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REFUTABILITY REVAMPED: HOW QUANTUM MECHANICS SAVES THE PHENOMENA

ABSTRACT. On the basis of the Suppes–Sneed *structural view* of scientific theories, we take a fresh look at the concept of *refutability*, which was famously proposed by K.R. Popper in 1934 as a criterion for the demarcation of scientific theories from non-scientific ones, e.g., pseudo-scientific and metaphysical theories. By way of an introduction we argue that a clash between Popper and his critics on whether scientific theories are, in fact, refutable can be partly explained by the fact Popper and his critics ascribed different meanings to the term 'theory'. Then we narrow our attention to one particular theory, namely quantum mechanics, in order to elucidate general matters discussed. We *prove* that quantum mechanics is irrefutable in a rather straightforward sense, but argue that it is refutable in a more sophisticated sense, which incorporates some observations obtained by looking closely at the practice of physics. We shall locate exactly where non-rigourous elements enter the evaluation of a scientific theory – this makes us see clearly how fruitful mathematics is for the philosophy of science.

1. PRELUDE

Popper (1934, p. 41) famously propounded *refutability*, or synonymously *falsifiability*, as a demarcation-criterion for scientific theories: a theory (considered by Popper logical-positivistically as a class of sentences closed under deduction) or a hypothesis (a single sentence) is *scientific* iff it contradicts some logically possible, observable event (fact, state of affairs). The standard objection against Popper's demarcation-criterion, levelled by a variety of philosophers of science (see Lakatos (1978) and contributors to Schilpp (1974) and O'Hear (1995)), is that not a single theory accepted by the scientific community is, in fact, refutable. The scientific community never specifies rejection-conditions in advance that will be acted upon scrupulously. Science is not Law. This would entail that Popper's criterion fails to make sense of science in that the success of science cannot be explained by the putative fact that all scientists live by this Popperian norm – for they contravene it. Thus a humble goodbye to refutability.

But can refutability be so easily dismissed? Not according to the following 'transcendental argument'.

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A blunt fact of the scientific enterprise is that some theories (models, hypotheses) are accepted whereas others are rejected. Another blunt fact is that the phenomena (results of observation and experimentation) fill a major rôle in this play of acceptance and rejection. Most if not all scientists see the phenomena playing a decisive rôle: accepted theories are accepted mainly *because* they are confirmed by a large variety of established phenomena, and other theories are rejected mainly *because* they conflict with certain established phenomena. But then our theories must be such that it is possible for them to be in agreement and to be in conflict with phenomena – if not categorically then conditionally on some generally accepted conditions, e.g., simplicity. For if they were not, phenomena could have little bearing on the acceptance or rejection of theories, *contra* the two blunt facts. So the *confirmability* as well as the *refutability* of our theories (conditional if not categorical) are necessary conditions for the possibility of science. Thus refutability cannot be dispensed with when we want to make sense of science.

An obvious manner to steer away from this collision about refutability is to reject it as a demarcation-*criterion* but to retain it as a *necessary condition* for a theory to be scientific – confirmability and consistency being other such necessary conditions. But steering away on this route still commits one to explain what it is that makes a theory refutable. For otherwise we are still in the target area of Popper's critics and, when hit, have to admit that not a single theory rejected by the scientific community can be said to be refutable.

The main purpose of the present paper is to take a fresh look at refutability on the basis of the structural view of scientific theories. In particular we shall show that one can, for a particular theory, present set-theoretical definitions of a theory whose scientific character does not stand in need of commendation to the rational mind – namely quantum mechanics –, and of its confirmability, its refutability, the phenomena relevant for the theory (data) and a few related concepts (Sections 3, 4). Such rigourous renditions have the virtue they make it in principle possible *to prove* and *to disprove* philosophical theses on the basis of some modest set-theory when it is possible to express these theses faithfully in the (perhaps a little enriched) language of set-theory, rather than to make such theses plausible (or implausible) by examples (or counter-examples) from the practice of science in past and present. (We presuppose that the axioms of some modest set-theory are not controversial; cf. the Appendix.) This is not to say that the practice of science of past and present is irrelevant for the philosophy of science, because the definitions we shall propound are, of course, inspired by looking closely at the practice of science. But, as Suppes (1968) has so eloquently argued, it is to say that standards of (informal) rigour must be obeyed in the philosophy of science as much and as often as possible. We shall *prove* that quantum mechanics is irrefutable in a straightforward sense (Section 5). Then we shall spell out what is needed in order to make it refutable; this will lead to a revised notion of refutability on the basis of which we shall argue that quantum mechanics is refutable after all (Section 6). Corollary to these investigations is a clear view how far informal rigour (supplied by set-theory) can help to elucidate, or even to decide, philosophically significant questions (Section 7). But first of all we need to set the stage by reviewing briefly how and why refutability went down (Section 2).

2. DUHEMIAN RHAPSODY

The main criticisms against Popper's demarcation-criterion are variations on a theme of Duhem, composed in his pioneering work (1915), in which he takes an incisive look at branches of physics in the XIXth century. This incisive look revealed that much more than just a Theory (**T**, for brevity) is needed to predict an Observable Event to occur under certain conditions (ObsEvent(**T**)). First of all, one needs data (broadly construed: initial and boundary conditions, values of parameters, constants of nature); secondly, background knowledge (BackKnow) is required, such as the laws of optics when telescopes or microscopes are involved; the laws of classical electro-dynamics when pieces of electrical apparatus are employed; parts of mathematics used in the derivation annex calculation of the quantitative prediction. And last but not least, there is a seemingly inexhaustible stock of tacit presuppositions often referred to as 'the *ceteris paribus* clause' and that we refer to as *Pandora's Box* (denoted by Pandora(**T**), where **T** between brackets indicates that each assumption in Pandora's Box is logically compatible with **T** – see below). Symbolically (all terms are classes of sentences):

$$(1) \qquad \big(\mathbf{T} \wedge \mathrm{Data} \wedge \mathrm{BackKnow} \wedge \mathrm{Pandora}(\mathbf{T})\big) \longrightarrow \mathrm{ObsEvent}(\mathbf{T}).$$

Duhem essentially observed that whenever the prediction is falsified, logic does not tell us *which* conjunct carries the blame, because the conjunct that forms the condition of the indicative conditional (1) is negated as a whole:

$$(2) \qquad \neg\mathrm{ObsEvent}(\mathbf{T}) \longrightarrow \neg\big(\mathbf{T} \wedge \mathrm{Data} \wedge \mathrm{BackKnow} \wedge \mathrm{Pandora}(\mathbf{T})\big),$$

which is by virtue of one of De Morgan's laws equivalent to:

$$(3) \qquad \neg\text{ObsEvent}(\mathbf{T}) \longrightarrow \big(\neg\mathbf{T} \vee \neg\text{Data} \vee \neg\text{BackKnow} \vee \neg\text{Pandora}(\mathbf{T})\big).$$

So logic compels you to pick on at least one disjunct of (3), but does not tell you which one. (We have written down statements (2) and (3) symbolically for easy reference to each of them below, not because we desire to be pedantic.)

