*, Paris, Vrin

Einstein, A., B. Podolsky, and N. Rosen (1935) "Can quantum-mechanical description of reality be considered complete?" *Physical Review* **47**, 777-80

Glymour, C. (1975) "Relevant evidence" *The journal of philosophy* **72**, 403-26

—— (1980) *Theory and evidence*, Princeton University Press

Grice, H. P. and P. F. Strawson (1956) "In defense of a dogma" *Philosophical review* **65**, 141-58

Grünbaum, A. (1960) "The Duhemian argument" *Philosophy of science* **27**, 75-87

—— (1962) "The falsifiability of theories: total or partial? A contemporary evaluation of the Quine-Duhem thesis" *Synthese* **14**, 17-34

Hahn, L. E. and P. A. Schilpp, editors (1986) *The philosophy of W. O. Quine*, La Salle, Open Court

Helmholtz, H. von (1870) "Über den Ursprung und die Bedeutung der geometrischen Axiome" Vortrag, gehalten im Dozentenverein zu Heidelberg

Katz, J. (1967) "Some remarks on Quine on analyticity" *Journal of philosophy* **64**, 36-52

—— (1974) "Where things stand on the analytic-synthetic distinction" *Synthese* **28**, 283-319

Krips, H. (1982) "Epistemological holism: Duhem or Quine?" *Studies in history and philosophy of science* **13**, 251-64

Laudan, L. (1965) "Grünbaum on 'The Duhemian argument'" *Philosophy of science* **32**, 295-9

Lepore, V. L. and F. Selleri (1990) "Do performed optical tests refute local realism?" *Foundations of physics letters* **3**, 203-20

Mates, B. (1951) "Analytic sentences" *Philosophical review* **60**, 525-34

Needham, P. (2000) "Duhem and Quine" *Dialectica* **54**, 109-32

Perrie, W., A. J. Duncan, H. J. Beyer and H. Kleinpoppen (1985) "Polarization correlation of the two photons emitted by metastable atomic deuterium: a test of Bell's inequality" *Physical review letters* **54**, 1790-3

Quine, W. V. O. (1953) "Two dogmas of empiricism" in *From a logical point of view*, Harvard University Press 1953 pp.20-46

—— (1960) *Word and object*, New York, Wiley

—— (1962) "A comment on Grünbaum's claim" in *Can theories be refuted?*, edited by S. G. Harding, Dordrecht, Reidel 1976 p.132

—— (1986) "Reply to Jules Vuillemin" in Hahn and Schilpp (1986) pp.619-22

Strawson, P. F. "A logician's landscape" *Philosophy* **30**, 229-37

Suppes, P. (2002) *Representation and invariance of scientific structures*, Stanford, CSLI

Vuillemin, J. (1986) "On Duhem's and Quine's theses" in Hahn and Schilpp (1986) pp.595-618

Walther, T. and E. S. Fry (1997) "A Bell inequality experiment based on molecular dissociation – extension of the Lo-Shimony proposal to ^{199}Hg (nuclear spin 1/2) dimers" in *Experimental metaphysics* edited by R. S. Cohen, M. Horne and J. Stachel, Dordrecht, Kluwer 1997