Committee I
The Limits of Science?

DRAFT--6/15/91
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THE LIMITS OF SCIENTISTS' SCIENCE

by

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The Eighteenth International Conference on the Unity of the Sciences
Seoul, Korea August 23-26, 1991

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It is often said and even believed that scientists on the whole are free of dogma and that scientific enquiry is characterised by the by the Latin tags 'Felix qui potuit rerum cognoscere causas' and 'Omnia probate bonum tenete'. In other words scientists are continuously trying to find out why things are or happen and success in their endeavours brings them happiness; they try everything and retain what is good.

I want to discuss in this paper to what extent we scientists live up to these lofty ideals. We may be free of dogmas, we may refuse, sometimes at considerable risk, to adopt concepts or theories laid down by religious or political authorities but we are on the whole conservative and suspicious of new ideas which seem to contradict accepted theories and we are slow in accepting them. Once observations or experiments seem to have confirmed beyond reasonable doubt a certain theory we tend to believe in it. We also regard it as axiomatic that phenomena perceived by the senses are governed by and are capable of explanation by well-established laws. This attitude may retard or thwart the progress of science. It is somewhat disingenuous for scientists to reproach the Church for its persecution of Galilei for his heretical views. We scientists have no Pope but we have a powerful 'esprit de corps'. I shall give some case histories, some of them well documented others anecdotal. They also differ greatly in their effect on the advancement of science or on society. They range from a case in which the rejection of a new idea resulted in the deaths of literally thousands of human beings to cases where the effects were less dramatic, perhaps just a few red faces.

THE CAUSE OF PUERPERAL OR CHILDBED FEVER.

Childbed fever has been known since ancient times but it did not become a scourge until the 17th century. Thus it was quite common in Europe for 10% of the women brought to bed in Lying-in Hospitals to die of the disease and it was said that during the 1773 epidemic not a single woman survived childbirth in Lombardy.

A Hungarian doctor, Ignác Fülöp Semmelweis became, at the age of 28, Assistant in the Maternity Division of the Vienna General Hospital in 1846. It was he who discovered the cause of puerperal fever and prescribed a method to prevent it or at least to reduce the frequency of its occurrence. At that time the generally accepted explanation was that the contagion spread through the air and that atmospheric conditions, temperature, humidity all had influence on the effectiveness of the 'miasma'. It must be remembered that Semmelweis's discovery predated by nearly 20 years Lister's work on antisepsis and Pasteur's discovery that fermentation as well as putrefaction in wounds etc. are caused by living organisms.

The Lying-in Hospital in Vienna was divided into two clinics each with its own staff. As the following table shows the mortality rates were alarming.

	Clinic I.	Clinic II.
1841	7.8%	3.5%
1842	15.8%	7.6%
1843	8.9%	6.0%
1844	8.2%	2.3%
1845	6.9%	2.0%
1846	11.4%	2.8%
Average 1841-1846	9.9%	3.3%

Moreover those in Clinic I were 2 to 3 times higher than those in Clinic II, a fact that became well known in Vienna. So much so that, since admissions to the two clinics were on predetermined alternate days, women went to great lengths to save themselves from admission to Clinic I. Semmelweis looked at all possible causes for the difference, e.g. structure or fabric of the buildings, ventilation etc but could find nothing. He even suspected that a religious practice might be responsible. The priest, always preceded by an acolyte ringing the bell, called to administer the last rites could in Clinic II go directly to the sick ward, but in Clinic I had to pass through several lying-in wards. Semmelweis wondered whether this reminder of the closeness of death could have put a harmful stress on the lying-in women so, to quote his own words: "I appealed to the sense of humanity of the servant of God and, without difficulty it was arranged that for the future the priest would take a roundabout route, without ringing the bell, so as to reach the sick chamber in silence and unobserved. The two divisions were made similar in this respect but the difference in their mortality still remained'.

