identity and synonymy of the nearly one hundred thousand so-called species of fungi in mycological literature are insuperable, as has been found by actual experience in studying a few genera, even with all the library and herbarium facilities of America and Europe available.

In order to facilitate the preparation of a list on a basis of usage it may be found desirable to omit from consideration under this plan names of less than 25 years standing, as such names could not perhaps in most cases be regarded as established by usage. As there is considerable dissatisfaction with present codes and their operation even among their adherents—as is evidenced by recent proposals in America and also by the report of the English committee cited—we hope careful consideration may be given to the usage plan.

This plan has the great merit of relieving us of the necessity of abandoning many of the names with which we have long been familiar and learning new and strange names in their place. This is a matter of great practical importance with most users of plant names and has been the source of much of their opposition to the various efforts to reform nomenclature. A well-prepared plan of this kind would probably receive the approval of the majority of botanists who are not particularly interested in taxonomy but still need to use plant names.

The selection of names for all plants on the basis of usage involves no difficulties other than those already overcome by the committee which prepared the list of standardized names mentioned. The zoologists have found it necessary to establish a commission to decide mooted questions regarding the choice of names under their code which is founded on the principle of priority, and a commission of expert plant taxonomists should find no greater difficulties in determining the choice of plant names on the basis of current usage.

As an example of the result of following usage as compared with priority among the fungi we may cite the genus Daldinia, a common and conspicuous Pyrenomycete. This generic name was applied by Cesati and de Notaris in 1863, and two species included *D. concentrica* and *D. vernicosa.* These are regarded as forms of one species by some mycologists. Fortunately, the priority rule has not yet been applied to the majority of fungus names and the name Daldinia has been generally used for these plants by the mycologists of the world ever since it was proposed. However, there are already known three other generic names which had previously been applied to this species. Perisphaeria, Roussel, 1808, and Peripherostoma, S. F. Gray, 1821, are typonyms, being based upon the same species as Daldinia. The third, Stromatosphaeria, Greville, 1824, included 19 species of which the first was *S. concentrica* and would therefore, according to the first species method, be taken as the type of the genus. What we propose is to accept Daldinia as the only valid name for this genus with the type species, *D. concentrica* Bolt., fixed and unchangeable. As to the specific name, *concentrica,* applied by Bolton in 1791, three other specific names of the fungus are already known which may claim priority of publication. These are *atrum* (*Lycoperdon atrum* Schaeffer, 1770), *tuberosa* (*Valsa tuberosa* Scopoli, 1772) and *tunicata* (*Sphaeria tunicata* Tode, 1791). On the priority basis the specific name would be *atrum.* We propose, however, to adopt the name *concentrica* because of general usage. As no original specimen of Bolton is known, a type specimen should be arbitrarily chosen. Cesati and de Notaris cite several specimens in connection with their description of the genus and the first of these might very properly be regarded as type of the species for future purposes. The specimen cited is Erb. Critt. Ital., No. 642. This set of exsiccati is found in the principal large herbaria, and typical specimens are therefore much more accessible to mycologists than the types of most authors.

As a reconsideration and modification of botanical codes is under discussion now, we would suggest that a more general and distinct recognition of usage be provided for in any revision that may be made.

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THE QUANTUM PUZZLE AND TIME

THE essential feature of the quantum theory is the postulate which restricts any periodic motion of an atom or molecule to a discrete series of allowed states of motion with wide gaps between which are not allowed. Stated in ordinary mechanical terms the quantum postulate may be exemplified as follows:

(1) *Simple to and fro vibration*: Consider a material particle of mass m, bound by a spring, and oscillating to and fro so that its distance q from its equilibrium position is

$$q = A \sin \omega t \qquad \text{(i)}$$

where A and ω are constants and t is elapsed time. Let p be the momentum ($= m \frac{dq}{dt}$) of the particle. Then

$$p = \omega m A \cos \omega t \qquad \text{(ii)}$$

and if we eliminate t from (i) and (ii) we get

$$\frac{A^2}{q^2} + \frac{p^2}{\omega^2 m^2 A^2} = 1 \qquad \text{(iii)}$$

This is the equation of an ellipse (if p and q are thought of as rectangular coordinates). The area of this ellipse is

$$\int p.dq = \pi \omega m A^2,$$

and because $p.dq$ is a quantity of the same nature as Planck's constant h the idea has arisen that the area of the pq-ellipse might be equal in general to an integral multiple of Planck's constant, or

$$\pi\omega m A^2 = nh \qquad \text{(iv)}$$

where n is an integer. This equation expresses Bohr's quantum postulate as applied to simple to and fro vibration, and the reader must not expect to know the "why" of this postulate, because no one knows the "why" of it. We deliberately and arbitrarily put $\pi\omega m A^2$ equal to nh, that is all there is to it—except results of the most remarkable kind; and when we put $\pi\omega m A^2$ equal to nh we are said to *quantize* the motion.

