A History of Physics as an Exercise in Philosophy

E J Post

PREFACE

Religions are known to become rigid and despotic when the icons of religion are beginning to replace the teachings in order of importance. In a similar manner some formulas and equations of physics have been assuming an icon-like position for the physical sciences. It raises questions whether there are mathematical icons that are now unduly dominating the scene in physics with the possibility of holding back the exploratory spirit. Since the latter has always been physics' badge of honor, this essay calls on the help of history and philosophy to pinpoint possible onsets of early rigidity wherever they may have occurred.

Unfortunately, many physicists have been imbued with ideas that it is exactly philosophy that is in need of such help. The origin of such suspicion goes back millennia. Early philosophers had regrettably misguided their contemporaries about the value of physical experimentation. Ever since the discipline has paid for this attitude of early arrogance

For a long time physics has been able to steer clear of a similar arrogance in their discipline until the beginning of this century. When quantum physics came along, the pitfalls of 'exclusivism' became too numerous and the shield of self defense assumed an unintended edge of arrogance. Physicists began congregating in the legendary land of Hans Anderson under the inspiring guidance Nils Bohr.

Rarely had scientists encountered so many new things in so short a time. Faced with the daunting task of sorting out an inebriating avalanche of new perspectives, they started giving known words new meanings in physics. Examples are: *nonclassical probability, absolute a priori uncertainty, particle-wave duality etc.*. Einstein supposedly was quoted as having referred to their activity as an *epistemological orgy.* If he really said this, his sense of physical reality was superb as usual, even if the word "epistemology" had been mildly misused.

The early phases of epistemology, to which Einstein presumably referred, are now known as "ontologies." Mario Bunge calls it "the furniture of the world." We know there is good and bad furniture and last, but not least, furniture should harmonize with its intended environment.

It is an irony of life that, as philosophy in the early days, now physics stumbled into its own trap of arrogance. If physics made some questionable ontological choices, this booklet may refer to them as rare brands of very durable Copenhagen furniture, yet beautiful in their own right. The problem has been more one of fitting rooms, tables and chairs together and less one of discarding furniture items, because in a short time the house of physics had been changing in a very dramatic fashion.

To the extent possible, these pages attempt a 'nontechnical account' of modern physics' facts and sins. In practice this means, there are very few *formulas* in this book to prevent icons from dominating the scene. If that is a shortcoming, let a more extensive discussion of *formalisms* make up for it. The result is a subject presentation which, in an optimistic sense, might be said to be reaching from preschool to beyond university.

Since Kant's *Critique of Pure Reason* had philosophy confessing to its sins, followed by a penance long ago, physics is now invited to do the same by confessing to its arrogance. The court of public opinion has sufficient grounds to consider charging the physics profession with:

(1) abandoning a royal road to quantization.

(2) inadequate consideration of alternatives to Copenhagen views.

(3) intimidating rather than convincing others of what are believed to be right choices.

Since mathematics is a discipline without which much of physics could not be, the last chapter discusses some parallel developments between the two. A moderate measure of mathematical rigor and down-to-earth philosophy go a long way in helping physics home in on common corrective measures that can disentangle situations where physics may have gone overboard amidst an abundance of too much new in too little time.

Pondering the very valuable help rendered by Christine Brunak and Ruth Jackson in the process of proofing the text, it still puzzles and despairs me as to why this subject matter has either something akin to pulling teeth or tends to bring people to the brink of nervous breakdown.

E J Post
Westchester CA, Sept. '97

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PREAMBLE ABOUT PHILOSOPHY

The average physicist is not known to have kind and warm feelings for philosophy. Many of them feel that philosophy is not very relevant to their discipline. If there is any relevance, it is one that comes after the facts. They regard the philosophy of science as an occupation for those who are beyond their creative years in science.

There is a reason for this rather disparaging attitude. It goes back all the way to the ancient Greeks who thought that their pure rationalism could solve, if not all, at least most problems of the world. Although there were other Greeks who did not agree with such radicalism of pure reason, their voices did not weigh in sufficiently to effectively open doors for alternatives. Mankind has always had a streak for the bold and the radical.

It would take almost two millennia before Galileo Galilei (1564-1642) was able to put a dent in the arrogant dominance of pure rationalism. Galilei taught his fellow man how to observe nature in a more discerning manner; he re-initiated conscious efforts to ask nature questions by doing experiments. It was a new insight that fitted the renaissance era. While Galilei became known for new insights in dynamics, his counterpart in the low countries Willebrord van Snel van Royen (1580-1628), now referred to as "Snellius" or "Snell", established the law of optical refraction. Snell's work became more widely known through Christiaan Huygens (1629-1695); better telescopes & microscopes were the results.