In going through the earlier statistics of the Hospital Semmelweis found that during the period 1833-1839 the mortality rate, albeit high was similar in the two clinics: 6.2% in Clinic I, 5.7% in Clinic II. This gave Semmelweis the clue to the solution. Until 1839 both clinics trained medical students as well as midwives but from 1840 there was a demarcation of the respective functions: medical students in Clinic I, trainee midwives in Clinic II. Semmelweis knew that there was one important difference between the training of medical students and of midwives. The latter did not perform autopsies while the former did. In fact the autopsy sessions were usually early in the morning, often on the cadavers of women who had died in the previous 24 hours, and the students would then proceed, after a perfunctory washing of hands, to the daily round of the labour wards. Semmelweis wondered whether some 'cadaveric particles' still attached to the examining hands could be the cause of the infection. To test this hypothesis he ordered the following notices to be displayed in the clinics: 'All students or doctors who enter the wards for the purpose of making an examination must wash their hands thoroughly in a solution of chlorinated lime which will be placed in convenient basins near the entrance to the wards. This disinfection is considered as sufficient for the visit. Between examinations the hands must be washed in soap and water'.

The result was dramatic. The mortality rate which in April 1847 stood at 18.3% dropped to 12.2% in May, 2.4% in June and 1.3% by the end of the year. The average for the period 1847-1861 was 3.3% (I) and 2.9% (II) as compared with 9.8% and 3.8% respectively for 1840-1846.

Originally Semmelweis attributed childbed fever to 'cadaveric particles' but an incident which unfortunately caused the deaths of eleven women made it clear that organic material from other causes may also cause it. "In October 1847 a parturient was admitted who was suffering fron a discharging medullary carcinoma of the uterus. She was allocated the bed at which the visit always began. After the examination of this parturient we, the examiners, merely washed our hands with soap: the result was that 11 of the 12 women delivered along with her died. The discharge was not destroyed by the soapy water and was carried over by the examination to the other patients. Thus childbed fever is caused not only by cadaverous particles clinging to the hand but also by ichorous discharges originating in living organisms and for that reason the hands of the examinar must be cleansed by chlorinated lime ..... after examining each patient".

Here then was a simple method to combat a scourge which in Europe alone may have caused the deaths of tens of thousands of women per year, yet instead of being accepted enthusiastically it was ignored or attacked by most of the medical profession. This was probably mainly due to a reluctance to part with the theory that puerperal fever was a contagious disease which spread through the atmosphere. Even Oliver Wendell Holmes of Harvard who came pretty close to making Semmelweis's discovery speaks about the 'miasma generated within the walls of lying-in hospitals tenaciously so as in some cases almost to defy extirpation'. But perhaps even more important was the fact that accepting Semmelweis's doctrine would be tantamount to an admission that they personally were responsible for the deaths of women from puerperal fever by infecting them with their own hands.

Semmelweis left Vienna in 1850 for Budapest where he became Professor at the University. He continued the fight to get his doctrine recognized, published his magnum opus "The Ætiology, the Concept and the Prophylaxis of Puerperal Fever" and wrote many strongly worded 'Open Letters' to some of the greatest obstetricians of the time. The following brief extracts give the flavour of these bitter and emotional outbursts. Writing to Spaeth and referring to the 1924 women who, in the Vienna Lying-in Hospital, lost their lives through avoidable infection he says: "In this massacre you, Herr Professor, have participated. This homicide must cease". In his letter to Scanzoni in Wurzburg he writes: "Your teaching, Herr Hofrath, is based upon the dead bodies of lying-in women slaughtered through ignorance..... I have formed the unshakeable resolution to put an end to this murderous work..... If, however, you go on to write in support of the epidemic puerperal fever I denounce you before God and the world as a murderer".

His lack of success in getting his theory accepted, the thought of the tens of thousands of wemen whose lives could have been saved weighed heavily on Semmelweis's mind and was undoubtedly a contributory cause of his mental illness. He died on 13 August 1865, an embittered and tragic figure and did not live to hear the news that, on the previous day, 12 August 1865, Joseph Lister, professor of Surgery at the University of Glasgow, carried out with complete success the first ever surgical operation using the antiseptic technique - a clear vindication of Semmelweis's doctrine.

