Studies in plausibility theory, with applications to physics

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ai miei tre tesori:
mia mamma, mia sorella, e il mio amore

nonna Matilde Onnis Porta
nonno Giovanni Porta
morfar Lewis Nyberg
farmor Alice Hult
in memoriam

Abstract

The discipline usually called 'probability theory' can be seen as the theory which describes and sets standard norms to the way we reason about *plausibility*. From this point of view, this 'plausibility theory' is a province of logic, and the following informal proportion subsists:

$$\frac{\text{plausibility theory}}{\text{common notion of 'plausibility'}} = \frac{\text{deductive logic}}{\text{common notion of 'truth'}}.$$

Some studies in plausibility theory are here offered. An alternative view and mathematical formalism for the problem of induction (the prediction of uncertain events from similar, certain ones) is presented. It is also shown how from plausibility theory one can derive a mathematical framework, based on convex geometry, for the description of the predictive properties of physical theories. Within this framework, problems like state assignment — for any physical theory — find simple and clear algorithms, numerical examples of which are given for three-level quantum systems. Plausibility theory also gives insights on various fashionable theorems, like Bell's theorem, and various fashionable 'paradoxes', like Gibbs' paradox.

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There is *one* that I have to anti-thank and for whom I must express my innermost despise (and that of the rest of the world, I suppose). He has continuously caused problems, and he is the responsible for all the errors and the lacks of quality of this work (besides, of course, for the problems of world hunger and war). Who am I speaking about, if not

PIPPO!!!!

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List of public-made papers and other contributions

(Note: In later papers I have decided to sign with my mother's family name, 'Porta', as well as my father's, 'Mana'.)

Papers included in this work

Paper (A) Porta Mana, P. G. L., 2003, *Consistency of the Shannon entropy in quantum experiments*,
eprint `arXiv:quant-ph/0302049`; also publ. in a peer-reviewed journal.

Paper (B) Porta Mana, P. G. L., 2004, *Distinguishability of non-orthogonal density matrices does not imply violations of the second law*,
eprint `arXiv:quant-ph/0408193`.

Paper (C) Porta Mana, P. G. L., 2003, *Why can states and measurement outcomes be represented as vectors?*,
eprint `arXiv:quant-ph/0305117`.

Paper (D) Porta Mana, P. G. L., 2004, *Probability tables*,
eprint `arXiv:quant-ph/0403084`; also published in a conference proceedings volume.

Paper (E) Porta Mana, P. G. L., A. Månsson, and G. Björk, 2005, *On distinguishability, orthogonality, and violations of the second law*,
eprint `arXiv:quant-ph/0505229`; submitted to a peer-reviewed journal.

Paper (F) Porta Mana, P. G. L., A. Månsson, and G. Björk, 2006, *'Plausibilities of plausibilities': an approach through circumstances*,
eprint `arXiv:quant-ph/0607111`.

Paper (G) Porta Mana, P. G. L., A. Månsson, and G. Björk, 2007, *The Laplace-Jaynes approach to induction*,
eprint `arXiv:physics/0703126`, eprint `PhilSci:00003235`.

Contribution of the author in **Papers (E)–(G)***:* I was the main developer; made the literature search; wrote the papers.

Paper (H) Månsson, A., P. G. L. Porta Mana, and G. Björk, 2006, *Numerical Bayesian state assignment for a three-level quantum system. I. Absolute-frequency data, and constant and Gaussian-like priors*,
eprint `arXiv:quant-ph/0612105`.

Paper (I) Månsson, A., P. G. L. Porta Mana, and G. Björk, 2007, *Numerical Bayesian state assignment for a three-level quantum system. II. Average-value data, and constant, Gaussian-like, and Slater priors*,
eprint `arXiv:quant-ph/0701087`.

Contribution of the author in **Papers (H)–(I)***:* I contributed to the work on three-level systems; jointly worked on the creation and running of the program codes; made the main part of the literature search; in the first paper I wrote the introductory and concluding sections and rewrote part of the rest.

More generally, as regards **Papers (F)–(I)**: I formulated the original idea of 'circumstance' and proposed the project of developing this idea in the general framework presented in **Papers (C)** and **(D)**.

(J) Björk, G., and P. G. L. Porta Mana, 2003, *A size criterion for macroscopic superposition states*,
eprint `arXiv:quant-ph/0310193`; also publ. in a peer-reviewed journal.

(K) Björk, G., and P. G. L. Porta Mana, 2004, *Schrödinger-cat states: size classification based on evolution or dissipation*, in Proceedings SPIE: Fluctuations and Noise in Photonics and Quantum Optics II, vol. 5478, pp. 335–343.

Contribution of the author in **Papers (J)–(K)***:* I gave feedback through discussions on physical and mathematical points; gave a minor help in the literature search; jointly wrote the introductory and concluding sections of the papers.

Other contributions

- Cadoni, M., and P. G. L. Porta Mana, 2000, *Hamiltonians for a general dilaton gravity theory on a spacetime with a non-orthogonal, timelike or spacelike outer boundary*,
eprint `arXiv:gr-qc/0011010`; also publ. in a peer-reviewed journal.

- Porta Mana, P. G. L., and A. Månsson (presenter), 2004, *Maximum-entropy methods with Shannon's entropy for a two-level quantum system*, poster presented at the CNRS Summer school on 'Quantum logic and communications', Cargese, France.
- Porta Mana, P. G. L., and G. Björk (speaker), 2005, *Uncertainty, information and entropy*, invited talk at the 'Ninth International Conference on Squeezed States and Uncertainty Relations', Besançon, France.

I. Prologue

> Aber wie wollte ich gerecht sein von Grund aus! Wie kann ich Jedem das Seine geben! Diess sei mir genug: ich gebe Jedem das Meine.
>
> *Zarathustra* [558]

Truesdell, Jaynes, and Hardy

In the present notes I present part of the studies pursued to achieve the title of 'philosophiae doctor'. Well, I have not really pursued them for the sake of the title (which like all titles is seldom a guarantee of anything, especially today), but rather spurred by curiosity about certain questions stemming for a passion (which, like all passions, is not a guarantee of success and quality) for physics, mathematics, logic, and philosophy — in a word, a passion for *natural philosophy*.

My final undergraduate studies concerned general relativity, and the first thought was to continue my graduate studies in that beautiful field. But I still had a strong curiosity and antipathy towards quantum mechanics, which originated from the related undergraduate studies. So my graduate studies took a quantum-mechanical turn. The subject was, and still is, absolutely counterintuitive. Teachers say that it is the physical phenomena described that are counterintuitive. I believe that it is the model we have build of them that is counterintuitive, not the phenomena themselves; or I could wickedly say: it is the *absence* of physics in the model that is counterintuitive.[1] As I say later in these pages, quantum mechanics looks like a Linnaean 'catalogue' and systematisation of interesting phenomena; but its phenomenological system lacks physics as Linné's system lacked genetics, so to speak.

A very important turn in my studies was the finding and reading of Jaynes' *Probability Theory: The Logic of Science* [392]. I can still recall the wonder and enlightenment I felt reading the first chapters of that book. Probability theory had up to that point been for me just a difficult and obscure subject. Suddenly it was shown by Jaynes to be just a

[1] To this, one should also add the unscientific attitude of many physicists who indulge in speaking about 'unsolvable mysteries' or the like; such physicists would have made very good priests.

province of logic, with few, simple, and clear principles, from which all the rest can be derived and on which all applications are based. It turned out that this point of view was quite old, at least as old as Leibniz' studies, and that it had been and still is cultivated not only by physicists but also by logicians like Hailperin [312, 313]. From this new point of view, and reading other studies by Jaynes and by others (e.g. Caves, Fuchs, Schack) having similar views, quantum mechanics began to appear somewhat less incomprehensible. More precisely, some of its mathematical objects and structure acquired new meanings, quite different from those I had been taught. But their origin, the physical idea behind them, was and still is missing.

Another turn, rapidly following the Jaynesian one, was the reading of Hardy's *Quantum theory from five reasonable axioms* [316]. Hardy presented a mathematical structure completely isomorphic to that of quantum mechanics, but without imaginary numbers. I had not thought that something like that could ever be possible; but it is (and, a posteriori, one sees that this possibility is a quite trivial fact). More than that, Hardy started from a mathematical framework that is as much simple as it is general, and can be applied to classical as well as quantum mechanics, and provides a lot of insight into the mathematical structures of these theories. My curiosity was drawn at first principally toward that framework. It appears to have been around at least from the fifties.

The third turn, no less important than the other two, was the reading and studying of Truesdell's work and studies in rational continuum thermomechanics. Also in this case I felt enlightenment and enthusiasm. Truesdell and the other workers in rational thermomechanics — Coleman, Ericksen, Noll, Owen, Serrin to name just a few — present thermodynamics in the same way as rational mechanics, or analytical point mechanics, or continuum mechanics are usually presented. Not only that: they present continuum mechanics (including electromagnetism) and thermodynamics as one whole beautiful subject. They dispelled many of the preconceptions I had learnt to parrot in thermodynamics: that temperature and entropy are 'defined' only in equilibrium and for 'quasi-static' processes, that the entropy function determines the equation of state, et similia. But Truesdell's writings — which are often literary gems and like Jaynes' are colourful, provocative, personal, unlike the many dull, dry writings that follow some recommended 'scientific style' in order to achieve a semblance of objectivity — have given and give me much more than only thermodynamic insights. They are a guide and reference in scientific thinking; in understanding what physics and physical theories are; and in quality standards — which, unfortunately, I seldom meet albeit I strive to achieve them.

What about quantum theory, after these studies? On the one side, I am no longer interested in it. 'Classical' physics is more beautiful; it is 'classical' also in the literary sense. On another side, I am still interested in quantum mechanics in the sense in which they say 'know your enemy'. Its understanding can eventually lead to its resolution into 'classical' physics, a possibility for which I see no physical or conceptual obstacles. I discuss this in more detail at various places in these notes.

Purpose and contents of these notes

The public-made articles reprinted with these notes, a list of which is given on page xiii, are a part of the fruits of the studies inspired by the work of Jaynes, Truesdell, and Hardy. They concern plausibility theory and the general features of the plausibilistic properties of physical theories. They are minuscule notes and bricks that I hope may be used in the music and edifice of natural philosophy: to replace older and partly eroded bricks, to modulate between two or more arie. It is ennobling to contribute, however modestly, to the same sublime music and great edifice to which the geniuses of Aristotle, Euclid, Newton, Leibniz, Euler, Laplace, Gauss, Cauchy, Hamilton, Maxwell, Boole, Gibbs, Poincaré, Hilbert, Einstein, Wittgenstein, Tarski, Truesdell and many others contributed foundations, themes, vaults, counterpoints, buttresses, stretti, pillars, rhythms.

The chapters that follow are only meant as concrete or ligatures amongst the reprinted articles, to show their unity and fill some gaps. Thus, these notes are not meant to be a short introduction to plausibility theory, or plausibility logic, or statistical models, or classical or quantum mechanics, or other subjects touched in the discourse. Rather, knowledge of these subjects is to a great deal *presupposed* in order to read the articles presented. These notes may rather be, for some readers, *an invitation to read and study* the cited works on these subjects, so that these readers can build by themselves a context against which to read and understand the articles; for other readers, *a reminder* of those works and subjects. As invitation or as a reminder, they are accompanied by personal remarks on some of those works and on the subjects themselves. The presentation is not linear, since the subjects presented interconnect and touch one another in a variety of points and ways. This is also the reason for which I have chosen to divide the text in continuously numbered sections.

'What do you mean by "plausibility theory"?' some will ask. Plausibility theory is that province of logic which describes and formalises the way we reason, or we think we ought to reason, about plausibility. In precisely the same sense, well-known deductive logic describes and formalises the way we reason about truth. There are many *mathematical* similarities between plausibility theory and the various probability theories that stroll and limp around. But its strength lies in its meaning and its interpretation; and on this account it is intimately related to Bayesian probability theory. Chapter II, containing a short introduction and summary of plausibility logic, with references and brief historical remarks, provides some context against which to read **Papers (F)** and **(G)**, which represent my small contributions to the problem of induction within plausibility theory. In this chapter I also give some additional remarks and addenda to those papers.

'And what do you mean by "*plausibilistic* properties of physical theories"?' others will ask. I mean those quantitative properties of the predictions, expressed as plausibility distributions, that a physical theory allows us to make, and above all the certainties or plausibilities of these predictions, independently from what these predictions are physically about. There is a mathematical framework which is particularly suited to this study. I call it the 'vector' or 'convex' framework since it is based on linear algebra and convex geometry plays an important part in it. It is intimately related to the theory of statistical models. Chapter III is a sort of introduction to this framework from a very general perspective and with some historical remarks, and thus provides some context against which to read

Papers (C) and **(D)**. A panoramic of some advantages and insights provided by the vector framework is also given. **Papers (B)** and **(E)** are examples of such insights.

Plausibility theory as discussed in chapter II is at the heart of the vector framework discussed in chapter III. Indeed, chapter IV shows that the problem of state assignment, as formulated in the vector framework, is just a case of a problem of induction in plausibility theory. This presents the ideas and the general formulae on which **Papers (H)** and **(I)** are based. Additional remarks are also given.

The remaining part of my studies' fruits, especially those on thermodynamics, statistical mechanics, and continuum mechanics, is not presented here (if not en passant in **Papers (B)** and **(E)**) although it constitutes a good 50% of the total, because it consists more in personal insights than in original contributions, and also because of spatio-temporal limitations. In chapter V, however, I sketchily present some ideas of mine for future studies, and it will be clear that many of these concern thermodynamics, statistical mechanics, and continuum mechanics.

Since I think that the study of past literature, which often hides extremely beautiful gems, is essential to research and gives many insights, I have tried to provide as many bibliographic references as possible.

The notation follows ISO [362] and ANSI/IEEE [358] standards.

Other remarks

Of course, under my post-graduate studies I have also formed some opinions and got a clearer personal view on research in general. These opinions and view surely emerge in the remarks presented on the following pages. As well as opinions I have also had some 'revelations', unfortunately mostly negative, like e.g. the fact that the 'peer-reviewed' literature of our times counts too many unworthy and erroneous papers. I do not mean papers which present theories that at a deeper analysis prove to be inconsistent, or that yield wrong predictions; for these are part of the crab march of science. I mean papers whose authors clearly do not know the subject they are working with, or do not have a minimum of logic, or the mathematics is completely erroneous at an elementary level. And I am not saying that such papers are the majority, but their number is for my own taste too high. The problem is not really with such papers themselves and with their content, for 'quadratures of the circle', equations like '$2 + 2 = 5$', et similia have always been, and will always be, presented. The problem is that those papers were supposedly 'peer-reviewed'. This simply means that 'peer-reviewed' publication is not a guarantee for quality. Perhaps it gives a guarantee for *plausible* quality; but from my personal experience as reader this plausibility does not reach, not for all but yet for many famous journals, a level which I can personally accept.

A *far greater* problem with many famous 'peer-reviewed' journals is that they have often been and still are a vehicle for ostracism from the current 'establishment'. New or provocative but mathematically and logically irreproachable ideas meet resistance in the *publication* phase; but the naturally understandable resistance should be met in the *reading* phase, not in the publication one. So we are in the perverse situation in which ridiculous papers are 'peer review'-accepted and published simply because they parrot what the Celebrity of the Moment (often only a scientific pygmy) has said, and beautiful papers are 'peer review'-rejected simply because they say something provocative though ingenious. Jaynes' papers are an example of the latter kind; he had to publish mostly in conference proceedings. Another 'problem' are the constraints that 'peer-reviewed' journals impose on the style of a paper. These constraints are completely understandable, since each journal is characterised by its style, has its aficionados, and must find formal devices to maintain an appearance of objectivity. And yet, my personal opinion is that with regard to style there are no authorities other than those one self chooses, and the *Chicago Manual of Style*, e.g., may well be a bible for someone and au pair with toilet paper for someone else. I am not willing to adapt the style of a paper to that of a particular journal only to be published therein.

Fortunately today, thanks to the internet, all the problems above are no problems anymore. *Any* scholar can post his or her studies, written in a completely personal style, in scientific archives which are *freely and publicly available* — an essential requisite of scientific studies —, like `arXiv.org` (`http://arxiv.org`) and `mp_arc` (`http://www.ma.utexas.edu/mp_arc/`), and the 'peer reviewers' are the *final readers*, who may praise the paper or throw it away, and hopefully send criticisms and comments to the author in both cases. No ostracism can be excised in between: the responsibility of judging the paper lies entirely on the readers, not on ignote third parties — and I think this is a very important point in science. Much time is also gained in avoiding discussions with 'referees' who sometimes are even incompetent as regards the paper's subject or have no reasons to criticise a paper other than that it shows weaknesses in some work of their own or of their friends and allies.[2] In order to have opinions and criticisms about some study of mine I prefer to ask some colleague who I *know* to be competent and frank. It is for this reason that, as of 2007, I have for the moment decided not to submit any papers to 'peer-reviewed' journals any longer. I post them mainly to `arXiv.org`, and whoever wants to judge them must do so by *reading* them, not by looking where they are published.

Another negative revelation has been the knowledge-tight separation between different physical disciplines. Too many quantum physicists, e.g., have no idea of the new results achieved in thermodynamics since the sixties (many will still tell you that temperature is defined only at equilibrium[3] and that equations of state can be derived from the entropy function); and many quantum experimentalists are unaware of the theorists' profound insights, some of which are seventy years old, into the mathematical structure of quantum mechanics. This separation can even become puerile hostility in the relationship between physics and mathematics, or physics and philosophy. Quite disappointing is the philosophical naïveté of so many physicists, even of some otherwise very competent; grotesquely, it is usually accompanied by scorn for the philosophers' activities.[4]

[2] I think that a useful thing would be if 'peer-reviewed' journals periodically published a list of rejected papers along with the names of the referees who rejected them and the grounds for rejection.

[3] They could as well say that 'position' is defined only at rest.

[4] I have read somewhere that Wheeler said 'Philosophy is too important to leave to the philosophers'. I hope he was joking. Surely I should not leave philosophy to people talking about 'it from bit', 'complementarity', or other cheap and puerile ideas.

II. Plausibility logic and induction

A Bayesian statistical test

1. Suppose that a person called Ingo is in a certain room at a certain time and, because of various reasons that need not interest us now and that we denote by I, Ingo strongly believes or is almost sure that a person called Ambrose will come into the room within the next minute, a hypothetical fact that we denote by the proposition A.

Someone asks Ingo what is the probability of A, and he says

$$\mathrm{P}(A|I) = 0.001. \tag{II.1}$$

A minute passes, and Ambrose has not come in, therefore Ingo and we agree that *A is false*.

Now call eq. (II.1) a 'model', consider all the facts that I have told you, and answer the following test question: Do you think that eq. (II.1) is a good or a bad model?

If you answer:

'Yes' — then plausibility theory, and Bayesian probability theory in particular, is not for you.

'No' — then plausibility theory and Bayesian probability theory are probably for you.

Or perhaps you have a third answer — the most appropriate one: 'Wait a moment: first you must tell me *what* eq. (II.1) is a model for!'. The point in fact is that those who answer 'Yes' evidently conceive $\mathrm{P}(A|I)$ as a 'model' of how things are, in this case those denoted by A. Those who answer 'No' instead conceive $\mathrm{P}(A|I)$ as a 'model' of how the *beliefs* about A of the person who knows I are.

In plausibility theory, $\mathrm{P}(A|I)$ quantifies a belief, not the 'reality of a fact' or 'how often a facts has happened' or 'the tendency of a fact to happen' or something similar. And an equation like (II.1) is a 'model' for a *belief* about A, *given* the knowledge I. Hence eq. (II.1) is, in this particular case, a really *bad* model, since it does not reflect nor quantify well at all what Ingo believed about A when he knew I (remember that he was almost sure that Ambrose would come). It does not matter, for the purposes of judging the model, if A is then observed or not, if Ambrose came or not.

Deductive logic

2. Physicists have generally a great respect for logic and formal logic[1] and try never to break its rules, even if this sometimes happens anyway. If for instance a physicist, in a given context I, first asserts the proposition A — and this can be symbolically written as $I \models A$ or $\mathrm{T}(A|I) = 1$ — and then the proposition B, $\mathrm{T}(B|I) = 1$, and later asserts that A and B are together false, $\mathrm{T}(A \wedge B|I) = 0$, then this is sure to bring forth a lot of papers and comments attacking his work and perhaps even him personally as well. Because we all know that

$$\begin{array}{ll} \text{if you state} & I \models A, \\ \text{and you state} & I \models B, \\ \hline \text{then you must state} & I \models A \wedge B \end{array} \tag{II.2}$$

Because thus is the way we *reason* about facts and hypotheses and their truth, and we are not willing to accept conclusions by a person who does not follow this way of reasoning. In this sense formal logic is *normative*: in it we have distilled, formalised, and in some cases simplified the way any rational person should reason about facts and hypotheses.

An important characteristic of formal logic is that it is not directly concerned with the subject of the reasoning it formalises. If I state that all fairies are under ten centimetres tall, then assert that Uranium is a radioactive element, and finally assert that it is *not* true that all fairies are under ten centimetres tall *and* that Uranium is a radioactive element, you would certainly object to my conclusion. Not on the grounds that fairies do not exist, but on the grounds that I violate the conjunction rule above. In fact, I may even reason about statements that are *empirically* false, but if I respect the rules of logic my reasoning is completely *self-consistent*. In this case you can argue against the truth of my conclusions not because of my reasoning, but because the *premises* I used were empirically false.

We see then that in logic we have some 'initial' propositions (the premises or axioms or hypotheses) and some 'final' propositions (the conclusions), constructed by means of various logical connectives like 'and' ($\wedge$), 'or' ($\vee$), 'not' ($\neg$), and others. Logic is only concerned about the relationships between the truth or falsity of the former with that of the latter; but it is not concerned on whether the former or the latter are actually true or false.

3. The form in which formal logic is constructed stems from the fact that the common notion of 'truth' can be taken to be, in many situations at least, dichotomic. This means that the truth or falsity of a proposition A can be represented by one of two values — 't' and 'f', '$\top$' and '$\bot$', '0' and '1', or whatever — associated to that proposition. So we could write something like $\mathrm{T}(A) = 1$ in order to express that A is true. In this notation, however, some

[1] Many are the books and textbooks in logic. A list including same classics can count Mill [529], De Morgan [160], Johnson [400–402], Lewis and Langford [465], Tarski [687], Quine [609, 610], Strawson [673, 674] Copi and Cohen [135, 136] Suppes [680], Hamilton [315], Barwise and Etchemendy [51, 52] (see also [48–50]), van Dalen [149]. See also Adams [1, 2] and Hailperin [313], who present the connexions between formal logic and probability theory.

$T(A\|I)$	$T(B\|I)$	$T(\neg A\|I)$	$T(A \wedge B\|I)$	$T(A \vee B\|I)$
0	0	1	0	0
0	1	1	0	1
1	0	0	0	1
1	1	0	1	1

Table II.1: Truth tables for negation, conjunction, and disjunction.

important details are left implicit. Perhaps the proposition A is empirically true, or perhaps we are entertaining its truth only hypothetically or conditionally on other hypotheses or on a model. In other words, the truth of a proposition is always best referred to a *context*. Denoting such a context by I, we can then write '$\mathrm{T}(A|I) = 1$', as in fact we did in § 2. This expression can be read 'the truth-value of A, given (or: in the context) I, is "true"'. In formal logic this is sometimes written '$I \models A$'. Note that, here and in the following, the discussion is restricted to propositional calculi; first- and higher-order calculi are not considered. Therefore, thanks to the completeness theorem of the propositional calculus, we need not care about the distinction between '$I \models A$' and '$I \vdash A$' (for this distinction see the refs in footnote 1).

The relationships between the truth-values of the premises and those of the conclusions are usually given by *truth tables* like those of Table II.1. They can also be compactly written as the following set of equations:

$$\mathrm{T}(\neg A|I) = 1 - \mathrm{T}(A|I), \tag{II.3a}$$

$$\mathrm{T}(A \wedge B|I) = \mathrm{T}(A|I) \times \mathrm{T}(B|I), \tag{II.3b}$$

$$\mathrm{T}(A \vee B|I) = \mathrm{T}(A|I) + \mathrm{T}(B|I) - \mathrm{T}(A|I) \times \mathrm{T}(B|I). \tag{II.3c}$$

Note that these equations rely on the interpretation of the symbols '0' and '1' as mathematical symbols and on their mathematical properties (the tables II.1, on the other hand, do not rely on this interpretation).