Solving equation for A we get

$$A = \sqrt{n}\ \sqrt{h/\pi\omega m} \qquad \text{(v)}$$

and the restriction which equation (iv) places on the simple to and fro motion of equation (i) is that the amplitude A of the motion can not have any value whatever but only a discrete series of values which are proportional to the square roots of the successive integers $n = 1, 2, 3, 4$, etc. This conclusion is absurd from the ordinary mechanical point of view because it means, for example, that a pendulum bob can not oscillate with any amplitude whatever but only with amplitudes which form a discrete series as expressed by equation (v).

(2) *Simple rotation*: When applied to simple rotation Bohr's postulate restricts the speed of rotation of a given body to a discrete series of speeds for which the angular momentum of the rotating body is an integral multiple of $h/2\pi$, which means that the only possible speeds in revolutions per second are those which are integral multiples of $h/4\pi^2K$, where K is the moment of inertia of the body.

This conclusion is absurd from the ordinary mechanical point of view because it means, for example, that a grindstone can not have any speed whatever, but that all possible speeds must constitute a discrete series so that if a grindstone were speeded up it would have to increase its speed by sudden jumps!

These sudden jumps in speed evidently mean discontinuity; and, in general, Bohr's postulate means discontinuity of time or discontinuity of space, or both. It seems strange, therefore, that we should take Bohr's postulate seriously, considering that the postulate is merely a "happy thought" of Bohr's which nobody understands (Bohr himself does not pretend to understand it) and considering that the postulate leads to results which are absurd from the ordinary mechanical point of view. We do take Bohr's postulate seriously, however, because it has led to a theory of line spectra which is in extremely exact agreement with nearly all the known facts of spectrum analysis, to say nothing of several other highly important applications of the Bohr and Planck quantum postulates. On the other hand, the apparent absurdity of the Bohr postulate from the ordinary mechanical point of view can not be demonstrated experimentally, that is to say, the discrete series of allowed states of motion of a pendulum, for example, are so extremely close together that the discrete series is indistinguishable experimentally from a continuous series.

We do an injustice to the remarkable ingenuity of Bohr when we speak of his postulate as a mere "happy thought." It would be much nearer the truth to say that Planck was constrained to his original quantum postulate by a keen appreciation of the experimental facts of heat radiation and that Bohr was constrained to his quantum postulate by his keen appreciation of the experimental facts concerning line spectra. The quantum postulates (Planck's and Bohr's) are perhaps the most ingenious contrivances ever evolved from the contemplation of experimental facts in physics.

It is not the purpose of this note to set forth even the simpler aspects of Bohr's quantum theory of line spectra but rather to point out that our notion or intuition of time seems to become meaningless in connection with quantized motion.

It is to be noted that the time t is at least formally eliminated from equations (i) and (ii) to give equation (iii), and to describe the state of a simple oscillator on the basis of equation (iii) is to make use of the momentum p as a basic idea instead of time. Similarly, angular momentum becomes basic in the quantum-theory description of the state of a simple rotor. Of course momentum, as ordinarily thought of, involves the idea of velocity and therefore also the idea of time; but if we postulate momentum as a basic idea and if we could refrain from "thinking" of the state of an oscillator or rotor as "motion" we would have time-free descriptions of the states of an oscillator or rotor. This statement should call to the reader's mind the central paradox of Bohr's theory, namely, that although the Bohr states of the hydrogen atom are described as orbital motions when we wish to *think* about these states, yet such descriptions seem to be essentially artificial, and the motion, as we think of it, really non-existent. The difficulty is that time is to us an essential mode of thought, whereas there is no actual physical condition or thing bound up in a Bohr state which corresponds to time as a fact. This is, of course, a dilemma; any resolution of this dilemma, however consistent and logical, will be necessarily unthinkable; and, before proceeding to discuss an unthinkable resolution of this dilemma, let me paraphrase a statement of Bohr's.

