C.F. von Weizsäcker's Philosophy of Science and the Nature of Time

Michael Drieschner

Carl Friedrich von Weizsäcker has always regarded the structure of time as being the most important aspect of his philosophy.[1] Since this Festschrift deals mainly with physics I want to emphasize the influence reflections about the structure of time have on understanding physics. This is one way how (good!) philosophy can help physicists to better penetrate the meaning of their own theories.

So as a consequence we deal here with the connection between science and the theory of time. We have to take into account two aspects of time that have been treated quite differently in tradition, namely:

1. Time in its modes of past, present, and future: It is characteristic for an event to change its features by these three modes "in time". Alternative version: each event changes its features by these three modes ... An event that begins with being *possible* becomes *actual*, and then it is *factual*, past. Scientists often regarded this aspect as purely subjective, with no legal place within science.
2. The sequence of events, and measurement of times, represented e.g. as a sequence and count of years or days or nanoseconds. The focus is in this case the sequence of events and the time lapse between them. They do not change in time. Thus in modern science time is a real parameter (t). This represents time in a way that is regarded "objective" in philosophy of science and in logic.

The distinction between these two aspects of time is closely related to the distinction between the A-series and the B-series of events, as described in the well-known paper by the McTaggarts.[2] Nevertheless even the McTaggarts suppose that time is a sequence of events both in future and in past. Thus their view is very close to

[1] This is hinted at in the title of his monumental work: "Zeit und Wissen" (time and knowledge), München (Hanser) 1992. He describes it in the introduction, e.g. pp. 27–32, in more detail in I,6; I,7.

[2] John McTaggart, Ellis McTaggart, The Unreality of Time. Mind XVII (1908), pp. 457–474.

L. Castell et al. (eds.), *Time, Quantum and Information*
© Springer-Verlag Berlin Heidelberg 2003

the "ontology of classical physics" or, as Meyer-Abich calls it, treating the future as a past to come.[3]

It is C.F. von Weizsäcker's contribution to an understanding of science having pointed out the importance of what the true character of future is. This paper is supposed to give an overview of this contribution.

1 Philosophy of Time

Time is so fundamental that a special effort is needed to describe its structure. "So what is time?" Augustine asks. "As long as nobody asks me I know it. But when somebody asks me, and I want to explain it to him, I do not know it."[4]

This is the strange character of time: Everything goes on "in time". But when I say so I am already using a spatial metaphor, for time is not like a container with events "in" it. Temporality is so all-embracing that I barely notice it, and I am almost unable to describe it, if not by spatial images.

Physics treat time like a spatial coordinate. Time is a parameter t that takes on real values from $-\infty$ to $+\infty$. This comes in rather handy for the usual calculations. The parameter t symbolizes the position of the hand on a clock at the time of an event. The laws of physics give us the possibility to *predict* events, depending on the position of that hand.

But the parameter t represents only a small aspect of what time can be. I cannot understand anything from it about my present life, about future being quite different from past, about the fact that I cannot move arbitrarily in time (as I can in space), and much more like that. Physicists are inclined to look at the parameter t as the original description of time, and at everything else that makes up time as "subjective accessories". From this fact we can understand that physicists, when they speculate about reality, think quite seriously about 'reversal of time' or about 'closed loops of time' or about 'several times at once' (where, again, 'at once' is a *temporal* characterization).

In the writings of the founders of philosophy, Plato and Aristotle, a term like 'time' is used only in passing. *Plato*, however, gives the famous definition according to which the world-craftsman, the demiourgos, makes time "of eternity that abides in unity, an everlasting like-ness moving according to number – that to which we have given the name Time."[5] Plato's text itself explains that what is meant is the course of days, months, and years. So this is apparently something similar to what modern physics use the parameter t for. Only for Plato the cyclic character of that process is important.

[3] K.M. Meyer-Abich: *Die andere Ordnung des Lebendigen*. In: R.-M.E. Jacobi, P.C. Claussen and P. Wolf (Hg.): *Die Wahrheit der Begegnung*. Würzburg 2001, pp. 347–366; here p. 351; cf. his paper in this volume

[4] Aurelius Augustinus: *Confessiones* XI, 14: "Quid est ergo tempus? Si nemo ex me quaeret, scio; si quaerenti explicare velim, nescio."

[5] ... ποιεῖ μένοντος αἰῶνος ἐν ἑνί κατ' ἀριθμόν ἰοῦσαν αἰώνιον εἰ κόνα, τοῦτον ὅν δὴ χρόνον ὠνομάκαμεν. Plato, (Tim. 37d7f). Translation: F.M. Cornford: *Plato's Cosmology*, London 1937.

Aristotle, on the other hand, derives time from motion in general; motion does not have to be cyclic. Motion, in turn, he derives from the pair of concepts *potential* and *actual*, fundamental for his philosophy. He defines motion thus: "The actuality of that which potentially is, as such, is motion."[6] This formulation has often been misunderstood, still today some English translations (and most German ones!) give, instead of 'actuality', e.g.: "the progress of the realizing"[7] or "realization of their potentiality"[8]. This translation looks more plausible at first sight, but it is of no use as a definition since the concept of 'realization' presupposes the very *process* that is to be defined.[9] – The definition by Aristotle, read correctly, is especially interesting because it associates *time* with *potentiality*, as we will do below as well.