4. The relations between the truth-values of premises and conclusions expressed in table II.1 and eqs. (II.3) are quite standard. But we can present the reasonings that they express in a slightly different way, and modify the formal relations accordingly. Take relation (II.3b) for instance. Its form apparently indicates that the assertions of the truth or falsity of A and B are made independently; i.e., when we state B for example, we do not need to know what has been asserted about A, and vice versa. Indeed, the expression '$\mathrm{T}(B|I)$' e.g. bears no trace of the proposition A. This way of reasoning can be weakened. We can assert the truth of A and then, *given this assertion*, assert the truth or falsity of B as well. We can express this *conditional* truth-value[2] by the formula '$\mathrm{T}(B|A \wedge I)$'. It expresses the fact that the truth-value of B is referred not only to the context I, but to the truth of A as well. In the same way can we introduce the formula '$\mathrm{T}(A|B \wedge I)$'. From this point of view

[2]Not to be confused with the 'material conditional', for which see again the refs in footnote 1.

we have that if we assert A, and then, given A, we assert B, then we must necessarily also assert their conjunction $A \wedge B$. The truth table for the conjunction can then be modified in the two following equivalent ways:

$\mathrm{T}(A\vert I)$	$\mathrm{T}(B\vert A \wedge I)$	$\mathrm{T}(A \wedge B\vert I)$
0	nd	0
0	nd	0
1	0	0
1	1	1

$\mathrm{T}(B\vert I)$	$\mathrm{T}(A\vert B \wedge I)$	$\mathrm{T}(A \wedge B\vert I)$
0	nd	0
0	nd	0
1	0	0
1	1	1

where 'nd' stands for 'non-defined': it does not make sense to assign, e.g., a truth-value to B given A, if A is not given. In terms of 'conditional' truth-values, the rules (II.3) can be rewritten as follows:

$$\mathrm{T}(\neg A| I) = 1 - \mathrm{T}(A| I), \tag{II.4a}$$

$$\begin{aligned} \mathrm{T}(A \wedge B| I) &= \mathrm{T}(A| I) \times \mathrm{T}(B| A \wedge I) \\ &= \mathrm{T}(B| I) \times \mathrm{T}(A| B \wedge I), \end{aligned} \tag{II.4b}$$

$$\begin{aligned} \mathrm{T}(A \vee B| I) &= \mathrm{T}(A| I) + \mathrm{T}(B| I) - \mathrm{T}(A| I) \times \mathrm{T}(B| A \wedge I) \\ &= \mathrm{T}(A| I) + \mathrm{T}(B| I) - \mathrm{T}(B| I) \times \mathrm{T}(A| B \wedge I). \end{aligned} \tag{II.4c}$$

5. I must now hurry to make clear that what I have called 'conditional truth-value' is not a part of the logic usually taught in undergraduate courses; although Adams [1, 2] and Hailperin [313] introduce calculi which include this conditional truth-value. Something similar to it is also present in the formalisms of natural deduction or sequent calculus, for which see e.g. [46, 51, 52, 149, 261, 271, 356, 446, 581], although these formalisms concern the logical rules more from the syntactical point of view than from the semantical one [686, 688] apparently adopted here. There also seem to be approaches that try to take the context into consideration, for which see e.g. Barwise [48, 49], Gaifman [254]. The general questions related to conditional truth-values and the notion of context, together with the related studies, are quite exciting. In my opinion they are also likely to contribute to the current discussion and synthesis of the syntactic versus semantic approach to logic, for which read the innovative work and entertaining discussions by Girard [271, 272, 274, 275] (cf. also [409, 679]).

The reason why I introduced a conditional truth-value is to make a smoother shift to inductive logic, also called probability logic.

6. To summarise: formal logic, in its basic form,

- is a model — a simplification, schematisation, formalisation — of the way we reason about 'truth';
- simplifies and represents 'truth' as a dichotomic notion;
- is concerned about the relationships amongst the truth-values of premises and conclusions;
- does not prescribe any truth-values for the premises.

Note that if we intend 'true' in a more specialised sense, e.g. as 'provable' as the intuitionists do [90, 271], then the formalisation of the way we reason about it can be different and we can have, e.g., intuitionistic logic, which can be said to have three truth-values and wherein the law of excluded middle is not valid.

Plausibility logic

7. Suppose now that we want to build a model of — i.e., simplify, schematise, formalise — the way we reason about *plausibility*. I say 'plausibility', and not 'probability', because the latter word is amongst scholars so impregnated with contrasting connotations and denotations — frequency, 'propensity', chance, etc. — as to render it unsuitable for our purposes. In fact, what interests me is the everyday meaning of 'plausible', the one present in utterances like 'Yes, that's likely!'. Forget about frequency, chance, indeterminism, propensity, and other similar concepts.

For the purpose of building a model, a first characteristic of the notion of plausibility that we notice is that it comes in *degrees*, in contrast to truth which, at a basic level, has dichotomic characteristics. We say 'very plausible', 'barely plausible', 'very unlikely', and, at the extremes, 'impossible' and 'certain'. Moreover, the last two expressions are often connected with (though they are *not* synonyms for) the expressions 'false' and 'true'. This suggests two things. The first is that in our model we can represent plausibility by a *continuum*. Of course, in our everyday expressions about plausibility we do not have the resolution of a continuum; but this schematisation offers many mathematical advantages. The second thing is that two points of this continuum — those representing 'impossible' and 'certain' — must somehow correspond to our representation of 'true' and 'false'. Already at this point we see that our model for plausibility is likely to require a more complex mathematical apparatus than that for truth.

A second characteristic noted about these plausibility degrees is that they are often *ordered*. We say 'this is more plausible than that', 'that's less likely', and so on. This suggests that our modelling continuum be an *ordered* one, and to keep things simple we may take it as completely ordered. Hence a generic interval $[a, b]$ like the ordered extended real line $[-\infty, +\infty]$, or $[0, 1]$ will suit our purposes. The lower and upper limit of the interval must correspond to the notions of 'impossible' and 'certain'.

We can therefore introduce the expression '$\mathrm{P}(A|\, I)$', taking value in a for now unspecified interval, to represent the plausibility of the proposition A in the context I. Remember that in formal logic we can use beside the expression '$\mathrm{T}(A|\, I) = 1$' also '$I \models A$'; and beside '$\mathrm{T}(A|\, I) = 0$' also '$I \models \neg A$'. A notation similar to that employing '$\models$' would not be possible in plausibility logic, since we have there a continuum of possible values [cf. 314, § 2]. (But see how Hailperin [312, 313] extends the meaning of the symbol '$\models$'.)

Some authors, like Gaifman [253], Dubois and Prade [178], and perhaps de Finetti [235] (depending on how one reads his words), see the plausibility of a proposition as defined 'on top' [178, § 3.3] of its truth value. This point of view seems to me superfluous and is not the one I adopt. I rather see the plausibilistic and the truth-value structures to live beside each other. For we usually say 'It is likely that she went there', not 'It is likely that it is true that she went there'; the second proposition is just a roundabout form of the first.

8. Having found a way to represent the notion of plausibility, we must decide which rules connect the plausibility values of the premises to those of the conclusions formed by means of the logical connectives. Just as in the case of formal logic, the rules must reflect — although necessarily in a simplified and schematised way — what we consider as the 'correct' way of reasoning about plausibilities. In this first phase plausibility logic has therefore a descriptive rôle. An explicit analysis of how these rules should reflect our plausibilistic reasoning is apparently first given by Cox [138, 139] and then by Jaynes [372–374, 392] and Tribus [704, 705]; cf. also Jeffreys [398, 399] and Pólya [596–598]. I say 'explicit' because this conception of the theory of probability is already held by Maxwell, by Laplace [452, p. CLIII]:

> la théorie des probabilités n'est, au fond, que le bon sens réduit au calcul ; elle fait apprécier avec exactitude ce que les esprits justes sentent par une sorte d'instinct, sans qu'ils puissent souvent s'en rendre compte

and even by Leibniz and Jacob Bernoulli, as discussed in Hacking's studies [309–311]; see also Hailperin's [313] historical discussion. Once this analysis is done, the correct way of reasoning about plausibilities should be represented by a set of equations like (II.3) — we cannot express the rules by tables like II.1 because, to repeat myself, we are dealing here with an infinite set of degrees of plausibility. The ensuing set of equations depends on the interval we have chosen to represent the plausibilities; though all these sets are, of course, isomorphic to one another; i.e. they all represent the same reasoning. For example, if we make the convention that $\mathrm{P}(A|\,I) \in [0, 1]$ (clearly for general A and I), where '0' represents impossibility and '1' represents certainty, then the rules are

$$\mathrm{P}(A|\,I) \in [0, 1], \tag{II.5a}$$

$$\mathrm{P}(\neg A|\,I) = 1 - \mathrm{P}(A|\,I), \tag{II.5b}$$

$$\begin{aligned} \mathrm{P}(A \wedge B|\,I) &= \mathrm{P}(A|\,I) \times \mathrm{P}(B|\,A \wedge I) \\ &= \mathrm{P}(B|\,I) \times \mathrm{P}(A|\,B \wedge I), \end{aligned} \tag{II.5c}$$

$$\begin{aligned} \mathrm{P}(A \vee B|\,I) &= \mathrm{P}(A|\,I) + \mathrm{P}(B|\,I) - \mathrm{P}(A|\,I) \times \mathrm{P}(B|\,A \wedge I) \\ &= \mathrm{P}(A|\,I) + \mathrm{P}(B|\,I) - \mathrm{P}(B|\,I) \times \mathrm{P}(A|\,B \wedge I), \end{aligned} \tag{II.5d}$$

formally identical with those for truth-values in terms of conditional truths. These are the rules which we all know. With the convention that $\mathrm{P}(A|\,I) \in [-\infty, +\infty]$ instead, where '$-\infty$' represents impossibility and '$+\infty$' represents certainty, the rules take the form

$$\mathrm{P}(A|\,I) \in [-\infty, +\infty], \tag{II.6a}$$

$$\mathrm{P}(\neg A|\,I) = -\,\mathrm{P}(A|\,I), \tag{II.6b}$$

$$\begin{aligned} \mathrm{P}(A \wedge B|\,I) &= \mathrm{P}(A|\,I) + \mathrm{P}(B|\,A \wedge I) - \ln\!\left[1 + \mathrm{e}^{\mathrm{P}(A|I)} + \mathrm{e}^{\mathrm{P}(B|A\wedge I)}\right] \\ &= \mathrm{P}(B|\,I) + \mathrm{P}(A|\,B \wedge I) - \ln\!\left[1 + \mathrm{e}^{\mathrm{P}(B|I)} + \mathrm{e}^{\mathrm{P}(A|B\wedge I)}\right], \end{aligned} \tag{II.6c}$$

the rule for the disjunction having a very complicated expression. You can find that expression by yourself from the isomorphism relating the rule set (II.5) to the rule set (II.6); here it is:

$$\mathrm{P}(A|\,I) \mapsto \ln \frac{\mathrm{P}(A|\,I)}{1 - \mathrm{P}(A|\,I)}. \tag{II.7}$$

Other plausibility-representation intervals are possible, with corresponding rules; you can find some in Tribus [705], pp. 26–29. It is evident that the first set of rules is the simplest; it does not involve, e.g., transcendental functions, and has the remarkable advantage of making the plausibility of incompatible events additive. It is moreover the one we are quantitatively more accustomed to. The second set, on the other hand, would have the advantage that since the plausibility ranges between $-\infty$ and $+\infty$ we could not *confuse* or conflate plausibility with *frequency* — this confusion plagues us still today.

In any case, the representation (II.5) is the standard one and I shall not discuss any other equivalent representation any further.

9. I shall not repeat all the arguments leading to the above rules; for this I refer to the studies by Cox, Jaynes, Tribus, and the others already cited; they are an enjoyable and illuminating reading. But the argument leading to the rule (II.5c) for the plausibility of the conjunction, $A \wedge B$, is particularly compelling and quite intuitive: I present it by counterposing it to other possible rules. So consider the question of relating the plausibility $\mathrm{P}(A \wedge B|\,I)$ to the plausibilities $\mathrm{P}(A|\,I)$, $\mathrm{P}(B|\,I)$, or possibly also $\mathrm{P}(B|\,A \wedge I)$ and $\mathrm{P}(A|\,B \wedge I)$. Consider a rule identical with (II.3b) for truth logic,

$$\mathrm{P}(A \wedge B|\,I) = \mathrm{P}(A|\,I) \times \mathrm{P}(B|\,I). \tag{II.8}$$

Would this rule agree with common sense? At first sight it has some desired properties; for example, the plausibility of the conjunction is less than those of the conjuncts. But let us examine a concrete example. Take the proposition $A :=$ 'The left eye of the next person you meet is blue'. In the context I of a person living, like I, in Sweden, we could say that the plausibility of A is 1/2. Also the proposition $B :=$ 'The right eye of the next person you meet is brown' has, in the same context, a plausibility around 1/2. Now according to the rule (II.8) the plausibility of the conjunction, viz. of the proposition $A \wedge B \equiv$ 'The next person you meet has a blue left eye and a brown right one', is 1/4. But this is too much (in the context I); I personally should assign a plausibility of 1/100 or less. The rule (II.8) does not satisfy our way of assigning plausibilities. The same problem remains and gets possibly worse with similar rules like some proposed in artificial intelligence, e.g.

$$\mathrm{P}(A \wedge B|\,I) = \min[\mathrm{P}(A|\,I), \mathrm{P}(B|\,I)]. \tag{II.9}$$

According to this rule the plausibility that the next person I meet have eyes of the two different colours would be 1/2.

The standard rule (II.5c), instead, gives an answer in accordance with common sense. It requires in fact that we specify not the plausibility of *B per se*, but that of *B given A*. This plausibility I should state at 1/50, and then the plausibility of the conjunction is 1/100, which accords with my judgements.

Of course, plausibility logic describes, in a simplified way, the way in which we think an *ideal* rational person *ought to* reason. But in practise we often do not reason according to our own standards, in plausibility logic as well as in deductive logic. There are very interesting studies by Kahneman, Tversky, et al. [78, 410, 411, 718–721] on typical errors committed in plausibilistic reasoning. Example of common errors in deductive reasoning are given in the first chapters of Copi's book [136].

'Plausibilities of plausibilities', induction, and circumstances: a generalisation

10. The theory that emerges from the rules (II.5) is mathematically identical with probability theory as presented in standard textbooks, and subsumes all usual applications of probability theory. For this reason I do not think necessary to further discuss various theorems like e.g. Bayes', the theorem on total plausibility, or standard applications. For these I refer to the works by Jaynes [392], Jeffreys [398, 399], Bernardo and Smith [66], de Finetti [240, 241], Gregory [294], and to the works by Hailperin [313] and Adams [1, 2], which are very relevant yet virtually unknown to the physics community.

The import of the plausibility-logical point of view is not so much on the purely mathematical side, as on the vast new range of applications and insights that it opens. Two of these insights concern the interpretation of the parameters in various statistical models, especially the 'generalised Bernoulli' one; and the relation between plausibility and induction. These subjects are discussed in **Papers (F)** and **(G)**, the contents of which I shall hereafter assume to be known (i.e., this is an appropriate place to read those papers). My purpose here is to extend the Laplace-Jaynes approach to multiple kinds of measurements, so as to make clearer its connexion, to be presented in ch. IV, with the vector framework for the plausibilistic properties of physical systems introduced in the next chapter.

11. The extension of the Laplace-Jaynes approach, as presented in § 5 of **Paper (G)**, to multiple kinds of measurements is straightforward. We introduce different kinds of measurement, denoted by M_k. Each measurement has a set of outcomes $\{R_i \mid i \in \Lambda_k\}$; we shall often omit the indication of the index set Λ_k, since no ambiguity should arise. Different *instances* of these measurements and of their outcomes are denoted by an index '(τ)', as in the paper.

Remember that the terms 'measurement' and 'outcome' have broader denotations than usual. See § 2 of the mentioned paper on this point.)

The main question is to determine plausibilities of the form

$$\mathrm{P}(R^{(\tau_{N+L})}_{i_{N+L}} \wedge \cdots \wedge R^{(\tau_{N+1})}_{i_{N+1}} | \mathfrak{M} \wedge R^{(\tau_N)}_{i_N} \wedge \cdots \wedge R^{(\tau_1)}_{i_1} \wedge I), \tag{II.10}$$

where the symbol '$\mathfrak{M}$' has the same meaning as described in **Paper (G)**, § 2, but for the fact that it can concern different kinds of measurement. The introduction of a set of circumstances proceed as usual; only properties (II) and (III) need to be amended, as follows:

III. The amendment to the second property is clear and does not need any comments:

$$\mathrm{P}(R_i^{(\tau)} | M_k^{(\tau)} \land D \land C_j^{(\tau)} \land I) = \mathrm{P}(R_i^{(\tau)} | M_k^{(\tau)} \land C_j^{(\tau)} \land I)$$

for all τ, k, $i \in \varLambda_k$, j, and all D representing a conjunction of measurements, measurement outcomes, and circumstances of instances different from τ. (II.11)

IV. Also the amendment to the third property is quite clear:

$$\mathrm{P}(R_i^{(\tau')} | M_k^{(\tau')} \land C_j^{(\tau')} \land I) = \mathrm{P}(R_i^{(\tau'')} | M_k^{(\tau'')} \land C_j^{(\tau'')} \land I) =: p_{ij} \quad \text{for all } \tau', \tau'', \tag{II.12}$$

which mathematically expresses the fact that we consider the $M_k^{(\tau)}$, $R_i^{(\tau)}$, $C_j^{(\tau)}$, for different τ and fixed k, i, j, as 'instances' of the 'same' measurement, the 'same' outcome, and the 'same' circumstance. Thanks to this property we can use an expression like '$\mathrm{P}(R_i | M_k \land C_j \land I)$' unambiguously; it stands for

$$\begin{aligned} \mathrm{P}(R_i | M_k \land C_j \land I) &\coloneqq \mathrm{P}(R_i^{(\tau)} | M_k^{(\tau)} \land C_j^{(\tau)} \land I) \quad \text{for any } \tau, \\ &\equiv p_{ij} \quad \text{with } i \in \varLambda_k. \end{aligned} \tag{II.13}$$

12. Having made the above alterations to the properties of the circumstances, the determination of the plausibilities (II.10) proceeds along the lines of § 5.2 of **Paper (G)**. Given the plausibilities

$$\mathrm{P}(R_i | M_k \land C_j \land I) \equiv p_{ij} \qquad (\coloneqq \mathrm{P}(R_i^{(\tau)} | M^{(\tau)} \land C_j^{(\tau)} \land I) \text{ for any } \tau), \tag{II.14}$$

$$\mathrm{P}(C_j | I) \equiv \gamma_j \qquad (\coloneqq \mathrm{P}({\textstyle\bigwedge'_j} C_j^{(j')} | I) \equiv \mathrm{P}(C_j^{(\tau)} | I) \text{ for any } \tau), \tag{II.15}$$

(with $i \in \varLambda_k$), and given some data D consisting in some outcomes of various kinds of measurements,

$$D \coloneqq \underbrace{R_{i_N}^{(\tau_N)} \land \cdots \land R_{i_1}^{(\tau_1)}}_{R_i \text{ appears } N_i \text{ times}} \qquad (\text{with } i_a \in \varLambda_{k_a},\, a = 1, \dots, N), \tag{II.16}$$

the plausibility assigned to any collection of L measurement outcomes, with frequencies (L_i), is given by

$$\mathrm{P}(\underbrace{R_{i_{N+L}}^{(\tau_{N+L})} \land \cdots \land R_{i_{N+1}}^{(\tau_{N+1})}}_{R_i \text{ appears } L_i \text{ times}} | \mathfrak{M} \land D \land I) =$$

$$\sum_j \Bigl[\prod_{k, i \in \varLambda_k} \mathrm{P}(R_i | M_k \land C_j \land I)^{L_i} \Bigr] \mathrm{P}(C_j | D \land I) \equiv \sum_j \Bigl(\prod_{k, i \in \varLambda_k} p_{ij}^{L_i} \Bigr) \mathrm{P}(C_j | D \land I), \tag{II.17}$$

with

$$\mathrm{P}(C_j | D \land I) = \frac{\Bigl(\prod_{k, i \in \varLambda_k} p_{ij}^{N_i} \Bigr) \mathrm{P}(C_j | I)}{\sum_j \Bigl(\prod_{k, i \in \varLambda_k} p_{ij}^{N_i} \Bigr) \mathrm{P}(C_j | I)}. \tag{II.18}$$

13. The next step of this generalisation is to define an equivalence relation amongst circumstances. The natural choice is, of course,

Definition 1. *Two preparation circumstances C', C'' are said to be* equivalent *if they lead to identical plausibility distribution for each kind of measurement M_k:*

$$C' \sim C'' \iff \mathrm{P}(R_i | M_k \land C' \land I) = \mathrm{P}(R_i | M_k \land C'' \land I) \quad \textit{for all } k \textit{ and } i \in \Lambda_k. \tag{II.19}$$

The propositions above should really all have an index '(τ)', and the equivalence relation should be defined for each instance τ; but, thanks to the properties (I)–(IV) of the circumstances, the instantial index can be unambiguously left out.

In **Papers (F)** and **(G)** we proceeded with disjoining equivalent circumstances together and with 'plausibility-indexing' the resulting disjunctions. I shall however follow a slightly different path in the present case, to be discussed in § 39.

Miscellaneous remarks

14. We have not discussed the meanings of the three logical connectives '$\neg$', '$\land$', and '$\lor$'; and I really do not plan to discuss them either, for they should be intuitively clear; appropriate references to logic texts are given in footnote 1. Yet, whilst every logician and mathematician — even if amateurs —, and most philosophers and physicists have a clear understanding of those meanings, there are some individuals that attribute to those symbols 'non-standard'[3] meanings, apparently out of nowhere, as testified by the literature. A non-standard meaning that I have a couple of times noted in the literature is that of conceiving the conjunction as having a sort of *temporal* meaning, which it should properly not have. E.g., some people interpret the expression '$A \land B$' as implying that B is a statement about matters that *temporally precede* those of A. Others interpret it as saying that A and B concern *simultaneous* matters. All I can say in this regard is: this is not standard (not to say erroneous)! People employing 'conjunction' that way should better find another name and another symbol for it, since their idiosyncratic use clashes not only with old logical and mathematical conventions and usage, but also with engineering standards like ISO [362] and ANSI/IEEE [358] (which adopt the standard logical usage).

Analogous idiosyncrasies I have noted with respect to the conditional symbol '|', although in this case they are more common and can even form minor schools. Some people attribute to the conditional symbol '|' a *temporal* or even a *causal* meaning, interpreting '$A| B$' as saying that B comes before A or that B causes A. That this is not the case is easily seen by considering

$$A := \text{'The sky is cloudy'}, \qquad B := \text{'It is raining'}. \tag{II.20a}$$

Given that it is raining, I think you agree with me that it is very likely that the sky is cloudy as well; in symbols, this is written

$$\mathrm{P}(A | B \land I) = a, \tag{II.20b}$$

[3] In this case the boundary between 'non-standard' and 'erroneous' is very fuzzy.

where a is near 1. This expression makes complete sense. But in it the rain comes surely not before the cloudiness, nor is the rain a cause of the cloudiness (one could rather say the opposite). This very simple example shows that the conditional symbol '|' expresses a *logical*, not a temporal or causal relation here. It expresses *relevance* in respect of *knowledge*. We have another example if we consider two persons, say husband and wife, only one of whom has, in the pocket, the only key to a particular door. Suppose the husband does not know whether the key is in his own pocket. But he comes to know (e.g., by a telephone call, or reading a note on a paper) that the wife — who can be miles away — does not have the key in her own pocket. This automatically implies for the husband that the key is in his pocket. Denoting by

$$A := \text{'The husband has the key'}, \qquad B := \text{'The wife does not have the key'}, \tag{II.21a}$$

and by I the whole context (which include the assumption that none lost the key), we can rightfully write

$$\mathrm{P}(A|\, B \wedge I) = 1. \tag{II.21b}$$

But again, there are no temporal or causal connections between the facts stated by A and B here (the wife's not having the key is not the 'cause' of the husband's having it). The '|' expresses the *relevance* of the second piece of data, B, for the first, A.

I do not understand why some people would like to limit the scope of the conditional symbol '|' to temporal or causal connexions only, thus forbidding completely meaningful expressions like (II.20) and (II.21). Such relevance relations and judgements are routinely used, whether implicitly or explicitly, also in laboratory practise, where it is often the case that some detail of a set-up tells us something about some other detail, yet without being a 'cause' of the latter or 'preceding' it in time. And of capital importance is their use in communication theory, where a fundamental problem is to increase our knowledge about a *sent* signal or message, A, from knowledge of a *received* one, B. The plausibility $\mathrm{P}(A|\, B \wedge I)$ is here the fundamental quantity to be judged, and the temporal and causal arrows clearly go *from A to B*, not vice versa! Communication theory would simply be impossible if the meanings of the conditional and the conjunction symbols were restricted as those people use them.

When such people, ignoring their own idiosyncrasies, read and comment works that use standard notation, some preposterous results may arise indeed. As in the case of Brukner and Zeilinger's analysis of Shannon's entropy in quantum experiments [94]. Shannon's use of the conjunction and of the conditional symbol in his deservedly famous article [655; cf. also 656] is quite standard of course — otherwise he could not have built any theory at all on the problem of 'reproducing at one point either exactly or approximately a message selected at another point' [655, Introduction]. Concrete example: in § 12 he explicitly calculates and states that $\mathrm{P}(A|\, B \wedge I) = 0.99$, where $B :=$ 'A 0 is received' and $A :=$ 'A 0 was transmitted'. But Brukner and Zeilinger did apparently not notice this, and based their analysis on the tacit understanding that '$A|\, B$' means that the measurement related to B temporally precedes that related to A. This led them, amongst other things, to change a whole experimental set-up, in which A preceded B, into a new one in which the order of measurement was reversed — all in order to calculate quantities involving '$A|\, B$'! It is as if, in order to calculate the plausibility of a particular input given the output of a communication channel, we needed to *exchange* the input with the output; with the only result of constructing — granted the possibility of that operation — a *different* channel. Simply meaningless! A recount and more detailed analysis of this and related questions is given in **Paper (A)**.