ENERGY CONVERSION AND VECTORIAL METABOLISM IN THE LIVING CELL

My second case history of a scientific theory being attacked to begin with and accepted only after a considerable time has not the drama of the Semmelweis story. No human suffering was involved and the 16 year delay in recognizing the correctness of Dr. Peter Mitchell's chemiosmotic theory of energy transfer in cells may not have appreciably slowed down the progress of cell-biology.

Mitchell was born in 1920, was an undergraduate in Cambridge and stayed on doing research in the Department of Biochemistry. In 1955 he went to the University of Edinburgh where he set up a chemical Biology unit in the Department of Zoology, became successively Senior Lecturer and Reader and remained there till 1963 when, for health reasons, he had to resign his post. He went to live in Cornwall and for the next 2 years he did no scientific work but, fired by his love for beautiful old buildings, and helped by his expertise in restoring them and, if necessary, adapting them to modern uses, he spent his time restoring and converting into a family home and small research institute a derelict Regency mansion which he had bought a year earlier.

Ever since his Cambridge days Mitchell's main scientific interest had been the way in which the energy of the processes of chemical transformation, known collectively as metabolism, is associated with the force and flow of the vital processes of dynamic action that are characteristic of living systems. In the 1950s he invented the idea of vectorial metabolism, according to which the chemical transformation processes involve the spatially-directed translocation of specific chemical groups through the catalytic domains of enzymes and catalytic carriers - so that metabolism itself, seen in a new light as vectorial metabolism, would be the prime mover in the vital processes of motion in living systems. An important part in metabolic processes is played by the chemical compound adenosine triphosphate, commonly denoted as ATP. It is a useful energy store because it can easily release energy by shedding one of its phosphate groups under the influence of an enzyme. The energy so disbursed is rather small - about one fortieth of the energy released in the oxidation of a molecule of glucose. The remaining adenosine diphosphate (ADP) has now become useless, just like a railway ticket which has been cancelled. Before it can be reused it must be revalidated, and it occurred to Mitchell that the concept of vectorial metabolism might help to explain this mechanism of 'cancellation' amd 'revalidation'.

In some cases the mechanism of phosphorylation,(the turning of ADP into ATP) using the energy given off by the metabolism of glucose was well established, e.g. in the case of the alcoholic fermentation of sugar under the influence of yeast (an enzyme). It was found that the process involved several intermediate compounds and it was understandably assumed that some similar mechanism would explain phosphorylation in simple organisms like bacteria and in the mitochondria of tissue cells. However none of these energy-rich intermediate compounds could be identified and the chemical theory of energy transfer in oxidative phosphorylation ran into difficulties.

It was in 1961 in a paper to Nature that Mitchell proposed a radically new mechanism which relied on the hypothesis that energy transfer in the oxidative phosphorylation process can take place by the movement of protons, oxide ions and electrons. This 'chemiosmotic' transfer is made possible by the intermediary of the membranes separating various regions of the organism. These are very thin electrically polarized structures containing enzymes and endowed with high electric fields of the order of 1 MV^{-1} (1 million volt per metre). This revolutionary idea did not commend itself to the great majority of biochemists especially since at that time there was no experimental evidence to support it. This was not forthcoming until work at the GlynnResearch Institute began about 1965. From then on progress was steady but even so it took over a decade from its formulation for the 'chemiosmotic' theory to gain general acceptance. Mitchell was elected a Fellow of the Royal Society in 1974 and was awarded the Nobel Prize in 1978.

