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Michael Drieschner *Editor*

Carl Friedrich von Weizsäcker: Major Texts in Physics

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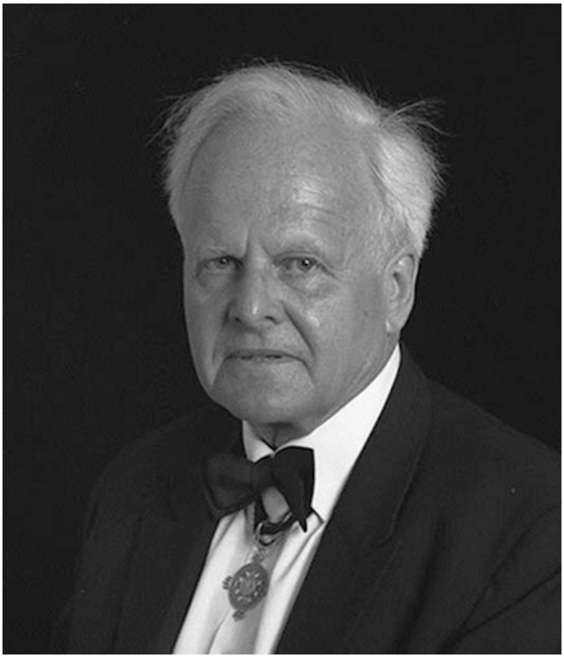

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- Michael Drieschner (Ed.): *Carl Friedrich von Weizsäcker: Major Texts in Philosophy*. Springer Briefs on Pioneers in Science and Practice No. 23. Subseries Texts and Protocols No. 11 (Cham–Heidelberg–Dordrecht–London–New York: Springer-Verlag, 2015).

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- See also the website on these five books with texts by Carl Friedrich von Weizsäcker, at: <http://afes-press-books.de/html/SpringerBriefs_PSP_C.F.v._Weizsaecker.htm>.

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1. "On Elementary Transmutations in the Interior of Stars. Paper II", in: *Physikalische Zeitschrift* 39(1938): 633–646, new translation.
2. "The Formation of the Planetary System", in: *Naturwissenschaften*, 33(1946): 6–14, new translation.
3. Introduction to "The History of Nature", in: *The History of Nature* (Chicago: Chicago UP, 1949); translation of: *Geschichte der Natur* (Göttingen: Vandenhoeck & Ruprecht, 1948).
4. "The Second Law and the Difference between Past and Future", in: *The Unity of Nature:* (New York: Farrar Straus Giroux, 1980); translation of: *Die Einheit der Natur* (Munich: Hanser, 1971): II, 2.
5. "A Sketch of the Unity of Physics", in: *The Unity of Nature:* (New York: Farrar Straus Giroux, 1980); translation of: *Die Einheit der Natur* (Munich: Hanser, 1971): II, 4.
6. "Quantum Theory", in: *The Unity of Nature* (New York: Farrar Straus Giroux, 1980); translation of: *Die Einheit der Natur* (Munich: Hanser, 1971): II, 5.
7. "Probability and Abstract Quantum Theory", in: *Brit. Journ. for the Philosophy of Science*, 24 (1973): 321–337; *republished in: The Structure of Physics*, (Heidelberg, Springer, 2006); translation of: *Aufbau der Physik,* (Munich: Hanser, 1985): 100–111.
8. "The Philosophy of Alternatives", in: *Quantum Theory and the Structures of Time and Space I* (Munich: Hanser, 1975): 7–9; 213–229.
9. "Matter and Consciousness", in: *The Unity of Nature* (New York: Farrar Straus Giroux, 1980); translation of: *Die Einheit der Natur* (Munich: Hanser, 1971): III, 3.
10. "Matter, Energy, Information", *The Unity of Nature* (New York: Farrar Straus Giroux, 1980); translation of: *Die Einheit der Natur* (Munich: Hanser, 1971): III, 5.

Contents

Chapter 1
Carl Friedrich von Weizsäcker: Major Texts in Physics—A Brief Introduction by the Editor

This volume is part of a small series that intends to make major texts by the German scientist and philosopher Carl Friedrich von Weizsäcker (1912–2007) available in English to a wider audience. Through his writings, many of which have already appeared in an earlier English translation, he has become known as a preeminent theoretical physicist and one of the most important German philosophers of the 20th century. This volume is devoted to texts in physics while 4 other volumes of this series present an overview of his writings, selections of texts in philosophy, religion, and on politics and peace research, respectively.

When planning his university studies in 1929, Weizsäcker had been mostly interested in philosophical questions. Since by then he had known Werner Heisenberg for some time, he talked his plans over with him. He relates that Heisenberg told him,

> "that to practice philosophy relevant to the twentieth century one had to know physics, that one learns physics only by practicing it, and that one succeeds best in physics before thirty and best in philosophy after fifty. I followed his advice, studied theoretical physics, and have never regretted it."[1]

Weizsäcker studied physics in Heisenberg's group, the members of which were occupied mainly with the newly discovered quantum theory. Weizsäcker wrote about his works in physics:

> "I chose nuclear physics as my specialty, succeeded in doing some satisfactory work (on the partially empirical theory of nuclear masses, on nuclear isomerics, and on dual (3-)disintegration), and wrote a textbook on atomic nuclei that I unfortunately did not later republish because of other preoccupations. Because of my old love, I chose astronomy as the third subject, after physics and mathematics, for my doctoral examination. While writing the book, I realized that the knowledge of nuclear reactions now permitted an assault on Eddington's problem of the energy sources of stars. I thought out the carbon cycle that Bethe found simultaneously, and which he worked out more thoroughly. This

[1] See CF. von Weizsäcker (1988). See also the website on this book on Carl Friedrich von Weizsäcker, at: <http://afes-press-books.de/html/SpringerBriefs_PSP_C.F.v._Weizsaecker.htm>.

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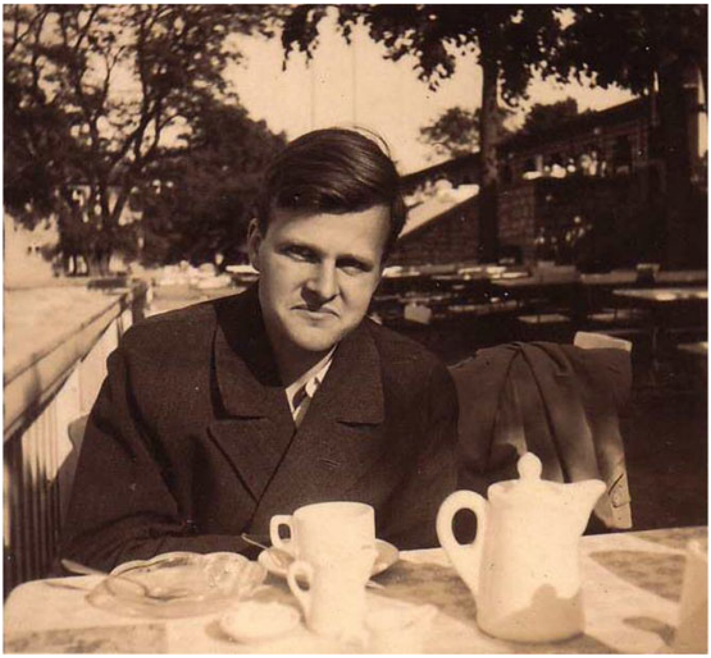
lead to the question of the historical development of stars and to drafting a theory on the evolution of the planetary system, which gratified me. I later recognized it as an application of Kant's theory, using modern methods."[2]

The "partially empirical theory of nuclear masses" had been found by Bethe simultaneously, as well. Both results have become rather famous under the names of "Bethe-Weizsäcker-formula" (1935) and "Bethe-Weizsäcker-cycle" (1937/1938), respectively. We reprint here the second part of Weizsäcker's paper on the energy source of stars (Chap. 2), where the formula for the 'cycle' appears only in passing (end of Sect. 2.2.2); the paper also mentions the oxygen cycle by which the energy could be produced as well. In view of physics that paper by now is old-fashioned. We reprint it from historical interest, and because it is a good example for the rather tentative style of publications at the beginning of a new field of research—nuclear physics, in that case. In 1938 Weizsäcker also published the paper he mentions on the formation of the planetary system.

The original papers on the mass formula and on the planetary system are too technical to be of interest in this collection. But we include the reprint of a public talk he held several times about his theory of the planetary system (Chap. 3).



Carl Friedrich von Weizsäcker (1936). © The Weizsäcker Family represented by Dr. Elisabeth Raiser who granted permission to use this photo

[2] Cf. footnote 1.

There he continued the theory proposed by I. Kant and P.S. Laplace around 1800, in applying the newer theory of turbulent motion.

During the war Weizsäcker participated in a group—led by Heisenberg—that worked on nuclear energy and the development of an atomic bomb. Actually, he was never interested in technological questions. But he had dreamed, as he confessed later, of gaining influence on Hitler's policy through his knowledge of nuclear physics; in an Interview of 1984[3] he said,

> "I say, looking back, that it was only by grace of God that I was saved—because it didn't work. For it would have been deadly gone wrong." Probably the draft of an application for a patent on a plutonium bomb (which was found in Moscow in the 1990s) should be seen in the connection with that dangerous dream. In any case we can imagine that he welcomed one consequence of his participation in the 'uranium project', namely not being drafted into service at the front during the war. About his and Heisenberg's intention while working for that project, he writes: "Throughout the war, I collaborated in the 'uranium project'. But we were spared the decision of whether or not to construct atom bombs for Hitler. After little more than a year, we realized that this was far beyond our capabilities, and we concentrated on working on a reactor model."[4]

After the war he turned more and more—as he had planned at the beginning of his studies—to philosophical questions. Already in 1943 the first edition of his "World View of Physics" had appeared, at that time containing only five papers (the newer editions contain 18 papers). In 1946 he gave a series of lectures in Göttingen about *The History of Nature*[5] that turned out to become one his most important philosophical texts. We reprint the introduction (Chap. 4) where he emphasizes for the first time the importance of a philosophy of time, and proposes his image of a circular movement ('Kreisgang') as the best way to do philosophy.

As a first application of his emphasis on time in physics, he presented the explanation for a problem that had occupied philosophers of science for decades already: The question how the 'direction of time' comes into physics, since the fundamental theories are all 'reversible', i.e. they do not distinguish formally between past and future. Thermodynamics, on the other hand, the theory of heat phenomena, is irreversible: If, e.g., an iron rod is heated on one side, the heat will dissipate over the whole rod, but never a rod of equally distributed temperature will heat up on one side, by itself. The problem appears with the possibility to derive thermodynamics from the mechanics of the molecules with statistical methods: how can a reversible theory turn irreversible just by applying statistics, i.e. by neglecting the details? Weizsäcker presented his solution in a paper called "The Second Law and the Difference between Past and Future" (Chap. 5): It is the theorist himself who introduces the difference between past and future in applying probability consideration to the future only but not to the past. It is interesting enough that physicists were very reluctant to accept this rather simple solution; the

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[3] "Die Atomwaffe", Interview 1984 with H. Jaenecke for the periodical *Stern*, in: Carl Friedrich von Weizsäcker (1988).

[4] Cf. footnote 1.

[5] See Carl Friedrich von Weizsäcker (1980).

question has been presented as unsolved even after Weizsäcker had found the solution—a rewarding task for historians of science and psychologists of scientists.

In 1965, Weizsäcker gave a speech "A Sketch of the Unity of Physics" (Chap. 6), addressed to the annual meeting of the German Physical Society. It is a philosophical account of the unity physics could acquire. He based his considerations mainly on the concept of experience and on the structure of time: "experience [means] roughly, that we can learn from the past for the future." 'Sketch' here is a translation of the German word 'Entwurf', which could be translated into English as 'draft' or 'program' as well: It is a fascinating philosophical essay grouped around the terms 'unity', 'physics', 'experience', and 'time', where 'unity' or 'the whole' or (in Platonic language) 'the One' is a central concept of Weizsäcker's philosophy.

Weizsäcker calls the text, "Quantum Theory" (Chap. 7), 'the centre of the book', namely of his volume *The Unity of Nature*.[6] Five of our texts are taken from that collection; it is the most comprehensive representation of his philosophy of physics. The text "Quantum Theory" gathers in a concise form most of Weizsäcker's ideas concerning the foundations of modern physics: It starts out with an account of the 'Copenhagen interpretation' of quantum mechanics, emphasizing the role of 'classical concepts' in Niels Bohr's sense. It continues with a discussion of the 'plateaus and crises' in the development of science (something like 'normal science' and 'revolutions' in the language of Thomas S. Kuhn). Weizsäcker agrees with Karl Popper (and David Hume) that there is no empirical justification of general laws of nature, but he proposes an entirely different solution, namely that the laws of nature are not truly empirical. He proposes a foundation of logic on something he calls 'tense logic', which nevertheless is quite different from the tense logic of, e.g., A.N. Prior: It means logic derived from the structure of time; the logic for the future would then be probability theory—a short account of what he exposes in more detail in the following text. He continues with an account of founding quantum mechanics on very general postulates that apply to any scientific theory. Finally he gives a short description of his proposal of the 'theory of ur-objects' as a fundamental physics of elementary particles. In short: The text is an overview of what is described in more detail in other texts of this volume.

In the text "Probability and Abstract Quantum Theory" (Chap. 8) Weizsäcker presents his consideration in more detail: Starting from the observation that neither probability can be founded on experience nor experience on probability, he gives a presentation of his foundations of probability, leading to the definition that probability is predicted relative frequency. After a digression into the question of empirical determination of probabilities he discusses methodological aspects of a 'reconstruction' of quantum theory and then gives another account of his proposal for such a reconstruction: Similar in its result to the account given in "Quantum Theory" (Chap. 7), it is a different presentation that may be read as a comment to the other one.

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[6] See Carl Friedrich von Weizsäcker (1980).

Starting from his considerations about the unity of physics, Weizsäcker's program led to a proposal for that unity which put forward the idea that 'atoms' in the strictest sense could be the key to the fundamental problems of physics: In quantum mechanics the smallest possible objects are those with a two-dimensional state space (which he calls "ur-objects"). These objects are indivisible, literally 'átoma', because parts of such an object are not even thinkable, they are impossible even conceptually. Weizsäcker presented this idea in "Philosophy of Alternatives" (Chap. 9) in the first of a series of biannual conferences at his institute in Starnberg 1974–1984 (after having worked on it for many years). He based his considerations on the fact that the symmetry of the two-dimensional state space, SU(2), is 1:2-isomorphic to the rotational symmetry of ordinary three-dimensional space, SO(3). He uses this structure in order to suggest a fundamental physics based on the combination of very many of those 2-dimensional ('ur'-)objects. We reprint the introduction and the philosophical keynote of the first conference; the mathematical details are contained in the conference volumes "Quantum Theory and the Structures of Time and Space", vols. I–VI. The mathematics of Weizsäcker's proposal turned out to be so complex that until now it has not become clear whether that proposal is viable.

The last two texts of this volume are genuinely philosophical, one could even say speculative in nature: "Matter and Consciousness" (Chap. 10) starts out from the question, sometimes asked in connection with the quantum mechanical measurement process, how the result of a measurement finally comes into the consciousness of the observer. Weizsäcker discusses the question under the headings of 'How is matter in consciousness?' and 'How is consciousness in matter?' One of his daring theses is: "The body is the soul insofar as the soul can be perceived as an object by the senses." And he concludes: "Physics is possible only against the background of negative theology," referring to texts in the following volume "Major Texts in Philosophy" (PSP 23) of this series.

The text "Matter, Energy, Information" (Chap. 11) is reprinted here because it is a characteristic example of Weizsäcker's way of combining foundations of modern science with interpretation of classical philosophy. 'Matter', in Latin 'materia', is a translation of the Greek 'hyle'. In Aristotle's philosophy, hyle was what could take on a form; it was defined in that way to be the counterpart of Aristotle's concept of 'eidos', in Latin 'forma'. But now Weizsäcker turns to modern physics, where matter and energy are in a certain way the same, according to Einstein's famous formula $E = mc^2$. They are both 'substance' in that their quantity is conserved. What is 'substance'?—Descartes divides the world into two substances, namely 'res extensa' and 'res cogitans', the extended and the thinking substances, respectively. The extended substance—matter—here is what can be described geometrically. Thus it is actually form, Aristotle's counterpart of matter. How concepts change in history! And the term 'information', on the other hand, is derived from Aristotle's concept of 'forma'. And information is, in its modern scientific formalization, closely related with probability. Weizsäcker discusses all these relations in a fascinating overview, including a digression on economic goods and money, and says, e.g., "matter is the capacity to move itself". He ends with a sentence from classical metaphysics: "God is not the totality of forms, but their ground."

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Chapter 2
On Elementary Transmutations in the Interior of Stars: Paper II (1937)

2.1 Problems with the Build-Up Hypothesis

2.1.1 The Significance of the Build-Up Hypothesis

In a foregoing paper it was attempted to establish whether transmutations of atomic nuclei occur in the interior of the stars and what significance these transmutations have on stellar structure and development.[1,2] Further investigations have shown that some of the hypothetical presumptions made there cannot be upheld. Consequently, the present paper cannot present any quantitative implementation of the theory; it confines itself to a renewed qualitative discussion of the problem under modified preconditions.

The theory is initially expected to predict, at least in certain simple cases, which nuclear reactions spontaneously occur in a piece of matter of given physical and chemical properties. Its task does not end there, however. As we cannot directly observe the physical and chemical conditions prevailing in a stellar interior, the theory must first define them itself. At this point a hypothesis is needed, as we do not know a priori whether other hitherto unknown effects alter these conditions or are not taken into account besides elemental build-up by nuclear processes whose quantitative description is the aim of the theory. In Paper I, it had been assumed that such effects were not present; this assumption was called the build-up hypothesis *Aufbauhypothese*.

This version of the hypothesis contains another uncertainty, though. The nuclear reactions exert two different influences at the same time: They change the physical state of the matter by releasing energy and its chemical composition by

[1] This text was originally published as: "Über Elementumwandlungen im Innern der Sterne. II", in: *Physikalische Zeitschrift*, 39 (1938): 633–646, This text is for the first time available in English and was translated into English by Ms. Ann Hentschel with the financial support of the Udo-Keller-Foundation (*ed.MD*).

[2] See von Weizsäcker (1937).

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transmuting the elements. The generation of energy is the unproblematic part of the theory to consider: Nuclear reactions or effects of similar energy yield are necessary to explain stellar radiation; and the build-up hypothesis is equivalent to the assumption that the nuclear processes sufficed for that on their own as well. Transmutation of the elements, however, is to a certain extent a side-effect of the nuclear reactions, yet nothing is known about its importance in the history of stellar lifetimes. The empirical frequency distribution of the chemical elements exhibits characteristic regularities apparently quite uniformly valid throughout the entire cosmos, which compel us to attempt to explain it by assuming a uniform formation process. It would suggest itself to look for this process in the element transmutations necessarily connected with the generation of energy in the stars. Yet we cannot exclude at the outset the possibility that the chemical elements were formed by another process prior to the formation of the stars as we know them today and that the present energy-generating reactions only brought on a slight change in the original frequency distribution. Hence we must distinguish between a narrower interpretation of the build-up hypothesis, which is limited to the role of energy production by the nuclear reactions taking place in a star today, and a broader interpretation that does not take into regard any other processes of element formation in the history of the cosmos besides the connected element build-up.

In Paper I, it was argued that the broader hypothesis was the simplest of possible assumptions. It became evident that it was impossible to make the same process directly responsible for the generation of energy as well as for element development because the deposit of hydrogen, which is necessary for energy production, does not lead to the build-up of heavy elements. However, a causal link was established between both processes by the assumption that the energy-generating reaction produced neutrons as a side-product, which was then supposed to take over the further element build-up. In attempting to carry it out quantitatively, this assumption now runs up against a series of problems that seem hardly surmountable. First, it is uncertain whether neutrons form in notable amounts and—if they did develop—it seems certain that helium would have to be produced at the same time in an amount that would be irreconcilable with the astrophysical data on the frequency of that element. Second, the build-up of considerable amounts of uranium and thorium via very short-lived radioactive intermediates apparently cannot be explained even by the remedial measures taken in Paper I. The build-up of neutrons ultimately does not deliver any satisfactory explanation for the empirical parallelism between binding energy and the frequency of the various sorts of nuclei.

We are therefore probably compelled to do without the broader build-up hypothesis. In fact, no empirical reason speaks against a restriction to the more limited version. It is entirely possible that the element formation occurred before the stellar formation in a state of the cosmos substantially different from the present one. The energy-generating processes necessarily lead to a change in the element distribution over the course of stellar development. Nevertheless, the stars are probably still so young that they have not had enough time yet to change their chemical composition substantially over the course of their lives. The hydrogen reserves of a sun originally composed of pure hydrogen suffice to cover its present emission for roughly 3×10^{11} years. On the other hand, geological and astronomical data do not force us to ascribe to the Sun an age older than about 3×10^9; and if one may interpret the redshift in the spectra of spiral nebulas as a Doppler effect, then extrapolating backward, this explosive type of motion gives a concrete reason to ascribe to the universe a substantially different physical state from now for a point in time lying roughly 3×10^9 years back. Accordingly, the Sun would have transformed just 1 % of its mass by now. It is interesting that renouncing the broader build-up hypothesis leads to a new, independent age determination that agrees well with the mentioned figures. The radioactive elements still present today, if they are not being constantly generated in the stars, would then have to have been formed at a time lying in order of magnitude not further back than around their half-life time. Quantitative estimates[3] have yielded an age for the present element distribution of approximately 5×10^9.

The present paper treats, in the first part, the reasons speaking against the broader build-up hypothesis. The second part of this paper intends to underpin and expand upon the more limited interpretation. Finally, the third part attempts to assemble the conclusions that one might perhaps be able to draw about the state of the universe at the time the elements were formed.

2.1.2 The Generation of Neutrons

It has not been possible up to now to indicate with certainty which specific reactions yield the energy of the stars and therefore we do not know at all yet whether neutrons are produced in considerable amounts in these reactions. All the reaction cycles proposed in Paper I seem to be nuclear physically impossible. We dispense with a more detailed discussion of possible reactions here because we would have to go through them again further below (Sect. 2.2.2) under altered preconditions; and for the benefit of the broader build-up hypothesis, we assume that a neutron-delivering reaction has been found. Under this assumption we can determine a lower limit for the frequency of helium in the star, compared to the total frequency of the heavier elements, which seems to be in disagreement with the empirical frequency of helium.

According to Paper I, α-particles must form simultaneously with each generated neutron. Hence heavy elements cannot be generated without helium simultaneously being formed. The most efficient neutron-producing reaction is a collision between two deuterons, at which under all conditions one nucleus of mass 3 is formed that must somehow be formed into[4] He and furthermore at which one neutron is produced in about half of all cases. Thus, on average, two helium nuclei

[3] See St. Meyer (1939), Wefelmeier (1939).

[4] Mr. Biermann pointed this out to me. Cf. Cowling (1939).

form for one neutron. If the mean atomic weight of the heavier elements formed by the combining of neutrons onto helium is A, then a heavy nucleus contains, on average, one of the produced helium nuclei and $(A - 4)$ neutrons. Consequently, in order for a heavy nucleus to be able to form, $2(A - 4)$ α-particles must form, one of which is built into a heavy nucleus; the proportion of the number of helium atoms to heavier elements thus is $(2(A - 4) - 1)$ to 1. For $A = 10$ that already is 11:1; for the empirically approximately correct value $A = 25$, it is 41:1. The mass ratio is $4(2(A - 4) - 1)$:A, i.e., 6.6:1 for $A = 25$; for $A = 10$ one obtains 4.4:1; for $A \to \infty$, 8:1.

This value is a lower limit, as all the neglected effects tend to make the helium content even higher. They are the following:

1. Neutrons are generated more rarely than assumed above. Any reaction ever drawn into consideration up to now leads to the formation of α-particles, whereas just a few to the formation of deuterons; likewise, a neutron can only form when one deuteron hits a second deuteron, whereas any reaction between a deuteron and a proton leads to the formation of an α-particle without leading to neutron yield. Even though the neutron-producing process is the dominant one in the star, side-reactions will surely always be occurring simultaneously that raise the relative frequency of helium.
2. Neutrons are used more rarely than assumed above for the formation of heavier nuclei. For, the build-up by neutrons from He or perhaps Be does not proceed smoothly; rather there is a certain probability that some intermediary nuclei will intervene in that decay. Thereby a part of the neutrons will always be transformed into helium.

One should therefore not be surprised if the relative frequency of helium lay substantially above the indicated limit. Experience teaches the opposite. From the new book by Unsöld[5] we gather the following figures:

In class B stars, which one could hardly regard as particularly lacking in helium, one obtains the frequency ratio in numbers of atom type:

helium:hydrogen $\sim$ 1:100

with an uncertainty of about factor 4 (p. 301). In solar protuberances (pp. 416, 419) this proportion results at about 1:30, in the extreme case 1:15. On the other hand,

hydrogen:metals $\sim$ 50:1 (p. 136),
and thus one obtains for
helium:metals $\sim$ 1:2–3:1.

[5] See Unsöld (1938).

Even the best ratio 3:1 remains below the theoretical lower limit by a factor 10. Although the empirical data may still be very uncertain (Russell calculated for hydrogen:metals $\sim$ 1000:1), the attempt to bring theory and observation into agreement can scarcely count as promising.

2.1.3 The Formation of Uranium and Thorium

In Paper I, it was shown that uranium and thorium can only be built up by neutrons if during this process a heavy nucleus captures one neutron on average at least every minute because otherwise the unstable intermediates between lead and thorium decay again prior to the further build-up. On the other hand, the rarity of all elements above iron shows that on average a combined nucleus (above helium) does not capture more than about 50 neutrons throughout its entire lifetime inside the star. Consequently, the total span of time during which a nucleus may stay in a region of the star in which it can be built-up further by neutrons is, on average, of the order of magnitude of 1 h. Now, the amount of matter that is just undergoing such kinds of transmutations relates to the total mass of the star as this time span to the time in which the total stellar mass will have undergone the build-up, hence about as 1 h to 10^{11} years or as $1{:}10^{16}$. Thus that fraction of the stellar material in which energy is being generated should fit inside a sphere whose radius is smaller than the 10^5th part of the star's radius, therefore about 10 km for the Sun.

Contrary to the hope expressed in Paper I, such a concentration of the sources of energy in a centrally regular stellar model with temporally stationary generation of energy seems to be impossible. This is because the low core density of the point-source model mentioned in Paper I vanishes when one takes convection into account,[6] and the temperature does not rise rapidly enough against the core to distinguish such a small area, given the relatively weak temperature-dependence of the energy generation (about $e \sim T^{10}$).

One way out would be the assumption that the energy is not generated uniformly at all but instead in the form of always rapidly expiring small explosions. These explosions would just have to be frequent enough not to produce any visible fluxes in the star's radius or luminous intensity. There would then be no well-defined energy-generating region in the interior of the star, just a zone of instability in the proximity of the star's centre whose magnitude would be about the same as the magnitude of the energy-generating region in the normal model. The volume of the actually exploding regions could then be very much smaller.

It is very questionable, though, whether the real reactive mechanism could lead to such a model. In any case, there must be a cause effecting the extinction of explosions of a certain magnitude, because otherwise no stable stars could exist.

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[6] Cf., e.g., Bodenstein (1937) and the presentations and discussions in *Zeitschrift für Elektrochemie*, 42 (1936): 439ff.

One must obviously invent a very special mechanism, in which an explosion is not prevented from developing by this same cause but only starts to work at a certain magnitude. Looking at the model cycle proposed in Paper I, for instance, one would in fact assume that it would either not work at all or proceed completely explosively. Physical chemistry knows of two criteria for the explosive course of a reaction[7]: a strong rise in probability of a reaction with the temperature; and a branching of the chain of reactions, that is, the generation of a product that itself can become the start of a new chain over the course of a normal chain of reactions. Both conditions are satisfied in the model cycle. On the other hand, the effect of the first condition is removed if during the reaction the released energy is too rapidly dissipated for a considerable temperature rise to be able to occur; and for the second condition, if the reaction chains abort. These two constraining factors are also active inside the star. There is available for the transport of energy, apart from radiation and convection, also the transformation of gravitation into potential energy through star expansion occurring at the speed of sound; and the β-decays may not break off the reaction chains definitively, but each time it may be for a span that ought to suffice to restore the equilibrium with the surroundings in the interim. A precise discussion of the problem is certainly very difficult and we abstain from carrying it out here; for the purpose sought here it should suffice to point out that it is not comprehensible why the restraining factors do not act much earlier to prevent the introduction of an explosion if they are able to stop an already begun explosion.

We must therefore conclude that at least uranium and thorium had already formed before the Sun existed in its present state. Because both these elements comply well with the frequency distributions of their more stable neighbours in the periodic system, one would be compelled to drop further elaboration of the build-up hypothesis for all heavy nuclei as well.

2.1.4 The Relationship Between Mass Defect and Frequency

One basic observation necessarily demanding explanation by a theory of element formation is the overwhelming frequency of energetically particularly stable nuclei. It is most clearly apparent in Harkins's rule, which states that nuclei with even numbers of particles, which are undoubtedly energetically more stable than those with odd numbers, also occur more frequently in nature than the latter, almost throughout. An attempt at an explanation of this fact was made in Paper I, within the framework of the hypothesis of neutron build-up. Meanwhile it has been demonstrated that the theoretical foundation of this explanation was not tenable. A correlation between mass defect and frequency extending beyond Harkins's rule

[7] See Landau (1937).

now seems to have been empirically verified, which in any case does not grant the mentioned explanation.

In Paper I, it was assumed that the more energy had been gained by the combining of neutrons whose energies correspond to the star's temperature, the larger was a nucleus's mean activation cross-section. Odd-numbered nuclei would then indeed have greater cross-sections and, consequently, in stationary operation be rarer than the even ones. The cause of this postulated relationship between activation cross-section and combining energy ought to be that the number of resonance levels for neutron capture per energy interval increases very rapidly with the neutrons' combining energy.

In the meantime, Landau[8] has made it very likely that the "reduced partial neutron width," which is proportional to the probability of capture in one level, is, for its part, inversely proportional to the number of levels per energy interval. That way, the effect of high density on a given level would be exactly compensated: The activation cross-section, averaged over many levels, would just be independent of the number of levels per energy interval.

At higher energies, at which the levels become so densely occupied and broad that they are no longer separable, the activation cross-section must in any event become equal to the geometric nuclear cross-section. Setting out from arguments of mathematical simplicity, Landau has now demanded this independence of the mean activation cross-section from the level density also for a region with disjunct levels. This extrapolation can also be supported by the following physical argument. The narrowness of a level signifies long lifetime of the nucleus in the relevant state. The empirically required very long lifetimes are explained, according to Bohr, in that owing to the interplay between the internal motions of an excited nucleus, only rarely does the necessary energy for escape concentrate onto one particle. This temporary energy concentration on a particular particle is evidently a classical concept that can only be defined at all in the limiting case of high quantum numbers, i.e., by the superpositioning of many quantum states. The further apart the states shift from each other, the further removed one is from this limiting case; and in the end, one cannot determine what the energy concentration on one particle in a single very deep state, such as, even the ground state, means anymore; consequently, this reason for the level narrowness falls away. This consideration does not prove Landau's approach, but it does show that at the current state of our knowledge there is no reason to take a different approach. Given the assumption of neutron build-up, Harkins's rule accordingly does not result as a consequence of modern nuclear physics.

Wefelmeier had put forward the general relationship between mass defect and frequency and evaluated it in nuclear theory.[9] It is apparent above all when one compares light even-numbered elements, hence, for instance, C, O, Ne, Mg, Si and

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[8] Landau (1932, 1938), Hund (1936) Anderson: *Veröff. d. Univ.-Sternw. Dorpat*. Cf. on the following: Gamow and Teller (1938a).

[9] See Wefelmeier (1937a, b, 1939).

S. This relationship is certainly not part of the discussed interpretation of Harkins's rule, as that interpretation yields not that a stable nucleus would be particularly prevalent but that a nucleus lighter by one unit is particularly rare; only where there is regular alternation as in the case of the even-odd rule are both the same.

The problem that we encounter here offers not only an argument against the broader build-up hypothesis but simultaneously also an indication of the demands that must be made of a correct theory of element formation. The stability of one kind of nucleus can only exert a direct influence on its frequency when during its formation not only build-up processes occur but also processes in which one building block is split off the nucleus without combining; then the unstable nucleus is most easily decomposed. For this, energies of the order of magnitude of nuclear binding energies are necessary, however. We must therefore look for conditions under which such energies can act on a large scale.

2.2 Second Part: The Mechanism of Energy Generation

2.2.1 Survey of the Known Energy Sources

We shall now treat the generation of energy independently of the question of element formation. It seems advisable to recheck the foundations of the theory against an itemization of the energy sources coming into regard according to the present state of physics. In general, four kinds of energy generation can be considered:

1. Contraction without a change in the star's chemical composition. The energy released is gravitational energy.
2. Element build-up. The energy released is nuclear energy.
3. Contraction during transmutation of part of the matter in densely packed neutrons.[10] The released energy is, again, gravitational energy as well as the zero-point energy surplus of the degenerate electron gas, compared to the energy of the forming neutron gas, as a result of the transmutation; for this, nuclear energy must be expended corresponding to the mass surplus of the neutrons above the formerly present atoms. At high density this energy balance can be favourable.
4. Complete disintegration of matter into radiation. The released energy is the matter's energy at rest.

In the following we shall only take into account the second energy source and must therefore justify why the other three sources have no importance. Pure contraction is eliminated, at least for sun-type stars, owing to their low output. By

[10] Landau (1932, 1938); Hund (1936); O. Anderson: *Veröff. d. Univ.-Sternw. Dorpat*. Cf. for the following: Gamow/Teller (1938).

contrast, the energy sources under 3 and 4, if they can become effective at all, are more productive than the element build-up.

Complete disintegration, for instance, by an electron uniting with a proton, has lost its likelihood in that no cause capable of bringing it about has been found in physics to date. Since the discovery of the neutron and the positron, it seems that in the cancelling out of the positive and negative charges, just the electron mass transforms into radiation energy and the proton mass is retained. The fact that this disintegration into radiation has not been observed in the laboratory until now is also a strong argument against its occurrence in a star. As the interiors of the planets and even smaller bodies do not contain this energy source, the temperature in a stellar interior must surely be an essential factor for its triggering. On the other hand, in the laboratory we can nowadays subject at least individual particles to the action of energies by orders of magnitude higher than those in all probability occurring inside a star and find no radiation-emitting disintegration. If one also takes into consideration that the assumption of radiation-emitting disintegration is not necessary to explain the empirical energy production, it does seem legitimate to drop it altogether.

The possibility of the third energy source mentioned above does follow directly out of modern physics, though. Hence we can only eliminate it if we show that the conditions under which it becomes effective do not occur in the known stars. As a matter of fact, the usual estimates for the interior of a star do yield a density far below the critical density at which this energy source starts to operate. However, we can perhaps make ourselves independent of these estimates as well by a genetic consideration. If a star begins its lifetime as a gaseous ball of low density, as the density increases, first energy source 1 will become accessible to it, then 2, and finally 3. If under contraction the star undergoes a series of equilibrium states, it will not increase its density and temperature more rapidly than the energy that is released at this increase can be radiated. Energy source 2 becomes active at a quite precisely defined temperature, and therefore the star should remain in the vicinity of this temperature (10^7) for a long while. It fits here that empirically temperatures of this order of magnitude have to be assigned to all main-sequence stars. Since this energy source can last for a time span that is about a hundred times longer than the presumed age of the Sun, one should assume that the Sun (and likewise the entire main sequence) is not old enough yet to have attained the density necessary for energy source 3.

The sole possibility to achieve this density more rapidly would accordingly be in a stellar development that does not undergo a series of equilibrium states. It does appear, though, that such a development must always lead to the star exploding. This is because an increase in density is connected to an increase in temperature which leads to an accelerated release of nuclear energy. Thereby the star is at least returned to the state of equilibrium; if the deviation from equilibrium was already too large, it can only explode, either immediately or along the route of ‘overstable’ pulsation. This argument can also be stated this way: At the high density of matter required before a “neutron clump” can form and at the corresponding temperature, the thermodynamic equilibrium of the nuclear reactions must set in promptly (cf. also the third part of this paper). At equilibrium the proportionate mix of elements is defined by the physical conditions. Every star that does not have the right ratio mix at the outset must set it during contraction and if this process is completed in a period shorter than about 10^{11} years, the energy emitted in the process destroys the star's cohesion.

2.2.2 *The Course of Energy-Producing Nuclear Reactions*

Which special nuclear reactions are in fact responsible for the generation of energy could not be decided in Paper I. Advances in nuclear physics since then still do not allow a sure answer. The assumption that the elements had essentially already been formed before the development of the present state of the stars casts this problem in a new light, however. For, now all the known stable nuclei are available as initial substances of chain reactions, and yet, the properties of neutron generation and autocatalysis do not have to be demanded of those chains anymore; hence it is less significant for the basic assertions of the theory which reaction should ultimately prove to be the most important.

On the question regarding which of the hitherto proposed reactions are possible, nuclear physics today provides the following information:

The model cycle of Paper I can only run if the nuclei of mass 5 are able to persist. According to experimental research published in the interim, this does not seem to be the case. They have not been found as stable nuclei and on the basis of nuclear reactions a mass has been attributed to ^{5}He that is larger than the sum of the masses of a α-particle and a neutron.[11]

Most apparently possible ways for a build-up, circumventing mass 5, lead via the nucleus ^{8}Be. But this nucleus seems not to be able to survive either. At least, one would have to assign it a lifetime that with considerable likelihood is too short, compared to α-decay, for another charged particle to be able to add itself on within that period under the prevailing conditions inside the star.[12]

Reaction chains starting out with helium, which lead to the build-up of higher nuclei or to a multiplication of the amount of helium, solely through the addition of hydrogen or of helium itself, accordingly seem not to be possible by two-body collisions; and three-body collisions ought not to play a part in stars of normal density.

One must, however, reckon with the possibility of a direct reaction between two protons. Albeit, according to present-day knowledge about the forces between two protons, the thereby initially formable nucleus. He would not be any more stable.

[11] Williams et al. (1937). As Mr. Gamow has informed me by letter shortly before completion of this paper, according to new findings by Joliot, ^{5}He and ^{5}Li are stable, after all. Then the model cycle would have to be regarded as relevant and only heavy hydrogen must be assigned the energy source of giant stars (cf. Sect. 7).

[12] Cf. Livingston/Bethe (1937).

During the brief period of its existence, this unstable nucleus can emit a positron, though; so the process $H + H = D + e^+$ does take place, overall. This process was proposed as an energy source by Atkinson[13] and has been more recently examined quantitatively by Bethe.[14] Nevertheless, no more can probably be said than that our knowledge about the nuclear physics is not adequate to exclude it as an energy source. The very low a priori probability of β-decay occurring in it is balanced out by the frequency of collisions between two protons inside a star; it is very difficult to find a reliable estimate for the probability of the β-decay, though. The assumption of this energy source would probably present astrophysical difficulties because its temperature dependence is weak, owing to the faint Coulomb field between two protons; within the main sequence the approximate constancy in the core temperatures, despite the very different requirements that stars of different masses set for their energy sources, would be difficult to explain by it.[15]

At this point another proposal by Döpel[16] should be mentioned: that reactions particles are unable to undergo by thermal energy could be triggered by particles accelerated by the electric fields within the star. To this the reply must be that electric fields are surely only maintainable in the outermost atmospheric layers of the stars, because the stellar interior has to be an ideal conductor of electricity due to the great density of free electrons.

If we now assume that all the elements in the star had been there at the outset, then we are no longer limited to reactions beginning with hydrogen or helium. On the basis of laboratory experiments, the following is predictable about the behaviour of the immediately higher elements within a star.

All known stable isotopes of lithium, beryllium and borium are decomposed by proton additions and thus ultimately lead to the formation of helium. However, added to this must be a cycle of reactions setting in on the ^{12}C nucleus, during the course of which helium is also produced but the initial nucleus remains unchanged and hence just has the effect of a catalyst.[17] This is the cycle:

$$^{12}\mathrm{C} + \mathrm{H} = {}^{13}\mathrm{N}; \quad {}^{13}\mathrm{N} = {}^{13}\mathrm{C} + \mathrm{e}^+; \quad {}^{13}\mathrm{C} + \mathrm{H} = {}^{14}\mathrm{N};$$
$$^{14}\mathrm{N} + \mathrm{H} = {}^{15}\mathrm{O}; \quad {}^{15}\mathrm{O} = {}^{15}\mathrm{N} + \mathrm{e}^+; \quad {}^{15}\mathrm{N} + \mathrm{H} = {}^{12}\mathrm{C} + {}^{4}\mathrm{He}.$$

The energy source of the star would accordingly first constitute a decay of the elements below carbon and thereafter the indicated cycle. If owing to secondary reactions the frequency of carbon should also ultimately diminish, an analogous cycle beginning from oxygen is available.

13 Atkinson (1936). Cf. also Döpel (see note 5 [16] below).

14 Pursuant to an oral note by Mr. Gamow.

15 Mr. Biermann pointed this out to me.

16 See Döpel (1937).

17 Mr. Gamow informed me that Bethe has recently examined this cycle quantitatively.

2.2.3 Consequences for the History of the Formation of Stars

One can estimate the alteration to the element distribution in the Sun that has taken place by the proposed processes up to now. In order to have simple figures, let us assume the Sun were originally composed of equal mass proportions of hydrogen and heavy elements and the latter were evenly distributed over all the atomic weights from 1 to 50, so that for 25 hydrogen atoms there is one atom that is more massive. In its lifetime up to now the Sun has transmuted about 1 % of its mass from hydrogen into higher elements, therefore, it has lost a 50th part of its hydrogen. On average, two hydrogen atoms react with the same heavy nucleus or, resp., its daughter products before it has transformed completely into helium (e.g., for ^{6}Li, one needs 2 protons; for ^{7}Li, 1; for ^{9}Be, 3, etc.); thus a fourth part of all heavy nuclei should be affected by the transformation if each heavy nucleus reacts just once (hence disregarding cyclic reactions). The lightest nuclei are affected first, and consequently over the elapsed history of the Sun's development the element distribution should be decomposed just about up to carbon. At carbon, at the onset of the cycle, decomposition stops anyway. It is known that Li, Be and B are particularly rare in the Sun,[18] not only compared to other elements but apparently also compared to the Earth,[19] in which element transmutation ceased a very long time ago, of course.

The frequency in helium occurrence now also results in satisfactory agreement with experience. About as many helium nuclei should form as protons disappear. Thus results a ratio in numbers of atoms of about H:He = 50:1.

These considerations can be translated onto the other stars of the main sequence, with appropriate modifications. A problem is posed by giant stars, however, which combine high luminosity with a core temperature that is about ten times lower than in the main sequence. It is certain that the energy sources for nuclear reactions in the giant branch and the main sequence cannot be the same. Yet, given the assumption that giant stars still drew their energy from pure contraction, it is known that such a short time span is available that the pulsation periods of the Cepheids ought to have already changed as a consequence of the contraction within historical times—in contradiction to a series of observations.

One must thus surely seek two different nuclear reactions for giant stars and for main sequence stars. The above considerations offer two different possibilities. As Bethe has noted,[20] one can assume that giants are still decomposing Li, Be and B, whereas the main sequence is already engaged in the carbon cycle. One can also assume that large amounts of heavy hydrogen isotopes are still present in giant stars, and they are yielding energy whereas the decomposition of lithium and its neighbours is either limited to a rapidly progressing intermediary state or else

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18 The significance of the rarity of these elements was probably first emphasized by Goldschmidt (1926)

19 At least Li and Be. Cf. Goldschmidt (1938). Mr. Wefelmeier pointed out this matter to me.

20 Pursuant to an oral note by Mr. Gamow.

already belongs to a stellar type that is in very close proximity to the main sequence. As the energy gain is essentially a function of Z^2/T, the first assumption would reduce the required core temperature of giant stars, compared to the main sequence, by about factor 4, the second by about a factor that is in any case greater than 10. The first assumption may seem less forced; the second perhaps lends clearer expression to the characteristically wide separation between giant and dwarf stars. Which of the two is right depends on the as yet unknown original frequency distribution of the lightest elements. Independent of this individual issue, the build-up hypothesis suggests, in any event, that one must assign to the main sequence a stationary reaction cycle and to giant stars the decomposition of nuclei lighter than the nuclei participating in the cycle; they therefore already have to have been converted before the temperature could rise high enough to stimulate the cycle.

This assumption has a few consequences that can perhaps be tested empirically. According to it, giant stars must be very young. Based on the above estimates, the decomposition within the Sun of a single sort of nucleus of average frequency lasts around 10^8 years. Since giants possess a higher specific luminosity than the Sun, for them this period is lowered to about 10^7 years or less. That star clusters do not simultaneously contain B-stars and yellow or red giants points in the direction that a uniform age (within certain limits) must be assigned to giant stars. But perhaps more precise tests exist. Furthermore, giants would have to contain light elements that are already very rare in the main sequence, in particular, either lithium and the next-heavier elements or heavy hydrogen (the latter obviously in an amount perhaps no longer spectroscopically detectible, besides light hydrogen).

Against the assumption that giants also had normal nuclear reactions as one source of energy, Gamow[21] has raised the objection that they would then have to be ordered in a line approximately parallel to the main sequence, whereas in reality the giant branch is positioned about perpendicular to the main sequence. One has to note, though, that the giant branch is very diffuse and in reality fills an extended level range above the main sequence. According to our assumption, the energy source of the main sequence is temporally roughly constant, whereas that of giant stars is spent over time. Accordingly, a giant's luminosity can change over the course of its development, such that giants observed today as differing in mass and age should, in fact, fill one level in the Russell diagram. The line of the largest number of stars in the diagram, normally called a giant branch, then does not need to be a line of constant age along which the stars would be distributed according to mass; they could, conversely, depict the developmental path of the mass occurring most frequently among giants. Perhaps, owing to the mainly convective construction of giant stars, another mass-luminosity relation is valid than for dwarfs.

Gamow[22] has pointed out another problem for the entire build-up hypothesis. He has shown that the mass-luminosity relation of a star depends on its hydrogen

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21 Gamow (1938a).

22 See Gamow (1938b).

content and if that changes during stellar development, the empirical continuance of a universal mass-luminosity relation[23] (at least for the main sequence) is not comprehensible. The assumption of a resonance in the energy-generating process introduced by Gamow himself to remove this difficulty has a low a priori probability and seems, in addition, only to reduce the problem but not remove it.[24] Perhaps, however, the fact recently put forward by Gamow,[25] that the strong deviation from the normal relation is limited to a short part of the star's lifetime, suffices for that. The whole problem disappears, though, if one abandons the broader build-up hypothesis and assumes that during the lifetimes of the stars to date, their original chemical compositions have not changed essentially at all. This remark suffices for stars with luminosities smaller per gram than ten to a hundred times the Sun's luminosity. Accordingly, the cosmos would still be so young that stellar evolution had not taken place yet, apart from going through the giant stage. Just the existence of white dwarfs poses a problem for this interpretation; almost the only explanation that the build-up hypothesis can offer for their low luminosity is the assumption that their entire hydrogen content has already been depleted.

These considerations on the historical development are still at a speculative stage. But the concepts they use stem throughout from a part of physics whose foundations today can already be regarded as elucidated. One may therefore hope that some experimental advances and a closer collaboration between astrophysics and nuclear physics will permit a well-rounded theory to be completed in the near future.

2.3 Third Part: The Formation of the Elements

2.3.1 The Necessary Physical Conditions

If we do not want to abstain entirely from understanding the development of the elements, after having given up the broader build-up hypothesis, we must draw out of the frequency distribution of the elements conclusions about the former state of the cosmos in which this distribution could arise. It is self-evident that such conclusions rest on a very much more uncertain basis than the theory of energy production, as they, unlike the latter, presume not only the spatial permanence but also the temporal permanence of our natural laws. That the laws of nature retain their form in the transition from the terrestrial laboratory to planetary space and from there into galactic space has been confirmed by experience at least for a series of individual cases. How far we can describe an essentially different earlier state of the cosmos by the laws of nature as we know it is, a priori, wholly

[23] The German original reads 'Reaktion', apparently a typo. (*ed.MD*).

[24] See Gamow/Teller (1938b).

[25] See footnote 20.

uncertain; and the essence of time dictates that as soon as the past is no longer accessible to our memory, we cannot possess any direct experiences, but must instead rely on conclusions drawn from documents. There is no other choice than initially to presuppose hypothetically the temporal permanence of our natural laws and to be prepared that an error may be revealed upon comparison of this theory against documents from the past still available today.