15. We already saw that what plausibility logic does is to give us the plausibilities of some propositions, the conclusions, provided that we give those of other propositions, the assumptions. And as remarked this is completely analogous to what truth logic does. An aspect on which there has been and there still is much debate is the question of making the initial plausibility assignments. The various positions within the Bayesian community as regards this aspect can be roughly grouped into two groups. One group, the 'objectivists', maintain that the plausibility of whatever proposition A is determined by the context I, if the latter is completely specified. To this group seem to belong Jeffreys (but see [754]), Carnap, and Jaynes amongst others. The other group, the 'subjectivists', maintain that the plausibility of whatever proposition A is a matter of subjective judgement, and two persons sharing exactly the same context could nevertheless assign different plausibilities to the same proposition. To this group belong de Finetti and Savage amongst others.

I must say that the statement that 'two persons sharing exactly the same context can nevertheless assign different plausibilities' appears a bit vacuous to me, since no two persons can ever share exactly the same context. I think that it is always possible to trace a difference in plausibility assignments back to different experiences, i.e., different contexts (I have done some informal experiments upon friends on this — try yourself: just keep asking 'why?' long enough). This would seem to strengthen the thesis of the objectivists. And yet, I think it also impossible to specify a context *exactly* (in those experiments, the answers to the whys always tend to be hazier and hazier). So the objectivist programme appears somewhat vacuous as well. In fact, I am not convinced that the methods that have been proposed to 'mechanically' assign a plausibility from some kinds of contexts really achieve their purpose. However, I do not mean that those methods are not *useful*; quite the contrary. My personal position simply is that the question of the initial plausibility assignments does not concern, and cannot be answered by, plausibility theory — just like the question of the initial truth-value assignments does not lie within the jurisdiction of formal logic. Both assignment problems are certainly very important, but their settling resides at a 'meta-theoretical' level that I think cannot be fully caged into a mathematico-logical formalism. The assignment of initial plausibilities and initial truth-values is a matter of discussion and convention that lie outside any precise theory.

16. A deplorable habit of not few physicists, and not few mathematicians and logicians either, is that of neglecting the *context* in which a given plausibility assignment applies. This is very surprising as regards physicists, because they are surely the first to pay attention to, and to specify as exactly as possible, the details and boundary conditions of their experiments. The problem is that this negligence leads to errors, seeming contradictions, and false statements. An egregious case is the statement, which I have read more than once, that 'quantum mechanics violates the sum rule of probability theory', i.e., rule (II.5d):

$$\mathrm{P}(A \lor B|\, I) = \mathrm{P}(A|\, I) + \mathrm{P}(B|\, I) - \mathrm{P}(A \land B|\, I), \tag{II.5d)$_\mathrm{r}$ (}$$

especially with regard to the double-slit experiment and similar ones. Let us see what plausibility theory says about this experiment. See also Szabó's discussion [681, 682] (although he assumes a frequentist point of view).

Consider the usual set-up with two slits, called 'A' and 'B', the source of particles, the detection screen, etc. Introduce the mutually exclusive propositions

$$I_{\mathrm{A}\bar{\mathrm{B}}} \coloneqq \text{'Slit "A" is open, "B" is closed'}, \tag{II.22}$$

$$I_{\bar{\mathrm{A}}\mathrm{B}} \coloneqq \text{'Slit "B" is open, "A" is closed'}, \tag{II.23}$$

$$I_{\mathrm{AB}} \coloneqq \text{'Both slits are open'}, \tag{II.24}$$

and the propositions

$$A \coloneqq \text{'The particle passes through slit "A"'}, \tag{II.25}$$

$$B \coloneqq \text{'The particle passes through slit "B"'}, \tag{II.26}$$

$$D_x \coloneqq \text{'The particle is detected at the screen position } x\text{'}. \tag{II.27}$$

Our invariable situation will be the following:

$$I \coloneqq \text{'A particle is sent towards the slits, and is detected somewhere in the screen'}. \tag{II.28}$$

So we shall not consider the situation in which the particle does not arrive at the screen, e.g. because it did not pass through any slit. In our situation the particle must have passed through one of the slits, hence we have

$$\mathrm{P}(A \lor B|\, I) = 1, \tag{II.29}$$

and note that this holds in each of the cases $I_{\mathrm{A}\bar{\mathrm{B}}}$, $I_{\bar{\mathrm{A}}\mathrm{B}}$, I_{AB}. We also assume that we are speaking of particles that do not break into parts, so that

$$\mathrm{P}(A \land B|\, I) = 0; \tag{II.30}$$

i.e., the particle cannot pass through both slits.

Now consider the case in which both slits are open, I_{AB}. What is the plausibility that the particle is detected at x? By the rules of plausibility logic, it is

$$\begin{aligned}\mathrm{P}(D_x|\, I_{\mathrm{AB}} \land I) &= \mathrm{P}[D_x|\, (A \lor B) \land I_{\mathrm{AB}} \land I],\\ &= \mathrm{P}(D_x|\, A \land I_{\mathrm{AB}} \land I)\ \mathrm{P}(A|\, I_{\mathrm{AB}} \land I) + \mathrm{P}(D_x|\, B \land I_{\mathrm{AB}} \land I)\ \mathrm{P}(B|\, I_{\mathrm{AB}} \land I),\end{aligned} \tag{II.31}$$

i.e., the sum of the plausibility that the particle is detected at x *given that* it passes through 'A' times the plausibility that it passes through 'A', and the analogous product for 'B'. What more does plausibility theory says? *Nothing!*

To have a numerical answer we must specify the functions

$$\mathrm{P}(D_x|\, A \wedge I_{\mathrm{AB}} \wedge I) = f_{\mathrm{A}}(x), \qquad \mathrm{P}(D_x|\, B \wedge I_{\mathrm{AB}} \wedge I) = f_{\mathrm{B}}(x), \tag{II.32}$$

and the quantities

$$\mathrm{P}(A|\, I_{\mathrm{AB}} \wedge I) = p_{\mathrm{A}}, \qquad \mathrm{P}(B|\, I_{\mathrm{AB}} \wedge I) = p_{\mathrm{B}}; \tag{II.33}$$

but that is not something that plausibility theory can do. Those plausibilities must be dictated by some *physical* theory. Until this has been done, plausibility theory leaves open an infinity of possibilities; in particular, also those dictated by quantum mechanics: *plausibility theory does not exclude 'interference effects'*. So why do some people say that 'quantum mechanics violates plausibility theory'? probably what they mean is that the plausibilities (II.32)–(II.33) assigned by quantum mechanics are different from those assigned by some 'classical' physical theory.

First conclusion: quantum mechanics does not violate the rules of plausibility theory; it only assigns plausibilities that differ from those of some classical theory. But both theories respect the plausibility rules.

From the form of eq. (II.31) we also notice another fact. Since, from eqs. (II.29) and (II.30), $\mathrm{P}(A|\, I_{\mathrm{AB}} \wedge I) + \mathrm{P}(B|\, I_{\mathrm{AB}} \wedge I) = 1$, it follows that the plausibility for the particle to be detected at x can never be equal to the sum of $\mathrm{P}(D_x|\, A \wedge I_{\mathrm{AB}} \wedge I)$ (the plausibility that the particle be detected there given that it passes through 'A'), and $\mathrm{P}(D_x|\, B \wedge I_{\mathrm{AB}} \wedge I)$ (similarly, but passes through 'B'). At most, there can be a *proportionality* with factor of $1/2$ when $\mathrm{P}(A|\, I_{\mathrm{AB}} \wedge I) = \mathrm{P}(B|\, I_{\mathrm{AB}} \wedge I) = 1/2$.

But then, what about Feynman's formulae [231, § 1-1; 232, ch. 1]

$$\text{`}\, P_{12} = P_1 + P_2 \,\text{'}, \tag{II.34a}$$

$$\text{`}\, P_{12} \neq P_1 + P_2 \,\text{'?} \tag{II.34b}$$

His notation with 'P_{12}', 'P_1', and 'P_2', as is clear from his discussion, stands for

$$P_{12} \coloneqq \mathrm{P}(D_x|\, I_{\mathrm{AB}} \wedge I), \tag{II.35}$$

$$P_1 \coloneqq \mathrm{P}(D_x|\, I_{\mathrm{A\bar{B}}} \wedge I), \tag{II.36}$$

$$P_2 \coloneqq \mathrm{P}(D_x|\, I_{\mathrm{\bar{A}B}} \wedge I); \tag{II.37}$$

i.e., P_{12} is the plausibility that the particle be detected at some point *given that both slits are open*, P_1 the plausibility *given that slit 'A' is open and 'B' closed*, and P_2 the plausibility *given that slit 'B' is open and 'A' closed*. You note that these are plausibilities conditional on *incompatible* propositions; so *the expressions $P_{12} = P_1 + P_2$ and $P_{12} \neq P_1 + P_2$ have nothing to do with the sum rule of plausibility theory*, eq. (II.5d). Indeed, the following theorem of plausibility logic holds:

Theorem II.1. *Given that*

$$\mathrm{P}(D_x|\, A \wedge I_{AB} \wedge I) = f_A(x), \tag{II.38}$$

$$\mathrm{P}(D_x|\, B \wedge I_{AB} \wedge I) = f_B(x), \tag{II.39}$$

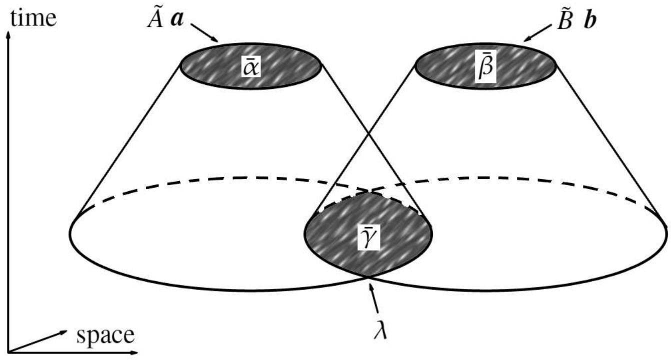

Figure II.1: Space-time diagram for the Einstein-Podolsky-Rosen thought-experiment

with $f_A(x), f_B(x) \in [0,1]$ *fixed but arbitrary, the only constraints imposed by the rules of plausibility logic to*

$$\mathrm{P}(D_x | I_{AB} \wedge I) \tag{II.40}$$

are that it be non-negative and not greater than one — i.e., no constraints at all apart the usual ones for plausibilities.

The theorem can be proven by standard linear-programming methods [151, 152, 312, 313].

The conclusion is again: statements asserting violations of plausibility theory on the part of quantum mechanics are completely false. See also the discussion by Koopman [441] and Strauß [672].

17. Another egregious misapplication of plausibility theory lies at the core of one of the most celebrated theorems of our time: Bell's theorem [54, 55, 126, 127]. This misapplication (that does not concern fair sampling or other similar arguments) is an example of those discussed in § 14; it is first pointed out by Jaynes [387], and lucidly discussed by Morgan [534] (see also Kracklauer [442–444]).

Consider the usual set-up of the Einstein-Podolsky-Rosen experiment [193] as modified by Bohm. In each of two space-like separated regions $\bar{\alpha}$ and $\bar{\beta}$ a spin measurement is performed on an 'object' — usually considered a particle of a pair, but from a field-theoretical viewpoint it should rather be a field — whose state depends on the set of 'hidden' variables λ of a region $\bar{\gamma}$ given by the intersection amongst a space-like hypersurface and the past light-cones of the regions $\bar{\alpha}$ and $\bar{\beta}$, as in Fig. II.1. In the region $\bar{\gamma}$ the two 'objects' are prepared according to a fixed procedure. We know some details of the experimental set-ups in the two measurement regions, viz. the orientations $\boldsymbol{a}$ and $\boldsymbol{b}$ of the two measuring Stern-Gerlach apparatus. What we ask is the plausibility that the measurements yield outcome $\tilde{A}$ for the measurement in region $\bar{\alpha}$ and outcome $\tilde{B}$ for that in $\bar{\beta}$. What we assume is that there is some *local* and *deterministic* theory — possibly a field theory — that describes all the fundamental *microscopic* 'hidden' variables involved in the preparation and the measurements. Note that $\boldsymbol{a}$, $\boldsymbol{b}$, $\tilde{A}$, $\tilde{B}$ are *macroscopic* parameters or quantities, i.e., they are related through some sort of 'coarse graining' (space or time averages [359, 421–425, 539–550], or Fourier transforms [623]) to the underlying fundamental microscopic quantities in the respective regions; much like pressure in relation to the positions and momenta of the particle of a gas and of its environment. The variables λ, on the other hand, are amongst the fundamental *microscopic*, 'hidden' quantities, and are not obtained by coarse graining; this means, in particular, that they cannot be affected by 'thermalisation' or similar processes.

The assumption of determinism implies that the values of the fundamental microscopic quantities in region $\bar{\alpha}$, and hence also the values of all the derived 'coarse-grained' quantities therein, are completely determined (through some functions or functionals) by the values of the microscopic quantities in earlier space-time regions. The assumption of (relativistic) locality implies that only the microscopic quantities in the past-light cone of region $\bar{\alpha}$ are necessary and sufficient for this determination. The same holds for region $\bar{\beta}$. See again Fig. II.1. In formulae,

$$\tilde{A} = f_{\tilde{A}}(\lambda, \dots), \qquad \boldsymbol{a} = f_{\boldsymbol{a}}(\lambda, \dots), \tag{II.41}$$

$$\tilde{B} = f_{\tilde{B}}(\lambda, \dots), \qquad \boldsymbol{b} = f_{\boldsymbol{b}}(\lambda, \dots), \tag{II.42}$$

where the dots stand for other microscopic variables in the non-intersecting parts of the two past-light cones. In more generic terms we can say that *knowledge of λ is in general relevant to knowledge of each of $\boldsymbol{a}$, $\boldsymbol{b}$, $\tilde{A}$, $\tilde{B}$*. Note that the equations above cannot in general be inverted to obtain the value of λ from those of $\boldsymbol{a}$, $\boldsymbol{b}$, $\tilde{A}$, $\tilde{B}$ (we should need all the microscopic quantities in the future light-cone of region $\bar{\gamma}$ to do this); but knowledge of these can imply some restrictions in the possible values that λ may have. In formulae,[4]

$$\lambda \in f_{\tilde{A}}^{-1}(\{\tilde{A}\}) \cap f_{\tilde{B}}^{-1}(\{\tilde{B}\}) \cap f_{\boldsymbol{a}}^{-1}(\{\boldsymbol{a}\}) \cap f_{\boldsymbol{b}}^{-1}(\{\boldsymbol{b}\}). \tag{II.43}$$

In other words, *the knowledge of each of $\boldsymbol{a}$, $\boldsymbol{b}$, $\tilde{A}$, $\tilde{B}$ is in general relevant to the knowledge of λ*.

Note that the assumption of determinism can be weakened: we may assume that the theory only plausibilistically determines the values of $\boldsymbol{a}$, $\boldsymbol{b}$, $\tilde{A}$, $\tilde{B}$ from those of λ. But in respect of relevance, the points made in the previous paragraphs still hold.

Let us put all this in form of propositions. Introduce the context I, which includes a description of all our assumptions and set-up, and the propositions

$$a := \text{'The apparatus in region } \bar{\alpha} \text{ is oriented along } \boldsymbol{a}\text{'}, \tag{II.44}$$

$$b := \text{'The apparatus in region } \bar{\beta} \text{ is oriented along } \boldsymbol{b}\text{'}, \tag{II.45}$$

$$\varLambda_\lambda := \text{'The 'hidden variables' in } \bar{\gamma} \text{ have values } \lambda\text{'}, \tag{II.46}$$

$$A := \text{'The result of the measurement in region } \bar{\alpha} \text{ is } \tilde{A}\text{'}, \tag{II.47}$$

$$B := \text{'The result of the measurement in region } \bar{\beta} \text{ is } \tilde{B}\text{'}. \tag{II.48}$$

[4]I should really write something like '$(f_{\tilde{A}}^{-1})_1$' to indicate that I only consider the first argument of $f_{\tilde{A}}$ for the inverse mapping, etc.; but I do not want to encumber the notation.

The plausibilities that interest us are

$$\mathrm{P}(A \wedge B | a \wedge b \wedge I). \tag{II.49}$$

This plausibility can be written as a marginalisation on the possible values of λ:

$$\mathrm{P}(A \wedge B | a \wedge b \wedge I) = \int \mathrm{p}(A \wedge B \wedge \Lambda_\lambda | a \wedge b \wedge I)\, \mathrm{d}\lambda, \tag{II.50}$$

and using the product rule we obtain

$$\mathrm{P}(A \wedge B | a \wedge b \wedge I) = \int \mathrm{P}(A | B \wedge \Lambda_\lambda \wedge a \wedge b \wedge I)\ \mathrm{P}(B | \Lambda_\lambda \wedge a \wedge b \wedge I)\ \mathrm{p}(\Lambda_\lambda | a \wedge b \wedge I)\, \mathrm{d}\lambda. \tag{II.51}$$

Up to now our analysis coincides with that of Bell; and no other assumptions than determinism and locality have been used. But at this point Bell makes the following additional assumptions:

First,

$$\mathrm{P}(A | B \wedge \Lambda_\lambda \wedge a \wedge b \wedge I) = \mathrm{P}(A | \Lambda_\lambda \wedge a \wedge I), \tag{II.52}$$

i.e., knowledge of the orientation and outcome in region $\bar{\beta}$, *given that* λ *is known*, is not relevant to knowledge of the outcome in $\bar{\alpha}$. This is acceptable for the following reason. Knowledge of $\boldsymbol{b}$ and $\tilde{B}$ is relevant in that it can restrict the possible value of λ and of the other microscopic quantities in the non-intersecting part of past light-cone of $\bar{\beta}$. But λ is already known, so knowledge of its restrictions is superfluous; and the restrictions on the other microscopic quantities are, by locality, irrelevant for region $\bar{\alpha}$, since they lie outside its past light-cone. Therefore $\boldsymbol{b}$ and $\tilde{B}$ cannot tell us anything new for $\boldsymbol{a}$ and $\tilde{A}$. Note, however, that had λ denoted only *part* of the microscopic quantities in $\bar{\gamma}$, then $\boldsymbol{b}$ and $\tilde{B}$ would have been relevant and Bell's assumption (II.52) would not have held.

Bell's second assumption is that

$$\mathrm{P}(B | \Lambda_\lambda \wedge a \wedge b \wedge I) = \mathrm{P}(B | \Lambda_\lambda \wedge b \wedge I), \tag{II.53}$$

which is also acceptable for a reasoning analogous to that of the first assumption, provided that λ denotes all the microscopic quantities in $\bar{\gamma}$.

Bell's third assumption,

$$\mathrm{p}(\Lambda_\lambda | a \wedge b \wedge I) = \mathrm{p}(\Lambda_\lambda | I), \tag{II.54}$$

is however untenable. It says that knowledge of $\boldsymbol{a}$ and $\boldsymbol{b}$ is irrelevant for the knowledge of λ. But we have seen from eqs. (II.41), (II.42), and especially (II.43) that knowledge of $\boldsymbol{a}$ and $\boldsymbol{b}$ can impose restrictions on the values of λ *and is therefore relevant to its knowledge*. This is the meaning of leaving '$a \wedge b$' in the context of '$\mathrm{P}(\Lambda_\lambda | a \wedge b \wedge I)$'. Bell's assumption is unwarranted and, in general, unphysical. In general, it contradicts the assumption of determinism. Not only in the case of strict determinism, but also in the case of a plausibilistic relation between λ and $\boldsymbol{a}$, $\boldsymbol{b}$.

Many physicists suddenly turn into mystics when it comes to Bell's last assumption above. They say: 'But the propositions a and b concern the orientation of the apparatuses, which may be chosen by *us*; you mean then that we have no free will in our decisions?' — Of course I do! or have they perhaps forgotten that our assumption was *determinism*? They must decide: do they want this theorem to concern local deterministic physical theories, or to concern a religious question? What will their next assumption invoke? the Holy Trinity? Here lies another, perhaps deeper problem: the fact that we physicists often take a philosophical pose, but our philosophical remarks are usually naïve and cheap, no better than those of a gymnasial student making the first reflections on human nature. Indeed, the question of 'free will' was definitively shown by Wittgenstein [745–747] to be simply void, only a *Sprachspiel*.

So we cannot accept the last assumption, since in general it contradicts one of the two basic assumptions of the theorem. Once it is discarded, our sought plausibility takes the final form

$$\mathrm{P}(A \wedge B|\, a \wedge b \wedge I) = \int \mathrm{P}(A|\, \mathit{\Lambda}_\lambda \wedge a \wedge I)\ \mathrm{P}(B|\, \mathit{\Lambda}_\lambda \wedge b \wedge I)\ \mathrm{p}(\mathit{\Lambda}_\lambda|\, a \wedge b \wedge I)\, \mathrm{d}\lambda, \quad \text{(II.55)}$$

and it can be shown that the inequalities obtainable from this decomposition have an upper bound of 4 (the maximum possible), instead of the usual 2 presented in the literature [534]. The conclusion is that (a) Bell's theorem, as is usually stated and taught, proves nothing about general local deterministic 'hidden-variable' theories; (b) the various experiments [23, 24] that confirm (pace loopholes) a violation of Bell's inequalities, have nothing to say as regards the exclusion of general local deterministic 'hidden-variable' theories, since they only show a violation of the bound of 2, not 4 as required in our derivation. The latter bound, however, is the maximum possible and cannot be violated by any experiment — in the same sense in which no experiment can ever show a relative frequency greater than 1.

III. A mathematical framework for the plausibilistic properties of physical theories

What is a physical theory?

18. The official place that plausibility logic has in natural philosophy is very prominent today — I say 'official' because, at least at an intuitive and informal level, plausibility logic has always been an essential element in the mathematical study of nature. Only the idea of trying to list some research areas primarily based on plausibility logic seems meaningless: these areas are too many, and there are no well-defined boundaries between them.

Wanting to make a sort of schema of the ways in which plausibility logic is used in a physical theory, we should need a general idea of what a physical theory is. Let me first quote this beautiful passage from Truesdell [714, Prologue]:

> A theory is a mathematical model for an aspect of nature. One good theory extracts and exaggerates some facets of the truth. Another good theory may idealize other facets. A theory cannot duplicate nature, for if it did so in all respects, it would be isomorphic to nature itself and hence useless, a mere repetition of all the complexity which nature presents to us, that very complexity we frame theories to penetrate and set aside.
>
> If a theory were not simpler than the phenomena it was designed to model, it would serve no purpose. Like a portrait, it can represent only a part of the subject it pictures. This part it exaggerates, if only because it leaves out the rest. Its simplicity is its virtue, provided the aspect it portrays be that which we wish to study. If, on the other hand, our concern is an aspect of nature which a particular theory leaves out of account, then that theory is for us not wrong but simply irrelevant. For example, if we would analyse the stagnation of traffic in the streets, to take into account the behavior of the elementary particles that make up the engine, the body, the tires, and the driver of each automobile, however "fundamental" the physicists like to call those particles, would be useless even if it were not insuperably difficult. The quantum theory of individual particles is not wrong in studies of the deformation of large

> samples of air; it is simply a model for something else, something irrelevant to matter in gross.

What I need for the present discussion is to try to identify some general features that all physical theories and models have in common. One of these features, which also characterises truth logic and plausibility logic, is the following: a physical theory or model is something which, given some — possibly hypothetical — knowledge about particular facts, like observations, set-ups, etc., provides us with — possibly hypothetical — knowledge about other related facts.[1] We can call these knowledges 'a priori' and 'a posteriori'; but I must hurry to remark that these adjectives do not refer to *temporal* characteristics of the objects or facts of knowledge. In fact, the 'a priori' knowledge may e.g. engage quantities at a time t_0 and the 'a posteriori' one quantities at a time t_1 with $t_1 < t_0$. I shall sometimes also use the terms 'initial' and 'final', but the same remark holds for them as well — even more. Chronologically more neutral terms, which I shall also use, are 'input' and 'output'; they unfortunately suggest that some sort of 'connecting algorithm' is available, which is not always the case.

Exemplifying interlude (which also introduces some notation)

19. To make the very general characterisation of a physical theory given in § 18 more concrete, let me give some descriptive, but not yet mathematical, examples of 'classical' theories.[2] I shall then make one of these examples mathematically explicit in § 20.

In 'classical' theories we usually have a basic system of equations, which includes *general laws* — expressing e.g. principles of balance or conservation — and possibly some constitutive equations. By specifying additional particular constitutive equations as well as initial- and boundary-value data, we obtain a system of integro-differential equations with a unique solution. This usually describes the 'motion' of a particular set of quantities. Note that the term 'motion' can generally mean (e.g., in relativistic theories) the specification of the resulting quantities in given space-time regions.