Augustine, whose treatise on time we have quoted above, is known in the history of philosophy as the first one to treat time in the same way as it is mainly treated in the philosophy of today: He treats it as that strange structure of past, present, and future, where always I am *present* myself and looking from my point of view, which is *now*, onto past and future (which is, again, a spatial metaphor!).

Augustine takes up Aristotle's question how time can *be*; because the past is no more, the future is not yet, and the present is only the division between past and present. Augustine even takes up a formulation by Aristotle where he says that time is within soul. But for Augustine this gets a different color. Whereas Aristotle puts *counting* of time into soul, Augustine adds that future is within soul as an expectation, and past is within soul as a memory. In that way Augustine solves the problem in a quite different way from Aristotle's. He, so to speak, brings the "modern", subjective view into the philosophical discussion.

Before *Kant* could set a new beginning in the theory of time, there had been the invention of classical mechanics by Isaac Newton. Kant takes up Newton's theory. His new idea was to define time as the *form of inner intuition*. This makes time "subjective", on the one hand, according to his transcendental philosophy. On the other hand he was able to give time all the features that are necessary for the construction of classical Newtonian mechanics. Kant's theory of time, which is central for his philosophical system, remains therefor entirely within the framework of classical physics. Time is viewed essentially as a sequence of events, its structure as present, past, and future does not occur in Kant's writings. Thus neither in the Kant dictionary by Schmid[10] nor in that by Eisler[11] the keywords past or future are found. To this it fits perfectly that Kant, in treating causality, considers quite naturally nothing but deterministic causality. A different possibility, i.e. statistical causality, does not

---

[6] ἡ τοῦ δυνάμει ὄντος ἐντελεχεια, ᾗ τοιοῦτον, κίνησίς ἐστιν. Aristotle (Phys. 201a10f.) Translation: E. Hussey: *Aristotle's Physics*, Book III and IV. Oxford 1983. Cf. W. Wieland: *Die Aristotelische Physik*, Göttingen. 1961,[2]1970, p. 298.

[7] Ph.H. Wicksteed, F.M. Cornford: *Aristotle, the Physics*. London, Cambridge 1957.

[8] W.D. Ross: *Aristotle, Physics*. Oxford 1936.

[9] cf. Wieland, l.c.; E. Hussey, l.c.

[10] C.Chr.E. Schmid: *Wörterbuch zum leichtern Gebrauch der Kantischen Schriften*. Jena (Cröker) 1798.

[11] R. Eisler: *Kant Lexikon*. Berlin (Mittler) 1930.

appear until the 20th century. It did occur to Kant no more than to other scholars of the 18th century – we will come back to this.

An especially beautiful formulation of that determinism can be found in *Laplace*, interestingly enough in his treatise on probability: In Laplace's writings probability occurs only in connection with ignorance. The course of events itself is strictly determined in his description. He writes[12]: "An intelligence that for a given instant knew all the forces that animate nature, and the respective situation of all beings that constitute it, if, besides, it was vast enough to submit those conditions to analysis, it covered in the same formula the movements of the largest bodies of the universe and those of the lightest atom: Nothing would be uncertain for it, and future as well as the past were present before its eyes." – In respect to time it is characteristic for determinism that Laplace considers past and future as *present* before the eyes of such super-human intelligence. Time, in its temporal structure, disappears entirely in view of a strict determinism. Everything that happens is equally present in some higher sense.

It seems that for *Einstein* the space-time continuum, that gives, according to his theory of relativity, a place to everything that is real, is *simultaneously* present in the same sense as for Laplace. Einstein gives the shortest and most brilliant formulation in a letter of condolence after his friend Besso died, shortly before Einstein died himself[13]: "Now he has preceded myself a bit even in the farewell to this strange world. This does not mean anything. For us faithful physicists the separation between past, present, and future means nothing but an, although obstinate, illusion." – Here Einstein takes the framework of space-time, that is best suited for the description of measurable events, as the true reality. The structure of time, on the other hand, which we know before any physics, to him looks like an illusion.

In logic, time appears, if at all, in a similar manner. Traditionally logic treats propositions that are valid for all times. They claim, in this sense, "eternal presence". Modern formal logic has its application mainly in propositions of mathematics or logic itself, in any case in propositions that are not assigned to specific times. Even formal temporal logic, as e.g. A. Prior's "tense logic",[14] presupposes as self evident that time consists of a series of events that just "are there". They suppose that the future is nothing but the past to come. It is C.F. von Weizsäcker who, on the contrary, proposes by his idea of a "logic of temporal propositions" a proper status for temporality even in logic, especially for the logic of future. Up to now, though, Weizsäcker only gives programmatic sketches. It would be worthwhile developing those sketches into a system.

---

[12] P.S. de Laplace: *Essai philosophique sur les probabilités*. Paris 1814, p. 2. "Une intelligence qui, pour un instant donné, connaîtrait toutes les forces dont la nature est animée, et la situation respective des êtres qui la composent, si d'ailleurs elle était assez vaste pour soumettre ces données à l'analyse, embrasserait dans la même formule, les mouvements des plus grand corps de l'univers et ceux du plus léger atome: rien ne serait incertain pour elle, et l'avenir comme le passé, serait présent à ses yeux." (translation MD)

[13] P. Speziali (ed.): *Albert Einstein – Michele Besso. Correspondance* 1903–1955. Paris 1972, p. 537 (translation MD).