The relationship between mass defect and frequency of occurrence compels one to assume that while the elements were forming kinetic energies were available to the coreactants of the order of magnitude of nuclear binding energies. At such high energies thermodynamic equilibrium must set in very rapidly in the nuclear reactions. For, first, Coulomb repulsion no longer plays a part anymore then, at least for lighter nuclei; and second, the transmutation of free protons into neutrons then becomes a very frequent process, so an arbitrary amount of neutrons is available for the combining and decomposition of heavy nuclei.

The first impression that the frequency distribution of the elements leaves contradicts this assumption, of course. It should be noted, however, that nuclear processes were still taking place in the cosmos even after the basic features of the element distribution had come about. The theory has thus already achieved what one can expect of it, if it shows that the discrepancy with the present-day distribution can be explained by equilibrium in nuclear processes that we must anyway require as having occurred for physical or astronomical reasons also after the first act of formation. Thus the abnormal rarity of the lightest elements is a consequence of energy-generating reactions in present-day stars. We shall discuss another discrepancy at the end of Sect. 2.3.2.

Hence, if one sets the original distribution at thermal equilibrium, one can attempt to calculate out of the empirical frequency of the elements what temperature and what density must have dominated at the time of their formation. The frequency relation between two neighbouring nuclei must be set according to the Saha equation.[26] We shall look at a neutron being taken up. Let n_A denote the number of nuclei of atomic weight A per cm^3, hence specifically n_1, the number of neutrons, and E_A the energy that must be expended in order for one neutron to be torn away from the nucleus of mass A, then

$$\frac{n_{A-1} \cdot n_1}{n_A} = g_A \cdot e^{-\frac{E_A}{kT}} \tag{2.1}$$

holds, with

$$g_A = \frac{G_{A-1}}{G_A} \cdot \frac{2(2\pi MkT)^{3/2}}{h^3} \tag{2.2}$$

[26] This attempt has been undertaken often already. The following publications are known to me: Farkas/Harteck (1931), Sterne (1933), Guggenheimer (1934).

G_A is the statistical weight of the nucleus of mass A, hence a number of order of magnitude 1. The second factor in (2.2) contains the statistical weight of the free neutrons; it has the dimension of a reciprocal volume, namely, it signifies the number of neutrons per unit volume for which phase cells are available at the given temperature.

One can now apply Eq. (2.1) to two neutrons being successively taken up. One obtains from this the following equations for the temperature and neutron density:

$$kT = \frac{E_A - E_{A-1}}{\ln \frac{n_{A-2} n_A}{n_{A-1}^2} \cdot \frac{G_{A-1}^2}{G_{A-2} G_A}} \tag{2.3}$$

and

$$\frac{n_1}{g_A} = \frac{n_A}{n_{A-1}} e^{-\frac{E_A}{kT}}. \tag{2.4}$$

We apply these formulas to the reactions

$$\left. \begin{aligned} {}^{16}\mathrm{O} + {}^{1}n &= {}^{17}\mathrm{O} \\ {}^{17}\mathrm{O} + {}^{1}n &= {}^{18}\mathrm{O} \end{aligned} \right\}. \tag{2.5}$$

If one sets $n_{16} = 10^4$, then empirically, $n_{17} = 4$ and $n_{18} = 20$. Furthermore, $E_{17} = 4.5$ *TME* and $E_{18} = 9.8$ *TME*. G_{16} and G_{18} may be set equal to one. The spin of ^{17}O is unknown; if it is set equal to ½, then follows $G_{17} = 2$. It follows that

$$kT = 0.44\, TME = 0.41 \text{ MeV}$$

or

$$T = 4.7 \times 10^9 \text{degrees}.$$

Furthermore $n_1/g_{18} = 1.2 \times 10^{-9}$.

With the just calculated temperature,

$$1/g_{18} = \left(5 \times 10^{-12}\text{cm}\right)^3,$$

hence $n_1/g_{18} = 1$ would be a neutron density already comparable to the density inside the atomic nucleus. The real figure yields a mean distance between the two neutrons that is about a thousand times larger. There results $n_1 = 10^{25}$, that is, about tenfold the density of water just for the neutrons.

These figures are still very imprecise. Above all, the density determination can still be wrong by a number of powers of ten because of the exponential dependence on the energy. Yet given the inaccuracy of the indicated mass defects and occurrence frequencies, a factor of order of magnitude 2 must also be left open in determining the temperature. The next task of theory would therefore be initially to check many other reactions by an analogous consideration for whether somewhat uniform values for temperature and density are generally determinable out of the entire frequency distribution of the elements and, if this hope is confirmed, to define the numerical values as precisely as possible. Perhaps by this route one can come far enough along ultimately to use the empirical frequencies of the individual nuclei directly as quantitative information about their mass defects.

Unfortunately, we possess accurate knowledge about mass defects at the same time as their frequencies required for this examination only for the nuclei below oxygen, whose frequency is completely changed by secondary processes. A couple of data on heavier nuclei yield figures of the same order of magnitude as those calculated here. We shall abstain from continuing on toward a quantitative analysis in this paper and shall only regard more exactly a qualitative feature of the empirical distribution.

2.3.2 The Conditions for the Formation of Every Kind of Nucleus in Comparable Amounts

In the bigger picture, the empirical frequency distribution of nuclei exhibits conspicuous uniformity, despite strong individual fluctuations. It often happens that a nucleus occurs 10^3 times more frequently than its neighbour; nonetheless, the frequency of any kind of nucleus, from oxygen to lead, varies only by about factor 10^6. If starting out from oxygen one wants to calculate the frequency of lead, formula (1) must be applied almost 200 times (where the build-ups applied are partly neutrons, partly protons); in order for the resulting relation to become $n_{\mathrm{O}}/n_{\mathrm{Pb}} \sim 10^6$, the factor $g_A e^{EA/kt}/n_1$, which indicates the relevant frequency ratio between two successive nuclei, must have a mean value that is almost exactly equal to one, despite the very large individual fluctuations. If we call the mean value f, then $f^{200} \sim 10^6$ must be valid and hence $f = 10^{0.03} = 1.07$. Such a noticeable relation between temperature and neutron density cannot be chance; we must rather expect the theory to explain this matter physically. In fact, this relation proves to be an almost direct consequence of the saturation of the nuclear forces.

Let us have the density of the matter vary at constant temperature. At low density, f is large against one (the statistical weight of the free protons is so great that its influence outweighs the energetic preference for the bound state); that is why the lightest nuclei are present practically on their own. At increasing density the balance shifts in favour of composite nuclei. If, however, the binding energy is proportional to the number of particles, that is, if in the mean E_A is independent of A, this shift never leads to favouring one sort of nucleus over the next lighter one but instead, in the limiting case of very large density, yields a distribution in which nuclei of every size are exactly equally frequent (hence $f = 1$). The only precondition for this is that the density stay small against the density of the matter in the interior of the nuclei, i.e., that the formation of separated nuclei is generally still possible.

We ground this assertion on a simplified model. We first just consider the combining of a single sort of particle of concentration n_1 and mass M; that is, we disregard the difference between neutrons and protons. This is unproblematic because neutrons and protons become practically equally frequent particularly at high temperature and density. We assume furthermore exact saturation of the nuclear forces and thereby substitute E_A by the constant E. We continue to neglect for now the upper end of the periodic system (which is, of course, determined only by the deviations from saturation in the heaviest nuclei because of the Coulomb force), thus we assume that A could run from one to infinity. Finally, we set all weighting factors $G_A = 1$; g_A then takes on a constant value g. (2.1) then becomes

$$\frac{n_{A-1}}{n_A} = \frac{g}{n_1} e^{-\frac{E}{kT}} = f; \tag{2.6}$$

f is independent of A, and there results

$$n_A = n_1 f^{-(A-1)}. \tag{2.7}$$

If the total mass density ρ of the matter is defined, then n_1 (and hence f) is determined out of (2.6) and the additional condition:

$$\rho = \Sigma_A n_A \cdot AM = n_1 M f \Sigma_A A f^{-A} = \frac{n_1 M}{(1 - 1/f)^2}. \tag{2.8}$$

One notes that the density becomes infinite even for $f = 1$ as there are then infinitely many sorts of nuclei with the same occurrence frequency. $f < 1$ therefore cannot occur at all.

This result can also be expressed physically like this: We examine the condensation of a neutron gas. So that $n_{A-1} = n_A$, that is, in order for "drops" of every size to form with the same probability,

$$n_1 = g e^{-\frac{E}{kT}} \tag{2.9}$$

must be valid. This is precisely the vapour-pressure equation of the neutron liquid, which indicates the concentration of a neutron gas at equilibrium with its condensate. This condensate, however, which by its presence provides for the validity of Eq. (2.9), is the forming nuclei themselves.

The question now is whether the presumptions made apply to the real nuclei. Exact saturation is the only doubtful assumption. The finiteness of the periodic system presents no difficulty as, although the elements above lead are radioactive, they are viable and their frequency diminishes according to (2.7) exponentially with the mass; if they decay subsequently, the frequency of lead, compared against its neighbouring elements, is merely raised by factor 10–100, which fits well with experience.

The temperature must be high enough to cancel out the empirical deviation of the binding energies from saturation (that is, not the fluctuations but the systematic course). The outcome is a substantially higher value than in the foregoing paragraphs. If the combining energy of a neutron varies over the 200 mass numbers from oxygen to lead by ε, and if this variation is set linearly, then even for very great ρ, oxygen must occur more frequently than lead by the factor $e^{100 \cdot \varepsilon kT}$. With $e = 3$ MeV and $n_{\mathrm{O}}/n_{\mathrm{Pb}} = 10^6$, there results

$$\mathrm{kT} = 20\ \mathrm{MeV}; \quad T = 2.3 \times 10^{11}\,\mathrm{degrees}.$$

This very rough estimate yields, in any event, that a temperature must be assumed of an order of magnitude as would arise from a complete conversion of the nuclear binding energy into heat. The associated density is likewise already in the neighbourhood of the density inside the nucleus.

It is not surprising that the comparison between immediately neighbouring nuclei yielded a very much lower temperature when one considers the strong fluctuations in the frequency distribution on the small scale. In the physics, it must be taken into account that the temperature must have steadily dropped after the formation of the elements, even if perhaps very rapidly. Nuclear reactions must have still been going on in the process. Above all, if the density was diminishing simultaneously and thereby the lighter nuclei were then being favoured in the equilibrium distribution, it is possible that the energy of the gas simply did not suffice anymore to produce the distribution corresponding to the new temperature, but instead just to allow a couple of more reactions to run their course in the vicinity around each nucleus and thereby conform the fine structure of the distribution to the lower temperatures. Perhaps one ought to be able to read off from the present distribution on the large scale the highest temperature attained, and from the distribution on the small scale the lowest temperature attained at which reactions were still occurring. The latter temperature could be, according to its value calculated above, about the temperature at which the formation of neutrons out of free protons and electrons stops.

How can these notions be tested further, now? On the practical side, above all, more precise knowledge about mass defects of many series of isotopes in the manner of the oxygen isotope applied above would be desirable, which would make possible further quantitative applications of Eq. (2.1). From theory one can ask for verifiable postulates about when and where in the history of the cosmos the required temperatures and densities could have been realized.

2.3.3 *Materializing the Conditions in the History of the Cosmos*

Inasmuch as we know the present state of the cosmos, it does not contain any areas at the temperature required. Neither can we imagine an earlier state of the cosmos out of which the stars could have steadily emerged, that is, by going through a sequence of equilibrium states and during which such temperatures would have prevailed. For, according to Sect. 5, a star can only attain very high temperature without subsequently exploding if it had already practically depleted its hydrogen. So, even if heavy elements should be forming somewhere in the cosmos along this steady path of development, this at least cannot be in the presently existing stars, as these stars still contain their hydrogen (the hydrogen content that is today being attributed to the stars lies far above the equilibrium amount corresponding to the distribution of heavy elements and can only be interpreted as an admixture to the heavy elements by matter that had not participated in the thermal equilibrium). We must therefore look for a possible state of matter prior to the formation of the present stars.

This state must have been a stellar-type cluster of matter, at any rate, as we could not understand the high density required without the participation of gravitation. Now, all empirically known star types are stable and hence unsuitable for attaining those high temperatures; we must therefore look for an empirically unknown but possible type of star. It suggests itself to think of those stars with a mass lying above the empirical upper limit of stellar masses. In Paper I, it was supposed that these stars would be unstable against pulsations of augmenting amplitude. The details of the arguments given there were probably wrong because they postulated a too low real stability for the stars, even without any delay in the generation of energy.[27] Nevertheless, the assumption that these stars are unstable is legitimate because, on one hand, they should otherwise be empirically locatable at least in a few exemplars; and on the other hand, the slower a star reacts to changes in the state of its interior, the more defenceless it becomes against the nuclear reactions taking an explosive course, irrespective of how they specifically occur; and as the connection between pulsation period and luminosity already shows, at any event, the sluggishness of a star to react increases with growing mass.

One can therefore assume major original conglomerations of matter that perhaps were composed of pure hydrogen. By contracting under the influence of gravitation and thereby raising their central temperature, they finally reached a state at which nuclear reactions occurred in their interiors. If their mass was small enough, they could remain stable as stars; if the mass was too large, the nuclear reactions proceeded explosively and blasted the star apart which then either got lost in space as a diffuse cloud or clustered together into new smaller stars. With

[27] Cf. Cowling (footnote 4).

these the same game repeated itself until only stable stars were left behind. These explosions, if they proceed completely at a certain volume, can attain temperatures of the order of magnitude required above, because then the total energy contents of the nuclei are temporarily converted into heat.

How large, now, may one imagine the first conglomeration to have been? Theory does not set any upper limit on its mass, and our imagination has the liberty to conceive not just the Milky Way but the whole cosmos known to us as united within it. One can even draw an empirical fact into the field in support of this speculation. The energy released by nuclear reactions is about 1 % of the energy of matter at rest and conveys to the nuclei on average a velocity of order of magnitude of a tenth of the speed of light. A star's debris should have flown apart at about that velocity. In answer to the question where such velocities of this scale are still being observed, they are found only in the escaping motions of spiral nebulae. That is why one should at least reckon with the possibility that this motion has its origin in a starting catastrophe of the kind considered. Milne[28] pointed out a few years ago that any 'gas' escaping into empty space from spiral nebulae must exhibit the Hubble relation between distance and radial velocity as soon as its expansion has become large against its original volume. Our proposal fits into this picture. However, it distinguishes itself from the present theory by Milne in that it indicates a concrete cause for the expansion and instead dispenses with an infinite and uniform distribution of spiral nebulae. From the standpoint of cosmological speculation, Milne's theory or one of the older ones about the "expanding universe" may be more elegant; it does seem to us, though, rather an advantage of this new proposal that it makes dispensable the difficult-to-prove assumption that the part of the universe known to us had the same structural quality as the whole.

An empirical test of this proposal would be possible if a drop in the frequency of spiral nebulae, which ought to start above a certain critical radial velocity, lay within the observational range of modern telescopes. The rough estimate for this critical velocity, of about a tenth of the speed of light, is surely wrong by a factor in order of magnitude 2. To be able to obtain a more accurate prediction, one would have to be able to describe the explosion process quantitatively.

2.4 Conclusion

2.4.1 First Part

1. The assumption that all known chemical elements were formed and are still forming in the presently existing stars is abandoned; for the following reasons:
2. The heavier elements, according to Paper I, would have to be built up by neutrons whose generation is necessarily connected with the formation of

[28] Milne (1933).

helium. A quantitative analysis of this mechanism leads to the setting of a lower limit in the frequency of helium occurring in a star that is incompatible with experience.
3. Uranium and thorium must be built up by rapidly decaying intermediate nuclei. Contrary to the assumption in Paper I, the spatial concentration of the energy sources does not suffice to give the build-up process the necessary velocity.
4. Nuclear physics cannot justify the explanation for Harkins's rule offered in Paper I. The general relationship between mass defect and distribution frequency compels the assumption of element formation at a temperature permitting equilibrium to set in between decomposition and build-up processes.

2.4.2 Second Part

5. The generation of energy by stars is probably uniquely based on the reactions of lighter nuclei.
6. Which reactions are the most important cannot be decided yet. If composed elements don't just form in present-day stars, no autocatalytic reaction cycle is called for. The model cycle of Paper I is probably nuclear physically impossible. A cycle in which carbon acts as a catalyst in the formation of helium is the most likely.
7. The frequency distribution of the lightest elements following out of these cycles is in conformity with experience. One should probably assume two different types of reactions for giant and dwarf stars, namely, a stable cycle for the dwarfs, and the decomposition of nuclei lighter than those participating in the cycle for giant stars.

2.4.3 Third Part

8. It is assumed that the elements were formed in nuclear reactions at thermodynamic equilibrium. For some elements above oxygen, precise knowledge about the mass defects of many successive isotopes would be necessary for exact verification of the posited formulas. A confirmation of these formulas would legitimate drawing quantitative conclusions about mass defects from the frequency of types of nuclei.
9. From the—in the mean—smooth frequency distribution of the elements, an initial formation temperature of around 2×10^{11} degrees follows that just corresponds to the energy released on the whole by nuclear reactions. The fine structure of the distribution seems to have been ultimately defined by a lower temperature of about 5×10^9.

10. In a ‘star’ of very large mass, the required temperatures could probably temporarily arise but would lead to its explosion. The connection between this notion and the escape motion of spiral nebulae is discussed.

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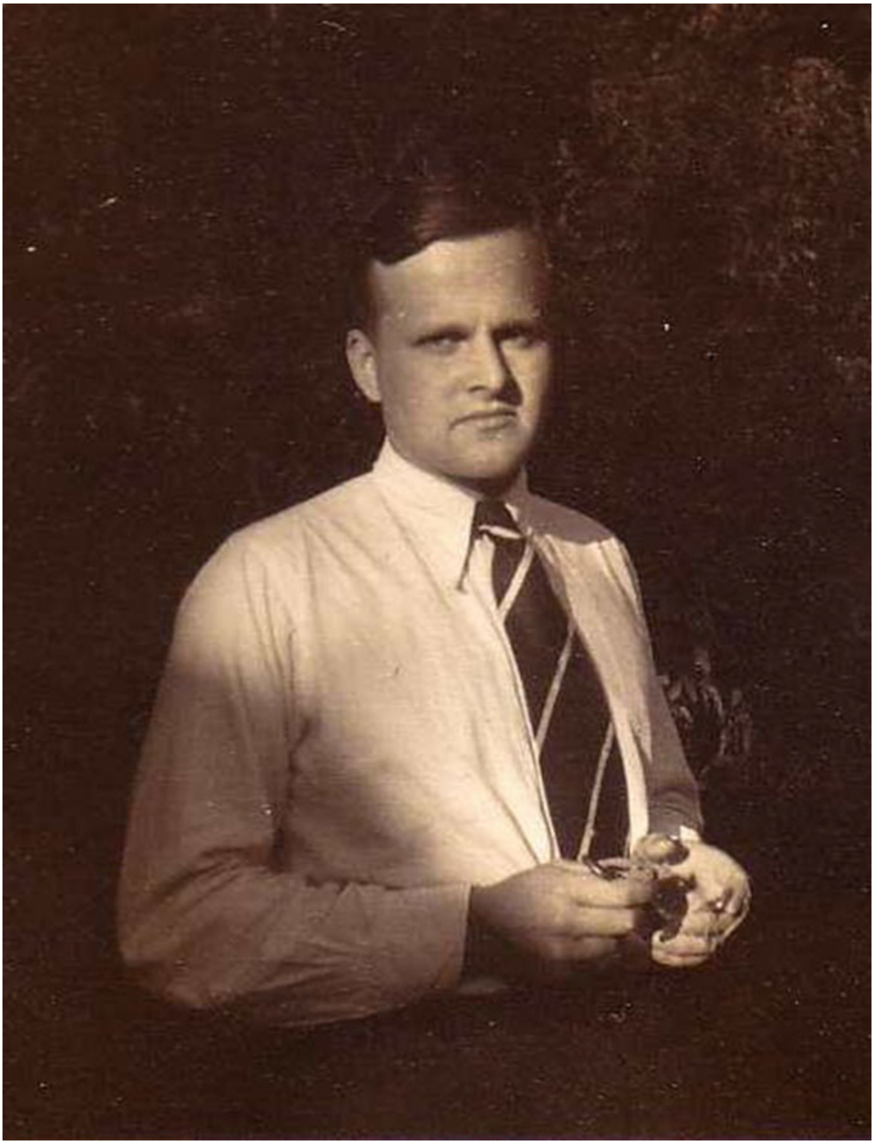

Carl Friedrich von Weizsäcker at the age of 24 in 1936. © The Weizsäcker Family represented by Dr. Elisabeth Raiser who granted permission to use this photo

Chapter 3
The Formation of the Planetary System

3.1 The Structure of the Planetary System

Let us take a look at the western sky on a clear night this month of March![1] When the Sun has set, the constellations familiar to us since childhood then appear: the great hunter Orion; the Gemini, Castor and Pollux; and the charioteer's pentagon. But between these eternal figures we find some stars that aren't always positioned in that region of the sky. Above Orion we see two of these visitors: red Mars and paler Saturn. Deep down on the horizon, at the end of this month Mercury will perhaps still become visible for a short time amongst the beams of the setting Sun. High up in the south, though, Jupiter shines forth with steady brilliance as the brightest star of the sky, the star of kings and philosophers.

These stars are visitors; day by day they change their locations in the sky. That is why they are called planets, i.e., wandering stars. Just now Mars is wandering past Saturn and is beginning to approach Jupiter; Mercury is emerging out of the Sun's rays and disappears again amongst them. Often we can see none in the sky or just one, sometimes two of them close together, like now, Mars and Saturn, or 3 years ago, Jupiter and Saturn—that constellation thought to be the star of Bethlehem. But their motions are not entirely irregular. Above all, they always move along a definite large circle in the sky. It is called the ecliptic or zodiac and is characterized by the famous 12 constellations with animals ranging from the ram, lion and scorpion to the fishes. Sometimes, when the planets are evenly distributed across the sky, such as during the winter of 1939–1940, they allow us to see the zodiac directly, so to speak. It is especially beautiful when the moon, which also wanders about along the zodiac, then gradually passes by them over the course of a sequence of clear nights, like a great ship gliding past a row of beacons along a distant coastline. The Sun also wanders along the zodiac. If you extend the line that now connects Jupiter with Mars and Saturn down to the horizon and

[1] Talk first given in Munich in winter 1943–1944. This text is for the first time available in English and was translated into English by Ms. Ann Hentschel with the financial support the Udo-Keller-Foundation.

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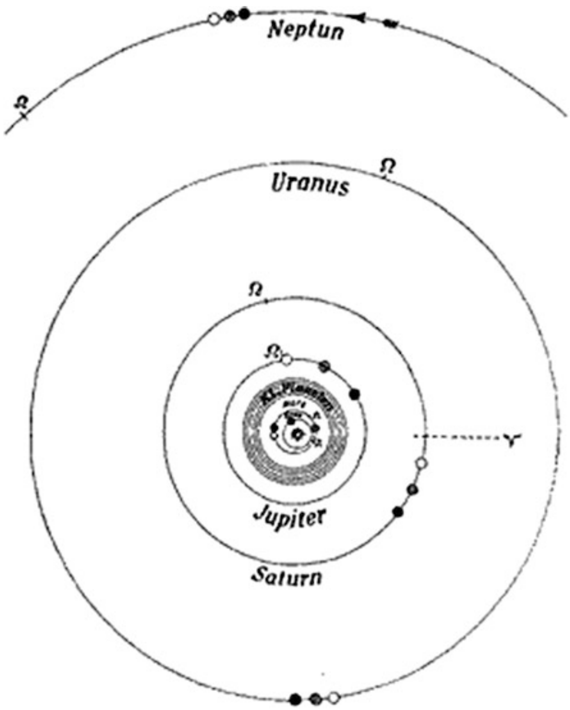


Fig. 3.1 The planetary system

beyond, it goes through the spot at which the Sun is positioned, obscured from our view by Earth. We only do not observe the Sun meeting a planet along the zodiac because the planet then is in the sky synchronously with the Sun, hence at daytime, and thus remains invisible to us in sunlight.

The laws of planetary motion are surely the oldest scientific study of humanity. Antiquity acknowledged that the planets form a great congruous system in space nearer to us than the seemingly immobile mass of fixed stars.

The ecliptic is actually a plane in which all the planets describe their orbits. As Earth itself lies on this plane, we do not see this surface either from above or below, but only as a narrow strip circling around us: that is, as the zodiac. The planets are in this plane at various distances from us. The moon is closest to us, Saturn the farthest among those visible to the naked eye. The prevailing astronomical doctrine since antiquity assumed that Earth itself was the centre of the planetary orbits. Modernity in science begins with the discovery by *Copernicus* that the Sun is the midpoint of the system.

A picture will illustrate how we conceive the planetary system since *Copernicus* (see Fig. 3.1). The Sun, which is not a planet anymore but the centre at rest, is surrounded by the almost circular orbits of the planets. The two innermost ones, Mercury and Venus, are not depicted. Then comes Earth, which this time is another planet. Just the moon, not shown in this picture, orbits directly around it. Further outwards follow Mars, Jupiter, Saturn and the planets still unknown to

Copernicus, Uranus and Neptune. Today the outermost one to be added would be small Pluto. Finally, you see between Mars and Jupiter a region occupied by the orbits of many small bodies, so-called planetoids.

This is an initial rough overview. We follow the historical developments by acquainting ourselves with the system's regularities. *Copernicus* deemed the orbits of the planets to be circles. He was thus adhering to the old belief that the celestial bodies had to move in circles merely because the circle is the most perfect curve. In order to stay in agreement with observation, he had to assume, of course, that the Sun was not the common centre point of these circles but that each circle had its own centre somewhere next to the Sun. In our picture we see, for instance, that the orbits of Earth and Mars do not lie concentrically.

This slight deviation from a strictly hierarchical structure could only be a thorn in the side for a mind believing in the perfection of the celestial motions. *Kepler* was one such mind. He was convinced that the Sun had to be positioned not just somewhere near the centre of the planetary orbit but at a point mathematically distinguished by the planetary orbit's shape. A new notion encouraged this conviction. He held the Sun to be not merely an abstract centre of the system but the seat of the physical force guiding the planets along their orbits. It is clear that an orbit must then have a shape describing the seat of the force as a singular point. However, this notion means doing away with the circular orbits sanctioned by a 2000-year tradition. A circle has no other distinguished point besides its centre. Through careful analysis of the observations by his predecessor *Tycho Brahe, Kepler* showed that the planets travel along ellipses and that the Sun is at one focus of these ellipses. These ellipses, however, have only slight eccentricity and thus approximate the circular shape.

Kepler set up three famous laws of planetary motion. I just mentioned the first, which indicates the shape of the orbits. The second is less important within our context. The third compares the orbits of the different planets and teaches that the squares of the orbital periods of two planets around the Sun relate to the cubes of their major orbital axes. Only one simple consequence of this law will concern us: A planet moves more slowly along its orbit around the Sun the farther away it is from it.

It makes sense that of two planets moving at the same velocity, the one farther away from the Sun will take longer to orbit around the Sun. *Kepler*'s third law states that there is more to this difference in orbital period: The outer planet moves more slowly objectively as well; it traverses fewer kilometres per second than one orbiting closer inside.

Kepler posited these laws empirically; *Newton* explained them mechanically. As *Kepler* had already suspected, the Sun is the seat of a force. It is the same force as Earth's gravity pulling a falling stone toward the ground. Why don't the planets fall into the Sun then, too? Because according to the law of inertia found by *Galileo*, they have the tendency to continue in a straight line along their orbits. Thus the true orbit is, so to speak, a compromise solution: If the law of inertia were to act alone, the planet would hurry on in a straight line and remove itself ever further from the Sun; if the gravitational force were to act alone, it would fall straight into the Sun. In reality, its orbit bends constantly toward the Sun and yet never reaches the Sun.

Along an elliptical orbit it repeatedly approaches the Sun in order to repeatedly distance itself from it. Along a circular orbit, gravity and inertia keep each other exactly at equilibrium. Thus we understand *Kepler*'s third law. Gravity is weaker the farther away the planet is from the Sun. Hence only a correspondingly weaker inertial effect can maintain the equilibrium: In order to stay in orbit, the planet has to travel more slowly the farther away it is from the Sun.

Did *Newton* explain the planetary system fully? He explained its present behaviour but not its origin and stability. Given the location and velocity of a planet at one instant, one can calculate its further course. But how did it arrive at that location? Where did it get that velocity from? It has been following its orbit since time immemorial; but didn't some cause put it onto that orbit? And will it always stay on that orbit, or won't the mutual attractions between the planets eventually bring growing confusion to the system? *Newton* posed both these questions; but he could not answer them. Whereas his explanation of *Kepler*'s laws allowed him to interpret the present motions of the planets as a consequence of mechanical necessity, he believed he was able to perceive in the mechanically inexplicable genesis and stability of the system traces of direct intervention by God in the universe.

The stability of the system was explained purely mechanically in the 18th century by *Laplace*. I do not want to go into this mathematically intricate question here. Independent attempts at a mechanical explanation of the genesis of the system were made in that same century by *Kant* and *Laplace*. The so-called *Kant-Laplace* theory, if not considered confirmed, was for a long time considered probable. Around 1900 one began to realize that it had very great faults. New theories appeared, which were just as unsatisfactory, however. Today I would like to report to you about a new attempt to solve this problem that I have undertaken. It embarks from *Kant's* version of the old theory but takes into account findings that have been made since.

Allow me, before I introduce you to the details of this question, to make a brief basic observation. We want to explain the genesis of the planets—but that means also the genesis of Earth which bears us humans. Can we hope, may we dare to thus unveil the preconditions of our own existence? *Newton* thought he saw the direct work of God. Is man granted this glimpse at the secret of creation? And if he may do so, is mechanics the right tool to express what we should have seen there?

Not every age had inquired into the mechanism of planet formation. It was still far from *Copernicus* to pose that question. To him, the planetary system issue was a problem of structure not a problem of mechanical causality. If we may use the old Aristotelian distinctions, we can say: He does not ask about the *causa efficiens*, the acting cause of the system, rather just about its *causa formalis*, the principle of its form. To the question: Why do planets move along those particular orbits? He would answer: Because those orbits have a perfect shape. This perfection is not explained further. We experience it directly; and if it refers back to anything else, then not to a mechanical cause but to God as the principle of all perfection.

The physical *causa efficiens* enters the scene with *Kepler's* thought that the Sun is the seat of a motive force. Yet *Kepler* does not give up the older thinking, according to which form is the guiding principle. Indeed, he gives it the polish of development evinced by the contention with the opposite principle. By recognizing the perfect architecture of the planetary system, he reiterates the thought of divine Creation. Even physical effects obey mathematical—hence, intellectual—laws; and these laws also, in their own characteristic way, mirror the perfection of God.

But this way of thinking goes astray over the course of its development. When *Newton* concludes God's action out of gaps in his own mechanical explanation, he thereby admits that the laws of mechanics themselves provide no indication of God. Thus, however, the concept of God is pressed into a role not appropriate to it. It has become a special *causa efficiens*, an auxiliary cause to which one takes recourse when the physical explanation fails. *Laplace* was convinced that he had filled the gap in *Newton's* mechanical explanation; and it was only consistent for him to exclude the concept of God as a superfluous hypothesis in his picture of the world. Indeed, mechanical explanation means that every material fact is attributed to other facts likewise material. This thinking does not leave the level of matter and rightfully excludes the concept of God from its causal nexus. Conversely, the ancient conceptualization, which does not ask about cause but about form, leads every factual truth to truths of a higher order. In this arrangement, recourse to God is the necessary highest step.

Where do we of the present day now stand, if we are to succeed in explaining the genesis of the planets mechanically? Without a doubt, we are perfecting the program of causal thinking. One can ask about the causes and one obtains answers to those questions. There is hardly anything more imposing than closed causal theories of science. But the way these theories are normally understood, they perfect an inherent abstention from such questions as were still self-evident to *Copernicus* and *Kepler*. And the merciless realm of technology shows us what a human world looks like in which such questions are no longer asked and their due answers are no longer heard.

One cannot expect, nor ought one to wish, that science dispense with asking about causes as long as there is still a prospect of an answer to that question. This talk today intends to portray theories that remain entirely within the framework of causal thinking. I do believe, though, that we are obliged to remind ourselves that other questions besides the causal ones can still be posed. I do not want to pursue in this presentation the philosophical questions connected to material causality: What is matter? Why do natural laws hold? Is there among the set of causes a primary one? I just want to pose one question that witnessing scientific discoveries impresses upon us: Would it not be more correct to think of the relation between material things and God less according to the scheme of effect and cause than according to the scheme of sign and meaning? Is a natural law a competitor to God or is it rather a mirroring, a first indicator of Him? Once we have recognized a truth somewhere, be it even within the realm of matter, have we thereby not experienced in simile what truth itself is?

Not a word more shall I say about these issues. They will accompany unsaid whomever they touch at all.

3.2 Kant's Theory

In introduction to the problem of the formation of the planets I must briefly describe *Kant*'*s* theory to you. *Kant* sets out from an observation. In addition to the zodiac, we also see a second distinct larger circle in the heavens: the Milky Way. It, too, we conceive as a plane in which we ourselves are located. Its white shimmer is the summated light of countless stars crowded together in this plane. *Kant* asks what this giant stellar system would look like from the outside, in which our Sun plays a role of even lesser significance than a planetoid in the planetary system. He believes that certain elliptically shaped nebulous spots in the sky are faraway Milky Way systems. Today we know that this assumption of his was right. I show you one of these nebulas in this picture (Fig. 3.2), the large spiral nebula in the stellar constellation Andromeda. The nebula is evidently visible skewed from the side; the discus-like shape is clearly recognizable.

Why do such large clusters of matter in space have this shape? *Kant* explained their structure by rotation. They rotate around an axis and flatten out in the process, like any rotating body. They are simultaneously under the influence of gravity. All their parts tend toward the common centre. The law of the conservation of angular momentum holds, according to which a rotating body must rotate faster the closer its parts shift toward the rotational axis. The flattening increases at the same time. The final state is reached when every part of the nebula describes a circular orbit around the centre, just like a planet revolving around the Sun, at exact equilibrium between gravity and inertia; in this state the entire nebula must have become flat like a plate. We should note, by the way, that it cannot rotate like a solid plate. For then the outermost parts of the nebula would have to complete their orbit around the centre in the same period as the innermost ones. In fact, however, the equilibrium of inertia and gravity demands, according to *Kepler*'*s* third law, that the inner parts revolve rapidly, the outer ones slowly.

Against such a nebula, just a million-fold smaller, *Kant* compares the earliest state of our planetary system. Similar to the Milky Way system, probably originally a diffuse gaseous mass having disintegrated into individual stars while retaining its shape, the planetary system also supposedly gradually formed individual bodies out of its gaseous state: one large central body, the Sun, and as smaller peripheral bodies, the planets, comets and meteorites.

This conception explains numerous facts in a single blow. Remember what *Kepler*'*s* laws do and do not encompass!

Part of *Kepler*'*s* second law and an elementary consequence of mechanics is that every planetary orbit has to be a flat curve. It does not, however, follow from mechanics that all planetary orbits must lie on the same single plane. Mechanically it would be just as well possible, for instance, that Jupiter's planetary orbit occur on a plane orthogonal to the planet Saturn's, and so forth. Then there would be no zodiac. If, however, we conceive the planets as the remains of a mass that had originally rotated uniformly, we immediately grasp that their orbital planes are identical and that they all travel around the Sun in the same direction. The planets'

Fig. 3.2 The large spiral nebula in the stellar constellation Andromeda

rotations around their own axes are also performed in the same direction (albeit with certain angular oscillations) and the moons also orbit around their planets in the zodiacal plane. Only the outermost planets, Uranus and Neptune, which perhaps had been subjected to a subsequent perturbation, are exceptions to this. On the whole we may say: In the real planetary system a close relationship exists between the orbits of the various planets, while mechanics initially treats each planet as if it could move independently of all the other planets. According to *Kant* this relationship is not an actual mechanical one but historical fact: It is the visible remains of the original physical unity of the entire system.

Kepler's first law also leaves too much freedom. It only establishes that the orbital curve be an ellipse. For all large planets it is, in fact, almost a circle. This too is explained by the nebula concept. The nebula particles in regular rotation had to travel in circles and that is why the planets having formed out of them also do so.

Kepler's third law, finally, indicates how quickly a planet must move around the Sun, given its distance from the Sun. Yet it does not predict anything about the distances that really occur. You surely remember from the first picture I showed you that the distances between the planets and the Sun form a quite regular series. This regularity is mathematically expressible as a rule that *Titius* and *Bode* posited in the 18th century. In a simplified version valid only for the planets Mars and upwards, it states that every planet is approximately twice as far away from the Sun as the one preceding it. This rule is not very accurate. But it has become famous because a new planet was predicted on its basis. According to this rule, there is a gap between Jupiter and Mars. Jupiter is not twice as far away but four times as far away from the Sun as Mars. Therefore, another planet ought to be between the two. In 1800 a planet was, in fact, discovered there. It later became apparent, though, that not just one planet was in that region there but a cluster of planetoids.

Kant's theory, however, encounters a limitation with the rule by *Titius* and *Bode*. According to this theory it is not implausible that the distances between the planets and the Sun would be arranged according to some law. But the precise form of this law could never be explained, not even by later theories. It seems especially strange that although there is a law for the spacings between planets, there is none for their sizes. Right next to Jupiter, the largest of all the planets and containing more matter than all the other planets put together, are the planetoids, which together have a mass smaller than Mercury, the smallest of the major planets. If one considers planets as having formed from an approximately homogenously distributed gaseous mass, there ought to be a relation between the interspacings between the planets and their sizes. Then a very large planet ought to be far away from its neighbours; a very small one ought to be followed a short distance away by another. There is only a suggestion of this rule in that the outermost planets whose interspacings are large are, on the whole, also more massive than the inner ones; but taken individually, the rule certainly does not hold.

This is not the only trouble with *Kant's* theory. The orbital direction of the moons poses another problem. The moons, in fact, travel in the same direction around their central planets as the planets around the Sun. According to *Kant's* theory it ought to be the opposite. The moons are formed out of matter that the planets attract from the surrounding gaseous mass. According to *Kepler's* third law, this matter generally does not have the same orbital velocity around the Sun as the planet. The matter that was originally closer to the Sun than the planet must travel faster than it; the matter coming from the outside must travel slower than the planet. If moons are formed out of such matter revolving around the planets, they must obviously always overtake the planets on the inside and must lag behind on the outside, that is, when they are farther away than it from the Sun. In actual fact the moons rotate in the opposite sense.

An even more fundamental difficulty is that it has never been possible to show that a diffuse mass revolving around the Sun does unite into individual large planetary bodies at all. These considerations are quite convoluted but the various versions of this hypothesis offered by *Kant, Laplace* and their successors have led not to solutions but ever only to new problems.

Finally, there remains not another internal contradiction but an open question: The theory does not tell us where the original rotation of the system comes from.

I cannot go into the numerous more recent hypotheses supposed to replace *Kant's*. Now I want to describe how according to my suppositions the problems with *Kant's* theory can be removed when current-day astrophysical knowledge is consistently applied.

3.3 New Version of the Theory

Kant did not know how the Sun and the planets are chemically composed.[2] Today the following can be said about this: All bodies in the world as we know it consist of the same chemical elements: only the mixture ratio between the elements exhibits certain variations. These variations also chiefly concern the proportions of the lightest elements, primarily hydrogen and helium, to the other heavier elements, while the latter manifest roughly the same distribution throughout the whole cosmos. The planets are composed almost solely of these heavier elements. By contrast, about half of the Sun's weight constitutes hydrogen and the other half is almost exclusively composed of light gases up to oxygen. Elements heavier than oxygen make up about half of Earth's weight but less than one hundredth of the Sun's weight.

Unfortunately I can only hint at how these findings were made. We are able to examine the surface of Earth directly. Some geological conclusions can be drawn about Earth's interior. The appearance of the planets' surfaces likewise permits conclusions. Furthermore, we know the specific weights of the planets because we can determine their volumes directly and know their total masses from their gravitational influence on other bodies. The chemical composition of the solar surface follows from the spectrum of light emitted by the Sun. We cannot, of course, see into the interior. However, a series of solid theoretical grounds chiefly supported by atomic physics speak for the great prevalence of hydrogen, demonstrated by its surface spectrum, also existing within its interior. For example,

[2] Complete exposition of the new theory in: *Zeitschrift für Astrophysik* 22 (1943) 319.

today we know that the enormous amount of heat radiated continuously from the Sun must be generated by the transformation of the atomic nuclei of hydrogen into helium in the Sun's deepest interior. The way this energy is transported outside also essentially depends on the Sun's hydrogen content. Finally, perhaps the strongest argument is the consideration that in the Sun's interior, which due to the high temperature is necessarily gaseous and, on the other hand, due to the Sun's rotation is in constant motion, there must be a continual powerful mixing of the material through convection currents; so a difference in composition between the interior and the surface could not be sustained.

What conclusion follows out of this mechanical difference between the Sun and the planets? It would be strange if the uniform gaseous mass out of which all bodies were formed should manifest such differences in composition. It is much more probable that the difference occurred while the planets were forming. Precisely this follows from a closer analysis of the processes of planet formation.

Everything we know about the distribution of hydrogen in the cosmos suggests that the Sun's composition is normal and that of the planets is exceptional. We should thus assume that the gaseous mass out of which the planets formed also largely consisted of lighter gases. This mass in the vicinity of the radiating Sun would have been at a temperature roughly comparable to the temperature currently prevailing on the planets, such as on Earth. What physical and chemical state was it in at that temperature? All the elements in gaseous form on Earth, i.e., hydrogen, nitrogen, oxygen and all the noble gases, must also have been gaseous in it. The elements we know as solid bodies or liquids must have condensed within it into small clumps or drops. These clumps and drops were swirled about inside the gas envelope, collided against each other and thus merged together into ever larger bodies. One can estimate that bodies of planet size would have to have formed by such a process over a period that may have lasted between 10 and a 100 million years. Since we can determine from radioactive material that the Earth is at least 2 billion years old, a period of a 100 million years can still be considered an appropriate duration for the formation.

We now understand why the planets contain only the heavier elements: Only those condense at the prevailing temperature. Through chemical bonding with the condensing material, some hydrogen, nitrogen and oxygen is likely to have been compounded in the planets. The noble gases, on the contrary, which do not bond, had to stay outside of the planetary bodies. Experience confirms this conclusion. The noble gases only occur on Earth in traces, even though at least two of them, helium and neon, belong among the most frequent elements in the cosmos.

Now, if the entire remainder of the gaseous envelope did not condense into planets, why isn't it still there today? One can show that it must necessarily dissipate over time because its state of motion was not stable. The reason for it is the internal friction of the gas. According to *Kepler's* third law, the outer portions of the gas move more slowly than the inner ones. The amounts of gas moving at different velocities are not separated from each other but rather touch and thus rub against each other. Friction attempts to balance out their differences in velocity. The inner portions of the gas are thereby slowed down, the external ones are accelerated. The inner portions now move too slowly. The inertia is no longer able to counter the gravity with the necessary resistance and they gradually sink into the Sun. Conversely, the external portions now travel too rapidly. The gravitational force is no longer able to control their motion and they escape little by little into space. I think that every rotating gaseous mass must eventually dissipate in this way. The picture of the spiral nebula that I showed you likewise reveals a disintegration into a concentrated core and a very flattened envelope streaming outwardly away. An estimate of the time needed to disperse the Sun's gaseous envelope completely leads again to about a 100 million years.

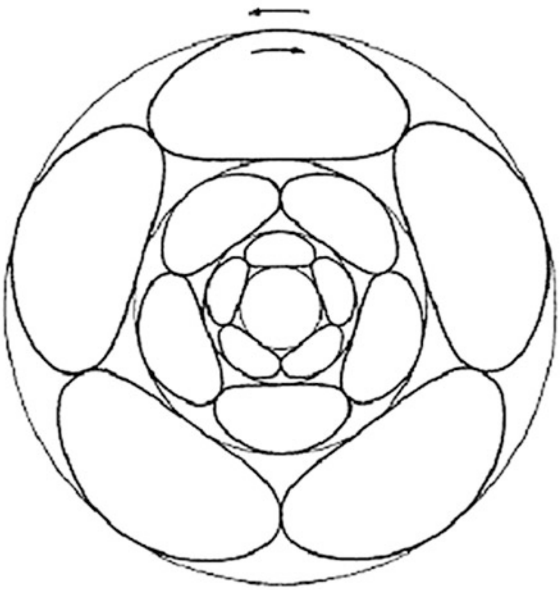
That planets formed at all has surely become understandable based on our assumptions. Can we eliminate the other difficulties of *Kant*'*s* theory as well?

The two issues about the *Titius-Bode* law for the spacings between the planets and the rotational orientation of the moons prove to belong together. Their explanation is, of course, somewhat more complicated than the foregoing one; it also represents the part of my talk that I myself still feel is the most hypothetical. We must examine the internal motions within the gas envelope somewhat more closely. I said before that the gas envelope rotates such that each of its parts moves along its own orbit according to *Kepler*'*s* third law. On the other hand, the internal friction causes part of the material to stream inwardly, part of it outwardly. The internal friction attacks all portions of the gas envelope simultaneously, however. Hence inward and outward motions must arise everywhere at the same time. Obviously a turbulent motion results. I do believe, though, that this motion nevertheless does not proceed entirely in an unorderly fashion rather that a regular system of currents forms. In this last image I illustrate these hypothetical currents as a diagrammatic sketch (Fig. 3.3).

The outer arrow indicates the sense of rotation of the gaseous mass as a whole. The drawn-in vortex systems consequently all move in this sense around the Sun, which should be thought of as a small point at the common centre. The bean-shaped figures represent the edges of each of the regions of the gaseous mass which circle around the Sun together and do not dissolve as long as they are not disturbed externally. In doing so, seen from their own midpoints which themselves are rotating around the Sun, they have an internal motion of revolution indicated by the inner arrow, that is, they also rotate around themselves just once during a single revolution around the Sun. All the vortices within one round ring travel at the same speed around the Sun and therefore do not change their positions against one another. But the vortices of the inner rings travel faster than those of the outer ones, in accordance with *Kepler*'*s* third law. Just three rings of vortices are drawn in; in reality this figure can be continued analogously inwardly and outwardly.

The notable thing about this current motion is that it can also be performed under the sole influence of gravity and inertia. The individual gas particles within the vortices, regarded from the Sun, describe exact *Kepler* ellipses. That means, the ellipses of all the particles situated in one and the same vortex have the same period of revolution around the Sun, and just for that reason the vortex constantly stays together. In the interior of the vortices the motion is completely still as a consequence and practically no internal friction takes place; the entire friction is displaced to the external edging of the vortices. I would suppose that this is the

Fig. 3.3 Vortices in the pre-planetary gas



state of the gas envelope with the least internal friction and that it is stable for just that reason. Now, if at the inner and outer edges of the gas envelope matter is constantly streaming away, inwardly into the Sun and outwardly into space, then this material can be constantly supplemented from the adjacent vortices; the vortices act like water wheels.

The boundary areas between the vortices are precisely what is important for the formation of planetary bodies. This is because the motion is so uniform in the interior of the vortices that the clumps of condensed matter are swirled around very little as well and therefore have little opportunity to come together and accrete. Within the zones in between, however, in which oppositely moving vortices almost touch against each other, a hefty turbulent motion must prevail; there the lumps often collide and rapidly agglomerate. Thus numerous planet-like bodies will form on every contact circle between two vortex rings. The planetoids probably constitute a remnant of this state. The consolidation of these numerous bodies into a single large planet probably only occurs when one of them has become big enough to be able to pull the others into itself by its own gravity. Among the planetoids none has become large enough for that; so their large number is probably a consequence of their low total mass.