Experimentation from then on became a sine qua non for just about all physical developments that were to follow. Man had freed himself from a too-puritanical confinement to rationalism; instead, he had learned a new way of asking questions. In fact, he became better at it all the time and, in doing so, opened up new worlds that made new and challenging demands on his earlier capability for reasoning. The evolution from Galilei's dropping objects from the tower of Pisa to Millikan dropping charged oil droplets in order to measure elementary electric charge, is a rather spectacular development in experimental sophistication. Experimentation, earlier regarded by many of the old Greeks as a futile pastime, had added dramatic new dimensions to man's capabilities for observing nature. Rather than indulging in untestable speculations about the nature of the world, man now had new ways of getting answers to his inquiries.

Radical rationalism so became to be regarded as a manifestation of arrogance. The inclination to experiment testifies to a frame of mind willing to learn from what nature has to say as something, over and above, of what pure reason can do. The old Greek philosophers had been stymied by an attitude reminiscent of a child telling us: I can do it alone. Granted, whatever we learn through own experience is extremely valuable. However, as there is so much to learn, there would not be enough time for each individual to go through the whole process of private discovery. Through the process of experimentation, we find how nature already solved problems long before we had the awareness for asking certain questions, much less having even hinted at their solutions.

Experimentation is an exercise in humility. It is asking nature about its earlier experiences, perhaps going back to eras when the human species, as we now know it, had not yet evolved. The art of experimentation began hesitantly in the early 17th Century and came to greater bloom in the 18th Century. The emergence of steam engines had man think about the fundamentals of heat and *thermodynamics.* In many ways, the Cavendish experiment, which established a value for the Newtonian gravitational "constant," was one of the highlights of that period. Regardless whether or not the gravitational constant is a real constant, the major fact was that man had gained insights about a quantity that had something to say about the whole universe.

The next century of experimentation might well be named after the great-grandmaster of this art: Michael Faraday. It is known that answers to questions critically depend on how questions are asked. Faraday knew how to ask questions. Contemporary physicists have been heard saying: *Faraday had his own private telephone line to God.*

Continuing in the footsteps of their 19th Century predecessors, the 20th Century experiments became bolder and bigger. When the first accelerator machineries yielded precious new information about the constitution of matter, the next idea was building more and bigger accelerators. Yet, mindful of the diminishing returns of the radicalism of rationalism in ancient times, there is now an awakening that "more and bigger" in experimentation has its own limit of diminishing returns.

The great generators of insight in the latter part of the 20th Century seem to be trading bigger for subtler. These changes have been partly the result of well-considered human initiative; others have been brought about by serendipity, yet requiring the watchful eye and alertness of an experimenter to recognize the importance of an unexpected phenomenon. The Mössbauer and quantum Hall effects are challenging new phenomena, yet they were unexpected on the basis of current insights. The Josephson effects, though, were a result of a more premeditated effort.

All of which goes to show that experimenters have now been facing their own crisis of radicalism. Suddenly they are at a road junction, requiring Solomon's wisdom for making decisions not to let all traffic go in the directions of the biggests noise. It is good to remember that nobody is safe from the trap of arrogance. From the old Greek philosophers looking down on inquiries by doing silly experiments, all the way to modern-time experimenters scoffing at the futility of what philosophy can do for physics, it is always a measure of arrogance that is holding back the potential for honest, open communication.

The objective of this preamble is to build a case for making more attempts at opening up interdisciplinary channels of communication. The idea is one of making physicists more receptive to aspects of philosophy relevant to physics, and familiarizing philosophers with physical developments fitting the patterns of the modern theory of knowledge. Branches of philosophy expected to have classifying function for physics are: **empiricism, phenomenology, intuitionism, ontology, and epistemology.**

These conceptual subdivisions in philosophy were neither specifically created for purposes of physics nor meant to please the world of physics. On quite general grounds, they are to be considered as related to man's perception of his universe. Hence, one may trust that this branch division of philosophy is not unduly tainted by conceivable physical prejudices. If these subdivisions nevertheless are relevant to physics then, all by itself, that fact is evidence of their measure of universality. By the same token, an ensuing subdivision and classification of physical disciplines in the perspective of these universal guidelines hold great instructive virtue.

The present objective is an attempt at classifying physical disciplines in the perspective of these subdivisions of philosophy. Yet, since the mentioned subdivisions of philosophy may not be common knowledge in physics' circles, a brief outline of philosophy structure and a definition of relevant terminology is now in order.

Outline of Philosophy Branches

Originally philosophy was primarily concerned with man's ability to reason about matters of human concern. This statement is to be taken in the context of the times of which we speak. It then stands to reason that ancient philosophers were confronted with a world that was entirely different from our contemporary society. To accomplish the task of reasoning requires precision of argument and word definition. The meaning of the Greek word *philosophy* in translation ranges from *wisdom of language* or *love of language* to *wisdom of words*. Most western languages have borrowed the word "philosophy" from the Greek language. Dutch iss an exception in this respect; it uses a homegrown compound-noun *wijs-begeerte*, which literally translated conveys *desire for wisdom.*.