[14] A. Prior: *Past, Present and Future*, Oxford 1967.

2 Statistical Thermodynamics

In the middle of 19th century the idea began to be accepted that thermodynamics – beginning with the theory of gases, later thermodynamics in general – could be derived from the statistics of its smallest parts. For a gas, e.g., this is the statistics of its molecules. The great advantage of this statistical theory is that it is derivable from very general principles. So it teaches understanding thermodynamics, the science of steam engines, which looked very special in the beginning, as a general theory of approximate description of any physical system. The generality of the objections against that theory corresponded to the generality of the theory itself. Until now among these objections the "reversal objection" is felt to be particularly serious. This objection, first formulated by Lord Kelvin and J. Loschmid in 1875, says: The second law of thermodynamics says that the entropy of a system that is closed energetically as well as materially, can increase or stay the same, but cannot decrease. This describes e.g. two systems at different temperatures. Their temperatures converge when they are brought into contact, the temperatures will never become different by themselves. This "irreversibility" of thermodynamic processes can be derived from the mechanics of a system consisting of very many partial systems, e.g. from the mechanics of a gas that consists of very many (10^{23}!) freely moving molecules.

Now the problem is that mechanics is a reversible theory. This means that with every process the reverse process is possible as well, according to mechanics, where velocities have reverse direction and the sequence of the positions is reversed. Statistics adds to mechanics nothing but a reduction of detail in the description of processes, in such a way that only average values are retained. One cannot see how a reduction like this could change anything about the basic reversibility of the theory.

This problem is stated more precisely in the "reversal objection": Regard any development of a thermodynamic system, where entropy increases. Now imagine that in the basic mechanical system all velocities are reversed. Then also the thermodynamic states the system has just passed will be passed in reversed order, in such a way that entropy will decrease. Mechanically the latter process is possible as well as the former, but thermodynamically it is impossible. Thus it is not possible, says the reversal objection, that thermodynamics is derived from mechanics just by making the description incomplete.

Here the structure of time enters the scene. Boltzmann gives several arguments in defense of statistical thermodynamics. At first he says that usually, in considering thermodynamic systems, we start with a state of low entropy; thus, in regarding *all* mechanical systems that belong to this thermodynamic state, we find an overwhelmingly large probability for an increasing entropy. Later he explains his point of view in regard of the whole universe as follows[15]:

[15] L. Boltzmann: *Vorlesungen über Gastheorie*, 2 Vols. Leipzig 1898, 1896/98; 21910, 90: "Für das Universum sind also beide Richtungen der Zeit ununterscheidbar, wie es im Raume kein oben oder unten giebt. Aber wie wir an einer bestimmten Stelle der Erdoberfläche die Richtung gegen den Erdmittelpunkt als die Richtung nach unten bezeichnen, so wird ein Lebewesen, das sich in einer bestimmten Zeitphase einer solchen Einzelwelt befindet, die

"For the universe, the two directions of time are indistinguishable, just as in space there is no up or down. However, just as at a particular place on the earth's surface we call "down" the direction toward the center of the earth, so will a living being in a particular time interval of such a single world distinguish the direction of time toward the less probable state from the opposite direction (the former toward the past, the beginning, the latter toward the future, the end). By virtue of this terminology, such small isolated regions of the universe will always find themselves "initially" in an improbable state. This method seems to me to be the only way in which one can understand the second law – the heat death of each single world – without a unidirectional change of the entire universe from a definite initial state to a final state."

This is a particularly obvious manifestation of the prejudice of a typical physicist. He thinks that the only true description of the world is the one by equations of mechanics or thermodynamics, where the real parameter t governs processes. And on the other hand he thinks that past, present and future are "subjective" accessories of the individual, that have to be unmasked as soon as they lead to difficulties about the true, objective description by physical equations. – That Boltzmann calls his abstruse proposal the only method to think the structure of time can be read today as his admission of failure.

In the beginning of 20$^{\text{th}}$ century J.W. Gibbs completed Statistical Thermodynamics theoretically. Naturally he encountered the same problem, but he dismissed it rather pragmatically. He writes in his standard treatise[16]:

"But while the distinction of prior and subsequent events may be immaterial with respect to mathematical fictions, it is quite otherwise with respect to the events of the real world. It should not be forgotten, when our ensembles are chosen to illustrate the probabilities of events in the real world, that while the probabilities of subsequent events may often be determined from the probabilities of prior events, it is rarely the case that probabilities of prior events can be determined from those of subsequent events, for we are rarely justified in excluding the consideration of the antecedent probability of the prior events."

Here Gibbs hints, in a rather hidden way, at an idea that should later bring the solution of the problem: Probability is generally applied only to *predictions*, not to propositions on past events. It is possible, admittedly, to give propositions about past events a good sense, like e.g.: "Probably Napoleon was born in 1769". But the uncertainty we indicate by the word 'probably' does not refer to the past fact itself. For

---

Zeitrichtung gegen die unwahrscheinlicheren Zustände anders als die entgegengesetzte (erstere als die Vergangenheit, den Anfang, letztere als die Zukunft, das Ende) bezeichnen und vermöge dieser Benennung werden sich für dasselbe kleine aus dem Universum isolierte Gebiete, "anfangs" immer in einem unwahrscheinlichen Zustande befinden. Diese Methode scheint mir die einzige, wonach man den zweiten Hauptsatz, den Wärmetod jeder Einzelwelt, ohne eine einseitige Änderung des ganzen Universums von einem bestimmten Anfangs- gegen einen schließlichen Endzustand denken kann." – English edition: Lectures on gas theory; translated by Stephen G. Brush. Berkeley 1964; New York 1995.