From the point of view of the general characterisation given in the previous section, the additional constitutive equations and the boundary conditions represent our 'a priori' knowledge, and the solution of the equations our 'a posteriori' knowledge. In this example

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[1] Göran Lindblad amusingly remarked at a seminar of mine that the above characterisation could also be given of religion; and he is surely right. One could therefore add that the knowledge provided by a model is the distillate and often the generalisation of experience and observations, not an ecstatic revelation sent by the gods. Surely there are many other qualifications that ought to be given in trying to describe what a physical model is. But my humble purpose is only to try to motivate the mathematical framework to be introduced presently, not to start a discussion of philosophy of science. For such discussions I can but refer to greater and lesser masters like Duhem [181], Poincaré [594, 595], Truesdell [708, 713] (see also [709] and the introductions in [712, 714–716]), Bunge [102, 104] (see also [99, 100, 103]), and others.

[2] Unfortunately many physicists (especially quantum mechanicians) that occupy themselves also of 'foundations' seem to always have in mind the equation 'classical (Galilean-relativistic) physics' = 'classical mechanics of point particles'; thus leaving out the greatest and most complex part of classical physics, the continuum electromagnetothermomechanical theories [e.g., 74, 158, 159, 197, 203, 206, 207, 213, 214, 216, 217, 509, 634, 660, 710, 712, 715, 716, 733, 734], of which analytical mechanics is only a special case.

both types of knowledge are precise and unique, or we could say *certain*. This is often the meaning of the adjective 'deterministic' applied to these theories.

But the analysis can be generalised. If we specify only a part of the necessary constitutive equations or a part of the boundary values, then the solution is no longer unique but belongs to a more or less restricted class of possible solutions. This class represents in this case our a posteriori knowledge, even if it is not as detailed as that represented by a unique solution. The next step of this generalisation is to consider boundary data (or even constitutive equations) that are *uncertain*, introducing some plausibilities for them. The solutions will then also have a plausibilistic nature.

It is easy to formulate concrete examples. The first and surely the most familiar to quantum physicists is that of a 'classical' system of point particles whose interaction is specified either by assigning a system of forces, or a Hamiltonian or Lagrangian function, or a more general evolution operator. The a priori knowledge usually pertains a particular value for the set of positions and velocities (and, in rare cases, even accelerations) of the particles at a given instant; the a posteriori knowledge pertain a whole and unique *trajectory* for the positions of the particles, for preceding and subsequent instants. But we can also specify a given time interval (positive or negative) amongst the a priori data, and in this case the a posteriori knowledge may simply concern the value of the set of positions, velocities, etc., after or before such a time interval. Thus this example also shows that there are no precise prescriptions as to what is 'a priori' and what is 'a posteriori' knowledge: this division depends on what we already know ('a priori') and what we ask ('a posteriori'). The generalisation of this example leads to statistical mechanics [603]. If our a priori knowledge consists not in a precise value for the set of positions etc., but in a set of possible ones with assigned plausibilities, then the a posteriori knowledge will consist in a set of possible trajectories with particular plausibilities.[3]

An example from more general modern continuum theories is provided by a body on which the fields of stress, (free) energy, heating flux, and entropy (and possibly electromagnetic fields) are defined and satisfy appropriate balance laws, as well as additional constitutive equations that characterise a particular material. The a priori knowledge generally consists in the *history* (or equivalence classes of histories) of the deformation and temperature fields of the body. Required is the a posteriori knowledge of the motion of the other fields. Plausibilistic generalisations of this example are not so common, see e.g. Beran [65]; yet they are conceptually straightforward, even if mathematically demanding.

20. Let us rephrase the point-particle example of § 19 in mathematical notation. The positions, velocities or momenta, etc. of the particles can be called 'phase coordinates' and

[3]The plausibilities for the initial data are usually called — by many still today, unfortunately — an 'ensemble'; a term that dates back to the days in which many physicists were still confused about what plausibility is, and needed to imagine an infinity of fictive copies of the physical situation under study. Today we do not need such fictions, and that term causes only confusion. We have only *one* physical situation — the one under our senses — and all we are doing is making plausibility judgements about some unknown details of its. Note that by 'ensemble' I do not mean a *real* collection of a *finite* number of systems or objects; I call this an 'assembly', a term proposed by Peres [584]. Unfortunately the literature witnesses also a confusion about 'ensemble' and 'assembly' [130, 131, 478, 479, 691].

denoted by z. Their space[4] is Γ. The dynamics is specified by a mapping, or 'evolution operator',

$$\boldsymbol{U}\colon (t_0, t_1) \mapsto \boldsymbol{U}_{t_0,t_1}, \qquad \boldsymbol{U}_{t_0,t_1}\colon z_0 \mapsto \boldsymbol{U}_{t_0,t_1}(z_0) \tag{III.1}$$

that maps the phase point z_0 at time t_0 to the point $\boldsymbol{U}_{t_0,t_1}(z_0)$ at time t_1, and satisfies the usual Chapman-Kolmogorov law [125, 507]

$$\begin{gathered} \boldsymbol{U}_{t_0,t_1} \circ \boldsymbol{U}_{t_1,t_2} = \boldsymbol{U}_{t_0,t_2}, \\ \boldsymbol{U}_{t,t}(z) = z. \end{gathered} \tag{III.2}$$

We assume we have all needed smoothness. The evolution operator can represent a solution of the equations of motion $\dot{z}(t) = \boldsymbol{v}[z(t), t]$ for a time-dependent vector field $\boldsymbol{v}(z, t)$. The a priori knowledge then consists in a pair of values t_0, z_0, and the a posteriori knowledge consists in the motion $t \mapsto \boldsymbol{U}_{t_0,t}(z_0)$ or, if a particular time t_1 is specified (and remember that we could have $t_1 < t_0$), simply in the phase coordinate $z_1 = \boldsymbol{U}_{t_0,t_1}(z_0)$.

All this can be rephrased in a language appropriate to plausibility logic. We must keep in mind that the phase coordinates z at a time t represent the possible 'outcomes' $\{R^{(t)}_z \mid z \in \Gamma\}$ of a particular 'measurement' $M^{(t)}$ performed at that time[5]. What I call 'measurement' is not always an observation: sometimes it is an active *selection*. Our a priori knowledge C_c can be expressed by saying that the proposition $R^{(t_0)}_{z_0}$, given $M^{(t_0)}$, is true or *certain*:[6]

$$\mathrm{P}(R^{(t_0)}_{z_0} \,|\, M^{(t_0)} \wedge C_\mathrm{c} \wedge I) = 1, \tag{III.3}$$

or better, in terms of a plausibility distribution,

$$\mathrm{p}(R^{(t_0)}_{z} \,|\, M^{(t_0)} \wedge C_\mathrm{c} \wedge I)\,\mathrm{d}z = \delta(z - z_0)\,\mathrm{d}z. \tag{III.4}$$

The knowledge implicit in the equations of motions (which is included in the proposition I) can be expressed in the same logical terms. The equations say that given the data $R^{(t_0)}_{z_0} \wedge M^{(t_0)}$ (irrespectively of C_c), and given that we perform a measurement $M^{(t_1)}$ at time t_1, we are certain to obtain the outcome $R^{(t_1)}_{z_1}$ such that $z_1 = \boldsymbol{U}_{t_0,t_1}(z_0)$:

$$\mathrm{p}(R^{(t_1)}_{z} \,|\, M^{(t_1)} \wedge R^{(t_0)}_{z_0} \wedge M^{(t_0)} \wedge I)\,\mathrm{d}z = \delta[z - \boldsymbol{U}_{t_0,t_1}(z_0)]\,\mathrm{d}z. \tag{III.5}$$

These two pieces of knowledge together lead to our a posteriori knowledge, simply by the rules of plausibility theory (II.5) (more specifically, the theorem on total plausibility):

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[4] I would only say 'manifold', since no additional structures are assumed for Γ in this example.

[5] Which also implies the tacit performance of a time measurement.

[6] Note that the derivations that follow, including the integrations and the products of deltas, are mathematically rigorous, even if some details are left out. I am using Egorov's theory of generalised functions [164, 185, 186] (see also [162, 163, 471, 566]), which means that the deltas are implicitly specified by appropriate sequences.

given C_c, the plausibility that a measurement at time t_1 yields an outcome within $\mathrm{d}z$ is

$$\begin{aligned}
&\mathrm{p}(R_z^{(t_1)} | M^{(t_1)} \land C_c \land I)\,\mathrm{d}z = \\
&\quad\int_\Gamma \mathrm{p}(R_z^{(t_1)} | M^{(t_1)} \land R_{z'}^{(t_0)} \land M^{(t_0)} \land C_c \land I)\ \mathrm{p}(R_{z'}^{(t_0)} | M^{(t_0)} \land C_c \land I)\,\mathrm{d}z'\,\mathrm{d}z = \\
&\qquad\int_\Gamma \delta[z - \boldsymbol{U}_{t_0,t_1}(z')]\,\delta(z' - z_0)\,\mathrm{d}z'\,\mathrm{d}z = \delta[z - \boldsymbol{U}_{t_0,t_1}(z_0)]\,\mathrm{d}z; \qquad \text{(III.6)}
\end{aligned}$$

in other words, we are *certain* of the outcome $z_1 := \boldsymbol{U}_{t_0,t_1}(z_0)$.

Up to now we could have comfortably done without plausibility theory: eqs. (III.4)–(III.6) have only restated in a cumbersome way what was basically already implicit in eqs. (III.1) and (III.2). But let us now consider the generalised case with a priori data C_u upon which knowledge of a specific z_0 at time t_0 is *uncertain*. We have a plausibility distribution

$$\mathrm{p}(R_z^{(t_0)} | M^{(t_0)} \land C_u \land I)\,\mathrm{d}z = f_{t_0}(z)\,\mathrm{d}z \qquad \text{(III.7)}$$

for some specific normalised positive generalised function f_{t_0}. The knowledge provided by the equations of motion is unaltered,

$$\mathrm{p}(R_z^{(t_1)} | M^{(t_1)} \land R_{z_0}^{(t_0)} \land M^{(t_0)} \land I)\,\mathrm{d}z = \delta[z - \boldsymbol{U}_{t_0,t_1}(z_0)]\,\mathrm{d}z. \qquad \text{(III.5)}_\mathrm{r}$$

To derive our a posteriori knowledge, i.e. the answer to the question 'if we perform a measurement $M^{(t_1)}$ at time t_1, with which plausibility can be obtain a result around $R_z^{(t_1)}$?', we really need this time the rules of plausibility theory:

$$\begin{aligned}
&\mathrm{p}(R_z^{(t_1)} | M^{(t_1)} \land C_u \land I)\,\mathrm{d}z = \\
&\quad\int_\Gamma \mathrm{p}(R_z^{(t_1)} | M^{(t_1)} \land R_{z'}^{(t_0)} \land M^{(t_0)} \land C_u \land I)\ \mathrm{p}(R_{z'}^{(t_0)} | M^{(t_0)} \land C_u \land I)\,\mathrm{d}z'\,\mathrm{d}z = \\
&\qquad\int_\Gamma \delta[z - \boldsymbol{U}_{t_0,t_1}(z')]\,f_{t_0}(z')\,\mathrm{d}z'\,\mathrm{d}z = f_{t_0}[\boldsymbol{U}_{t_0,t_1}^{-1}(z)]\left\|\frac{\partial \boldsymbol{U}_{t_0,t_1}^{-1}}{\partial z}\right\|\mathrm{d}z. \qquad \text{(III.8)}
\end{aligned}$$

That is, defining $f_{t_1}(z)\,\mathrm{d}z := \mathrm{p}(R_z^{(t_1)} | M^{(t_1)} \land C_u \land I)\,\mathrm{d}z$, our final knowledge is simply expressed by the plausibility distribution

$$\begin{aligned}
\mathrm{p}(R_z^{(t_1)} | M^{(t_1)} \land C_u \land I)\,\mathrm{d}z &= \mathrm{p}\Big[R^{(t_0)}_{\boldsymbol{U}_{t_0,t_1}^{-1}(z)}\Big| M^{(t_0)} \land C_u \land I\Big]\left\|\frac{\partial \boldsymbol{U}_{t_0,t_1}^{-1}}{\partial z}\right\|\mathrm{d}z, \qquad \text{or} \\
f_{t_1}(z)\,\mathrm{d}z &= f_{t_0}[\boldsymbol{U}_{t_0,t_1}^{-1}(z)]\left\|\frac{\partial \boldsymbol{U}_{t_0,t_1}^{-1}}{\partial z}\right\|\mathrm{d}z.
\end{aligned} \qquad \text{(III.9)}$$

We recognise in this equation the most general form of Liouville's equation, which in terms of the vector field $\boldsymbol{v}$ previously introduced is equivalent to the more common form

$$\partial_t f_t(z) = -\boldsymbol{\nabla}_z \cdot [\boldsymbol{v}(z,t)\,f_t(z)] \qquad \text{(III.10)}$$

[cf. 19, 20, 223, 224, 282, 475, 587, 611, 612, 717].

Formula (III.9) represents the answer to our question for a particular time t_1; but can also be interpreted as a set of answers for different such times t_1. It is in a sense somehow redundant; for instead of asking, for each t, 'What is the plausibility of obtaining the outcome $R_z^{(t)}$?', we can directly ask: 'What is the plausibility of a particular phase trajectory?'. Denoting: a trajectory by $\zeta : t \mapsto z = \zeta(t)$, the 'measurement' of the trajectory (i.e., of the history of the system) by M, and the outcome consisting in a particular trajectory ζ by R_ζ, the answer to our question above is

$$\text{‘}\ \mathrm{p}(R_\zeta | M \wedge C_\mathrm{u} \wedge I)\,\mathrm{d}\zeta = \delta[\zeta - \boldsymbol{U}_{t_0,\cdot}(z_0)]\, f_{t_0}(z_0)\,\mathrm{d}\zeta\ \text{’}, \tag{III.11}$$

which I have put within quotation marks since it requires analytical and topological care. The measure $\mathrm{p}(\dots)\,\mathrm{d}\zeta$ (as well as the delta) is in fact defined over a trajectory space, i.e., integrations in respect of this measure are *path integrals*.

It would seem that there is no much difference between the two questions above, the one about individual times and the one about trajectories. And indeed conceptually there is not. Yet, after the grounding work of Boltzmann, Maxwell, and Gibbs, it took so long time for statistical mechanics to proficuously attack non-equilibrium processes precisely because the point of view and the question asked had always been restricted to the possible phase coordinates of the system, instead of the possible *motions* of the system, — an approach that is clearly untenable as soon as the relation between trajectories and phase points at a given time ceases to be bijective. Today, especially after the work of Jaynes, statistical mechanics is based on 'ensembles' (plausibility distributions) defined not in phase space, but in *path space*. Much could be said on this extremely interesting subject; unfortunately, for reasons of space and time, I can only refer to the work of Jaynes [376, 379, 381, 385] and many others' like Mori, McLennan, Lewis, Zwanzig, Hobson, Robertson, Zubarev and Kalashnikov, Grandy, Baker-Jarvis, Gallavotti, Maes, Dewar, et multi alii [27–30, 32, 34, 142, 161, 167–169, 174, 175, 184, 196, 220, 255, 256, 265, 285, 287–289, 344–346, 369, 395, 459, 469, 470, 496–500, 515–517, 531, 535, 572, 616–618, 620–622, 637, 639, 664, 690, 730, 757, 760–762].

21. Before continuing, I should like to offer a remark concerning locutions like 'temporal evolution of the plausibility distribution' or 'the equation of motion of the plausibility distribution' so often used in statistical mechanics. Our a priori knowledge ($C_\mathrm{u} \wedge I$ in the above equations) is always the same; and the plausibility distributions simply concern questions that regard different times. These plausibilities are assigned as soon as we *perform the calculations*, and as soon as these are done our knowledge and our plausibilities do not change a iota, independently of how long the system has evolved. Thus saying that the plausibility 'evolves in time' is somehow inappropriate and misleading. An example may perhaps illustrate what I mean: If we *now* read and learn a timetable for a local bus, we know where the bus stops at particular times; but we know it all *now* — we do not acquire that knowledge 'along the bus' trip'. We know *now* that the buss will stop at Baker Street at 12 o'clock, we are not 'going to know it' at 12 o'clock. Our knowledge is not 'evolving'. The sentence that 'the plausibility evolves in time' conceals a conception of plausibility as a sort of physical thing — which it is not. The same remark also holds for 'wave-functions' and other mathematical objects whose rôle is only that of encoding plausibilities (see ch. III).

Three special kinds of propositions. Definition of 'system'. Insights

22. The derivation of the general mathematical setting for statistical mechanics given in § 20 is not the one commonly found in textbooks, even though its imports are roughly the same. It was as near as possible to the point of view of plausibility logic, and the notation introduced there will be further used in the following discussion.

Let us now proceed to the construction of a logico-mathematical framework which make allowance for the characterisation, given in § 18, of a physical theory. I introduce three kinds of propositions to be used in the plausibilistic description of a physical theory or model. They will generally be denoted by the symbols $\bar{S}$, M, and R. The propositions $\bar{S}$ will be called *states* or *preparations*, and will be meant to express part of our a priori knowledge. The propositions M will be called *measurements*, and will represent additional a priori knowledge. Finally, the propositions R will be called *outcomes* and will represent those details about which we have some a posteriori knowledge. The difference between the '$\bar{S}$-' and 'M-propositions' is that the latter delimit the scope of the a posteriori knowledge required; so to speak, they define and confine the particular 'question' we are asking. The propositions R are the possible 'answers' to such questions.

When a given 'system' is considered, the set of possible states and the sets of possible measurements and outcomes are automatically circumscribed. Here I want to take the opposite point of view, which is much more profitable:

Definition 2. *A set of states* $\{\bar{S}_j\}$ *and a set of measurements* $\{M_k\}$ *with the relative outcomes* $\{R_i\}$ *together* define *the physical 'system' under study.*

(Cf. Bridgman's 'universe of operations' [85].) This definition is profitable for at least two reasons. One is that some people sometimes indicate a system by indicating a *body* or collection of bodies, or sometimes a region of space. But a body (or a space region) may be studied in many different ways, and can correspond to different *systems*, depending on whether we are interested in its mechanical, or thermodynamical, or electromagnetic, etc., properties (cf.Jaynes [375, § 1.2]) — we cannot be interested in all of its properties at once (and moreover it can possess properties that have not yet been discovered). The other reason is that the laws governing a system are meaningful and correct only in respect of given sets of quantities, variables, and processes. If these sets are altered the laws may become meaningless or incorrect. There are at least three illustrious examples of discussions (which have now become tedious) about 'paradoxes' that originated simply because of carelessness in defining the system and in inspecting whether certain laws were really meant to apply to *that* kind of system:

- The 'Gibbs paradox' [266, pp. 166–167],[7] one version of which being that the entropy of mixing of two gases varies discontinuously as the gases gets chemically

[7] Gibbs did not use the word 'paradox'.

'more similar' and eventually identical.[8] The point missed by the enthusiasts of this version of the 'paradox' is that the *system* they consider provides for *no* such process of chemical change, so their very formulation of the 'paradox' is meaningless. In a system — a *different* system — which made allowance for such a process, there would then also be an additional related parameter; the entropy function and the entropy of mixing would depend on it and would therefore have different expressions (there would be a sort of entropy of convection), would satisfy different balance laws, and would be governed by different evolution equations. There are in fact thermodynamic theories and systems that describe such processes, with no 'paradox'; see e.g. Faria [226, 227].[9]

- A putative argument by von Neumann [552, § V.2] and Peres [584, § 9-4] stating that if two non-one-shot-distinguishable states of a quantum system could be distinguished in one shot, a violation of the second law could follow. The point missed here is that, if we consider a given *system* and say that two of its possible states cannot be distinguished in one shot, then evidently the system by definition does *not* admit measurements or processes that can distinguish those states in one shot. Then why would we entertain such a non-admitted process? It is clear that *logical* contradictions then arise; a derivation of 'physical' consequences is then only a vacuous exercise (from contradictory assumptions *any* proposition can be derived) and has no physical meaning at all. Admitting such a kind of distinguishing process only means that you are considering a *different* system, in which those states *are*, by definition, one-shot distinguishable, and for which the entropy function and evolution equations have different forms. Analyses of von Neumann's and Peres' inconsistent argument, from partially different points of view, have been given in **Paper (B)** and **Paper (E)**. In a paper in preparation I also show that von Neumann's and Peres' putative argument is valid for classical mechanics as well; hence the discussion is not peculiar to quantum mechanics.

- 'Maxwell's demon' [512, pp. 338–339; 693]. In this case one imagines having an envelope with two samples, A and B, of a gas at the same temperature and separated by a diaphragm. In the diagram there is a small hole and 'a being, who can see the individual molecules, opens and closes this hole, so as to allow only the swifter molecules to pass from A to B, and only the slower ones to pass from B to A. He will thus, without expenditure of work, raise the temperature of B and lower that of A, in contradiction to the second law of thermodynamics' [512, ibid.]. The amount of discussion about this observation has been enormous, even with volumes

[8] This is only one of the versions in which this 'paradox' is formulated. The versions are sometimes presented as equivalent but are in fact different — which contributes to the confusion.

[9] In all versions of this 'paradox' (see preceding footnote) it has been shown that (a) there is no paradox, but only confusion and carelessness in formulating the argument; (b) quantum mechanics has nothing to do with the 'resolution' of the 'paradox'. See the analyses by Gibbs himself [266, p. 166], Larmor [456, § 59], Schrödinger [640], Bridgman [85, pp. 168–169], Grad [284], Boyer [75], van Kampen [413], Jaynes [390], who have to a greater or lesser degree clarified one or another version of the 'paradox' (though sometimes at the detriment of the clarity of other versions). See also [173, 188, 286, 435, 585].

specially dedicated to it; a very small sample being [64, 86, 87, 95, 119, 123, 182, 183, 418, 436, 564, 575, 588, 630, 659, 662, 694, 756]; cf. also the references for 'Gibbs' paradox' above. Amongst these discussions, I find Earman and Norton's [182, 183] the most lucid. But the simple point here is again that the *system* for which the second law is stated (which is characterised by measurements of the volumes, temperatures, and pressures of the two gas samples) does *not* admit any observations of individual molecules. If such kinds of measurement are allowed, we have then a *different* system, for which the entropy and heat functions either have different expressions and therefore satisfy a different form of the second law, or are not defined at all. I think this is the point made by Maxwell himself immediately after stating his example [512, p. 339]:

> This is only one of the instances in which conclusions which we have drawn from our experience of bodies consisting of an immense number of molecules may be found not to be applicable to the more delicate observations and experiments which we may suppose made by one who can perceive and handle the individual molecules which we deal with only in large masses.

I.e., different sets of measurements ('more delicate observations and experiments') define different systems — even if these systems concern the same physical body —, and different systems may satisfy different laws ('conclusions . . . may be found not to be applicable').

23. The propositions representing states, measurements, and outcomes introduced in the previous section are required to satisfy some properties which reflect some characteristics of those notions. All these properties can be stated in terms of plausibilities:

(I) The states are mutually exclusive and, in the majority of problems, also exhaustive:

$$\mathrm{P}(\bar{S}_{j'} \land \bar{S}_{j''} | I) = 0 \quad \text{if } j' \neq j'', \tag{III.12a}$$

$$\mathrm{P}(\bigvee_j \bar{S}_j | I) = \sum_j \mathrm{P}(\bar{S}_j | I) = 1. \tag{III.12b}$$

Note that the plausibilities of each state are unspecified, of course.

(II) Also the measurements are mutually exclusive and, in the majority of problems, exhaustive as well:

$$\mathrm{P}(M_{k'} \land M_{k''} | I) = 0 \quad \text{if } k' \neq k'', \tag{III.13a}$$

$$\mathrm{P}(\bigvee_k M_k | I) = \sum_k \mathrm{P}(M_k | I) = 1. \tag{III.13b}$$

Also in this case the plausibilities of each measurement are not specified.

(III) To each measurement M_k is associated a unique set of outcomes $\{R_i \mid i \in \varLambda_k\}$ which are, given the measurement, mutually exclusive and exhaustive:

$$\mathrm{P}(R_{i'} \land R_{i''} | M_k \land I) = 0 \quad \text{if } i' \neq i'', \tag{III.14a}$$

$$\mathrm{P}(\bigvee_i R_i | M_k \land I) = \sum_i \mathrm{P}(R_i | M_k \land I) = 1, \tag{III.14b}$$

As already appears from these equations, we shall usually omit to indicate to which measurement a particular outcome R_i belongs (which would be otherwise indicated by '$i \in \varLambda_k$'), since it is usually clear which is the measurement intended.

Requirement (I) expresses the fact that we can prepare (or select) a system in a certain way or in another, but not in both. You perhaps would argue against this requirement as follows: 'I can prepare, say, a rigid body so that it has a given position $\boldsymbol{x}$ (preparation $\bar{S}_1$), or so that it has a given moment of momentum $\boldsymbol{L}$ (preparation $\bar{S}_2$); but also in both ways, so that it has position $\boldsymbol{x}$ and moment of momentum $\boldsymbol{L}'$ (preparation $\bar{S}_1 \land \bar{S}_2$) . But if you rephrase your statement more carefully you see that it is not true. Ask yourself: in the preparation $\bar{S}_2$, what is the position? Either it has some value $\boldsymbol{x}'$, or it is unknown. If it is unknown, you cannot have both $\bar{S}_2$ and $\bar{S}_1$, since in the latter the position is *known*, and it cannot, of course, be both unknown and known. If it is known to be $\boldsymbol{x}'$, then you can have both preparations only if (1) the moment of momentum in $\bar{S}_1$ is also known, say with value $\boldsymbol{L}'$, and (2) $\boldsymbol{x} = \boldsymbol{x}'$ and $\boldsymbol{L} = \boldsymbol{L}'$. But this means that $\bar{S}_1$ and $\bar{S}_2$ are the *same* preparation, and then $\bar{S}_1 \equiv \bar{S}_2 \equiv \bar{S}_1 \land \bar{S}_2$.