It is interesting to compare these two case histories. Both have in common that a new idea was not favoured because it did not conform to firmly believed theories: infection by direct contact as opposed to 'miasmatic' contagion through the air in Semmelweis's case, chemiosmotic energy transfer vs. chemical energy transfer in Mitchell's case. The delay in acceptance was not much different: about 20 years for Semmelweis, 14-18 years for Mitchell. It seems significant that, although during the hundred years which separate Semmelweis from Mitchell the rate of new scientific discoveries has increased manifold, resistance to new ideas is as strong as ever. On the other hand discoveries of new materials or new phenomena are often accepted with alacrity (e.g. high temperature superconductors) and sometimes too readily (room temperature nuclear fusion).

Turning now to the difference between the two cases it can be explained by the differing personalities of the two protagonists and by the fundamentally different nature of their respective work. Semmelweis wanted to save the lives of tens of thousands of women. Once the statistical evidence and his clinical work convinced Semmelweis of the correctness of his doctrine he passionately defended it against all attacks and endeavoured to get his method accepted and put into practice the whole world over. When this did not happen his behaviour became increasingly aggressive and certainly the tone of his Open Letters was not what one would expect in communications between scientists. One might speculate whether a more tolerant attitude on the part of Semmelweis might have hastened the acceptance of his ideas. But it was not to be and the fate of Semmelweis's theory about the cause of puerperal fever and the prescription for its avoidance can be described by

Max Planck's dictum which I give in Peter Mitchell's abridged translation: "A new scientific idea does not triumph by convincing its opponents but rather because its opponents die".

How different is the story of Mitchell's chemiosmotic hypothesis which having originally been shunned and attacked eventually rose to the status of the generally accepted chemiosmotic theory. Mitchell was not trying to save lives: he wanted to introduce a fundamentally new way of looking at biochemical processes - of which the phosphorylation of ADP was an early example. So the passion, the pain, the despair that runs through the Semmelweis story is missing in the Mitchell case. Instead it seems to be the perfect example of how in scientific argument doubters can be won over and attackers can become allies. I can do no better than quote some passages from an address Mitchell gave at the Annual General Meeting of the Royal Institution of Cornwall in 1979.

"Naturally enough the chemiosmotic theory, which looked superficially more like physics than chemistry, was not well received by most of my fellow biochemists when I introduced it as a working hypothesis in 1961..... My new theory was, in fact, little more than a fantasy of the mind - what the philosopher Karl Popper would call a conjecture. Moreover, most of the established workers in the field deeply resented my suggestion that the remarkable energy-rich coupling factors that they had confidently been working on for some twenty years simply didn't exist. They felt that my new theory was tantamount to the insulting suggestion that they were up a gum tree".

"Therefore, when Dr. Jennifer Moyle and I were planning our practical research programme at Glynn in 1965, very little had been done to test the predictions of the chemiosmotic theory with appropriate experiments, so we decided to concentrate our attention on doing that."

" Time will not allow me to give experimental details of the work on this problem, or to elaborate on the slow progress of experimental research and human communication and persuasion, which involved very many scientists in laboratories all over the world. Suffice it to say that the advent of the new theory led more and more experimentalists to do new types of experiments ..... and thus the old theory was gradually and painfully rejected in favour of the new theory because the scientists were persuaded that the experimental facts were described better by the new theory than by the old one .

"The word 'persuaded' is much more important here than you might think. Winston Churchill once said: 'We shape our buildings: therefore they shape us'. This applies very nicely to scientists, who tend to get locked into the conceptual framework that they themselves have evolved - so that they then cease to be able to see the realities outside. We, at Glynn, became very conscious of this problem, because practically all the established workers in the field of oxidative energy transfer began by being the builders and supporters of the chemical theory. Naturally, they were reluctant to agree to the demolition of the intellectual building - the theory - that they had spent so much time and effort erecting and perfecting, even though it turned out to have insecure foundations. Moreover communication between the proponents of the opposing chemical and chemiosmotic theories proved to be very difficult, partly because the basic concepts and attitudes of mind were so different, and partly because the existing large-scale system of communication in science often tends to encourage competitive antagonisms rather than open-minded appreciation between workers of different schools of thought. To help to overcome this difficulty we developed a person-to-person style of communication at the Glynn Research Institute. By arranging for individual research workers to stay at Glynn House and enjoy the pleasant atmosphere of the Institute and the hospitality of the resident Director of Research, it was found that minds could relax, prejudices could be overcome, and difficulties could often be resolved in a way that is not normally achieved in the competitive atmosphere of busy scientific conferences, or through the public forum of the scientific press. We came to the conclusion that such appreciative methods of person-to-person communication were the only proper way of resolving major conflicts of ideas...... ".