[16] J.W. Gibbs: *Elementary Principles in Statistical Mechanics*. New York 1902. Reprint: Woodbridge, CT, 1981, p. 150–151

Napoleon was born in 1769 or he was not born that year; the fact exists, independently of whether we know it or not. What is uncertain is what we will possibly know, in *future*. Thus even when we assign probabilities to past facts we mean a *possibility*, our knowledge that may become real in future.

C.F. von Weizsäcker picks up this thread when he gives a refutation of the "reversal" objection in his paper of 1939[17]: The difference between past and future, which is characteristic for thermodynamics, does not mysteriously come into theory by an approximate description. It is rather ourselves who introduce this difference from outside, just in applying probability only to future. This appears to be so self-evident that nobody made it explicit before 1939. In 1971, when his paper was printed again, Weizsäcker himself writes: "When I wrote it I felt that I had set forth something rather trivial". He calls his text nothing but an attempt at explaining Gibbs' words.

In his paper C.F. von Weizsäcker begins by stating that Boltzmann's H-theorem does not imply a difference between past and future. What Boltzmann proves is that with any state of non-maximal entropy all neighboring states have, with overwhelming probability, higher entropy, i.e. past states as well as future ones. Past and future are entirely symmetric. From Boltzmann's assumption of thermodynamic probability (i.e. thermodynamic equilibrium) there follows rather "that a non-maximal value of entropy of a system we know nothing else about is, with overwhelming probability, a relative minimum of entropy", as Weizsäcker puts it.[18] The phrase "we know nothing else about" again indicates the assumption of equilibrium, i.e. of equal probability of all microstates.[19]

For a *prediction*, the original application of probability, this entails growth of entropy. For the past, however, we need additional considerations.

Suppose you know that the system you consider is in thermodynamic equilibrium. Then Boltzmann's considerations are immediately valid, a state of non-maximal entropy is most probably an extreme of a fluctuation. Often, however, we consider a system about the past of which we have or can infer some information. When I see, e.g., a pot of lukewarm coffee on a table I can be rather sure that the coffee was hot before and has cooled down, increasing its entropy. This conclusion seems reasonable, considering European household customs, i.e. from implied *facts* of the past. The idea, on the other hand, that lukewarm coffee could be the result of a fluctuation is absurd, considering imaginable past facts.

Thus the problem how the difference between past and future comes into Statistical Thermodynamics is resolved rather convincingly: We ourselves introduce that difference into our considerations. Once we have drawn out attention to this structure it is not mysterious any more. – It is a pity, though, that this solution, that has been given as early as in 1939, has not yet entered the discussion within the

[17] C.F. von Weizsäcker: Der zweite Hauptsatz und der Unterschied von Vergangenheit und Zukunft. *Annalen der Physik* 36(1939), 275. Reprinted in: Die Einheit der Natur. München 1971, p. 172–182

[18] l.c., p. 174

[19] A detailed discussion is found in: M. Drieschner: *Voraussage – Wahrscheinlichkeit – Objekt.* Berlin etc. 1979, p. 48–57, 215–219.

scientific community. Recent presentations[20] still reproduce Boltzmann's discussion, which apparently is unsatisfactory. Not even Gibbs' idea (of 1902!) has brought a modification of those presentations.

3 Probability

We have already mentioned the connection between probability and the structure of time when we dealt with thermodynamics: Future is the tense of possibility, and quantified possibility is probability. To put it more exactly: future events are *possible*; some of those possible events will be *actual*, then they will be present; and immediately afterwards they will be *factual*, facts of the past.

I am able to try to predict possible events, e.g.: "Tomorrow there will be an eclipse." But there are predictions that are not as unambiguous – this is a discovery of the 18th century – , predictions with a probability. What do we mean if we give a probability? What is, after all, *probability*?

There is the so-called classical definition of probability by Laplace, saying: "Probability is the ratio of the number of favorable cases to the number of possible cases."[21] This applies mainly to the combinatorial considerations that were usual in the beginning of probability theory, e.g. for calculating the chances in games of cards or of dice: The probability to draw a king in a deck of 52 cards is $1/13$, namely 4 (the number of kings, the *favorable* cases) divided by 52 (the number of all cards, the *possible* cases). One sees quickly that this is not a proper *definition*: Laplace himself puts his "definition" under the condition that "we see no reason why one of those cases would occur more easily than any other one."[22] He could have put it more clearly: This applies if we suppose equally probable cases. Thus Laplace reduces unequal probabilities to equal probabilities, but he does not define the *concept* of probability. A true definition of probability, on the other hand, ran into all but insurmountable difficulties. The reason is the inherent vagueness of probability that cannot be removed by a definition, sharp as that definition might be: Apparently probability has to do with relative frequency. But it cannot simply be identified with relative frequency since the latter equals probability only roughly.