Requirement (II) expresses the fact that we can perform a particular measurement, or another one, but not both, in analogy with the requirement for the states. Against the present requirement, though, reasonable arguments can be levelled. For we can imagine e.g. a measurement M_1 with three outcomes, and then another measurement M_2 which is identical to the first but for the fact that two of the outcomes are 'grouped together' and considered as one — more precisely, we take their disjunction. The measurement M_2 is called a 'coarsening' of M_1 (cf. [333] and see § 24). What is in this case the status of the conjunction $M_1 \land M_2$? The formal position I assume here is that the specification of a measurement includes also a 'description' of the possible outcomes, including their number. So in our example the conjunction $M_1 \land M_2$ is impossible, because we can either perform a measurement with three outcomes, or one with two, but not both. This is only one choice, but I have noticed that it has many advantages.

You have noticed that the requirement (III.14) for the outcomes is conditional on some measurement. It seems best not to fix any particular requirements when a measurement is not given. For example, in some situations one can require *all* outcomes, from all measurements, to be mutually exclusive,

$$\mathrm{P}(R_{i'} \land R_{i''} | I) = 0 \quad \text{if } i' \neq i'';$$

whilst in other situations it can be convenient to conceive that an outcome can belong to more than one measurement: $i \in \varLambda_{k'}$ and $i \in \varLambda_{k''}$ with $k' \neq k''$ (or more simply, $\varLambda_{k'} \cap \varLambda_{k''} \neq \varnothing$). Such is the case, e.g., when a measurement can be considered as a coarsening of another one, in the sense explained above.

In any case it is always best to state precisely which measurement one is speaking about. This point may seem so obvious as to be trivial. And yet, very many discussions and statements about quantum mechanics and plausibility theory reveal that it is — still today — not clear at all. Cf. the discussion on the double-slit experiment of § 16.

Introducing the vectors

24. As already said, the propositions $\{\bar{S}_j\}$ are meant to represent the possible a priori data about a given system, and the propositions $\{M_k\}$ the possible 'questions' we can ask, the propositions $\{R_i\}$ being the possible 'answers'. It is clear then that *the theory* (whose content and 'laws' we assume included in the proposition I) *that concerns the system must allow us to assign the plausibilities*

$$\mathrm{P}(R_i|\, M_k \wedge \bar{S}_j \wedge I) \quad \textit{for all } j, k, \textit{ and } i \in \Lambda_k. \tag{III.15}$$

In other words, if we specify the initial data and 'ask a (sensible) question' the theory must allow us to assign plausibilities to the possible 'answers'.

The theory thus provides us with a sort of table like the following:

$$\begin{array}{cc|cccccc}
 & & \bar{S}_1 & \bar{S}_2 & \bar{S}_3 & \bar{S}_4 & \dots & \bar{S}_\sigma \\
\hline
M_1 & R_1 & p_{11} & p_{12} & p_{13} & p_{14} & \dots & p_{1\sigma} \\
 & R_2 & p_{21} & p_{22} & p_{23} & p_{24} & \dots & p_{2\sigma} \\
 & R_3 & p_{31} & p_{32} & p_{33} & p_{34} & \dots & p_{3\sigma} \\
M_2 & R_4 & p_{41} & p_{42} & p_{43} & p_{44} & \dots & p_{4\sigma} \\
 & R_5 & p_{51} & p_{52} & p_{53} & p_{54} & \dots & p_{5\sigma} \\
\dots & \dots & \dots & \dots & \dots & \dots & \dots & \dots \\
 & R_{\rho-2} & p_{\rho-2,1} & p_{\rho-2,2} & p_{\rho-2,3} & p_{\rho-2,4} & \dots & p_{\rho-2,\sigma} \\
M_\mu & R_{\rho-1} & p_{\rho-1,1} & p_{\rho-1,2} & p_{\rho-1,3} & p_{\rho-1,4} & \dots & p_{\rho-1,\sigma} \\
 & R_\rho & p_{\rho,1} & p_{\rho,2} & p_{\rho,3} & p_{\rho,4} & \dots & p_{\rho,\sigma}
\end{array} \tag{III.16}$$

where $p_{ij} \coloneqq \mathrm{P}(R_i|\, M_k \wedge \bar{S}_j \wedge I)$, and it has been assumed that the sets $\{\bar{S}_j\colon\ j = 1, \dots, \sigma\}$, $\{M_k\colon\ k = 1, \dots, \mu\}$, and $\{R_i\colon\ i = 1, \dots, \rho\}$ are finite, and (only as an example) measurement M_1 has two outcomes R_i with $i \in \Lambda_1 \coloneqq \{1, 2\}$, measurement M_2 has three outcomes with $\Lambda_2 \coloneqq \{3, 4, 5\}$, etc.

The table corresponds to a matrix (p_{ij}). The idea now is to associate some kind of mathematical objects to the propositions $\bar{S}_j$ and to the pairs of propositions R_i, M_k with $i \in \Lambda_k$ in such a way that, when we ask 'if I specify the preparation $\bar{S}_i$ and ask the "question" M_k, what is the plausibility that the theory assigns to obtaining the "answer" R_i ($i \in \Lambda_k$)?', we can obtain that plausibility by appropriately combining those mathematical objects. In communication-theoretical jargon, we want to *encode* the plausibilities (p_{ij}) into pairs of mathematical objects. This idea is quite easy to realise: it turns out that those mathematical objects are just real-valued vectors, and that the way to combine them is to take their scalar product. I.e., we make the associations

$$\bar{S}_j \mapsto \boldsymbol{s}_j, \tag{III.17}$$

$$R_i \mapsto \boldsymbol{r}_i \quad \text{with } i \in \Lambda_k \text{ for some } M_k, \tag{III.18}$$

in such a way that

$$\mathrm{P}(R_i | M_k \wedge \bar{S}_j \wedge I) \equiv p_{ij} = \boldsymbol{r}_i{}^{\mathsf{T}} \boldsymbol{s}_j, \tag{III.19}$$

where I write the scalar product $\boldsymbol{r}_i \cdot \boldsymbol{s}_j$ as the matrix product between the transpose $\boldsymbol{r}_i{}^{\mathsf{T}}$ of the column vector $\boldsymbol{r}_i$ and the column vector $\boldsymbol{s}_j$. It is important to note that *the mappings* (III.17) *and* (III.18) *are generally not bijective, although they are surjective by construction.* The vectors $\boldsymbol{s}_j$ and $\boldsymbol{r}_i$ are called 'preparation vectors' and 'outcome vectors' respectively; collectively, we call them 'proposition vectors'. With regard to the measurements M_k, we simply associate to them the corresponding sets of outcome vectors:

$$M_k \mapsto \boldsymbol{m}_k := \{\boldsymbol{r}_i \mid i \in \Lambda_k\}. \tag{III.20}$$

The explicit construction of this vector representation is given in **Papers (C)** and **(D)** together with examples, a general discussion, applications, and uncommented historical references. I shall not present the construction and the discussion again here, but rather assume that the main points of those papers be known (i.e., this is a good point to read those papers). I should like, however, to present some additional, partly historical, remarks.

In the following, the vectors associated to generic propositions $\bar{S}_x$, $\bar{S}'$, R_y, and similar will be denoted by $\boldsymbol{s}_x$, $\boldsymbol{s}'$, $\boldsymbol{r}_y$, etc. in an obvious way.

Structures of the vector sets and connexion with plausibilistic reasoning in physics

25. To the sets of propositions $\{\bar{S}_j\}$ and $\{R_i\}$ are associated the sets of vectors $\{\boldsymbol{s}_j\}$ and $\{\boldsymbol{r}_i\}$, and to the set of propositions $\{M_k\}$ is associated the set of sets of vectors $\{\{\boldsymbol{r}_i \mid i \in \Lambda_k\}\}_k$. As the various propositions generally concern some physical quantities (e.g., phase coordinates etc.), it is important to distinguish carefully between

1. the set or space of physical quantities,
2. the set of related propositions,
3. the set of proposition vectors.

These sets have in general different mathematical, logical, and geometrical structures.

To the structures of the sets of propositions and proposition vectors we turn now our attention. The analysis of their structures is particularly important because it is very intimately connected to the plausibilistic reasoning we make in our theory — e.g., with questions like 'state assignment' (or 'reconstruction' or 'retrodiction' or 'estimation') and 'measurement assignment'.

Remember once more that the 'preparations', 'measurements', and 'outcomes' — i.e. the propositions $\bar{S}_j$ etc. — concern actual or hypothetical facts, often stated in terms of physical quantities. Consider the following situations as regards these facts:

Preparation 'mixing': In a given circumstance we may find ourselves in a condition of uncertainty C between two (or more) preparations $\bar{S}'$, $\bar{S}''$. This is quantitatively expressed by a plausibility distribution

$$\begin{gathered} \mathrm{P}(\bar{S}'|\, C \land I) = \alpha', \qquad \mathrm{P}(\bar{S}''|\, C \land I) = \alpha'', \\ \text{with} \quad \mathrm{P}(\bar{S}' \lor \bar{S}''|\, C \land I) = \alpha' + \alpha'' = 1. \end{gathered} \tag{III.21}$$

Given a measurement M_k, what is the plausibility distribution that we assign to its outcomes $\{R_i\}$ in such a circumstance? From the rules of plausibility logic, more precisely from the theorem on total plausibility,

$$\begin{aligned} \mathrm{P}(R_i|\, M_k \land C \land I) = \mathrm{P}(R_i|\, M_k \land \bar{S}' \land I)\ \mathrm{P}(\bar{S}'|\, C \land I) +{} \\ \mathrm{P}(R_i|\, M_k \land \bar{S}'' \land I)\ \mathrm{P}(\bar{S}''|\, C \land I), \\ = \boldsymbol{r}_i{}^{\mathsf{T}} \boldsymbol{s}'\, \alpha' + \boldsymbol{r}_i{}^{\mathsf{T}} \boldsymbol{s}''\, \alpha'' \equiv \boldsymbol{r}_i{}^{\mathsf{T}} (\alpha' \boldsymbol{s}' + \alpha'' \boldsymbol{s}''), \end{aligned} \tag{III.22}$$

where we have made the assumption that C becomes irrelevant if $\bar{S}'$ or $\bar{S}''$ is known,[10] used eq. (III.21), and introduced the vectors associated to the various propositions. The numerical value of the plausibility conditional on C is a weighted average of the plausibilities conditional on $\bar{S}'$ and $\bar{S}''$.

The last line of the preceding equation shows that we can associate the vector

$$\boldsymbol{x} \coloneqq \mathrm{P}(\bar{S}'|\, C \land I)\, \boldsymbol{s}' + \mathrm{P}(\bar{S}''|\, C \land I)\, \boldsymbol{s}'' \equiv \alpha'\, \boldsymbol{s}' + \alpha''\, \boldsymbol{s}'' \tag{III.23}$$

to the proposition C. This vector is a *convex combination* of the vectors associated to $\bar{S}'$ and $\bar{S}''$. All these results are straightforwardly generalised to conditions of uncertainty concerning more than two preparations. A condition of uncertainty between two or more preparations is often called a 'mixture' of those preparations. In the following, we shall call propositions like C *circumstances*, as we did in the Laplace-Jaynes approach to induction; this is not a confounding nomenclature, since we shall see that the 'circumstances' of the Laplace-Jaynes approach and the ones introduced here are basically the same notion. When we want to be more specific, we may call those here presented 'preparation circumstances'.[11] Their associated vectors, like $\boldsymbol{x}$, will be called 'circumstance vectors'.

There is a natural relation of equivalence amongst different circumstances, and amongst circumstances and preparations themselves:

[10] I.e., $\mathrm{P}(R_i|\, M_k \land \bar{S}' \land C \land I) = \mathrm{P}(R_i|\, M_k \land \bar{S}' \land I)$, and analogously with $\bar{S}''$.

[11] I know that the terms '(preparation) circumstance' and 'measurement circumstance', introduced infra, are ugly and liable to be confused with their non-technical homonyms. But I have not found better alternatives yet (cf. footnote 4).

Definition 3. *Two preparation circumstances C', C'' are said to be* equivalent *if they lead to identical plausibility distribution for each measurement M_k; or, equivalently,*[12] *if their associated vectors are identical:*

$$\begin{aligned} C' \sim C'' \iff& \mathrm{P}(R_i | M_k \wedge C' \wedge I) = \mathrm{P}(R_i | M_k \wedge C'' \wedge I) \quad \textit{for all } k \textit{ and } i \in \Lambda_k, \\ \iff& \boldsymbol{x}' = \boldsymbol{x}''. \end{aligned} \tag{III.24}$$

Analogously, one can speak of the equivalence of a circumstance and a preparation, or of two preparations.

It must be remarked that this equivalence relation is *heavily dependent on the specification of the measurement and outcome sets*. Adding or subtracting a measurement to or from the set $\{M_k\}$ — and thus considering a *different system*, cf. § 22 — may render two previously equivalent circumstances inequivalent or vice versa. Thus the equivalence is not an 'intrinsic' property of the preparations, or related to the characteristics of the preparations alone.

Unfortunately the above remark is very often forgotten in quantum mechanics. There, also, two 'preparation procedures' can be 'indistinguishable', in which case they are represented by the same density matrix.[13] But this indistinguishability is only relative to some set of measurements, not 'intrinsic' to the procedures. (After all, if we are speaking of *two* procedures it is because we can distinguish them somehow.) Also the statement that the two procedures lead to the 'same state' must be qualified relatively to some measurement set. It may well happen that a new measurement be found that distinguishes 'states' that were previously thought to be the 'same state': just think about the discovery of isotopes. Statements asserting that 'two states are indistinguishable *in principle*' or something of the kind (and such statements abound in quantum mechanics), are simply vacuous, untestable, and usually of limited historical duration.

Measurement 'mixing': In another circumstance a condition of uncertainty W may regard two (or more) measurements M', M'':

$$\begin{gathered} \mathrm{P}(M' | W \wedge I) = \beta', \qquad \mathrm{P}(M'' | W \wedge I) = \beta'', \\ \text{with} \quad \mathrm{P}(M' \vee M'' | W \wedge I) = \beta' + \beta'' = 1. \end{gathered} \tag{III.25}$$

In this circumstance W we can expect an outcome of M' or M'', so that the set of possible outcomes is the union of the outcomes of the two, R_i with $i \in \Lambda' \cup \Lambda''$. Given a preparation $\bar{S}_j$, the plausibility distribution over this set of outcomes in this circumstance is, from the theorem on total plausibility and eq. (III.25),

$$\begin{aligned} \mathrm{P}(R_i | W \wedge \bar{S}_j \wedge I) &= \mathrm{P}(R_i | M' \wedge \bar{S}_j \wedge I)\ \mathrm{P}(M' | W \wedge I) + \\ &\qquad \mathrm{P}(R_i | M'' \wedge \bar{S}_j \wedge I)\ \mathrm{P}(M'' | W \wedge I), \\ &= \begin{cases} \beta' \, \boldsymbol{r}_i{}^{\mathsf{T}} \boldsymbol{s}_j & \text{if } i \in \Lambda', \\ \beta'' \, \boldsymbol{r}_i{}^{\mathsf{T}} \boldsymbol{s}_j & \text{if } i \in \Lambda'', \end{cases} \end{aligned} \tag{III.26}$$

[12]Since $\boldsymbol{r}_i{}^{\mathsf{T}} \boldsymbol{x}' = \boldsymbol{r}_i{}^{\mathsf{T}} \boldsymbol{x}''$ for a complete set of linearly independent $\boldsymbol{r}_i$.

[13]As explained in **Papers (C)** and **(D)**, density matrices constitute only a different representation of the preparation vectors of quantum-mechanical systems.

where the assumption has been made that W becomes irrelevant if M' or M'' is known.[14]

A condition of uncertainty between two or more measurements is often called a 'mixture' of those measurements. In the following we shall call propositions like W *measurement circumstances*. (Cf. also Holevo [350].)

Coarsening: There are also circumstances in which the outcomes $\{R_i \mid i \in \Theta\}$ of one or more measurements M_k, with $\Lambda_k \subseteq \Theta$, cannot be observed; but we can observe other 'events' described by mutually exclusive and exhaustive propositions $\{E_{\bar{\imath}}\}$, and we have (e.g., from some theory) some plausibilities for the latter given the former:

$$\begin{aligned} &\mathrm{P}(E_{\bar{\imath}}|\, R_i \wedge I) = Q_{\bar{\imath}i}, \\ \text{with}\quad &\sum_{\bar{\imath}} Q_{\bar{\imath}i} = \mathrm{P}(\bigvee_{\bar{\imath}} E_{\bar{\imath}}|\, R_i I) = 1. \end{aligned} \tag{III.27}$$

The last line implies that the matrix $\boldsymbol{Q} \equiv (Q_{\bar{\imath}i})$ is a *stochastic matrix* [601] (see also [13, 58, 145, 508]). The plausibility for one of the 'events' $\{E_{\bar{\imath}}\}$, given a measurement M_k such that $\Lambda_k \subseteq \Theta$ and a preparation $\bar{S}_j$, is then obtained by marginalisation over the outcomes $\{R_i \mid i \in \Lambda_k\}$:

$$\begin{aligned} \mathrm{P}(E_{\bar{\imath}}|\, M_k \wedge \bar{S}_j \wedge I) &= \sum_{i\in\Lambda_k} \mathrm{P}(E_{\bar{\imath}}|\, R_i \wedge I)\ \mathrm{P}(R_i|\, M_k \wedge \bar{S}_j \wedge I), \\ &= \sum_{i\in\Lambda_k} Q_{\bar{\imath}i} \boldsymbol{r}_i{}^{\mathsf{T}} \boldsymbol{s}_j, \end{aligned} \tag{III.28}$$

where we assume that the M_k are irrelevant if the R_i are known.

A set of such 'events' is often called a 'coarsening' of the outcome set $\{R_i \mid i \in \Lambda_k\}$; in fact, from now on we shall call the propositions $E_{\bar{\imath}}$ 'coarsened outcomes', or even simply 'outcomes', instead of 'events'. (Cf. also Holevo [350].) A simple example of coarsening is the disjunction of two outcomes in a set of three: $\{E_1, E_2\} := \{R_1 \vee R_2, R_3\}$. In this case the stochastic matrix will contain the submatrix

$$\begin{pmatrix} Q_{11} & Q_{12} & Q_{13} \\ Q_{21} & Q_{22} & Q_{23} \end{pmatrix} = \begin{pmatrix} 1 & 1 & 0 \\ 0 & 0 & 1 \end{pmatrix}. \tag{III.29}$$

It is quite natural to consider and combine a measurement circumstance together with a set of coarsened outcomes, which manifest a condition of uncertainty about the outcomes of those measurements. Denote this 'combined' measurement circumstance also by W. We can have e.g. a measurement circumstance W regarding some measurements M_k with plausibility distribution $\{\beta_k\}$, and also a coarsening $\{E_{\bar{\imath}}\}$ of their sets of outcomes with stochastic matrix $(Q_{\bar{\imath}i})$, $i \in \bigcup_k \Lambda_k$. The plausibility for the coarsened outcome $E_{\bar{\imath}}$, conditional on W and on a preparation $\bar{S}_j$, is then (with the assumptions already discussed) given by

$$\begin{aligned} \mathrm{P}(E_{\bar{\imath}}|\, W \wedge \bar{S}_j \wedge I) &= \sum_{k,i\in\Lambda_k} \mathrm{P}(E_{\bar{\imath}}|\, R_i \wedge I)\ \mathrm{P}(M_k|\, W \wedge I)\ \mathrm{P}(R_i|\, M_k \wedge \bar{S}_j \wedge I), \\ &= \sum_{k,i\in\Lambda_k} Q_{\bar{\imath}i}\, \beta_k\, \boldsymbol{r}_i{}^{\mathsf{T}} \boldsymbol{s}_j. \end{aligned} \tag{III.30}$$

[14] I.e., $\mathrm{P}(R_i|\, M' \wedge W \wedge \bar{S}_j \wedge I) = \mathrm{P}(R_i|\, M' \wedge \bar{S}_j \wedge I)$, and analogously with M''.

To this measurement circumstance W we can therefore associate the set of vectors

$$\begin{aligned} &\bar{\boldsymbol{m}} := \{\bar{\boldsymbol{r}}_{\bar{\imath}}\}, \qquad \text{with} \\ &\bar{\boldsymbol{r}}_{\bar{\imath}} := \sum_{k,i\in\Lambda_k} \mathrm{P}(E_{\bar{\imath}} | R_i \wedge I)\ \mathrm{P}(M_k | W \wedge I)\, \boldsymbol{r}_i, \equiv \sum_{k,i\in\Lambda_k} Q_{\bar{\imath}i}\, \beta_k\, \boldsymbol{r}_i. \end{aligned} \tag{III.31}$$

In analogy with the preparation circumstances, there is also a natural equivalence relation amongst 'combined' (in the sense above) measurement circumstances, as well as amongst these and the measurements themselves:

Definition 4. *Two measurement circumstances W' and W'', with coarsened outcomes $\{E'_{\bar{\imath}}\}$ and $\{E''_{\bar{\imath}}\}$ having the same cardinality, are said to be* equivalent *if they lead to identical plausibility distribution for each preparation $\bar{S}_j$; or, equivalently,*[15] *if their associated vector sets are identical:*

$$\begin{aligned} W' \sim W'' &\iff \{\mathrm{P}(E'_{\bar{\imath}} | W' \wedge \bar{S}_j \wedge I)\} = \{\mathrm{P}(E''_{\bar{\imath}} | W'' \wedge \bar{S}_j \wedge I)\} \quad \textit{for all } j, \\ &\iff \bar{\boldsymbol{m}}' = \bar{\boldsymbol{m}}''. \end{aligned} \tag{III.32}$$

Analogously, one can speak of the equivalence of a measurement circumstance and a measurement, or of two measurements.

Note that the equivalence regards the *plausibility distributions* only: the sets of outcomes of the equivalent measurement circumstances may be from a physical point of view completely different. As in the case of the equivalence relation (III.24), also in this case it must be remarked that the present equivalence relation is dependent on the specification of the set of preparations. Any alteration to the set $\{\bar{S}_i\}$ — which alteration implies a change of system, cf. § 22 — may render two measurement circumstances, which were previously equivalent, inequivalent; or vice versa.

26. Preparations $\bar{S}$, measurements M, and outcomes R on one side, and circumstances C, W, and coarsened outcomes E on the other, live on different logical planes. The former are nearer to the notions, the concepts, and the primitives of the theory than the latter. The latter represents different kinds of states of knowledge we can have on those notions, concepts, and primitives. It is for this reason that when we study a physical system from a plausibilistic point of view, it is natural to shift from the sets $\{\bar{S}_j\}$, $\{M_k\}$, $\{R_i\}$, to the sets of propositions like C, W, and E. This shift also has some mathematical consequences and advantages.

We can imagine all possible kinds of circumstances C. Mathematically this means that in eqs. (III.21) and (III.23) we can entertain all possible plausibility distributions $\{\alpha_j\}$. The associated vectors $\boldsymbol{x}$ evidently form a *convex set*, viz. the convex hull [14, 89, 297, 518, 625, 723] $\mathrm{conv}\{\boldsymbol{s}_j\}$ of the set of preparation vectors. Note that the correspondence between circumstances and their vectors is not injective: as eq. (III.24) shows, equivalent circumstances have the same vector. Each equivalence class is then uniquely characterised

[15] See footnote 12.

by a vector $\boldsymbol{x}$; membership of a circumstance C_j to such equivalence class will be denoted by $j \in \boldsymbol{x}^{\sim}$. From the same equation it also follows, by the rules of plausibility theory, that

$$\begin{aligned} \mathrm{P}[R_i | M_k \wedge (\bigvee_{j \in \boldsymbol{x}^{\sim}} C_j) \wedge I] &= \mathrm{P}(R_i | M_k \wedge C_j \wedge I) \quad \text{for all } j \in \boldsymbol{x}^{\sim}, \\ &= \boldsymbol{r}_i{}^{\mathsf{T}} \boldsymbol{x}. \end{aligned} \tag{III.33}$$

Hence the same vector $\boldsymbol{x}$ can also be associated to the disjunction of all equivalent circumstances — which is itself but another 'circumstance'.