And, while Planck's dictum rang true in the Semmelweis case, Mitchell had the satisfaction of being able to say: "I think that the general acceptance of the chemiosmotic theory, while most of the original proponents of the chemical theory are still in the prime of their scientific lives, has shown that we succeeded - but not, of course, without the admirable good will and altruism of those very people who were originally our most powerful and persuasive scientific opponents". One might add that the files of Mitchell's personal correspondence about the chemiosmotic theory comprises several hundred items.

There is however another dictum, this by Schopenhauer, which seems to apply not only to problems in philosophy but also to scientific discoveries: "Every problem passes through three stages before its acceptance. In the first it appears ridiculous, in the second it is attacked and in the third it is regarded as obvious".

THE NON-CONSERVATION OF PARITY.

If the above title had read "The non-conservation of energy" or "The non-conservation of momentum" the chairman of this Committee would have crossed out, without consulting the author, the word "non" as being a typing error. The same fate would probably have befallen the above title 35 years ago. It is significant that the first paper to argue seriously the possibility of a violation of the principle of the conservation of parity had a more subdued title "Questions on parity conservation in weak interactions".

What then is this principle which had been regarded, until its overthrow in 1956, as one of the fundamental conservation laws of physics? It could best be characterized by saying that "left" and "right" are indiscernible from each other in an absolute sense. Or, to put it in another way, there is no absolute distinction between an object and its mirror image. The paper by T.D.Lee and C.N. Yang mentioned above and published in October 1956 in the Physical Review made the bold statement that there may be physical phenomena for which the indiscernability of an object and its mirror image no longer holds. They proposed this in order to solve the conundrum of the seeming identity of two elementary particles, namely the tau and theta mesons. When they decay the resulting pi mesons have different parities, hence if the tau and theta mesons are identical then in one of the decays parity is not conserved. Lee and Yang put forward the hypothesis, supported by theoretical considerations, that, while parity invariance is valid for the great majority of physical phenomena, it may not be observed in transformations governed by very weak interaction forces, e.g. in the tau and theta decays. They then proposed a conceptually simple and technically not too difficult experiment to test this hypothesis.

Radioactive decays resulting in the emission of beta particles are known to involve weak interactions. Let us consider an array of spinning radioactive nuclei all pointing in the same direction (i.e. all spinning clockwise or anti-clockwise); if the same number of beta particles (electrons) is emitted in the "up" and the "down" direction the real object, when turned upside down, is indiscernible from its mirror image, parity is conserved. But, if there is a difference in the "up" and "down" emissions, the object and its mirror inage are distinguishable, hence parity is not conserved. Fortunately a few years earlier a high degree of polarization had been achieved in beta-emitting ^{60}Co nuclei at very low temperatures and thus, by measuring the forward-backward asymmetry (if any) of the beta emission, the Lee-Yang hypothesis could be tested.

To show the relevance of this story to the general theme of this paper let me describe some of the antecedents. The study of "nuclear orientation", i.e. the measurement of the anisotropy of gamma radiations emitted by oriented radioactive nuclei began in 1951 and the first nucleus to be studied was ^{60}Co which decays by the emission of beta rays to an excited state ^{60}Ni which in turn decays by the emission of gamma rays to the ground state of ^{60}Ni. The measurement of the gamma ray anisotropy was rendered easy because, by virtue of the conservation of parity, the