In his *opus magnum*, Popper (1934, p. 50), who had listened to the Duhemian Rhapsody (he refers to it in several places: see the Index), admitted that "no conclusive disproof of a theory can ever be produced". Popper brushed aside the unreliability of experimental data and the background knowledge as generically implausible carriers of the blame (*opcit* p. 82 and Popper (1963, p. 112)) – glossing over the enormous problem how to ground the background knowledge non-inductively. But Pandora's Box, surely the leading theme of Duhemian Rhapsody, cannot be brushed aside so easily. For in contradistinction to the other conjuncts, it has *prima facie* devastating consequences for Popper's criterion. The idea is that for every phenomenon which **T** is supposed to save, it is possible to invent some hypothesis which in combination with **T** saves that phenomenon. In this context the sensational discovery of Neptune in 1848 was often discussed: from the observation that the orbit of Uranus did not agree exactly with calculations based on Newton's law of universal gravitation and his laws of motion (together **N** for brevity), it was not deduced that **N** had to go or that the data were flawed or that the auxiliary laws of optics governing light-rays through telescopes were wrong (3), but that there was another planet perturbing the orbit of Uranus – which is an hypothesis from Pandora's Box of **N**.

In general, Pandora's Box consists of a stock of hypotheses, formulated in the language of **T** – that are all consistent with **T**, otherwise advancing one of them would lead to logical disaster for **T**, rather than opening the possibility to save **T** from refutation. The content of Pandora's Box is seemingly inexhaustible, because human ingenuity is the limit. Since today we do not know which hypotheses will be found tomorrow (in Pandora's Box), a refuted theory can never be said to be beyond resurrection.

Popper (1934, pp. 82–83) had, however, taken care of Pandora's Box in a manner consistent with his view of science: advancing an hypothesis from Pandora's Box, H say, is *admissible* iff H is independently refutable; that is to say, H must imply at least one testable statement that is not implied by **T** (and the same data and background knowledge but) without H. The hypothesis 'there exists a planet in our solar system that orbits in accordance with **T** but which has so far escaped observation' is a case in point: it leads to the prediction of seeing a novel body in the heavens that cannot be made without assuming its existence. This hypothesis can be refuted by looking through a telescope at a particular spot in the night-sky at a particular time. Therefore the hypothesis is admissible. An hypothesis which is not independently testable, Popper baptised *ad hoc* and he deemed it *unscientific* to advance *ad hoc* hypotheses merely to save your cherished theory from falsification. Scientific behaviour is to seek confrontation with the phenomena, pseudo-scientific behaviour is to avoid it.

Popper's response to the presence of Pandora's Box essentially comes down to *denying its very existence as something separate from* **T**. This looks like ostrich-policy, but it is not, because for Popper, all tacit assumptions are part and parcel of the theory under investigation (**T**); if you mentioned **T**, you have mentioned it all, thus mentioning Pandora's Box is "not necessary" anymore (in Schilpp (1974, pp. 1186–1187, fn.75)). So

(4) $\text{Pandora}(\mathbf{T}) \subset \mathbf{T}$.

If one denies one of the tacit assumptions of **T** that is brought to the surface, and subsequently replaces it with another assumption (such as H above), we have *another theory*, $\mathbf{T}'$ say. Theories **T** and $\mathbf{T}'$ then are inconsistent, which is no problem for Popper – provided the tacit assumption is logically independent of **T**. But this notion of a theory of Popper's stands orthogonal to the scientist's notion of a theory: whether there are seven or eight or nine planets in the solar system is considered immaterial for **N**; all three are logically compatible with **N**. What Popper's critics wanted to consider – and what Popper ought to have considered too – is the falsifiability of **N**. For Popper, I would dare say, **N** is strictly speaking not a scientific theory because it is by itself not refutable (the point of his critics, see notably Putnam on **N** in Schilpp (1974, pp. 222–229)), but when we apply **N** to our solar system and add some hypothesis about its constitution to it (and take for granted the background knowledge and the data used in calculations), then we obtain a refutable theory (which none of his critics denied).

In this context, Lakatos (1978, p. 17) quoted Popper rhetorically asking: "What kind of clinical responses would refute to the satisfaction of the psychoanalyst not merely a particular diagnosis but psychoanalysis itself?" Then Lakatos asked (*ibid.*): "But what kind of observation would refute to the satisfaction of the Newtonian not merely a particular version but Newtonian theory itself?" By 'the theory itself' Lakatos means **N** and by 'a particular version' he can be taken to mean: any theory in the language of **N** that includes **N**. The 'structural view' on scientific theories will make Lakatos' distinction between 'a theory itself' and 'a particular version' of it rigourously clear, as well as in what sense Popper's contention (4) that Pandora's Box is part and parcel of the theory is correct (Section 3).

In our view, Popper responded convincingly to Lakatos' rhetorical question: **N** would be refuted if, for example, some planets were to move in rectangular orbits; or if the velocity of all planets were to decrease when they approach perihelium, rather than increase as one of Kepler's laws implies, which are approximately implied by **N** (in Schilpp (1974, pp. 1004–1006)). Lakatos' notorious fictional story of "planetary misbehaviour", allegedly illustrating how Pandora's Box creates trouble for Popper by suggesting the story is generic, is indeed, as Popper correctly pointed out in Schilpp (1974, p. 1007), "an extremely exceptional case": a small perturbation on exactly the expected orbit! (For further discussion on falsifiability we refer to Schilpp (1974, pp. 976–1013), Lakatos (1978, pp. 8–93, pp. 139–151) and Newton-Smith and Worral in O'Hear (1995).)

We further agree with Popper (in Schilpp (1974, p. 86)) that a single, isolated falsification does not and should not lead to a *rejection* of a theory, but one phenomenon repeated independently several times does lead to a rejection *in the sense of "eliminated as a contender for truth, not necessarily abandoned"* (Popper in Schilpp (1974, p. 1009)). We can retain the refuted theory as *observationally adequate* with respect to a certain accuracy and with respect to a well-delineated class of phenomena, and employ it for other purposes, e.g., in technological applications. For example, **N** has been refuted and superseded by the general theory of relativity, but NASA uses without exception **N** to launch satellites, space-shuttles and what have you, it never uses the general theory of relativity.

To conclude this Section, we summarise symbolically what Popper's inference from (2) is:

$$(5) \qquad (\text{Data} \wedge \text{BackKnow}) \longrightarrow \big(\neg\text{ObsEvent}(\mathbf{T}) \longrightarrow \neg\mathbf{T}\big),$$

where we have used Popper's claim (4) to eliminate Pandora's Box. Given Popper's 'tentative acceptance' of Data and BackKnow, we can now deduce that **T** is falsified as soon as an experimental result has been established that conflicts with ObsEvent(**T**). We finally emphasise that on the basis of the scientists (and Popper's critics) understanding of what a scientific theory is, the move from (2) to (5) is a *non sequitur*, because they reject (4) as a consequence of their different conception of what a theory comprises.