It is therefore natural to consider the set of such disjunctions instead of the original set, since the members of each disjunction are for us the same for the purpose of assigning plausibilities to measurement outcomes. (This point is made more precise in § 39; cf. eq. (IV.5).) From the above equations it follows that this disjunction is *uniquely* characterised by such a vector, which can therefore be used as a unique index:

$$S_{\boldsymbol{x}} := \bigvee_{j \in \boldsymbol{x}^{\sim}} C_j. \tag{III.34}$$

Such disjunctions will be called '$\boldsymbol{x}$-indexed circumstances', or more generically 'plausibility-indexed circumstances', in analogy with the Laplace-Jaynes approach (remember in fact that the rôle of the vector $\boldsymbol{x}$ is to partly encode the plausibilities $\mathrm{P}(R_j | M_k \wedge S_{\boldsymbol{x}} \wedge I)$). Since the vectors $\boldsymbol{x}$ belong to a convex space, the set $\{S_{\boldsymbol{x}}\}$ is continuous. For a more careful discussion of this point, see **Paper (F)**, § 4, and **Paper (G)**, § 5.3.

In a similar way can we imagine all possible kinds of *measurement* circumstances W; and the set of imaginable coarsened outcomes contains at least all possible disjunctions of outcomes R_i. Mathematically this means that in eqs. (III.25), (III.27), and (III.31) we can entertain all possible plausibility distributions $\{\beta_k\}$ and at least those stochastic matrices $(Q_{\bar{\imath}i})$ that have noughts or ones as elements. The associated vectors $\bar{\boldsymbol{r}}$ can then be shown to form a convex set as well, the convex hull $\mathrm{conv}\{\boldsymbol{r}_i\}$ of the set of outcome vectors. In fact, there is more than only a convex structure: this set is a double cone, as discussed in **Paper (C)**, § III.C; cf. also the references given infra in § 37. The set of sets $\bar{\boldsymbol{m}}$ associated to uncertainty conditions on measurements has an even more complex structure, possibly with a partial order (cf. [13, 70, 145, 508, 536]).

Measurement circumstances can also be grouped together into disjunctions of equivalent ones, according to the equivalence relation (III.32). The process is analogous to that for preparation circumstances. I shall not take this step, however; for preparations and measurements have different offices, and it is not yet clear to me whether a 'plausibility-indexing' of measurement circumstances and outcomes is as useful as for preparation circumstances.

The vector framework in classical point mechanics

27. Up to now the discussion has been conducted on an abstract and general plane. But it is simple to apply the framework to concrete cases. A simple example concerning a toy system is given in § 2.1 of **Paper (D)**. Here I want briefly present quite natural applications to systems of classical point mechanics and quantum mechanics;[16] but note that they are not the only possible ones.

With 'a system of classical point mechanics' I intend here specifically a closed[17] system specified by some phase coordinates z, like the positions and momenta of a collection of particles, and by a set of measurements which can pertain e.g. the position coordinates, or energy, or other quantities like the intensity of the total electric field at a given point, et similia. The measurements may more generally depend on external parameters, and these may be unknown (for a simple example see Peres [583]), so that we have *plausibility distributions* for their outcomes. An evolution operator which may be time-dependent, or less generally a Hamiltonian (usually corresponding to the energy) or a Lagrangian is also given, but it will not interest us here.

If we fix a time instant t_0, it is natural to introduce a (continuous) set of states $\bar{S}_z$, each stating that the system has been prepared or selected, at that instant, with particular phase coordinates z. Amongst the measurements considered for this kind of systems the basic one is that yielding the phase coordinates themselves; we denote it by M^{ph}. Its outcomes R_z are the various phase-coordinate values, and the theory tells us that

$$\mathrm{p}(R_z | M^{\mathrm{ph}} \wedge S_{z'} \wedge I_{\mathrm{c}})\, \mathrm{d}z = \delta(z - z')\, \mathrm{d}z, \tag{III.35}$$

whose meaning I think needs no explanation. Another almost universally considered measurement concerns the energy h, and we denote it by M^{en}_{ω}. In the case of a closed system the energy value is determined by the phase coordinates z, and by other possible parameters as well as the time, which I denote collectively by ω:

$$h = H(z, \omega). \tag{III.36}$$

If ω is known and fixed the plausibility of obtaining the outcome h is given by

$$\mathrm{p}(R_h | M^{\mathrm{en}}_{\omega} \wedge S_{z'} \wedge I_{\mathrm{c}})\, \mathrm{d}h = \delta[h - H(z', \omega)]\, \mathrm{d}h. \tag{III.37}$$

Analogous considerations holds for other kinds of measurements.

The plausibilities in the equations above are those 'given by the theory', and are more concrete examples of the generic ones of eq. (III.15). Although, as already said, we have an uncountable infinity of states, we may roughly imagine how the construction of a plausibility table like (III.16) could proceed, as well as the derivation of the proposition vectors as in **Papers (C)** and **(D)**. The various vectors are in this case infinite-dimensional and are realised as generalised functions[18] over an appropriate space, their scalar product being realised as the integral of the product of those generalised functions. The following

[16] The first case requires some mathematical care since the resulting convex spaces are not finite-dimensional; but for the general presentation given here I think that no particular mathematical (topological) comments are necessary.

[17] In the thermodynamical meaning of the term.

[18] Or rather, measures.

associations hold in particular:

$$\bar{S}_{z'} \text{ is represented by } \boldsymbol{s}_{z'} \coloneqq [\hat{z} \mapsto \delta(\hat{z} - z')], \tag{III.38}$$

$$R_{z'} \text{ is represented by } \boldsymbol{r}_{z'} \coloneqq [\hat{z} \mapsto \delta(\hat{z} - z')\,\mathrm{d}z'], \tag{III.39}$$

$$R_h \text{ is represented by } \boldsymbol{r}_h \coloneqq \{\hat{z} \mapsto \delta[h - H(\hat{z}, \omega)]\,\mathrm{d}h\}, \tag{III.40}$$

in such a way that

$$\mathrm{p}(R_{z''} | M^{\mathrm{ph}} \wedge \bar{S}_{z'} \wedge I_{\mathrm{c}})\,\mathrm{d}z'' = \boldsymbol{r}_{z''}{}^{\intercal}\boldsymbol{s}_{z'} \equiv \Big[\int \delta(\hat{z} - z'')\,\delta(\hat{z} - z')\,\mathrm{d}\hat{z}\Big]\,\mathrm{d}z'', \tag{III.41}$$

$$\mathrm{p}(R_h | M^{\mathrm{en}}_{\omega} \wedge \bar{S}_{z'} \wedge I_{\mathrm{c}})\,\mathrm{d}h = \boldsymbol{r}_h{}^{\intercal}\boldsymbol{s}_{z'} \equiv \Big\{\int \delta[h - H(\hat{z}, \omega)]\,\delta(\hat{z} - z')\,\mathrm{d}\hat{z}\Big\}\,\mathrm{d}h, \tag{III.42}$$

etc. We clearly recover eqs. (III.35) and (III.37).

Note that, although there is a bijective correspondence between phase coordinates z and the preparation-vectors $\boldsymbol{s}_z$ of the corresponding propositions S_z, the two mathematical objects live in two different spaces, with different relevant properties. In the space of the z there may be a linear structure (e.g., when the z are positions and momenta), a symplectic one, etc. In the space of the $\boldsymbol{s}_z$ the relevant structure is the *convex* one, and it turns out that this space consists in the extreme points of a simplex.[19] From this property many other well-known properties follow, e.g. the fact that all states are distinguishable in one shot, and that every measurement is equivalent to some coarsening of M^{ph}.

28. In the particular application of the vector framework to classical point mechanics described in the previous section, a preparation circumstance C represents a state of uncertainty regarding the phase coordinates or, better, the propositions $\bar{S}_z$. Conditional on such circumstance we have a plausibility distribution

$$\mathrm{p}(\bar{S}_z | C \wedge I_{\mathrm{c}})\,\mathrm{d}z = f(z)\,\mathrm{d}z, \tag{III.43}$$

and, according to § 24, eq. (III.23), we can associate to the proposition C the vector (which is again a generalised function)

$$\boldsymbol{x} \coloneqq \int \mathrm{p}(\bar{S}_z | C \wedge I_{\mathrm{c}})\,\boldsymbol{s}_z\,\mathrm{d}z = \Big[\hat{z} \mapsto \int f(z)\,\delta(\hat{z} - z)\,\mathrm{d}z\Big] \equiv f. \tag{III.44}$$

It is clear that a circumstance C is what we usually represent by a *Liouville function*.

29. It is interesting to note that, in the vector framework, Liouville functions can be seen from two different points of view. From the first, they are plausibility distributions over the phase coordinates (or better, over the propositions S_z); and this is the usual point of view. From the second point of view, they are just *vectors* that, when combined with other vectors, yield a plausibility distribution; from this point of view their normalisation and positivity properties are not necessary. In fact, the vector representation (III.38)–(III.40)

[19]Some qualifications would be necessary in this infinite-dimensional case. See e.g. [14, 427–433, 632, 736].

is only a particular realisation: we could have chosen functions different from deltas. A simple alternative would be, e.g., the associations

$$\bar{S}_{z'} \text{ is represented by } \boldsymbol{s}_{z'} := [\hat{z} \mapsto -\delta(\hat{z} - z')], \tag{III.38$'$}$$

$$R_{z'} \text{ is represented by } \boldsymbol{r}_{z'} := [\hat{z} \mapsto -\delta(\hat{z} - z')\, \mathrm{d}z'], \tag{III.39$'$}$$

etc.; in this case to the circumstance C would be associated the vector (function) $-f$, clearly non-positive. This second point of view is quite useful especially when we compare classical mechanics with quantum mechanics, since it implies that Liouville functions and, e.g., Wigner functions or other 'pseudo-distributions'[20] are homologous mathematical objects, i.e. *they play the same rôle* within the respective theories; in particular, they are *not* plausibility distributions, but only mathematical objects that, combined with others, yield plausibility distributions.

30. It is perhaps useful to give an example of a measurement circumstance as well. Consider the energy function $z \mapsto H(z, \omega)$ and suppose that the value of the parameter ω is unknown, with a plausibility distribution $g(\omega)\, \mathrm{d}\omega$. This situation constitutes a measurement circumstance W, conditional on which the measurements M^{en}_{ω} for different ω have plausibilities

$$\mathrm{p}(M^{\mathrm{en}}_{\omega} | \, W \wedge I_{\mathrm{c}})\, \mathrm{d}\omega = g(\omega)\, \mathrm{d}\omega. \tag{III.45}$$

Conditional on W and on a state $\bar{S}_z$, the possible outcomes R_h have a plausibility distribution

$$\mathrm{p}(R_h | \, W \wedge \bar{S}_z \wedge I_{\mathrm{c}})\, \mathrm{d}h = \left\{ \iint \delta[h - H(\hat{z}, \omega)]\, \delta(\hat{z} - z)\, g(\omega)\, \mathrm{d}\hat{z}\, \mathrm{d}\omega \right\} \mathrm{d}h \tag{III.46}$$

and to the outcomes we can associate the vectors

$$\bar{\boldsymbol{r}}_h := \left(\hat{z} \mapsto \left\{ \int \delta[h - H(\hat{z}, \omega)]\, g(\omega)\, \mathrm{d}\omega \right\} \mathrm{d}h \right), \tag{III.47}$$

which formula is but a particular case of eq. (III.31).

The vector framework in quantum mechanics

31. With regard to quantum mechanics, the application of the vector framework is as straightforward as it was for classical point mechanics. Here I take as example a closed,[21] finite-level system. Such a system is characterised by an infinite collection of preparation circumstances which includes a set of 'special ones', indexed by the rays ϕ of a complex Hilbert space of given dimension. The physical reasons as to why such a set should be picked up are still unknown — and also unsought within the main-stream research of trade science [707]. A collection of measurements is also given which pertain various quantities such as the components of intrinsic angular momentum, energy, et similia. The collection

[20] See references in § 31 infra.

[21] In the thermodynamical meaning of the term.

of preparations and measurements has the specific feature — whose reason is also unknown and unsought — that at most K preparations can be distinguished in one shot by some measurement; the system is therefore called a K-level system. An evolution operator or less generally a Hamiltonian is usually given as well, but it will not interest us here.

Fixing a time instant t_0, one can introduce the set of preparation circumstances $\{C_j\}$ and the set of measurements $\{M_k\}$, each with a set of outcomes $\{R_i \mid i \in \Lambda_k\}$. If we imagine to specify a plausibility table like (III.16) and to decompose it as usual, we arrive at sets of circumstance- and outcome-vectors $\boldsymbol{x}$ and $\boldsymbol{r}_i$ that have particular convex structures; in particular, *the set of preparation vectors is convex-structurally isomorphic to the set of positive-definite K-by-K complex Hermitian matrices with unit trace*, and the set of outcome vectors is the largest convex set compatible with the structure of the preparation-vector set (in the sense of **Paper (C)**, § III.C). These convex structures are quite complicated; an example for a three-level system is shown in Fig. 1 of **Paper (H)**, and others are available upon request [602].

The circumstance- and outcome-vectors are realised in a variety of ways in quantum-mechanical studies and applications, depending on the purpose. The most common realisation is as particular Hermitian matrices: *statistical operators* [225] ρ and *positive-operator-valued-measure elements* [15, 22, 106–108, 110, 111, 113, 156, 157, 349, 352, 370, 445, 642] E_i, the scalar product corresponding to the trace of the product of these matrices:[22]

$$\boldsymbol{x} \mathrel{\hat{=}} \rho, \tag{III.48a}$$

$$\boldsymbol{r}_i \mathrel{\hat{=}} E_i, \tag{III.48b}$$

$$\boldsymbol{r}_i^{\mathsf{T}} \boldsymbol{x} \mathrel{\hat{=}} \operatorname{tr}(E_i \rho). \tag{III.48c}$$

Another realisation very common in quantum optics is as Wigner functions [420, 738] W or other functions like Husimi's Q or Glauber and Sudarshan's P [264, 276–278, 343, 462, 678], defined on a particular parameter space; the scalar product is realised as the integral of the product of the functions:

$$\boldsymbol{x} \mathrel{\hat{=}} [y \mapsto W(y)], \tag{III.49a}$$

$$\boldsymbol{r}_i \mathrel{\hat{=}} [y \mapsto V_i(y)], \tag{III.49b}$$

$$\boldsymbol{r}_i^{\mathsf{T}} \boldsymbol{x} \mathrel{\hat{=}} \int V_i(y)\, W(y)\, \mathrm{d}y. \tag{III.49c}$$

There have been many discussions about the fact that such functions, often called 'pseudo-probability distributions', do not generally have the properties of a plausibility distribution. But we see that this is in fact not necessary, since their rôle is *not* that of plausibility distributions, but of objects that, combined with similar objects, *yield* plausibility distributions.[23] Put it otherwise, the fact that e.g. a Wigner function may have negative values is no more troublesome than the fact that a statistical operator has complex entries: for neither is a plausibility distribution.

[22] The symbol '$\hat{=}$' means 'corresponds to'. Cf. the ISO [362] and ANSI/IEEE [358] standards.

[23] In some cases the objects to be combined with are sort of 'identities', and thus the combination is not apparent. This is the case e.g. for the Q and P pseudo-distributions.

I have already mentioned that the sets of circumstance- and outcome-vectors have particular and complicated convex structures. Some consequent properties are well known. E.g., the circumstances represented by the extreme points of the preparation-vector set are not all distinguishable in one shot, as instead is the case for most systems of classical point mechanics. A noteworthy property of the set of measurements is that not all measurements can be obtained as coarsenings of a single one, as is the case for most classical systems; this is related to the 'construction' of some measurements by means of Najmark's theorem [see e.g. 12, 347, 349, 352]. For detailed discussions of the convex and related properties of these sets see e.g. the studies by Holevo [347, 349, 350, 352], Schroeck [642], Busch, Grabowski, and Lahti [106, 108–111], Beltrametti and Bugajski [56], and references therein.

Miscellaneous remarks

32. In the study of some now fashionable topics, like entanglement, cloning, and other communication-theory related ones, the vector framework appears to me the most appropriate since it offers a clear geometric point of view, uncluttered by mathematical objects and notions — like eigenprojectors, Hermitian conjugates, kets, bras, and other Hilbert-space paraphernalia — that are not always necessary. This seems to have been noticed in the literature recently [40–42, 666]. Note, in fact, that the notions of distinguishability, measurement sharpness, mixing, coarsening, and many other related ones[24] are all of a convex-geometrical nature, and are therefore most easily expressed and studied in convex-geometrical terms. Note that I am not saying that Hilbert-space concepts are unnecessary; they are necessary in the sense that the Hilbert-space structure determines the particular convex one. But when one e.g. wants to localise some points in this convex structure, or delimit particular regions by hyperplanes, or just choose some basis vectors, there is no need to invoke, say, SU-group generators and the like. The mathematics introduced in some works that I have seen in the literature seems more like a sort of exorcising ritual rather than an efficient mathematical apparatus set-up to achieve a given purpose.

My statement, indeed, is that *in quantum mechanics, the only scope of assigning a Hilbert-space structure is to compactly assign the convex one*. And I hope that alternative ways of assigning the convex structure will be found some day; and that these will have some understandable physical content.

33. This leads me to another observation. In quantum theory it is the convex structure of the sets of circumstances and measurements that is given at the start (by postulating a complex Hilbert-space structure), and from this the plausibilities for the measurement outcomes, conditional on the circumstances, are derived. This is the opposite of what we do in classical physics, where instead we give physical laws — which concern notions, concepts, ideas that are distillations and idealisations of our experience — and based on these we assign plausibilities, from which the convex structures are finally derived. As

[24] E.g., the topic of 'cloning', which centres around the notion of distinguishability.

already said, it is still unknown what are the reasons of the particular convex structures of quantum mechanics, and one of the greatest achievements in natural philosophy will be to derive them from humanly understandable principles. Until then quantum theory will only be a very successful black-box theory, a sort of modern Linnaean '*Systema naturæ per spatia Hilberti*' of the microscopic fauna and flora. Note that by very nice group arguments one can derive the standard quantum-mechanical commutator structures, see e.g. Lévy-Leblond [464] and Holevo [349]; but these derivations assume at the start the particular, unexplained convex structure of the set of quantum-mechanical states.

Some physicists are of the opinion that quantum theory needs no 'interpretation' (e.g., Fuchs and Peres [249]; see also the comments on their articles and their reply [677])[25]. Well, that depends on what one means by 'interpretation'. I should say, e.g., that 'thermodynamics needs no interpretation', since the primitives and the laws of this theory are distillates and idealisations of our daily experience and concepts. And yet, I would not say that it is useless to encumber thermodynamics 'with hidden variables [. . .] without any improvement in its predictive power'; for otherwise I should be condemning the first studies in statistical mechanics, which added a lot of 'hidden variables' (positions and momenta of microscopic particles) without, at that time, any improvement to thermodynamics' predictions.

I also think that the parallel Fuchs and Peres draw with Euclidean geometry misses the point. They imagine Euclid saying: 'Geometry is an abstract formalism and all you can demand of it is internal consistency. However, you may seek material objects whose behavior mimics the theorems of geometry, and that involves interpretation' [677]. But geometry was created as an idealisation, systematisation, formalisation of some provinces of *experience*, not as an exercise in axiomatics. The primitives of point, straight line, surface, etc., and the associated postulates were all introduced to reflect and idealise some facts of experience. The same is true of the foundations of classical physics [219, 554, 716], based on primitives like body, distance, motion, force, temperature, and on the postulates that characterise these. But the primitives of quantum mechanics — eigenprojectors, statistical operators — which experiential facts do they idealise? I agree with Fuchs and Peres: they are not meant to idealise facts, they are just parts of 'an algorithm for computing *probabilities* for the macroscopic events ("detector clicks") that are the consequences of our experimental interventions' [249]. I.e., again: quantum theory is a black-box theory, not a proper physical theory.

Appleby [21], citing Bell [55], states the point in a direct way:

> This does not mean that I find the Copenhagen interpretation satisfactory, as it stands now. Bell [55, pp. 173–174] argues that the Copenhagen interpretation is "unprofessionally vague and ambiguous". I think he is right. I also think he is right to complain that quantum mechanics, when interpreted in traditional Copenhagen terms, seems to be "exclusively concerned with 'results of measurement' and [seems to have] nothing to say about anything else". I

[25] Although I paraphrase some statements from Fuchs and Peres' article here, I do not really want to attribute them the *meaning* here intended to those authors. I find the statements at the beginning and at the end of their article contradictory; surely because I have not understood what they mean by 'interpretation'.

> share Bell's conviction that the aim of physics is to understand nature, and that counting detector "clicks" is not intrinsically any more interesting than counting beans. If prediction and control were my aim in life I would have become an engineer, not a physicist.

There is also a facetious side in the assertion (see again Fuchs and Peres' quotation, supra) that quantum theory encodes probabilities for macroscopic events: this would mean that quantum theory is *not* meant to describe 'microscopic' phenomena!

One could argue: 'but in the microscopic domain there cannot be, by the very meaning of "microscopic", any experiential facts, since those phenomena are not immediately accessible to your senses'. And that is true. But in fact the original programme and approach was to try to imagine or to represent to ourselves those microscopic phenomena by means of macroscopic concepts — we could say with Nietzsche [558]:

> — Aber diess bedeute euch Wille zur Wahrheit, dass Alles verwandelt werde in Menschen-Denkbares, Menschen-Sichtbares, Menschen-Fühlbares! Eure eignen Sinne sollt ihr zu Ende denken!

And the beautiful statistical-mechanical and kinetic-theoretical studies of Maxwell, Boltzmann, Gibbs, amongst others, are examples of such a programme. Many physicists today say that this programme is no longer feasible, and some even mention 'proofs' of such impossibility, e.g. Bell's theorem. But even if there really was a theorem showing a contradiction of locality and determinism with experimental data: who cares? Fuchs and Peres [249] say that a non-local theory 'would eventually have to encompass everything in the universe, including ourselves, and lead to bizarre self-referential logical paradoxes'. That is a bizarre statement: Newtonian mechanics is in principle non-local, but that has never hindered anybody to apply it for very concrete purposes, like calculating where a bomb should land, with no paradoxes at all. And non-local theories are studied and used in modern continuum mechanics [204, 205, 207–212, 214, 215, 218]. In any case, with regard to Bell's theorem we have seen in § 17 that its import in respect of locality and determinism is naught. In fact, there is a different theorem, and a very simple one, whose content goes in a very different direction than that of Bell's theorem: it states that any quantum system, in fact, any system *whatever*, can *always* be considered as a classical one in which some measurements are 'forbidden'; see e.g. Holevo [349, § I.7].

Fortunately, the programme of Maxwell, Boltzmann, Gibbs has not been abandoned, and many studies [43–45, 76, 77, 91, 124, 141, 327, 328, 340, 377, 378, 380, 382, 383, 387–389, 391, 392, 394, 576, 626, 635, 676, 724–729] (see also [116–118]) — some more, some less interesting, some convincing, some unconvincing — are pursued today in its spirit. Moreover, thanks to the appearance of freely and publicly available scientific archives like `arXiv.org` (`http://arxiv.org`) and `mp_arc` (`http://www.ma.utexas.edu/mp_arc/`), these studies may finally appear freely, without the ostracism of 'peer-reviewed' periodicals.[26] Representative of this ostracism, still present today, is the

[26] Some periodicals are exceptions; in particular *Foundations of Physics*, to the editor of which, Alwyn van der Merwe, I pay my homages.

following remark by Boyer at the end of a study in which he apparently shows that the Aharonov-Bohm phase shift 'may well arise from classical electromagnetic forces which are simply more subtle in the magnetic case since they involve relativistic effects of the order v^2/c^2' [77]:

> I should also like to thank the Editor for his decision to accept for publication my two papers dealing with the Aharonov-Bohm phase shift and related classical electromagnetic theory, despite the existence of a (minority) opinion among the several referees that urged rejection on the grounds that, although my calculations might be correct, my conclusions were "certain" to be wrong and thus would "lead to unnecessary confusion" regarding the Aharonov-Bohm phase shift.

34. In the previous section I have mentioned a theorem by Holevo [349, § I.7], stating that any physical system whatever can always be considered as a classical one in which some measurements are 'forbidden'. There is an analogous theorem stating that any physical system whatever can always also be considered as a quantum one in which some measurements are 'forbidden'. The proof, that I do not give here [603], is based on the fact that a simplex of any dimension, say D, can always be obtained as a projection of the state space of a D-level quantum system. Projecting a state space (viz., the set of preparation vectors), thus obtaining a new one, corresponds to declaring some measurements unfeasible (i.e., to eliminating some vectors from the outcome-vector set). To understand this fact one may draw a parallel with the standard topology of $\mathbb{R}^n$: you can derive it from the set of balls or from the set of hypercubes: it does not matter which since every ball can be inscribed in a hypercube and every hypercube in a ball.[27] These theorems show that one should not give some, but not 'too much', physical meaning to the particular simplicial and 'Hilbertian' convex structures of classical and quantum systems.

There are some reasons which make me believe that the Hilbertian convex structure of quantum theory is not fundamental, whereas the simplicial one of classical physics is the fundamental one.

First, the horrible dimensional jump of the state space for systems of different dimensionality: if a quantum system has a state space of dimension N (with $N = D^2 - 1$ for some natural D), the next 'larger' quantum system has $2\sqrt{N+1} + 1$ more dimensions![28] Compare this with a classical system, where this dimensional jump is simply 1 instead, independently of N. This has important and annoying consequences in the actual study of some systems. Consider e.g. the case in which we are studying a two-level quantum system which is a subsystem of a larger one. As long as we are interested in measurements and preparations of the subsystem only, we only need to work with 3 real independent variables (from the independent components of the statistical operator). But as soon as we consider a single measurement or preparation concerning the larger system (e.g., we want to keep

[27] But this topological fact is not true in infinite dimensions. Analogously I wonder whether the reciprocal 'inscribableness' of quantum and classical systems holds for infinite-dimensional ones.