the intensity of the distribution is symmetrical with respect to the nuclear "equator" and therefore it was not necessary to "polarize" the nuclei, it was sufficient to "aline" them, all pointing in the same direction, but not in the same sense. Results obtained on both polarized and alined nuclei were indeed identical - but these experiments were not done to test the law of parity conservation which, for electromagnetic radiation, had been found correct for many decades. Nevertheless it occured to some "nuclear orienters" that one might look at what happens in beta-radiation but their colleagues regarded such an experiment just as futile as verifying the impossibility of perpetual motion. It must be emphasized that to do the beta-ray experiment would have involved more than just changing some of the detection apparatus since, gamma rays being penetrating could be detected outside the cryostat while beta-counting had to be done inside the cryostat close to the sample. Therefore the strictures levelled at experimentalists (e.g. by P.M.S. Blackett) that they blindly believed the theoreticians and that they could have made the discovery predicted by Lee and Yang several years earlier by carrying out a very simple experiment, is not quite fair. As we shall see, many months of preparation would have been needed.

When the Lee-Yang hypothesis became known during the summer of 1956 through preprints many physicists were rather sceptical and thought that carrying out the Lee-Yang test was a waste of time. One might speculate on what would have been the attitude of the referees and the Editor of Physic al Review if the manuscript on the question of parity conservation had come from an unknown physicist in a little known institution rather than from two distinguished physicists at Columbia University and the Princeton Institute for Advanced Study respectively. The publication delay (22 June - 1 October) would probably have been much longer - the manuscript might not even have been accepted. (I mention in parenthesis the story or myth about a paper sent to Physical Review in the early 1920s by an unknown phy sicist propounding ideas similar to those published by de Broglie a couple of years later - the paper was not published, but its existence is not proven).

One of the physicists who thought that the test proposed by Lee and Yang was worth carrying out was Professor C.S. Wu of Columbia University. Being an expert on beta ray spectroscopy she needed the collaboration of a team which could provide the polarized ^{60}Co sample. In 1956 there were only four laboratories in the whole world having the expertise in and the equipment for low temperature nuclear orientation and thus capable of carrying out the experiment in a relatively short time. Two of those were in the U.S.A.

So, some time in July 1956 Professor Wu went to see Dr. Louis Roberts at Oakridge and proposed to him the parity experiment. However Roberts was at that time engaged in experiments looking for possible directional effects in nuclear fission and Professor Wu tried Dr. Ambler and Dr. Hudson at the National Bureau of Standards. Both of them were low temperature experts and well versed in experimenting at millikelvin temperatures. In addition 3 years earlier Ambler was the first to achieve a high degree of nuclear polarization of ^{60}Co nuclei.

Preparations for the test began in August 1956, the crucial experiments were performed in December, the paper was received by The Physical Review on 15 January 1957 and was published in the 15 Febr. 1957 issue. At the New York meeting of the American Physical Society a special plenary session was devoted to the experimental and theoretical aspects of the overthrow of parity.

The NBS (National Bureau of Standards) experiment on parity violation is frequently and mistakenly referred to as the Wu experiment and it is not perhaps out of place in this historical review to explain the origin of this misnomer. The experiment was clearly a team effort in which, in addition to Professor Wu, Drs. Ambler and Hudson (low temperature physicists), Dr. Hayward (nuclear physicist) and Mr. Hopper (research assistant), all of them staff members of the NBS, took part; the published list of the authors reads: C.S. Wu, E. Ambler, R.W. Hayward, D.D. Hopper and R.P. Hudson. This order of the names was purely accidental and was occasioned by a misplaced gentlemanly gesture by Ambler. When the assembled authors, having finalized the manuscript, came to the question of the order of the names there was an embarrassed silence. To break it Ambler turned to Wu and said: "Since you are the only woman among us perhaps you would like to sign first?". She answered "Yes". Things have changed in the last 35 years. Ambler's question would today be regarded as an insulting sexist remark and the answer would probably have been "Why should I? Let's have alphabetical order."

HOAX, GARBAGE, GENUINE ?