3. THEORIES

The specification of some scientific theory (call it **T** again) involves answering three questions:

(1) What sort of entity is **T** and which species is it?
(2) *How* does **T** relate to *which* phenomena?
(3) How does **T** (not) relate to reality?

In this paper we bracket the deep question (3) and concentrate wholly on questions (1) and (2).

There are two well-worked out answers to question (1) available. According to the *formal-linguistic view*, endorsed by (most of) the logical-positivists, **T** is a *formal-linguistic entity* (or can be faithfully rendered as one): the deductive closure of a set of postulates formulated in an extensional 1st-order formal language. According to the *structural view*, pioneered by Suppes (1953, 1960) (see Sneed (1979) for the first rigourous treatise of the structural view on classical mechanics), **T** is a *set-theoretical entity*: a set of set-structures in the domain of discourse of informal, standard set-theory (Zermelo–Fraenkel set-theory, abbreviated by ZFC; cf. Appendix). We adhere to the structural view, because it has a variety of well-known advantages over the formal-linguistic view (the present paper we submit as another illustration to that effect). The language of ZFC, denoted by $\mathcal{L}_\in$, is an extremely simple 1st-order language with only set-variables and the binary membership-relation as its only primitive predicate. Only sentences of $\mathcal{L}_\in$ can be proved on the basis of ZFC. We mention this explicitly because at some points in this paper we shall introduce 'sets' that cannot be defined in $\mathcal{L}_\in$ and therefore neither their existence nor any proposition in which they occur can be proved on the basis of ZFC (see Appendix A).

Since we shall focus our attention on a single theory, namely *quantum mechanics*, we characterise it structurally right now. We first define the set of pentuples consisting of: some separable Hilbert-space $\mathcal{H}$; some state-operator W, i.e., a member of the convex set $\mathcal{S}(\mathcal{H})$ of all bounded, positive, self-adjoint operators on $\mathcal{H}$ having trace equal to 1; some self-adjoint operator $A : \mathcal{H} \supseteq D_A \to \mathcal{H}$ and its spectrum σ_A; and the (infinitary) Kolmogorovian Born-Von Neumann probability measure

$$(6) \qquad P : B(\mathbb{R}) \to (0, 1), \quad \Delta \mapsto P(\Delta) \equiv \mathrm{Tr}\, W P(\Delta),$$

where $B(\mathbb{R})$ is the Borel-algebra of $\mathbb{R}$ and $P(\Delta)$ the relevant member of the unique spectral family of A (spectral theorem). We speak of a 'Born measure' whenever the state is pure, i.e., W is a projector. Hence the species of structure that co-defines quantum mechanics is:

$$(7) \qquad \text{QM} \equiv \big\{ \big\langle \mathcal{H},\ W,\ A,\ \sigma_A,\ P \big\rangle \mid \text{items as just explained} \big\}.$$

(Note that it follows immediately that QM $\neq \varnothing$, which would amount to a consistency proof of quantum mechanics iff QM (7) is regarded as a class of models of quantum mechanics construed formal-linguistically.) Of course, operator A is supposed to correspond to the physical magnitude of interest. Needless to say that characterisation (7) can be extended to consider more than one physical magnitude, or to consider time-dependence, in which case W becomes a function from $\mathbb{R}$ to $\mathcal{S}(\mathcal{H})$. Also a more sparse set of more specific structures can be considered, such as of type $\langle L^2(\mathbb{R}^3, d^3q), H, \sigma_H \rangle$, where H is the Hamiltonian; in this structure neither a state nor a probability measure occurs. The early wave-mechanical models that Schrödinger (1927) first considered were of this type. When all types of quantum-mechanical set-structures are defined, leading to sets $\text{QM}_0, \text{QM}_1, \ldots, \text{QM}_n$ say, then we proceed with their union. For the purposes of the present paper, however, characterisation (7) suffices.

The general idea behind definitions such as (7) is that in every branch of physics where phenomena are 'modelled quantum-mechanically', such as atomic physics, solid state physics, quantum chemistry, quantum optics and quantum transport theory, the concept of a 'quantum-mechanical model' can be construed as a set-structure of some type such as defined above in (7) or of some sibling type (cf. Suppes (1960)). If a model cannot be construed as a pentuple of sort (7), it is not and should not be considered as a *quantum-mechanical* model. Quantum mechanics is thus identified with all possible quantum-mechanical 'models'. This is simple, rigourous and makes lots of sense of the practice of physics.

A structural definition of a theory helps us immediately to make sense of Popper's contention that Pandora's Box is part and parcel of a theory and of Lakatos' distinction between 'the theory itself' and 'a particular version of it' (see Section 2) – when applied to quantum mechanics. First Lakatos' distinction: 'quantum mechanics itself' *is* QM and 'a particular version of quantum mechanics' *is a member of* QM. Next Popper's contention: every single 'model' we intuitively recognise as purely 'quantum-mechanical', and which could pass by Popper's lights as 'a quantum-mechanical theory', say, is a member of QM. Any new hypothesis of Pandora's Box chosen to save quantum mechanics when confronted with adverse data, must lead to a 'model' one can construe as a member of QM (7), otherwise we would simply no longer accept the 'model' as 'quantum-mechanical'.

The set QM just *is* Pandora's quantum-mechanical box because it contains *every possible* quantum-mechanical model. This motivates the following definition:

(8) $\text{Pandora}(\text{QM}) \equiv \text{QM}$,

modulo enlarging the definition of QM, as discussed in the text immediately following definition (7). Given some puzzling phenomenon that quantum mechanics is supposed to deal with, human ingenuity is needed 'to find the right model' in the vast class QM.

To specify quantum mechanics further, we have to answer question (2) of *how* QM relates to *which* phenomena.

4. PHENOMENA

A phenomenon is some observable occurrence. Phenomena are *qualitative*. For quantum mechanics in particular and for science in general, a phenomenon must be characterised *quantitatively*, e.g., as is standardly done in the presentation of the results of some scientific experiment; in science only measurements count. This quantitative characterisation is called a *data structure*. Over the years Suppes c.s. have classified all types of data structures encountered in science, investigated under *which* qualitative conditions *which* kind of data structure comes about, and proved 'representation theorems' for data structures; this programme is called *measurement theory* (for an historical introduction and overview, see Diez (1997) and references therein).

For the purpose of the present paper, we need the following type of data structure D_n: a set consisting of $n \in \mathbb{N}$ measurement-intervals $I_j \equiv [r_j, r_{j+1}) \subset \mathbb{R}$ and n concomitant relative frequencies $f_j \in [0, 1]$:

(9) $D_n \equiv \big\{ \langle I_j,\ f_j \rangle \in \mathcal{I}(\mathbb{R}) \times [0, 1] \mid j \in n \big\}$,

where $\mathcal{I}(\mathbb{R})$ is the set of *all* left-closed, right-open real intervals having rational end-points, i.e., of type (r, s), where $r, s \in \mathbb{Q}$. Every single value found in a measurement of some physical magnitude A falls in some $I_j \in \mathcal{I}(\mathbb{R})$; these intervals subdivide the measurement scale (always a finite real interval) in accordance to the measurement accuracy. Every measured value $a \in \mathbb{Q}$ of magnitude A falling in $I_j \in \mathcal{I}(\mathbb{R})$, say, is counted and the total is divided by the total number of measurements n; this yields the relative frequency $f_j \in [0, 1]$ of I_j.