The problems with the foundation of objective probability led to the introduction of a *subjective* concept of probability.[23] The latter refers to the subjective assessment

[20] E.g. A. Grünbaum: *The Anisotropy of Time*, 1967. In: T. Gold and D.L. Schuhmacher (eds.) *The Nature of Time*. U.P. Cornell; or: A. Grünbaum, *Philosophical Problems of space and time*. (Ed. by R.S. Cohen and M.H. Wartofsky) Dordrecht, Boston ^{2}1973 (Boston Studies in the Philosophy of Science. Vol. XII; Synthese Library.); similarly in: L. Sklar, *Physics and chance: philosophical issues in the foundations of statistical mechanics*. Cambridge 1996.

[21] P.S. de Laplace: Recherches sur l'intégration des équations différentielles aux différences finies et sur leur usage dans la théorie des hasards. In : *Mémoires de l'Académie Royale des sciences de Paris (Savants étrangers)* 7 (1773) 1776; reprinted in : *Œuvres complètes de Laplace*, Vol. VIII, Paris 1891, p. 69–197; this quote p. 146

[22] Laplace, loc. cit.

[23] Bruno de Finetti since the 1920s; in English: Probability, Induction, and Statistics. London (Wiley) 1972

for the degree of truth of a proposition, made explicit e.g. in the willingness to bet. According to this view the rules of probability theory contain nothing but the conditions for the consistency of such assessments. We can make them explicit in the condition that a bet has to be *fair*.

C.F. von Weizsäcker's contribution to this debate consists, to begin with, in his hint that, according to probability theory itself, the probability is the *expectation value* of the relative frequency.[24]

This insight is not in itself a *definition* of probability, but it contributes to a consideration of consistency. From our joint work a definition has resulted that sounds almost ridiculously simple: "Probability is a predicted relative frequency." – Here the relation to the structure of time becomes apparent: A probability statement always refers to future events. Even if its propositional content refers to the past, as in our example of the Napoleon's birthdate, probability refers to the future possibility that the assertion about the past fact *will* prove true.

For the concept of probability, as for thermodynamics, the inclusion of the structure of time gives amazingly simple solutions:

At first it is clear that predictions do not have to come exactly true. Probability theory itself gives a prediction for the mean *deviation* of relative frequency from the predicted value in an actual series of experiments. In order to specify this prediction, in turn, one can calculate the deviation of those deviations from their predicted value, for series of series of experiments, etc.[25] Thus probability has a hierarchical structure that can be continued as far as one likes. In this structure we can specify the place of the consistency considerations by C.F. von Weizsäcker. The expectation value he refers to in: "probability is the expectation value of the relative frequency" is derived from the probability of the next higher step in the hierarchy, namely the probability of the results of *series* of experiments.

The definition of probability we have given, as a predicted relative frequency, allows us to see the systematic place of the difference between objective and subjective probability: *Prediction* always contains a *subjective* element, predictions may turn out wrong. But predictions made in science are supposed to prove true empirically, i.e. to indicate *objective* facts. We could describe the relation thus: The subjective interpretation of probability emphasizes the character of proposition, of knowledge: the subjective opinion about what the relative frequency will be. For the objective interpretation, on the other hand, the emphasis is on the content of prediction, on the real future relative frequency that would confirm a true prediction.

Let me add a remark on the concept of probability in general. Our definition refers to one of many possible meanings of probability. There could be (and are) other ways to use this word. The structure of the argument is thus: Science deals with relative frequencies and their prediction. I find that what traditionally is called probability usually agrees with my concept of predicted relative frequency. And I also find that for

[24] C.F. von Weizsäcker, *Zeit und Wissen*, München (Hanser) 1992, part II, 4; C.F. von Weizsäcker, *Aufbau der Physik*, München (Hanser) 1985, Chapt. 3.

[25] Cf. M. Drieschner: *Moderne Naturphilosophie*, Paderborn 2002, in more detail in: M. Drieschner: *Voraussage, Wahrscheinlichkeit, Objekt*, Berlin etc. 1979.

this concept of predicted relative frequency some problems that are usually discussed in relation to probability find a solution. But it is quite possible that there are other concepts of probability that are not affected at all by this argument of mine.

Now we can ask whether probability is objective in the sense of a measurable quantity or not. The fact that probability is found in a measurement only approximately is not a counter-argument. For this is true for all measurable quantities. But for probability this inaccuracy is of very fundamental nature: What we measure is a relative frequency; and above we have seen that probability is *not* the same as relative frequency. If we want to interpret probability as a property of a physical system we have to treat it, apparently, as a kind of disposition, a "propensity", as Popper[26] calls it, to produce certain relative frequencies. This propensity does not appear directly as a result of a measurement. What we measure, the phenomenon, depends on the propensity in a well-known way, but it is not identical with it. – In the discussion of quantum mechanics we will come across such structures again.

In spite of the systematically unavoidable inaccuracies of the prediction of relative frequencies probability theory allows exact calculations with real numbers. How is that?

Probability theory has become pure mathematics since its axiomatization by A. Kolmogorov in 1933.[27] The crucial point in his axiomatics is banishing the problematic relation between probability and relative frequency entirely from mathematics into the "application". In his axiomatics he included only the *relations* among probabilities, that could be stated exactly and rather simply. In fact probability theory from the beginning dealt with nothing but relations among probabilities and their "consistency", as mentioned above.