The above question must often go through the minds of many editors of scientific journals. They bear a heavy responsibility. Publication in a respected periodical bestows upon new theories or experiments some respectability and credibility and it is not always easy to decide to which of the 3 categories a manuscript belongs. Quite apart from credibility the date of publication counts when it comes to priority which may be important for scientific honours and awards and for patent rights.

Talking about hoax-publications one must distinguish between fraudulent experiments carried out to achieve fame and theoretical discussions whose chief aim is to ridicule ill-founded theories and peoples' attitudes to them. I shall only discuss the second category since the first would involve a discussion of the vast array of fraud and deception in science and that would require a whole paper - perhaps even a whole Committee.

Most of us have our pet hoaxes but many of them appeared in general publications and not in the scientific press. I believe that the best is one that appeared 60 years ago in the German weekly scientific periodical, "Die Naturwissenschaften" and I give below a facsimile of the original and my translation.

Bemerkung zur Quantentheorie der Nullpunktstemperatur.

Wir betrachten ein hexagonales Kristallgitter. Der absolute Nullpunkt desselben ist dadurch charakterisiert, daß alle Freiheitsgrade des Systems einfrieren, d. h. daß alle inneren Bewegungen des Gitters aufhören. Ausgenommen ist dabei natürlich die Bewegung eines Elektrons auf seiner BOHRschen Bahn. Jedes Elektron besitzt aber nach EDDINGTON $1/\alpha$-Freiheitsgrade, wo α die SOMMERFELDsche Feinstrukturkonstante ist. Außer den Elektronen enthält unser Kristall nur noch Protonen, für welche offenbar die Anzahl der Freiheitsgrade dieselbe ist, da nach DIRAC ein Proton als Loch im Elektronengas angesehen werden kann. Um also zum absoluten Nullpunkt zu gelangen, müssen wir einer Substanz pro Neutron (= 1 Elektron + 1 Proton; unser Kristall soll ja im ganzen elektrisch neutral sein) $2/\alpha - 1$ Freiheitsgrade entziehen, da ja ein Freiheitsgrad wegen der Umlaufsbewegung bestehen bleibt. Wir erhalten daher für die Nullpunktstemperatur

$$T_0 = -(2/\alpha - 1) \text{ Grade.}$$

Setzen wir $T_0 = -273^\circ$ so gewinnen wir für $\frac{1}{\alpha}$ den Wert 137, welcher mit dem auf einem gänzlich unabhängigen Wege gewonnenen Werte innerhalb der Fehlergrenzen vollkommen übereinstimmt. Man überzeugt sich leicht, daß unser Resultat unabhängig von der speziellen Wahl der Kristallstruktur ist.

Cambridge, den 10. Dezember 1930.

G. BECK. H. BETHE. W. RIEZLER.

"Remark on the Quantum Theory of the Zero Point of Temperature.

Let us consider a hexagonal crystal lattice; its absolute zero is characterized by the "freezing in" of all degrees of freedom of the system, i.e. all internal movements of the lattice must stop. Naturally the movement of the electrons in its Bohr orbit is excepted. According to Eddington each electron possesses $1/\alpha$ degrees of freedom where α is the Sommerfeld fine structure constant. Besides electrons our crystal also contains c/Kurt

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If we take $T_o = -273^\circ$ we obtain for $1/\alpha$ the value 137 which agrees fully within the limits of error with the value obtained in an entirely independent way. One can easily satisfy oneself that our result is independent of the choice of the crystal structure.

Cambridge, 10 December 1930. G. Beck, H. Bethe, W. Riezler."

Published 9 January 1931.

The manuscript reached Dr. A. Berliner, a respected physicist and for many years Editor of the Naturwissenschaften, in the middle of December 1930. Two of the authors, G. Beck and H. Bethe (later Nobel Laureate) were young but internationally known physicists, the title of the paper sounded reasonable, so perhaps Berliner did not even bother to read the paper properly but passed it for publication. The authors were astounded to see their little joke published as a serious paper rather than a lighthearted contribution to the Christmas festivities. Because, so the story goes, it was concocted one evening in a pub when the three suthors were discussing the significance attached in some circles to the reciprocal of the fine constant, $1/\alpha = hc/2\pi e^2 \approx 137$, often referred to in those days as Eddington's number.