Next we lump all possible data structures D_n (9), for every $n \in \mathbb{N}$, in a set called $\mathcal{D}$. The set $\mathcal{D}_@ \subset \mathcal{D}$ is the set of all *actual* data structures of type D_n (9); we define it loosely as all data structures of this type that represent the measurement results of some experiment described in a publication in a respectable science journal – we take the index @ also to refer to the current date on the Gregorian calender, because $\mathcal{D}_@$ grows over historical time. We remark that the notorious background knowledge is used in accepting some D_n as actual, i.e., in the definition of $\mathcal{D}_@$; it therefore need not concern us anymore, because we simply take it from here. The 'definition' of $\mathcal{D}_@$ clearly is not purely mathematical (not some sentence in $\mathcal{L}_\in$), unlike the definition of $\mathcal{D}$, but the definition is nonetheless crystal clear. (Exactly here enters a non-rigorous element in our considerations.) Further, not all actual data structures $D_n \in \mathcal{D}_@$ are *relevant* for quantum mechanics: some are relevant for astronomy, some for optics, etc. A story has to be told *how* some D_n is obtained, i.e., in *what kind of experiment* (performed or not), in order to decide whether quantum mechanics is supposed to save it or not. Call this set of *all actual relevant* and *all possible relevant data structures* $\mathcal{D}_@^{\mathrm{QM}}$ and $\mathcal{D}^{\mathrm{QM}}$, respectively. (The sets $\mathcal{D}_@^{\mathrm{QM}}$ and $\mathcal{D}^{\mathrm{QM}}$ would be akin to Sneed's sets of *intended* and *potential applications*, respectively, when his ideas were transplanted from classical to quantum mechanics.) Then $\mathcal{D}_@^{\mathrm{QM}} = \mathcal{D}_@ \cap \mathcal{D}^{\mathrm{QM}}$. No matter how the sets $\mathcal{D}_@$ and $\mathcal{D}^{\mathrm{QM}}$ are precisely described, we surely always have the following relations:

$$(10) \qquad \mathcal{D}_@^{\mathrm{QM}} \subset \mathcal{D}_@ \subset \mathcal{D} \quad \text{and} \quad \mathcal{D}_@^{\mathrm{QM}} \subset \mathcal{D}^{\mathrm{QM}} \subset \mathcal{D}\ .$$

Thus all quantum-mechanical set-structures float in a sea of stories that are needed to connect (some of) them to each other (see Muller (1998, pp. 284–292) for an elaboration on this). This connexion we define next.

The connexion must capture the Prime Directive of Physics: *calculated numbers and measured numbers should agree*. Let $\mathfrak{Q} = \langle \mathcal{H}, W, A, \sigma_A, P \rangle$ be a quantum-mechanical structure in QM and let $D_n \in \mathcal{D}$. Definitions: $\mathfrak{Q}$ *saves* D_n iff the relative frequencies in D_n coincide with the probability measure P; QM *saves* D_n iff some $\mathfrak{Q} \in$ QM saves D_n; and QM *has saved the phenomena up till now* (or by definition synonymously, *is observationally adequate*) iff QM has saved all actual data structures relevant for quantum mechanics. The set D_n is also a function from a subset $D \subset \mathcal{I}(\mathbb{R})$ to [0, 1], $I_j \mapsto f_j$; so requiring that f_j coincides with $P(I_j)$ means to say that the restriction of function P to D is identical to D_n. Succinctly ($\equiv$ means: is by definition logically equivalent too):

$$(11) \qquad \begin{aligned} &\mathrm{Saves}(\mathfrak{Q}, D_n) &&\equiv\ D_n \subset P \in \mathfrak{Q}. \\ &\mathrm{ThSaves}(\mathrm{QM}, D_n) &&\equiv\ \exists \mathfrak{Q} \in \mathrm{QM} : \mathrm{Saves}(\mathfrak{Q}, D_n). \\ &\mathrm{ObsAdeq}(\mathrm{QM}) &&\equiv\ \forall D_n \in \mathcal{D}_@^{\mathrm{QM}} : \mathrm{ThSaves}(\mathrm{QM}, D_n). \end{aligned}$$

(The free occurrence of P in the *definiens* of Saves($\mathfrak{Q}$, D_n) is not a logical mistake, because it occurs also in the *definiendum*: it sits in $\mathfrak{Q}$.) Notice that the sea of stories only enters in determining the range of the universal quantifier in the last-mentioned definition of (11); everything else is formulated in $\mathcal{L}_\in$.

Questions (1) and (2) have now been answered; the theory of quantum mechanics is specified as the following ordered pair:

$$\text{(12)} \qquad \left\langle \text{QM}, \ \mathcal{D}^{\text{QM}} \right\rangle.$$

We next return to Popper's demarcation-criterion.

5. THE IRREFUTABILITY OF QUANTUM MECHANICS

The translation and application of falsifiability to quantum mechanics is now clear: quantum mechanics is *refutable* iff there is a data structure relevant for quantum mechanics that QM does not save:

$$\text{(13)} \qquad \text{Ref(QM)} \equiv \exists D_n \in \mathcal{D}^{\text{QM}} : \neg\text{ThSaves(QM}, D_n).$$

Suppose that QM does not save a single data structure $D_n \in \mathcal{D}^{\text{QM}}$. This would have happened if QM were empty. Would we, then, still consider the theory thus defined 'scientific'? We venture to answer in the negative. It must be *possible* for the theory to save a phenomenon, otherwise we surely would not regard it as scientific. Refutability is not enough – *pace* Popper. We define quantum mechanics to be *confirmable* iff there is a data structure relevant for quantum mechanics that QM saves:

$$\text{(14)} \qquad \text{Conf(QM)} \equiv \exists D_n \in \mathcal{D}^{\text{QM}} : \text{ThSaves(QM}, D_n).$$

Now suppose QM saves *all* data structures, not only the relevant ones. Then what? Then testing quantum mechanics would be a futile activity, because every experimental result would count as a confirmation of it. Confirmability, too, is not enough – *pace* Carnap. So we define quantum mechanics to be *scientific* iff it is refutable and confirmable:

$$\text{(15)} \qquad \text{Sc(QM)} \equiv \text{Ref(QM)} \wedge \text{Conf(QM)}.$$

We shall now first prove that quantum mechanics is confirmable but irrefutable. The confirmability is trivial (it also follows from the irrefutability); we leave it. The theorem that QM is irrefutable is a corollary of a theorem we prove in the Appendix but explain here.