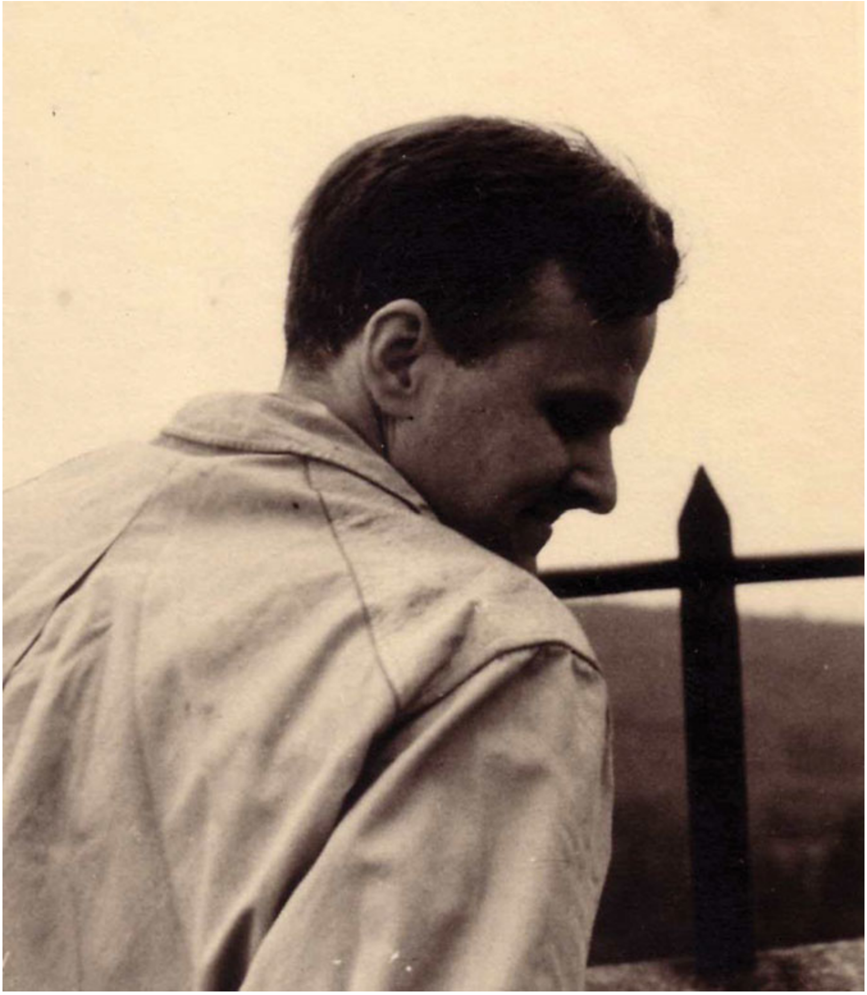
The *Titius-Bode* law may almost be read off this diagram. The number of vortices on a ring should always be about the same, irrespective of how far away the ring is from the Sun. Therefore the diameters of the vortices and thereby the ring spacings should increase proportionally to the distance from the Sun, just as experience shows. I have not managed to deduce theoretically how many vortices must lie on a ring. Taking as a basis the empirical spacings between the planets, there should be just about 5 for the region between Mars and Uranus. That is how it is drawn in the figure. Inside the orbit of Mars the number of vortices per ring must be somewhat larger. Now we also see why a law holds for the spacings between planets but not for their sizes. The location of a planet's formation is determined by the vortex system. How much condensed matter comes together there can depend on chance, however. It is only in the neighbourhood of a very large planet, such as Jupiter, that a zone particularly low in matter may exist, like the one in which the planetoids lie; Jupiter had presumably 'sucked it up' during formation.

Which revolving direction must the planetary rotations and the orbits of their moons have? The internal revolution of the vortices is directed counter to the total revolution of the system; this necessarily follows from the condition that every particle of the vortex travels around the Sun on a *Kepler* ellipse. On the contact circles between the vortices the material is rotated like a ball bearing between two oppositely rotating wheels, again in the opposite direction, hence in the direction of the total revolution. Yet here is where planets and moons form; thus they must necessarily have the sense of rotation of the total system, just as experience instructs. The motive scheme is not adequate as a planar arrangement for Uranus, whose moons travel on a plane perpendicular to the zodiacal plane.

In closing I turn to the still open question of the origin of the rotation of the system. Although we cannot answer this question fully, the uncertainty can be pushed one step back. The entire solar system is supposed to have formed, just as the other fixed stars, out of the originally diffuse material of the large Milky Way system. The Milky Way system is also rotating as a whole but in a way that the inner parts travel faster than the outer ones. Strong turbulent inner motion had to form in it, too, as a consequence of the differences in motion. This motion caused the matter not to sink evenly from all sides into the forming Sun; rather in the process hefty sideways motions occurred at the same time. These motions may have been randomly distributed. On average they would then not have cancelled themselves out completely, instead a resultant rotational motion must have remained in some unpredictable direction.

The question has often been raised whether other planetary systems exist in the universe or just our own. The planets of other stars could not be looked at owing to their great remoteness; so for this problem we must rely on theoretical conclusions. Among the considerations I presented to you, there is none that could not be applied to other stars as well. I would therefore like to assume that there are many other planetary systems in the universe. In some cases the planet has perhaps grown so much that it has become a second Sun and a binary star has been formed instead of a planetary system, just like many we know of in the sky. Perhaps in other cases the matter has sufficed only for the formation of meteors and comets. But surely a large number of cases remain in which planetary systems similar to our own have been constructed.

If we wished to examine the origin of the rotation further, we would have to turn our attention to the development of the Milky Way system and the genetic relations between the spiral nebulas themselves. I do not want to broach this question today.



Carl Friedrich von Weizsäcker as a young Professor of Astronomy in Strasbourg (1943). *Source* The Weizsäcker Family represented by Dr. Elisabeth Raiser who granted permission to use this photo

Chapter 4
The History of Nature, Introduction

4.1 Introduction

My subject is the history of nature. This means that I have set myself a theme far broader than is the custom in the run of academic writing.[1] No scientist can master all the fields of knowledge touched upon in an essay of this sort. Therefore, I should perhaps begin with an attempt to justify my choice of subject.

We are becoming more and more aware of the danger that lies in the specialization of the sciences. We are vexed by the barriers that are raised to separate the various disciplines from one another. Specialized science is powerless to give us a world-view that could sustain us in the confusion of our existence. And so we are longing for synthesis, searching for the point of vantage from which to gain perspective.

But if we are to overcome the errors that lie in the self-limitation of the several disciplines, we must first of all understand what in this self-limitation is justified. Specialization does not spring from accident, or from a whimsy of the scientists, but is a fateful consequence of the very character of science. A man's fate is most often shaped by that trait of his character which contains at once both the possibility of his greatest virtue, and the temptation to his greatest vice. In the true scientist, this determining character trait is his awareness of his intellectual responsibility for the particular. Others may sense, believe, profess—the scientist inquires. He believes only the results that he has gained from inquiry. He believes only where he knows. Now it is impossible to know all one would like to know, or even all one ought to know. It follows that the scientific attitude always means a resignation and self-denial—even for the most fertile scientist. This self-denial is the source of specialization. Like any sacrifice made in full awareness, it may deserve our admiration, when it is due to the feeling of responsibility. Or it may

[1] This text was first published as: "Introduction", in: Carl Friedrich von Weizsäcker: *The History of Nature* (Chicago: Chicago University Press, 1949). This text was translated into English by Fred D. Wieck from: *Geschichte der Natur*, (Göttingen: Vandenhoeck & Ruprecht, 1948).

M. Drieschner (ed.), *Carl Friedrich von Weizsäcker: Major Texts in Physics*, SpringerBriefs on Pioneers in Science and Practice 22,
DOI: 10.1007/978-3-319-03668-7_4,

deserve our contempt, when it is due merely to the lack of effort for perspective. In any case, it has become the fate of science.

Those of our characteristics that become our fate are seldom wholly independent of our will. If we want to counteract specialization, what is there in the scientist's nature and mind to which we might appeal?

The scientist is never only a scientist. He is at the same time a living human being, he is a member of mankind. And so, his responsibility for the particular is counter balanced by his share of responsibility for the whole. He has to ask himself: What is the meaning of my inquiry for the lives of my fellows?—Can I answer for the effects that my work has upon the life of mankind?

The effect of science upon life is summed up in a well-known saying: Knowledge is Power. We will have to admit this truth today even if until recently we may have been inclined to doubt it. But, is power good? Man has learned to build the instruments of power. Will he learn to master them?

Some people are convinced that man will never learn this mastery, and so they believe that we ought to renounce a science that places such instruments of power in our hands. I can hardly presume all by myself to settle this problem once and for all. But it is my feeling that what is proposed here as a solution is something that is impossible. I believe that our world cannot in fact give up the knowledge that is the source of power. This or that individual man may give it up, but the world in general will not give it up. And if this is true, then is not the man who does give up science, dodging perhaps the responsibility he should help to bear?

But how can he bear the responsibility?

If knowledge and the power that comes from knowledge can be good at all, they can be so only in the hands of good men. The choices that must here be made do not lie in the sphere of science but in that of morals. I would add as my personal conviction that they do not even lie solely in the sphere of morals, but in that of religion. It is not my task here to discuss the practical aspects of the choices. But I believe I must point out that they are unavoidable. And it is my task, I believe, to raise the question what our science must be like if we as scientists want to shoulder our share of responsibility for the whole. Power is only one side of knowledge. The other side I would call insight—an inadequate term.

I admit. The knowledge that brings power is instrumental knowledge. It asks, what ends can I accomplish with the given means? What means do I require to bring about a desired end? The human motives that prompt me to desire a specific end are not considered. Man in this context appears as a completely free and irresponsible being. He is the subject who as master confronts an object in no way akin to him. Instrumental knowledge is knowledge of fragments, and is content with that. It can be satisfied with a science that is completely specialized. Insight, on the other hand, I would call that knowledge which considers the coherence of the whole. Insight must be especially concerned with man himself, his motives and his aims, and with the inner and outer conditions of his existence. Insight may not separate subject and object fundamentally, but must recognize their essential kinship, their mutual dependence and, consequently, their inseparable coherence.

Let me be quite clear: I do not think that such insight constitutes in itself a cure for our age. Every insight can be abused by an ill will, even by a merely misdirected will. But those among us who in this instrumentalized world of ours want to work for the good will search for insight. They will search for the coherence within which the instruments have their origin and their potential meaning. Not that insight produces the decision for what is good, but this decision, once it is made, desires insight.

As we are trying to achieve such insight, the notion of responsibility for the whole acquires a specific, concrete meaning. It now means the responsibility for the whole which is the totality of all sciences, the *universitas literarum.* For insight in this sense clearly is to be found, not in the several disciplines but only in their interconnection, not in the single bricks but only in the whole edifice of the sciences. It is in this sense that the present chapter is concerned with the totality of science.

Is this totality real? Is it not an empty dream?

The deepest rift that is at present dividing the edifice of science is the cleavage between natural science and the humanistic disciplines. Natural science, by means of instrumental thinking, enquires into the material world around us. The humanistic disciplines study man, and they accept him such as man knows himself: as soul, consciousness, and mind. The cleavage resides not so much in the subject matter—that overlaps in part—as in approach and method. Natural science is founded on the sharp distinction of the comprehending subject from the object which is comprehended. To the humanistic disciplines falls the much harder task of turning even the subject, in his subjectivity, into an object of their understanding. Many an attempted conversation shows that these two modes of thought understand each other only very rarely. But it seems to me that, beyond this mutual misunderstanding, there lies in readiness the possibility of an objective connection between both groups of disciplines, waiting only to be seen and to be made a reality. Let me indicate this connection by a simile: Natural science and humanistic disciplines appear to me like two half-circles. They ought to be joined in such a way that they combine to form a full circle, and this circle ought then to be followed round fully, many times. By this I mean:

On the one hand, man is himself a being of nature. Nature is older than man. Man has come out of nature and is subject to her laws. An entire science, medical science, is successfully engaged in studying man as a part of nature, with the methods of natural science. In this sense, the humanistic disciplines presuppose natural science.

On the other hand, natural science is itself made by man and for man, and is subject to the conditions of every intellectual and material work of man. Man is older than natural science. Nature had to be so that there could be man—man had to be so that there could be concepts of nature. It is possible as well as necessary to understand natural science as a part of man's intellectual life. In this sense, natural science presupposes the humanistic disciplines.

Both groups of disciplines see as a rule only one side of this mutual dependence. The thinking in the circle of mutual dependence may even awaken the suspicion that we are thinking in a vicious circle where A is proved by B and B in turn by A. But we are dealing here not with logical deductions but with the dependence of real things on one another, and they often close in a circle. Conceptual thinking has split the original unity of man and nature into the opposition of subject and object. The circle of which I am speaking is meant as the first, though perhaps not the last step in a direction which should once again make the unity of the two opposites accessible to our thinking. Only when we understand clearly and in detail the dependence of man upon nature, and of the concepts of nature upon man, only when we have rounded the circle many times, only then, at best, may we hope to see reality as one, science as a whole.

I am not aiming in this chapter at so comprehensive an achievement. Excepting a few digressions, I shall speak only of one of the half-circles—of nature, and man's origin in nature. But in return, I mean to speak of the whole of nature, as far as that is possible in such limited space. Again, this cannot be done in all fullness but only under one specific aspect. The aspect I have chosen is the historic side of nature. Of course, I have made this choice so as to keep before our eyes the connection with the humanistic disciplines. For one of the most important of humanistic disciplines is the one called history.

Among the fundamental convictions of many humanists is that man and man alone is a historic being. To this conviction I would oppose the assertion: Man is indeed a historic being, but this is possible because man comes out of nature and because nature is historic herself. What distinguishes man is not that he has a history, but that he has an understanding of his history. Here I must first explain the sense in which I am using the concept 'history'. In doing so I shall at the same time explain the purpose of the present chapter.

History is what happens. Yet it includes not only what happens now, but also what has happened and what will happen. History occurs in the past, the present, and the future—for short, in time. History in the broadest sense is the essence of what happens in time. In this sense, nature undoubtedly has a history since nature herself is in time. History of nature, then, would be the totality of what happens in nature.

But the humanistic disciplines and philosophy, particularly in recent times, have become accustomed to a narrower concept of history. We shall arrive at this narrower concept by a gradual refinement of the concept with which we have started.

History is only where there is change. In pride or in agony, mankind experiences the turmoil of its history—eternally unmoved, without history, the starred heavens look on. A stone that sleeps beneath the ground millions of years has no history—above it, historic life blooms and withers, hurries and grows.

History is only where there is irrevocable change. The planets revolve even in the skies, but since billions of years their paths have been forever the same. The planetary system is in constant motion, but fundamentally it does not change. Hence it is without history. The same seems to hold true of living nature. Every spring anew the woods cover themselves with leaves, and every fall they turn bare again. They are a symbol for us of the unchanging cycle of history-less nature. But man experiences events that separate past and present irrevocably. In himself alone, not in nature, does man undergo the basic experience of the historic: we do not step twice into the same river.

But nature's appearance of being without history is an illusion. All depends on the time scale we use. To the mayfly whose life spans 1 day, man is without history; to man, the forest; to the forest, the stars; but to a being who has learned to contain within his mind the idea of eternity, even the stars are historic essences. A 100 years ago none of us was alive. Twenty thousand years ago the forest did not stand, and our country was covered with ice. A billion years ago the limestone that I find in the ground today did not yet exist. Ten billion years ago, there was most likely neither sun nor earth nor any of the stars we know. There is a theorem of physics, the Second Law of thermodynamics, according to which events in nature are fundamentally irreversible and incapable of repetition. This law I should like to call the law of the historic character of nature. In the fourth chapter it will be discussed at length.

However, there is indeed a fundamental difference between man and nature. Nature undergoes history, but she does not experience it. She is history but does not have history, because she does not know that she is history. And why does man alone have a conscious, experienced history? Because he alone has consciousness and experience. And so it does seem to me meaningful after all to see man's distinction not in his historic existence as such, but in his awareness of his historic existence. True, the historic existence of a conscious being is likely to differ from that of a non-conscious being. And as we study in what manner man has a history, we shall add to our knowledge of history in general, and to our knowledge of the history of nature.

At this point the concept of time needs closer study.

Time is the past, the present, and the future. What is the meaning of these three mysterious words?

Let us first consider a notion of time that disregards the differences between past, present, and future. The astronomer who studies the motion of the planets, or the physicist who studies the waning oscillations of a pendulum, treat time as little else than a fourth coordinate beside the three coordinates of space. This spatialization of time finds its most striking symbol in the graphic time schedule of the railroader. This schedule shows on one coordinate the distance the train travels, on the other coordinate the length of time in which the train covers the distance. Which portion of the train's course is past, which future, and where the present moment lies—that is irrelevant in the nature of the schedule. The schedule is not concerned with the question whether the train already has run according to it, or will run according to it in the future.

This concept of time, however, is an abstraction. You and I are concerned whether the train runs on schedule, and there is then a decisive difference between past and future. It can be known whether or not the train has in the past run on schedule. That is an unalterable fact. But whether the train will run on schedule in the future that cannot be known with certainty, not even in the best of all railroad systems. It is possible, it is to be hoped, it is likely—but it is not a fact, until it has become a present and then, quickly, a past event. Past and future events have different modes of being: past events are factual, future events possible. Neither are real in the strictest sense; actually real is only the present. But both past and future events are of the greatest significance for us. The past has created the framework of facts in which our present is held inescapably. History is fate. And the future will at one time be the present, so that we have a vital interest in it. We attempt to penetrate the future, to guide it. We constantly anticipate future events in our imagination. Our imagination treats that which is possible as if it were already actual. And indeed, at one time it will be actual.

This structure of time is what I want to call from here on its historic character. It has no analogies in space, or only very imperfect ones. It follows that the spatialization of time of which I have spoken is doing violence to the phenomenon 'time'. And this holds true not only for time as man experiences it, but also for the concept of time that we have to have in order to give an adequate description of nature. Perhaps I can explain this best by offering my defence against a counter-assertion that the pure natural scientists might easily throw up to me in reproach, and that might run like this: Everything that happens is in reality predetermined. That we do not know it is merely a sign of our human frailty. Man is only a small part of nature, and if we want to define so basic a concept as time, we must not introduce into the definition the limitations of our human understanding, but must transcend them.

This objection might first of all be countered with the question: Why is it, then, that the future is hidden from us so much more completely than the past? However, our methodical inquiry must search deeper, and we must now remember the full circle whose two halves are natural science and the humanistic disciplines. The tendency to blur the distinction between past and future by calling the future completely predetermined, is a characteristic of natural science. Natural science itself, however, is a specific product of the human mind, and the question that we must raise is, what does this tendency of natural science signify for the life of man.

After all, we know nature only through the medium of human experience. As concerns man, now, the past certainly is that which is factual and largely known, the future that which is merely possible and unknown. We have a memory for the past, but no corresponding faculty for the future. Still, even the future is not wholly inaccessible, and all our scheming and planning is intent on knowing something of the future beforehand. One of the means to gain foreknowledge is natural science. I have called its thinking instrumental. This means precisely that natural science strives to gain such knowledge as will allow us to know the future beforehand and perhaps even to influence it. The conceptual tool that natural science uses for this task is causal thinking. In many instances we find a strict connection of cause and effect that can be refined into the formulation of a precise mathematical relation between the states of things at different moments in time. Now the future can in part be calculated—in part, that is, as far as our chains of causes and effects will reach. The spatial representation of time I mentioned belongs in this world of a calculable future. Spatialization of time ends where future events cease being calculable by the law of causality. The technical tool of spatialization is, of course, the clock. On its dial we read elapsing spans of time as spatial lengths. But the clock is a mechanical apparatus. It 'runs' only as long as the causal connections in its movement take place without disturbance.

Now the assertion is made that there is 'in reality' no essential difference between past and future, since the future as such is likewise strictly determined. But this assertion presupposes that, if only we had a sufficient knowledge of nature, we should be able to give a description of nature in terms of cause and effect, down to the last detail. This is a hypothesis. It may even be a true hypothesis. I shall abstain from discussing here the reasons against determinism that have come to light in atomic physics. I only want to point out that determinism is not a matter of experience. The future as such may be determined, but to us it is not given as determined, neither our own future nor that even of inanimate nature. Immediate intuition, without reference to the question of determinism, shows us the following differences between past and future.

We cannot escape from the moment that is *now*. Every present becomes a past, every future at one time a present. The past, as the essence of facts that once were present, is determined. This determination has nothing to do with causal determination. For the strict chain of causation, starting within the present, does not enable us to compute an unknown past any more than it allows us to compute an unknown future. Basically, both the past and the future eclipses of the sun are equally easy or difficult of computation. Those in the past, however, have surely taken place, while those in the future can be predicted only with that degree of certainty with which we would dare to assert that no cosmic catastrophes will intervene. We know that man has existed 3,000 years ago, the earth 2 billion years ago. Would we dare to predict anything whatsoever for an equally distant future? Tomorrow all of us may die from a cause that is still unknown today—no power on earth can change that we were alive yesterday. In later chapters I shall show that the two fundamental principles of the historic character of both animate and inanimate nature—the Second Law, and the tendency toward the evolution of differentiated forms—can be derived from the difference between past and future that I have described, and a few simple assumptions in addition. And that will establish in turn that some of the central theses of natural science can be understood, not by overcoming the historic character of time but only by recognizing it.

To sum up: The past is what at one time the present was. It is made up of factual, unalterable events. The future is what at one time will be the present. It contains possible events, events on which we can exert an influence. Nature is historic insofar as her events take place objectively within the time that is defined as historic by these facts. Man is historic further in that he experiences subjectively the historic character of time, and acts in the manner in which a conscious being has to act in his position: determined by the past, reaching ahead into the future with his cares and his plans.

It follows from the historic character of time that a science of history exists only for the past. Historical science is concerned with telling what has in fact happened, regardless at first whether or not it had to happen. The future, to a certain extent, can be construed hypothetically in advance, and only its transformation into the present will show if the construction was correct. But the past can be studied without such construction, for it consists of facts that have occurred, whether we know them or not.

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Chapter 5
The Second Law and the Difference Between Past and Future

This short paper was originally published in the *Annalen der Physik*, 36 (1939).[1] I include it here because it is the first and most systematic discussion of an idea to which the other essays often allude: the idea of basing irreversibility on the structure of time. When I wrote the paper I was under the impression of having stated a fairly obvious matter, obvious especially to every empiricist or positivist, since I merely pose the question of in what manner temporal processes are given to us. I was somewhat astonished to find that the idea seemed rather foreign to most physicists. It plays a decisive role in the course of our further arguments (cf. II.5b).

5.1 The Problem

In practical life, the most important difference between past and future is that the past has irretrievably occurred, whereas the future is still undetermined and modifiable by our will. The difference exists for a mere observer as well, since, in principle, he can have detailed knowledge of the past, but not of the future. In the world view of physics, this difference seems not to occur. If the present is known, the determinism of classical physics fixes the future as much as the past, while the merely statistical propositions of quantum mechanics render inferences from the present to the past as indeterminate as inferences from the present to the future. Only the Second Law of Thermodynamics clearly identifies a direction in time. This essay tries to show that, within the framework of physics, it is the statistical interpretation of the Second Law which exhibits the structure of real time we have

[1] *Annalen der Physik*, vol. 428 (5. Folge, vol. 36), (1939): 275–283. It was included in: Carl Friedrich von Weizsäcker (1980). It is a translation of: *Die Einheit der Natur* (Munich: Hanser, 1971): II.2 by Francis J. Zucker (*ed.MD*).

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just described; and that this structure of time must be presupposed if the irreversibility of processes in nature is to be reconciled with the temporal symmetry of the fundamental laws of mechanics.

Gibbs seems to have been the first to have stated this fact, though not in a manner that is easily understood:

> 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.[2]

The following reflections are an attempt at explicating these sentences.[3] Their goal in physics is merely to eliminate certain obscurities that occasionally arise in the statistical interpretation of the Second Law. The results concerning the concepts of time and probability may, however, be of philosophical interest as well.

5.2 The Temporal Symmetry of the H-Theorem

In the following discussion we assume that the ergodic problem has been solved, hence that the H-theorem has been proved in sufficient generality for the questions we shall encounter. We can therefore define an entropy, and we know that a closed system whose entropy deviates at some particular time from its largest possible value will, with overwhelming probability, have a larger value of entropy at every neighbouring (in fact at every other) point in time.

In this form, the H-theorem does not and, on the basis of its derivation, cannot distinguish between directions in time. For apart from the concept of thermodynamic probability, which always refers to an individual moment in time, the proof of the H-theorem presupposes only the laws of mechanics, which do not change their form if the direction of time is reversed. We see this more plainly by recalling the line of argument. According to Boltzmann, the mathematical basis of the H-theorem lies in the extraordinary increase of the statistical probability; i.e., of the number of possible microscopic realizations of a macroscopically defined state, as

2 See Willard Gibbs (1902/1960).

3 These reflections were stimulated by N. Bohr's interpretation of Gibbs's ideas, mentioned in discussions and hinted at in his Faraday Lecture (*Journal of the Chemical Society*, 1932: 376f). Analogous ideas were developed by M. Bronstein and L. Landau (1933) who drew incorrect conclusions from them, however.

the maximum value of the entropy is approached. A state of non-maximal entropy has therefore an incomparably greater choice of neighbouring states with higher entropy than with lower. Assuming the ergodic problem to be solved—i.e., assuming that the statistically more probable state also occurs more frequently in time—it follows that a below-maximum value of the entropy in a system about which nothing else is known represents, with an overwhelming probability, a temporary relative minimum in the entropy rather than part of a monotonically increasing or decreasing series of entropy values. One can therefore infer with an overwhelming probability that the entropy of the system will be larger at a later time. This is the customary formulation of the H-theorem. But we can also equally well infer, with the same probability, that the entropy of the system was larger at an earlier time. This contradicts the Second Law, which demands for the past, too, that every entropy value in a closed system was preceded by a smaller or, at most, by the same value.

Thus the Second Law does not directly follow from the H-theorem. The two can in fact be reconciled only if one allows only future, but never past entropy values of a system known at a particular time to be calculated in accordance with the H-theorem. That is Gibbs's thesis, which I will now try to justify in two steps.

5.3 The Earmarking of a Direction in Time in Experiments Conducted by Man

I first assert: in any experiment conducted by man, the H-theorem can be used only for the calculation of the future state of the experimental object. One does not infer a past state by means of probability arguments, since one already knows it.

Every experiment whose course can be predicted by means of the Second Law does indeed begin with a state which the experimental object, as a closed system, would never have reached from the state of maximum entropy; for example, experiments on heat conduction or heat machines begin with a temperature differential, experiments on chemical reactions begin with a deviation from chemical equilibrium, and experiments on diffusion begin with the spatial separation of materials. Thus, directly or indirectly, this initial state must be produced artificially. One therefore knows how the state arose, and also roughly how the entropy of the system varied just prior to the experiment. Only the future change in entropy is unknown; its prediction is properly handled in terms of the probability argument of the H-theorem.

This applies not only to experiments, incidentally, but to all events in daily life, for in general one knows the past or can at least find out about it, but one does not know the future. It would be senseless to ask how probable it is that the chemical energy in this piece of coal was contained in it at an earlier time. For I know how long the coal has been lying in my cellar, and my coal dealer will, on request, inform me about its history since it left the coal mine: I know the past of this energy and could not care less how probable it is. But once I toss the coal into the oven I must rely on prophecy for its further fate, and it is only the great certainty of statistical thermodynamics that allows me to describe in advance the first steps in the ensuing dispersion of energy.

5.4 The Earmarking of a Direction in Time in the Universe

So far it has only been shown that in the case of past events known to us there is no reason to conclude that the entropy was higher at an earlier time than at a later one. The Second Law goes beyond this in two ways. First, it asserts that in the past, too, the entropy of an earlier state was lower than (or at best equal to) the entropy of a later state. Secondly, it asserts this also for that part of the past which is directly known neither through the memory of an individual physicist nor through tradition.

The first extension is an expression of direct experience. The second is the sort of generalization repeatedly practiced in physics. Anyone who rejects these extensions must apply the H-theorem to the part of the past not directly known as surely as he must apply it to the future; he thereby infers the existence of porridge that warmed itself up by cooling the environment, and of stars that did not radiate light but absorbed it concentrically. The absurdity of these instances underlines the justification for this generalization more emphatically perhaps than the conceptual remark that it is impossible to draw a sharp line between direct judgments of experience and those that are mediated by theory.

One must therefore base the Second Law not on the subjective human knowledge of past events but on an objective and universal property of the past. For this purpose, as will be shown in section g, the difference between past and future mentioned in the beginning suffices. However, many physicists will refuse to acknowledge that this difference, though a basic fact of our consciousness and cognitive ability, is also an objective property of physical events. We will therefore first of all examine the only other cogent formulation concerning the past that has been proposed as a justification of the Second Law.

It is due to Boltzmann and, briefly summarized, states: Very long ago, the part of the world that is known to us was in a statistically highly improbable state. Within the approximation in which one may regard this part of the world as a closed system, the increase in entropy then follows immediately for all later times.

The content of this assertion is surely correct. But its justification is problematical. Before entering on it, I wish to point out a peculiar interrelationship.

5.5 Consequences for the Physical Knowledge of the Past and the Future

The concept of a document of a past event shows that, far beyond the limits of our memory, we know more about the past than about the future. For example, the historian can infer from an old record that people of a certain character lived 1,000 years ago; a fossil proves that living organisms of a particular form existed long ago, and the lead found in a uranium ore enables us to compute, in millions of years, the exact age of that fossil. Yet no such discovery allows us to conclude that living organisms will still be inhabiting the earth 1,000 or 1 million years from now. Anyone who asserts that the surface of the earth was obliterated by a cosmic catastrophe 1,000 years ago is ignorant of the facts; anyone who asserts that this will happen in a 1,000 years cannot today be refuted.

Why then do we have physical documents of the past, but none of the future? Only the Second Law earmarks a direction of time in physics, and this fact can actually be derived from Boltzmann's formulation of the Second Law. The type of question which is answered by documents is the same as the type which leads to the concept of probability. A document is always something a priori improbable; for in order to be a document it must have such special properties as to make essentially certain that it could not have come about 'by chance'. According to the Second Law, an improbable state must have been preceded by a state more improbable still, and will be followed by one more probable. But states that are more improbable still, in which the document might have originated, exist only in small numbers; a very special assertion concerning the past can therefore be inferred. On the other hand, almost every other state we can think of is 'more probable', and thus, so far almost nothing has been asserted about the future. For example: to produce a piece of paper with a written text, a being as special as a man with particular abilities is needed; afterwards, on the other hand, the piece of paper can burn up, moulder, or dissolve in water, and in each case a more probable state of the world—which might be the same as if the piece of paper had never existed—is produced. The probable is the structure less, whereas we are asking for structure.

This property of documents of course does not explain memory as a fact of consciousness. Most likely it does, however, constitute a necessary physical precondition for the functioning of memory, especially if we consider that no sharp line can be drawn between direct remembering and the (usually unconscious) inferring from documents.

5.6 A Critique of Boltzmann's Formulation

It would not do to accept Boltzmann's formulation without further justification. In characterizing even that remote state of the world as highly improbable, the formulation poses a challenge: how could it have happened that such an improbable state was realized and became the starting point of all the events known to us, when, on the other hand, the statistical foundation of thermodynamics rests on the presupposition that it is always the probable which happens? One immediately senses the inappropriateness of a question that refers to the probability of an event unique as far as we know; but it is merely the conceptual inadequacy of Boltzmann's formulation—despite its practical usefulness—that here expresses itself. Anyone who wishes to reject the question as meaningless must describe the past in terms not based on the concept of probability. And conversely, anyone who wishes to hold on to the fundamental significance which the probability concept has in Boltzmann's formulation must answer the question; i.e., must try to show that, on the basis of statistical arguments, the appearance of the initial state was to be expected. The first approach, which seems to me the correct one, will be discussed in the following section. Boltzmann, however, chose the second; his arguments must therefore be examined.

In Section 90 of his *Lectures on Gas Theory*, Boltzmann regarded the known world as a real fluctuation phenomenon in a universe of much greater spatial and temporal extension.

> Then in the universe, which is in thermal equilibrium throughout and therefore dead, there will occur here and there relatively small regions of the same size as our galaxy (we call them single worlds) which, during the relative short time of eons, fluctuate noticeably from thermal equilibrium, and indeed the state probability in such cases will be equally likely to increase or decrease. 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 centre 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 latter toward the future). By virtue of this terminology, such small isolated regions of the universe will always find themselves 'initially' in an improbable state.[4]

The explanation offered for the fact that we happen to be observing such an extremely unlikely fluctuation appears to be that only such a fluctuation can provide the basis for the process of life.

However, the consistent application of probability calculus to this picture of the universe leads to unacceptable consequences. Consider the state of our 'individual world' at a time shortly after the state of lowest entropy. According to the H-

[4] See Boltzmann (1964).

theorem, this state is already a good deal more probable than that 'initial state'. But then a very much greater number of individual worlds whose 'beginning' corresponds to this 'later' state (with all its details) must exist. Admittedly this state also contains numerous 'documents' of events that occurred between the 'true beginning' and itself. It certainly does not follow that these events would actually have occurred in all the individual worlds in whose history they are recorded. For indeed it is statistically much more probable that all these documents came into existence in a fluctuation than that the preceding states of lower energy which we infer from them actually occurred. The point is that improbable states can count as documents only if we presuppose that still less probable states preceded them. It is therefore statistically far more probable that not the initially postulated beginning but some later moment in time represented the entropy minimum. And the most probable situation by far would be that the present moment represents the entropy minimum while the past, which we infer from the available documents, is an illusion.[5]

The statistical inference of the past thus leads to absurd consequences also within the framework of Boltzmann's picture. We therefore return to the opposite point of view.

5.7 The Actual Structure of the Past

The following premise suffices for the derivation of the Second Law: At every moment, the past is an accomplished fact which is to be regarded as known in principle; the future, on the other hand, is still undetermined and can in principle be predicted with the help of statistical methods, with the degree of uncertainty that is peculiar to these methods. We first of all infer from this an increase in entropy for the future. But every past moment was, at one time, in the present; from this we infer an increase in entropy for all times that were then in the future, thus also for times that today lie in the past.

The following derivation also suffices: Let us characterize some state of the world in the remote past in terms of its physical condition at the time. If this condition deviated in the least from thermal equilibrium, the Second Law is guaranteed for all later time. In this context, Boltzmann's formulation appears like a derived result, for every actualized fact is one among very many possible facts and thus a priori statistically improbable. For our present knowledge of the

[5] From a somewhat different point of view, M. Bronstein and L. Landau (cf. footnote 3 above) throw light on this matter by noting that a fluctuation would give rise to an observer without a surrounding world much more frequently than it would give rise to the entire world known to us; so that no explanation is being offered for why we should be observing such an enormous fluctuation.

universe, it would probably suffice to assume that 10^{10} years ago the world consisted of thinly distributed hydrogen of constant spatial density, in a state of rest and at an absolute temperature of zero.

The second derivation is, in a sense, merely a concrete expression of the first. After all, one will presuppose only a past state that one believes really existed. If we are interested merely in the recent past, we can rely on memory to assist us in determining its properties; if we are interested in the history of the entire universe, we use the documents that are available today in determining the properties of the past. (For example, in the above case we would infer the constant parameter in the distance-velocity relation for spiral galaxies, as well as the relative amounts of the chemical elements.[6])

These documents acquire their status as documents, however, only because we presuppose the structure of time invoked in the first derivation.

But one can also reverse the relation. Perhaps we will someday be able to earmark a particular past state of the world by means of certain special conditions. For example, it is in keeping with the spirit of modern cosmological speculations to demand mathematical simplicity not only of the laws of nature but also of the initial conditions in the universe. It is of course conceivable that these initial conditions for a past (or future) inaccessible to all direct experience suspend the preconditions for the application of our familiar conception of time. For the span of time accessible to us, however, reflections of the kind described in section d must of themselves formally lead back to the difference between past and future that we presupposed; just as quantum mechanics, in the case of large quantum numbers, leads back to classical mechanics, which, methodologically, is its presupposition.

The two conceptions are not mutually exclusive, but complementary. On the one hand, the difference between past and future that we described is one of the indubitable facts of consciousness which constitute the preconditions of all possible knowledge and, therefore, the only certain foundation of science. Since the concept of probability presupposes that of experience, and since experience cannot be defined or described without referring to the difference between past and future, the application of the probability concept to the past, which we criticized above, is meaningless in a strictly logical sense; the absurd consequences merely illustrate this. On the other hand, the external reality whose properties we deduce, if only hypothetically and gradually, with the help of this methodologically grounded science, constitutes the physical precondition of our own existence. One is therefore entitled to start from these preconditions in justifying the content of the assertions that were previously methodologically presupposed. Only the complementary interplay between the two conceptions can stake out the limits of the possibilities of scientific knowledge.

[6] See C. F. von Weizsäcker (1938).

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Prof. C. F. von Weizsäcker at a Quantum Logic conference in 1984. © Hans Berner who granted permission for its use in this volume

Chapter 6
A Sketch of the Unity of Physics

A lecture delivered under the title: "The Unity of Physics" at the Convention of Physicists in Munich, 1966.[1] Published in *Physikalische Blðtter*, 23 (1967): 4–14, and (in English) in *Boston Studies in the Philosophy of Science*, vol. V (Dordrecht, Holland, and Boston: D. Reidel Publishing Company, 1969): 460–73. This lecture takes up the theme of the unity of Physics, with emphasis on questions of interest to a wide cross section of physicists. Section 6.3 points to "Quantum Theory" in this volume.

6.1 What Do We Mean by the 'Unity of Physics'?

Permit me to start with a semi-serious query in the sociology of science. Anyone who watches the profusion of specialists at a contemporary scientific meeting will ask himself whether the unity of science is not an empty phrase. Let us therefore submit the following problem to the sociologists of science: find n good physicists, none of whom really understands the specialty of the other; how large can we make n? 100 years ago n was, perhaps, still equal to 1: every good physicist understood all of physics. When I was young, I would have estimated that n = 5. Today n is most likely a not-so-small two-digit number.

I would like to claim, nevertheless, that physics is characterized by a greater real conceptual unity today than at any time in its history. Secondly, I would assume that completing the conceptual unity of physics is a finite task, and that this task will be solved someday in history—a day that might even be close—if mankind does not ruin itself physically or spiritually before then. Thirdly, I would assume that, in one possible meaning of that term, physics will then be completed;

[1] This text was first published as: "A Sketch of the Unity of Physics", in: Carl Friedrich von Weizsäcker: *The Unity of Nature* (New York: Farrar Straus Giroux, 1980), which is a translation of: Die Einheit der Natur, (Munich: Hanser, 1971): II.4.

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in other possible meanings I would consider it incompletable. This hints, fourthly, at a restriction: the thinkable completion of physics as a conceptual unity does not entail the completion or even completability of mankind's spiritual quest for knowledge.

I would like to devote today's lecture to a brief commentary on these four theses, under three headings:

(a) The historical development of physics toward unity,
(b) The unity of physics as a philosophical problem, and
(c) Work program for an attempt at actually constructing the unity of physics in our time.

6.2 The Historical Development of Physics Toward Unity

Sociologists of science must be under the impression that physics develops from unity toward plurality. My first claim appears to say the opposite that it develops from plurality toward unity. But I would prefer to say that physics develops from unity via plurality toward unity. The term 'unity' is used in two senses here: at the beginning, we have the unity of the basic scheme. It is followed by the plurality of experiences that can be understood in terms of the basic conception, indeed whose systematic experimental proliferation is made possible by the basic scheme in the first place. The insights gained in these experiences in turn modify the basic conception, and a crisis ensues. In this phase, the unity seems to be altogether lost. In the end, however, the unity of a new basic conception constitutes itself, which now encompasses in detail the plurality of the newly gained experiences. That is what physicists call a true 'theory'; Heisenberg coined the expression 'closed theory'.[2] A theory is not the unity of the scheme prior to the plurality, but the unity of the verified conception in the plurality.

A development of this sort occurred repeatedly in the history of physics: think of classical mechanics, electrodynamics, special relativity, quantum mechanics; we hope for the same development in the physics of elementary particles. In this process, the earlier theories are modified by the later ones. But they are not really overthrown; rather, their domain of validity is delimited. Within the context of original basic conception and final theory, we can describe this successive self-correction of physics roughly as follows: an older closed theory—classical mechanics, for instance—adequately describes a certain domain of experience. This domain, we later learn, is limited. But so long as the particular theory is all physics can say about that domain of experience, physics simply does not know its borders; the theory does not delimit its own validity. For this very reason, the completed theory serves also as the initial scheme for the opening up of a much

[2] In German 'abgeschlossene Theorie', which means 'closed' as well as 'completed' (*ed.MD*).

wider domain of experiences. Somewhere within this wide domain it then comes up against the limits of what it can grasp with its concepts. Out of this crisis in the initial basic scheme a new closed theory finally arises—special relativity, for example. This theory now includes the older one as a special case, and thereby delimits the accuracy within which the older theory applies in particular instances: only the new theory 'knows' the limits of the old. The new theory in turn is an initial scheme with regard to a still wider domain of experiences, whose borders it might intuit but cannot sharply delimit.

What I have just described is commonly accepted by today's physicists in their methodological reflections; I do, however, wish to parenthetically voice my suspicion that the philosophy of science has not yet developed the concepts required for a description of these structures. Now I will go beyond what is commonly accepted among physicists. My four theses can be contracted into the claim that all of physics, by its very nature, tends toward becoming a single closed theory. If this is true, then all four theses would in fact make sense: (1) Physics is closer to conceptual unity today than ever before, because it more closely approximates its completed, closed form. (2) Achieving this form is a finite task. (3) Beyond this form there will be no further, more encompassing closed theory that, in the present and past sense of the term, would still be called physics. (4) The completed physics will indeed have limits of application; it can, however, as physics, merely suspect and not designate these.

Let us try to concretize these general theses by presenting the concepts with which one hopes to construct the unity of physics.

To the extent that a conceptual unity of physics (including chemistry) has already been achieved in our century, it may reasonably be said to have been constituted by the concept of the atom. I will base my further discussion on this concept. It is important, however, to first understand the changes undergone by the meaning of that term.

The originators of our concept of the atom are said to have been the Greek philosophers Leukippus and Democritus. According to their philosophy, there are two sorts of things: the Full and the Void. The Void we may equate, as a first approximation, with the modern concept of empty space. The Full is then the atoms, which occupy certain subregions of space. Atoms are 'true being' in the sense of the reinterpreted Eleatic philosophy peculiar to the atomic philosophers; i. e., they are uncreated and imperishable, and therefore also indivisible. Variety and changes in appearance are due to differences in the magnitude, form, position, and motion of the atoms.

Plato is the first to offer an essential modification of this conception by proposing its deduction from simpler principles. He imagines the smallest bodily constituents of perceptible things to be the regular ('Platonic') solids, whose faces are the essential element of which they are constituted. The faces are regular polygons, which he constructs out of triangles whose sides (in turn, the elemental constituents of the triangles) are related to each other in particular, permanent number ratios. Number, however, arises from the two basic principles of the One and the Many. Actually, the only real principle is that of the One; for the Many can be known only insofar as we think of it as the One.

We will not be concerned with the details of Plato's construction scheme, since modern physics developed in different directions. But its inherent critique of Democritus is relevant: if there are so-called atoms of different size and form, if the so-called atoms indeed have magnitude and form in the first place, then why are they assumed to be indivisible? We can understand that something is indivisible if it has no parts at all, if its structure excludes divisibility on purely conceptual grounds. But since the volume occupied by Democritus' atom is itself divisible, the atom consists, conceptually at least, of parts; who can guarantee that these will forever cohere? Plato therefore does not call his smallest bodies atoms, since ἄτομον means that which has no parts. He allows their triangular faces to dissociate—for example, during changes in state of aggregation—and to recombine in new ways. I am convinced that he aimed his conception not simply at the construction of bodies in a presupposed space, but at the mathematical structure of space itself.

Modern science, in fortunate philosophical naïveté, took up Democritus' atom concept, which introduced such fruitful order into experience. Step by step, modern physics then sharpened its concepts on the basis of experience, and analysed this model in terms of simpler constituents.

To describe atoms accurately in the era of classical physics, one had to apply classical mechanics to them. This was the general theory of the motion of bodies, based on the four concepts time, space, bodies, and force. Indeed: motion is always the change of something in time. What changes, in classical mechanics, are the positions of bodies in space. Classical mechanics is a theory; i.e., it specifies the laws that govern this motion. What determines the motion in a particular case is called the force acting in that case. The differences between bodies show up in the differences between the forces they exert on each other and (via the constant called mass) in the differences between their reaction to the given forces. What kinds of bodies there can be, what masses and forces therefore really do or could exist, mechanics does not say. Within the context of the mechanistic world view, in which all physical reality consists of bodies (in the sense of that term in mechanics), classical mechanics is already, in a way—namely, as a general scheme of laws—the unified, final physics. This scheme, however, demands its completion through a further theory, which explains what sorts of bodies there really are. Such a theory would be, abstractly speaking, a deductive theory of forces and masses. As an intuitive model of such a theory, the atomic hypothesis offered itself.

This theory was a failure within the context of classical physics, and we can believe today that we understand why it had to fail. Two models of the final building blocks could be tried: they could be regarded either as extended bodies or as mass points. Both models failed because of the insuperable difficulties encountered in the rigorous classical dynamics of the continuum. If the atoms were extended, one could hope to derive the interaction forces from their nature as bodies; i.e., from their impenetrability. But then it remained unclear what forces governed their interior cohesion. Atoms that permit displacements between their parts can absorb internal energy; for atoms whose interior is a dynamic continuum, there can be no thermal equilibrium. Completely rigid atoms, as Boltzmann therefore assumed them to be, appear to beg the question, and we know since special relativity theory that they cannot exist. If the atoms were mass points, then the problem could be shifted to the forces postulated as acting between them. The nineteenth century came to realize—and special relativity here too proved it to be necessarily so—that these forces cannot be acting at a distance, but are fields with an inner dynamic. Planck found that this model, too, because of the continuum dynamics of fields, cannot be in thermal equilibrium.

This crisis in the initial scheme of Democritus' atoms has been resolved, for the time being, in a new closed theory, quantum mechanics. At first, quantum mechanics developed as a new general mechanics; i.e., as a theory of the motion of arbitrary physical objects. By means of the probability concept, it unified the duality of wave and particle. It describes the extended atoms of chemistry as a system of quasi-point elementary particles possessing a finite number of degrees of freedom with a discrete manifold of possible internal states of motion. Thus it avoids, to begin with, the difficulties of the classical continuum dynamics. Its empirical success is without equal.

Like classical mechanics, however, quantum mechanics is only a general theory of arbitrary objects. It therefore seems that it must be complemented by a theory telling us what objects can exist in the first place. This is the theory that, we hope, the theory of elementary particles will someday become. If quantum mechanics is the correct theory of motion for arbitrary objects; if, further, all objects consist of elementary particles; and if, finally, elementary particle theory will deduce all properties (masses and forces) of the elementary particles from a unified set of laws, then it will indeed seem that physics will have been completely unified. This development, which is taking place before our eyes, is what I had in mind when I conjectured at the beginning that the unification of physics might be a finite task, and that the time of its completion might even be near.

6.3 The Unity of Physics as a Philosophical Problem

So long as physics has not achieved a unity that can be recognized as final, no argument taken from the historical development can prove anything for or against the possibility of that unity. The hope that elementary particle theory will unify physics may indeed seem reasonable to us today. However, when Max Planck entered the University of Munich 90 years ago to study physics, his teacher Jolly warned him that no new fundamental insights can be expected in physics. Do we have better arguments than Jolly had? On the other hand: what significance is there in repeatedly false predictions? Was the insomniac right who said, "This is the fourth time tonight that I've awakened without seeing the dawn; I must conclude that it will never get light?" Neither successes nor failures can be reliably extrapolated. But we might perhaps try to raise our level of reflection above physics, and to search not for theories about nature but for a theory about possible theories. We can ask: What would physical theories have to be like if physics is to be, or not be, completable? Much will have been achieved if we realize the embarrassing consequences of either assumption.

We conceive of the conceptual advance of physics as a sequence of closed theories. If physics is completable, one of these theories might be the final one. Should we assume that this final theory has no limits to its validity? But if it has such limits, is there to be no further theory that designates what these limits are? If, however, a theory designating these limits does exist, to what science does it belong if not to physics? Conversely: if physics is not completable, should we suppose an infinite sequence of possible completed theories? Should the supply last to provide mankind for, say, a million years with three discoveries per century ranking in importance with the theory of relativity, quantum mechanics, and the theory of elementary particles? If the supply doesn't even last for that, it surely must be finite.

I do not want to pursue these questions. My attempt merely to face them lies in the assumption that physics is completable, but does have limits of validity. The real service rendered by such a dilemma does not lie in its resolution, unachievable today, but in its teaching us to marvel again at what we had forgotten to marvel about. Difficult though it be to imagine physics either completable or incompletable, it is perhaps even more difficult to imagine that physics should be possible at all.