[28] This obviously follows from the fact that a D-level quantum system has a state space of dimension $N = D^2 - 1$. Note also that $N = K - 1$ for K as defined in **Papers (C)** and **(D)**.

track of a global quantity), we have to increase the number of variables, and the *minimal* increase allowed by the quantum mechanical formalism is by 5 real variables! It may well be the case that some of these are unimportant for us, but we have to drag them along anyway. Another way is to consider only the useful additional variables as extra parameters. Situations of this kind made it necessary to consider non-completely positive maps and their restricted domains [115, 245, 406–408, 577, 633, 652, 731]. On the other hand, in a situation analogous to the above, but for a classical system, the minimum necessary number of additional variables would just be 1. Classical theories are more flexible. Cf the remarks given in the earlier versions (available at `http://arxiv.org`) of **Paper (B)**.

Another reason, related to the first one, is the excessive number of variables that appear when we 'compose' two or more systems, an operation implemented by the tensor product in the quantum-mechanical formalism. E.g., composing two three-level systems, described by 8 variables each, we suddenly have to handle with 80 variables. In the classical case this number would be 64 instead. The additional variables and the extra correlations that accompany them make me believe that the tensor-product operation of quantum mechanics puts in, physically, some more 'systems' or more generally speaking some 'additional phenomena' than just the systems entering the tensor product. In any case, I think that we ought to consider alternative mathematical approaches to the relation between 'subsystems' and 'supersystems' than Cartesian or tensor products. I made some remarks on this in **Paper (C)** and a mathematical study is almost ready [603].

35. It should be noted hew in the vector framework here presented there is a clear difference between preparations and measurement outcomes, and this difference is reflected in their mathematical representatives, the preparation vectors and outcome vectors. In fact, the convex spaces of these two kinds of vectors can be very different. In particular, *they need not have the same number of extreme points*.

In view of this fact, classical and quantum systems are particular because their outcome- and preparation-vector spaces have not only the same cardinality, but can even be given the same linear, respectively Hilbertian structure. It is this peculiarity that allows us to associate to every 'ket' $|\phi\rangle$ a 'bra' $\langle\phi|$, and vice versa, in quantum mechanics. Hence what I am saying is that in general physical theories a correspondence or 'pairing' analogous to the quantum-mechanical $|\phi\rangle \leftrightsquigarrow \langle\phi|$ does not exist.

This leads me to two brief comments. The first is that I do not like the oft-heard sentence 'the probability that a system prepared in the state $|\phi\rangle$ be found in the state $|\psi\rangle$ is. . . '. This sentence would be meaningless in other physical theories: in general we can only speak about the plausibility that a measurement yield this or that *outcome*; and it is not completely clear to me what 'finding a system in a given state' means. We can infer that it was prepared in a given state, which is a different statement. From this point of view the Kochen-Specker [438] and similar theorems lose their (meta-mathematical) meaning. Note also that the statement that a quantum system 'is found' in some state needs qualification: amongst which states? It usually understood that the states in which it can be found is a complete orthonormal set; but then I do not see why one could not ask for the plausibility (density) that the system be found in the state $|\psi\rangle$ *amongst all possibile states*, orthogonal or not. This question would simply correspond to a positive-operator-valued measure with a continuum of results.[29]

The second comment is that we perhaps ought to search for some 'physical meaning' for the fact that classical and quantum systems have that particular isomorphism between the spaces of outcome- and preparation-vectors.

36. Some people regard the schematisation of what we do with and within physical theories into the propositions $\bar{S}$ etc. as 'operational', as I also did once. But now I must confess that I do not see exactly what is that this adjective should put into relevance, or demarcate, or exclude. Are not all proper physical theories (i.e., excluding toy theories and mathematical *divertissements* like string theory) 'operational'? Which theories are not? I should like to quote a passage from Truesdell's review [706] of a book by Jammer:

> Thus, on page 120 [of Jammer's book], "In contrast to a purely hypothetico-deductive theory, as for instance axiomatized geometry, where primitive notions (like 'point,' 'straight line,' and so forth) can be taken as implicitly defined by the set of axioms of the theory, in mechanics semantic rules or correlations with experience have to be considered and a definiendum, even if defined by an implicit definition, must ultimately be determinable in its quantitative aspects through recourse to operational measurements." This is simply nonsense. If a physicist says, "I take a sphere of one inch radius weighing one pound," why is only the pound and not the sphere or the inch in need of "operational" definition? Cannot the sort of person who derives comfort from "operational" definitions manufacture them for geometry, too? And when we are told, "Mach did not say what 'mass' really is but rather advanced an implicit definition of the concept relegating the quantitative determination to certain operational procedures," are we really expected to find any meaning here, or is it just a smooth transition to the next chapter in a sociological essay?

Surely all proper physical theories are created to mathematically frame and describe matters of experience, and therefore can but be 'operational' in this sense. But surely they also involve abstractions and generalisations, otherwise they would not be theories but mere catalogues (and even catalogues imply a certain degree of abstraction), as Poincaré [594, ch. IX] said with other, famous words. I probably do not need to mention the fact that the very devising and set-up of every physical experiment is imbued with theory. All in all, the main problem with all this discussion on 'operationalism' seems to lie in the maniacal urge to paint as all white or all black something which has more colours than the rainbow.

The propositions $\bar{S}$ etc. are not meant to exclusively represent 'laboratory instructions' or the like. From this point of view the chosen names of 'preparation', 'measurement', and 'outcome' are particularly unfortunate; but I have not yet found more suitable alternatives.[30] In the example of § 20, e.g., the proposition C_c represented the specification of

[29] An example for a two-level quantum system is the positive-operator-valued measure $\{|\Omega\rangle\langle\Omega|\,\mathrm{d}\Omega\}$, where Ω represent the coordinates of a surface element on the Bloch sphere.

[30] I often contemplate using the Latin terms *praeparatio* for the $\bar{S}_j$, *quaestio* or *mensuratio* for the M_k, and *prouentus* for the R_i.

a particular phase point, and the proposition I represented the specification of the evolution operator amongst other things. If these specifications are 'operational' or not does not interest me, in view of the discussion above.

Historical notes and additional remarks

37. The basic ideas behind what I have here called, quite laconically, 'vector framework' have a relatively long history, though less than a century long. I shall try to give some references, but they will be very incomplete.

I think one can recognise two and a half fundamental elements in the framework. The first is the introduction of the notions of preparation (or state), measurement, and outcome; or similar ones. They are the arguments of plausibilities. The second is the association of mathematical objects to those notions, objects that encode the plausibilities and are principally characterised by a convex structure. The 'half' element are the particular relations that we can introduce amongst preparations, measurements, and outcomes; specifically, what I have called 'mixing' and 'coarsening'. I consider this a 'half' element because it is quite naturally derived from the first one.

The natural notions of preparation, measurement, and outcome have basically always been present in physics; but they have often been directly identified with their mathematical representatives, without the formal intermediary of logical propositions. Some may think that such intermediaries are in fact unnecessary; but I disagree. When we add (or subtract) a new proposition in a set of given ones there are usually no dramatical changes in this set, provided the new proposition is not inconsistent with the rest. We simply need to assign new plausibilities (or some become irrelevant, in the case of subtraction); the existing ones usually remain valid. On the other hand, the mathematical structure of the associated mathematical objects may have to be changed dramatically. In this resides part of the usefulness of the propositions as intermediaries. We have seen e.g. how gentle the 'transition' between a classical and a quantum system is from the propositional point of view: we have only taken away some propositions regarding some measurements. And yet the associated mathematical structure has changed from that of a phase space to that of a complex Hilbert space, and the convex structure from that of an infinite-dimensional simplex to that of a finite-dimensional non-simplicial convex body. The first studies known to me in which the notions of preparation etc. were introduced more or less explicitly as propositions are those by Strauß [672], and Foulis and Randall [246–248, 613–615], which also introduced the notion of a plausibility table; see also Ludwig, Dähn, and Stolz [146–148, 485–493, 669, 670], Ekstein [194, 195], Hellwig and Kraus [337, 338], Giles [268–270], Gudder [301–304], Lubkin [484], Holevo [349], Hardy [316]. In other studies those notions are rather connected to their mathematical representatives than to general propositions; but in many cases it is indeed difficult to put a demarcation line. Therefore the studies I mention in the next paragraphs should also be taken into account.

Amongst the first studies, known to me, of the mathematical representatives, especially from a convex-geometrical point of view, I can mention Mackey's book [495], followed by numerous other studies amongst which of particular importance are those by Ludwig, Dähn, and Stolz [146–148, 485–493, 669, 670] already mentioned, Gleason [279], Kochen and Specker [437, 438], Gudder [96, 298–304], Mielnik [524–528], Davies and Lewis [153–157], Hellwig and Kraus [337, 338], Holevo [347–350, 352], Ali, Emch, and Prugovečki [15–17], Bloore [71], Wright [748–751], Harriman [318–324], Ivanović [363–367], Bugajski, Lahti, Busch, Schroeck, Grabowski, Beltrametti [56, 57, 96, 97, 105–

113, 448, 449, 641–645]; see also Jones [404, 405]. In many of these works the notions of state and measurement have narrower connotations than those presented here (some of these authors, e.g., speak about preparations of 'beams' of particles; although I suspect they are aware that their formalism has more general applications). Moreover, the notion of state has almost always been associated to a particular time instant, which is not necessarily the case in my presentation, cf. § 21.

Almost all of the above studies were borne out of a desire to understand quantum theory. There is also another seam of studies, principally concerned with partially ordered sets and various kinds of lattices, that from the point of view of the framework presented here represent the structure induced in the set of measurements (not outcomes; cf. § 26) by the notions of measurement mixing and coarsening explained in § 24. The germs may be found in an article by Birkhoff and von Neumann [69]. Notably, their paper begins thus:

> One of the aspects of quantum theory which has attracted the most general attention, is the novelty of the logical notions which it presupposes. It asserts that even a complete mathematical description of a physical system $\mathfrak{S}$ does not in general enable one to predict with certainty the result of an experiment on $\mathfrak{S}$, and that in particular one can never predict with certainty both the position and the momentum of $\mathfrak{S}$ (Heisenberg's Uncertainty Principle). It further asserts that most pairs of observations are incompatible, and cannot be made on $\mathfrak{S}$ simultaneously (Principle of Non-commutativity of Observations).

Here one wonders what is so *logically* novel about these notions. They may be *physically* novel, or better, unusual; but surely not 'logically' novel. There is nothing in the axioms of logic that contradicts these notions. But the authors continue:

> The object of the present paper is to discover what logical structure one may hope to find in physical theories which, like quantum mechanics, do not conform to classical logic.

But we have seen in the preceding sections that quantum mechanics is just a *particular* application of 'classical' logic and 'classical' plausibility theory, so it surely 'conforms' to both. We see here one of the seeds of that confusion of logic and probability/plausibility theory with physics that still plagues us today. It is remarkable that these two eminent mathematicians planted one of those obnoxious seeds. Fortunately, Strauß [672] (and later Koopman [441]), making analogous studies, recognised the point I have just made. Also Bodiou [72] has in his studies a clearer view than Birkhoff and von Neumann:

> le principe des probabilités composées n'est pas vérifié par les pondérations conditionnées quantiques. On interprète, d'un point de vue classique cette non-vérification en considérant que le "conditionnement quantique" est, en réalité, un "changement de catégorie d'épreuves".

In other words, we have different contexts (or 'sample spaces' as many statisticians would say), one for each measurement; therefore the specification of the measurement M is an essential part of the framework. In our list Pool [599, 600] and Greechie [291, 292] follow next. This kind of studies then joins the seam on convex structures, so I may refer to the references of the previous paragraph. Studies related to the notions of measurement mixing and coarsening, though not from a lattice point of view, are those by Blackwell [70], and Morse and Sacksteder [536].

More recent studies, which I do not try to put into a category or another, are e.g. those in refs [4–11, 37–42, 58–63, 115, 128, 129, 290, 334–336, 417, 482, 483, 579, 580, 665, 666, 739–741, 751, 763]; many of them, although relatively relevant to the vector framework, are specifically concerned with quantum mechanics.

Amongst all the above studies, those by Holevo [347–350, 352] deserve special mention; he systematised the whole framework within the general theory of *statistical models* and proved many important results that are still rediscovered today. Hardy's work [316] also deserves special mention as it presented the basic ideas in the simplest possible mathematical form.

Finally, it is worth mentioning that many intimate connexions exist amongst the vector framework, the theory of statistical models [e.g., 66, 67, 259, 514], and system theory [412, 743, 753] (cf. also [412, 743, 744]).

IV. A synthesis: induction and state assignment

Putting together the Laplace-Jaynes approach and the vector framework

38. You have probably noticed that some notation and terminology from ch. III had already appeared in ch. II, especially in §§ 11–12. And you have probably thought, correctly, that that similarity was not accidental. Consider the following two situations:

(a) We have a problem of induction concerning some measurement instances $\{M^{(\tau)}_{k_\tau}\}$ with outcomes $\{R^{(\tau)}_{i_\tau}\}$; these regards *a given physical system*. We decide to adopt the Laplace-Jaynes approach; we therefore need to specify the set of circumstances $\{C^{(\tau)}_j\}$ and the prior plausibilities

$$\mathrm{P}(R_i | M_k \land C_j \land I) \quad \text{for all } i, k, \text{ and } i \in \Lambda_k, \tag{III.15$_\mathrm{r}$}$$

and this specification we do using the theory describing the physical system.

(b) We have a given physical system for which we have introduced sets of preparation circumstances $\{C_j\}$, measurements $\{M_k\}$, and outcomes $\{R_i\}$, with the associated vectors. In a given collection of measurements we know that the preparation circumstance was always 'the same', though we do not know which. Our problem is to assign plausibilities to the circumstances given the evidence of the outcome obtained. *This is the typical situation that in plausibilistic physics we call 'state assignment' or 'estimation' or 'retrodiction'.*

It is clear that the two problems are faces of the same coin. We solve both at once by applying the formulae for the Laplace-Jaynes approach as given in § 5 of **Paper (G)** or in §§ 11–13 above; in these formulae the plausibilities (III.15), dictated by the physical theory, will be encoded into the outcome- and preparation-vectors, as in eqs. (III.19) and (III.23):

$$\mathrm{P}(R_i | M_k \land C_j \land I) \equiv p_{ij} = \boldsymbol{r}_i{}^{\mathsf{T}} \boldsymbol{x}_j. \tag{IV.1}$$

Given data D consisting in N outcomes of various kinds of measurements, with frequencies (N_i),

$$D \coloneqq \underbrace{R_{i_N}^{(\tau_N)} \land \cdots \land R_{i_1}^{(\tau_1)}}_{R_i \text{ appears } N_i \text{ times}} \qquad (\text{with } i_a \in \Lambda_{k_a},\, a = 1, \dots, N), \tag{II.16$_\mathrm{r}$}$$

the plausibility of the circumstance C_j is therefore given by eq. (II.18):

$$\mathrm{P}(C_j|\, D \land I) = \frac{\Bigl(\prod_{k, i \in \Lambda_k} p_{ij}^{N_i}\Bigr)\, \mathrm{P}(C_j|\, I)}{\sum_j \Bigl(\prod_{k, i \in \Lambda_k} p_{ij}^{N_i}\Bigr)\, \mathrm{P}(C_j|\, I)}, \tag{II.18$_\mathrm{r}$}$$

which can also be rewritten, using eq. (4) of **Paper (C)** or **(D)**, or equivalently eq. (III.19), as

$$\mathrm{P}(C_j|\, D \land I) = \frac{\Bigl[\prod_l (\boldsymbol{r}_l{}^\intercal \boldsymbol{x}_j)^{N_l}\Bigr]\, \mathrm{P}(C_j|\, I)}{\sum_j \Bigl[\prod_l (\boldsymbol{r}_l{}^\intercal \boldsymbol{x}_j)^{N_l}\Bigr]\, \mathrm{P}(C_j|\, I)}. \tag{IV.2}$$

The plausibility that, performing a new instance τ_{N+1} of some measurement M_k, we obtain the outcome R_i is

$$\begin{aligned}
\mathrm{P}(R_i^{(\tau_{N+1})}|\, M_k^{(\tau_{N+1})} \land D \land I) &= \sum_j \mathrm{P}(R_i|\, M_k \land C_j \land I)\; \mathrm{P}(C_j|\, D \land I), \\
&= \sum_j \boldsymbol{r}_i{}^\intercal \boldsymbol{x}_j\, \frac{\Bigl[\prod_l (\boldsymbol{r}_l{}^\intercal \boldsymbol{x}_j)^{N_l}\Bigr]\, \mathrm{P}(C_j|\, I)}{\sum_j \Bigl[\prod_l (\boldsymbol{r}_l{}^\intercal \boldsymbol{x}_j)^{N_l}\Bigr]\, \mathrm{P}(C_j|\, I)} \\
&\equiv \boldsymbol{r}_i{}^\intercal \sum_j \boldsymbol{x}_j\, \frac{\Bigl[\prod_l (\boldsymbol{r}_l{}^\intercal \boldsymbol{x}_j)^{N_l}\Bigr]\, \mathrm{P}(C_j|\, I)}{\sum_j \Bigl[\prod_l (\boldsymbol{r}_l{}^\intercal \boldsymbol{x}_j)^{N_l}\Bigr]\, \mathrm{P}(C_j|\, I)}.
\end{aligned} \tag{IV.3}$$

The last equation shows that *to the conjunction of data and prior knowledge $D \land I$ we can associate an 'effective' circumstance-vector*

$$\boldsymbol{d}_{D \land I} \coloneqq \sum_j \boldsymbol{x}_j\, \frac{\Bigl[\prod_l (\boldsymbol{r}_l{}^\intercal \boldsymbol{x}_j)^{N_l}\Bigr]\, \gamma_j}{\sum_j \Bigl[\prod_l (\boldsymbol{r}_l{}^\intercal \boldsymbol{x}_j)^{N_l}\Bigr]\, \gamma_j}. \tag{IV.4}$$

39. The above formulae are all expressed in terms of a set of preparation circumstances $\{C_j\}$. We have seen that in both the convex framework and the Laplace-Jaynes approach there is a natural equivalence relation amongst circumstances. For the convex framework it was defined in § 25, eq. (III.24); for the Laplace-Jaynes approach it was defined in § 13, eq. (II.19). It is clear that the two definitions coincide.

We also saw that in both frameworks it is quite natural to disjoin equivalent circumstances together, since neither framework can lead to relative differences in the plausibilities of equivalent circumstances, other than those that were already present in the prior data. More precisely: for each pair of equivalent circumstances C', C'', the ratio of their updated plausibilities cannot change and is equal to that of their prior plausibilities:

$$C' \sim C'' \implies \frac{\mathrm{P}(C' | D \wedge I)}{\mathrm{P}(C'' | D \wedge I)} = \frac{\mathrm{P}(C' | I)}{\mathrm{P}(C'' | I)}, \quad \text{for whatever data } D \text{ consisting of outcomes.} \tag{IV.5}$$

In the case of the Laplace-Jaynes approach we stopped short of disjoining equivalent circumstances and 'plausibility-indexing' them. The reason is that we can follow the steps taken in the convex framework instead, as described in § 26: we take disjunctions

$$S_{\boldsymbol{x}} := \bigvee_{j \in \boldsymbol{x}^{\sim}} C_j. \tag{III.34)$_\mathrm{r}$$$

of circumstances having the same circumstance-vector $\boldsymbol{x}$. As you remember, we called these disjunctions '$\boldsymbol{x}$-indexed circumstances', and the vectors $\boldsymbol{x}$ form a convex set.

In terms of $\boldsymbol{x}$-indexed circumstances, our state-assignment eqs. (IV.2) and (IV.3) take the form

$$\mathrm{p}(S_{\boldsymbol{x}} | D \wedge I)\,\mathrm{d}\boldsymbol{x} = \frac{\Big[\prod_l (\boldsymbol{r}_l{}^{\mathsf{T}}\boldsymbol{x})^{N_l}\Big]\,\mathrm{p}(S_{\boldsymbol{x}} | I)\,\mathrm{d}\boldsymbol{x}}{\int\Big[\prod_l (\boldsymbol{r}_l{}^{\mathsf{T}}\boldsymbol{x})^{N_l}\Big]\,\mathrm{p}(S_{\boldsymbol{x}} | I)\,\mathrm{d}\boldsymbol{x}}, \tag{IV.6}$$

$$\mathrm{P}(R_i^{(\tau_{N+1})} | M_k^{(\tau_{N+1})} \wedge D \wedge I) = \boldsymbol{r}_i{}^{\mathsf{T}}\boldsymbol{d}_{D \wedge I}, \qquad \text{with} \tag{IV.7}$$

$$\boldsymbol{d}_{D \wedge I} := \int \boldsymbol{x}\ \mathrm{p}(S_{\boldsymbol{x}} | D \wedge I)\,\mathrm{d}\boldsymbol{x}. \tag{IV.8}$$

40. The state-assignment formulae above *are valid for any physical theory* — or at least, for those theories whose plausibilistic properties can be formalised through the vector framework; but we have seen that classical mechanics and quantum mechanics are counted amongst these. When we change to quantum-mechanical *notation* — because the change is only notational, nothing more — we obtain in fact the formulae of §§ 1 and 2 of **Paper (H)**; in **Paper (I)**, § 3, they have been generalised to data D_f consisting of disjunctions of outcome collections:

$$D_\mathrm{f} := \bigvee_{(i_a) \in \Xi} (R_{i_N}^{(\tau_N)} \wedge \cdots \wedge R_{i_1}^{(\tau_1)}) \qquad (\text{with } i_a \in \Lambda_{k_a},\ a = 1, \dots, N). \tag{II.16)$_\mathrm{r}$$$

In these papers the joint convex-Laplace-Jaynes framework was used in the state assignment for a three-level quantum system.

Remarks

41. I should like to add here some remarks concerning the state-assignment framework presented above.

The first remark concerns a comparison of the Laplace-Jaynes approach in the case of multiple kinds of measurements, and of the approach based on (unrestricted) partial infinite exchangeability [238; 66, § 4.6.2]. Equations (IV.6) and (IV.7) above can be rewritten, when no reference is made to the vector framework, as

$$\mathrm{P}(R_i^{(\tau_{N+1})} | M_k^{(\tau_{N+1})} \wedge D \wedge I) = p_l(\boldsymbol{x} \int p_i(\boldsymbol{x})\, \mathrm{p}(S_{\boldsymbol{x}} | D \wedge I)\, \mathrm{d}\boldsymbol{x}, \qquad \text{with} \tag{IV.9}$$

$$\mathrm{p}(S_{\boldsymbol{x}} | D \wedge I)\, \mathrm{d}\boldsymbol{x} = \frac{\left[\prod_l p_l(\boldsymbol{x})^{N_l}\right] \mathrm{p}(S_{\boldsymbol{x}} | I)\, \mathrm{d}\boldsymbol{x}}{\int \left[\prod_l p_l(\boldsymbol{x})^{N_l}\right] \mathrm{p}(S_{\boldsymbol{x}} | I)\, \mathrm{d}\boldsymbol{x}}, \tag{IV.10}$$

$$p_i(\boldsymbol{x}) \coloneqq \mathrm{P}(R_i | M_k \wedge S_{\boldsymbol{x}} \wedge I). \tag{IV.11}$$

We realise that they are apparently not completely equivalent, in respect of their mathematical form, to those obtained from the theorem on partial infinite exchangeability. The difference consists in the fact that within the exchangeability approach we have

$$p_i(\boldsymbol{x}) \equiv x_i, \quad \text{i.e.,} \quad \boldsymbol{p}(\boldsymbol{x}) \equiv \boldsymbol{x}, \tag{IV.12}$$

and moreover $\boldsymbol{x} \equiv \boldsymbol{p}$ has a definite range, viz. the Cartesian product of the simplices associated to the plausibility distributions of the various measurements M_k:

$$\boldsymbol{x} \equiv \boldsymbol{p} \in \bigtimes_k \Delta_k, \qquad \Delta_k \coloneqq \{(p_i) \mid i \in \Lambda_k, p_i \geqslant 0, \sum_i p_i = 1\}; \tag{IV.13}$$

whereas in the Laplace-Jaynes approach the parameter $\boldsymbol{x}$ belongs to a generic convex space that depends on the meaning of the set of circumstances (e.g., the physical system from which they stem), and must thus be specified by us, with the sole restriction

$$\boldsymbol{p}(\boldsymbol{x}) \in X \subseteq \bigtimes_k \Delta_k. \tag{IV.14}$$

This difference lies clearly in the fact that in the exchangeability approach the parameter $\boldsymbol{x}$ is 'uninterpreted', but not so in the Laplace-Jaynes approach, where $\boldsymbol{x}$ indexes propositions that have particular meanings and whose plausibilities may be dictated by some physical theory[1].