Consider the Birkhoff–Von Neumann lattice $\mathcal{P}(\mathcal{H})$ of all projectors of a given Hilbert-space $\mathcal{H}$. We define a *state measure* as a map $\mu : \mathcal{P}(\mathcal{H}) \to [0, 1]$ such that $\mu(\hat{1}) = 1$ and for every sequence of orthogonal projectors P_j, for $j \in I$ ($I \subseteq \mathbb{N}$ is an index-set: an initial sequence of $\mathbb{N}$) it holds that:

$$(16) \qquad \mu\Big(\bigoplus_{j=0}^{\#I} P_j\Big) = \sum_{j=0}^{\#I} \mu(P_j),$$

where $\#I$ is the cardinal number of set I. Equation (16) is just σ-additivity adjusted to the fact that $\mathcal{P}(\mathcal{H})$ is not a Boolean (but an ortho-modular) ortho-complemented lattice. One easily proves that the state measure μ has all the familiar attributes of a probability measure. For instance, it is monotonous with respect to the partial-ordering on $\mathcal{P}(\mathcal{H})$, and $\mu(P) + \mu(P^{\perp}) = 1$.

THEOREM 1. Every state operator $W \in \mathcal{S}(\mathcal{H})$ generates a state measure μ_W by means of definition:

$$(17) \qquad \mu_W : \mathcal{P}(\mathcal{H}) \to (0, 1), \qquad P \mapsto \mu_W(P) \equiv \mathrm{Tr}\, WP.$$

The concept of a state measure permits us to give a succinct formulation of a celebrated theorem.

THEOREM 2 (Gleason's Theorem). For $\dim(\mathcal{H}) > 2$, every state measure is generated by some state operator.

For a proof, see the Appendix in Hughes (1989). From Theorems 1 and 2 it follows there is a one-one correspondence between state operators and state measures (hence the last-mentioned's name).

A state measure is not a Kolmogorovian probability measure because of its *domain*, which is an ortho-modular rather than a Boolean lattice. In contrast, a projector-valued measure $P(\cdot) : B(\mathbb{R}) \to \mathcal{P}(\mathcal{H})$ is not a Kolmogorovian probability measure because of its *range*, which is not $[0, 1] \subset \mathbb{R}$ but $\mathcal{P}(\mathcal{H})$; it is called a *measure* nonetheless because its range is a spectral family and spectral families relate to $B(\mathbb{R})$ in a manner which, again, reminds us of a probability measure. The Born–Von Neumann measure from the Probability Postulate of quantum mechanics (6), however, demonstrably is a Kolmogorovian probability measure from $B(\mathbb{R})$ to $[0, 1]$. This raises the question how these three measures relate to each other. The answer is given by the following two theorems.

THEOREM 3. The composition of a state measure $\mu_W : \mathcal{P}(\mathcal{H}) \to [0, 1]$ and a projector-valued measure $P(\cdot) : B(\mathbb{R}) \to \mathcal{P}(\mathcal{H})$ is a Born–Von Neumann measure and hence a Kolmogorovian probability measure.

THEOREM 4 (State Theorem). Every Kolmogorovian probability measure is a Born–Von Neumann measure, i.e., the composition of some state measure and some projector-valued measure.

Theorem 4 is a representation theorem of sorts for Kolmogorovian probability measures: they can always be written as a particular kind of composition which involves some Hilbert-space. Worth mentioning is that the proof of the State Theorem does not proceed by *reductio ad absurdum*: the relevant Hilbert-space, state-operator and projector-valued measure are defined explicitly (see Appendix). Theorem 4 also implies, together with Gleason's Theorem, the announced Corollary; the proof of this implication is so simple that we spell it out here.

COROLLARY. Quantum mechanics is irrefutable, i.e., for every possible data structure, relevant or not, actual or not, there is a quantum-mechanical structure that saves it.

(18) $\quad \forall D_n \in \mathcal{D},\ \exists \mathfrak{Q} \in \text{QM} : \ \text{Saves}(\mathfrak{Q}, D_n).$

Proof. Given some arbitrary member of the set of data structures $\mathcal{D}$, D_n (9) say. Note that D_n is a map from $\mathcal{I}(\mathbb{R})$ to [0, 1]. Then $P \supset D_n$ for every Kolmogorovian probability measure $P : B(\mathbb{R}) \to [0, 1]$ whose restriction to the domain of function $D_n : I_j \mapsto f_j$ coincides with D_n. There are lots of them, as a moment's reflection will reveal. Let P_0 be such a probability measure. According to Theorem 4 there is a Hilbert-space, $\mathcal{H}_0$ say, a state measure μ_W (hence by virtue of Gleason's Theorem a state operator in $\mathcal{S}(\mathcal{H}_0)$ that generates it, call it W_0), and a projector-valued measure (that determines a self-adjoint operator by means of the integral equations of the spectral theorem, call it A_0), such that the Born–Von Neumann measure they compose is identical to the probability measure P_0. Then

(19) $\quad \mathfrak{Q}_0 \equiv \left\langle \mathcal{H}_0,\ W_0, A_0, \sigma_{A_0},\ P_0 \right\rangle \in \text{QM}$

saves the given data structure D_n. Q.E.D.

Corollary (18) vindicates the idea that (Pandora's Box of) quantum mechanics (7) *is rich enough to accommodate all possible data structures* $D_n \in \mathcal{D}$, hence certainly the ones we deem relevant for quantum mechanics by virtue of (10).

On the sole basis of our rigourous renditions and of ZFC, we now face the following dilemma: either (i) accept quantum mechanics as an *unscientific* theory, or (ii) reject Popper's demarcation-criterion. Choosing for horn (i) is sick: if quantum mechanics no longer counts as a scientific theory, then arguably there are no scientific theories at all. But those who choose horn (ii) of the dilemma are now committed to say what makes quantum mechanics *scientific*.

They could adumbrate Lakatos' view of *scientific research programmes* – a superbly dubbed questionable hybrid of Popper's falsificationist view and Kuhn's sociological paradigm view –, and point to the enormous success of the research programme called quantum mechanics They could thus adopt a Lakatosian demarcation-criterion: a theory is scientific iff it gives rise to a research programme that has been progressive over a period of historical time. They face however the question what to make of the fact that the Corollary entails, in Lakatosian terms, that the programme will, strictly speaking, *never* face anomalies (because this has become a mathematical impossibility); it therefore never *can* degenerate. Whenever we have a recipe to save every phenomenon when construed as a data structure in $\mathcal{D}$ (which the proof of the State Theorem indeed provides us with), then quantum-mechanical research as we know it becomes redundant. We are, then, in essentially the same predicament as we were before.

One gets the feeling there is something deeply wrong with all of this. But what? In the rest of this paper we shall try to put the finger on it, repair it and argue that the dilemma then does not arise anymore.

6. THE REFUTABILITY OF QUANTUM MECHANICS

We are able to prove the State Theorem and by implication Corollary because the universal quantifiers involved run over the entire set QM: we used the freedom to choose *any* state operator and *any* projector-valued measure without further restriction whatsoever. But this is not how things are done in the practice of quantum mechanics. Consider the following three examples.