Another brilliant simplification in Kolmogorov's work is his treatment of the so called product rule saying that the probability for event A *and* B is the product of the probability of A alone and the probability of B alone, provided the two events are *independent*. Kolmogorov does not have to give a criterion for the independence of events but he introduces the product rule by a *definition*. This definition reads something like: "We call two events A and B independent if the product rule is true for them." – Seen this way, probability theory is pure mathematics. Mathematicians put aside the problems we mentioned above as "application problems".

We want to introduce the opposite view as well, which proves probability theory to be a *science*. This is again aided by regarding the structure of time. For we can see, from the structure of time, that the most general law of nature is a probability law. Since this is a very special assertion in the framework of our investigations, I will explain it a bit more in detail.

What is a law of nature? – Reduced to the most general scheme every law of nature is a prescription how to get empirically testable predictions from the present state of affairs. Thus our assertion reads: The most general empirically testable

---

[26] Karl Popper: *The propensity interpretation of the calculus of probability and the quantum theory*. In: S. Körner (ed.): *Observation and Interpretation*. London 1957

[27] A. Kolmogorov: *Grundbegriffe der Wahrscheinlichkeitsrechnung*. Berlin 1933. English: *Foundations of the Theory of Probability*, Chelsea Publish. Company, New York, 1950.

prediction is a probability statement. Above we introduced probability as a predicted relative frequency. So now we assert: The most general empirically testable prediction predicts a relative frequency.

Let me give a short sketch of the argument[28]: We can give, evidently, unambiguous "simple" predictions, yes or no. But one could also think of specifying predictions like: "Sometimes yes, sometimes no" in a way to make them empirically testable. This kind of predictions should be *general*, just as the simple ones. This means that they cannot apply, e.g., to a definite number or a definite sequence of yes and no. For this would again be a simple prediction, only for a more complex experiment. – The only general prediction that specifies "Sometimes yes, sometimes no", it turns out, is the prediction of a relative frequency. This means it is a probability.

We then can derive the well-known rules of probability calculus from the definition "predicted relative frequency". In doing so we cannot, as Kolmogorov, introduce *independence* by a definition. But we can specify the independence of events A and B by the condition that the predicted relative frequency for A is the same if either B is the case or if non-B is the case. With those premises we can derive Kolmogorov's theory. Using the structure of time the definition of probability given turns out to be the basis for the whole theory of probability.

We have seen above that we are able to consider at most, as a property of the system, the *propensity* to produce certain relative frequencies. A relative frequency, in turn, is a property of an actual series of measurements. This could be, e.g., a series of 14 throws of a dice, and the result could twice be "1"; whereas the corresponding probability, the propensity of the system to produce the result "1", could have been 1/6. This latter disposition, the propensity, is usually (as in our example) not confirmed exactly by the actual frequency. But the disposition is valid, by its definition, for *any* actual series of experiments. – What does that mean?

It has often been argued if one can apply probability to single events or only to series of events. This is contending about a goat's wool: One can assign probability to the class of all possible series of experiments; but with the same right one can assign probability to one experiment, as representative for that class; for it is constitutive for that class that its series consist all of one and the same type of experiment. – In this description it is a problem, which experiments are "of the same type", with that same probability; it is a question of the skill of the one who devises the experiment to ensure that "same type" for all experiments.

4 Quantum Mechanics

Quantum mechanics can be interpreted as a generalized probability theory. We can understand it much better, again, in considering the structure of time, as introduced by C.F. von Weizsäcker into the interpretation of quantum mechanics.

Kolmogorov's axioms of (classical) probability calculus allow a generalization to a quantum mechanical probability theory. Kolmogorov bases his axioms on the set $\mathcal{F}$

[28] The argument is presented in more detail in: M. Drieschner: *Voraussage, Wahrscheinlichkeit, Objekt*, Heidelberg 1979 (in German).

of random events, where every random event is represented by a set of *elementary* random events. His first axiom reads:

"I. $\mathcal{F}$ *is a field of sets.*"

A field of sets is what is today called a Boolean lattice (of sets). For quantum mechanics we instead use as a first axiom:

"I'. $\mathcal{F}$ *is a lattice of closed subspaces of Hilbert space.*"

The difference between these two axioms contains all differences between classical physics and quantum mechanics; Kolmogorov's other axioms remain the same. Those differences become clearer, again, from considering the structure of time. In fact, basing the theory on a lattice of subspaces instead of a field of sets entails a fundamental *indeterminism*.[29]

Indeterminism mirrors future's peculiarity as contrasted with the past: Future events are *possible*; in general with every event also alternative events are possible: future is open. The quantum mechanical lattice of propositions can be understood most easily as an expression of open future, as a lattice of *predictions*. We find that in this lattice it is never possible to make *all* propositions with certainty, i.e. with probability 0 or 1. There will always be propositions with probability *between* those two extreme values. This is the fundamental indeterminism of quantum mechanics.