Indeed, if physics were not known to be a fact, would we dare to prophesy its possibility? Were not those Greeks, and again the scientists of the seventeenth century, unbelievably daring in their hope to contain the overflowing river of phenomena in mathematical structures? If we wish to understand under what conditions physics might be completable, then we should perhaps first ask under what conditions physics is possible. I will ask this question in two steps. First, I will presuppose that physics is in fact possible and will ask why it develops in a sequence of closed theories. Only then will I ask how physics is possible in the first place. You will not expect me to answer either one of these two questions fully. I want to show the trend of these questions, and point out aspects of the answers.

What is a closed theory? I have so far only given examples. Let me now try a purposely rather loose definition. A closed theory is a theory that cannot be improved by small changes. What 'small changes' might be, I will not define; but in any case, we shall call a shift in the value of a material constant a small change, the introduction of entirely new concepts a large one. How can a theory have such extremal properties? I suspect: it can if it is deducible from a few simple postulates. Again I will not define when a postulate is simple; it will at any rate not be called simple if it still contains continuously variable parameters.

The theory of special relativity is a classic example of a closed theory. It follows, within the framework of mechanics and electrodynamics, from the two postulates of the principle of relativity and the constancy of the speed of light. Do not object on the grounds that the speed of light is a continuously varying parameter; it is, rather, if this theory is right, the natural measure of all speeds. An axiomatic analysis of physics—a difficult task that has never been fully carried through—would in this manner, I am sure, reduce all closed theories to simple initial postulates. We must therefore deal with these initial postulates; the remainder is a matter of applied logic.

This brings us to the second question: How is physics possible in the first place? Why can the multiplicity of events be subjected to the consequences of a few simple postulates?

It is important to see, first of all, that the otherwise laudable principle of thought economy does not help at all in this question. The main argument for this assertion is that the fraction of events of greatest interest to us lies in the future. It may be possible to organize economically a completed set of past events. But how do we explain that physics prophesies? How do we explain that physics, repeatedly in the past, correctly predicted events then still in the future, on the basis of events then already in the past? The sharpest formulation of this problem, which I would like to call that of the bottomlessness of empiricism, is due to David Hume. That the sun rose every day until now does not logically entail its rising again. The entailment holds only if I also assume the principle that what happened in the past will under equal circumstances recur in the future. But how do we know that? That the principle proved itself in the past again does not logically entail its doing so in the future. Thus the physicists who had correctly prophesied in the past on the basis of this principle could draw no logically compelling conclusion from their past about their then future experience. The past and the future, Hume concludes, are bridged only by a "belief based on 'custom'". Intellectually honest as he is, he says that the success of this belief can be understood only on the basis of a pre-established harmony between the events of nature and our thinking.

I think, though, that wherever we posit a pre-established harmony, we posit a structural necessity we have not seen through. To find this structure, I now claim, secondly, that Hume's scepticism is confusing because it is incomplete. For instance, Hume clearly believes in logic; a logical connection between past and future, if only there were one, would satisfy him. But what is logic based on? Every logical conclusion at least in some way uses the fact that we can recognize a thing, a word, and a concept as the same after a passage of time; every application of logic to the future tacitly assumes that in the future it will be likewise. But this is just a special case of that constancy of events which Hume considers logically unprovable. The validity of logic in time itself thus appears not to be logically necessary. Neither is it logically necessary that time should exist; e.g. that there should be a future at all, or that the past was really the way we remember it to have been.

Why do I pursue scepticism this far, to apparently absurd consequences? I am certainly not trying to disprove it. Absolute scepticism cannot be put into words, because the use of words still presupposes some confidence. For that very reason, scepticism cannot be refuted. The purpose of the question "What is physics?" thus cannot be the refutation of scepticism. Its purpose can be, however, to bring out how much we have already acknowledged and therefore, though not expressly, presupposed, by acknowledging that there is such a thing as experience in the first place. We, who cannot live together without a certain minimum of confidence in the world and in each other, do not live in absolute scepticism. I daresay that anyone who is alive does not doubt absolutely. Absolute doubt is absolute despair. I do not, in an analysis of physics, explicitly ask the reason for the grace that saved us from absolute despair. I do ask, however, what all of us have always already acknowledged by the mere fact of our living and continuing to live. Clearly we have at least acknowledged that there is such a thing as time, with its three modalities: the current present, the unchangeable past, and a future partly open to efforts of the will and partly open to conjecture. With these concepts Hume himself formulated his sceptical argument; anyone who does not understand them cannot even understand his scepticism. We have acknowledged that there is such a thing as experience, if we mean by this, roughly, that we can learn from the past for the future. We have acknowledged that this learning can be formulated in terms of concepts, that there are therefore things such as—to put it tentatively—recurring events which we can recognize with recognizable words.

Let us call the total of what we thus acknowledge the existence of experience or, for short, 'experience'. Thus defined, 'experience' does not refer to the set of individual experiences but to the structures necessary for there to be individual experiences in the first place. Now I can formulate the central philosophical hypothesis of this lecture: anyone who could analyse with sufficient acuity under what conditions experience is at all possible, would have to be able to show that these conditions already entail all general laws of physics. The physics thus deduced would be precisely the unified physics previously conjectured.

Like any hypothesis, this one too is being advanced without fervour either for or against, as the formulation of a possibility of thought, as a challenge to prove or disprove it. What is being claimed by this hypothesis? What would be true if it were true?

To begin with, the hypothesis acknowledges the fact of experience without asking beyond it. It of course also acknowledges that physics could have been discovered only through the amassing of innumerable individual experiences. But we have just conjectured that the present physics follows from a few simple postulates; we may further conjecture that the content of these postulates becomes less specific the more comprehensive the theory in question is; i.e., the further along it is in the sequence of closed theories. At that point we still did not yet ask whether the contents of these postulates can in their turn be further justified. I now conjecture that these fundamental postulates of the final closed theory of physics formulate nothing but the presuppositions for the possibility of experience in general.

You will have noticed that I am talking here in Kant's language, but that I go beyond Kant's claims. I do this, as I said, hypothetically, in order to bring a problem that has in my opinion been solved neither in Kant's philosophy nor in any other (including empiricism) closer to its solution. It is the problem of how laws are based on experience. Even if, as I am proposing with Kant, we place the validity of the possibility of experience at the head of our theory of physics, Humean scepticism toward each particular induction of a law from a particular experience remains justified: there is no necessity, so it seems, for that experience to repeat itself. General laws, Popper says, are empirical, i.e., falsifiable only through specific cases, and never verifiable. To be precise, by the way, one cannot even falsify them, since the interpretation of each individual experience already presupposes general laws. I can think of only one way to lend as much credibility to general laws as to the fact of experience itself: by seeing them as the preconditions for the possibility of that fact. That this might hold for all laws of physics must have seemed absurd in Kant's time; but if all these laws follow from a few postulates, then perhaps it is not absurd.

Physics will be completable, in the sense of this hypothesis, when and to the extent that the preconditions for the possibility of experience can be listed. Because of this, however, physics will probably be incompletable in another sense, as mentioned in the beginning. It might be that the set of structurally different possible individual experiences is unlimited. The general laws of physics would then admit an unlimited set of possible structures that satisfy them, whose investigation would result in an unlimited realm of problems in concrete physics, and probably also in an unlimited sequence of ever ascending physically possible structures. This is the path that leads, for example, to biology and cybernetics. Furthermore, the concept of experience as I have used it will probably turn out to be too inexact. It might be that the preconditions of one kind of experience—e.g., what we call objectifying experience—can be listed and thus lend themselves to the construction of a completable physics, but that beyond this domain other kinds of experience are already accessible to man, or are waiting to become so.

6.4 Work Program for the Construction of Unified Physics

The hypothesis that unified physics can be deduced from the preconditions of the possibility of experience can be fruitfully discussed only if it is used as a heuristic in the search for unified physics. This is a very large task. I can therefore present no final results, only a work program and certain more restricted hypotheses that suggested themselves to me along the way.

Time—with its threefold modalities: present, future, past—stands at the head of the preconditions of experience. Every physical statement refers, directly or indirectly, to past, present, or future events. In investigations that are unfortunately incomplete, I have occupied myself with the logic of such temporal propositions. Let me cite only one problem from this domain that of the truth values of propositions concerning the future. It does not seem sensible to assign to them the values 'true' and 'false', and I would rather use the so-called modalities 'possible', 'necessary', 'impossible', etc., or their quantification in terms of probabilities. This leads immediately to the fundamental questions of probability calculus, specifically to the question of the empirical (objective) meaning of probability. These questions, which do not belong to physics in the narrower sense, I will skip today, and likewise the connection of the logic of temporal propositions with the usual logic of timeless propositions and with mathematics. I will also skip the connection between this time concept and the Second Law of Thermodynamics, which I have frequently discussed in the past.

The fundamental theories of physics available today, or hoped for, can be roughly divided into quantum mechanics, elementary particle theory, and cosmology.

As the general theory of motion of arbitrary objects, quantum mechanics requires only the fundamental concepts of time and object. All objects are characterized by isomorphic manifolds of possible states; the states of each object constitute a Hilbert space. ‘Motion’ here means, quite abstractly, ‘change of state’. The concept of physical space does not belong to general quantum mechanics. Specific objects are characterized by specific variations of state in time (i.e., Hamilton operators), and only these specific Hamilton operators determine physical space.

The existence of three-dimensional physical space, plus Lorentz invariance, constitutes a fundamental law that, in our division, belongs to elementary particle physics as the theory of actually existing objects. To this must be added the fundamental postulates of relativistic causality, and certain further invariance groups. Heisenberg, in his nonlinear spinor theory, tries to reduce all of elementary particle physics to Lorentz and isospin invariance, and to causality. This program seems full of promise to me. One may hope that, in the end, gravitation too can be built into such a theory.

If quantum mechanics is the theory of arbitrary objects, and elementary particle physics the theory of the actually existing types of objects, cosmology is in turn the theory of the totality of existing objects. Its central problem to date appears to be that of the ‘world model’; i.e., of that solution of the general equations of motion which represents the real world as a whole.

I should think it both necessary and possible to combine these three disciplines —quantum mechanics, elementary particle theory, and cosmology—in a unified argument. In the session on elementary particles, I will sketch a possible approach. Here I will conclude by citing the philosophical reasons that in my opinion make this a plausible undertaking.

The first step in such an undertaking is the justification of the foundations of quantum mechanics. A number of relevant and interesting axiomatic studies already exist. A fundamental concept in quantum mechanics is that of the experimentally decidable alternative or, as one says in physics, the observable. The laws of quantum mechanics govern the probabilities of every possible outcome of every possible measurement of an alternative. From a philosophical point of view, the concepts of alternative and probability require nothing further than a theory of experience would require. Theory means concept, and alternative means empirical decision concerning the appropriateness of concepts to events; probability, as previously indicated, is the mode of possible knowledge of the future. Additional postulates are, to be sure, presupposed by the particular form of the quantum theoretical probability laws. I believe that one needs roughly the following: the existence of ultimate, not further analysable, discrete alternatives (i.e., in effect, the separability of Hilbert space); indeterminism; and certain group theoretical postulates such as homogeneity of time, isotropy of state space, and reversibility of motion. I cannot discuss here the connection between these postulates and the preconditions of experience. I wish only to remark that the group postulates depend, in my opinion, on the very possibility of concept formation; a world that lacks all symmetry surely would not admit the formulation of general concepts.

Secondly, one ought to try to deduce elementary particle physics and cosmology from quantum mechanics. For it seems to me philosophically unsatisfying to first introduce a general theory of motion of arbitrary objects, and then a second theory that designates a subclass of these possible objects as ‘actually possible’. Equally unsatisfying, I feel, is a cosmology that describes the only real existent, i. e., the entire world, as the special solution of a general equation of motion. What meaning could there be in the objects of quantum mechanics excluded by elementary particle physics; or in those solutions of the equations of motion that cannot hold in the world model? I conjecture that elementary particle theory and cosmology are already logical consequences of a quantum mechanics in which one requires the forces themselves to be described as objects; i.e. in the end as fields.

It should then be possible to construct elementary particle theory from the theory of the simplest possible objects permitted by quantum mechanics; these would be the only atoms, in the original philosophical sense of strict indivisibility. Objects of this type would be defined by a single measurement alternative, a yes-no decision. Their quantum mechanical state space is a two-dimensional complex vector space which, as is well known, can be mapped onto a three-dimensional real space. In this mathematical fact, I believe, lies the physical reason for the three-dimensionality of physical space. The so-called elementary particles would then appear as complexes of such ‘archetypal objects’, and would thus be transformable into each other. The symmetry groups of elementary particle physics, as well as the topology of space, then ought to follow from the structure of the quantum mechanical state of the archetypal objects. I am not mentioning this hypothesis in order to announce it as the correct solution, but to show that we have no reason to regard the deduction of elementary particle physics and cosmology from quantum theory as an impossibility.

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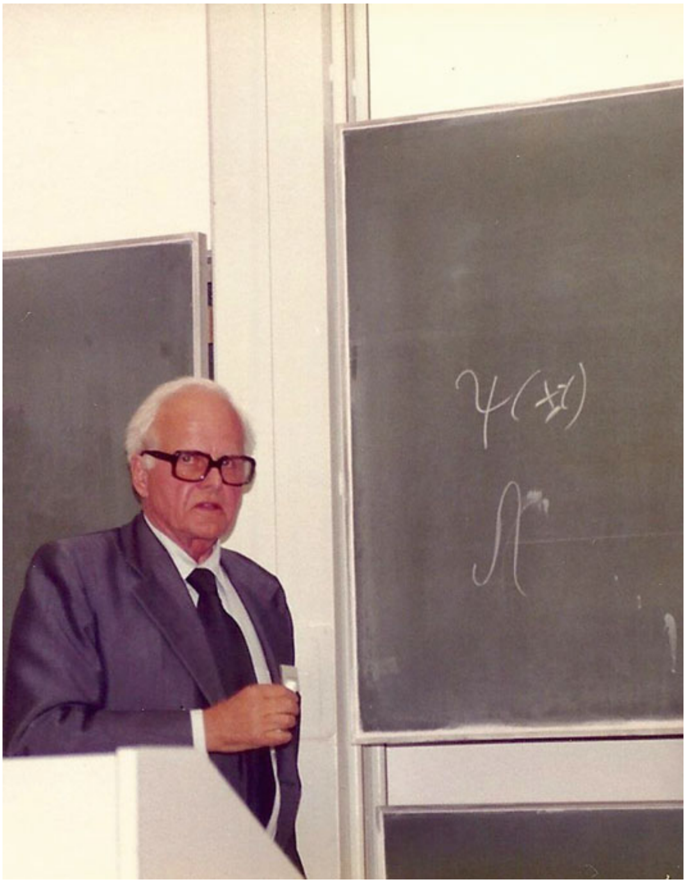

Prof. C. F. von Weizsäcker at a Quantum Logic conference in 1984. © Hans Berner who granted permission for its use in this volume

Chapter 7
Quantum Theory

I come to praise the quantum, not to bury it.

F. Bopp

This essay is the centre of the book[1]; the conciseness of the presentation, which creates difficulties for the critical participation of the reader, seems particularly painful to me here. The essay sums up the contents of several lecture courses, especially one given in Hamburg, in the summer semester of 1965, under the title "Time and Probability in Thermodynamics and Quantum Theory"; I hope to be able to publish a more detailed version within the next few years. The essay was written in English for a conference held in Cambridge, England, in 1968, and published (in two parts) in T. Bastin, ed., *Quantum Theory and Beyond* (New York: Cambridge University Press, 1971). It is intended for readers who are theoretical physicists.

The essay asserts that quantum theory, consistently interpreted, constitutes not merely a general framework for physics, but is in a certain sense already the unified physics itself. The problem is presented in the first two sections; we discuss, in the light of present knowledge, the interpretation of quantum theory and its systematic place among the four other fundamental theories of physics. The third section makes the methodological point that in quantum theory we are dealing with logical problems not studied so far—namely, with the logic of temporal propositions. In my opinion, it is only this logic that makes a consistent theory of objective probability possible. Section d presents an axiomatic construction of quantum theory as a general theory for the prediction of empirically decidable alternatives. The construction deviates from the usual quantum theory in one point, which I designate with the term 'finitism.' Its epistemological interpretation is that only a finite number of alternatives can ever be decided; its mathematical consequence is the use of finite-dimensional Hilbert spaces. Section e sketches a unified structure for all of physics. Its point of departure is the realization that a finitistic quantum theory directly implies a cosmology, a theory of elementary objects, and a connection between the two.

[1] This text was first published as: "Quantum Theory", in: von Weizsäcker (1980).

M. Drieschner (ed.), *Carl Friedrich von Weizsäcker: Major Texts in Physics*, SpringerBriefs on Pioneers in Science and Practice 22,
DOI: 10.1007/978-3-319-03668-7_7,

Several of the subsequent essays resume these problems from philosophical points of view. III.3[2] discusses the relation between mind and matter in connection with the Copenhagen interpretation of quantum theory. III.5[3] interprets the unity of substance in terms of the unity of matter, motion, and form based on time, and, in III.5e, directly connects with the theory of 'ur-alternatives' in section e of the present essay. IV.6[4] examines the limit of the quantum theoretical notion of an object against the background of Bohr's complementarity principle and Platonic dialectics.

7.1 The Copenhagen Interpretation

One can think of two reasons for wishing to go beyond quantum theory: dissatisfaction with the present status of the theory and a general belief in further progress. I shall treat the first reason in this section, the second one in the ensuing section, and the remaining three sections will be devoted to the consequences of this discussion.

There is no known reason to be dissatisfied with the success of quantum theory, with the possible exception of the difficulty in reconciling it with relativity in a quantum field theory of interacting particles. This difficulty, however, belongs to a theory as yet unfinished, and physicists may have good reason to believe that it will disappear with the completion of elementary particle physics; I shall briefly return to it in the last section. Thus the mathematical formalism of quantum theory and its usual mode of application to practical problems do not seem to need improvement.

On the other hand, considerable dissatisfaction has been expressed over the last decades with what is generally called the Copenhagen interpretation of quantum theory. I would prefer to call it the Copenhagen interpretation of formalism, an interpretation by which formalism is given a clear enough meaning for it to become part of a physical theory. In so saying, I express the opinion that the Copenhagen interpretation is correct and indispensable. But I have to add that the interpretation has, in my view, never been fully clarified. It needs an interpretation itself, and only this interpretation will be its defence. This situation should not seem surprising, however, once we realize that we are here concerned with the basic problems of philosophy. To use a simplifying example: one should not expect it to be easier to understand the atom than to understand numbers; and the

2 "Matter and Consciousness" (Chap. 10) in this volume—*ed.MD*.

3 "Matter, Energy, Information" (Chap. 11) in this volume—*ed.MD*.

4 "Parmenides and Quantum Theory" in: Drieschner (2014).

battle between logicism, formalism, and intuitionism over the meaning of numbers is so far by no means decided.

In the present chapter I cannot treat this question within its full philosophical horizon. I shall only point out what I consider to be the crucial point in the Copenhagen interpretation. The point is eyed, but not very happily expressed, in Bohr's famous statement that all experiments are to be described in classical terms. Correctly interpreted, this statement is, I maintain, true and essential; its main weakness is the insufficient elucidation of the meaning of the term 'classical'. Its clarification is as difficult as it is necessary because Bohr's statement implies an apparent paradox: classical physics has been superseded by quantum theory; quantum theory is verified by experiments; experiments must be described in terms of classical physics. The paradox will be resolved only by stating it as pointedly as possible.

It will serve our purposes if we first state that Bohr was not a positivist and explain why. A few years ago E. Teller reminded me of two remarks Bohr made when we were working together at his institute. One of them was made after Bohr had addressed a congress of positivist philosophers. Bohr was deeply disappointed by their friendly acceptance of all he said about quantum theory, and he told us: "If a man does not feel dizzy when he first learns of the quantum of action, he has not understood a word." They accepted quantum theory as an expression of experience, and it was their *Weltanschauung* to accept experience; Bohr's problem, however, was precisely how such a thing as a quantum of action can possibly be an experience.

The Copenhagen interpretation is often misrepresented both by some of its adherents and by some of its adversaries as stating that what cannot be observed does not exist. This is logically inaccurate. What the Copenhagen interpretation requires is only the weaker statement: "What is observed certainly exists; about what is not observed we are still free to make suitable assumptions." This freedom is then used to avoid paradoxes. Thus Heisenberg's discussion of thought experiments with respect to the uncertainty principle is no more than the refutation of an accusation of inconsistency. If we accept the formalism of quantum theory in its usual interpretation, we have to admit that it does not contain states in which a particle has both a well-defined position and a well-defined momentum; the exclusion of these states seems[5] essential in order to avoid contradictions with the probability predictions of the theory. The objection is raised that both position and momentum can be measured, hence they exist. This objection is countered by the statement that *if* quantum theory is correct, position and momentum cannot be measured at the same time, and hence the defender of quantum theory cannot be forced to admit that they exist at the same time; i.e., in the same state of the particle. Thus in Heisenberg's view it is only the positive knowledge of the state of

[5] I here leave aside the question of whether the contradictions might be avoided by theories like David Bohm's. We are at this point concerned with the consistency and not with the uniqueness of the Copenhagen interpretation.

a particle that excludes the rejected states—states which then, as consistency would lead us to expect, also turn out to be unobservable. Heisenberg made the simple but relevant comparison: we did not reject the theory that the earth's surface is an infinite plane merely because we were unable to go beyond certain bounds, but because we were fully able to go around the earth and thereby to prove that it is a sphere. Admittedly, this comparison does not express the fact that quantum theory rejects not particular models but the very concept of 'intuitive' models altogether.

These last remarks on the difference between the Copenhagen interpretation and positivism were logical and negative. The difference is formulated in an epistemological and positive manner in Bohr's statement on classical concepts. At least the more naive positivist schools have held that there are such things as sense data, and that science consists in connecting them. Bohr's point is simply that sense data are not elementary entities; that what he calls phenomena are given, rather, only in the full context of what we usually call reality, a context that can be described by means of concepts; and further, that these concepts fulfil certain conditions which Bohr took to be characteristic of classical physics. The second Teller anecdote may be appropriate here. Once at afternoon tea in the Institute, Teller tried to explain to Bohr why he thought Bohr was wrong in thinking that the historical setup of classical concepts would forever dominate our way of expressing our sense experience. Bohr listened with closed eyes and at the end merely said: "Oh, I understand. You might as well say that we are not sitting here, drinking tea, but that we are just dreaming all this."

Bohr expressed his point here without proving it. What are the conditions to be fulfilled by sense experience, and why should they be so unalterable?

Bohr used to speak of two conditions: space-time description and causality. They go together in the objectified model of events offered by classical physics, but they are broken asunder by the discovery of the quantum of action, and reduced to complementary means of description. If a physical system is to be used as an instrument of measurement, it must be describable both in the space and time of our intuition and also as something functioning according to the principle of causality. The first condition means that we are able to perceive it at all, and the second that we can draw reliable conclusions from its visible properties (like the position of a pointer on a scale) about the invisible or dimly visible properties of the object that we observe by it. If Bohr is right in saying that space-time description and causality go together only in classical physics, his view that a measuring instrument must admit of a classical description seems inevitable.

Before asking the next question I should like to point out how this analysis distinguishes Bohr's view from positivism. To him, what can be described classically is a 'thing', in the common-sense meaning of the word. The fact that classical physics breaks down on the quantum level means that we cannot describe atoms as 'little things'. This does not seem to be very far from Mach's view that we should not invent 'things' behind the phenomena. But Bohr differs from Mach in maintaining that 'phenomena' are always 'phenomena involving things', because otherwise the phenomena would not admit of the objectification without which there can be no science of them. For Bohr, the true role of things is that they are not ‘behind’ but ‘in’ the phenomena. This is very close to Kant’s view that the concept of an object is a condition of the possibility of experience; Bohr’s dichotomy of space-time description and causality corresponds to Kant’s dichotomy of the forms of intuition and the categories (and laws) of reason, which only in cooperation make experience possible. The parallelism between the two views is the more remarkable since Bohr does not seem ever to have read much of Kant. Bohr differs from Kant in having learned from modern atomic physics the lesson that science extends beyond the realm in which we can meaningfully describe events in terms of properties of objects considered independent of the situation of the observer; this is expressed in his idea of complementarity. Bohr differs from Mach even more radically in denying that sense data are distinct from what can be observed in things. Since his positivist audience did not understand that, they disappointed Bohr by their easy-going acceptance of the quantum of action. It is typical of this attitude that it has no real use for the idea of complementarity.

The next question is how Bohr knew that our space-time intuition is unalterable and that space-time description and causality go together only in classical physics. My proposed answer is that Bohr was essentially right but that even he himself did not know why. This is indicated, I think, in the ways in which he expressed himself. He never stated in so many words that space-time intuition cannot change and that the set of classical laws (say, Newton’s mechanics and Maxwell’s electrodynamics) comprised what was necessary and sufficient for objectification; yet he expressed himself in a manner that would seem indefensible if these two statements were wholly wrong. I should like to offer a hypothesis of my own on the question.

Classical physics is a very good approximation but not an exact description of phenomena. Perhaps before stating my hypothesis I should stress that not only is classical physics refuted empirically, but it also has or had a very scant a priori chance of being an exact theory of phenomena, at least if it is to embrace both thermodynamics and an account of the continuous motion of continuous bodies. Planck’s ultraviolet catastrophe would probably have shown up in any sufficiently elaborate classical thermodynamics of continuous motion. (I think, by the way, that this difficulty would turn up in any attempt to undermine quantum theory by a classical theory of hidden parameters.) Having thus to accept the essential falsity of classical physics, if taken literally, we must ask how it can be explained as an essentially good approximation. This amounts to asking what physical condition must be imposed on a quantum theoretical system in order for it to show the features we describe as ‘classical’. My hypothesis is that this is precisely the condition that it should be suitable as a measuring instrument. If we ask what this presupposes, a minimum condition seems to be that irreversible processes must take place in the system. For every measurement must produce a trace of what has happened; an event that goes completely unregistered is not a measurement. Irreversibility implies a description of the system in which some of the information we think of as being present in the system is not actually being used. Hence the system is certainly not in a 'pure state'; one describes it as a 'mixture'.[6] I am unable to mathematically prove that the condition for irreversibility would suffice to define a classical approximation, but I feel confident that it is a necessary condition.

If the hypothesis is correct it might show that Bohr, far from stating a paradox, instead stated a truism, but a philosophically important one: a measuring instrument must be described by means of concepts appropriate to measuring instruments. It is then not unnatural to assume further that classical physics, in the form in which it developed historically, simply describes that approximation to quantum theory which is appropriate to objects, to the extent that objects really can be fully observed. If we accept this we have to conclude that, at least to the extent that quantum theory itself is correct, no further adaptation of our intuitive faculty to quantum theory is needed or possible. A mind that observes nature by means of instruments themselves described classically cannot possibly adapt to the actual laws of physics (i.e., to the quantum laws) other than by describing nature classically. To ask for another description of phenomena would be to ask for an intrinsic impossibility—unless quantum theory is wrong after all.

This goes as far as I feel able to interpret the Copenhagen interpretation. One would certainly wish to understand why nature should obey, of all things, the laws of quantum theory that were accepted in this section as given. This will be one of the guiding questions in the following section. First I would like to explicitly formulate a methodological principle that I have used implicitly in this section. I propose to call it the *principle of semantic consistency*.

A mathematical formalism such as, for example, Hamilton's principle with its mathematical consequences, Maxwell's equations with their solutions, or Hilbert space and the Schrödinger equation, is not eo ipso a part of physics. It becomes physics by an interpretation of the mathematical quantities used in it, an interpretation one might call physical semantics. This semantics draws on a previous, though incomplete knowledge of the phenomena that we hope to describe more accurately by the formalized theory. Thus we know beforehand what we mean by a body, a length, or, for the later theories, by a field of force or an observable entity like energy, momentum, etc. Since the theory ascribes mathematical values to quantities like length, force, and momentum, it is even necessary that we possess prior knowledge of the practical methods of measuring these quantities. But then in many cases the measuring instruments will themselves be objects of a description in terms of the new theory. This possibility submits the whole theory

[6] Wigner (1936: 6) has objected to the description of the measurement process in terms of mixtures by claiming that not even the unitary (i.e., quantum mechanically admissible) transformation of one mixture into another can increase the entropy; thus it cannot describe the irreversible aspects of the measurement process. This is true, but it is not an objection. Even in classical physics the increase in entropy is not an 'objectively' describable event. Stirring an incompressible liquid colored white and red in equal parts (Gibbs) does not result, objectively speaking, in pink regions, but only in multiply entwined borders of white and red; if the borders are sufficiently entwined, *we* can no longer follow them and we see pink. Analogously in quantum theory, no unitary transformation of a mixture can correspond to an irreversible process; instead, our description must 'jump' to a mixture of higher entropy.

(i.e., the formalism plus its physical semantics) to an additional condition, namely the condition of semantic consistency: The rules by which we describe and guide our measurements, and which thus define the semantics of the formalism, must be in accordance with the laws of the theory; i.e., with the mathematical statements of the formalism as interpreted by its physical semantics.

It is by no means trivial that a new theory will automatically fulfil this condition. Thus Einstein's famous analysis of simultaneity was an analysis of the semantic consistency of a theory that obeyed the mathematical condition of Lorentz invariance; it showed that space and time had to be reinterpreted in order to reconcile everyday experience with the theory. The quantum theory of measurement aims at a proof of the semantic consistency of general quantum theory. So far no theory in physics has ever been fully submitted to the test of semantic consistency, for all theories accept as given certain phenomena that they do not explicitly describe. Thus quantum theory accepts that there are objects which are in the nature of particles, a fact that will perhaps in the end be explained by elementary particle theory.

7.2 The Unity of Physics: Part One

Is there a way that leads beyond the mere acceptance of quantum theory as an empirically well-established set of laws? We would like a better understanding either of its necessity or of its possible future alterations. For both purposes it may be useful to ask what further progress in physics can be expected with some degree of plausibility.

Heisenberg has described the past progress of theoretical physics as a series of distinct 'closed (*abgeschlossene*) theories'. In sharp contrast to the steady accumulation of empirical data and of their explanation by existing well-established theories, the basic theories seem to advance in infrequent large steps or jumps. The decisive step forward is certainly historically prepared, but in many cases not with the accompanying feeling of growing clarity but rather with the increasing awareness of some unresolved riddles. This historical phenomenon is most clearly seen in the years preceding the formulation of special relativity and quantum mechanics. A closed theory is generally characterized by an intrinsic simplicity, but no methodology of science has so far been able to clearly define what we mean by 'simplicity' in such a statement. In any case, closed theories show a remarkable ability to answer those questions that can be clearly formulated within their conceptual framework, and to give their followers the feeling that the questions that cannot be thus formulated may be altogether meaningless. In the historical sequence of closed theories, the later ones usually reduce their predecessors to some 'limited' or 'relative' truth, assigning them the role of approximations or limiting cases. Thus we have learned to speak of the field of applicability of a theory, the limits of which are not known in the beginning and are clearly defined only by the later theories.

One of the most important tasks of an epistemology of science is to explain why theoretical progress should have this particular form. I venture the hypothesis that any good, i.e., widely applicable, theory will be deducible from a very small set of basic assumptions. A possible description of the feelings of good physicists about a 'good theory' is that it does not admit of minor improvements. If a good theory follows from very few qualitative postulates, this would be expected; the only possible changes in such a theory are changes in the basic postulates, which will be felt to be 'great' changes. This hypothesis presupposes that a successful theory in physics stands under very severe constraints. Logical and mathematical consistency is only one of them; semantic consistency may turn out to be the most severe one.

We can find a content for this methodological scheme by looking at the present situation in theoretical physics. Perhaps one can provide an orderly summary of our present knowledge and our present expectations by saying that five interlinking fundamental theories either exist or are being sought; the question of how they interlink will be our main problem. They are:

(1) a theory of space-time structure (special or perhaps general relativity),
(2) a general mechanics (quantum theory),
(3) a theory of the possible species of objects (elementary particle theory),
(4) a theory of irreversibility (statistical thermodynamics), and
(5) a theory of the totality of physical objects (cosmology).

The list excludes theories of special objects like nuclei, atoms, molecules, wave fields, stars, etc., which, at least in principle, can be deduced from the fundamental theories. We are inclined today to consider 1, 2, and 4 as more or less final, while much work is being done in order to find 3 and perhaps 5.

In contradistinction to earlier theories, these current theories no longer depend on particular fields of experience such as sense data (optics, acoustics, etc.), moving bodies (classical mechanics), or fields of force (electrodynamics). They seem to arrange themselves like parts of a systematic unity of physics that is as yet dimly seen. We may try to express the principle of this unity by saying: There are objects in space and time. Hence an account of space and time must be given (1). 'Being in space and time' means for an object that it can move. Hence there is a set of general laws that govern the motion of all possible objects (2). All objects can be classified in more or less distinct species; were it not so, no general concepts of objects could be formed and there would be no science. Hence there must be a theory telling what species of objects are possible (3). This theory describes objects as composites of more elementary objects. The composition can be described in detail, leading to the higher species (atoms, molecules, etc.). It can also be described in a statistical manner (4). All objects of which we know somehow interact, for otherwise we would not know about them. Hence some theory concerning the totality of all existing objects may be needed (5).

This is only a preliminary account of a possible unity of physics. Its shortcomings are seen when we more closely analyse the interlinkage of the theories and the problems connected with the concepts I used in the description.

(1) The space-time structure interlinks with the four other fields in a rather puzzling way. (1 and 2) Quantum theory, in the only form in which it is available to us, presupposes time but does not presuppose space; it describes only the manifold of the possible states of any object in terms of the highly abstract concept of Hilbert space. (1 and 3): According to general relativity, the space-time structure is described by gravitation, which, on the other hand, seems to be a field that one would like to deduce from elementary particle theory. (1 and 4) Thermodynamics deals not only with statistics but also with irreversibility, which seems to be a feature of time. (1 and 5) Cosmological topology is itself a theory of the space-time structure, which in turn perhaps depends on the gravitational field.

(2) The concept of a physical object (or 'system') presupposed by quantum theory is replete with problems. An object seems to be an object for a subject (for an observer of phenomena); the fact that the observer is himself part of the objective world is accepted as fundamental in the Copenhagen interpretation, but is not objectively described by any of our five theories.[7] If we leave this question aside as too philosophical, we still encounter conceptual problems in the use of the idea of an object. The concept of an isolated object is only an approximation, and, according to quantum theory, a very bad one. The rule of composition states that the Hilbert space of a composite object is the tensor product of the Hilbert spaces of its parts. This implies that only a set of measure zero out of the states of a composite object can be described by assigning definite states to its parts. Yet in any concrete instance we formally describe the largest objects under consideration as if they were isolated (or, what amounts to the same, as if they were in a stationary environment). (2 and 5) It seems quite speculative to describe the entire universe as a single quantum mechanical object that might have a defined state vector.[8] (2 and 4) If my analysis of the Copenhagen interpretation was correct, for the specification of actual quantum states we rely on measuring instruments in whose description, since we consider their functioning irreversible, we explicitly relinquish the precise knowledge of their quantum state; this seems to mean that we draw on thermodynamics in order to give meaning to quantum theory.

Other puzzles arise if we analyse the usual way (which I tried to condense in the above description) in which the connection between theories 2, 3, and 5 is expressed (2 and 3): I described quantum theory as stating the general laws of motion for all possible objects, and elementary particle theory as hoping to describe all possible *species* of objects. What does this distinction mean? Either quantum theory and elementary particle theory will in the end turn out to be coextensive and, in that case, probably identical; or there will be objects that would be possible according to general quantum theory but that are excluded by the additional information of elementary particle physics. The second alternative expresses the conventional view. But then the quantum theory of the 'rejected' objects turns out to be physically meaningless; should we retain it at all? The

[7] Cf. Parts iii and iv of von Weizsäcker (1980).

[8] Cf. iv.6 of von Weizsäcker (1980).

distinction between universal laws and individual objects is meaningful so long as the presence of these particular objects (and of no others) is taken to be contingent, i.e., as not following from a universal law; but if there are laws that restrict possible objects to certain classes, what is the empirical meaning of more general laws? Hence I would tend to consider the first alternative as a real possibility. It might turn out that quantum theory can be semantically consistent only if Hilbert space is described by using a system of basis vectors that correspond to 'possible species of objects' in the sense of elementary particle theory. I shall resume this question in Sect. 7.5.

(1, 2, 3, and 5) Our usual way of speaking of cosmological models is perhaps even more conspicuously awkward. We first formulate a general equation; e.g., Einstein's equation of the metric field. Then we select one of its solutions and state: this one (hypothetically) describes the whole world. What, then, is the empirical meaning of the other solutions, and hence of the whole equation? Certainly the equation applies meaningfully to various situations within the world. Its initial and boundary conditions specify the particular contingent situation that is to be described by an appropriate solution of the equation. But is it meaningful to subsume the unique totality of being under a 'general' law? We are here approaching the ancient puzzle of Leibniz's concept of 'possible worlds', and the more recent riddle of Mach's principle.

(1, 3, and 5)[9] has shown that a Lorentz-invariant field theory of gravitation will admit of a gauge transformation that yields observable space-time coordinates with a Riemannian metric, and a gravitational field obeying Einstein's equation. Thus the observable space-structure's connection with gravitation is explained or rediscovered, and Einstein's approach of beginning with a Riemannian metric is justified. But what cannot be changed by the gauge transformation is the topology of space-time at large; i.e., cosmology. If we start in a Minkowski space, cosmic space will remain open in the final description; if we wish to get a closed Einstein universe, we must start with a field theory of gravitation in some closed space. This fact indicates that cosmology might not be a consequence of gravitation. Cosmic topology might be otherwise determined, in which case matter and gravitation (curvature of space) would be left to adjust themselves to the given boundary condition.

(2 and 4) I have already alluded to a difficulty in the relation between quantum theory and thermodynamics. We usually think of irreversibility as a secondary effect superimposed on essentially reversible basic laws, and as explicable by means of classical statistics; i.e., by our lack of knowledge. On the other hand, in section a, I used irreversibility as a precondition of measurement and hence of the semantics of quantum theory (or of any other theory that rests on observation). (4): In order to understand this apparently vicious circle, we must first remove a flaw in

[9] See Thirring (1959: 79).

the usual description of irreversibility.[10] Boltzmann's H-theorem proves that a closed system which at a time $t = t_0$ is in a state of non-maximal entropy will, with great probability, be in a state of higher entropy at a time $t > t_0$—that is, at a time that at to is still in the future. That proves the Second Law for the future, but not for the past. If we apply the very same consideration to a time $t < t_0$, we will of course find that with the same probability the system will be found to be at a higher entropy level at t than at t_0. Yet we know the Second Law empirically, which means from the past. Hence this naive application of the H-theorem does not prove the *known* Second Law at all; rather, it contradicts that law. The apparent paradox is removed by noting that the concept of probability in the particular meaning in which it is used here can be applied only to the future and not to the past. The future is 'possible', i.e., it is essentially unknown, and we can ascribe objective probabilities to its events. The past is 'factual', it can in principle be known and it is known in many cases; the probability of a past event means only the subjective lack of knowledge. I shall return to the logical structure of this argument in section c. If we accept this solution of the paradox, we can proceed to prove the Second Law for the past by remarking that every past instant was once a present instant. At that time the concept of probability could be meaningfully applied to what was then still in the future, leading to the correct prediction that entropy would increase.[11] (4 and 5) Boltzmann[12] tried to avoid this explicit use of the concepts of present, past, and future by proposing that the infinite universe is, on the average, in a state of statistical equilibrium, but contains regions in space and time where there are fluctuations. He thought that we live in a large fluctuation; that living organisms are possible only in such a fluctuation; and that these organisms "will always use one set of labels for the direction of time toward the more improbable states (the 'past', the 'beginning') and another set for the opposite direction (the 'future'). As a result of this labelling, a living organism will always view such a small region isolated from the universe as having been in an improbable state initially." This argument breaks down, however, in view of a lemma by T. Ehrenfest.[13] They show that any non-maximal entropy value of a system which, on the average, is in statistical equilibrium, lies with overwhelming probability not on the slope of a larger fluctuation but represents a minimum value; i.e., the peak of a fluctuation. (This statement is precise for discrete entropy changes; suitably modified, it also applies to continuous changes.) The Ehrenfest lemma is in fact required in the proof of the H-theorem. Applied to Boltzmann's idea, the lemma says that if we live in a fluctuation, it is overwhelmingly more probable for the present state, with all its apparent traces, fossils, and documents of the apparent past, to be merely an extreme fluctuation than for such a past,

[10] Cf. ii.2. of von Weizsäcker (1980), *identical with* "The Second Law …" (Chap. 5) in this volume—(Ed. MD).

[11] Cf. ii.2 and von Weizsäcker (1949).

[12] See Boltzmann (1964).

[13] See Ehrenfest (1959).

implying even lower entropy values, to have really existed. That, I think, means a *reductio ad absurdum* of Boltzmann's proposal, and thus an indirect justification of the use of the 'modalities of time' in physics.[14]

(1 and 4) These considerations would suggest that the Second Law is not fundamental in itself and cannot be deduced from reversible laws, but that, instead, it derives from a more fundamental structure of time that we express in speaking of the present, past, and future. This structure, then, must be a presupposition of the meaningful use of the terms of thermodynamics, as well as of those of quantum theory. It is neither described by special relativity nor contradicted by it (the distinction between past and future events for a given observer is Lorentz-invariant).

In trying to put together the pieces left in our hands by this critical analysis, we may guess that a fundamental unity lies behind the existing parts of the five theories. This unity might merely be hidden by the naive way in which we still use unclear but necessary concepts like space, time, object, measurement, probability, and universe. If this unity will again be expressed by a 'closed theory', we may wonder what its basic postulates will be. Thus we return to epistemology for a moment.

Epistemology has a tendency to lag behind the actual development of science. One of the much discussed problems, for example, is how we can establish empirical laws. That was a meaningful question at a stage of physics in which many apparently independent laws were brought forward as hypotheses and refuted or more or less validated by empirical evidence—including, e.g., the laws of Boyle and Mariotte, of Coulomb, of Ampere and others. Thus it was a quite meaningful question in the days of David Hume and Immanuel Kant. But in present-day physics these laws are no longer independent. We accept them as necessary once we have accepted the basic theories from which they follow. And so today the only meaningful question is how we establish basic theories. If the unity of present-day physics should in the end turn out to be embodied in one closed theory, the only remaining question would be how to establish (to derive, to confirm, or whatever expression you prefer) the basic postulates of that theory.

Now the epistemological question of whether one can establish a strict law by particular experience has, I think, been answered in the negative. This was known to Plato as well as to Hume. Popper correctly points out that a universal law cannot be verified by an enumeration of empirical instances. I think he is mistaken in believing that it can at least be falsified by counter-instance. For an empirical phenomenon to be counter-instance to a law we always need some theoretical interpretation of the observed phenomena; i.e., some laws. If these laws cannot be strictly verified in turn, it will be impossible to use them for a strict falsification of another law.[15]

[14] Cf. Böhme (1966).

[15] Cf. i.6d, iv of Weizsäcker (1980); cf. "Parmenides and Quantum Theory", in: Drieschner (2014).

I can think of only one justification of general laws vis-à-vis experience; i.e., a justification that isn't dogmatically aprioristic or doesn't simply beg the question. It is given in Kant's idea that general laws formulate the conditions under which experience is possible. We will still not know such laws to be necessary in themselves, for we do not know it to be necessary that experience should be possible. But they will hold to the extent that experience is possible, and hence they will have to be admitted by anyone who is prepared to accept empirical evidence. The question arises whether the basic assumptions of a unified physics might turn out to be the very assumptions that are necessary if there is to be experience.

7.3 Tense Logic

A possible definition of experience is that it means to learn from the past for the future. Any experience I now possess is certainly past experience; any use I can still hope to make of my experience is certainly a future use. In a more refined way one may say that science sets up laws which seem to agree with past experience and which are tested by predicting future events and comparing the prediction with the event when the event is no longer a possible future event but a present one. In this sense, time is a presupposition of experience; anyone who accepts experience understands the meaning of words like 'present', 'past', and 'future'. Grammatically speaking, he understands the meaning of tenses (i.e., of the modalities of time). Yet language is not the decisive factor here; if he speaks a language that does not express tenses in a simple manner, he still will understand the difference between yesterday and tomorrow, between what has happened and what will (or will not) happen.[16]

I will attempt to find out how much is already implied by the postulate that physics, if it is to be an empirical science, must refer to time. I shall formulate the postulate in the most general way by listing some basic ideas of a logic of temporal propositions; i.e., a tense logic. This logic will turn out to be closely connected with what has been called quantum logic.

J. von Neumann first proposed comparing projection operators in Hilbert space with propositions, and their eigenvalues 1 and 0 with the truth values 'true' and 'false'. Thus, in an *eigen state* of the operator, the corresponding proposition has the corresponding truth value; in other states it does not have a well-defined truth value but merely a 'probability of turning out to be true'. This logic is non-classical. The corresponding lattice of propositions is not Boolean; it is isomorphic with the lattice of the subspaces of Hilbert space; i.e., with a projective geometry.

Von Neumann had good reasons for using this logical language. Probability is a fundamental concept in quantum theory. If one inquires what laws the quantum

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[16] Cf. i.4. of von Weizsäcker (1980)—(*ed.MD*).

mechanical probabilities obey, the first question one will ask is whether they conform to Kolmogorov's axioms of probability. These axioms introduce probability as a real function defined on a Boolean lattice of what is called possible events. That events should form a Boolean lattice follows from classical logic, since two events can be connected by 'and' and 'or'. In quantum theory what is changed does not seem to be the set of ensuing axioms but the lattice to which they are applied; this in turn suggests a change in the underlying logic.

Yet there is an apparently strong argument against the possibility of a quantum logic. This logic seems to be derived from quantum theory—that is, from particular experience. Quantum theory, however, has been built up using classical mathematics; experience itself does not seem possible without the use of logic and mathematics. Mathematics rests on classical (or possibly intuitionist) logic. How, then, can quantum theory justify a logic that differs from the logic on which it is founded? To this argument there are two successive replies, one of which is a defence, the other a counterattack.

The defence says that classical logic (under which I here, for the sake of brevity, subsume intuitionist logic) refers to one type of propositions, quantum logic to another type. Classical logic is to be accepted for 'timeless' statements, such as 'two times three equals six'.

Quantum logic refers to 'contingent' statements, such as 'there is an electron at x'. If a timeless statement is true or false, it is true or false 'forever', without respect to time. Contingent statements, formulated with respect to the present, can be true now, false at another time. One can at least consider the possibility that these two different types of statements will obey different logical laws.

The counterattack says that tense logic, of which quantum logic is merely a special formulation, is not a result of particular experience but a presupposition of all experience. Quantum theory has only made us aware of logical distinctions we were allowed to neglect in classical science. This is analogous to the contention that, while Brouwer was historically later than Russell, Russell's paradox made it clear that one ought always to have thought more carefully about infinite sets. But this counterattack puts us under the obligation to actually show what logic is appropriate for contingent statements. I can mention only the main points here.[17]

I shall use a 'reflexive' method which corresponds to Bochenski's assertion that "the true logic is metalogic". Not just any formal system that resembles the usual logic is to be called a logic, but only those systems that formulate rules according to which we really argue, when we try to really convince one another. In particular, I follow the method of Lorenzen, which he used in developing the logical laws as the rules according to which one can win a discussion game in a fair dialogue.

As an example I offer Lorenzen's[18] proof of the implicative law of identity, $A \rightarrow A$. If one partner in the dialogue, called the proponent, is prepared to defend

[17] This tense logic is formally different from the existing systems of which Prior has given a thoroughgoing account: Prior (1957/1967).

[18] See Lorenzen's (1962).

a proposition of the form A → B ('if A, then B', or 'A implies B'), he in effect offers the opponent the following game: If the opponent is able to prove A, then the proponent will prove B. If the opponent cannot or does not prove A, then the proponent is not obliged to prove anything. The game is meaningful only if the partners agree beforehand on what they are prepared to accept as a proof of a 'primitive statement' of the sort for which A and B stand as symbols. If A and B are arithmetical propositions, the proof can be an arithmetical construction; if they are well-formed expressions of a formal system, the proof can be their derivation from some accepted initial expressions. Lorenzen's view is that the validity of logical laws will depend not on what sort of proof is agreed upon, but only on the existence of such an agreement. Now assume the proponent proposes to defend the universal law A → A. For the law to be universal, he must permit the opponent to insert for A whatever proposition he wishes. Let the opponent insert some particular proposition a (e.g., '$2 \times 3 = 6$'). The proponent asks him to prove a. Now the road forks. Either he does not give a proof. In which case the proponent is not obliged to do anything. Or the opponent gives a proof of a. The proponent is then obliged to prove what follows by inserting this a for A; that means he is obliged to prove a. He does this by taking over the opponent's proof. Thus he wins the game no matter what substitution for A the opponent may propose and, what is more, he knows in advance that he will always win, since he knows a winning strategy. This strategy (to take over the opponent's proof) is the 'meaning' of the law A → A.