(a) Suppose a theoretician has saved some relevant actual data structure $D_n \in \mathcal{D}_{@}^{\mathrm{QM}}$ by using the state W_0 and normal operator A_0 provided by the proof of the State Theorem. (Observe how strange this already is: in choosing a state and an operator we do not pay any attention the experiment that produces the data structure.) The experimentator next measures a different physical magnitude (he uses an entirely different piece of measurement apparatus), but subjects the (same type of) physical systems (electrons, say) to the *same* preparation procedure as before. This yields some other data structure, $D'_n \in \mathcal{D}^{\mathrm{QM}}_{@}$ say. This time the theoretician seems strongly committed to use the *same* state operator as he used before, i.e., W_0, and thus looses his freedom to choose any state operator he likes, in particular the one that is this time prescribed by the State Theorem in order to save D'_n. The theoretician can only do his trick again if he can convince us that for some reason or other the preparation procedure was not *really* the same as before, but was such that the state operator from the proof of the State Theorem miraculously happens to characterise the 'new' preparation procedure. Perhaps it depends on the weather, or on the expansion of the universe, or on vacuum fluctuations, etc. Not a promising line of argument to pursue. Actual experimentalist seldomly pursue it.

(b) Suppose now that an experimentator gives the theoretician some actual data structure $D_n \in \mathcal{D}^{\mathrm{QM}}_{@}$ and says to have measured the *position* of scattered electrons after they hit a target in some particle accelerator. Now the operators have been fixed in advance: three of Schrödinger's multiplication-operators, $X : \psi(x, y, z) \mapsto x\psi(x, y, z)$, and similarly Y and Z, one for every direction in $\mathbb{R}^3$, acting on Hilbert-space $L^2(\mathbb{R}^3, d\mathbf{q})$; this is the Cartesian position-operator in Euclidean space. The freedom *to choose* an operator is gone and therefore the proof of the State Theorem cannot even be appealed to once.

(c) So far we have deliberately ignored the evolution of physical systems over time and hence the dynamics of quantum mechanics. As soon as we introduce the unitary time-evolution, we have that at every instant of time $t \in \mathbb{R}$, the state $W(t) \in \mathcal{S}(\mathcal{H})$ is fixed if the state at $t = 0$, $W(0) \in \mathcal{S}(\mathcal{H})$, is given.

Examples (a) and (b) motivate a re-definition of the relevant type of *data structure*:

(20) $\widetilde{D}_n \equiv \langle D_n,\ W_{\mathrm{exp}},\ \mathrm{Magn}(\mathcal{H})\rangle,$

where $\mathrm{Magn}(\mathcal{H})$ is some set of operators which are candidates for corresponding to the physical magnitudes the experimentator has measured. If position is measured, then $\mathrm{Magn}(\mathcal{H}) = \{X, Y, Z\}$ (the multiplication-operators in Cartesian coordinates); if linear momentum is measured, then $\mathrm{Magn}(\mathcal{H}) = \{P_x, P_y, P_y\}$ (differential operators up to multiplication factor $-i\hbar \in \mathbb{C}$). If some scattering experiment is performed and energy is measured, then we have, on Hilbert-space $L^2(\mathbb{R}^3, \cos\theta d\theta\, d\varphi\, r^2 dr)$, a set of so-called *Schrödinger-operators* $\mathrm{Magn}(\mathcal{H})$, which consists of self-adjoint operators of the form $H(m, \ldots) = P^2/2m + V(\ldots)$, where $V(\ldots)$ is some (often spherically symmetric) scalar potential $\mathbb{R}^3 \to \mathbb{C}$, usually with several parameters, indicated by the dots. If the scattered particles are electrons, say, then the mass m is fixed and we obtain a subset of the mentioned set. For the choice of the potential V, there is however generally quite some leeway. But again, the required Hamiltonian will never coincide with the operator from the proof of the State Theorem because that one is not of the type required here.

The new data-structure (20) also contains a state-operator $W_{\text{exp}} \in \mathcal{S}(\mathcal{H})$. Of course *some* tempering with W_{exp} should be permitted, $W_{\text{exp}}+\delta\hat{1}$, for some small $\delta > 0$ within the bounds of experimental accuracy, but not further, because W_{exp} is supposed to be determined *experimentally* (see below). It seems odd to consider such comparatively abstract notions as Hilbert-space operators $W_{\text{exp}} \in \mathcal{S}(\mathcal{H})$ and the ones in Magn($\mathcal{H}$) as *experimental data*, because quantitative data standardly are rational or integer numbers. It becomes less odd when we answer the following questions. How does one know that *this* member of $\mathcal{S}(\mathcal{H})$ corresponds to the preparation procedure in the laboratory? How does one know that one of *these* operators on $\mathcal{H}$ corresponds to the physical magnitude that has been measured? To answer these questions, we make a Sneedian move (Sneed 1979, pp. 31–35): for a sufficient number of experiments we have to *assume* that the measured relative frequencies coincide with Born–Von Neumann measures of quantum mechanics, because then, and only then, can we assert to have determined the state-operator and magnitude operator *experimentally*. This makes state operators as well as magnitude operators *theoretical terms* of quantum mechanics (in the Putnam–Sneed sense), because not a single state operator or magnitude operator can arguably be determined experimentally without assuming that *some* structures from QM save certain phenomena. What is a 'sufficient number'? That depends on the details of the experiment. Consider a scattering experiment.

A particle accelerator prepares a beam of electrons in an initial state of a certain energy, $E_{\text{in}} \in \mathbb{R}$ say, in the positive x-direction. The initial pure state then is a Gaussian wave-packet, which is narrow and peaked around $p_x = (E_{\text{in}}/2m_{\text{e}})^{1/2}$ and $p_y = p_z \approx 0$ in momentum-space $L^2(\mathbb{R}^3, d^3p)$; this wave-packet is a solution of the free Schrödinger equation, because we consider the electrons in the beam to be non-interacting – one neglects their Coulomb-repulsion. In position-space $L^2(\mathbb{R}^3, d^3q)$ this state approximates a plane wave running in the x-direction. There is *some* leeway in choosing the width of the packet or even the shape of the packet, but that's the end of it: choosing for the prepared state the spherically symmetric Bessell-function of $n = 2002$, say, or a saw-tooth with erratically decaying tails, say, is here completely out of the question. When the position and the energy of the electrons scattered by the target are measured, we have – as we already mentioned above –, a single choice for the position-operator but a range of choices for the Hamiltonian from the class of Schrödinger-operators, that is, for the interaction potential V between electron and target. If we succeed in finding a particular potential, V_0 say (by intelligent guessing, perhaps based on classical physics, or by physical intuition), which leads to a calculated spectrum of $H(m, \ldots) = P^2/2m_e + V_0(\ldots)$ in agreement with the experiment, then we *assume* the relative frequencies to coincide with the Born measure in order to assert we have 'determined $V_0(\ldots)$ experimentally' by using one experiment. Now the choice for the interaction Hamiltonian in the next experiment, wherein we only double the energy of the beam of electrons, say, but use the same target, is heavily constrained by $V_0(\ldots)$. We are now restricted to use quantum-mechanical structures of type:

$$\text{(21)} \quad \langle L^2(\mathbb{R}^3, d^3q),\ \chi,\ H_0,\ \sigma_{H_0},\ P \rangle,$$

where $\chi \in L^2(\mathbb{R}^3, d^3q)$ is the wave-packet. This restriction makes quantum mechanics refutable, because the Born measures are now fixed.