If Berliner, although forewarned by the cheeky first (and last) sentence of the text, did not see the absurdity of proposing a connexion between a dimensionless expression comprising 3 fundamental physical constants and the temperature of the ice point on the Celsius scale, it shows how easy it is to attach physical significance to arbitrarily derived but often used numbers.

(I often wonder whether it is a good idea to introduce thermodynamic temperatures (as contrasted with temperatures defined by the senses) by reference to the expansion of an ideal gas. I prefer to state that thermodynamics only define ratios of temperatures and that by measuring the vapour pressure of water, its heat of evaporation, the specific heats and densities of water and water vapour between the triple point (T_t) and the boiling point (T_b) one can determine experimentally the ratio of these two fixed points and find $T_b/T_t = 1.33608$. If one then stipulates that $T_b - T_t = 100$ one finds $T_t = 100/0.36608 = 273.16$.)

It was relatively easy to recognize the article by G. Beck et al. as a well executed and amusing hoax, but the next example was not quite so simple. Europhysics Letters began publication on 1 January 1986 and one of the first manuscripts I received as the Journal's Editor-in-Chief was a brief article with the somewhat provocative title: "The Precise determination of the exact number of spatial dimensions". It came from the CERN Laboratory in Geneva and the authors were C. Jarlskog (Professor at the Universities of Stockholm and of Bergen) and F.J. Yndurain of the CERN staff. Not being familiar with field theory I found it difficult to accept the statement that in "perturbative expansions of field theory one can give sense to non-integer values of spatial dimensions", and wondered whether the manuscript was sent to find out how the Editor of our fledgling Journal would react to a cleverly written spoof. The reference to the "Spanish-speaking Mafia at CERN's TH Division" also pointed in that direction because it is alas rare to find such informal expressions in a solemn learned text. However, after consulting knewledgeable colleagues, I became satisfied that this was neither hoax nor garbage and accepted the paper for publication, on condition that the title was changed to "Is the number of Spatial Dimensions an Integer?".

CONCLUDING REMARKS.

I have given the above examples in the hope that they would stimulate the Committee to mention other cases in which scientists were slow or reluctant to accept new ideas - sometimes foolishly, sometimes wisely. However I have left out cases in which phenomena were disregarded or not believed by some sections of the scientific community either because they were not capable of explanation by the laws of physics or because important vested interests were involved.

The first category would cover phenomena like extrasensory perception, (ESP), telepathy, water-divining, medical uses of hypnosis and many others. These are very controversial subjects and, although I personally would have liked to hear a sober assessment and unemotional discussion of, for instance extrasensory perception this will have to wait for another ICUS.

The same is true for cases in which commercial or political pressures influence the scientists' views and attitudes. In the 1960s when overhead high voltage power lines became widespread there were often complaints, mainly from elderly ladies living near such power lines, about headaches, general discomfort etc. The appropriate electricity authorities made soothing noises, assured them that there could be no physical influence from the power lines and that it was "all in the mind". Some scientists perhaps honestly believed that such effects could not exist. And yet 20 years later it was found that weak, low frequency electromagnetic fields could indeed have an influence on biological material. At present many millions of dollars are being spent in various countries to get to the bottom of these phenomena because if the effects are found to be harmful to health the cost to the electricity supply industry could be enormous. The credibility of science may suffer if the public realizes that scientists occasionally refuse to believe in the existence of some phenomena and are slow in admitting their error.

Because of the very nature of this paper the examples are far from flattering to the scientific community. Nevertheless I think that on the whole we scientists run our affairs reasonably well, but we must always remember that being influenced by scientific dogmas can be just as harmful to the development of science as being dominated by religious or political dogmas.