Now let us repeat the game for contingent propositions referring to the present. For A we insert m, an abbreviation for 'the moon is shining'. How can we prove a statement about the present? The simplest and fundamental proof is by inspection: 'I show you the moon, please look at it'. The game now seems to follow the same pattern as before. The proponent offers to defend A → A. The opponent inserts m. Asked to prove m he says: "Here, look at the moon, just above the horizon!" The proponent admits the proof. Asked now to prove m, he says: "Here, look at the moon, just above the horizon!" The opponent has to admit the proof, which means to admit defeat.

Yet the proponent must, in this example, be careful to react fast enough. Otherwise the opponent who just before showed him the moon may deny the validity of the second proof: the moon has set in the meantime. If such things can happen, it is not self-evident that all laws of classical logic can be reproduced for contingent statements referring to the present. Mittelstaedt[19] has in fact shown that the law A → (B → A) does not hold in quantum logic for precisely such reasons. In classical logic the law is defended as follows: let the opponent insert some p for A. Let him prove p. Now the proponent must defend B → p. Let the opponent insert any q for B. Let him prove q. Now the proponent must prove p. He resumes the proof for p given by the opponent, and wins. In quantum theory, let p mean "this electron has the momentum p" and q mean "the same electron has the position q". Measurements of the respective quantities are admitted as proofs. The

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[19] See Mittelstaedt (1976).

opponent first measures the electron's momentum and finds p; then he measures its position and finds q. Now the proponent resumes the momentum measurement, but, alas, he does not find p again.

This way of arguing will provoke serious objections. One will say that this talk of statements referring to the present is imprecise because the present is not always the same point in time; thus statements that actually differ are being identified as the same statement. If in Mittelstaedt's example we denote the times of the three successive measurements by 1, 2, and 3, respectively, the first statement on momentum will read p_1: "at the time 1 the momentum is p", while the last one will read p_3: "at the time 3 the momentum is p." p_1 and p_3 are now statements about "objective time moments" that can be considered to be timelessly true or false. There is no reason why p_3 should be true if p_1 is true.

It is decisive for tense logic to stand firm against this objection. The counter-objection is that we have to rely on *some* statements about the present in order to be able to prove any statements at all, whether they are timeless or contingent, and whether they refer to objective instants in time or are in tensed form. A statement about an objective instant t_0 can be proved by inspection just once in history, namely at the time t_0. No proof by inspection of such a statement can ever be given at any other time. How can logical laws about them be justified, then? We can prove p_1 at time 1 by inspection. If we are to prove p_1 again (in contradistinction to proving p at time 3; i.e., p_3), we must rely on documents or traces of the past. If such traces exist we will have to recognize them at the given time by inspection, or by having a trace of the trace. This means: statements about past objective instants can meaningfully be made only if statements about the present can meaningfully be made. The possibility of formulating contingent statements referring to objective instants, which traditional logic gladly accepts, rests on the possibility of formulating them with reference to the respective present, which traditional logic would gladly eliminate. All this amounts to the truism that whenever "I am speaking" is true, "I am speaking now" is equally true; there is no way I can see a phenomenon, or utter a statement, except 'now'. I would be prepared to accept the view that we are here facing the deep mystery of time, but I would add that unless we actually face it we will not even understand what we mean by logic, let alone physics.

What about statements referring to the future ("the moon will shine tomorrow")? Statements referring to the past can rely on traces (documents, residues). Of the future there are no traces. I made use of this difference between past and future in discussing the Second Law of Thermodynamics in section b. I think we have to accept the difference as a fundamental structure of time. However, a consistency proof can be given. We can express the Second Law by saying that improbable states are followed by more probable ones, and are preceded by more improbable ones. A trace (a document) is an improbable state. It implies even more improbable states, i.e., much information, in the past, and more probable states, i.e., very little information, in the future. This sheet of paper contains letters; it is a document of the improbable event of some particular human thought in the past. Its future, however, is quite uncertain: it may be forgotten in a library, burned by fire, or gone with the wind. Thus the Second Law both presupposes and explains that there are traces of the past but none of the future.

I am prepared to call a statement true if it can in principle be proved, whether by direct inspection (present), by inspection of a trace (past), or by mathematical or similar arguments (timeless). But I would propose not to use the values 'true' and 'false' for statements about the future. This is, I suspect, what Aristotle had in mind when he wrote his Chap. 9 of *De Interpretatione*. A statement about the future can be proved or disproved by inspection only when it is no longer a statement about the future. Statements about the future can, however, meaningfully be called 'necessary', 'possible', 'impossible', 'probable with probability p', and so on; 'modalities', as the logicians say, can be applied to them. "It is necessary that the moon will shine tomorrow" means that my contingent knowledge of the present or the past, together with my knowledge of the laws of nature, implies that the moon will shine tomorrow. If A stands for my knowledge, and M for the prediction concerning the moon, what I can maintain is "necessarily if A then M". By an admissible rule of detachment this is transformed into "necessarily M". Yet this 'necessity' does not imply reality; it is a weaker statement than truth. After all, some unknown celestial body might interfere with the moon's orbit in the meantime.

From the tense logic thus outlined one can go on to form a theory of probability that confines the primary use of probability to a quantitative refinement of the modalities as applied to the future. This theory of course encounters the well-known difficulties that face any theory of empirical (observable) probabilities. A subjectivist theory of probability describes what people may expect if they think consistently. But that will not be enough for a physicist. He wants to know not only whether the assumption of a certain probability for some event is logically consistent, but also whether it is correct. The question is what he means by calling the assignment of a probability value to an event correct. At least in principle, an empirical confirmation ought to be possible. Probabilities are actually measured as relative frequencies in a large series of observations under constant conditions. The main conceptual difficulty here is that the relative frequency itself, in any finite sample, is not strictly predicted by assigning a probability to the event. Thus its measured value is not supposed to be identical with the assigned probability, and is therefore not its true empirical expression. However, this difficulty is not fatal to the theory. No theoretically predicted quantity can be compared directly with the result of a single measurement of that quantity. One must take into account the statistical distribution of measured values. In this sense, any predicted quantity can be verified empirically only with some probability, not with certainty, where the probability approaches the value 1 as the number of cases increases. We now have to admit that this is also the case for the particular quantity that we call probability. A probability can in this sense be defined as the prediction of a relative frequency or, to put it more precisely, as the expectation value of a relative frequency. This expectation value is defined by means of probabilities within statistical ensembles *of* statistical ensembles.[20] This definition is not circular but has an 'open end' which is closed by assuming that some probability sufficiently close to one can practically be equated with certainty. I would be prepared, in a rather lengthy discussion, to defend the view that this description of probability goes precisely as far as one can go in a theory of experience.

7.4 Axiomatic Quantum Theory

In this section I make use of a doctoral thesis by Drieschner.[21] Under the title of "preconditions of any experience" I have so far collected the structure of time as described by tense logic, and the concept of objective probability as applied to the future. It would be the ultimate goal of physico-philosophical ambition to uncover further preconditions of all possible experience in this manner, until their list would suffice to formulate the complete set of basic assumptions of a unified physics as postulated at the end of section b. This would presuppose two accomplishments, neither of which I am able to present. One would be a full and thoroughgoing philosophical analysis of concepts such as time, logic, number, and probability to replace the sketch given in section c. The other would be the extension of the analysis to basic concepts of physics such as object, state, change, and observation. These analyses might lead toward a theory of the probabilities with which we can predict the changes of the observable states of any object. The existing theory most closely corresponding to such a description is quantum theory. Thus it does not look a priori hopeless to deduce quantum theory from the preconditions of experience.

So long as this analysis has not been done, an easier, heuristic method that uses the so-called axiomatic method as one of its main tools may be applied. One can try to find a set of axioms that on the one hand suffices for deducing the known system of quantum theory, while admitting on the other hand of a simple interpretation in terms of so-called preconditions of experience which, while they cannot be said to be *strictly* necessary for any possible experience, still show some degree of plausibility of being necessary. This is Drieschner's approach.

I should like to use two additional preparatory paragraphs on the methodological meaning of axiomatic systems.[22] It is a Greek discovery that mathematics can be presented in the form of deductive systems, i.e., sets of propositions that are divided into two classes: the axioms, which are presented without proof, and the theorems, which can be logically deduced from the axioms. The tradition (with the notable exception of Plato) considered axioms to be self-evident and hence not in need of proof. This primitive attitude was psychologically shaken by the discovery, in the

[20] Cf. "Probability and Abstract Quantum Mechanics" in this volume (Chap. 9)—(*ed.MD*).

[21] See Drieschner (1970).

[22] Cf. i.5. of von Weizsäcker (1980)—(*ed.MD*).

19th century, that a non-Euclidean geometry is logically possible. I say 'psychologically' shaken, since the refuted hope of *logically* deducing the parallel postulate from the other axioms had in itself been due to a well-founded suspicion that this particular axiom could not simply be regarded as self-evident. What was shaken was the trust in the simplistic notion of 'self-evident propositions'. Under the influence of Hilbert's *Grundlagen der Geometrie,* another, more sophisticated, but in my view still simplistic notion took its place, and has dominated the 20th century. It is the view that the mathematician has no business knowing whether a certain set of axioms is true or not, or even what might be the meaning of calling it true or false. Certainly nobody can be forbidden to define terms in such a way that 'mathematics' is restricted to the analysis of the logical connection between sets of formal 'propositions'. When attacked by Brouwer, however, Hilbert had to admit that a field does exist (now called 'meta-mathematics') in which we study the presuppositions of 'mathematics'; these presuppositions are not 'axioms' but 'ways of action', for example, which we understand 'intuitively'. It seems that this field contains more than logic—e.g., the structure defined by the basic intuition of finite number. It is tempting to say that "true mathematics is meta-mathematics"; this would be the intuitionist standpoint. In any case, mathematics seems to face problems that are analogous to (and, as I personally think, basically identical with) those I am discussing here for physics.

In physics we encounter theories, such as quantum theory, having the structure of a mathematical formalism. Such formalism admits of an axiomatic analysis. Yet, as pointed out at the end of section a, a mere formalism is not yet part of physics; in order to be physics, it needs a physical semantics. If the formalism is given in axiomatic form it will suffice to give a semantic for the axioms. If this semantic refers to particular experience, we will call it empirical. Our present attempt is to refer it not to particular experience but to traits common to all experience, perhaps even to 'all possible experience.' De facto our attempt does not prove the necessity but only tends to show the plausibility of our axioms. In one point (finitism, see postulate D below) we dare to rely more on our epistemological arguments than on the present form of quantum mechanics.

Drieschner's paper begins with an ordinary-language discussion of the preconditions of experience, then formulates a set of axioms in the language of mathematical logic, and ends up by deducing ordinary quantum mechanics (with one deviation) from the axioms. I will condense the results of the first part into a group of 'postulates', after which I repeat Drieschner's 'axioms' in ordinary language, and comment on their meaning and consequences. I will assign a symbol (letter or number) to each postulate or axiom and, in addition, will give it a name that loosely indicates its most important aspect. The logical interlinkage of the postulates and axioms will not be examined.

A. Postulate of Alternatives. *Physics formulates probabilistic predictions concerning the outcome of future decisions of empirically decidable alternatives.* The claim of this postulate that physics makes probabilistic predictions for the future states the result of our tense-logical considerations. That these probabilities intrinsically cannot be reduced to certainties is not being postulated here; it will be postulated later (in axiom 5). The postulate adds that the probabilities refer to the decision of empirically decidable alternatives. An ‘alternative’ in the sense used here can be defined as a complete list of mutually exclusive temporal propositions. These propositions I will also call the ‘answers’ to the alternative, which can in turn be understood as a question. The alternatives are temporal propositions in the sense of section c. This means that in their simplest form they state something about the present (“it is raining in Hamburg”). They can be formulated so as to apply to some past, future, or ‘objectively defined’ time (“it was raining in Hamburg yesterday”, “it will be raining in Hamburg tomorrow”, “it ‘is’ [‘was’, ‘will be’] raining in Hamburg on July 1, 1968”). Throughout our investigation time will be considered as admitting measurement by clocks that ‘in principle’ assign it a value at any moment, given by the real variable t; the scepticism called for with respect to this simplification is beyond our present scope.[23] Our temporal propositions and hence our alternatives are ‘time-bridging’ in the sense that it is meaningful to speak of deciding the same alternative (“is it raining in Hamburg?”) for different times. The words ‘the same’ evidently do not mean ‘identical’, since it makes a difference whether it is raining in Hamburg on the first or the second day of July 1968. “The same” means “falling under the same concept” (“raining in Hamburg”). This definition of “the same” will suffice provided we always take the concept as narrowly as possible (not ‘raining’ but “raining in Hamburg”; not “a particle at position x”, but “this particle at position x”); the use of demonstrative pronouns for defining a concept with sufficient narrowness is probably inevitable. We consider alternatives as decidable without investigating how the decision is effectuated; it is merely characterized as ‘empirical#’; i.e., as depending on particular (‘contingent’) phenomena that may or may not occur at a given time. What may occur we regard as being formulated by the respective theory. Physics itself provides the lists of what I like to call “formally possible temporal propositions”, which formulate what may at all occur at any time. They are called ‘formally possible’ as distinct from ‘actually possible’, which refers to the future at some particular time; thus it is formally possible that it may rain in Hamburg at any time, including this afternoon, but knowing the meteorological forecast I conclude that rain this afternoon in Hamburg is actually impossible. The answers to a given alternative must be mutually exclusive; i.e., if one of them is actually true all others must be actually false. We are permitted to call a temporal proposition actually true or false only for the time in which the alternative to which this proposition is an answer is actually decided. It will be one of the main points of quantum theory that we are not permitted to assume the truth or falsehood of answers to sufficiently precise alternatives in situations in which these alternatives are not actually decided. At present this remark is only a warning against reading into

[23] Cf. iv.6d. of von Weizsäcker (1980).

the words 'true' and 'false' an interpretation that is not implied; it will be expressly excluded only by axiom 5. Finally, I define the answers to an alternative as forming a complete list if and only if the following holds: If all but one of the answers to an alternative are actually false, that one is actually true. Our postulate implies the existence of such complete lists, and we will confine the term 'alternative' to such sets. Thus, terminologically, an alternative is not confined to two possible answers (contrary to the literal meaning of 'alter'). If the number of its answers is a positive integer n, we shall call it n-fold; a 2-fold alternative will also be called a 'simple alternative'. For 'empirically decidable alternative' I will simply say 'alternative'. Our alternatives evidently correspond to what are usually called observables, and their answers to the possible values of the observables.[24]

B. Postulate of Objects. *The answers to an alternative ascribe contingent properties to an object* Logically this means that the answers to an alternative can be formulated as 'categorical judgments', which ascribe a predicate to a subject, here called object. The postulate is intended to imply that *one* alternative refers to *one* object, while different alternatives may or may not refer to the same object. I think that in a sufficiently developed theory postulate B will be a consequence of postulate A; i.e., the concept of an object will be reducible to the concept of (time-bridging) alternatives.

C. Postulate of Ultimate Propositions. *For any object there exist ultimate propositions, and ultimate alternatives whose answers are ultimate propositions.* Here an ultimate (contingent) proposition about an object is defined as a proposition that is not implied by another proposition about the same object (with the trivial exception of the 'identically false proposition', which by definition implies every proposition—*ex falso quodlibet*). The particular properties corresponding to ultimate propositions will be called 'states' of the object. For a classical object, a point in phase space represents a state, and the set of all these points represents its (unique) ultimate alternative. For a quantum theoretical object, a one-dimensional subspace of Hilbert space represents a state and any complete orthonormal system represents an ultimate alternative. I here omit the very interesting consideration with which Drieschner tries to show that this postulate belongs to the defining properties of any physical theory.

D. Postulate of Finitism. *The number of answers to any alternative for a given object does not exceed a fixed positive integer n which is characteristic of that object.* This is the postulate in which Drieschner's approach differs from the usual quantum theory. Of course one may expect that, by choosing n sufficiently large, it will be possible to avoid contradictions with the predictions of the usual quantum theory. One may also expect that this postulate will lead to a great simplification of the proofs in the ensuing axiomatic construction of the theory. But the two expectations might be mutually exclusive. If finitism leads to a true simplification of the theory, it will probably exclude some features of

[24] Much of the analysis of these concepts is due to Scheibe (1964).

possible objects of the theory that must be taken into account in an infinitistic theory, and that are consequences of 'true' infinities. It is Drieschner's and my view that in the case of conflicting consequences, the finitistic view should tentatively be considered the correct one. I think that the test case will arise only in the interaction problems of quantum field theory; cf. section e. At present I confine my argument for the truth of the postulate to the statement that no alternative with more than a finite number of answers can actually be decided by an experiment, and to the naive remark that a physicist ought to be surprised to find phenomena in nature in whose description the word 'infinite' could not be replaced by the words 'very large'. For a more refined argument I refer to Drieschner's thesis and, in part, to section e.

E. Postulate of the Composition of Objects. *Two objects define a composite object whose parts they are. The direct product of any two ultimate alternatives of the two parts is an ultimate alternative of the composite object.* If a_j (j = 1 … m) and b_k (k = 1 … n) are the answers to the two alternatives, $a_j \wedge b_k$ ($\wedge$ means 'and') are the answers to their direct product. This postulate seems logically natural enough; it has far-reaching consequences in quantum theory.
F. Postulate of the Probability Function. *Between any two states a and b of the same object a probability function p(a, b) is defined, giving the probability of finding b provided a is necessary.*

The language and content of this postulate rest on the fundamental assumption that whatever can be said about an object in a manner admitting of empirical testing must be equivalent to the prediction of certain probabilities. This we believe to be true because the empirical test of a proposition is the decision of an alternative at a time that lies in the future when the proposition is made, so that only probabilities can be predicted for it. This view prompts the phrasing 'provided a is necessary', which is usually expressed as 'provided the object is in state a'. For the empirical test of the statement that the object is in state a is simply an experiment for which the outcome 'a' is predicted with certainty, i.e., p (a) = 1; this we identified in section c with 'a is necessary'. In addition to this basic assumption, the postulate expresses our view that the relationship between the states of the same object admits of an intrinsic description; i.e., a description that does not refer to any objects besides the one under consideration. One can also express this by saying: The states of an object always remain the same, independent of its environment.[25] That this must be true is perhaps not self-evident; it appears rather a strong statement on the meaning of the concept of an object. I will accept it here as a principle.[26] If it is true, the postulate becomes quite plausible, for what other intrinsic description subject to empirical testing could there be besides the probability function?

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[25] Of course 'states' here means 'Schrödinger states'; we say that the object can change its state in time, while the definition of the states through which it passes can be given independently of time and—what matters here—of its environment.

[26] The reduction of objects to alternatives mentioned under postulate B leads to this principle.

G. Postulate of Objectivity. *If a certain object actually exists, one ultimate proposition about it is always necessary.* The premise "if a certain object actually exists" will turn out to be less trivial than it seems. 'A certain object' is to mean an object whose alternatives, or at least some of whose alternatives, it is known how to decide. Clearly if such an object does not exist at all, none of its states can be found, and hence no ultimate proposition about it can be necessary. Now assume the object actually exists. This is a proposition about the object. We assume that every existing object admits of k-fold alternatives, with $k > 1$. The proposition that it exists, therefore, is not an ultimate proposition, for it is implied by any answer to one of its alternatives. We further assume that a theory that makes predictions about the object's behaviour is possible; otherwise the object would not fall under our concept of experience. The predictions can consist only in ascribing probabilities to the contingent propositions about the object. Any possible contingent theoretical statement on the object will therefore be describable as a list of such probabilities; we call this a statistical characterization (s.c.) of the object. We now distinguish incomplete from complete statistical characterizations. In an incomplete s.c. some possible information is lacking; a complete s.c. gives as much information about the object as can be given according to its theory. Thus the knowledge that some state a is necessary is a complete s.c, which is expressed by the function p(a, b) for all b. By definition, $p(a, a) = 1$. Now consider any complete s.c. It is itself a contingent proposition about the object; let us call it c. It must be ultimate. Otherwise it would be implied by at least two ultimate propositions (if it were implied by a single one, it would in turn imply that one and would hence be equivalent to it); then an alternative could still be decided that goes beyond c, and c would not be complete. Now c implies itself [$p(c, c) = 1$], and hence one ultimate proposition is necessary. Two different ultimate propositions cannot both be necessary, since the s.c.'s corresponding to them would imply each other, reducing them to equivalence.

This rather delicate argument of Drieschner's rests on the distinction between complete and incomplete knowledge. In complete knowledge, probabilities different from 0 and 1 may (and will, according to axiom 5) occur. Unlike thermodynamical probabilities, they *cannot* be described as being due to lack of information. This means that the idea of an 'objective situation' that can be either known or unknown is not given up in this theory, and I have therefore chosen the name 'postulate of objectivity'.

We must, however, examine the consequences of the language adopted here. What 'propositions about the object X' are admissible here (X being a proper name of the object under consideration)? All ultimate propositions about X are admitted. Drieschner shows that, according to his axioms, and using appropriate definitions of 'not', 'and', and 'or', an ortho-complemented lattice of propositions about the object can be constructed. It can be described as consisting of all ultimate propositions and all finite 'logical sums' of the type 'a or b', 'a or b or c,' etc. This lattice is a model of von Neumann's quantum logic. 'Mixtures', however, are not admitted as 'propositions about the object', but only as "propositions about a (real) collection of objects"—a point on which I again must refer to the thesis.

Another proposition that cannot be accepted as a 'proposition about X' is any ultimate proposition about a composite object consisting of X and, say, Y that is not (speaking in terms of completed quantum theory) a product of pure states of X and Y. This must be so, because in a state of the composite object that is not a product state, none of the ultimate propositions about X is necessary. The solution of this apparent paradox (which is essentially the Einstein-Rosen-Podolsky[27] paradox) is that in such a state it is simply not permissible to say that object X actually exists.[28] It has only 'virtual existence'—i.e., the composite object can be divided into X and Y, but only by altering its state. This is the relevant sense of the premise of postulate G. One cannot then say that X 'does not exist at all', it merely does not 'exist actually'. I admit that I am not yet fully satisfied with the stringency of these distinctions, but so far I have not been able to do better.

I now turn to Drieschner's axioms.

1. Axiom of Equivalence. *If a and b are ultimate propositions, p(a, b) = 1 is equivalent to a = b.* This is a two-way implication. 'a = b implies p(a, b) = 1' means p(a, a) = 1, as stated above. On the other hand, 'p(a, b) = 1' means it is certain that b will be found (in an experiment appropriate to finding b) if a is necessary, and this is what we mean by saying that a implies b. But if b is ultimate, it cannot be implied by *another* proposition, hence a = b. We see in this simple example how these 'axioms' tend to express certain consequences of the basic assumptions formulated in the postulates.
2. Axiom of Finite Alternatives. *If n mutually exclusive ultimate propositions a_i (i = 1 … n) are given, then for any ultimate proposition b:*

$$\sum_{i=1}^{n} p(b, a_i) = 1.$$

This is a probabilistic version of what was meant in the commentary on postulate A by attributing a *complete* list of mutually exclusive answers to any alternative, combined with postulate D. For two mutually exclusive ultimate propositions a and b, we evidently must assume p(a, b) = p(b, a) = 0.

We now introduce 'propositions about X' as *sets* of 'ultimate propositions about X' (for the sake of brevity, we will from now on omit 'about X'). A proposition A then means "the necessary ultimate proposition is an element of the set A". Here postulate G is used in speaking of "the necessary ultimate proposition". $\overline{\text{A}}$ (i.e., 'not A') is defined as the set consisting of all those ultimate propositions that are mutually exclusive with all elements of A. A ∧ B ('A and B') is the set of all elements *contained* both in A and in B. A ∨ B ('A or B') is defined as $\overline{\overline{\text{A}} \wedge \overline{\text{B}}}$.

[27] See Einstein et al. (1935).

[28] Cf. iv.6d. of von Weizsäcker (1980).

3. Axiom of Decision. *For any A there exists an alternative* $a_1 \ldots a_n$ *such that* $a_1 \ldots a_e$ *are elements of A while* $a_{e+1} \ldots a_n$ *are elements of* $\overline{A}$. We have assumed in postulate A that all contingent propositions are decidable. We now assume that there are always ultimate alternatives 'adapted' to any decision between an A and $\overline{\mathrm{A}}$. We note, first of all, that A and $\overline{\mathrm{A}}$ form a simple alternative. This is not trivial since most of the ultimate propositions will belong to neither A nor $\overline{\mathrm{A}}$. But if A is decidable, an experiment is possible by which either 'A' or 'not A'—whatever 'not A' may turn out to mean—is found. If 'A' is found, then for an immediate repetition of the experiment, A will be necessary; if 'not A' is found, A will then be impossible. This is what we mean when we say that the statement 'A is decided' is subject to empirical testing. Now 'A' means that the necessary state belongs to the set A. 'Not A' then must mean that it belongs to a set of states excluding all states of A. The decidability of A also means that 'not not A' is equivalent to A. Thus the states of A also exclude those of 'not A', and 'not A' must indeed consist of all states mutually exclusive with those of A; i.e., not A $= \overline{\mathrm{A}}$. I omit the more complicated argument leading to axiom 3 in its fullest sense.
4. a. First Axiom of Completeness. *For any set of* $k < n$ *mutually exclusive ultimate propositions* $a_1 \ldots a_k$ *there exists an ultimate proposition a with* $p(a, a_i) = 0$ $(i = 1 \ldots k)$. This axiom, together with axiom 2, implies that all ultimate alternatives have exactly n answers. It may also be called a symmetry axiom for ultimate alternatives.
   b. Second Axiom of Completeness. *For any set of* $n - 2$ *mutually exclusive ultimate propositions* $a_3 \ldots a_n$ *and any ultimate proposition b, there exists an ultimate proposition* a_2 *which excludes all* a_i $(i = 3 \ldots n)$ *and b*; i.e., $p(a_2, a_i) = 0$ *and* $p(a_2, b) = 0$. These two axioms seem rather special. Perhaps they are not very strong statements, since certainly no ultimate alternative with more than n answers is acceptable, and alternatives with less than n answers can always formally be completed by adding a few ultimate propositions which, to conform with the assumption that the given alternative has less than n answers, must have probability 0 for all possible measurements. However, axiom 5 will exclude these cases, and is in this sense the truly strong statement.
5. Axiom of Indeterminacy. *For any two mutually exclusive ultimate propositions* a_1 *and* a_2 *an ultimate proposition b exists such that* $p(b, a_1) \neq 0$ *and* $p(b, a_2) \neq 0$. The weaker statement "a pair of mutually exclusive a_1 and a_2 exists such that…" would suffice to introduce indeterminacy; i.e., to exclude the trivial classical model of the other axioms in which all ultimate propositions are mutually exclusive. I feel that indeterminacy is a precise expression of what I called the openness of the future in sections b and c. Without this assumption, the application of probability to the future would merely express a lack of knowledge, and I cannot see how the Second Law of Thermodynamics could then be reconciled with our assumptions. The reader who remains unconvinced by this argument is invited to accept the axiom pragmatically, because it leads to

quantum theory. Its general formulation "for any a_1 and a_2 …" introduces full symmetry in state space. Concerning this symmetry, cf. section e, postulate M.

6. Axiom of Exclusion. *For ultimate propositions, $p(x, y) = 0$ implies $p(y, x) = 0$.* This is connected with the law of double negation $\overline{\overline{A}} = A$.

Using these axioms, Drieschner can show:

(a) The set of propositions is a complemented non-Boolean lattice.
(b) The lattice is a projective geometry of n − 1 dimensions.
(c) The lattice is isomorphic with the lattice of the subspaces of an n-dimensional vector space.
(d) In this space the probabilities define a metric.

We are then left with the question on what algebraic number field this vector space is erected. I formulate the basic assumptions[29] as:

H. Postulate of Measurability. *Any linear operator in the vector space that leaves the metric invariant*[30] *is an observable*, and
I. Postulate of Continuity. *The possible changes of state in time are described by one-to-one mappings, continuous in time, of the set of states onto itself.*

The probability metric, together with postulate I, shows that the number field must contain the real numbers, leaving only a choice among the real numbers themselves, the complex numbers, and the quaternions. If we try to describe measurements in our theory, we are led to conclude from postulate H that every linear operator must admit of diagonalization. Hence the number field must be algebraically closed, and we are led to choose the complex numbers. The result of these considerations is formulated by Drieschner in two axioms that I do not repeat here.

One may wonder how well postulates H and I are founded in the preconditions of experience. I cannot explicitly argue here for I, which contains both continuity and the basis for reversibility. This would entail a return to the theory of time on a higher level than in section c, which I have not achieved so far. Postulate H repeats von Neumann's famous general identification of operators with observables. In infinite-dimensional Hilbert space, this was a sweeping assumption that understandably provoked scepticism. In our finite-dimensional Hilbert spaces, it might look more plausible. Since certainly *some* operators will be observables, this assumption, too, expresses a high symmetry of the manifold of states. The question of why we assume symmetry must be asked once again, this time under a new heading.

[29] In the following two postulates my approach differs from Drieschner's. His postulate on the homogeneity of time would allow a weaker version of my postulate I. The relation between the two approaches has not yet been worked out in detail.

[30] This should actually read 'Any self-adjoint operator'—(*ed.MD*).

7.5 The Unity of Physics: Part Two

The four preceding sections presented an analysis of existing physics. I now offer a hypothesis concerning the road that will lead beyond it. We seek a unified theory that would generate the concepts and laws of the five theories discussed in section b from a few universal principles. The hypothesis says that the conceptual tools for this task are already assembled in the preceding analysis.

I hope that this analysis has sufficiently clarified quantum theory (2 in the list in section b), thermodynamics (4), and their mutual relationship, by reducing them essentially to theories on the decision of alternatives in time. 'Sufficiently' does not mean 'completely', since that, if possible at all, would presuppose a full philosophy of time; but it means sufficiently for the purpose of proceeding to the three remaining theories. We still have to introduce space both locally (1) and at large (5), as well as the ultimate objects (3). I think this can be done by consistent application of the one postulate in which our presentation differs from the usual quantum theory; i.e., the postulate of finitism (section d, D).

(2 and 5) I will begin with the question of the totality of objects and of space in the large, i.e., with cosmology, by stating a *preliminary* postulate:

J. Postulate of Approximately Stationary Cosmology. The *universe can be approximately treated as an object*. Treating the universe as an object is exactly what cosmological models do. The postulate will serve a similar purpose in the present theory: it will permit a convenient terminology. But the identification of the universe with an all-embracing object cannot be entirely correct, and in order to avoid misunderstanding I will first formulate the objections (cf. the corresponding passage in section b).

An object is an object for subjects in a world. This would be made even clearer if the concept of an object could be reduced to decidable alternatives (see commentary to postulate B). It seems philosophically absurd to reduce the world to what is possible only within a world.[31]

A more particular objection is that our concept of an object contains only 'time-bridging alternatives'. Hence no contingent questions can be asked about a given object in the future that would not have been equally possible in the past; furthermore, the postulate of finitism treats the number of answers to admissible alternatives as constant in time. As an example, let us briefly consider the meaning of continuity in a finitistic theory.[32] It is certain that no alternative that has ever been decided or will ever be decided will have more than a finite number of answers. But this number is not a priori fixed. A continuum, say a line, may have actually been divided in the past into no more than n parts, thus, e.g., answering the

[31] One might add that our reflections at the end of iii.3 and in iv.6d of Weizsäcker (1980) show the quantum theoretical concept of an object to be an exaggeration even *within* the world.

[32] Cf. **iv**.4 "Possibility and Movement" of Weizsäcker (1980), reprinted in the volume *Major Works in Philosophy,* in this series—(*ed.MD*).

n-fold alternative ‘in what part of the line is the material point P?’ (Let me stress that I always speak of a *physical* continuum, which I suppose to exist.) Yet continuity implies that it is possible to go beyond any fixed number n of the parts; we can go on dividing the line. The words ‘it is possible to…’ and ‘we can go on’ refer to the future. The next division will again be finite, but its n’ may be larger than n. In this sense a continuum is not an object, if we define objects according to section d; rather, it is an indefinite number of possible objects. The basic idea of the postulate of finitism can then be formulated as the converse statement: a given object is not continuous. That one should treat given objects as non-continuous is, I think, the fundamental truth of quantum theory. Quantum theory has eliminated the trouble created by continuity in classical physics (Planck), and it has run into troubles of its own where it attempted to build ‘true’ continuity into its frame; i.e., in the field theory of interactions. (A non-relativistic theory of mass points in continuous space does not describe ‘true’ continuity since it would not be essentially changed if space were replaced by a point lattice.) The word ‘given’ in the phrase ‘given objects’ means what is already given; i.e., it refers to the past as found in the present. Finitism says that only a finite number of answers has actually been given at any time; it should not rule out that the number of possible contingent propositions is infinite in the potential sense: for any given number of propositions, another proposition can be formulated and empirically proved. In this sense, continuity essentially means the openness of the future. But then in a continuum the number of ‘objects’ cannot be constant in time. Since we do not wish a priori to rule out continuity as a trait of the real world, we should not assume the world to be strictly an object.

(2, 3, and 5) Given these precautions we now draw some conclusions from postulate J, disregarding the word ‘approximately’ for the moment. Let the universe be an object with an n_U-dimensional Hilbert space. Our first question is how we can at all introduce a concept such as position or configuration space into the theory of such an object. I shall first discuss this question in a formal manner.

Historically, quantum theory was constructed the other way round. Configuration space was thought of as given, and vectors in Hilbert space were an abstract description of functions in configuration space. We now ask what conditions must be imposed on configuration space in order that the number of dimensions of the Hilbert space of its functions should be (a) denumerable and (b) finite. If I am not mathematically mistaken, condition (a) means that the configuration space must have a finite volume. Usually one imposes on it a periodicity condition. It seems natural to use a closed configuration space; this will be my working hypothesis. Then condition (b) can be satisfied by a cut-off condition, e.g., by using only the n_U first functions of an ortho-normal expansion. Assumption (a) is a cosmological assumption, assumption (b) looks like elementary particle physics. It is my hypothesis that this is not only apparently so. I think that finitistic quantum theory implies (a) a cosmology, (b) ultimate objects (by implying some sort of ‘elementary length’ and elementary particles), and (c) a necessary connection between these two.

(1 and 2) What is the physical semantics of space? Configuration space is simply a mathematical description of the set of positions of mass points in space.

What is the meaning of 'space'? I think it is the basic concept of the theory of a manifold of interacting objects.[33] Inverting postulate E, we say that an object can be viewed as consisting of objects that we call its parts. The inner dynamics of the given object must then be described partly as the inner dynamics of each of its parts, and partly as their interaction. Let us for the sake of simplicity speak of two objects A and B which are the two parts of an object C. Let A have an m-dimensional Hilbert space and B an n-dimensional one; then the Hilbert space of C will have m·n dimensions (I ignore problems like Fermi and Bose statistics). The conceptual splitting of C into A and B has the practical meaning that we do not always need to decide m·n-fold alternatives, but that there may be practicable measurements of m-fold alternatives of C which allow one to say that they are 'alternatives of A'. Practically this means that the interaction of A and B must be sufficiently small while a measurement on A takes place. We can use the approximation of isolated or 'free' objects even though we know that no absolutely isolated object can be an object for us (known to us). Thus the concept of interaction corresponds to the approximative way of talking on which all physics rests: we speak of separate objects or separate alternatives, knowing that they do not exist in a strict sense, and we correct this mistake by describing them as interacting objects.

I now maintain that the continuous parameter on which the interaction depends is called position, and that the manifold of the possible values of this parameter is the meaning of 'space.' Allow me to use concepts belonging to the language of particles for an illustration. Consider the composite object C in the absence of additional, 'external' objects. In order to speak meaningfully of A and B as parts of C, we need to posit 'separated' states of C; i.e., states in which the interaction between A and B is negligible. On the other hand, in order to speak meaningfully of C as consisting of A and B, we need to posit 'connected' states of C, in which the interaction between A and B is not negligible. Let us assume that C goes through a 'scattering process'; i.e., for $t \to +\infty$ and for $t \to -\infty$ the states are separated, but in the interval between t_1 and t_2 they are connected. How does the change of state of C determine the value of t_1; i.e., the time of the onset of interaction? We must, it seems, presuppose (a) that the states of A and B will change even without interaction. This I call the weak law of inertia; it appears to be a presupposition of the concept of separate objects. We must also presuppose (b) that t_1 is determined by the values of changing parameters that are defined even in the separated states; i.e., of parameters defined for 'free motion'. Let there be such parameters x for A and y for B, then the value of the interaction energy must depend on some relation between x and y. I maintain that position will turn out to be this parameter. This means: we are not given, first, a parameter called position, and, secondly, an interaction that happens to depend on position; but rather: there is a parameter on which interaction depends, and that is what we mean by position. Of course, this must be shown in mathematical detail. Here a third presupposition

[33] Cf. ii.3 g of von Weizsäcker (1980).

enters: (c) if the theory of free objects is invariant with respect to some group, then the interaction must be invariant with respect to the same group. This must be so because the relation between x and y on which the interaction depends must be invariant with respect to the free group, x and y being defined for free objects.

This consideration is only heuristic, for it depends on the assumption that there are 'separated' and 'connected' states, which will not be true for ultimate objects. I have not been able to replace it by a stricter one. In any case, it indicates that we will have to introduce space and the plurality of objects together.

(1, 2, and 3) How far can we go in splitting up the objects? We try as radical a hypothesis as possible:

K. Postulate of Ultimate Objects. *All objects consist of ultimate objects with $n = 2$.* In German I called them *Urobjekte,* and their alternatives *Uralternativen;* as a whimsy, I proposed the abbreviation *ein Ur*, 'an ur', for such an object, which I will use here, too. This postulate is trivial so long as we do not specify the law of interaction for ultimate objects. A Hilbert space of n dimensions can always be described as a subspace of the tensor product of at least r 2-dimensional Hilbert spaces, where $2^{r-1} < n < 2^r$; the $2^r - n$ unused dimensions can be excluded by imposing a law of interaction that leads to a super-selection rule between the two subspaces. The ultimate objects become meaningful if we use our previous heuristic consideration for stating the:

L. Postulate of Interaction. *The theory of the interaction of ultimate objects* ('*urs*') *is invariant under the same group as the theory of free ultimate objects*. This is a strong, non-trivial assumption. It obliges us, first of all, to study free urs.

A single ur is an object with a two-dimensional Hilbert space. It admits the transformation group SU_2. I should like to emphasize the physical meaning of this primitive mathematical statement. The 'theory of a free ur' describes not just the manifold of states of an ur, but also its law of motion. The equation of motion must be invariant under SU_2. It is easy to find the solutions of such an equation; the state vector can have only a common time factor $e^{-i\omega t}$ with state-independent ω, and hence the states themselves, being one-dimensional subspaces, remain unchanged in time. Yet the question is how we know this to be the correct condition to impose on the law of motion for the free ur. This is the form in which we now face the often repeated question of section d, why we should postulate symmetry for the state space. I formulate the principle that is involved here as a postulate:

M. Postulate of Symmetry. *None of the states of a single ultimate object is objectively distinguished from any other.* An 'objective' distinction is here a distinction 'by law of nature', as opposed to a 'contingent' one such as, e.g., 'the state which this object is in right now'; hence the *law* of motion must not distinguish among the states. What is meant here can also be said in the following way: an ultimate object is defined by a simple ultimate alternative. To distinguish between two of its states means to define at least one additional alternative. If an answer to this additional alternative implies one of the states of the original ur, the proposition that this state obtains is implied by another

> proposition. Hence it was not ultimate. Hence not one but at least two urs were present. In this sense the possibility of speaking of a separate object means simply that its state space is symmetrical; otherwise it is not a separate object. To put it differently yet again: the approximation within which we can describe objects as symmetrical is the same as that within which we can describe them as separate. Physics is the attempt to describe reality starting from this approximation. Perhaps this approximation is nothing other than the precondition for the use of concepts.

The Hilbert space of a single ur is a 2-dimensional representation space of the SU_2. Postulate L implies that the Hilbert space of many urs must be a higher-dimensional representation space of the same group. The law of motion of interacting urs must be invariant under the simultaneous transformation of all urs by the SU_2. Hence we seek a mathematical representation of the states of urs which from the outset will express this invariance. We obtain this representation from the so-called regular representation of the group. In this representation the elements of the group are linear operators acting on a vector space whose vectors are linear combinations of the group elements themselves. Thus the 'regular' representation of the state vectors of urs will describe them as functions *on* the SU_2. Now this group is a real three-dimensional spherical space on which the group elements, taken as linear operators, act as so-called Clifford screws. Only invariants of relative position in this space can serve as parameters on which the interaction of urs can depend. Hence this space must be identified with position space.

Our postulates thus force us to assume that position space (i.e., what we usually call 'space', or 'cosmic space') is a three-dimensional real spherical space. I take this to be the reason for the three-dimensionality of space; a quantum theory of ultimate objects with $n = 2$ would not admit of another invariant description.

We will now have to draw the consequences of our postulates for the three theories 1, 3, and 5.

(3 and 5) 'Elementary particles' must be built up from ultimate objects. The urs would be something like elements of a Heisenberg-type *Urfeld.* My attempts in this direction have so far not been successful enough to justify a description. I can merely say that this elementary particle theory is cosmological from the outset. The curvature of space signifies a quantization of momentum. The single ur corresponds to a particle with minimum momentum; it is therefore not localized in cosmic space. If we assume a radius of the world $R = 10^{40}$ nuclear units of length $= 10^{40} \times 10^{-13}$ cm, a particle of nuclear momentum or a particle localizable in a nucleus would have to consist of about 10^{40} urs. The total number of urs in the universe might tentatively be identified with the number of bits of information possible in the universe. Assuming for the sake of simplicity that there is just one sort of elementary particle, say a nucleon with Fermi statistics, one would estimate that there are as many bits in the world as there are cells of nuclear size, each of which can be either occupied or empty. This number is $N = R^3 = 10^{120}$. The number of dimensions of the Hilbert space of the universe would then be $n_u = 2^N$. If one nucleon consists of R urs, there ought to be

$R^2 = 10^{80}$ nucleons in the world. I do not yet dare to take the good agreement of this figure with experience[34] as the confirmation of a hypothesis that has not been sufficiently elaborated.

The cut-off used in limiting the total number of urs (or better: which represents their finiteness in space) must not simply be interpreted as an 'elementary length'. The precision with which we can determine a length depends on the momentum involved in the measurement; i.e., in this theory, on the number of urs present in it. If we wish to localize all nucleons of the world simultaneously, each one is assigned 10^{40} urs and thus occupies 10^{-13} cm. If we wish to measure a length more accurately than that, we would have to employ momenta corresponding to an energy higher than the rest energy of the nucleon. Assuming we use all urs in the world to measure one length, this might lead to an accuracy of 10^{-93} cm (10^{-120} times the world radius). In no case, however, will this theory describe space as a discrete lattice.

(1, 3, and 5) Elementary particle physics depends essentially on its symmetry groups. Their deduction from postulate M would have to be the goal of the present theory. This would require a model, which cannot be presented here, of how particles are built up of urs. But there seems to be a difficulty from the outset. The most generally accepted group in field physics is the Lorentz group; and the present theory is not Lorentz-invariant. Looking at its basic assumptions, Lorentz invariance was indeed not to be expected. We started with an absolute concept of time as one does in non-relativistic quantum theory. We have arrived at a cosmology with a static universe, and such a cosmological model in its turn is again not Lorentz-invariant. But it should not be difficult to find a law of interaction obeying postulate L which would make the theory locally Lorentz-invariant. One can take the view that this is all we should ask for. On the other hand, our theory will not be semantically consistent without a description of the measurement of time. It is possible that this consideration might induce us to change our basic assumptions such that the resulting cosmology would permit a group which contains the homogeneous Lorentz group; e.g., the de Sitter group. This question, which I leave unsolved here, leads us back to the approximate character of postulate J. I shall close here by formulating a last hypothesis:

N. Postulate of Expansion. *In second approximation the universe can be described as consisting of ultimate objects whose number increases in time.* This formulation takes account of the objections I raised in commenting on postulate J. It expresses the open future: if we wait long enough, we will be able to divide up a line very finely. It also takes certain cosmological objections into account.

[34] 10^{80} nucleons in 10^{120} elementary cells corresponds to an average of 10^{-40} nucleons per cell, or 10^{-1} nucleons per cubic cm, which agrees, very roughly, with the density of cosmic matter.

(1 and 5) It would seem strange for a theory built on principles of very high generality to contain a basic constant whose value is contingent: the number N of urs in the universe, $N = 10^{120}$. According to the new postulate, N would now be a measure of the age of the universe. If we measure this age t by ordinary clocks in nuclear time units, then, presumably, $N = t^3$. This formula is derived from the answer to a second objection: according to postulate J, the radius of the world should be a constant, while astronomical evidence would favour the equation $R = t$. In fact, in the present theory, the expansion of the universe is necessarily connected with the creation of matter by the formula $N = R^3$. The theory thus resembles the cosmology proposed by Dirac[35] and Jordan.[36]

A final word must be said on gravitation. The theory considers the topology of space at large to be a consequence of quantum theory. More precisely it considers the finitude of space as a natural expression of the finitude of experience. On the other hand, the empirical connection between the curvature of space and the cosmic density of matter is sufficiently close to that demanded by Einstein's equation to seem more than accidental. As a possible explanation I mention the idea that it is not the curvature of space that adapts to a given density of matter with a given constant of gravitation; instead, this so-called constant adapts to a given density of matter and curvature of space. Eddington's famous relation, expressed in our theory, says that the constant of gravitation, measured in nuclear units, is $g = R^{-1}$. A time dependence of g has been discussed by Jordan; and P. Sexl has pointed out to me that in some theories gravitation is a many-body force, in which case the constant g will appear to depend on the present density of matter.

In the present theory one may expect the following theory of gravitation. Elementary particle physics will lead to fields with many different transformation properties; among these will also be a field behaving like the gravitational field (probably containing scalar as well as tensor components). According to Thirring (see section b), a semantically consistent theory of this field will couple it with the metric tensor. This equivalence of gravitation and metric implies that our original assumption of a curved cosmic space says merely that there is a universal gravitational field. Einstein's theory of gravitation still contains one apparently contingent element: the value of the gravitational constant. The appearance of this constant is due to a dichotomy that Einstein never desired to retain but was unable to overcome: the dichotomy between matter and the metric field.[37] He had to describe their mutual dependence as some sort of interaction. The influence of the metric field on matter he was able to describe in a way that avoided contingent constants by requiring matter to move along geodesies. The influence of matter on the metric field, however, took the form of a Poisson equation with a non-dimensionless constant factor. One would expect that in a semantically consistent theory this constant would reduce to a dimensionless factor whose nature is

[35] See Dirac (1938).

[36] See Jordan (1937/1955).

[37] Cf. ii.1.c.iii of von Weizsäcker (1980).

determined by the theory. If we measure all quantities referring to matter in nuclear units, g turns out empirically to be R^{-1}. In the present theory this is precisely what we would expect, once the identity of gravitation and metric has been accepted. So long as we neglect local gravitational fields and confine ourselves to the cosmology presented in this section, the relation follows directly from the estimate of N; i.e., from the amount of information present in the universe. In the language used here, Einstein's equation as applied to cosmology *means* simply that there is coupling between the amount of matter and the curvature of space ($N = R^3$), which follows from the fact that the total volume of cosmic space, measured in nuclear units, *is* the number of bits in the world, and that ultimate objects are simply bits. Local gravitational fields are not yet contained in this description since they must be linked with the general theory of 'elementary' fields. If we accept Thirring's gauge transformation, however, we may expect that matter behaves locally as if it were part of a homogeneous universe with a metric determined by the local values of the metric field. In such a universe the density of matter would be different from the actual cosmic average, and precisely this connection between the mass tensor and the metric field would be expressed by Einstein's equation.