However, the exchangeability-based formulae can be made equivalent to the Laplace-Jaynes ones by appropriately restricting the support of the prior generating function to those values of $\boldsymbol{x}$ such that $\boldsymbol{p}(\boldsymbol{x}) \equiv \boldsymbol{x} \in X$. This corresponds to a particular a priori plausibility judgement with regard to the possible infinite collections of outcomes that we can ever observe; a judgement that may stem from some physical theory. From this point of view there is hence no substantial difference between the two approaches (but see **Paper (G)** for a discussion of other differences in range of applicability and purposes).

[1]Which in the end means: by us, since it is we who, based on our experience, create and distill physical theories.

42. The second remark, which has many connexions with the previous one, concerns a comparison of the framework for state assignment, as developed above, with that one presented by Caves, Fuchs, and Schack [121, 122, 250, 251], based on their 'quantum de Finetti representation theorem' [122, 355, 671]. In **Paper (G)** I lament the fact that the quantum de Finetti approach does not work with quantum mechanics on real and quaternionic Hilbert space; but a general state-assignment framework should apply to *any* conceivable, i.e. self-consistent, physical theory, even one that apparently describes (yet) unobserved phenomena. The framework here presented does satisfy this requirement.

Another criticisms can be levelled at the quantum de Finetti state-assignment approach[2]. One of its basic assumptions is that a collection of quantum systems (even ones localised at arbitrary space-time separations) can be handled as a quantum system in itself — with density matrices describing its preparations etc. A similar assumption was also made by Balian and Balazs [33, 35] in a tentative to justify the maximum-von Neumann-entropy principle in quantum statistical mechanics. I personally see this assumption (which also seems to be the cause of the inapplicability of the approach to real and quaternionic quantum mechanics) as completely unwarranted. We cannot take a collection of quantum systems and gratuitously state that they collectively behave as a quantum 'super-system': this would be a statement with important physical consequences and would surely need qualification. Imagine that we find a report on some quantum experiments made twenty years ago in a distant land, say 7 000 km from here. The report contains a very detailed description (including, e.g., the temperature and humidity of the laboratory et sim.) of a collection of experimental set-ups, each of which can be conceptually divided into a 'preparation set-up', which is always the same, and a 'measurement set-up' which is different for different experiments of the collection. The report also contains the outcome data obtained in these measurements. Now we can reproduce, by carefully following the reported instructions, the preparation set-up and perform new measurements on it and collect new outcome data. I should be willing, in this situation, to use the old and the new data together to assign a density matrix to the preparation set-up. And yet, I see no physical grounds for considering the old and the new experiments as performed on a 'quantum super-system' extending twenty years in time and thousands of kilometres in space!

The reason why in the example I am willing to use the old and new data is simply because I judge the *preparation scheme* to be the same in all old and new experiments (in the precise sense of eqs. (I), (IV) of **Paper (G)** and (II.11), (II.12) above), and want therefore to assign to it a preparation-vector that encode the plausibilities for experiments performed following that scheme. This is precisely what we do in the Laplace-Jaynes approach, without that additional, and in this case also very suspect, 'super-system' assumption.[3]

43. The studies of the above-mentioned papers originate from the observation that it is always possible to analyse a given context at a deeper logical level. Syntactically and

[2] N.B.: I am now speaking about the quantum state-assignment approach, not the quantum de Finetti *theorem*; the latter is in fact a very interesting piece of mathematics.

[3] Moreover, as discussed in **Paper (G)**, with the Laplace-Jaynes approach we do not need to entertain an infinite collection of additional fictive similar experiments either.

semantically this analysis is done by introducing some set of propositions so chosen as to satisfy particular plausibilistic properties (see **Paper (F)**, § 4; **Paper (G)**, §§ 5.1, 5.3). Such propositions have been called 'circumstances' for want of a better term,[4] and I have called the approach based on them the 'Laplace-Jaynes approach', since the main point of view is foreshadowed in Laplace [450] and the idea is briefly but explicitly expressed in Jaynes [393].[5]

What is important to stress is that the *meaning* of these circumstances is quite arbitrary (so long as their plausibilities satisfy the mentioned properties). This means that an analysis into circumstances can be done from the points of view of different theories and even of different philosophies. Hence two (or more) different persons, with background knowledges I' and I'', will choose in general different sets of circumstances $\{C'_{j'}\}$, $\{C''_{j''}\}$, as well as different conditional plausibilities $\mathrm{P}(R_i|\, C'_{j''} \land I')$ and $\mathrm{P}(R_i|\, C''_{j''} \land I'')$, for all the propositions R_i of common interest — call these 'outcomes'. The plausibilities for the circumstances themselves, $\mathrm{P}(C'_{j''}|\, I')$ and $\mathrm{P}(C''_{j''}|\, I'')$, will also be generally different of course.

However, as soon as the two persons perform the 'plausibility-indexing' of their respective sets of circumstances (as described in **Paper (F)**, § 4, and **(G)**, § 5.3), obtaining the new sets $\{S'_{\boldsymbol{p}}\}$ and $\{S''_{\boldsymbol{p}}\}$ with

$$\mathrm{P}(R_i|\, S'_{\boldsymbol{p}} \land I') = \mathrm{P}(R_i|\, S''_{\boldsymbol{p}} \land I'') \equiv p_i, \tag{IV.15}$$

they will find themselves in a sort of formal agreement on the plausibilities assigned to the outcomes R_i conditional on each $\boldsymbol{p}$-indexed circumstance, as the above equation shows. The agreement is only formal because the circumstances $S'_{\boldsymbol{p}}$, $S''_{\boldsymbol{p}}$, that have the same index $\boldsymbol{p}$ will have different meanings to the two persons. This difference will in fact be manifest in the difference between the plausibilities $\mathrm{P}(S'_{\boldsymbol{p}}|\, I')$ and $\mathrm{P}(S''_{\boldsymbol{p}}|\, I'')$.

But also the difference in those plausibilities can be reduced. If there are multiple 'instances' — in the sense discussed in **Paper (G)** — of the outcomes R_i, then the collection of an enough large amount of data will lead, under certain assumptions, to the mutual convergence of those plausibilities. The two persons will then formally agree on all the updated plausibilities involving the plausibility-indexed circumstances $S'_{\boldsymbol{p}}$ and $S''_{\boldsymbol{p}}$, even though the meanings of the latter will still be different. Thus a difference in philosophy or interpretation does not lead to a difference in the mathematical formalism and in the predictions.

44. Some of the points of the previous section, like e.g. the convergence of the updated total plausibilities, are also valid for the approach to induction and to the interpretation of 'plausibilities of plausibilities' based on de Finetti's theorem [e.g., 66, 170, 171, 233,

[4] I sometimes contemplate using the Latin term *condicio*.

[5] As regards Laplace, the above naming is not meant to reflect historical priority or attribution: I have very little historical knowledge on the subject. Laplace is usually presented as conceiving 'physical' causes for what I here denote by C and S. I think his '*causes*' could be read in a more general sense (cf. the passage where he calls 'cause' the fraction of white to black tickets [450, § II]; this is surely not a 'physical' cause). I could have included the name of Bayes, who in his work [53] expresses partially similar ideas; but he seemed to me less explicit than Laplace. Others could have been mentioned as well, like Johnson [403], de Finetti [233, § 20], or Caves [120]. I later discovered that Mosleh and Bier [537] also propose and discuss basically the same interpretation.

239–241, 329, 342; see also 386, 474]. This is also discussed in **Paper (G)**, together with the reasons for which the Laplace-Jaynes approach can be preferable to that based on exchangeability. I think that the former approach is far more suitable to problems in physics, since all theories introduce physical concepts that play the rôle of 'causes', or more generally, of 'circumstances'.

V. Epilogue

Summary and beginnings

45. There are many other topics, problems, conjectures, answers, ruminations about which I should have liked to talk. As said in the prologue, there was no time or space to talk about them. My hope for the present writing is that it have succeeded in showing its own unity, in showing that the discussion has explored a single unbroken territory, though from different directions.

That territory is plausibility logic. We have seen how it allows us to formalise and quantify our conclusions about unknown facts from known similar ones; how it provides insights and clarifications in various physical problems; how it yields a mathematical structure with which we can study the predictions of physical theories, including quantum mechanics, prescinding these predictions from their physical contents.

Did we find in this territory something unseen before? I cannot answer this question. I have seen many, many cases in which a study or a finding, both mine and of others, was apparently new to many researchers and surely to its finders; but a later fortuitous literature discovery showed it to have been found or studied — and often much more deeply — many years earlier. One of the studies here presented that I have, at present, not seen done somewhen else is the mathematical characterisation of what I have called the 'Laplace-Jaynes approach' to induction and to statistical models; especially the introduction of a set of propositions having particular plausibilistic properties that fit this approach. In what I have called the 'vector framework', I have not seen remarked before that the propositions it is based upon need not refer to particular time instants, but can pertain more general situations that extend over time ranges or that are even atemporal. In general those propositions can refer to the usual boundary- and initial-conditions of physical theories. Nor have I seen somewhere else the mechanical algorithm here presented to derive the specific structures of the framework for a given physical theory, given the probabilities specified by that theory, in the finite-dimensional case. Unseen for me was also the derivation through that framework of general state-assignment (and measurement-assignment) techniques as particular problems of induction, and in particular as applications of the Laplace-Jaynes approach. Finally, other passing remarks as those in § 22 and more elaborate ones as those in **Papers (B)** and **(E)** I have not previously seen either.

Is there something to 'conclude' from these studies? I have not drawn any 'conclusions', apart from the fact that there should be more communication and less feeling of self-sufficiency amongst physicists, mathematicians, logicians, and philosophers; that as a rule of thumb we physicists are philosophical asses; that most fundamental claims about quantum mechanics are unwarranted but quantum mechanicians pretend not to notice (fear of seeing their funding cut?); that the majority of scientific activity today is what Truesdell [708, lecture VI] calls 'trade science' and 'religion science'. Am I too pessimistic? No. Am I exaggerating? Not really; but it is true that I am withholding some positive thoughts on purpose. I do not want to do that, because the message that one gets from todays' journals, conferences, meetings is 'All is well in science!', 'We're doing just fine!', 'How intelligent and clever we are!'. Someone sometimes has to tell that in fact not everything is well, and some things are very bad indeed.

Other more physical or mathematical conclusions can the readers draw for themselves. For me the studies here presented are beginnings for future studies and projects.

Future research directions

> The Road goes ever on and on
>   Down from the door where it began.
> Now far ahead the Road has gone,
>   And I must follow, if I can,
> Pursuing it with eager feet,
>   Until it joins some larger way
> Where many paths and errands meet.
>   And whither then? I cannot say.
>
> *Bilbo Baggins* [700]

Plausibilistic physics

46. The work that has been presented here is particularly suited to study so-called 'inverse problems' in physics. The first field that comes to mind is, of course, statistical mechanics, in which we try to get some knowledge about the microscopic description of a given class of phenomena from their macroscopic descriptions, and then the other way round.

The foundations of statistical mechanics (and for definiteness I mean 'classical' statistical mechanics) are today based on the maximum-entropy principle or, more generally, on the maximum-calibre principle [379, 381], thanks to which the theory can consistently be applied to non-equilibrium phenomena. The basic idea of these principles is to assign a plausibility distribution to the preparation circumstances of a system (and 'preparation' can here mean more generally a description of a *trajectory* of the system in phase-space) such that (a) we get expectation values for particular coarse-grained (field) quantities that are equal to the measured values, and (b) the plausibility distribution maximise the Kullback-Leibler divergence in respect of a given 'reference' plausibility distribution.

This procedure is quite successful and completely self-consistent. In non-dissipative systems in equilibrium the reference distribution is chosen by symmetry or invariance arguments (e.g., invariance with respect to the symplectic structure of the phase space, which generally means invariance under the action of the evolution operator). But in some situations, in particular the most interesting ones that concern non-equilibrium phenomena, it is not always clear which reference distribution should be chosen. Examples are provided in the study of granular materials [31, 73, 80–84, 137, 150, 165, 179, 180, 228, 229, 258, 260, 280, 283, 354, 368, 415, 419, 439, 440, 458, 476, 477, 480, 481, 519, 520, 522, 523, 532, 533, 538, 559, 565, 568, 569, 573, 607, 608, 629, 631, 646, 667, 722, 742, 759].

The vector framework discussed in ch. III makes allowance for the application of the maximum-entropy principle to generic systems (in fact it leads to an application in quantum mechanics that apparently differs from that based on the *von Neumann*-entropy principle, as discussed in § 48 infra). But it also suggests alternative approaches. In particular, it suggests that the problem of assigning a plausibility distribution to the possible 'microscopic' circumstances in simply one of state assignment, of the kind discussed in ch. IV. As we have seen, in that approach we must assign a prior plausibility distribution over the possible circumstances; this seems to parallel the specification of a reference distribution in the maximum-entropy approach. The 'state-assignment approach' to statistical mechanics would appear, in a sense, to reflect the historical development of the various models and distributions that have been assigned during the development of statistical mechanics. Consider, simplifying a lot the real developments, that initially a (symplectic-invariant) distribution over the whole phase-space was assigned, but then, comparing its predictions to actual measurement outcomes, it was found that a distribution assigning reduced weights to circumstances corresponding to particle permutations — the famous division by $N!$ — yielded better predictions. This can be seen as a sort of historical 'update' of a prior distribution to a new one conditional on the observed results; although this update did not really followed Bayes' theorem, since it was a 'meta-theoretical' update. Something similar is today happening in the study of granular materials [see refs above]. The state-assignment approach to statistical mechanics goes along the same line, but the update is really done according to Bayes' theorem. Consistency should require that it lead to the same posterior distributions as the maximum-entropy principle. Does it? For which prior distributions? Can the maximum-entropy and -calibre principles perhaps be derived from the state-assignment one?

47. There are very interesting and general results, presented by Murdoch, Pitteri, Robinson, et al. [539–550, 589, 591, 592, 623, 624], on the derivation of the most general laws for continuum media from microscopic particle models, which generalise previous approaches by e.g. Kirkwood, Irving, et al. [359, 360, 421–426, 468–470] in the previous century (of course, there are much older studies, for which see e.g. Truesdell [711]; worthy of mention are those by Waterston [737]). Murdoch's and Pitteri's studies, however, stop short of including thermodynamic phenomena. On the other hand, twenty years ago Jaynes put forward a unifying statistical-mechanical framework [379, 381], a generalisation of the 'maximum-entropy principle', which is directly applicable to non-equilibrium phenomena. It is basically founded on the consideration of probability distributions on path space instead of phase space. It seems to me that Jaynes' framework can be used to extend Murdoch's and Pitteri's work to equilibrium and especially non-equilibrium thermodynamic phenomena (note that I mean something more general than the approaches based on 'local equilibrium' *à la* de Groot & Mazur [296]; see § 54 below), and I should like to study and develop this possibility.

I should also like to apply Jaynes' framework to the statistical-mechanical and thermodynamic study of granular materials [see refs in § 46], and show that from this framework one can derive all results concerning the various fluctuation-dissipation-related theorems that have been presented in the last few years [27–30, 142, 255, 344, 345, 369, 497, 499, 690]. Finally, I should like to put together some results on '*H*-theorems', Markov properties, and other insights by Vlad and Mackey [730], Crutchfield and Shalizi [143, 653, 654], Dewar [167–169], De Roeck, Maes, Netočný, et al. [161, 496, 498, 500], which are scattered in very different publications and apparently do not know of each other, and show that they explain many features and assumptions behind Jaynes' framework.

The use of probability logic in these investigations is essential, and the vector framework is also particularly suited to these investigations.

48. As already mentioned, when we apply the maximum-*Shannon*-entropy principle to a quantum system we obtain results that in general differ from those of the maximum-*von Neumann*-entropy principle. Cf. the discussion in **Paper (I)**, § 4.3. More precisely, the results of the first method depend on the choice of prior distribution. Is there a prior distribution that leads to the same results of the von Neumann-entropy-based approach? Which is it? What is its significance?

49. The application of probabilistic and statistical ideas and frameworks need not be confined to microscopic theories. In 1925 Szilard [683] proposed a probabilistic theory for *thermodynamics*, as opposed to statistical mechanics; i.e., for a *macroscopic* theory with no assumptions about microscopic features. (Also Einstein [189] studied a similar approach earlier). His studies were resurrected during short periods by Lewis [466, 467], Mandelbrot [502–505], and Tisza, Manning, and Quay [696–699]. But I have not seen works pursuing these studies today, although Primas [606] and Paladin and Vulpiani [571] mention Szilard's studies; nor have I seen them generalised to (macroscopic) mechanical theories, if not possibly in Beran's book [65]. I think that the probabilistic, or statistical, approach to continuum mechanics is in many contexts well worth of consideration. The fact that different microscopic models lead, in some contexts, to the same macroscopic statistical features, means that we can do without those microscopic assumptions altogether (just like when in the study of the generic 'vicinity' of points we need not bring whole metrics; just a topology can do the job). Note that I am not depreciating the usual statistical-mechanical approach: in many cases we are in fact interested in microscopic features, and that approach allows us to form some plausible conclusions about those microscopic features. What I am referring to are, rather, those situations in which we are not really interested in the microscopic details, but we introduce them only to get statistical macroscopic (or mesoscopic) theoretical consequences to test against experiments.

Moreover, theories based on Szilard's ideas can be useful as phenomenological 'intermediaries' in the study of the connexion between thermodynamics and statistical mechanics; diagrammatically,

thermodynamics ⟷ 'statistical thermodynamics' ⟷ statistical mechanics.

Such an 'interface' can be quite and especially useful when the phenomena in question are not just thermodynamical, but electromagneto-thermo-mechanical — and even in non-equilibrium.

Rational continuum mechanics

50. More work must be done concerning the foundations of electromagnetism *in pleno* in connexion with non-linear mechanical and thermodynamic phenomena. Although particular ad hoc formulae and data abound, as well as different partial theoretical foundations from first principles, especially within rational continuum mechanics [132, 133, 206, 210, 357, 416, 701–703, 716], a general derivation from first, simple principles is still lacking. It is remarkable, e.g., that there still is no unanimous *general* expression for the stress and energy density for bodies sustaining electromagnetic fields simultaneously with other thermodynamical and mechanical effects. The remarks made in this regard by, e.g., Robinson [623] in his book in the seventies are more or less repeated today by Ericksen [198–202]. (I wonder how it is possible that we are still in such a situation. Perhaps we have become too wont to ad hoc formulae?) Both Truesdell [708, Lecture IV] and DiCarlo [172] point out that the problem lies in the completely different conceptual and mathematical approaches to the mechanics of bodies and to the electrodynamics in bodies: the introduction of space-time as a relation amongst material points in the first, and as a sort pre-existing, absolute object in the second.

As I see it, one should pursue the line of attack discussed by old but still vigorous and ingenious Ericksen [ibid.], based on the idea that the electromagnetic field can sustain forces; but this line could be combined with Noll's [560–563; cf. 712, ch. 1] beautiful (and apparently not widely known) approach to classical Galilean-relativistic mechanics, in which inertial forces are considered as *real* forces (exerted by the rest of the masses of the universe — a point of view in line with general relativity) and the basic axiom is that the net force sustained by a material point, including the inertial ones, is always nought. In this approach momentum is just the expression of inertial forces; hence we have that the stress, that from control-volume or microscopic considerations [359, 422–425] is viewed as a combination of contact forces and of momentum transfer, can be again be simply considered as an expression of forces only. With regard to energy density, I think we would follow Serrin's predicament, motivated by his investigations in thermodynamics [648–651] (see also Owen [570]), to return to work and heat as fundamental quantities. This predicament should perhaps be followed also in general relativity: cf. the various studies that try to 'localize' the energy of the gravitational field [e.g., 93]. Serrin's proposal accords also with the fact that in many systems entropy and energy are non-extensive (cf. Gurtin and Williams [305, 308]).

Beside such a general approach from first principles, and partly as a point of reference therein, I should be curious to compile a sort of 'table' with the interpretations and expressions used by specialists for the magnetic field intensity $\boldsymbol{H}$, the electric displacement $\boldsymbol{D}$, and the polarisation and magnetisation $\boldsymbol{P}$ and $\boldsymbol{M}$ in diverse electromagneto-thermomechanical provinces, from piezo-electricity and birefringence to ferromagnetism, pyro-electricity, and hysteresis. Moreover, from the mathematical point of view it would be advantageous to use the differential-form framework advocated by Hehl, Obukhov, et al. [295, 330–332, 567] (see also [166, 472, 473, 735]), and much earlier even by Maxwell [513], which treats the electromagnetic quantities as differential forms of different degrees.

Surely these investigations would also help in constructing a (classical) relativistic theory of materials supporting electromagnetothermomechanical effects, which today, as far as I know, is basically in the same status as it was when developed by Bressan [79] amongst others. I see some usefulness also as regards field quantisation in non-linear media (the approaches that I have seen in the literature [e.g., 68, 176, 177, 257, 461, 582, 658] are all more or less ad hoc and apparently try to patch the linear-case approach in a way or another. A fresh new start is needed.)

Quantum theory

51. In ch. III I mentioned and briefly discussed the main open question about quantum theory: why that particular convex structure? We have seen that the vector framework helps in seeing and formulating clearly the question. It cannot, however, provide an answer by itself, since it does not contain *physical* principles or laws in itself, although it is very useful in displaying the plausibilistic consequences of such principles once they have been specified, and although it enables one to *prove* that quantum theory can be recovered from a classical theory.

The next step in 'clearing the quantum mysteries' is to think about and introduce real physical models and theories. Not 'toy' theories, of which there are plenty today, but serious, elegant, humanly understandable and 'picturable' physical theories. Perhaps we should begin from where the situation was immediately after Planck's presentation of his radiation law, using, however, the concepts and mathematical apparatus that in the meanwhile have been developed in rational continuum electromagnetothermomechanics [197, 203, 206, 207, 213, 214, 216, 217, 306, 307, 509–511, 570, 634, 660, 710, 712, 715, 716]. Some interesting studies have already been pursued by Jaynes [377, 378, 380, 382, 383, 387–389, 391, 392] whose 'neoclassical' electromagnetic theory has given many predictions identical with those of quantum field theory (e.g., Jaynes [377] derives orbit quantisation condition for the hydrogen atom within neo-classical theory), but without the need of invoking quantisation (see also [43, 45, 98, 141, 325–328, 394, 530, 636, 676]). A connected and still unsolved fundamental problem is that of radiation reaction in classical physics [e.g., 98, 252, 521, 627, 628, 636]. The variety of phenomena that classical mechanics can exhibit — e.g., think of 'orbit quantisation', as simply shown by the Rayleigh oscillator — gives much hope in the programme of deriving quantum theory from the full splendour of the classical theories.

52. Another research idea is partially related to Bell-like theorems: I have found some indications that, when we pass from a local deterministic classical field theory to a 'coarse-grained' one (be it classical or quantum) apt to describe coarser space-time scales, seeming 'non-localities' appear in the latter. This basically comes about from the fact that, whenever we study a compact region of space-time, we always need to specify non-local boundary conditions (e.g., on a time- or space-like cylindrical boundary [92, 93, 114]) for the relevant partial differential equations of the 'finer' field theory. This apparent fact has of course important implications for Bell's 'hidden-variable' theorem, which loses much of its importance (cf. [534, 604]).

Teaching

53. All courses in quantum mechanics that I have hitherto attended are still heavily based on the concepts and points of view of the works of Dirac and von Neumann. But the latest advances in quantum mechanics have shown that the theory can be initially presented with more general — and simpler! — concepts and points of views, related to the vector framework discussed in this work. Amongst the advantages of this framework: (1) the basic concepts are common to classical and quantum physics, and therefore are less counterintuitive to the students; (2) the mathematics is, at a first stage, based solely on *real* vector spaces ($\mathbb{R}^n$), and therefore even younger students may acquire a working knowledge of quantum theory; (3) the formalism used is especially suitable to the study of non-isolated quantum systems. Within this approach, a student may first learn to 'build' a state space and a measurement space that suit *any* particular class of phenomena of interest. The spaces thus constructed may be more or less similar (with a continuum of degrees) to those that characterise classical physics, and the student can freely explore the differences. Then the student learns that, amongst these spaces, there are some — the quantum ones — that describe well many classes of microscopic phenomena. In this way the student has met quantum physics without making conceptual and mathematical 'jumps' from classical physics; and is able, moreover, to identify which features of the quantum theory are general features of any theory, which are generally non-classical, and which are peculiarly quantum.

54. Also most thermodynamic courses are based on formalisms and points of view that are a century old. The progress in principles [see e.g. 197, 570, 634, 660, 710] and in applications (Atkin and Craine [25, 26] review particular examples) that started around the sixties is virtually ignored. Today thermodynamics can be taught in a way completely parallel to classical mechanics: (1) it is a *dynamical* theory, with quantities — including temperature and entropy — that depend explicitly on time (and are, in general, field quantities); (2) it is governed by few (two, in the simplest cases) basic axioms, comparable to

Newton's axioms in classical mechanics; (3) just like in classical mechanics, the specification of particular constitutive equations and of initial and boundary conditions leads to (differential) equations of motions with well-defined solutions. A student can thus study, e.g., not only the equation of state of an ideal gas, but also the behaviour of the thermal quantities of a non-ideal gas in rapid or free expansion.

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Note: `arxiv` eprints are located at `http://arxiv.org/`; `mp_arc` eprints are located at `http://www.ma.utexas.edu/mp_arc/`.

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