We are not at all justified in assuming that our human ways of thinking about things we see and handle are suitable ways of thinking of atomic action. Our human ways of thinking are bound up inextricably with our intuitions of space and time, and atomic action may not take place in what we call space and time.

This is the dilemma.

What fact or condition in nature is it that comes nearest to our intuition of time? or is most closely in accord with our feeling of an inevitable forward movement which we call time? What fact or condition in nature is it that most certainly justifies our idea that something or other in general in this world of ours always does go forward and never does nor ever can go backwards? What observed condition or thing is it that embodies as a fact the essential quality of intuitive time? The answer, as it seems to me, is evident. It is what in thermodynamics we call the irreversibility of natural processes. This condition or fact underlies the second law of thermodynamics, and the law of increase of entropy is the most completely non-anthropomorphic generalization that grows out of it. The law of increase of entropy and our intuition of time unquestionably grow out of the same condition in nature, but entropy and time as physical quantities differ[1] from each other very greatly because of our artificial methods of measuring them.

Imagine a purely mechanical system, a system not involving any irreversible action. After sufficient "time" such a system will certainly come back to its initial condition, and everything will be as at first; except that "time" has elapsed. What does this mean? Where has "time" elapsed? What is this elapsed "time" which makes a difference between the initial and final states of our system which initial and final states are exactly alike? The difficulty is that someone is supposed to look at or contemplate our supposed purely mechanical system. This someone is certainly not a purely mechanical thing, and therefore the totality of things under consideration is not purely mechanical in the sense of being entirely free from irreversible action. The moment you look at or contemplate a perfectly mechanical system the time idea or the time intuition becomes essential and real.

However, if time is an objective condition and if it is bound up with thermodynamic irreversibility it can not have a universal and uniform forward flow, it must go forwards irregularly and unequally as resident in different things, and it can not go forwards at all as resident in a purely mechanical thing.

[1] The simplest argument which leads to the notion of entropy makes increase of entropy proportional to lapse of time. See Nichols and Franklin's "Elements of Physics," Vol. II (1894), or see Franklin and MacNutt's "Heat" (1924), pp. 128–140.

A purely mechanical world would certainly be timeless. The introduction of a mere observer would introduce time as a fact, this fact-time would be wholly bound up in the observer as a non-mechanical thing, and this fact-time would be irregular if the observer is of the ordinary sleeping and waking kind! Furthermore, the introduction of a thinker would introduce time as a uniform forward blow, time as an intuition, time as a mode of thought.

About all we can say of the steady states of the hydrogen atom as conceived by Bohr is that they are *steady states,* states which involve no irreversible action, no absorption or emission of radiation, no change of any kind. Now a purely mechanical system is an unrealizable ideal because radiation exists everywhere and no mechanical system can be so isolated as to be free from irreversible action. But a hydrogen atom in a steady state is immune to radiation and probably immune to electron bombardment when certain threshold conditions are not over passed so that a hydrogen system in a steady state is perhaps entirely free from irreversible action, a kind of super-mechanical system, as it were. The idea of lapse of time would therefore seem to be an absurdity when applied to a hydrogen atom in a steady state, although we must necessarily make use of the idea of time in describing a Bohr steady state as "motion."

In the Bohr theory the atom is supposed to jump from one quantized state to another of lesser energy, and the energy lost is supposed to be radiated in accordance with Planck's postulate which is that

$$h\nu = \Delta W \qquad \text{(v)}$$

where h is Planck's constant, ΔW is the energy lost by the atom, and ν is the frequency of the emitted radiation. But what happens "while" the jump is taking place, if time does not go forwards at all during a steady state, and does go forwards discontinuously or with a jump "during" the transition from state to state? This is the kind of attempt at thinking that one is repeatedly making in considering the Bohr theory which seems really to demand the non-existence of time!