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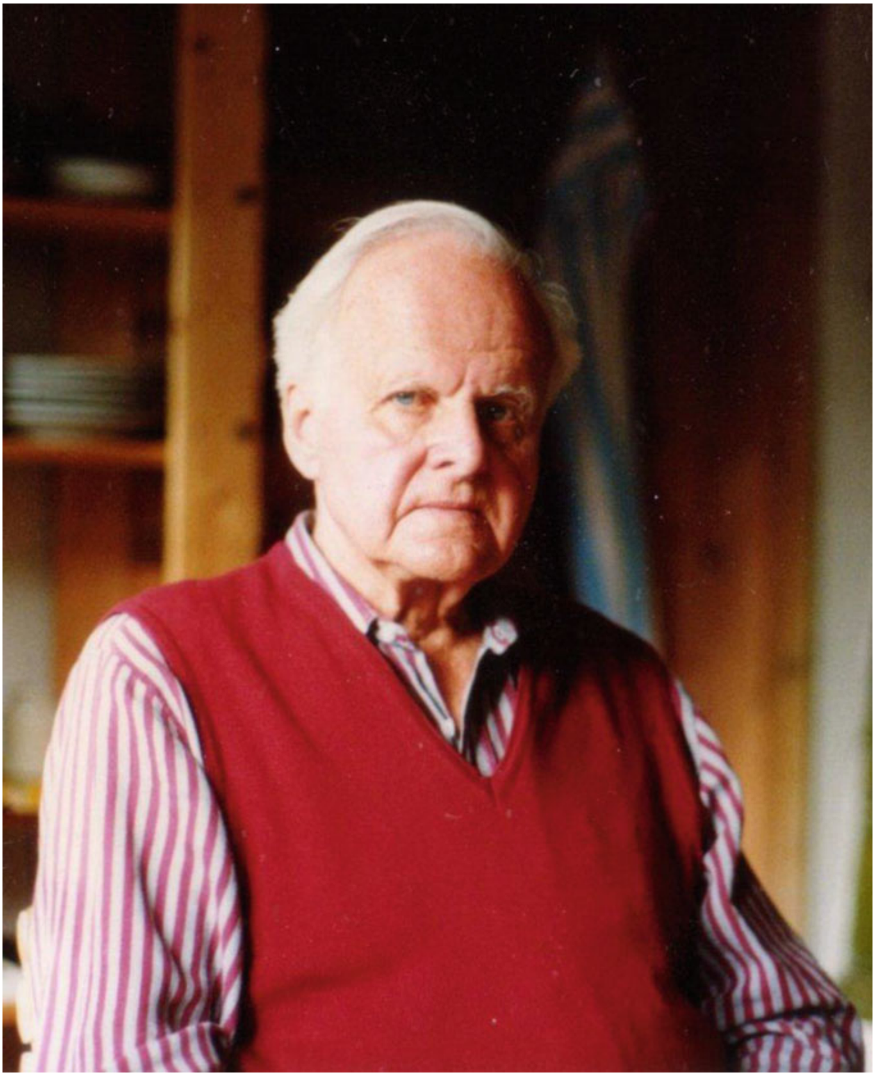

Prof. Dr. Carl Friedrich von Weizsäcker (1992, Griesser Alm). © Lili Bansa who granted permission to use this photo

Chapter 8
Probability and Abstract Quantum Theory

8.1 Probability and Experience

In memoriam Imre Lakatos

This and the following section are from my essay "Probability and Quantum Mechanics" (1973).[1] Imre Lakatos had read this work in the last years of his life, when he was a member of the Scientific Council of the Max Planck-Institute in Starnberg. He viewed it as an example of a 'rational reconstruction' and had it published in the British Journal for the Philosophy of Science, 24, 321–337. I dedicate these sections to his memory.

The theory of probability had its origin in an empirical question: Chevalier de Mere's gambling problem. Equally, the present-day physicist finds no difficulty in empirically testing probabilities which have been theoretically predicted, by measuring the relative frequencies of the occurrence of certain events. On the other hand, the epistemological discussion on the meaning of the application of the so-called mathematical concept of probability is by no means settled. The battle is still raging between 'objectivist,' 'subjectivist,' and even other interpretations of probability. Probability is one of the outstanding examples of the "epistemological paradox" that we can successfully use our basic concepts without actually understanding them. In many apparent paradoxes associated with fundamental philosophical problems, the first step toward their solution consists in accepting the seemingly paradoxical situation as a phenomenon, and in this sense as a fact. Thus we must understand that it is the very nature of basic concepts to be practically useful without, or at least before, being analytically clarified. This clarification must use other concepts in an unanalysed manner. It may mean a step forward in such an analysis to see whether a hierarchy exists in the practical use of basic concepts, and which concepts then practically depend on the availability of which other concepts, and also to see where concepts interlink in a non-hierarchical manner. I will try to show that one of the traditional difficulties in the

[1] This chapter was first published in: *British Journal for the Philosophy of Science,* 24 (1973): 321–337; it was republished in: *The Structure of Physics* (Heidelberg: Springer, 2006), which is a translation of: Aufbau der Physik (Munich: Hanser, 1985): 100–111.

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empirical interpretation of probability stems from the idea that experience can be treated as a given concept and probability as a concept to be applied to experience. This is what I call a mistaken epistemological hierarchy. I will try to point out that, on the contrary, experience and probability interlink in a manner that will preclude understanding experience without already using some concept of probability. I will offer a particular way to introduce probability in several steps.

We will interpret the concept of probability in a strictly empirical sense. We consider probability to be a measurable quantity whose value can be empirically tested, much like, for example, the value of an energy or temperature. What we need for defining a probability is an experimental situation in which different "events" E_1, E_2.. are the possible results of one experiment. We further need the possibility of meaningfully saying that an equivalent experimental situation (in short "the same situation") prevails in different cases (in different "realizations," "at different times," "for different individual objects" etc.) and that, given this situation, an equivalent experiment (in short "the same experiment") is carried out in each case. Let there be N performances of the same experiment, and assume that event E_k occurred n_k times. In this series of cases we call the fraction

$$f_k = \frac{n_k}{N}$$

the relative frequency with which event E_k occurred in the series. Now consider a future series of performances of the same experiment. Let us assume that our (theoretical and experimental) knowledge enables us to calculate a probability p_k of the event E_k in this experiment. Then we will take the meaning of this number p_k to be that it is a prediction of the relative frequency f_k for the future series of performances.[2] Finally, p_k will be empirically tested by comparing it with the values of f_k found in this and subsequent series of the experiment under consideration.

This is the simplistic view of the ordinary experimentalist. I think it is essentially correct and it will only need to be defended against the objections of the epistemologists. Of course we hope to understand it better by defending it.

Let us use a simple example in formulating the main objection. Our experiment will consist in the single cast of a die. There are six possible events. Let us choose the event that a "5" appears as the event of interest. Its probability p_5 will be 1/6 if the die is "good." Now let us cast the die N times. Even if N is divisible by 6, the fraction f_5 will only rarely be exactly 1/6, and, what is more important, the theory of probability does not expect f_5 to be 1/6. The theory predicts a distribution of the measured values of f_5 in different series of casts around the theoretical probability p_5. The probability is only the *expectation value* of the relative frequency. But the concept "expectation value" is generally defined by making use of the concept "probability." Hence it seems impossible to define probability by referring it to measurable relative frequencies, since that definition itself, if rigorously formulated,

[2] This formulation was proposed by Drieschner (1967).

would necessarily contain the concept of probability. It would, so it seems, be a circular definition.

We will not evade the problem by defining the probability as the limiting value of the relative frequency for long series, since there is no strict meaning to a limiting value in an *empirical* series which is essentially finite. These difficulties have induced some authors to abandon the 'objectivist' interpretation altogether in favour of a 'subjectivist' one which, e.g., reads the equation $p_5 = 1/6$ as meaning: "I am ready to lay odds of 1–5 that a 5 will come up next time." The theory of probability is then a theory of the consistency of a betting system. But this is not the problem of the physicist. He wishes to discover empirically who will become a rich man by his betting system. I am not going to enter into the discussion of these proposals,[3] but instead immediately offer my own.

The origin of the difficulty does not lie in the particular concept of probability but more generally in the idea of an empirical test of any theoretical prediction. Consider the measurement of a position coordinate x of a planet at a certain time; let its value predicted by the theory be ξ. A single measurement will give a value ξ_1, different from ξ. The single measurement may not suffice to convince us whether this result is to be considered a confirmation or refutation of the prediction. Thus we will repeat the measurement N times and apply the theory of errors. Let $\overline{\xi}$ be the average of the measured values. Then, comparing the distance $\left|\xi - \overline{\xi}\right|$ with the average scatter of the measured values, we can formally calculate a 'probability' with which the predicted value will differ from the 'real' value ξ_r ("ξ real") by a quantity $d = |\xi - \xi_r|$. This 'probability' is itself a prediction of the relative frequency with which the measured distance $\left|\xi - \overline{\xi}\right|$ will assume the value d, if we repeat the series of measurements many times. This structure of the empirical test of a theoretical prediction is slightly complicated, but well known. We can compress it into an abbreviated statement: "An empirical confirmation or refutation of any theoretical prediction is never possible with certainty but only with a greater or lesser degree of probability." This is a fundamental feature of all experience. Here I am satisfied to describe it and to accept it; its philosophical relevance is to be discussed in another context.[4] Whoever works in an empirical science has already tacitly accepted it by his practice. In this sense the concept of scientific experience in practical use presupposes the applicability of some concept of probability, even if this concept is not explicitly articulated. Hence the very attempt to give a complete definition of probability by recurring on a given concept of experience is likely to result in a circular definition. Of course it would be equally impossible to define the concept of an empirical test by using a preconceived concept of probability. These two concepts, experience and probability, are not in a relationship of hierarchical subordination.

In practice every application of the theory of errors implies that we consider relative frequencies of events to be predictable quantities. In this sense probability

[3] Cf. Weizsäcker (1992): I 4.

[4] Cf. Weizsäcker (1992): I 3 and I 4.

is a measured quantity. This implies that our "abbreviated statement" also applies to probability itself: The empirical test of a theoretical probability is only possible with some degree of probability. The appearance of the probabilistic concept of an expectation value in the 'definition' of probability is therefore not a paradox but a necessary consequence of the nature of the concept of probability; or it is a 'paradox' inherent in the concept of experience itself. Still, probability is not on the same methodological level as all other empirical concepts. The precise measurement of any other quantity refers us to the measurement of relative frequencies, that is, to probabilities; the precise measurement of probability refers us to probabilities again. Due to this higher level of abstraction the predictions of the theory are better defined. The scatter of the measured values of any quantity about its average value depends on the nature of the measurement device; the scatter of relative frequencies about their expectation values is itself defined by the theory.

8.2 The Classical Concept of Probability

We have yet to achieve a definition of probability that can avoid the objection of being circular. I will now sketch a systematic theory of probability as an empirical concept, i.e., a concept of a quantity which can be empirically measured. This is not a rigorously developed classical theory of probability, but a sketch for an analysis of its concept of probability that emphasizes aspects of the theory where epistemological difficulties usually arise. I hope that this analysis will suffice for the construction of a consistent classical theory of probability, where for the mathematical details we might use any good textbook. The word 'classical' means here only "not yet quantum theoretical."

This is done in three stages. We first formulate a "preliminary concept" of probability. It does not aim for precision; it is meant to describe in comprehensible terms how probability concepts are actually used in practice. Secondly we formulate a system of axioms of the *mathematical theory* of probability. In this section we can adopt Kolmogorov's system. Thirdly we give empirical meaning, *physical semantics,* so to speak, to the concepts of the mathematical theory by identifying some of its concepts with some concepts associated with the preliminary concept of probability. This three-step procedure can also be described as a process of giving mathematical precision to the preliminary concept. The most important part of the third stage is a study of the consistency of the whole process. The interpreted theory of the third stage offers a mathematical model of those structures which were imprecisely described in the preliminary concept. I propose to call a theory *semantically consistent* if it permits one to use the preliminary concepts without which it would not have been given meaning in such a manner that this use is correctly described by the mathematical model offered in the theory itself.[5] The *preliminary concept* is described by three postulates:

A. A probability is a predicate of a formally possible future event or, more precisely, a modality of the proposition which asserts that this event will happen.
B. If an event, or the corresponding proposition, has a probability very near to 1 or 0, it can be treated as practically necessary or practically impossible. A proposition (event) with a probability not very near to 0 is called possible.
C. If we assign a probability p $(0 < p < 1)$ to a proposition or to the corresponding event, we thereby express the following expectation: out of a large number N of cases in which this probability is correctly assigned to this proposition there will be approximately $\mathrm{n} = pN$ cases in which the proposition will turn out to be true.

The language in which we formulated these postulates needs further explanation. We first see that restrictive concepts like 'practically,' 'approximately,' "expressing an expectation" are used. Their task is to indicate that our preliminary concept is not precise but should be made more precise. We will see that in this process these restrictive concepts will not be eliminated but be made more precise themselves. The word "correctly" in *C* indicates that we consider ascribing a probability to an event not an act of free subjective choice, but a scientific assertion subject to test.

The language of the postulates refers to the logic of temporal propositions. For propositions about the future this logic proposes not to use the traditional truth values 'true' and 'false,' but the "future modalities": 'possible,' 'necessary,' 'impossible.' The postulate proposes to use probabilities as a more precise form of future modalities. With respect to the ordinary use of the word 'probability' this can be considered a terminological convention; further on, we wish to restrict the use of this word to statements about the future. But behind this convention lies the view that this is the primary meaning of probability and that other uses of the word can be reduced to it. For example, we apply it to the past in saying "it is probable that it was raining yesterday" or "the day before yesterday it was probable that it would rain the following day." But in the second example probability is referred to what was then the future; characteristically we say here "it *was* probable." In the first example we first of all admit lack of knowledge concerning the past; to make the statement operative we must apply it to the future in the sense "It is probable that, if I investigate, I will find out that it was raining yesterday."

For the *mathematical theory* we can adopt Kolmogorov's text literally, changing only some notation:

> Let M be a set of elements ξ, η, ζ … which we call ELEMENTARY EVENTS, and F a set of subsets of M; the elements of F will be called EVENTS.

[5] Cf. v. Weizsäcker (2006), 9.2 and v. Weizsäcker (1992), 5.2.7.

I. F is a [Boolean[6]] lattice of sets.
II. F contains the set M.
III. To every set A of F we assign a non-negative number p(A). This number p(A) is called the probability of the event A.
IV. $p(M) = 1$
V. If A_1 and A_2 are disjoint, then $p(A_1 + A_2) = p(A_1) + p(A_2)$.

We leave out axiom VI, which formulates a condition of continuity, since we will not discuss its problems here. We need, however, the definition of the expectation value:

Let there be a partition of the original set M

$$\mathrm{M} = \mathrm{A}_1 + \mathrm{A}_2 + \cdots + \mathrm{A}_R,$$

and let **x** be a real function of the elementary event ξ which is equal to a constant A_Q on every set A_Q. Then we call X a STOCHASTIC QUANTITY and consider the sum

$$E(x) = \sum a_q p(A_q)$$

the mathematical expectation of the quantity X.

We now turn to the *physical semantics*. In order to simplify the expression and concentrate on the essentials, we assume the set M of elementary events to be finite. We call the number of elementary events K; in the case of the die $K = 6$. We further consider a finite *ensemble* of N equivalent cases, e.g., of casts in the case of die. To every elementary event E_k (we write E_k instead of Kolmogorov's ξ; $1 < k < K$) we assign a number (nk) which indicates how many times this event E_k (say the "5" on the die) has actually happened in the particular series of N experiments which forms our given ensemble. Correspondingly we assign a $n(A)$ to every event A. It is easy to see that the quantities

$$f(A) = \frac{n(A)}{N} \tag{8.1}$$

satisfy Kolmogorov's axioms I to V if we insert them for $p(A)$. This model of the axioms is, however, not the one intended by the theory of probabilities, but we reach our goal by adding a fourth postulate to the preliminary concept:

D. The probability of an event (of a proposition) is the expectation value of the relative frequency of its happening (its coming true).

The expectation value used in D is not defined on the original lattice of events F. It can be defined on a lattice G of 'meta-events.' We call a meta-event an ensemble of N events belonging to F which happen under equivalent conditions. We here use the language that the 'same' events can happen several times ("it has been raining and it will be raining again"). G is not a subset of M or F, but it is a set of elements

[6] Added by the *ed.MD*.

of F with repetitions. Now we can assign a probability function $p(A)$ to F (it may express our expectation of the events A according to the preliminary concepts). Then the rules of the mathematical theory of probability permit us to *calculate* a probability function for the elements of G; it is only necessary to assume that the N events which together form a meta-event can be treated as independent. Assuming the validity of Kolmogorov's axioms for F we can then *prove* their validity for G and the validity of the formula

$$p(A) = E\left(\frac{n(A)}{N}\right). \tag{8.2}$$

We can now forget our preliminary ideas of the meaning of the $p(A)$ in F. Instead we can apply the three postulates A, B, C to the lattice G of meta-events. After having thus given an interpretation (in the preliminary sense) to the p in G, we use (8.2) to deduce an interpretation of the p in F. It is exactly what postulate D says: $p(A)$ is the expectation value of the relative frequency of A. If we now remember again how we would have interpreted the p in F without this construction, we would only have used A, B, C This preliminary concept is now justified as a weaker formulation of D. The concepts 'practically,' 'approximately,' 'expectation' can now be more precisely interpreted by estimating probable errors. The mathematical "law of large numbers" proves that the expectation values of these errors tend to zero as N increases.

What have we gained epistemologically? We have not gotten rid of the imprecise preliminary concepts, we have merely transferred the lack of precision from events to meta-events, i.e., to large ensembles of events. The physical semantics for probabilities rest on the preliminary semantics for meta-probabilities. This is a more precise expression of our earlier statement, that a probability can only be empirically tested with some degree of probability. The solution of the paradox lies in its acceptance as a phenomenon; no theory of empirical probabilities can be meaningfully expected to yield more than this justification, which at least makes its consistency more evident.

If we wish, we can iterate our process and call this ladder of meta-probabilities a "recursive definition" of probability. While a typical recursive definition offers a fixed starting point ($n = 1$) and a rule of recursion from $n + 1$ to n, the recursion here can go as high as we like. At some step of the ladder we must halt and rely on the preliminary concepts. Due to the "law of large numbers" it will suffice for this highest step to postulate A and B. This will yield A, B, and C for the next lower step, and D for the ones below that.

8.3 Empirical Determination of Probabilities

We distinguish the *probability of an event* from the *probability of a rule,* but assume, however, (in contrast to Carnap's 1962[7]) that the two quantities are of exactly the same nature but at different levels of the application. The probability of an event x is the prediction (the expectation value) of the relative frequency $f(x)$ with which an event of this type x will occur, upon frequent repetitions of the experiment in which x can happen. The content of a rule (an "empirical law of nature") is the specification of probabilities of events. Rules always specify *conditional* probabilities: "If y then x will occur with the probability $p(x)$." But in the same way, by the conditional probability one also means the probability of an event. After all, the relative probability can only be measured if "the same" experiment is always performed, i.e., if equivalent conditions are realized. One can say that empirically testable probabilities are by nature conditional probabilities. The probability of a rule is now meant as the probability that the rule is 'true.' An empirical rule is true if it proves itself in experience. Its probability is then the prediction of the relative frequency with which just that rule R proves itself, upon frequent repetitions of the same empirical situation for testing that rule. This initially formal definition we have only to elucidate in detail, and we naturally arrive at an interpretation of Bayes' rule. What we are looking for can simply be called an iterated probability $P(p(x))$. In *Zeit und Wissen* II.4.4b[8] we will see that one does better to speak of a "higher-order probability" $P(f(x))$. For the present discussion such finesse does not matter.

One can (and in general will) approach the empirical determination of a probability from a starting point that expresses the *prior knowledge.* Methodologically we remember that an objective, empirically testable probability is at the same time related to the prior knowledge of a subject. As an example we choose two dice, each successively thrown once. Observers A and B are to state the probabilities for obtaining a 12, A before the double throw, B, however, after the first die had been cast. A gives $p(12) = 1/36$, B on the average in one sixth of the cases $p(12) = 1/6$, on the average in five sixths of the cases $p(12) = 0$. Empirically both are correct as they refer to different statistical ensembles due to different prior knowledge.

The starting point for the desired rule expresses what one already knows before the test series. For simplicity let us first assume that one can describe the setup conceptually, but has never experimented with precisely this realization of the concepts. For example, one might be about to throw 'heads' or 'tails' with a coin or a "1…6" with a die, or to draw a ball from an urn containing w white and b black balls. Here is the legitimate application of Laplace's concept of *equal possibilities,* i.e., a *symmetry argument.* One knows which 'cases' are possible,

[7] Cf. Weizsäcker (1992): II.4.3.

[8] Cf. Weizsäcker (1992): II.4.4b.

i.e., one knows the catalog of all possible events. One does not know what would distinguish one of the elementary events (the atoms of the lattice of events) from any other. In this sense they are all equally possible. *Therefore* one assesses them to be all equally probable, i.e., one predicts an equal relative frequency for their occurrence. The empirically motivated assumption of symmetry is at this phase of the experiment essentially an expression of ignorance. This is the legitimate meaning of Laplace's approach, as subsequent investigation of the experiment will show.

At any rate, certain relative frequencies will be found in this experiment. Roughly speaking, we can distinguish three cases:

(a) relative frequencies arise that are consistent with the starting point
(b) relative frequencies arise which correspond to a different starting point
(c) relative frequencies arise which do not correspond to any uniform statistical distribution

The phrase "are consistent with" means "in agreement with the expected distribution," within the limits of error the observer has set himself, using the calculus of probability. For the observer there is essentially no certainty, only a probability he can assign to each of the available scenarios; how he does this we will discuss shortly in more detail in connection with Bayes' problem. That the frequencies correspond at all to a unique starting point is by no means self-evident. This is indicated by the listing of case c. In this case one suspects that the catalog of events needs to be expanded to bring conditions into view which do not vary statistically but systematically. In view of these possibilities, cases *a* and *b* are hardly self-evident, and one could ask by what right they can be expected to occur at all. At the present stage of our epistemological examination we can only recognize Hume's problem in this difficulty and reply that according to our present understanding the occurrence of regular statistical distributions is a condition for the possibility of experience. At a later stage we will recognize in Laplace's symmetries basic symmetries of the world, namely in the equal probabilities of the sides of a uniform coin or a uniform die representations of the group of spatial rotations, realized in objects with negligible interaction with their environment, and a representation of the group of permutations of objects in the equal probabilities for picking any of the balls in the urn. There we must justify, through a discussion of the interaction, why the inclusion of new objects cannot remove the symmetry of the world under this group, such that every deviation of individual objects from the symmetry only stems from their individual interaction with other objects.

The classical model of Bayes' problem involves several urns (say 11) with different mixing ratios of black and white balls (say in the zeroth urn 0 white and 10 black balls, in the kth urn k white and $10 - k$ black ones). In drawing from each urn we assume with Laplace equal probabilities; each of the urns is thus characterized by the probability p_w of drawing a white ball and p_b of drawing a black ball. According to our assumptions we have for the kth urn

$$p_w(k) = \frac{k}{10}, \tag{8.3}$$

and always

$$p_w + p_b = 1. \tag{8.4}$$

Now one picks one of the urns, without knowing which, and proceeds to draw and immediately return a single ball, n times in succession. If the outcome was n_w white and n_b black balls ($n_\mathrm{w} + n_\mathrm{b} = n$), how probable is it that this had been the kth urn? In other words, one then determines a probability P_k. One can interpret P_k as a prediction of a relative frequency in a twofold way. On the one hand, P_k is, according to Laplace's assumption applied to the selection of one urn, the predicted relative frequency with which a particular urn turns out to be the k-th one *if* just n_w white and n_b black balls have been drawn from the urn. On the other hand, P_k enables one to predict new probabilities $p_w{}'$ and $p_b{}'$ for subsequent drawing from the urn, according to the formulas

$$p_w' = \sum_k P_k \cdot p_w(k), \tag{8.5}$$

$$p_w + p_b = 1. \tag{8.6}$$

Before the start of the experiment, according to Laplace's assumption, one would set each P_k to 1/11 for the selection of one urn, and compute from it the 'a priori probabilities' $p_w^{(0)}$ and $p_b^{(0)}$, which in our case would both be ½. The test series of drawing n balls is then the empirical determination of newer, i.e., 'a posteriori probabilities'. Bayes' procedure thus assigns to each of the 11 possible rules (8.3) a probability of the rule, P_k, and determines the probabilities $p_w{}'$ proposed for the practical usage from the probabilities *according* to the rule ($p_w(k)$) and the probability of *the rule* P_k according to (8.5).

Bayes' procedure thus corrects an initially assumed equipartition by means of insight into possible cases leading to different rules, for which again an equipartition is assumed. Naturally this can also be modified. One can introduce unequal a priori probabilities for picking one urn. This again can be reduced to an equipartition by assuming different numbers of urns of each type. The practical value of the procedure depends on the fact that for large values of n the influence of the assumed a priori probabilities gradually disappears. With an ontological assumption that all phenomena are built from equally possible elementary events one can thus even further justify the empirical determination of probabilities. Without such an assumption one can still describe the empirical determination "as if" such an assumption were justified; we need the assumption to *count* cases, and thereby are able to *define* in this way absolute, as well as relative, frequencies.

8.4 Reconstruction of Abstract Quantum Theory, Methodological Aspects

The title of this section initially suggests three questions:

1. What is meant by reconstruction?
2. What is meant by abstract quantum theory?
3. What ways are there for a reconstruction of abstract quantum theory?

8.4.1 The Concept of Reconstruction

I mean by reconstruction its retrospective derivation from the most plausible postulates. I articulate, as I have done before, the difference between two kinds of such postulates. They may either express conditions which make experience possible, thus conditions of human knowledge; then we call them *epistemic*. Or they formulate very simple principles which we hypothetically, inspired by concrete experience, want to assume as universally valid for the particular area of reality; we call these postulates *realistic*.

I emphasized in the first chapter of my 'Structure of Physics' that my method of a *Kreisgang*[9] does not permit a completely sharp distinction between these two kinds of postulates. I merge in the *Kreisgang* two traditions of thought which, in the history of philosophy, were in hostile opposition most of the time. All our knowledge of nature is subject to the conditions of human knowledge; that is the epistemological question. Humans are children of nature and their knowledge itself is a process in nature; that is the evolutionary question. Even our evolutionary knowledge is, as human knowledge, subject to the conditions of such knowledge, as studied in epistemology. The back of the mirror,[10] we only see in the mirror as well. But the mirror in which we see the back of the mirror is also just the mirror with this rear surface; epistemology, like the cognition it investigates, is also an event in nature. In this way every epistemological postulate is at the same time a statement about a process of nature, and every realistic postulate is formulated subject to the conditions of our knowledge.

The historical phenomenon that there are closed theories, however, permits us a relative distinction of epistemic and realistic postulates, as regards a particular

[9] Weizsäcker chose the word 'Kreisgang' to characterize his overall philosophical method. The term is difficult to translate (and is not a common German notion, either). It is used as a technical term throughout Weizsäcker 2006. In its literal meaning it refers to a 'circular movement' of knowledge and cognition. The largest circle possible is captured by Weizsäcker's often used phrase: "Nature is older than humankind, humankind is older than natural science", which should indicate that there is no hierarchy of cognition, but that it is necessary to circle back to former insights again and again, every time on a new level of knowledge. *Eds. of v. Weizsäcker (2006) and of this volume.*

[10] Lorenz (1978).

theory. “Only theory decides what can be measured” (Einstein to Heisenberg[11]). We will begin the reconstruction of quantum theory with *one* postulate which, for quantum theory, is epistemic: the existence of separable, empirically decidable *alternatives.* An alternative characterized in this way expresses the quantum theoretical concept of an observable, reduced to its logical foundation. The fact that quantum theory is so successful, and that one is able to succeed with the concept of the alternative in the totality of physical experience known to us, is an empirical fact which a priori does not appear to be certain. In this sense the postulate of alternatives is realistic, but it is also epistemic in another sense. First, as just noted, it is epistemic in the context of quantum theory: it formulates a condition without which the concepts of quantum theory are inapplicable. But second also as a matter of principle: we scarcely can imagine how scientific knowledge might be possible at all without separable, empirically decidable alternatives. The high degree of generality of quantum theory thereby confers upon its basic postulate a position reminiscent of Kant’s perception a priori: *that* experience is possible at all we cannot know a priori; we can only know what circumstances must obtain *in order for* experience to be possible.

However, the *second* central postulate for the actual quantum theory, which we call the postulate of *expansion* or *indeterminism,* must also be considered *realistic* in the context of quantum theory. We cannot imagine a theory of probability predictions about decidable alternatives in which this postulate is not applicable. This question, however, we can only discuss after the reconstruction is accomplished.

8.4.2 Abstract Quantum Theory

Terminologically we distinguish abstract and concrete quantum theory. One can characterize abstract quantum theory by means of four *theses.* We use the concept of ‘thesis’ to distinguish it from the reconstructive concept of ‘postulate.’ These could be at the foundation of a formally axiomatic deduction of the theory. But they cannot claim to be ‘evident’ as we require it of the postulates. Rather, their explanation is the goal of our reconstruction.

A. *Hilbert space.* The states of every object are described by one-dimensional subspaces of a Hilbert space.
B. *Probability metric.* The absolute square of the inner product of two normalized Hilbert vectors x and y is the conditional probability $p(x, y)$ of finding the state belonging to y, if the state belonging to x is present.

[11] cf. Heisenberg (1971).

C. *Composition rule.* Two coexisting objects A and B can be considered to be a composite object $C = AB$. The Hilbert space of C is the tensor product of the Hilbert spaces of A and B.
D. *Dynamics.* Time is described by the real coordinate t. The states of an object are functions of t, described by a unitary mapping $U(t)$ of the Hilbert space onto itself.

We call this theory abstract because it is universally valid for any object. One example of an abstract theory is classical point mechanics. There is an equation that characterizes the universally valid law of motion for arbitrary numbers n of mass points with arbitrary masses m_i, and arbitrary force laws $f_{ik}(x_1 \ldots x_n)$. Von Neumann's quantum theory is even more abstract, as it does not presume the concept of a point mass and the existence of a three-dimensional configuration space. These concepts enter into quantum theory itself only via the special choices of the dynamics and the selection of certain observables associated with the dynamics. They belong to the *concrete* theory of specific objects.

8.5 Reconstruction via Probabilities and the Lattice of Propositions

This reconstruction path was chosen by Drieschner (1967) and described later (Drieschner 1979) in improved form. It follows most closely Jauch (1968) and the usual axiomatic theories; it goes beyond these in the way of the justification and the choice of postulates thereby implied. The reconstruction is sketched here to facilitate the connection to existing axiomatic quantum theories. This offers the opportunity to explain the abstract basic concepts within a familiar context.[12]

8.5.1 Alternatives and Probabilities

Physics formulates probability predictions about the outcome of future decisions of empirically decidable alternatives. The concept of probability is described in sections 1–3. Here, however, we will replace axiom I of Kolmogorov by another one; the catalogue of events is not the lattice of the subsets of a set.

We describe all possible observations as decision of n-fold alternatives. There n means either a natural number >2 or the denumerable infinite. An n-fold alternative represents a set of n formally possible events which satisfy the following conditions:

[12] We follow the layout and sometimes the wording of the presentation in Weizsäcker (1980): II.5.4. *Addendum by the ed.MD*: That presentation is reprinted in this volume as "Quantum Theory", part d, because it differs in many details from the present one.

1. The alternative is *decidable,* i.e., a situation can be created in which one of the possible events becomes an actual event and subsequently a fact. We then say that this event has happened.
2. If an event e_k ($k = 1 \ldots n$) has happened then none of the other events e_j ($j \neq k$) has occurred. The results of an alternative are *mutually exclusive.*
3. If the alternative has been decided and all events except one, thus all e_j ($j \neq k$), have not occurred, then the event e_k has happened. The alternative is defined as being *complete.*

Note about the nomenclature. Probabilities can be considered to be predicates of possible *events* or of *propositions.* About the philosophical interpretation of the difference between the two expressions see *Zeit und Wissen*[13] II 4. Here in this sketch we use both expressions indiscriminately; sometimes the one, sometimes the other is more convenient. This leads to the following expressions:

An alternative is a set of either *events* or of *propositions.* Both we call its *elements.* An event consists of the determination of a formally possible (conditional) *property* of an object at one time. Instead of this we also say that the object is at this time in a certain *state.* The word 'state' is in this context not restricted to "pure cases." The *proposition* which asserts the existence of a property or state is formulated in the *present* tense. This means that one can often decide "the same" alternative. An alternative can also be referred to as a *question;* the propositions are then its possible *answers.*

8.5.2 Objects

The elements of an alternative consist of the determination of formally possible properties of an object at one time.

We introduce the 'ontological' concept of an object in addition to the 'logical' concept of an alternative. The alternatives for an object are, speaking quantum theoretically, its observables. We follow here the mode of thinking customary in all of physics, in particular in quantum theory, which interprets all its catalogues of propositions as propositions about, respectively, an 'object' or a 'system.' These two words are practically synonymous in contemporary physics. 'Object' is perhaps the more general concept as it encompasses composite as well as the possibly existing elementary objects, whereas the word 'system' is more indicative of compositeness (systema, standing together). In this chapter we will therefore in general choose the term 'object.'

In the reconstruction we need the concept of the object to define the lattice of propositions which in each case is determined as the lattice of propositions about a fixed object (or the properties of a fixed object).

[13] v. Weizsäcker (1992).

The concept of the object, however, contains a fundamental problem which we will discuss in Sect. 8.5.5.

8.5.3 Ultimate Propositions About an Object

For every object there ought to be ultimate propositions as well as alternatives whose elements are, logically speaking, ultimate propositions. As an ultimate (contingent) proposition about an object we define a proposition which is not implied by any other proposition about the same object.[14] In the quantum theoretical language this means that there are pure cases. Lattice-theoretically these ultimate propositions are 'atoms,' i.e., the lowest elements of the lattice; Drieschner (1979) therefore calls them atomic propositions. Drieschner argues for the postulate of the existence of atomic propositions from the requirement that it ought to be possible in principle to completely describe every object in terms of its properties.

8.5.4 Finitism

Drieschner (1967) introduced the postulate of finitism, which one might perhaps formulate thus: "The number of elements of an arbitrary alternative for a given object does not exceed a fixed positive number K which is characteristic of that object." In contrast, we have also admitted denumerably infinite alternatives in 5.1. Furthermore, Drieschner (1979) no longer requires finitism. The technical benefit of the finitism postulate is that it avoids mathematical complications of an infinite-dimensional Hilbert space in the axiomatic reconstruction of quantum theory. Philosophically, behind this is the observation that no alternative with more than a finite number of elements can actually be decided by an experiment.

For convenience we will use here only finite alternatives. For physics, the infinite dimensions of Hilbert space become indispensable if we wish to unitarily represent in it the non-compact transformation groups of special relativity. In other words, we need it for relativistic quantum theory. In that regard, the present chapter is restricted to non-relativistic quantum theory. In Chap. 4 of my (2006) I define the simplest objects, particles, as representations of relativistic transformation groups, following Wigner; thereby for every object $K = \infty$. The 'objects' of finitism, however, retain an assignable meaning as representations of the compact part of the group in finite-dimensional subspaces. We will then call them "sub-objects."

[14] With the trivial exception of the "always false proposition" $\bigwedge$, which, by definition, implies any proposition—*ex falso quodlibet*.

8.5.5 Composition of Alternatives and Objects

Several alternatives can be combined to a *composite alternative.* This is done by "Cartesian multiplication." Given N alternatives (N finite or perhaps denumerably infinite): $\{e^{\alpha}_{k}\}$ ($k = 1 \ldots n^{\alpha}$; $\alpha = 1 \ldots N$), a combined event means that an event from each alternative occurs (not necessarily simultaneously). This is an element of the combined alternative which has $n = \Pi_{\alpha}\, n^{\alpha}$ elements.

Now N objects also define a total object of which they are parts. The Cartesian product of any alternatives of the parts is an alternative of the total object. In particular, the product of ultimate alternatives of the parts is a ultimate alternative of the total object.

The concept of an object, as we now see, contains some sort of self-contradiction which one cannot eliminate without eliminating all of physics known to us, which is built upon the concept of the object. Objects are known to us only through their interaction with other objects, ultimately with our own body. Completely isolated objects, free of any interaction, are no objects at all to us. The Hilbert space of an object describes just the possible states of only this one object. The introduction of dynamics, as we will perform it afterward, i.e., of a Hamiltonian operator, describes the influence of a fixed environment on the object and, insofar as one considers the object to be composite, the interaction of its parts with one another. To describe its influence on the environment one must combine it with other objects, thereby forming an aggregate object. In the Hilbert space of the aggregate object, however, the pure product states, in which the individual objects are in a definite state, are a set of measure zero. But it is just these definite states in terms of which quantum theory describes the individual objects. It appears that quantum theory could be formulated only approximately, which, if the theory is correct, would practically never be exactly valid. In short, the feasibility of theoretical physics rests upon its character as an approximation.

The philosophical problems herein I have discussed in detail in previous essays.[15] Let us accept here the concept of an object in its common usage.

8.5.6 The Probability Function

Between any two states a and b of the same object there is defined a probability function $p(a, b)$ which gives the probability of finding b if a is necessary. The formulation and content of this postulate depended on the assumption that everything which can be said about an object in an empirically decidable way must be equivalent to the prediction of certain probabilities. The empirical verification of a proposition lies in the future at the time to which this proposition refers. About the future, however, only probabilities can be stated, which of course may

[15] Weizsäcker (1980): II.3.5, IV.6.4; Weizsäcker (1988): II.1.9.

approach the values 1 and 0, certainty and impossibility. The formulation of the condition in $p(a, b)$ by means of "if a is necessary" includes the case "if a is present," as a is then, due to the reproducibility, necessary in the future, as well as the case that one knows the necessity of a for other reasons.

The really strong assumption in this postulate remains inconspicuous in the above formulation, namely that this probability function assigns to each pair of states a and b the value $p(a, b)$ *independent of the state of the environment.* This means at the same time that the states of an object admit an "internal description," consisting only of its relative probabilities without reference to 'external' objects. How one can identify the respective states through observation, however, is then only determined in terms of the interaction of the object with its environment.

This strong assumption of independence is the form in which the *identity* of an object with itself expresses itself in this reconstruction, which ought to hold independently of its changing environment. Here is a specification of the concept of an object which we need for the reconstruction but which here we do not justify any further.

8.5.7 Objectivity

If a certain object actually exists, then an ultimate proposition about it is always necessary. This, too, is a strong statement. For its justification we refer to Drieschner (1979: 115–117). There it is described as being equivalent to the statement: "Every object has at any time as property a probability distribution of all its properties." The premise "If a certain object actually exists" is necessary, because in states of composite objects which are not product states of the partial objects no ultimate statement about such a partial object is necessary. We then say that this partial object does not actually exist in such a state.

We call this postulated fact the *objectivity* of the properties of actually existing objects. For an actually existing object there is always an ultimate proposition, independently of whether we know it, i.e., it must inevitably be found if one looks for it. In other words, when one says that an object actually exists, one means that in principle one can know with certainty something about the object. Knowledge is not "merely a subjective state of the mind." To know means, putting it tautologically, knowing that the known is as we know it. Here as well we refrain from following up on the philosophical implications of our assertion.

8.5.8 Indeterminism

To any two mutually exclusive ultimate propositions a_1 and a_2 about an object, there is an ultimate proposition b about the same object which does not exclude either of the two. Two propositions x and y exclude one another if $p(x, y) = p(y, x) = 0$.

This is the central postulate of quantum theory. Following Drieschner it is called here the postulate of indeterminism. Within the context of the reconstruction it turns out to be equivalent to, e.g., the principle of superposition formulated by Jauch (1968: 106). It is the 'realistic' fundamental postulate; for it is at least not immediately obvious that experience without this postulate is not possible.

We can denote this postulate equivalently by the more abstract term *postulate of expansion*. The connection between the two names is as follows. Every alternative of ultimate propositions is expanded through this postulate by ultimate propositions about the same object which are not elements of the lattice of propositions which form the original alternative. The expansion is here formulated as a requirement on the probability function, i.e., on predictions: there are always predictions which have neither the value of certainty nor impossibility. This is juxtaposed with the postulate of objectivity according to which there are always necessary predictions. Both always exist. The requirement is at the same time formulated universally: it holds for *any* pair of mutually exclusive ultimate propositions. It implies that there can be no probability assignment of the catalogue of propositions about any object whatsoever for which every proposition is either true ($p = 1$) or false ($p = 0$). It thus implies the openness of the future as a matter of principle.

8.5.9 *Sketch of a Reconstruction of Quantum Theory*

For the implementation of the reconstruction we refer to Drieschner (1979). Here we merely mention the most important steps.

The catalogue of propositions is constructed about an object. Negation, disjunction, and implication are defined in terms of obvious requirements on the probability functions, such that the catalogue proves to be a lattice, and, in fact, for the case of finitism, a modular lattice. It can be shown that, with the imposed requirements, it is even a projective geometry. This can be represented as the lattice of the linear subspaces of a vector space. There remains the question of the field of numbers in which the vector space is erected. As a real metric is defined in it by means of the probability function, the field of numbers must contain the real numbers. Following Stückelberg (1960) Drieschner concludes from the uncertainty relations that it specifically must be the field of complex numbers. The dynamics is to be described in it, i.e., the time-dependence of the state, in terms of transformations under which the probability function remains invariant. These must be unitary transformations. In this fashion, abstract quantum theory is reconstructed.

For the time being, we forgo any attempt to examine how close the individual postulates have come to the ideal of an epistemic justification.

8.5.10 Historical Remark

The first formulation of the ideas utilized here, in my version, is given in the work *Komplementaritat und Logik* (Weizsäcker 1955). To Drieschner's indeterminism axiom, there corresponds, for example, the "theorem of complementarity" (Sect. 6): "To every elementary proposition there are complementary elementary propositions." But only the work of Drieschner transformed this "complementary-logical" way of thinking, together with the axiomatic quantum theory of Jauch (1968), into a reconstruction of quantum theory. The goal of the present historical note is to point out the reconstruction of quantum theory previously begun by F. Bopp. Bopp's work of 1954 I quoted in 1955 (Sect. 5). It provided me with essential suggestions for the elaboration of my arguments at that time; see also his more recent work (Bopp 1983). Bopp begins, as we do in Sect. 4.1, with a simple alternative ("Sein oder Nichtsein als Grundlage der Quantenmechanik"). He postulates, as in Drieschner's uncertainty postulate, the existence of additional states defined in terms of relative probabilities and the continuity of this state space to make a continuous kinematics of the states possible. He, however, takes the spacetime continuum for granted and considers the alternative to depend on position ("ur fermion").

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Chapter 9
The Philosophy of Alternatives

Summer surprised us, coming over the Starnbergersee
With a shower of rain;

T. S. Eliot: *The Waste Land* (1923).

9.1 Introduction

With a certain degree of simplification one might say that theoretical physics in our century has produced two fundamental theories: a theory of space and time, comprising special and general relativity and cosmology, and a general theory on states of physical systems and their changes in time, which is quantum theory. Physicists hope to complete a third fundamental theory, which would permit us to deduce what kinds of elementary particles can exist.[1]

In the theory of elementary particles relativity and quantum theory are simultaneously applied, in the form of a relativistic quantum field theory. So far, the consistency of the theory has been evident only for free fields. Agreement has not been reached on the correct way of expressing interactions.

The chapters published in this volume were presented at a conference on "Quantum Theory and the Structures of Time and Space", held in July 1974 at Feldafing on the Starnberger See, under the auspices of the *Max-Planck-Institut zur Erforschung der Lebensbedingungen der wissenschaftlich-technischen Welt* in Starnberg. The invitations to the participants were motivated by the discovery that there were several groups in different countries which were searching for a new connection between relativity and quantum theory. This would go beyond the simultaneous application of both theories and would rather indicate a common origin of the two theories.

The most direct indication of this possible connection is perhaps given by the relevant symmetry groups and their homogeneous spaces. The space in which non-relativistic quantum theory defines its wave-functions is built up from the Euclidean three-dimensional coordinate spaces of the particles. Relativistic quantum theory is invariant under the Poincaré group, and for the simplest case of zero mass under the conformal group SO(4,2)/C2. In general relativity the conformal group has now gained importance by the use Segal made of it in cosmology

[1] This text was originally published in: *Quantum Theory and the Structures of Time and Space I* (Munich: Hanser, 1975): 7–9; 213–229.

M. Drieschner (ed.), *Carl Friedrich von Weizsäcker: Major Texts in Physics*, SpringerBriefs on Pioneers in Science and Practice 22,
DOI: 10.1007/978-3-319-03668-7_9,

and by Penrose's twistor theory. On the other hand the Hilbert space of the simplest possible quantum object, a binary alternative, admits the SU(2) which is one-two-isomorphic to the three-dimensional Euclidean rotation group, and the combination of two such objects, connected like particle and anti-particle, yields the SU(2,2) or SO(4,2) as its symmetry group. In Starnberg we consider this coincidence as not being accidental. We have been pursuing the idea that the mathematical structure of the space-time continuum has a quantum-theoretical origin. Hence we tried to bring physicists and mathematicians together who had worked independently on fields related to this problem.

In a less formalistic manner the problem is presented by the task of applying quantum theory to general relativity. In a Minkowski space gravitation would be a field like other fields, and one may try to quantize it. In general relativity gravitation is another word for the structure of a Riemannian space; to quantize it somehow means to quantize the structure of space. Finkelstein and McCollum consider the treatment of space and time in ordinary quantum theory as the classical limiting case of a quantum theory of the space-time continuum; the quantum theory of the simplest time-steps leads them back to the above mentioned quantum-mechanical deduction of the structures of time and space.

It cannot be maintained that the connection for which we are seeking has already become clear. We in the Starnberg group were most grateful that the participants came and offered their chapters and their criticism; we hope that at least they enjoyed the discussions. What we present in this volume are versions of the chapters some of which were readjusted after the discussions held at the conference.

9.2 Mathematical Background

We had a lot of mathematics in this symposium, and I am not going to add to it. I shall comment on its connection with experience, especially with respect to Castell's paper. Because we are searching for a fundamental theory, this path leads through a field of philosophical considerations.

9.3 Philosophy

9.3.1 Semantical Consistency

This section gives the methodological background for the march through the four fields of logic, probability, abstract quantum theory, and the theory of the space-time continuum.

Semantical consistency is a term I have invented for a tentative description of a philosophically sound kind of physics. In a loose manner one might call a theory semantically consistent if it includes a theory of those measurements by which it gets its own meaning. I shall try to explain this in more precise language.

In any physical theory we can distinguish the mathematical formalism and the way in which this formalism is given a meaning in physics, a ‘physical semantic’. The formalism, even if mathematically well understood, is not yet physics. For instance I think Castell’s paper became sufficiently clear in its mathematical content, but many of the questions he was asked were of the character “what do you mean by that?” or “why do you do it?” Now, “why do you do it?” and “what do you mean by that?” are nearly the same question. The answer to such a question will in general be given not by writing down another formula, but in language. This verbalized language must be the language spoken by those physicists who do not yet know the theory we are telling them. The language used in order to explain a theory which we propose in mathematical form is a language which has been existing before the theory. On the other hand, it is not self-evident that this language has a clear meaning at all, because if it had a completely clear meaning probably the new theory would not be needed. Thus it may happen that by applying our new formalism to experience—an application made possible by our existing language—we may tacitly or explicitly change the rules of this very language.

We can describe the same problem speaking of operations instead of language. A radical operationalist will say that a mathematical formalism may claim to be used as a physical theory only in as far as we can tell what its terms, its mathematical quantities, mean operationally; for instance what operations can be applied in reality in order to find the actual value of a certain quantity. There is a well-founded criticism against this radical operationalism, maintaining that it is quite permissible to introduce concepts into a theory which are not directly operationally justified. But I would dare to defend a restricted operationalism in the following sense. If there are different theories which are distinguished only by such non-operational terms for which the theory itself tells that it will never be possible to give them an operational meaning, then every physicist will consider these theories to be the same theory. A physicist will not bother about differences of which he knows that precisely if the theories are correct their differences will never become operationally meaningful.

The insistence is here on the fact that he knows it. Yet this condition is connected with another point which implies a very profound criticism of the traditional ways of expressing the operationalist attitude. Heisenberg quotes it in his book[2] where he tells about his talk with Einstein after Heisenberg had invented matrix mechanics. Einstein told him that it was a wonderful chapter but that its philosophy that only observable quantities should be introduced was wrong. Heisenberg was quite surprised and told Einstein “But that is what we all learned

[2] See Heisenberg (1971).

from you." Einstein replied "Even if I said so when I was younger it is still nonsense ('Quatsch' in German)". When Einstein was asked to say more clearly what he meant, his answer was: "It is only the theory itself which tells what can be observed and what not." Thus the observational meaning of a theory will depend on the structure of the theory itself. Only after you know the theory will you be able to tell what is operationally meaningful and what is not.