One could, in principle, treat the classical lattice of propositions (Kolmogorov's field of sets) as a lattice of predictions as well. (Although in classical physics we can suppose that all predictions can be made "in principle" with certainty, i.e. with probability 0 or 1. Thus probabilities other than 0 or 1 must be due to our ignorance – as Laplace says in his classical formulation of determinism.) In this view the classical lattice of propositions is a degenerate case of the quantum mechanical lattice of propositions which, "accidentally", contains only probabilities 0 and 1.[30] So in Weizsäcker's view of the structure of time, quantum mechanics and classical physics admit a uniform classification, namely as theories of predictions.

Difficulties arise if one comes from the other side, the side of classical physics that presume that all predictions are certain. Such predictions can as well be understood as descriptions of properties that are there *within themselves*. If I can predict with certainty that I will find, e.g., planet X in position *y*, then I can as well say: "Planet X *is* really in position *y*". So predicting the result of a measurement has turned into stating a fact. For classical physics these are, as we can easily see, equivalent. But in quantum mechanics, with its fundamental indeterminism, this does not work any more.

This is apparently the source of many problems for someone who is used to the "ontology of classical physics", and this is where the dissatisfaction of "classical" physicists with quantum mechanics comes from.

In the same spirit "realism" in the interpretation of quantum mechanics asks what the *reality* described by quantum mechanics really is, or what lies *behind* the quantum

---

[29] M. Drieschner: *Voraussage, Wahrscheinlichkeit, Objekt*. Berlin etc. 1979.

[30] Technically speaking the quantum mechanical lattice of propositions becomes a classical one when there is a complete superselection rule, i.e. when no superposition of states is possible, and therefore all observables are compatible among each other.

mechanical description. – We are trying to answer this difficulty with recourse, again, to the structure of time:

The primary purpose of a physical theory is generating empirically testable predictions rather than a description of existing reality. In case the predictions can be made with certainty they can be reformulated, as we have seen above, as a description of reality. But this very possibility is excluded in quantum mechanics – if we exclude, for the moment, rather far-fetched variants like Bohmean mechanics.

In a second step we can specify the question of reality in a deeper way: Certainly every prediction presupposes a fundamental reality that allows describing the facts that form the basis for the prediction, and finally those facts that confirm or disprove the prediction. Niels Bohr calls this essential requirement for the interpretation of quantum mechanics the "necessity of classical concepts".

Fundamental difficulties result from this necessity of the classical concepts, which I can only sketch here: Quantum mechanics gives nothing but probabilities for the results of possible experiments. If we want to describe unambiguously any arrangements and results of measurements we need a language, concepts to describe reality, facts. Niels Bohr says we must be able to describe what we have done and what we have learnt. This is impossible in quantum mechanics alone, for this purpose we need the concepts and theories of classical physics.

But here a problem rises: Quantum mechanics was introduced, it was finally felt, as a relief, because it describes phenomena classical physics was unable to describe. Whenever the results of the two theories differ, quantum mechanics is right, classical physics is wrong. Then how is it possible that the (true) quantum mechanics presupposes, in the end, the (wrong) classical physics?

The practical physicist has an easy answer to this question: Where classical physics is needed for quantum mechanics, namely for describing arrangements and results of measurements, the two theories agree in a very good approximation. Thus we can assume the validity of quantum mechanics and still, in a good approximation, use the concepts of classical physics. For all practical proposes (FAPP, as an acronym) this is quite all right.

The philosopher, though, particularly if he is mathematically and logically minded, wants to know it more precisely. This "FAPP" may suffice for the practically working physicist, but the logician must conclude: Approximately correct means, if you take it seriously, wrong. So the whole theory is apparently inconsistent!

The discrepancy that shows up here also formally appears in the description of the process of measurement. Here we cannot present the theory of measurement in every detail, but let me at least sketch a rough outline.

The process of measurement in quantum mechanics is interesting mainly because the theory is indeterministic. This means that before measurement several results are *possible*, but after measurement only one of the results has become *actual*. It is true, this occurs in classical physics as well. But there we can console ourselves with the thought that "in itself" already before the measurement there existed but one possibility, and that it was only our ignorance that forced us to take more than one possibility into consideration. But in quantum mechanics even with the most exact description there remain, in general, more than one possibility for the result of a measurement. The theory is fundamentally indeterministic.

The *state* of the system leaves open, before measurement, several possibilities with their corresponding probabilities. After measurement this diversity is reduced to one single possible case. This case then has got (for an immediately following second measurement) probability 1. That change of state is called the "reduction of the wave packet".

There are "realists" among physicists (or, still more, among philosophers of science) who look for a physical mechanism that brings about this change of physical state. But if we take the structure of time seriously, as explained above, we can see that we do not need a physical mechanism. In fact, a prediction with more than one possibility *means* nothing but that in the end one of those possibilities will be realized, the others not. That is what is meant by the predictions of quantum mechanics. The change in description after measurement is the physicist´s own decision. He could just as well continue with the old description and keep all prior possibilities for further predictions, including corresponding probabilities. Then he would waive the chance of using the information won by the experiment, but that latter description would be as valid as the first "reduced" one. It is obvious that there *can* be no physical mechanism within the described system for a decision of the one who describes that system.

There is a possibility to waive information from measurement in the formal description of the process of measurement as well. We would presuppose for that description that a measurement has actually taken place, and that, consequently, one of the possibilities has become actual; but the information *which* one has become actual is waived. Then our description would contain all possible outcomes with the respective probabilities, it would be a "mixture" of states.