Hence all definitions of Sections 2, 3 and 4 can now be repeated with $\widetilde{D}_n$ and $\widetilde{\mathcal{D}}$ replacing D_n and $\mathcal{D}$ (with some minor modifications), respectively. In particular the crucial notion of saving the phenomena becomes:

$$\text{(22)} \quad \text{Saves}{*}(\mathfrak{Q}, \widetilde{\text{D}}_\text{n}) \equiv D_n \subset P \in \mathfrak{Q} \wedge W_{\text{exp}} \in \mathfrak{Q} \wedge A \in \text{Magn}(\mathcal{H}) \in \mathfrak{Q}\ ,$$

which evidently is stronger than definition (11). To refute quantum mechanics, you must now be unable to find an operator in Magn($\mathcal{H}$) – rather than in the far more encompassing set of all self-adjoint operators on $\mathcal{H}$ –, so that it recovers the given relative frequencies via the Born–Von Neumann measure. So quantum mechanics is *refutable* after all, with definition (22) replacing definition (11) in definition (13): compute all Born measures, one for each member of Magn($\mathcal{H}$), then QM is refuted if for every such Born measure P it holds that $D_n \not\subset P$. Since QM also is trivially confirmable (14), we conclude that quantum mechanics is a scientific theory after all (15).

We finally remark that in stead of strengthening the definition of $\mathcal{D}_{@}^{\text{QM}}$, one can also strengthen the definition of the theory of quantum mechanics (7).

7. REFLECTIONS

The claim that quantum mechanics is irrefutable, as we asserted in Corollary (18), turns out to be untenable, because based on a too weak characterisation of quantum mechanics. But can we claim to have *proved* rigourously in ZFC that quantum mechanics is scientific? Strictly speaking we cannot, for it is not possible to provide definitions in $\mathcal{L}_\in$ of the sets $\mathcal{D}^{\mathrm{QM}}$, $\mathcal{D}_@$, Magn($\mathcal{H}$) and W_{exp}. Precisely here, and only here, do non-rigourous elements enter our further rigourous arguments. To a certain extent all these sets are 'vague' or too 'open-ended', and this, and only this, makes all arguments that involve these sets not as rigourous as proofs of theorems of ZFC in $\mathcal{L}_\in$. But *eo ipso* these arguments are an improvement *qua* rigour when compared to arguments in prose, because the premises of the afore-mentioned arguments and the rules of deduction they employ are completely known and explicit. Further, the delineation of the sets $\mathcal{D}^{\mathrm{QM}}$ and $\mathcal{D}_@$ does not create problems of any philosophical significance: the philosopher of science can analyse the stories that float in the sea (Section 3), but there is no point in contravening them. The sets Magn($\mathcal{H}$) and W_{exp} look, however, more promising for the refutability-sceptic as a starting point to doubt the refutability of quantum mechanics. For consider again our scattering experiment.

Our sceptic might object by raising the question what happens if one cannot find a suitable potential V_0. Must we then consider quantum mechanics to be falsified and abandon it? No, we would say, because then the game of testing simply does not come of the ground: without some V_0 quantum mechanics is empirically mute. When there is no game, there are neither winners nor losers. We can also appeal to the history of quantum physics: sooner or later always a suitable V_0 was found in Pandora's Box of QM, given the experimental constraints on W_{exp} and the theoretical constraints on V_0 – just as rectangular orbits for celestial bodies are ridiculous in the context of Newtonian physics, so are numerous potentials ridiculous in the context of quantum physicists, e.g., $V_0(r) = -ar^{2002}$ for $r \leqslant$ the radius of our galaxy and $= 0$ beyond ($a > 0$); $V_0(r) = -a\exp(br)$ for $r \leqslant$ the radius of our solar system ($b > 0$), V_0 a saw-tooth with erratically decreasing tails, etc. If our sceptic takes *this* to be sufficiently firm ground to remain sceptical about the refutability of quantum mechanics, then we wish her luck, for it seems to be the only ground she has left to stand on.

To conclude, the structural view on scientific theories and phenomena, which is set-theoretical in nature, makes clear exactly where, how and why non-rigourous elements enter the characterisation and evaluation of a scientific theory (quantum mechanics in our case); it makes clear what feeble ground the critic of the refutability of this same theory has to stand on; and, last but not least, it makes clear that both the confirmability and refutability of this theory are respectable notions which surely rank among the necessary conditions for the scientific character of a theory. Finally, the construction of such rigourous construals of a theory and the data forces us to take a close look at the practice of science and thereby will enhance our understand of it.

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8. APPENDIX: PROOFS

First four remarks about set-theory for *cognoscenti*. (i) All unbounded quantifiers occurring in this paper range over the initial part of the cumulative hierarchy of all sets cut off at ordinal rank $\omega + \omega$ – often denoted as $\mathbf{V}_{\omega+\omega}$. In this tea-spoon of sets (when compared to the entire hierarchy) all of the mathematics lives that physics needs and ever will need. (ii) Set $\mathbf{V}_{\omega+\omega}$ will also act as the separation-set in Zermelo's separation schema, which one uses to define sets that exist; we do not mention it explicitly in the definitions presented in this paper. (iii) To denote that X is in the ordered pair-set $\langle X, Y\rangle$, say, we simply write: $X \in \langle X, Y\rangle$, although this is formally incorrect – correct is: $X \in^2 \langle X, Y\rangle$ when $\langle X, Y\rangle \equiv \{\{X\}, \{X, Y\}\}$. (iv) For the natural numbers we take the finite Von Neumann ordinals ($\mathbb{N} \equiv \omega$); then we can write '$j \in n$' in stead of '$j = 0, 1, \ldots, n-1$'. Now we prove the theorems.

THEOREM 1. Every state operator $W \in \mathcal{S}(\mathcal{H})$ generates a state measure μ_W by means of definition:

$$\text{(23)} \qquad \mu_W : \mathcal{P}(\mathcal{H}) \to (0, 1), \quad P \mapsto \mu_W(P) \equiv \mathrm{Tr}\, WP.$$

Proof. Theorem (Prugovecki (1981, p. 196)): given that P_j projects onto the 1-dimensional subspace spanned by normalised Hilbert-vector $\phi_j \in \mathcal{H}$, then all ϕ_j span the subspace $\mathcal{N} \subseteq \mathcal{H}$ upon which the sum-projector projects, and:

$$\text{(24)} \qquad \Big(\bigoplus_{j=0}^{\#I} P_j\Big)\psi = \sum_{j=0}^{\#I} \langle\psi|\phi_j\rangle\phi_j.$$

The proof of Theorem 1 consists in verifying that map (23) satisfies the definition of a state measure, which is trivial when given Theorem (24) Q.E.D.