Thus setting up a theory is some sort of a circular motion. You begin, already possessing a language and certain operations which you can describe in this language. Then you invent the theory. You invent it certainly for intuitive motivations, but you give it a mathematical shape. Then you try to say in language, verbally describing certain physical operations, what is the physical meaning of this mathematical formalism. But this meaning will depend on the laws which you formulate in the formalism itself. And in the end what you get out of this process, if you are very lucky, will be consistent. It will be consistent in the particular sense that now you no longer use linguistic terms of descriptions of operations in any manner which would disagree with the description given of these operations by the theory itself. This I call semantical consistency. A theory is semantically consistent if such inconsistencies do not exist in it. But it is a very hard task to prove that a theory is semantically consistent. Semantical consistency is some sort of a guide line, a principle we try to follow. I would dare the view that no theory which we ever have had has been clearly semantically consistent, except for the fact that many special theories are so limited in scope that they do not apply to their own meaning-giving operations at all, and hence the problem of semantical consistency does not yet arise. Semantical consistency is a condition to be applied to fairly general theories, and perhaps it can be fully satisfied only in a hypothetical final basic theory of physics. But in any case we should always try to make our theories as much semantically consistent as possible.

9.3.2 Temporal Logic

Let me approach temporal logic by briefly working backwards on my list of fundamental problems. In the present historical situation in physics, as I said in my introductory remarks, we try to bring quantum theory and relativity together, and some of us hope that the space-time-structure as described by the theory of relativity is an outcome of quantum theory. This is the tendency I am pursuing here (cf. Sect. 9.3.5). For this purpose it would probably be useful to understand quantum theory somewhat better than it has so far been understood. This is connected with the problems of quantum axiomatics (cf. Sect. 9.3.4). The basic concept of existing systems of quantum axiomatics is probability (cf. Sect. 9.3.3). I shall try to show that this concept is a mathematical refinement of the futuric modalities in temporal logic which I shall introduce in the present section.

In physics the axiomatic method must aim at a double goal (cf. Drieschner's paper[3]) which is closely connected with the tendency to approach semantic consistency most rapidly. On the one hand a set of axioms ought to fulfil the conditions known from mathematical axiomatics: to be consistent, non-redundant, and sufficient to deduce the theory whose axioms it is intended to formulate. On the other hand the axioms should permit a physical interpretation in which they would seem to be as plausible as possible. My personal approach to the second goal begins with the attempt to formulate such elements of a physical theory of which we, human beings in the present phase of our cultural development, cannot possibly imagine that they would be absent in a theory which applies to experience. Such elements would at least comprise the structure of the phenomenon of experience itself. I am not to enter into the full philosophical implications of this problem, but I shall concentrate on some traits of the structure of time.

Experience at least contains the element of learning from facts of the past and of predicting future events, testing the predictions when the events they predicted are no longer in the future but in the present or in the past. Hence, whenever we speak of experience we have a preliminary understanding of the meaning of the terms past and future. We learn from the past, we apply it to the future. Thus there can be no meaningful physics in which this distinction is not understood and not applied. Of course, it may be described differently after we have a theory than before, but it will still be described somehow. For instance Prof. Segal made use of these very same concepts in a particular way when he said we try to start out by causality. Causality is just one of the elements we understand when we speak about the relationship between past and future.

I have attempted to describe some aspects of this relationship by establishing a temporal logic (cf. "Classical and quantum descriptions"[4]). Statements about the future are of a quite different nature from all other statements, including statements about the past, statements that should be true for all times, or statements of a timeless nature, if this expression has any meaning. A statement about the future I propose not to describe as possessing one of the two truth values 'true' and 'false', but one of the so-called modalities, like 'possible', 'necessary', 'impossible' and so on. This has been discussed already by Aristotle in: *De Interpretatione* this chapter, where he gives reasons why the Law of the excluded middle cannot be applied to statements on the future. His example is the sentence: "There will be a naval battle to-morrow." We understand even the actuality of the example: there might be a naval battle near Cyprus to-morrow. (This was said on July 18, 1974). I am not saying that this sentence is now true or false but I propose to apply to it only the terms "This is necessary", "this is possible", "this is impossible", or perhaps "this has the probability p". I am not going here to justify this language philosophically, I just say: this is the language I choose.

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3 "Lattice Theory, Groups, and Space", in: Castell et al. (1975).

4 "Classical and quantum descriptions", in: Mehra (1973).

9.3.3 Probability

Again I mention only a few main points.[5] I am not satisfied with a merely subjective theory of probability. Of course we can define probability subjectively as a propensity to bet, which is a description of the state of mind of a person. Then we can establish the rules of the calculus of probability as the rules such a man must obey if he is to speak consistently about his expectations or his propensity to bet. But being physicists we are also interested in the question how successful he will be in betting, for instance whether he will gain or lose money by it. We are interested in his power of prediction. Thus, in accordance with Drieschner's paper, I stick to the view that probability is a concept which applies to experience and more particularly to the future; that means it is a prediction.

What sort of prediction is a probability? The obvious answer is that it predicts a relative frequency. But here we seem to run into trouble, and it will be elucidating to understand the meaning of this trouble. I give an example. When you predict that in playing dice you will find the five eyes 6 times out of 36 casts, this is a good prediction for a statistical ensemble, but of course you would not expect that it is precise in most of the single cases. If you do the experiment precisely 36 times and you find the five eyes only 5 times or 7 times you will not say that this result has falsified the prediction. This is so because the probability is not a prediction of the precise value of the relative frequency but it is its expectation value.

It is most important to see that this is not a particular weakness of the objective empirical use of the concept of probability, but a feature of the objective empirical use of any quantitative concept. If you predict that some physical quantity, say a temperature, will have a certain value when measured, this prediction also means its expectation value within a statistical ensemble of measurements. The same statement applies to the empirical quantity called relative frequency. But there are two differences which are connected with each other. The first difference: In other empirical quantities the dispersion of the distribution is in most cases an independent empirical property of the distribution and can be altered by more precise measurements or other devices; in probability the dispersion is derived from the theory itself and depends on the absolute number of cases. The second difference: In other empirical quantities the discussion of their statistical distributions is done by another theory than the one to which they individually belong, namely by the general theory of probability; in probability this discussion evidently belongs to the theory of this quantity, namely of probability, itself. The second difference explains the first one. But it seems to reveal an inherently circular nature of the objective theory of probability. If we define probability as an expectation value we have referred it to a concept which in its turn is to be defined by probabilities.

[5] For details cf.: "Probability and Quantum Mechanics", in: *British Journal for the Philosophy of Science*, 24 (1973): 321–337; reprinted as "Probability and Abstract Quantum Theory", in this vol. (MD).

This is a more precise expression of the apparent trouble into which the objective theory runs. But there are two subsequent answers. The expectation value of a relative frequency in an ensemble is not defined by a probability distribution referring to this individual ensemble but to an ensemble of such ensembles. Thus it is a regressive rather than a circular definition. But this first answer is not yet sufficient. The regress of the definition does not stop at one level but it goes up to larger and larger ensembles. Then the law of large numbers tells us that we will probably not make great mistakes by identifying a probability sufficiently close to One with necessity, and a probability sufficiently close to Zero with impossibility. But this statement contains the word ‘probably’, and I do not wish to hide it by a synonym. My methodological or philosophical point is that this is all we can ever hope to achieve in predicting human experience in the open future which is not precisely predictable. A strict discussion of Hume’s or Popper’s epistemological problems would lead us back to this same result. This is the second answer: to ask for more in the justification of the empirical meaning of probability would imply a misunderstanding of the meaning of experience.

Formally I conclude that even in classical probability it is meaningful to do the definition in separate steps. In the lowest step we describe a single experiment; there we have modalities but no probabilities. In the second step we describe an ensemble of equally prepared experiments. There we have modalities for relative frequencies. In the third step we speak about an ensemble of ensembles. There we have modalities for relative frequencies of relative frequencies. And so on. It is not necessary for practical use that we speak such complicated language, but it will prepare us for the problems of quantum theory.

9.3.4 Quantum Theory

I propose the view that general or abstract quantum theory is a general theory of probabilities and nothing else. Of course there is quantum theory applied to particular cases, e.g. to atoms or molecules, or to elementary particles. But in axiomatic quantum theory we do not talk of momentum, position, or similar concepts. There we do not need to know that there is a three-dimensional position-space. The only thing we must know in axiomatic quantum theory is that there are single cases and statistical ensembles of single cases, and that these can change in time. Hence we must describe time. This is usually done by a real variable. I feel that this is still a weakness of the theory. I am fully in agreement with Finkelstein thinking that this continuous time variable is just one of the classical elements which are not yet eliminated from quantum theory because we have not been able to do so. At present I am not doing better, I am using the real time variable as all people (except one) do.

What, then, is the difference between a classical theory of probability and quantum theory? I think an axiomatic quantum theory would be fully satisfactory if it were possible to show that it contains classical probability plus one axiom, and this axiom would have to determine what we call superposition or, equivalently, what we call indeterminism. Drieschner's paper shows how far we think here in Starnberg we have come in this point and what is still missing.

I shall add without a proof that the process of second quantization is exactly the quantum theoretical analogue of the steps I introduced in classical probability. It leads from one particular quantum state to an ensemble of such states, and third, fourth … quantization can be added. The components of the state vector are replaced by operators. These are non-hermitian and not directly measurable, but we can measure their absolute squares. The absolute square has integers as eigenvalues, and these are the numbers of individual cases, e.g. of particles, in the relevant states. They are the absolute frequencies from which we calculate those relative frequencies whose expectation values are the probabilities.

I now take quantum theory as given. Certainly quantum theory as we know it is not fully deduced from such considerations. But I feel these considerations make it plausible that quantum theory is not just one out of a thousand equally possible theories, and the one which happens to have pleased God so much that he chose to create a world in which it would be true. I rather think, if we had understood quantum theory just a bit better than we understand it so far it would turn out to be a fairly good approximation towards the formulation of a theory which contains nothing but the rules under which we speak about future events if we can speak about them in an empirically testable way at all. This is important for my philosophy because without such a trust in quantum theory I would hesitate to apply it in a manner as strong as I am going to do now.

9.3.5 Space–Time–Continuum

In addition to abstract quantum theory we know from experience that there are particles which have at least approximately observable positions in a three-dimensional Euclidean space, and that this space can be united with time in a Minkowskian continuum which perhaps is the tangential space of a Riemannian continuum. The proposal is to search for a theory which would deduce even these structures from quantum theory.

What we need in all experience is the possibility of deciding alternatives. The simplest case is the yes-no-alternative, the binary or, as I usually call it, the simple alternative. Thus the quantum theory of the simple alternative is the simplest thing to be described in a general theory of possible measurements. Drieschner's and Castell's papers describe this quantum theory of the simple alternative. It contains a two-dimensional complex vector space with a unitary metric, a two-dimensional Hilbert space. This theory has a group of transformations which is surprisingly near-isomorphic with the group of rotations in the real three-dimensional Euclidean space. This has been known for a very long time. I propose to take this isomorphism seriously as being the real reason why ordinary space is three-dimensional. The attempt to deduce this was made in the 1955 and 1958 papers,[6] and Castell's paper in this symposium is a far more modern version of these ideas. I shall comment on this presentation in the language I have now prepared.

It is certainly possible to decide any large alternative step by step in binary alternatives. This may tempt us to describe all objects as composite systems composed from the simplest possible objects. The simplest possible object is an object with a two-dimensional Hilbert space, the 'ur'. The word 'ur' is introduced to have an abstract term for something which can be described by quantum theory and has 'a two-dimensional Hilbert space, and nothing more. (Recently I discovered in J. L. Borges: *Tlön, Uqbar, Orbis Tertius*[7] the sentence: "Stranger and purer than the 'hrön' is sometimes the 'ur': the thing produced by suggestion, the object formed by hope." Borges, as we know him, would probably take the coincidence of his word with mine as not being accidental.) The ur is introduced in order to define a symmetry group for the dynamics of all systems, which in its turn is to define geometry according to the ideas of Felix Klein's Erlangen program.[8]

How do we get the group? If there were one isolated ur it would have the full symmetry described at least by the SU(2). If it had not this symmetry, some of its states would have some objective properties not possessed by some other states, and this would mean that some additional alternative relating to it could be decided. One would call such a property an external property, external to the object, and only defined by the object interacting with other objects in its environment. But this is what we had excluded by speaking of an isolated ur. Now assume two interacting urs, isolated from all other objects. It is meaningful to speak of their relative position in their common Hilbert space. Thus Castell spoke of their relative phase. But again it would seem meaningless to speak of a difference between one state of the two urs and another state which is reached by applying the same element of the symmetry group of the single ur to both of them. If this common rotation of two urs were observable, there would not be two isolated urs but at least three of them. Thus whenever we have a well-defined number of urs, then a common rotation of all of them by the same element of the symmetry-group of the single ur must transform their state into a state which cannot be distinguished from the original one. In this sense any system composed of urs ought to have the same symmetry group as the single ur.

Castell introduced a four-dimensional Hilbert space for the ur instead of the two-dimensional one. I shall give no additional comment beyond his arguments for this step. I accept his formalism and discuss its consequences. It describes systems consisting of any number of urs by representations of the SU(2,2). This group can be described as the conformal group of a Minkowski space. One can also introduce

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[6] "Komplementarität und Logik. Niels Bohr zum 70. Geburtstag am 7.10.1955 gewidmet", in: *Die Naturwissenschaften*, 42, 19 (1955): 521–529; *Komplementarität und Logik II*, 42, 20 (1955): 545–555; together with Erhard Scheibe und Georg Süßmann: "Komplementarität und Logik III: Mehrfache Quantelung", in: *Zeitschrift für Naturforschung*, 13a, 9 (1958): 705–721.

[7] See Borges (1982).

[8] See Klein (1985).

a de Sitter-space instead. In any case it leads to a description of these systems in the traditional space-time-formalism. Whether we can actually interpret this formalism as being used in a semantically consistent manner, depends on the success of our way towards experience.

9.4 On Our Way towards Experience

I am now going to say which problems we have not solved, even if all I said so far should happen to be true. I wish to emphasize that there is a very long way to go before our considerations might establish a theory of elementary particles. I speak of this long way not in a mood of pessimism but rather of optimism. If the way were not so long for intrinsic reasons, it would offer little hope of leading towards the theory of things as complicated as the so-called elementary particles.

9.4.1 What is a Particle?

An ur is not a particle. It is by definition quite different from a particle. Castell proposed the simplest case of a particle, or at least a thing that looks mathematically like a particle. He described it like a free neutrino. It has an infinite series of states, each of which corresponds to a well-defined number of urs and anti-urs, but this number differs from state to state. Thus even the simplest particle of which we can think in such a theory does not consist of a definite number of urs. This will be so for all other particles. The number of urs cannot be defined by the nature of the particle. It is defined by the particular state of the particle, if it is defined at all. In most states it will be undefined since they will not be *eigen states* of the ur-number-operator.

This was a negative remark. What can we say positively about particles?

As long as we are confined to massless particles we have not gone far. I should even say that what is described in Castell's paper is not yet a real neutrino. A real neutrino is a neutrino in the real world in which there are massive particles interacting with the neutrino which we have not considered here. But at least Castell's neutrino can be characterized by quantum numbers which probably will also apply to the real neutrino.

The mass is a Casimir operator of the Poincaré group but is not preserved by the conformal group. Mass zero is the only mass value which is invariant under the conformal group. As soon as we introduce particles having well-defined non-zero masses we break the conformal symmetry with which we were beginning. As Castell described we can go to the tangential space in some particular point. This point is of course that part of the world and of history where we are living. Here we can locally define fixed mass values. This means that the concept of a particle is a local concept and not a global one.

This theory begins by not distinguishing between elementary particle physics and cosmology. The ur is global. But when we come to the level where we can speak of well-defined particles having particular masses, then we must stay in a local system. It is meaningless to apply the concept of particle to a global situation. Of course a particle might move from this place to another place preserving its identity. Thus its future cone may contain world lines which may lead into any part of the world. But I doubt very much that a description of a particle by states which are globally defined all over space-time is meaningful.

In Castell's model the law of interaction may already be defined by the group. Under the conformal group his quantum number $S1 + S2$ is not proportional with the quantum number which he identified with energy. This can be described verbally by saying that there is a binding energy between the urs which make up a particular state of the neutrino. Whether this is the full interaction we need for understanding mass spectra is to be found out. I suspect that massive particles will have to be described by urs which are not symmetrically composed, i.e. by urs which obey Boltzmann statistics rather than Bose statistics. I return to this question in Sect. 9.4.3.

The mathematical background of Segal's paper is to some extent reproduced in this theory, but the interpretation is not yet clear. When we identify the S1-phase with time as Castell did we will have a discrete energy and a cyclical time. On the other hand in the local space we have, at least approximately, a continuous energy and another time-parameter. I am going to describe these problems in the language I spoke before I learned of Segal's work.

9.4.2 How Many Urs Do We Need? How Many Urs Do We Need for One Particle? How Many Urs Do We Need in the World?

For one particle there are good reasons to expect that in a state which we can observe in the laboratory it contains something like 10^{40} urs. Take Castell's neutrino as an example. Its states which contain well-defined ur numbers are not precisely momentum states but they will have an expectation value of momentum. A state which is one ur has no node or one node in the whole universe. Hence a single ur is smoothly distributed over the whole universe. For a particle state with a wave-length of 10^{-13} cm we must superpose as many urs as is the quotient between the radius of the universe and this wave-length. According to our usual cosmological assumptions this quotient is 10^{40}. Hence even a neutrino would contain 10^{40} urs in an observable state. The same would probably be approximately true for a massive particle. Note that, if localization in one dimension needs n (here: 10^{40}) urs, localization in three dimensions needs 3n (and not n^3!) urs.

How many urs in the world? If we speak of a finite world we will have to answer this question. Here I offer a different consideration, and unfortunately I cannot yet say how the two estimates are connected. We take the urs to be the ultimate alternatives that can be decided, the ultimate bits of information in the world. How many independent bits of information can there be in a finite world? Let us assume that we can localize stable elementary particles. There are not many types of stable elementary particles; for an estimate we can as well speak of one type only, say a proton. It is possible to localize a proton within a radius of 10^{-13} cm. If we try to localize it more precisely we can do so by concentrating a large amount of energy in one small volume, thereby producing new elementary particles. This can be done in a few selected places, but not simultaneously in the whole world. I would say that in nature as it offers itself to our view, if we do not apply local violence to it, the localization of protons goes down to 10^{-13} cm. This implies that the whole world consists of $(10^{40})^3$ cells in each of which one proton (or two, for spin) can be localized, since the protons are fermions. Thus the number of independent decisions that might be made in the world is of the order 10^{120}: for each elementary cell it can be decided whether it is filled or empty. This gives an estimated number of 10^{120} urs in the universe.

It would follow that there are around 10^{80} elementary particles in the world. This number agrees fairly well with the observed cosmic density of matter (a factor of the order 10 or 100 stays of course undetermined in our very rough estimates). I would not dare to take this agreement as an empirical argument for the theory before its structure is better understood. But I want to emphasize that the theory as presented so far contains one undetermined pure number which is the total number N of urs (or its expectation value). This number determines the radius of the universe as measured in the length unit corresponding to elementary particles as $N^{1/3}$ and the number of elementary particles as $N^{2/3}$. Hence it connects the density of matter with the curvature of space (both measured in atomic units). Similarly to, but more radically than in General Relativity we are not allowed in this theory first to introduce a space and then to fill it with an arbitrary amount of matter. Matter determines the structure of its own space. But it would be premature to attempt a comparison with General Relativity before the theory of interaction of the urs is established.

9.4.3 Production of Urs

If the number of urs defines the radius of the world as measured by atomic rods there cannot possibly be an expanding universe unless there is a production of urs. In speaking of expansion I am talking the language I have learned. It is the language corresponding to observations in which length and time are measured by atomic rods and clocks. In this language the law of interaction would have to be such that it does not preserve the number of urs.

Let me first draw a connection with Finkelstein's ideas. In his first papers he spoke of *chronons*, that is single steps in the process of time. The quantum theory of chronons led him, by the same mathematical structures which we used, towards a Minkowski space of events. I tried to understand the connection between the two theories. Since my starting point in temporal logic is the difference between future possibilities and irreversible facts, I tended to think of the presence of an *ur* as the simplest possible fact, and consequently of a *chronon* as the production-act of an ur. To put it differently, a *chronon* would be the event in which precisely one new bit of information is created. Something new has happened, one decision has been made. The fact that one decision has been made is one bit of information, and if it is fundamental or irreducible information it is what I call an ur. Hence the number of urs ought to increase with time as an expression of time's irreversible process character.

Conceptually I think it is plausible to speak such a language. On the other hand, I have so far treated urs like traditional quantum objects with a reversible quantum theory. The easiest quantum theory of a given type of objects is the one which keeps their number constant. This is what Castell's energy operator does with the number of urs. It even is a linear function of the absolute number of urs $N = n1 + n2 + n3 + n4$. But this is the theory which is global and invariant under the conformal group. If we describe particles locally we may for instance use the energy operator which corresponds to the Poincaré group. This energy has a continuous spectrum. It is still non-negative, but it generates a non-compact transformation and it does not preserve the number of urs. Thermodynamic considerations will show easily that if we begin with a finite number of urs, after a certain while their number will statistically increase. The dissymmetry of time is here introduced by our starting point, permitting us to speak of 'beginning' with a finite number rather than of 'ending' with it; temporal logic thus reproduces the common sense understanding of time.

An interaction which does not preserve the ur-number will mix up the different symmetry classes in the representations of the fundamental group. The usual super-selection rule between these symmetry classes does not exist, and hence the assumption of Boltzmann-statistics for the urs is natural.

The connection between this description and the one introduced independently by Segal and Castell is not yet clear. I tend to think that it is not a difference of testable predictions but of semantics. Different fundamental alternatives may be considered in both cases: atomic or local ones in my present language, cosmic or global ones in the language of Segal and Castell. Hence the word 'ur' would describe different things in both cases. Perhaps an ur in Castell's paper would correspond to the complete string of events that follows from one original ur in my language. I hope that further work will clarify these points soon.

9.4.4 Mass-Spectrum

If we concentrate on a local Poincaré group the mass spectrum is continuous. A particle can have any mass. The empirical fact that there are a small number of discrete masses is not at all understood from this point of view. We should not take this discrepancy easy. We should bury any hope of the sort: “Let us change the theory a bit and discrete masses will appear.” This is a most profound problem facing any theory. I propose to solve it without any change of assumptions by making full use of the theory as it stands. I cannot do it at present, but I know what trail I would pursue.

First: The mass-spectrum cannot be understood without a consistent theory of interaction. It must be a consequence of interaction.

Second: Even then it is not easy. Heisenberg and Dürr have developed a theory of a discrete mass-spectrum out of a non-linear operator-equation which describes an elementary interaction. Given a unique mass of one basic particle, say the proton, they give strong reasons to believe that the other masses will differ from it by discrete real numbers. They give a theory of mass ratios. I have not quite understood how the uniqueness of the basic mass follows from their theory; this is connected with the role of the scale transformation. In my theory, at any rate, there is a cosmological scale and hence the absolute value of the proton mass is a meaningful quantity; it somehow corresponds to the 10^{40} urs per particle. How is it determined?

I have a tentative answer to this question; it might be wrong even if the rest of the theory were sound. The proton mass might be understood by statistical considerations similar to those by which we understand the phenomenon of a melting point in thermodynamics. That means that the existence of a precise mass of a stable particle would only be explained by a cooperative effect of 10^{40} or perhaps of 10^{120} urs. In this case we would have to do thermodynamics of the urs which might not be too different from the thermodynamics of the solutions of, say, the Heisenberg equation. Perhaps it is hopeful that my considerations lead towards that field of difficulties which is already known to exist in elementary particle physics.

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International Symposium of Quantum Logik (Cologne in 1984). The photo shows in the *first row* Mittelstaedt, Pykacz, Aerts, Finkelstein, Piron and in the *second row* Strohmeyer, Busch, von Weizsäcker, Lahti, Stachow, Burghardt, Berner. © Hans Berner who granted permission to use this photo

David Finkelstein, Carl Friedrich von Weizsäcker © Hans Berner who granted permission to use this photo

Chapter 10
Matter and Consciousness

This essay represents the final part of a course given at the University of Hamburg during the summer semester of 1965 (the same course was previously referred to in the head note to "Quantum Theory", Chap. 7 in this volume).[1] It was preceded by a sketch of the structure and interpretation of quantum theory corresponding to the contents of sections a, c, and d of "Quantum Theory", to which I will refer here. It has not appeared in print before now.

The essay shows systematically in what sense a theory of the unity of matter and consciousness is compatible with quantum theory, even if such a theory is not deducible from quantum theory. It thereby takes up a problem touched on in II.1.dii and establishes the mode of thinking presupposed by the following two essays, "Models…" and "Matter, Energy, Information".

Our considerations thus far suffice for interpreting the quantum theory in its present form. The reference to classical instruments of measurement obviates an explicit appeal to human consciousness. In the justification of this reference, appeal is made, to be sure, to the conditions of knowledge; i.e., to consciousness. It is therefore legitimate to ask how the measuring apparatus and consciousness are in fact related to each other. This question transgresses the borders of the discipline we today call physics; what I will say about it in this lecture consists partly of hypotheses that, in my opinion, might turn out to be untrue without thereby falsifying the Interpretation of quantum theory developed up to this point.

So long as one keeps in mind the metaphorical nature of the preposition 'in,' one might perhaps formulate two questions for an initial orientation:

1. How is matter in consciousness?
2. How is consciousness in matter?

Here we already presuppose that matter "is in" consciousness and vice versa; what follows can also be taken as an explication of the 'in' and the 'is.'

[1] This text was first published as: "Matter and Consciousness", in: *The Unity of Nature* (New York: Farrar Straus Giroux, 1980); translation of: *Die Einheit der Natur* (Munich: Hanser, 1971): III,3.

M. Drieschner (ed.), *Carl Friedrich von Weizsäcker: Major Texts in Physics*, SpringerBriefs on Pioneers in Science and Practice 22,
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'Matter' here stands for all objects of physics—for everything, in our approach, that obeys the quantum theory. "Matter is in consciousness" now means: we know of matter. This formulation already implies a rejection of pure 'phenomenalism'; of 'idealism' in current Marxist terminology; of the doctrine, in other words, that equates matter with the contents of our consciousness. (Let me note that I regard this doctrine not so much as 'false' but as a meaningless combination of words, analogous to the confusion between relative and absolute doubt.[2]) To know means to know a fact; if I know there is an apple tree in front of my house, then there really is an apple tree in front of my house, and I know that. The question is only: 'How' do we know matter?

In the theory of measurement there are two basic ways of knowing matter: it can be phenomenally given or it can be inferred. That the apple has a brown spot I can see; this we call phenomenally given. That there is a worm in the apple I conclude from the brown spot and from my general acquaintance with apples and worms. The theory of measurement shows that the demarcation between the two ways of knowing is arbitrary up to a point. If my inferences are well founded, I consider even the inferred as phenomenally given: I 'see' that there is a worm in this apple. If I am very sceptical, I analyse even the phenomenally given, in terms of 'actual' phenomena and 'unconscious inferences': do I really 'see' that this medley of green and brown before me is an apple tree, or that the 'apple' isn't a bar of soap? But the relation between phenomena and inferences remains, wherever we look for it. There can be no knowledge of matter unless there are some phenomena I accept, and without inferences physics becomes equally impossible.

Bohr's recourse to classical concepts represents, in the sense of our present distinction, the recourse to an unequivocal terminology for the description of phenomena. It would be inconsistent, from this point of view, to extend the theory of measurement by, for example, adding onto the microscope with a photographic plate, as further 'measuring instruments', the human eye, the optic nerve, and the visual centre in the brain. The microscope and the photographic plate I see; they are phenomenally given to me. The eye I don't normally see (for in studying matter I look not into the mirror but at matter); the eye is an organ of knowledge precisely in that it is not an object of knowledge. The optic nerve and the Visual centre in the brain I don't perceive at all, I know of them only from physiology. How the Stimulation in the Visual centre finally becomes a visual experience not even physiology knows. All this is precisely not a phenomenon. Bohr is entirely

[2] This refers to a discussion of skepticism. It makes sense to doubt *any* one piece of intended knowledge by adducing other pieces that remain unchallenged in the context at hand; but one cannot doubt *all* intended knowledge *at one and the same time* and continue living. For example, given a known fact, one can reflect on its being known. The reflection makes us aware of the circumstance, among others, that the known fact is known to me and is not otherwise available to me except in this knowledge. It thus enables us to doubt whether the fact exists at all or 'only in consciousness'; i.e., whether it is really known or instead only imagined. One can reflect on this doubt in terms of other, undoubted facts. A searching reflection of this sort is a temporal, always finite process. It presupposes in general what it questions in particular. This 'presupposing in general' is part of continued living.

right: matter is what enters consciousness in space and time and under the rule of the causal laws of classical physics. I do not need to get behind the lawfully connected sense impressions to describe how matter is in consciousness. Anyone who wants to know more should analyse the given in its givenness, as Plato did, and Aristotle and Kant, or as phenomenology does in our Century.

The regression into physiology, though phenomenologically sterile, makes good sense under the motto of the second question: How is consciousness in matter? Nobody doubts that a multitude of material events occur in the body when one observes a photographic plate: ray refraction in the eye; photochemical processes in the retina; electrical signals in the afferent and, shortly thereafter, in the efferent nerves as well; and complicated brain processes that are still not well understood. It is legitimate to ask how all this is connected with the phenomena of consciousness that we know so much better.

This question requires a methodological stance essentially different from the first. There it was a matter of phenomenology; i.e., of the faithful perception and description of phenomena. Hypotheses are not really in order there, physiological hypotheses are in fact an escape from the problem; we are not meant to guess at something unknown but to become aware of something we have always known. Here, by contrast, it is a matter of research into connections that are entirely unknown to normal consciousness. The framing of hypotheses is as legitimate here as anywhere in the positive sciences.

I would like now to formulate a hypothesis that is actually not a scientific hypothesis in the narrow sense, since it refers to science as a whole rather than to particular relations within science. Nor is the hypothesis new; it merely invites a new and, I feel, intelligible interpretation of quantum theory. Let me express it in this way:

A. Identity Hypothesis: Consciousness and matter are different aspects of the same reality.

That the term ‘reality’ serves merely as a gap filler is obvious: one needs to employ a noun here that does not invoke any particular preconceived notions. The arguments for the hypothesis will clarify what the term ‘aspect’ is supposed to stand for.

The hypothesis would seem especially reasonable if the further hypothesis should turn out to be true:

B. Uniqueness of Physics: There is but one possible consistent theory of the temporal behaviour of objects; it is called ‘physics’.

‘Temporal’ and ‘object’ are to be understood according to the precise definitions developed in the course of the entire foregoing discussion. The comments on the postulates that are required for the construction of quantum theory were intended throughout to demonstrate that these postulates formulate little more than the conditions for a consistent theory of the lawful behaviour of objects in general. The third hypothesis may therefore seem reasonable:

C. The Status of Quantum Theory: Quantum theory is already a good approximation for the domain of physics concerned not with the existence of particular sorts of objects but with the behaviour of arbitrary objects.

Let the term 'object' be defined once again by:

D. Object and Alternative: Every object is uniquely characterized by a number of time-bridging alternatives; conversely, whatever can be thus characterized is called an 'object.'

This proposition is to be understood in a twofold sense: D′. Conceptual Characterization of Objects: The concept under which an object falls is defined by the conceptual detailing of the alternatives characterizing that object. D″. Individual Characterization of Objects: A particular object is defined by the individually recognizable decision of one of the alternatives that characterize it. We presuppose here that alternatives can be characterized and recognized conceptually; i.e., on the basis of their immanent structure (number of answers, subsumption under a mathematically given state space, etc.), according to the meaning of their answers (linguistic articulation, etc.), and also individually (I measure this here and now). How this is done, and what it involves, is a question of phenomenology; here we merely presuppose that it is possible. In this sense we already presuppose that there is such a thing as knowledge. The addendum that whatever is characterized by time-bridging (not: tenseless) alternatives is to be called an 'object' is a terminological Convention that may appear reasonable in view of our exposition of quantum theory.

We now State a further hypothesis that is open to subsequent revision:

E. The Conceptual Registration of Consciousness: Events in consciousness can be described in terms of time-bridging alternatives.

If we admit this, then consciousness consists of objects and, according to B, satisfies physics; in fact, according to C it even satisfies quantum theory to a good approximation. Now, in the contemporary view, there indeed is something about man that, to a considerable extent, perhaps exactly, does obey quantum theory—namely, his body. This circumstance suggests the hypothesis that my consciousness and my body are aspects of the same reality.

If one assents to this hypothesis, then one has, to begin with, merely stated a program. My consciousness and my body are certainly not synonymous. I am unconscious of many bodily processes. My awareness of bodily processes that might be equated with conscious acts is minimal; if brain currents are the 'bodily aspect' of conscious acts, they are still not the contents of these acts. Furthermore, consciousness is a very imprecise term. The conscious continuously fades into the unconscious psyche. If we term 'soul' whatever is in this manner continuously connected with consciousness, then we might formulate: The body is the soul insofar as the soul can be perceived as an object by the senses. Such a formulation of course places new tasks before us. It contains the term "sense perception," for example. Sense perceptions are, on the one hand, familiar to us as phenomena; I know how I see, hear, touch, etc. On the other hand, sense perception is dependent on bodily organs. Our formula does relate the two, but how? Roughly: Seeing is the material process in one man's eye as it is apprehended by someone else. This last formulation already involves man in the plural.

Our hypotheses would corroborate the cybernetic approach. Among the innumerable cybernetic tasks, the chief one might then be to detail that structure in material processes (equivalently in psychic processes) which accounts for the quality of consciousness in them.[3] Surely this structure will be altogether different from the simulation of isolated chains of intellectual operations effected by today's computers. We would have to know this structure in order to understand how the psychic element individuates itself into particular, separate 'egos'.

I now break off the enumeration of problems to be faced by a science of 'psychophysics' conceived in this way. We ought, in conclusion, to reflect on the probable limits of such a science.

I see no 'regional' limits. That is, I do not see the necessity of assuming objects that do not fall under this science. It seems more reasonable to assume that such objects cannot be delineated with conceptual means at all. The unequivocal designation of an object supposedly not subject to physics would seem, simply because of the unequivocalness of a designation in terms of alternatives, to be sufficient reason for the object's being subject (in conformance with our hypotheses) to physics after all. The limits of physics would then have to be the limits of conceptual thinking itself.

That conceptual thinking does have limits is indeed a likely assumption on the basis of our analysis of quantum theory. All fundamental terms such as 'alternative', 'object', 'fact', and 'document' have turned out to be dependent on approximations. Within the framework of a conceptual theory one can frequently compare such approximations with even closer approximations. For example, in a world in which there are documents, hence irreversibility, one can *state* the conditions under which an event involving an ideally isolated object can be described reversibly. However, one can no longer *state* what a description of the entire world (including the observer) is supposed to mean in reversible terms; such a description would not mean anything to anybody; i.e., it would mean nothing. We can on principle state the limits of conceptual thinking only within the framework of conceptual thinking; every such delimitation is then merely relative ("these concepts won't do, but those might"). This, however, is no reason for claiming that whatever can't be thought conceptually does not exist. Conversely, all conceptual

[3] Cf. the end of 'Models…' [in Michael Drieschner (ed.): *Carl Friedrich von Weizsäcker: Major Texts in Philosophy* (Cham—Heidelberg et al.: Springer, 2014)—*ed.MD*].

thinking proves to be dependent on the presupposition of what hasn't been conceptually thought out in it. Our analysis thus rather leads us to a proposition that is merely metaphorical: The genuinely real is what cannot be thought conceptually. Because of this, one cannot even apply the predicate 'being' (itself a concept) to the 'genuinely real'. Physics is possible only against the background of negative theology.[4]

[4] These last remarks are taken up in "Parmenides and Quantum Theory", in: (as footnote 3)!

Chapter 11
Matter, Energy, Information

Unpublished until now, this essay was written in 1969.[1] It takes up the ancient concept of form, in order to interpret the contemporary concept of information and to develop a unified concept encompassing both biology, as understood cybernetically, and physics, as the theory of decidable alternatives. The problem of the subjectivity of information is encountered along the way ('for whom is this event information?'). The objectification of the meaning of information, roughly analogous to measurement theory in physics, reduces information to the flow of information; i.e., to temporality. Thus, here, too, the transition is made from the subjective to the temporal point of view.

The line of argument begins once again with the development of physics toward unity (Sect. 11.1). Sections 11.2 and 11.3 discuss the concept of information especially in terms of its objective utilization in biology. We leave the domain of biology, however, without having contributed to the material problems of this science, since further clarification of the concept must first be sought in physics. The economic intermezzo (Sect. 11.4) represents a dilettante's calisthenics; I merely ask the professional economist to consider the layman's question of how it is that an almost universal yardstick for value can at all exist in the economic domain. The road into physics (Sect. 11.5) leads only to conjectures that cannot be tested until elementary physics has been explicitly constructed. The conjunction, in accordance with III.3, of matter, i.e. motion, i.e. form, with consciousness (Sect. 11.6) then leads to the threshold of what in our tradition is referred to as 'philosophy of the spirit'.

11.1 Matter and Energy

Historically speaking, matter is, to begin with, the conceptual opposite of form. A cupboard, a tree is made of wood. Wood is their 'matter'. The name of the term 'matter' is in fact taken from this example: materia = hyle, which means wood.

[1] Cf. von Weizsäcker (1980).

M. Drieschner (ed.), *Carl Friedrich von Weizsäcker: Major Texts in Physics*,
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But the cupboard isn't simply wood, it is a wooden cupboard. "Cupboard" is what it is intrinsically; cupboard is its eidos, its essence, its form. But a cupboard must be made of something; a cupboard without matter is a mere thought abstracted from reality. On the contrary, this cupboard made of wood is a real whole of form and matter, a *synholon*; form and matter are 'grown together' in it, it is something concrete.

In the realm of the concrete, then, no form exists without matter. Nor can there be matter without form. Wood not fashioned into furniture exists, to be sure—e.g., wood which is the matter of a tree. Wood also exists that is not (i.e., that is no longer) the matter of a living tree—this pile of firewood, for example. But wood is itself a form. Form and matter are relative terms. What is matter relative to cupboard and tree—namely, wood—is form relative to earth and water (in the language of the ancients), or relative to organic molecules (in modern language). All wood consists of 'earth and water', or of carbon, nitrogen, hydrogen, etc. According to Aristotle, a first matter (prima materia, prote hyle) that is no longer the form relative to another matter is merely a philosophical principle that neither exists concretely nor can be known, since whatever we know is form.

Modern physics, however, takes up another ancient philosophical tradition, that of atomism. According to this doctrine, a set, final form of first (and therefore true) matter does exist—namely, the space filling extendedness of the indivisible smallest bodies, the atoms. In this conception matter is no longer a relative term in the relation matter-form, which can be exhibited only by means of something concrete; rather, it designates what truly exists in itself. An atom is an atom regardless of what larger body it is a part of. Continuum theories of matter also conceive of what is extended in space—i.e., matter—as existing in itself. Thus matter becomes the only true existent in that monistic conception which rightly bears the name 'materialism'. In a dualistic conception such as Descartes', matter becomes the term that represents the opposite of consciousness. But consciousness is not a term that occurs in physics (or in natural science, as one said later, when 'physics' was reduced in scope), it is not a physical object. The existent in physics, it appears, is matter.

In the 19th century, a new term paired with matter arose—namely, energy. At first, energy was force become substance. This connection is important for our present theme. Already in the 17th century, physics was forced to introduce force as a second, problematic entity alongside matter. Physics is the science of the motion of matter. Motion is subject to laws. The laws prescribe how matter moves in given circumstances. But the circumstances are characterized in terms of the presence of causes of possible motion (or, in accordance with the law of inertia, causes of possible changes in motion), and these causes are termed 'forces'. Forces as individual entities were suspect in the 17th century as 'occult qualities'. One tried to reduce them to the essence of matter, to its filling out of space or—put in popular language—to pressure and collision. From this point of view, force as the cause of motion resides in matter itself; and from these reflections the concept of 'living force' or, as we now say, 'kinetic energy' finally developed. But pressure and collision turned out to be an unsatisfactory model for the movement of one body by another. Potential energy had to be placed alongside kinetic energy; it is the force-potential (capacity to exercise force) that does not manifest itself as motion.

We note: energy is the capacity for moving matter. This capacity is turned into substance as a result of the Law of the Conservation of Energy. Energy can be quantitatively measured, and it turns out that its quantity, just like the quantity of matter, is conserved in time. (Robert Mayer thought of the energy law as the quantitative formulation of the rule *causa aequat effectum*). If something is conserved, one regards it as a substance, as a substrate that remains itself in the world of changing appearances. Thus it seemed in the 19th century that physics dealt with two substances, matter and energy, the latter more often referred to as force in popular philosophical writings.

Relativity theory has, in a certain sense, taught us the identity of the two substances. In contemporary terminology, conservation of matter is called conservation of mass; and energy and mass are relativistically equivalent. The group-theoretical viewpoint (Noether's theorem) allows us to understand the reason for this 'unity of substance'. To every continuous real parameter of the symmetry group of the equation of motion corresponds a conserved quantity. The energy is the conserved quantity corresponding to translation in time; i.e., to the homogeneity of time. If time plays as fundamental a role in physics as I assume it does, it seems plausible that a particular conserved quantity would correspond to it; this is why already Kant had related the conservation of substance to the homogeneity of time.[2]

But the real significance of the unity of substance shows only in elementary particle physics. Group theory at first tells us merely that in every physical theory characterized in terms of an equation of motion, the conserved quantities defined by the symmetry group of that equation must exist; thus for every equation of motion invariant with respect to translation in time, an energy must exist. But the idea of the general energy law was, from the beginning, that all forms of energy were comparable among, and could at least in principle be transformed into, one another. It follows that something like a universal equation of motion embracing all kinds of energy would have to exist. Heisenberg's unified field theory constitutes an attempt in this direction. It does lead to a unified substance that Heisenberg rightly calls 'energy'; the elementary particles are merely its different quasi-stationary states.

What are the essential properties of this substance? Its quantity can be measured. The elementary particles and everything constituted by them are its possible forms of appearance, which represent the solutions of the basic equation of motion. The law itself, which we will not examine in detail now, admits of a fundamental interpretation. We arrived at substance through the identification of two entities—i.e., matter and energy—that at first were conceptually clearly distinct. Matter was introduced as the stuff of which things consist—i.e., as the 'substance', in the sense in which we are using the term. Energy was introduced as that which can move

[2] Cf. *Kant's 'First Analogy of Experience' and the Conservation Principles of Physics*, in Weizsäcker (1980), chap. iii, 5.

matter, its quantitative measure being at the same time a measure of the 'quantity of movement' it produces. (The latter is defined as the square of the velocity vector multiplied by the mass that executes the motion—a circumstance we do not find surprising nowadays since it follows from rotational symmetry). When matter and energy are identified now, one must say that matter is at the same time the capacity to move matter. This is just what the basic law of motion—in the hypothetical form of Heisenberg's nonlinear operator equation, for example—in fact expresses.

The meaning of the unity of substance can be stated more trenchantly, if at first in purely symbolic form, by starting not from matter but from energy—or, actually, from time. Starting from matter, we have been saying that matter is the substance of things. Energy is the capacity to move matter. If matter and energy are identical, then matter is the capacity to move itself. A dualism of substance and movement remains. Why does substance move at all, and why is it at the same time its own capacity to move itself? If we start from time as the fundamental concept of all physics, we can say: All there is, in the final analysis, is time. To be time, it must be change; i.e., movement. (The foregoing 'i.e.' is formally a verbal definition; only a theory that deduces space can justify the nature of 'movement' as spatial motion). Only insofar as movement does not remain identical with itself is it truly movement; it must therefore at the same time be the capacity to change; i.e., to move itself. Movement must therefore have the double aspect of that which moves and that which is moved.

11.2 Information and Probability

In the sense of traditional physics, information is neither matter nor energy. Rather, the concept of information brings into play the two older antipoles of matter—namely, form and consciousness.

Information can be defined as the quantity of form. I will discuss this with reference to one of the usual quantitative definitions. Let E be a formally possible event, and p its probability. Then

$$I = -\log_2 p$$

is the information obtained when E occurs. If, for example, $p = ½$, then $I = 1$ or, as one says, 1bit; if $p = (½)^n$, then $I = n$. The less probable an event is, the more information it furnishes. This introduction of the concept of information makes sense provided one already understands the concept of probability.

It would be wrong to argue that probability, being a conjecture, is subjective, and that information, therefore, is evidently '(not matter but) consciousness'. Every concept is 'subjective' as thought, even the concept of a thing or of matter; it is at the same time 'objective' insofar as it is 'true'. A concept is true, broadly speaking, if it can occur in true propositions on its object. A proposition may be called true if it can at least be verified intersubjectively. In this sense, probability is certainly an objective, true concept, assuming that probability judgments can be tested empirically. The sense in which this is possible was discussed in the justification of probability theory in terms of the logic of temporal propositions,[3] where probability appears as the prediction or, to be more mathematically precise, as the expectation value of a relative frequency.

The information of an event can also be defined as the number of completely undecided simple alternatives that are decided by the occurrence of the event. A simple alternative is said to be 'completely undecided' if neither one of its two possible answers is more probable than the other. One can define the quantitative measure of the form of an object as the number of simple alternatives that must be decided in order to describe this form. In this sense, the information contained in an object measures exactly its amount of form. The information 'contained' in an object is the information represented by the appearance, in the field of vision of an observer, of an object whose identity has been recognized.

Thus, information measures form. At the same time, however, information cannot—at least in this preliminary, still primitive conception—be defined except in relation to a consciousness. In a sense, this is true of every concept, but here something more is meant, for even the concept of objective probability is subject-related. As an example, allow me to discuss the casting of two good dice. Two observers A and B are asked to predict the probability p that the sum of the spots on the upturned faces is 2. A's prediction is to be made prior to the throw, B's only after he already knows the number of spots showing on the first die. A therefore predicts $p = 1/36$; B predicts $p = 1/6$ if the first die shows 1 and $p = 0$ if it does not. Both predict correctly (as can be verified objectively); their values for p differ merely because the event they predict is a sample out of two different statistical ensembles. The value of an objective probability, in other words, depends on the prior knowledge. The information of the event "total number of spots = 2" is less for B, who already knows "number of spots on first die = 1", than for A.

This example shows, first of all, that one must take 'probability' and 'information' as objective and, at the same time, as subject-related concepts; their conceptual meaning is the quantification of 'knowledge', and knowledge is always the knowledge someone has of something. Information measures, in particular, the increase in knowledge gained as the result of an event, and this must obviously depend on one's prior knowledge. That information measures knowledge does not contradict the thesis that information measures the amount of form since, in ancient philosophy, form (*eidos*) is precisely what can be known. But how can we assert that the amount of form in an object depends on prior knowledge? After all, form is supposed to be what can be known objectively, it is an objective property of something. With this question we enter upon a very lengthy investigation.

It is easy to 'objectify' our example. Prior knowledge here refers to one part of the formally possible contingent properties of the pair of dice. In conformity with the question asked by the dice players, one defines the objective amount of

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3 Cf. "Quantum Theory" in this volume, part 7.3: Tense Logic.

information in the pair of dice in terms of what can be registered upon looking after the dice have been cast, and what is therefore based on a prior knowledge that is acquainted with the formally possible outcomes (i.e., that knows: this is a good pair of dice), but not with any contingent facts concerning the dice. In this sense, the objective information of the event "spots add up to 2" equals $\log_2 36$. Thus, the amount of information in every measurement of a quantity described in advance as formally possible can be objectively stated.

But this does not exhaust the amount of form in the two dice. Only for a dice player do the two dice carry the information just discussed. His prior knowledge is extensive: he knows what a game of dice is, that the two wooden cubes are dice as understood in this game, what the number symbols on the faces say, etc. He must know all this to be able to recognize what he sees as the number of spots on two dice. The information is information for the dice player only by virtue of a semantics in which a great deal of knowledge—i.e., a great deal of other information—has already been invested. A large part of this 'semantic information' can also be considered as the 'form of the dice'. How much form, now, does the pair of dice contain in the end?