The most interesting point in this description is that this *mixture* is different from the *state* that results from the initial state of system + measuring instrument by the measuring interaction according to the Schrödinger equation. A long discussion has shown that here is a fundamental problem we cannot get rid of by simple tricks. The generation of a mixture described above has also been called – misleadingly – "reduction of the wave packet". I rather recommend using the more precise "disappearing of the interference terms". For the difference between the two descriptions is that the correlations between system and measuring apparatus, which are present after the measuring interaction in the *state* description, have disappeared in the *mixture*. This change is usually called the *cut* between system and measuring apparatus.

We can look upon this fundamental problem of the theory of measurement as the formal expression of the problem of *classical concepts* mentioned above: If we want to describe unambiguously what we have measured we have to waive the remaining correlations between system and measuring apparatus. With a good measuring apparatus this can be done quite easily. For in that case the interference terms are so small that they play no role for any practical purpose ("FAPP"); so there again is no problem for the practical physicist. But if we look closely we see that those interference terms, however small they may be, will always exist, they will never be zero exactly. Thus, strictly speaking, it is a mistake to neglect them. Eugene Wigner, who discussed this problem very carefully,[31] finally could offer no other way out than adding small non-linear parts to quantum mechanics that make the interference terms disappear within a short time after interaction.

In my opinion Wigner's solution is wrong. Again Weizsäckers analysis of the structure of time helps to solve the problem:

We are dealing with predictions within *physics*. Physics, however, contain *approximations* in its very foundations. This is seen rather easily: We can do physics only if we can deal with objects independently of their environment. But "in reality" there are no separate objects; everything is related with everything. One can see this already in celestial mechanics: Conceptually isolating e.g. a planet in the solar system from the totality of celestial bodies means an approximation. Strictly speaking, according to physics itself, every celestial body, however far it may be, has an influence on our planet from gravitation alone, not mentioning other types of interaction. These influences are so small that they can be practically neglected, but strictly speaking they are there. Treating the planets only under the influence of the sun and neighboring planets is an approximation and so, strictly speaking, it is wrong. If we did not use that approximation, however, we could not do physics at all. And, above all, we cannot describe a strictly independent object at all: At least an interaction with the measuring apparatus must exist in order that the object can be an object *for me*.

The approximation introduced by neglecting the interference terms is of exactly the same sort: We neglect the very small interaction that relates system and measuring apparatus still after measurement. Thus we introduce an approximation of the same sort as we have introduced in the very foundations of physics. – Translating this into the language of the structure of time means: We can give, fundamentally, only approximate predictions. This is true from their probability character alone, since probability propositions cannot be verified exactly. But it is true because of the fundamental approximation character of physics as well, which says: We only can give predictions about isolated objects which, strictly speaking, do not exist.

During the last decades many proposals have been published to solve this problem, e.g. under the name of "consistent histories" or of "decoherence".[32] Those proposals amount to the same solution under a different name, namely to the old suggestion to neglect the interference terms ("FAPP"). Unfortunately, the authors of such recent proposals give the impression that they could now offer, differently from the old authors, an *exact* solution of the problem. I am full of understanding, since making big noise is part of the business. But this claim would mean more than one could make good.

A common argument against the view put forward here reads like this: "One who puts so much emphasis on *predictions* has only eyes for the possibilities of manipulation, he has an 'instrumentalist's' view of nature. Genuine philosophy of

[31] E. Wigner: *Remarks on the Mind-Body Question.* In: I.J. Good (ed.). The Scientist Speculates, London 1961. New York 1962, p. 302; reprinted in: E.P. Wigner: *Symmetries and Reflections*. Bloomington and London 1967, p. 171–184.

[32] Cf. e.g. the works of Detlev Dürr or Roland Omnes and their collaborators.

nature should inquire more deeply, namely about what the *real* basis is, maybe hidden, of the outer appearances." A suggestion like this is, not easily recognized, founded again on the ontology of classical physics. For it presupposes that "in itself" and *behind* the appearances, there is something else that perhaps does not show itself easily but whose description is the genuine goal of philosophically oriented science.

A program like this may be understandable from the point of view of classical physics. But there is nothing to justify it in this generality. For if we ask, according to empirical science, about the general structures of reality, we ask about an *objective* description in the spirit of this science. This means we ask about a description that *everyone* could in principle verify *at any time*. But if we will be able to verify a proposition empirically this proposition must be a prediction: We must be able to look if it is true *after* it has been made. This is what we brought out by our analysis of the structure of time. It is a specialty of classical physics that such predictions can also be formulated as descriptions of reality *in itself*. What makes this speciality possible is the fact that in classical physics with maximal knowledge all predictions can be made with probability 1 or 0. Where we cannot presuppose that any more, as e.g. in quantum mechanics, there is no such *reality* in itself any more. But objective description is still possible. – Anyone who calls this view, from the perspective of the ontology of classical physics, "instrumental", spoils every chance of understanding a more generally objective description of reality.

We see that C.F. von Weizsäcker's analysis of the structure of time is not only helpful for the interpretation of science but that it is indispensable for that task. Apparently the results of science become entirely incomprehensible for anybody who tries to keep the structure of time out of his interpretation.