Non-existence of time; suppose such to be the case. But the emitted radiation has a definite measurable wave-length or frequency, and, surely, a frequency necessarily involves time! This is the way we think of a frequency, to be sure, and two things may be said in criticism of the way in which we think of such a thing; but before saying these things let me suggest (and I admit that my suggestion is extremely vague) that the time element which enters to fix the frequency of the emitted radiation may be bound up with an entropy change which is associated with the transition of the atom from state to state. The Bohr jumps are now thought to be reversible because radiation seems sometimes to be absorbed by an atom and cause a jump to perform itself backwards; but I certainly believe that some essential element of irreversibility must eventually be discovered in atomic action, because the older kinetic theory as developed statistically leaves irreversibility essentially unexplained.

Where is the fallacy of the time-idea in our notion of frequency? Look at a swinging pendulum and count its movements in a measured time. This you can do, and you thus find the frequency. Similarly, let me ask you to look at a hydrogen atom in a steady state and count the number of revolutions of the electron in a measured time. This sounds logical enough, but the atom in a steady state does not radiate, and there may be a fundamental fallacy in even imagining that one might look at such an atom. As we see things, so we think of them, and our see-thinking may be absurd when carried over to things which are essentially un-seeable; essentially un-seeable, mind you, not merely too small to see.

Or suppose you look at the "kinks" (waves?) of the emitted radiation as they come out of an atom when it jumps from Bohr state to Bohr state and count the number of kinks in a measured time. This also sounds logical enough, but after the jump the atom is in a steady state and no time elapses in the atom, and once a radiation is established the radiation is itself a steady state and no lapse of time can reside in the radiation. The idea of frequency would seem to be applicable to radiation only when the radiation has stretched out in our large-scale world and has come into relation with large-scale things where time is a legitimate idea. Our intuitive notion of time, as it seems to me, is tenable only in large-scale physics, or macro-physics, but untenable in small-scale physics, or micro-physics.

My suggestion that time does not exist in a purely mechanical system refers only to what we think of as continuous time or time flow, it is not intended to deny the reality of coincidences in time.

All, or nearly all, of our time and space experience grows out of coincidences in time and coincidences in space. Even the measurement of a length depends wholly on coincidence observations. A yard stick is fitted to the successive parts of the distance to be measured and each such "congruence operation" involves two coincidence observations, one at each end of the yard stick. The measurement of a time interval also consists almost wholly of coincidence observations. Furthermore, the vast complex of everyday life in its sense aspects involves little else than coincidence observations. But even men of the street are accustomed to express time and space experiences in terms of quantitative ideas, and this purely mental habit has come about, no doubt, because to express these experiences in purely experimental terms would be extremely tedious. Our quantitative notion of time as a continuum of duration and our quantitative notion of space as a continuum of extension come wholly, it would seem, from our mathematical predilections. No one, as it seems to me, could maintain that these quantitative notions of space and time are essential in the most complete sense-orientation of a man in any situation in life; but it would be impracticably and even unintelligibly tedious to talk about space and time experiences without using quantitative ideas. Herein lies the reason why any point of view which is contrary to our ideas of continuous space and continuous time is unwelcome. Our mathematical bias, note that I now use a stronger word than predilection, is wholly in favor of continuous mathematics and wholly opposed to discontinuous mathematics, and the reason of this bias is evident to those who have attempted to develop a discrete or discontinuous mathematics!

I am convinced that the qualifications of our ordinary notion of time which I have suggested[2] contain nothing whatever that is inconsistent with experience, and, however absurd these qualifications may seem to be, it must be admitted, as it seems to me, that the quantum puzzle becomes more clearly defined as a puzzle in terms of these qualifications.

If the Planck and Bohr postulates contain some new thing that is essential for the description of atomic action, and no one who is familiar with the amazing developments that have been made on the basis of these postulates can doubt that they do contain something new which is essential, then we must expect soon to see a more wonderful transformation of our conceptions of the physical world, a vastly more wonderful transformation, than that which has resulted from the relativity theory.

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SCIENTIFIC EVENTS

EXHIBIT OF THE ROYAL SOCIETY AT THE BRITISH EMPIRE EXPOSITION[1]

ONE of the most fascinating and impressive sections of the British Empire Exhibition, though admittedly one that is essentially specialist, is the exhibition of pure science arranged by the Royal Society. In connection therewith the Royal Society has now issued a handbook which is a great deal more than a mere catalogue, and is, indeed, a volume which might well be secured by students of pure science and amateurs,

[2] This suggestion was first made in a paper on "Entropy and time" in the *Physical Review*, Vol. XXX, pp. 766–775, June, 1910.

[1] From the London *Times*.