In looking for an answer to this question, an obvious approach is to investigate the dice physically; and then there is no stopping before the atomic level is reached. The amount of information in each die must then be $N \cdot i$, where N is the number of elementary particles of which the die consists, and i the formally possible amount of information of the individual elementary particle. At this point we are confronted with difficult questions concerning the foundations of physics, which we will refer to explicitly only in section e. Let me now say merely that this information remains 'virtual', since, in practice, no experiment can ever exhaust it. How is it possible to single out actual or actualisable information in this practically unlimited reservoir of virtual information?

The concept, as subjective knowledge, corresponds to the form, which is known objectively. We therefore expect to be able to measure the information of an object to the extent that we can subsume it under a particular concept. Subsumed under the concept "pair of dice in a game of dice", the two dice contain exactly $\log_2 36$ bits of information. Subsumed under the concept 'game', they contain additional information—for example, the information that they are a pair specifically of dice. The measure of this information clearly depends on the prior knowledge of how many different games there are. To every concept belongs a prior knowledge that constitutes its semantic information. (An extreme case would be the concept "structure made up of elementary particles").

If we wish to make these reflections more precise, we would have to try developing an "objectification of semantics." Consider a reliably functioning instrument that reads off and stores or processes that information concerning an object which falls under a particular concept. This would constitute an objectified operational definition of that concept. An example would be a measuring instrument; quantum measurement theory, which replaces the observer by an instrument, is a model for the objectification of semantics. Of course, to assure a clear-cut formulation of the question the measuring instrument itself must also be subsumed under particular concepts in this theory. Here lies the reason for the necessity, emphasized by Bohr, of classical concepts in the description of measuring instruments. Naturally, it is possible, by 'shifting the cut', to observe the measuring instrument in turn by means of a meta-measuring instrument; the 'classical' nature of the concepts of measurement is then intended as a guarantee that nothing new will result from such an iteration in the objectification of the semantics.

If one wishes to eliminate the *ultima ratio* of the observer, then one can try to have an apparatus not only store the measurement results, but evaluate them as well. Here we enter the theory of control systems. The most significant example of a "fully automated" control system is genetics. Organisms control their own growth by means of the genetic information stored in the DNA molecules; they reproduce this information and thereby themselves, and, assuming the Darwinian hypothesis to be correct, have produced with this system even the contemporary form of the system itself. Let us first consider only the cybernetics of reproduction, leaving aside that of evolution.

One can, in principle, easily calculate the amount of information contained in the DNA chain of molecules: every molecule is known to contain 2 bits, a chain of n molecules thus contains 2n bits. Theoretically speaking, this is the information corresponding to the concept 'genetic constitution'. A much larger amount of information would of course correspond to the concept 'chain of atoms', which admits arbitrary atoms as components in the chain; most of that information is already contained in the employment of the concept 'DNA chain', which excludes all other possible atomic combinations. Furthermore, in a particular case we can ascertain that we are dealing with a DNA chain only by doing a chemical analysis; but contemporary genetics utilizes the information invested in its conceptualizations for presupposing without further ado that DNA is indeed the carrier of the genetic constitution. It was in this sense that we were entitled to use the term 'genetic constitution' in the definition of the amount of information.

With regard to this definition, we said above that we were speaking 'theoretically'. After all, we are interested just now in the objectification of semantics. But in the reality of organic life, the DNA molecule carries 2 bits only if a mechanism exists for transforming this information into the growth of somatic structures in the organism—into the production, as a first step, of certain proteins. It is known that the mechanism for the production of proteins already involves a certain loss in information as compared with the theoretical value of 2n bits per chain. We see from this that it is only the semantics (the objectified semantics of the production mechanism for albumen, in this instance) which determines the amount of information.

We now generalize and formulate, in two theses, the relation between information and objectified semantics with which we have just become acquainted:

1. Information is only what can be understood. 'Understood' can be interpreted here in as objective a sense as the DNA information has been 'understood' by the mechanism for the production of proteins, when it transforms this information into protein structures. The structures produced are themselves

information. In fact, the meaning of the objectification of semantics lies in enabling us to calculate the amount of information contained in the semantics. The first thesis can now be further developed:

2. Information is only what produces information. It should be pointed out that virtual information, which is capable of producing information, must be distinguished from actual information, which actually produces information.

The second thesis presents the flow of information in the form of a closed system: information exists only when, and in the degree that, information is produced—i.e., when and insofar as information flows. In this form also we talked of energy, at the end of section a, as the amount of movement: movement exists only insofar as it is moved (insofar as it changes). There I referred to this way of speaking as "purely symbolic." What it lacked for a precise formulation was the connection between movement and information, the question that now confronts us. Can energy and information, too, be identified?

But we must first attend to a few further reflections.

11.3 Information Flow and Natural Law

How much information is contained in the objectified semantics of a given amount of information? How many bits are needed to understand one bit?

One is tempted to give two very dissimilar answers, both of which I will illustrate in connection with genetic information. Let the genetic information be 2n bits for some particular animal species; how many bits are contained in the objectified semantics of the life of this species?

First answer: as many bits as correspond to the quantity of form contained in an entire organism—a very large number, that is. A single DNA 'alphabet'-triple in the chromosome of a newly formed cell produces many—say, m—albumen molecules identical in structure, whose information content must therefore be multiplied by m. These proteins cooperate in constructing the cell, whose metabolism no doubt produces additional bits. And in a multicellular organism the average information of the single cell must be multiplied by the number of cells. The difficulty with this answer is its failure to articulate the concept governing the calculation of the information in the organism.

Second answer: exactly 2n bits. For the organism develops from its genetic endowment and transmits these same 2n bits (apart from mutations) to its offspring. These bits are necessary and sufficient for the definition of the species; they are therefore the true amount of information in the organism. Anyone who completely understands the laws governing the functioning of an organism ought to be able to derive its form and functions simply from knowledge of the DNA chain in the nucleus of any one of its cells. He would know, therefore, that the huge amount of information arrived at by the first answer is redundant and reducible to 2n bits. Only the second answer subsumes the organism under the concept of a living being, which is of course appropriate to it; the first answer subsumes the organism under the concept of a physical object. The excess information in the first answer is simply the information contained in the concept of a living being.

One can argue against the second answer by saying that the individual has many characteristics that are determined not by its genetic endowment but by the conditions of growth, by the vicissitudes of life, and perhaps by pure (quantum theoretical) chance. One could mediate between the first and second answers by distinguishing between species-specific information in the sense of the second answer, and individual-specific information, freeing the latter, too, of its redundancy by referring to the relations governed by natural law. In the case of man, dependent as he is on learning, the information we called 'individual' includes historical information, which is actually 'super-individual'. This last remark leads us to the question of progress, which we do not wish to raise here.

If we confine ourselves, for the time being, to specific information, we must still ask how, under this aspect, the first answer is related to the second. If viewed separately, both answers interpret the theses of the preceding section in too narrow a sense; i.e., both fail to take the theses seriously. The first answer interprets thesis 2, "Information is only what produces information", in the purely external sense that the genetic information does indeed produce the rich information of the phenotype, without considering to what extent the phenotype is a 'semantics' of the genetic information. The second answer, in asserting that the same amount of information is contained in the phenotype as in the genotype, takes thesis 1, "Information is only what can be understood", literally by interpreting the phenotype exclusively as a new representation of the genotype. The first answer loses sight of the understanding, the second of the production of information; but the two theses had been intended in the sense that the production of information is the understanding. It seems that both answers forget we are dealing with the flow of information, not with static information.

To clarify the situation further, let us consider the concept of natural law, which plays an essential part in the second answer. By virtue of the laws of nature, the answer says, the information of the phenotype is identical with that of the genotype. We are to interpret this 'identity' as 'understanding'. What is the structure of a natural law? Let us examine the basic laws of physics.

According to Newton's law of motion, the change in the velocity of a body is proportional to the force acting on it. Let us assume, for the sake of simplicity, that the force is given ("fixed environment = external force"); the body we take as a point mass. The law describes the change in the state of the body as being determined by the state itself. The state designates those properties of the body which, rather than being implied by its essence (here its essence as a point mass), are freely assignable ('contingent'). It is the form of the law itself that specifies what properties are contingent. The state ('phase') of a point mass is characterized by its position and velocity. The contingent information concerning a point mass consists of the specification of its phase. Only if the accuracy with which position and velocity are measured is known can the actual value of the information be calculated; these problems I wish to omit here. The information always refers to a particular time; e.g., the present. The phase of the point mass differs at another time. It appears, therefore, that the complete trajectory of a point mass during an interval of time contains a lot more information than its phase at a time (again omitting problems relating to the accuracy of time measurements). The point mass continuously produces, so to speak, new information. If an apparatus external to the point mass is available for storing it, this information is not lost; in the point mass it does disappear, however, to be replaced by new information. For one who knows the law of motion and the external force, the new information is nothing but a necessary consequence of the old; the two are equivalent. The new information is merely the form that the old information takes on at another instant.

We can thus say, in a purely formal manner: The contingent information of a point mass at a time produces another information equivalent to it at all other times, and is thus understood. This description is formal because the point mass is so simplified a system as not to give rise to the processes characteristic of the phenomenon of understanding. Not only does the description leave aside the consciousness of a possible observer, it even omits mention of an apparatus or process that 'objectifies' or 'objectively simulates' the understanding in the manner of a measuring instrument or an organism. On the other hand, the point mass exhibits in its greatest possible simplicity the structure of a law-governed information flow, on which all complicated 'processes of understanding' are in turn based.

To understand the basis of physics, it is important to remember that the elementary laws predict probabilities. Old information is lost when a radium atom decays, and the new information is not equivalent to the old. It seems as if the elementary event in physics presupposes not the conservation of information but its change. 'Understanding', however, presupposes conservation; i.e., a sufficient degree of the deterministic causality characteristic of classical physics. For this reason, as well as because of the irreversible processes required for storage, understanding can arise only in sufficiently large composite systems.

The cybernetics of understanding is not our present theme.[4] The central issue concerning understanding is the 'cybernetics of truth', which is merely hinted at in the essay just cited.

11.4 Digression on Economic Goods and Money

How many bits are there in one dollar? I will discuss this semi-playful question as an exercise in the application of the concept of information.[5]

Not unlike length, mass, energy, and information, money is a universal measure for very different kinds of things. The scientist is inclined to see a degree of

[4] Cf. essay III.4 of Weizsäcker (1980), reprinted as "Models…" in Drieschner 2014.

[5] I leave aside the trivial answer, $1 = 8 bits, which follows from the fact that a coin that existed in the early 19th century was worth one-eighth of a dollar and was called a bit.

arbitrariness in this measure ("I pay as much as I like to pay"). But the remarkable fact remains that this seemingly arbitrary measure has prevailed quite universally in human society. If one asks Darwinistically for the survival value of money, one will find that here, too, the answer involves objective structures from which the monetary value of economic goods deviates empirically, just as the empirical graylag goose deviates from the graylag goose of the zoologist, which conforms to an objective ecological niche. In the case of money this structure will not be something like a specific species or niche, but a feature essential to the entire economic realm. The question "How many bits are there in one dollar?" formulates the hypothesis that, in the last analysis, money measures information. According to this hypothesis, the universality of money could be explained in terms of the universality of information.

The conception of economic goods as exchangeable commodities leads to the creation of a measure for this exchange value—i.e., money. How is it that one can find a common measure of exchange for goods so diverse as bread, fur, bricks, and a boat trip—which, furthermore, have such diverse value with respect to the subjective needs and subjective preferences of different people? What is it that the exchange value really measures? It seems to be something that all goods have in common. What can be had without effort has no exchange value—even if it is necessary to life, such as the air we breathe, or water (in water-rich regions). The idea therefore arose that it is the labour required to produce a good that constitutes its value. This idea formed the basis of classical economic theory as developed by Smith and Ricardo, and it later became the basis of Marx's economic teachings; but contemporary economic science no longer accepts it. Let me first explain what it would mean if this idea were correct.

What is the labour expended on a good? One might measure it in terms of the time required to produce it. The relevant time is, of course, not the empirical time required for the production of a particular good—the worker might have been clumsy and excessively slow, or there might have been some other deviation from the rule. What is intended, rather, is the socially necessary time, the time in which the good is normally produced in a free and competitive market. Under given conditions of production, this time establishes itself in law-like fashion.

But what is labour, what is production? A good is manufactured—a cupboard, say. Its matter does not have to be produced, the wood was already in existence; production consists of shaping it into the form of a cupboard. The amount of labour required to produce it is the work required to give the wood the form that makes it a cupboard. The 'degree of processing' of the product is therefore measurable in terms of its amount of form, its information. And the information is the one belonging to it by virtue of its falling under the concept of a cupboard. Now 'wood' itself is also a form. And the raw material wood does indeed have a value that, in accordance with the theory we are discussing, is measured in terms of the human labour required to grow the wood as a tree, to cut it down as timber, and to transport it to the furniture factory. In this wood inheres the information that belongs to it insofar as it falls under the concept of timber, etc.

Human labour is therefore the production of information. If one assumes that a worker produces a constant flow of information—i.e., the same amount of information per unit of time—then the working time becomes a measure of the information being created; and if money measures the socially necessary working time, it is thus information that it measures.

While showing up some of the weak points of the labour theory of value, these reflections also contribute to the clarification of the basic idea. Consider a few obvious counter-examples to the theory. A cupboard that takes a carpenter a good deal of time to manufacture is worth far less than a drawing that Picasso dashes off in a minute on the back of a menu, or than a diamond that a South African farmer accidentally finds on his property. In neither case is the empirical time of labour a meaningful yardstick of value. The socially required time might come closer: Picasso had to work for a lifetime so as to today be capable of producing this drawing in a minute; in the case of diamonds, it is not the time expended on its chance discovery that counts, but the expectation value of the time a systematic search would require. It is not everyone, however, but only a great painter who acquires the capability illustrated in the Picasso example; and it is profitable to spend a lot of time looking not for an arbitrary mineral but only for one that has the natural properties of diamond (hardness, crystal structure, transparency, etc.). Fashion, too, plays a role. The value of jewels and of fur fluctuates—and is Picasso's drawing really so overwhelmingly superior to the drawings of hundreds of his contemporaries who earn much less?

Let us first discuss the problem of fashion. There is no doubt that the price frequently cannot be correlated with an objective yardstick. One could try to use the subjective valuation of an economic good by a sufficiently large number of people as a measure of its value, and thus construct a purely 'subjectivist' theory that discards objective valuations. But the theory we are looking for sticks to the idea that values are based on objective matters of fact, just as the behaviour of the graylag goose is objectively based (and therefore self-reproducing) on the dovetailing of its genetic endowment with the ecological niche. In the economic domain, these matters of fact are demand and performance, the thesis being that the performance which satisfies a demand defines the concept enabling one to measure the information of the good produced by this performance. The 'theoretical price' of the good would then be a measure of this information. Fluctuations of the empirical price about the theoretical value would normally be the 'healthy' play of the actual about the established value. A fashion would be the irrelevant or possibly 'erroneous' ('ill') deviation of the established value from its 'healthy' or 'average' value.[6]

The theory we have in mind, it appears, searches for the 'truth' of the value of a good. That is why the case for it can be made more convincing the more primitive and non-exchangeable the demands and performances under consideration are. Even then, however, it will be possible to construct the theory only if a sufficiently subtle concept of information is available. The inescapable problems faced by this theory construction can best be seen in our two extreme examples.

[6] Cf. "Models…" (Drieschner 2014), paragraph c. 'Progress'.

Let us valuate the diamond merely according to the utility it has by virtue of its hardness. Its price will depend on its usefulness and scarcity. The scarcity governs the amount of work required to discover the diamond—provided one wants to discover it at all. Scarcity is the improbability of being discovered, i.e., a large amount of information; in this sense, 'scarcity value' is information. Whether one wants to have the diamond, or for what price one would still want to have it, depends on its utility. Does utility admit of a measure in the language of information theory? As the hardest crystal there is, the diamond cuts all other materials. Thus, one single diamond, without being used up, contributes to the shaping of many objects (glass panes, for example); it therefore produces much information. If information is only what produces information, the diamond as this concrete hard crystal contains much information, which is defined in terms of a concept referring to human labour. The reader will be keenly aware of the distance separating these few sentences from a thorough analysis of the actual technical and economic process; I wanted merely to indicate the direction in which the investigation would have to be continued.

The 'collector's value' of a painting is even more problematical. Its discussion would require a theory of the information value contained in human culture. The 'objective' estimate of the value of art work requires familiarity with the 'truth of art'. The discrepancy between the social valuation as expressed in the price, and the 'true value', which posterity sometimes perceives more clearly, is part of the historicity of culture itself; i.e., part of the constituents of the phenomenon. An adequate economic theory of these valuations is therefore not to be expected. Our approach can at least explain why it must be so, and it can thereby help us to assess the scope of certain concepts on the basis of their meaning. Here lie the limits of information itself: the probabilities defined on the basis of natural law, which this concept presupposes, lose their meaning when it comes to the uniqueness of historical events. Conversely, though, an information theory of value will be meaningful in all cases in which money may be regarded as a true measure of value.

This was a digression, an exercise. Let us return to physics.

11.5 Form as Substance

I begin with a terminological point. In the 'exact' sciences, the term designating an entity is often confused with the term designating its quantitative measure. We are here distinguishing between the three entities matter, movement, and form, and their three quantitative measures, mass, energy, and information. In section a—putting it now in precise terms—we identified matter and movement as substance by relying on Einstein's identification of mass and energy, and by regarding both of these as quantities of substance. 'Movement' was taken in its active mode; i.e., not as the actual process of being moved but potentially, as that which moves. The connection between the potential and the actual was not discussed, and the thesis that movement is the production of movement therefore remained 'symbolic'. At the end of section b we discovered the similar structure that showed information to be the production of information, and we announced that we would use this structure to explicate the thesis concerning movement. We now do so in the form of the hypothesis that substance is information.

This hypothesis entails the following theses: Substance is form. More specifically: Matter is form; movement is form. Mass is information; energy is information.

The theory of ‘ur-alternatives’[7] attempts to carry out this hypothesis. I will now discuss the theory from this point of view; prior acquaintance with the text mentioned is assumed.

I begin with an obvious objection. How can substance be form? Form, after all, is the form of a substance. Is form then the form of a form? Or, more peculiar still, is substance the substance of a substance? I note first that we have defined the concept of substance as matter = ‘energy’ so narrowly that certainly not all forms can be substance in that narrow sense. ‘Substance is form’ is not equivalence but a predication: substance is subsumed by form. Thus, in our way of talking, substance is not the substance of a substance, but form can be the form of a form. From the point of view of ancient philosophy it is quite natural that this should be so. ‘Cupboard’ is the form of wood, but wood, too, is a form. Our ‘substance’ corresponds to ‘first matter’ (not to *ousia*). On the level of reflection attained by Aristotle, first matter cannot be further predicated. In the less well developed science of the 17th century, first matter is characterized by a ‘first form’ (extension in space). How should we conceive of this today?

First matter cannot be characterized other than in terms of the form one can find in it. What characterizes it as matter in this sentence is the ‘can’: according to Aristotle, matter is the possibility of form. The ‘in it’ in the sentence is therefore a pleonasm: first matter is not a something ‘in which’ a form can be found; this would be true only if it were a form still different somehow from the form ‘in it’. Rather, it is the possibility of finding form. What can be found is ipso facto form.

How can form be characterized in terms as general as possible? Whether a particular form is or is not there constitutes an alternative. To distinguish among many different forms we must decide a multiplicity of simple alternatives. In general we can therefore say: Wherever a particular form is found empirically, a number of simple alternatives are being decided empirically. Formulated as the basic hypothesis of ‘ur-alternatives’, we say that all forms consist of combinations of ‘final’ simple alternatives.

In the spirit of the axiomatic construction of quantum theory and of the theory of ur-alternatives, we try to base all postulates and axioms on the analysis of such terms as ‘decision’, ‘prediction’, ‘probability’, and on a fundamental assumption concerning symmetry. The latter is meant to follow from the notion that every decision is a decision among ur-alternatives, and that for every given finite number of ur-alternatives a deviation from the symmetry of its state space would imply the decision of an additional ur-alternative, contrary to the assumption of a given definite number. What this program is worth can be discovered only upon its

[7] Cf. “Quantum Theory”, Sect. 7.5 in this volume: “The Unity of Physics, Part Two”.

completion. I will now assume it to be successful, in order to discuss it as a model of the thesis that substance is form, and of the consequences of this thesis.

Matter is form. Today we understand matter in terms of elementary particles. These are to be constructed in terms of ur-alternatives. Ur-alternatives are the final elements of possible forms; decided ur-alternatives are the final elements of actual forms. The simplest example of form is spatial structure. The theory must therefore deduce the possibility of spatial structure, and does this by reducing space to the quantum mechanical state space of the isolated simple alternative. State space is in turn structured by the probability function, whose meaning rests on the possibility of counting relative frequencies; i.e., on the repetition of the same experiment with many identical simple alternatives. Spatial structure, then, really does consist of many ur-alternatives; actual spatial structure consists of many simultaneously decided ur-alternatives, whose actual frequencies are proportional to the computed probabilities.

Mass is information. In the force-free approximation (and I have not as yet mastered the theory of interactions), the information of an event is simply the number of ur-alternatives in it. In the case of the simplest model of a particle with mass, the rest mass of the particle is the number of ur-alternatives required for the construction of the particle at rest; thus, it is simply the information invested in the particle. The mass of a moving particle is larger, and contains proportionately more ur-alternatives.

Energy is information. The relativistic equivalence of mass and energy enables us to transfer to energy all we have said about mass. The formal relation is simplest in the case of kinetic energy. A free ur-object has a constant energy, the smallest possible cosmic energy quantum E_0. A 'naked neutrino' with the energy $E = n \times E_0$ is simply the superposition of n parallel ur-objects, n being the information of the neutrino.

Movement is form. The flow characteristic of form (information is only what produces information; movement moves itself) only now becomes apparent. The stepwise clarification proceeds as follows:

To begin with, the free ur-object is assumed to be in motion (its state vector contains the factor $e^{-iE_0 t}$). Why? This is in fact the solution of the most general SU_2-symmetric equation of motion; but why, in the absence of external forces, do we not set $E_0 = 0$? (If the number of ur-objects is indefinite, this is not a mere matter of convention.) A first answer is given by the argument for the 'weak law of inertia': interaction can be understood (at least in the case of localizable particles) only if particles move even in the absence of interaction. The weak law of inertia seems to be a precondition for the possibility of interaction. Objects without interaction would not be observable, they would be objects for nobody. In our present way of putting it: information that does not produce information would be information for nobody.

Secondly, the law of inertia in its 'strong' form (i.e., with the factor being precisely $e^{-iE_0 t}$) is a consequence of the action of the universe as a whole on an individual ur-object. Therefore, only this theory fully satisfies the Aristotelian-Machian postulate of no motion without a cause. 'Cause' is meant in the traditional sense of an effectual thing (not of an abstract fact, such as the prior motion of the moving object). Mach accepts the causal paradox of the law of inertia and wishes merely to specify the reference system, which defines uniformity of motion, in terms conformable to his philosophy; i.e., in terms of perceptible things. In locating the cause of motion in the structure of the universe, our theory does more. We can also put it this way: the ur-object is the simplest form; at the same time, it is the quantum of movement. It is form because it is form (through interaction) for something else, it is form in a universe in which it moves something else. It is itself moved as a form in the universe by which it is moved.

The universe appears in this theory as the totality of forms. In its basic conception, the theory thus carries out the radical objectification of semantics. Form is understood only by the form it produces. In this sense, understanding is a part of the great process of self-movement. Whether this is a merely metaphorical or a rigorous expression will be discussed in section f.

The finiteness of the universe is a central problem of the theory. I do not as yet see through this matter, but would like to conjecture as follows. Form occurs in this theory only as form that is knowable in principle. Who is it who knows here, who is the subject? It is man, in a sense, since this is a theory made by men for men; what man in principle cannot know does not occur in the theory. But the theory objectifies semantics; should it then not also describe knowing in terms of measuring devices; i.e., by means of physical objects? Let us first stick to man, so as not to lose all sense of direction.

How much information can men have? Only a finite amount, at any given time. Is there an upper limit on the amount of additional information that could be acquired? Not as far as we know. If the universe is the totality of knowable forms, it must at any given time be finite. But the universe can acquire an additional finite amount of information in a finite time interval. Invoking the objectification of semantics, I interpret this growth in the amount of knowable form as the expansion of the universe. The growth of space, in this sense, is the openness of the future.

11.6 Mind and Form

As we conceive of it, movement always appears as self-moving, form as forming, form also as knowable, and knowledge as form. Is this a theory that objectifies consciousness, or are our formulations obtained in an underhand way?

In the mechanistic world view, the idea that matter can think is an empty postulate. The explanatory power of that view depends on the explicit specification of the defining properties of matter (extension in space, impenetrability); all that could ever be derived from it is the movement of matter thus defined. In our view, however, matter is nothing but the possibility of the empirical decision of alternatives. This presupposes a subject who decides. If this subject can know itself and express this knowledge in terms of empirically decidable alternatives, one must assume that it itself is a part of the universe that is the totality of these alternatives. One can say: We have presupposed knowledge and need assume no more than that knowledge can know itself.

The uniformity of all ur-alternatives now implies that all substance is in principle of the nature of knowledge that knows itself. It will be up to a 'cybernetics of truth' to describe how 'virtual knowledge' can become actual—an immense task, to be sure. The limits of human knowledge must be specified not only with respect to man's organic life (to which the feelings and pressures of our embedment in the environment can be attributed); one must also show in what sense human knowledge knows not only objects but also the knowledge of other men; i.e., in what sense subjective knowledge is trans-subjective. And one must at least ask how the finite human knowledge known to us is limited at its apex. If the essence of substance is form, and form is mind, then it is not a matter of course that mind is limited to man. The Neo-Platonist doctrine that the ideas know themselves seems natural in this context.

For all these questions, however, the conceptions of our approach no longer suffice. Objectifiable forms are static, they can be repeated; the concepts of probability and information are of this kind. In its historical development, knowledge transcends this static quality. The cybernetics of truth would have to describe the process of objectification; and in the objectifying delineation of the possibility of its own method, it would then come up against the limits of that method, which are the limits of objectification itself. From the point of view of transcendental philosophy, the idea of the objectification of a final subject confuses the empirical with the transcendental. It is of the nature of meditation not to objectify. God is not the totality of forms, but their ground.

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Prof. C. F. von Weizsäcker with Prof. E. Scheibe at a Quantum Logic conference in 1984; in the Background Prof. W. Ochs. © Hans Berner who granted permission to use this photo

Carl Friedrich von Weizsäcker with his wife at the Griesser Alm in 1994. © Lili Bansa who granted permission to use this photo

Carl Friedrich von Weizsäcker Society

WISSEN UND VERANTWORTUNG
Carl Friedrich von Weizsäcker-Gesellschaft e.V.

Knowledge and Responsibility
Carl Friedrich von Weizsäcker Society

Modern science, especially the natural sciences, has given us the power of Greek gods. However, we would need the wisdom of Solomon to use this power sensibly. This is not something we have achieved, but rather it is a task facing us—possibly the single most important task of our time. In 1994, the Carl Friedrich von Weizsäcker Society had 18 founding members. Today, the Society's activities include the organization of international symposia and the development of projects on the decisive challenges of our time.

Knowledge Means Responsibility: Responsibility Needs Knowledge

"Knowledge and Responsibility" is our programme in a nutshell. Inspired by the concerns and by the work of Carl Friedrich von Weizsäcker, the Society tries

- to achieve an unbiased and rigorous analysis of our time in five working areas, and
- to develop projects that particularly address the challenges and responsibilities of our time.

"What must we do?" is first and foremost a question of insight; but it carries with it the task of furthering insights by gaining them a hearing and weight. Key programme tasks of the *Carl Friedrich von Weizsäcker Society* are therefore, for example, public conferences, expansion of membership, sponsors, partners and friends; but also to strive to develop in the longer term a "network of reason".

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Areas of Activities

Physics, philosophy, theology, economics and *altered awareness* are the areas of activity that will be addressed in our projects. Throughout his life as a scholar, Carl Friedrich von Weizsäcker has continued to address these areas. This is one motive for your choice. The second is the way they create our history and our future: nowadays all societies and cultures more or less depend on scientific and technical civilization, up to and including the solution of their economic and social problems. Still, *physics* may be considered as a "key science", *philosophy* as a warning voice, "Do you know what you are saying, and do you know what you are doing?" *Theology* is the effort to understand what religious tradition can teach us for today and tomorrow, *economics* tries to understand social, environmental and political problems. *Altered awareness*, finally, the fifth area of activity, and which pervades all the others, explicitly or implicitly, systematically addresses questions of action and ethical stance in our time.

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Carl Friedrich von Weizsäcker Foundation

CARL FRIEDRICH VON WEIZSÄCKER-STIFTUNG

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Modern science, especially the natural sciences, has given us the power of Greek gods. However, we would need the wisdom of Solomon to use the power sensibly. This is not something we have achieved, but rather it is a task facing us—possibly the single most important task of our time. The activities of the *Carl Friedrich von Weizsäcker Foundation*, established in 2002, focus on the organization of international symposia, on the preservation and publication of the scientific legacy of Carl Friedrich von Weizsäcker, and on the development of projects on the key challenges of our times.

The Central Guiding Questions

What should we know? What must we do? What may we hope for?

Immediately bring to mind Kant's "What can I know? What should I do? What may I hope for?" At the same time they imply a change of perspective towards reason jointly applied to the challenges of our time, the practical problems that humankind faces today:

- Science and technology model a world without borders,
- Innovations, technology and the market drive change in our time,
- The global population is growing and increasingly divided into 'young' and 'old' societies,
- The gap between poverty and wealth widens ever further: locally, regionally and globally,

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- The potential for war and terrorism continues to grow, encompassing ethnically and culturally driven conflicts,
- Our use of resources is increasing, placing stress on the biosphere,
- Human power challenges the inherited constitution of nature,
- Overall political order is dominated more and more by the laws of the market,
- Democratic influence on political processes and decisions is waning,
- Ethical stances become relative in the bazaar of opinions.

In the Chap. 8 of his book *Der Mensch in seiner Geschichte* [Humankind in its History] von Weizsäcker reflects on his adaptation of Kant's questions under the heading "Where are we going?": poverty and wealth, war and peace, human beings and nature, the problems are not resolved. But "with jointly applied reason they would be solvable". This is what Carl Friedrich von Weizsäcker has argued for throughout his life as a scholar: not from the perspective of a developed theoretical system but with rationality following the example of everyday speech, "Be reasonable!" And "Our task for today is the global search for truth". And "Reason means recognizing the necessary, and applied in common, to bringing into being what has been recognized as necessary." If we fail to broaden and deepen our understanding of what lies at the core of the challenges of our time as far as we can, there is a constant danger that we might cause more harm than good. "Hope is the perception of the possible" wrote von Weizsäcker in answer to his third question, and at the end of his book he speaks of his hope in these words: "I have tried to speak about what I have experienced. Others may experience other things, more things. They will act."

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Federation of German Scientists

Founding members: G. Burkhardt, C.F. v. Weizsäcker. W. Gerlach

The Federation of German Scientists (FGS; German acronym VDW) was founded in 1959 in West-Berlin by renowned nuclear scientists, including Carl Friedrich von Weizsäcker and the Nobel Prize laureates Max Born, Otto Hahn, Werner Heisenberg, and Max von Laue.

Two years earlier this group of experts had become well-known to the public as 'Göttinger 18': Nuclear scientists who had publicly argued against a nuclear armament of the German Bundeswehr. Since then the FGS feels bound to the tradition of responsible science. It has nearly 400 members from different fields of the natural sciences, the humanities, and social sciences, so that a large range of topics is approached at a high level of competence. With the results of its interdisciplinary work the Federation of German Scientists not only addresses the general public, but also the decision-makers at all levels of politics and society.

The members of FGS stand in this tradition. They feel committed to taking into consideration the possible military, political, economic and social implications and possibilities of atomic misuse when carrying out their scientific research and teaching.

In Annual Conferences and in interdisciplinary Expert Groups as well as public comments it addresses issues of science and technology on the one hand, and peace and security policy on the other. At the same time, the role of science itself

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in genesis and in solution of socio-technological problems is subject of examination and expertise. FGS’ membership lists also include representatives of the humanities and social sciences, so that a large range of topics is approached at a high level of competence. With the results of its interdisciplinary work the Federation of German Scientists not only addresses the general public, but also the decision-makers at all levels of politics and society. According to its statutes of 1959, the FGS aims to

- keep up and deepen the awareness of those working in science for their responsibility for the effects which their work has on society;
- study the problems which result from the continuous development of science and technology;
- assist science and its representatives in making public the questions related to the application of scientific and technical developments;
- provide advice and thus exercise influence on decisions as long as they are assessable and can be dealt with by means of scientific knowledge and methods, and to point out all forms of misuse of scientific and technical results;
- to defend the freedom of scientific research and the free exchange of its results and to expand and strengthen the traditional international cooperation of scientists.

Carl Friedrich von Weizsäcker had been part of the famous ‘Göttinger 18’, the group of renowned nuclear scientists who publicly opposed a possible nuclear armament of West Germany in the 1950s, and was among the founding members of the Federation of German Scientists in 1959. His spirit, his way of perceiving the world and his understanding of the role and responsibility of science for society and the development of humankind profoundly shaped the self-perception and sphere of influence of the FGS in its early years and later on. He also repeatedly served in public functions of the FGS, most notably as its chairman from 1969 to 1973.

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Udo Keller Stiftung Forum Humanum

UDO KELLER STIFTUNG
FORUM HUMANUM

Mission Statement

The name reflects the programme of action. The Foundation, set up by the Hamburg businessman Udo Keller, sees itself as a *Forum Humanum*—a forum for all those who would like to investigate the question of the truly human. At a time when technology and economic processes are increasingly influencing human choices, the Foundation addresses the importance of the moral and religious heritage of human cultures worldwide. The Foundation assumes that the future development of human beings will decisively depend on whether we succeed in harnessing the rich potential of these traditions for the future. In this way the Udo Keller Foundation argues for a revival of the question of the purpose of human life in twenty-first century terms.

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Funding Priorities

The *Udo Keller Foundation Forum Humanum* contributes to an interdisciplinary dialogue between natural sciences and the humanities as well as to the multi-faith dialogue between world religions. These goals are being realized at its headquarters in Neversdorf near Hamburg and at its study centre in Tübingen, the *FORUM SCIENTIARUM at the Eberhard Karls University of Tübingen*.

Funding Activity

The *Udo Keller Foundation Forum Humanum* is a co-founder of the interdisciplinary project *FORUM SCIENTIARUM at the Eberhard Karls University of Tübingen* and is one of several inaugurators of the *Academy of World Religions* at the University of Hamburg. The Foundation has sponsored the *Verlag der Weltreligionen* (World Religions Press) since its establishment in 2007, and has initiated various lecture series in Hamburg and Tübingen—including *Thinking the future* (ZUKUNFT denken) in Hamburg in cooperation with the Hamburg Planetarium (2010–2014) and the *Unseld Lectures* at Tübingen (from 2008). Together with the German Literary Archives in Marbach, the Foundation has funded since 2008 the *Udo Keller Scholarship for Contemporary Research into Religion and the Modern Age*.

Additional information on the work of the *Udo Keller Foundation Forum Humanum* may be accessed in German on its website at: www.forum-humanum.org.
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Ruhr University Bochum

Portrait

Located in the midst of the dynamic, hospitable metropolitan area of the Ruhr, in the heart of Europe, the Ruhr-University Bochum (RUB) with its 20 faculties, RUB's disciplinary institutional units, is home to 5,600 employees and over 41,000 students from 130 countries. All the great scientific disciplines are united on one compact campus.

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The trio of values, people-centred—cosmopolitan—high-performance, represent the cornerstones of the RUB environment. This space is more than just the sum of its individual elements: People-centred and cosmopolitan means to respect diverse cultures and to give guests a home. People-centred and high-powered means jointly developing creative forces, to 'tackle' things with verve and ambition. 'Campus RUB' is the contemporary *universitas*—the community in which people take centre stage.

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The members of the *universitas* teach others and, at the same time, learn from each other—whether in science, studies, engineering, or management. *Universitas* promotes and demands codetermination from everyone—a far cry from academic or hierarchical structures. Living universitas is: When students in the eTutoring project plan courses; when the promotional funding line of the research fund is judged solely by doctoral students; the uncomplicated interaction of researchers from various disciplines.

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About the Author

Carl Friedrich Freiherr von Weizsäcker[1] (June 28, 1912—April 28, 2007) was a German physicist and philosopher. A member of the prominent Weizsäcker family, he was son of the diplomat Ernst von Weizsäcker, elder brother of the former German President Richard von Weizsäcker, father of the physicist and environmental researcher Ernst Ulrich von Weizsäcker, and father-in-law of the former General Secretary of the World Council of Churches Konrad Raiser.

Born in Kiel, he was raised in Stuttgart, Basel, and Copenhagen. From 1929 to 1933, Weizsäcker studied physics, mathematics and astronomy in Berlin, Göttingen and Leipzig supervised by and in cooperation with Werner Heisenberg and Niels Bohr, among others. The supervisor of his doctoral thesis was Friedrich Hund.

Weizsäcker made important discoveries in theoretical physics regarding the masses of atomic nuclei, energy production in stars from nuclear fusion processes, and on planetary formation in the early Solar System. During World War II he participated in the German program for developing nuclear energy and atomic bombs. In his later career, he focused on philosophical and ethical issues, and was awarded several international honours for his work in these areas.

Work on nuclear physics Weizsäcker's special interest as a young researcher was the physics of the atomic nucleus. Simultaneously with Hans Bethe he found a

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mechanism or pathway for the cyclic process of fusion in stars (Bethe-Weizsäcker process, published 1937–1939). This discovery should not be confused with his 1935 development of the Bethe-Weizsäcker formula, or Semi-Empirical Mass Formula (SEMF) for nuclear masses, again simultaneously with Hans Bethe.

Work on planetary formation In 1938, Weizsäcker developed a theory of the formation of the Solar System, based mainly on considerations of turbulent motion of gases and dust. The theory also helped to explain the empirically observed regular pattern of increase in the diameters of the orbits of the planets of the Solar System, from inward to outward.

Work on atomic weapons As a theoretical physicist, Weizsäcker (and by his own estimate, 200 other physicists) had recognized immediately after nuclear fission had become known (by Otto Hahn) in 1938 that nuclear weapons could potentially be built. He discussed the upsetting implications in February 1939 with philosopher friend Georg Picht.

During World War II, Weizsäcker joined the German nuclear energy project, participating in efforts to construct an atomic bomb. For some time he had been hoping for political influence growing out of participation in a successful nuclear weapons project. In July 1940 he was co-author of a report to the Army on the possibility of "energy production" from refined uranium. The report also predicted the possibility of using plutonium for the same purpose including the production of a new type of explosives. During summer 1942 Weizsäcker drafted a patent on a transportable "process to generate energy and neutrons by an explosion … e.g., a bomb", which was never filed. The draft was found in the 1990s in Moscow.

Historians have been divided as to whether Heisenberg and his team were sincerely trying to construct a nuclear weapon. In a 1957 interview with the German weekly Der Spiegel, Weizsäcker frankly admitted to the scientific ambitions of those years: "We wanted to know if chain reactions were possible. No matter what we would end up doing with our knowledge—we wanted to know." Weizsäcker said that they were spared the decision on building the bomb as they saw rather soon that the German war economy was unable to mobilize the necessary resources.

Weizsäcker worked later during the war as a professor in Strasbourg. The American capture of his laboratory and papers there in December 1944 revealed to the Western Allies that the Germans had not come close to developing a nuclear weapon.

Post-war career In 1946, Weizsäcker became director of the department for theoretical physics in the Max Planck Institute for Physics in Göttingen. Weizsäcker felt that the scientists who had developed the foundations of such powerful theories as that of the atomic nucleus, should take on the responsibility for the consequences. In 1957, it was mainly he who formulated the protest of the 'Göttinger 18', a group of prominent German physicists, against the idea that the West German armed forces should be equipped with tactical nuclear weapons. He suggested that West Germany should declare its definitive abdication of all kinds of nuclear weapons. From 1957 to 1969, Weizsäcker was professor of philosophy at the University of Hamburg. From 1970 to 1980, he was head of the Max Planck

Institute for the Research on Living Conditions in the Modern World in Starnberg. He researched and published mainly on philosophy and foundations of physics, but also on the danger of nuclear war, which he thought underestimated by the public and the political establishment, on the conflict between the First World and the Third World and the consequences of environmental degradation, and on the world as an interlocking whole ('Weltinnenpolitik'). In the 1970s he founded, together with the Indian philosopher Pandit Gopi Krishna, a research foundation "for western sciences and eastern wisdom".

After his retirement in 1980 he intensified his work on the conceptual foundations of physics and on philosophical issues. In the 1980s he invested much of his creative energy in the promotion of what was originally called a "Council for Peace". The movement resulted in the "World Convocation on Justice, Peace and the Integrity of Creation" in Seoul in 1990.

Weizsäcker developed the theory of ur-alternatives (archetypal objects), publicized first in his book Die Einheit der Natur (1971; English translation "The Unity of Nature", 1980) and further developed through the 1990s. The theory axiomatically constructs quantum physics and uses it to discuss the foundation of a universal physics on the quantum mechanics of binary alternatives. Weizsäcker used his theory, a form of digital physics, to derive the 3-dimensionality of space. The program has not, so far, come to an end. In 2007, Weizsäcker died at the age of 94 in Starnberg, Germany.

Awards and honours Max Planck Medal (1957), Goethe Prize of the city of Frankfurt am Main (1958), Pour le Mérite for Science and Art (1961), Peace Prize of the German Book Trade (1963), Erasmus Prize of the city of Herdam (1969), Austrian Medal for Science and Art (1969), Grand Merit Cross with Star and Sash of the Federal Republic of Germany (1973) Ernst Hellmut Vits Prize of the University of Münster (1982), Heinrich Heine Prize of the city of Düsseldorf (1983), Sigmund Freud Prize for Scientific Prose (1988), Templeton Prize for "Progress in Religion" (1989), Theodor Heuss Prize "for his world-renowned, diverse and dedicated contributions to humanity themes: peace—justice—Integrity of Creation" (1989), Prix Arnold Reymond (University of Lausanne), Hanseatic Goethe Prize, Karl IV Prize of the City and University of Prague.

Honorary degrees *Law* Free University of Amsterdam, University of Alberta, University of Aberdeen; *Theology*: University of Tübingen, University of Basel; *Science*: Karl Marx University, Leipzig; *Philosophy*: Berlin Institute of Technology, University of Aachen.

Memberships Max Planck Society for the Advancement of Sciences, German Academy of Sciences Leopoldina, Göttingen Academy of Sciences, Saxon Academy of Sciences, Austrian Academy of Sciences, Bavarian Academy of Sciences, Bavarian Academy of Fine Arts, German Physical Society, Académie des Sciences Morales et Politiques, American Physical Society, Croatian Academy of Sciences and Arts, German Academy for Language and Literature, Joachim-Jungius Society of Science/Hamburg Academy of Sciences, Hamburg Institute for Human Sciences.

Among his major publications are *Zum Weltbild der Physik* (Leipzig 1946, 2002, 14th edition, renewed and with introduction by Holger Lyre) [*The World View of Physics* (London, 1952); Le Monde vu par la Physique (Paris 1956)]; *Der begriffliche Aufbau der theoretischen Physik* (Lecture Notes 1946) (Stuttgart 2004); *Die Geschichte der Natur* (Göttingen 1948) [*History of Nature* (London 1951)]; *The Relevance of Science* (London - New York 1964); [*Die Tragweite der Wissenschaft* (Stuttgart 1990)]; *Die Einheit der Natur* (Munich 1971) [*The Unity of Nature* (New York 1980)]; *The Biological Basis of Religion and Genius*, Gopi Krishna (New York 1971), intro. by Carl Friedrich von Weizsäcker, which is half the book; *Wege in der Gefahr* (Munich 1976); [*The Politics of Peril* (New York 1978)]; *Der Garten des Menschlichen* (Munich 1977) [*The Ambivalence of progress*, essays on historical anthropology (New York 1988)]; *Deutlichkeit: Beiträge zu politischen und religiösen Gegenwartsfragen*, (Munich 1978); *Der bedrohte Friede* (Munich 1981); *Wahrnehmung der Neuzeit* (Munich 1983); *Aufbau der Physik* (Munich 1985) [*The Structure of Physics* (Heidelberg 2006)]; *Die Zeit drängt* (Munich 1986); *Bewusstseinswandel* (Munich 1988); *Der Mensch in seiner Geschichte* (Munich 1991); *Zeit und Wissen* (Munich 1992); *Große Physiker* (Munich 1999). See also the website on this book on Carl Friedrich von Weizsäcker, at: <http://afes-press-books.de/html/SpringerBriefs_PSP_C.F.v._Weizsaecker.htm>.

Prof. Dr. Michael Drieschner (the editor), Prof. Dr. Ernst Ulrich von Weizsäcker (a son of the author) and Prof. Dr. Hartmut Grassl visiting the photo exhibition organized by the German Federation of Scientists (GFS or VDW) as part of a symposium on the occasion of the 100th birthday of Carl Friedrich von Weizsäcker in Berlin, 29 June to 1 July 2012. *Source* German Federation of Scientists which granted the permission to publish this photo

About the Editor

Michael Drieschner (born 1939) is a professor em. of Philosophy of Nature at the University of Bochum, Germany. After passing the 'Diplom' exam in physics (Munich 1964) he obtained his PhD in philosophy at the University of Hamburg in 1968 in the research group of Carl Friedrich von Weizsäcker with a work on the axiomatic structure of quantum mechanics. From 1970 to 1978 he was a researcher at the 'Max-Planck-Institute for Research on the Conditions of Life in the Scientific-Technological World' in Starnberg, Germany, again collaborating with C. F. v. Weizsäcker. In 1979 he published his *Voraussage—Wahrscheinlichkeit—Objekt* (Heidelberg 1979), a treatise on the foundations of quantum mechanics. From 1986 to 2006 he taught 'Naturphilosophie' (philosophy of nature) at the University of Bochum.

Further books are (Titles translated into English): *Introduction to the Philosophy of Nature* (Darmstadt: 1981/91); *Carl Friedrich von Weizsäcker—an Introduction* (Hamburg 1992); *Modern Philosophy of Nature* (Paderborn 2002). With L. Castell and C. F. v. Weizsäcker he was a co-editor of the first two volumes of: *Quantum Theory and the Structures of Time and Space* (Munich: Hanser, 1975 and 1977); he edited the collected works of C. F. v. Weizsäcker on CD-ROM (Berlin 2011). His papers on physical themes in English language are: "Lattice Theory, Groups, and Space", in: L. Castell, M. Drieschner, C. F. v. Weizsäcker (Eds.): *Quantum Theory and the Structures of Time and Space* (Munich: Hanser, 1975): 55–69; "Is (Quantum) Logic Empirical?", in: *Journ. Philos. Logic*, 6 (1977): 415–423; "The Abstract Concept of Physical Object", in: L. Castell, M. Drieschner, C. F. v. Weizsäcker (Eds.): *Quantum Theory and the Structures of Time and Space 2* (Munich: Hanser, 1977): 20–31; "The Subject Matter of

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Home page http://www.ruhr-uni-bochum.de/philosophy/staff/drieschner.

Carl Friedrich von Weizsäcker with his wife on their Alm in the Alps (1982). © The Weizsäcker Family represented by Dr. Elisabeth Raiser who granted permission to use this photo

About the Book

This volume includes a collection of texts by the German physicist and philosopher Carl Friedrich von Weizsäcker (1912–2007) in English, for the use in seminars on the history, the epistemology, and the structure of physics. Weizsäcker became famous through his works in physics, mainly in the early development of nuclear physics. Later he was well known as a philosopher and analyst of contemporary culture. Texts include the original publication of the "Bethe-Weizsäcker cycle" that explains the source of energy in the sun; it was developed by Bethe at the same time independently of Weizsäcker. Further texts deal with Weizsäcker's explanation of the origin of planets, his explanation of the 'time arrow' in Statistical Mechanics, and his analyses of Quantum Mechanics and the structure of physics in general.

The selected texts by one of the most important physicists of the twentieth century focus on the history of physics, nuclear physics and its foundations, and on the foundations of physics and science. They are unique for graduate students studying the history and epistemology of physics. They offer original sources for discussions on the foundations of quantum mechanics and the structure of physics. See also the website on this book on Carl Friedrich von Weizsäcker, at: <http://afes-press-books.de/html/SpringerBriefs_PSP_C.F.v._Weizsaecker.htm>.

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