We call to mind the definition of a *projector(-valued) measure*: a function from the Borel algebra of $\mathbb{R}$ to some Birkhoff–Von Neumann lattice,

$$(25) \qquad P(\cdot) : B(\mathbb{R}) \to \mathcal{P}(\mathcal{H}), \quad \Delta \mapsto P(\Delta),$$

such that the following requirements are met (projectors $\hat{0}$ and $\hat{1}$ play the parts of 0 and 1, respectively):

$$(26) \qquad P(\varnothing) = \hat{0}\,, \quad P(\mathbb{R}) = \hat{1} \quad \text{and} \quad P(\mathbb{R}\backslash\Delta) = P^{\perp}(\Delta),$$

and further $P(\cdot)$ satisfies the following additivity requirement. Let Δ_j, where $j \in I \subseteq \mathbb{N}$, be a sequence of disjoint Borel sets ($\Delta_j \cap \Delta_k = \varnothing$ for all $j, k \in I$, but $j \neq k$); then it holds that the value of $P(\cdot)$ of the union-set $\bigcup_j \Delta_j$ equals the sum of the values of $P(\cdot)$ of the separate Borel sets Δ_j:

$$(27) \qquad P\big(\textstyle\bigcup_j \Delta_j\big) = \displaystyle\sum_{j=0}^{\#I} P(\Delta_j).$$

The notion of convergence involved here is that of the strong-operator topology, which is the norm-topology of the supremum-norm: $\| \; A \; \|$ is the supremum of the positive numbers $\| \; A\phi \; \| \in \mathbb{R}$ for all $\phi \in \mathcal{H}$ such that $\| \; \phi \; \| = 1$. Sums of projectors only give projectors if the summands are orthogonal, which is here the case because projectors on disjoint Borel sets are orthogonal. The range of a projector-measure is called a *spectral family* of projectors.

THEOREM 3. The composition of a state measure and a projector measure on any Hilbert-space is an infinitary Kolmogorovian measure over the Borel algebra $B(\mathbb{R})$.
Proof. Given some Hilbert-space $\mathcal{H}$, the composition of a state measure and a projector measure:

$$(28) \qquad \mu_W(\cdot) \circ P(\cdot) : B(\mathbb{R}) \to [0, 1], \quad \Delta \mapsto \mu_W\big(P(\Delta)\big) = \mathrm{Tr}\, W P(\Delta)$$

a Born–Von Neumann measure, which is easily verified to be an infinitary Kolmogorovian probability measure by using Equation (27). Q.E.D.

THEOREM 4 (State Theorem). For every infinitary Kolmogorovian probability measure over $B(\mathbb{R})$ there is some Hilbert-space, some state measure and some projector measure such their composition yields the given Kolmogorovian probability measure as in (28).

Proof. We realise that every set $L^2(\mathbb{R}, \mu)$ of square-μ-integrable complex functions on $\mathbb{R}$, where μ is an arbitrary measure, is a Hilbert-space, because the inner-product, defined in terms of a Lebesgue-integral, is a measure-dependent entity: $\langle\phi|\psi\rangle \equiv \int_{\mathbb{R}} \psi^* \phi d\mu$; see Prugovecki (1981, p. 103). We are given some infinitary Kolmogorovian measure on $B(\mathbb{R})$, P_K say. We consider the following Hilbert-space:

(29) $\quad \mathcal{H}_K \equiv L^2(\mathbb{R}, P_K).$

We now (i) present a projector(-valued) measure, then (ii) a state operator that together with the projector measure generates a state measure, and (iii) finally show that their composition, as in (28), equals the given Kolmogorovian measure.

(i) We associate with every Borel set Δ an operator $\hat{1}_\Delta : \mathcal{H}_K \to \mathcal{H}_K$ that multiplies a function $\phi \in \mathcal{H}_K$ (29) with the indicator-function on Δ: $\phi \mapsto 1_\Delta\phi$, where: $1_\Delta(x)\phi(x) \equiv \phi(x)$ if $x \in \Delta$, otherwise it is 0. The verification that this association yields a projector measure (25) is elementary, given the following theorem: an everywhere defined, hence bounded operator is a projector iff it is self-adjoint and idempotent (Prugovecki 1981, p. 200). (Note: this spectral family of indicator-functions is the spectral family of the position-operator only in $L^2(\mathbb{R}^3,\, dx\, dy\, dz)$, not in the Hilbert-space $\mathcal{H}_K$.)

(ii) Consider the Constant function $C(x) \equiv 1$ for all $x \in \mathbb{R}$, which is a member of $\mathcal{H}_K$; then $\| C \| = 1$. Let C be the projector that projects onto the 1-dimensional closed sub-space of $\mathcal{H}_K$ spanned by Hilbert-vector C. Operator C qualifies as a state-operator because all projectors do: $C \in \mathcal{S}(\mathcal{H}_K)$. The associated state measure generated by the spectral family of projectors from (i) is: $\hat{1}_\Delta \mapsto \mathrm{Tr}\, C\hat{1}_\Delta \in [0, 1]$.

(iii) We now have to verify that for all $\Delta \in B(\mathbb{R})$:

(30) $\quad \mathrm{Tr}\, C\hat{1}_\Delta = P_K(\Delta),$

where P_K is the given Kolmogorovian probability measure on $B(\mathbb{R})$. First we call to mind that the operator C is a bounded and self-adjoint because all projectors are. Then for *every* two $\phi, \psi \in \mathcal{H}_K$ one verifies easily that $\langle\phi \mid C\hat{1}_\Delta\psi\rangle = \langle\hat{1}_\Delta C\phi \mid \psi\rangle$.

We next choose a basis $\{\phi_j\} \subset \mathcal{H}_K$ that has the normalised vector C as a member (the trace does not depend on which basis is chosen to compute it), $\phi_0 \equiv C$ say. Then $C\phi_j = \delta_{0j}\phi_0$, where δ_{jk} is Kronecker-delta.

Using these results, we obtain:

$$\text{(31)} \quad \operatorname{Tr} C \hat{1}_\Delta = \sum_{j=0}^{\infty} \big\langle \phi_j \mid C \hat{1}_\Delta \phi_j \big\rangle = \sum_{j=0}^{\infty} \big\langle \hat{1}_\Delta C \phi_j \mid \phi_j \big\rangle$$

$$= \sum_{j=0}^{\infty} \big\langle \hat{1}_\Delta \delta_{0j} \phi_j \mid \phi_j \big\rangle.$$

This leaves us with a single term ($j = 0$), which is a definite Lebesgue-integral:

$$\text{(32)} \quad \operatorname{Tr} C \hat{1}_\Delta = \langle \hat{1}_\Delta C \mid C \rangle = \int_{\mathbb{R}} \big(1_\Delta(x) C(x)\big)^* C(x)\, dP_{\mathrm{K}}$$

$$= \int_{\mathbb{R}} 1_\Delta(x)\, dP_{\mathrm{K}}.$$

For every measure μ, the definite Lebesgue-integral over $\mathbb{R}$ of the indicator-function of Δ is equal to the measure of Δ; then for $\mu = P_{\mathrm{K}}$:

$$\text{(33)} \quad \int_{\mathbb{R}} 1_\Delta\, dP_{\mathrm{K}} = P_{\mathrm{K}}(\Delta).$$

From Equations (32) and (33) it follows what we had to prove: identity (30). Q.E.D.

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