From these defining expressions we infer that philosophy conveys a sense of purism about the language medium we use for communication. This medium of communication should remain well defined, lest we end up with a situation resembling a confusion of tongues as referred to in the book of Genesis. Mindful of the connection between philosophy and language, founding fathers of academia had the wisdom of joining the study of *art, letters and philosophy* under a single faculty heading.

In the course of time, the subject matter about which man desires to communicate has grown dramatically. The experimenters, for one, have dramatically expanded the realm of human perception. This brings about ramifications for processing information. The extended field of perception invokes choices and decision-making as to what is to be recorded as scientific information. Value judgments need to be made between recordings that appear to be meaningless collections of arbitrary data versus those carrying interesting and meaningful scientific structure. This quality-based process of phenomena selection of scientific information is called **empiricism**. The Ptolemaic and

Copernican systems of describing planetary motion are both valid **empiric** descriptions of what is taking place. Today, Copernicus' picture is preferred as closer to the true nature of things. It is closer to the reality of what is taking place. The Copernican system is then regarded as an **ontological** improvement of the Ptolemaic situation. To the extent that the Copernican view can be obtained through logical reasoning, it could be called a **phenomenological** improvement of the Ptolemaic view.

Since these concepts have a cardinal role in what follows, here is another formulation that emphasizes their mutual relation. Starting from an *empiric selection* of scientific phenomena, the application of *pure logic* may then lead to additional phenomena. One so establishes a **phenomenological system of interrelated manifestations**. In physics, this is called a "theory." Since mathematics is a major part of physical theorizing, phenomenology tends to be mathematical in nature.

Whereas phenomenology can be regarded as a hard-nosed application of logic, an **ontic** element injects something new, say, an hypothesis. If the injection is successful in the sense that it permits a consistent treatment using rules of logic, one may have an **ontology**. Such disciplines have a strong element of inventiveness and an element of art; by the same token, it requires honesty and courage to reject an ontic invention when it leads to contradictions and has been proven to be unworkable. The notion of the subconscious in psychology is an example of an ontic invention, which may well be regarded as successful.

The world has known many unsuccessful ontic propositions. Even if they did not quite pan out, they served a goal before they could be abandoned. The moral of the story is: don't beat a dead horse; give it a dignified burial with thanks for services rendered. In life in general, and also in physics, recognizing limitations and inadequacies of ontic propositions remains a difficult and trying experience.

In the course of these discussions, an epithet of quality was found to be associated with ontic propositions. While the Ptolomaic and Copernican world views are both recognized as ontic propositions permitting reliable predictions of events, the contemporary convictions hold the Copernican view as closer to physical reality than the Ptolomaic view. This example illustrates a distinction between two kinds of ontic propositions. The *work-hypothesis* is an ontic proposition that leads to correct results, whereas a *truth-hypothesis* not only yields true results; it over and above reflects additional elements of physical reality. So a *truth-hypothesis* **is a** *work-hypothesis*, yet, conversely, a *work-hypothesis* **is not a truth-hypothesis**, unless it is proven to meet additional reality requirements. All of which raises questions about how we know what we know. Philosophy has created a discipline assigned the task of a quality grading of ontic propositions. In modern times, it is being referred to as **epistemology**, which comes from the Greek *episteme* meaning "to know." These concepts are succinctly delineated by Zerin.[1]

In a nutshell, we see here how philosophy creates its own language for a precise communication that can deal with the wider realm of human awareness.

The creation of these new words and compound words simplifies communication. Let us complete the vocabulary by assigning adjective and noun qualities to these words. For instance the adjective **ontic** refers to a proposition that may or may not lead to a complete system description deserving of the name **ontology**. By the same token, an **epistemic** exploration may or may not lead to a logically complete description; only if it does, is it deserving of the name **epistemology.**

Let us inquire about more specifics of how ontologies and epistemology relate to one another. The comparisons between Ptolomaic and Copernican world views gave us already an inkling about which one is closest to an epistemology. Mario Bunge[2] has given an interesting metaphor by comparing ontologies to the furniture of the world. While ontologies have already a measure of logical completeness to be deserving of that name, the Bunge metaphor conveys the need for an integration of the furniture into what may be regarded as a home. By the same token, homes and other buildings constitute a township and a conglomerate of villages and towns brings out images of a society. In other words: **an epistemic view of a subject matter calls for a global integral view of its ontic components.**

Mathematics and associated parts of physics have been found to yield perspectives bearing out the relevance of these processing subdivisions that have been independently generated in philosophy. Some preliminary observations can help in whetting an appetite for these matters.

Much of physics has been developed from a local point of view, meaning physical laws have been cast in the form of differential equations. In fact, the Newtonian breakthrough would have been impossible without a simultaneous creation of the calculus. Global aspects of such locally initiated activities are solely obtained by solving differential equations and adapting solutions to initial and/or boundary conditions. Not all global structure can be generated as if originating from an infinitesimal local level. Atomicity and quantization have intrinsic global qualities. Yet manifestations of ensemble behavior of single *atomic* entities lend themselves to being treated by differential equations with boundary values.

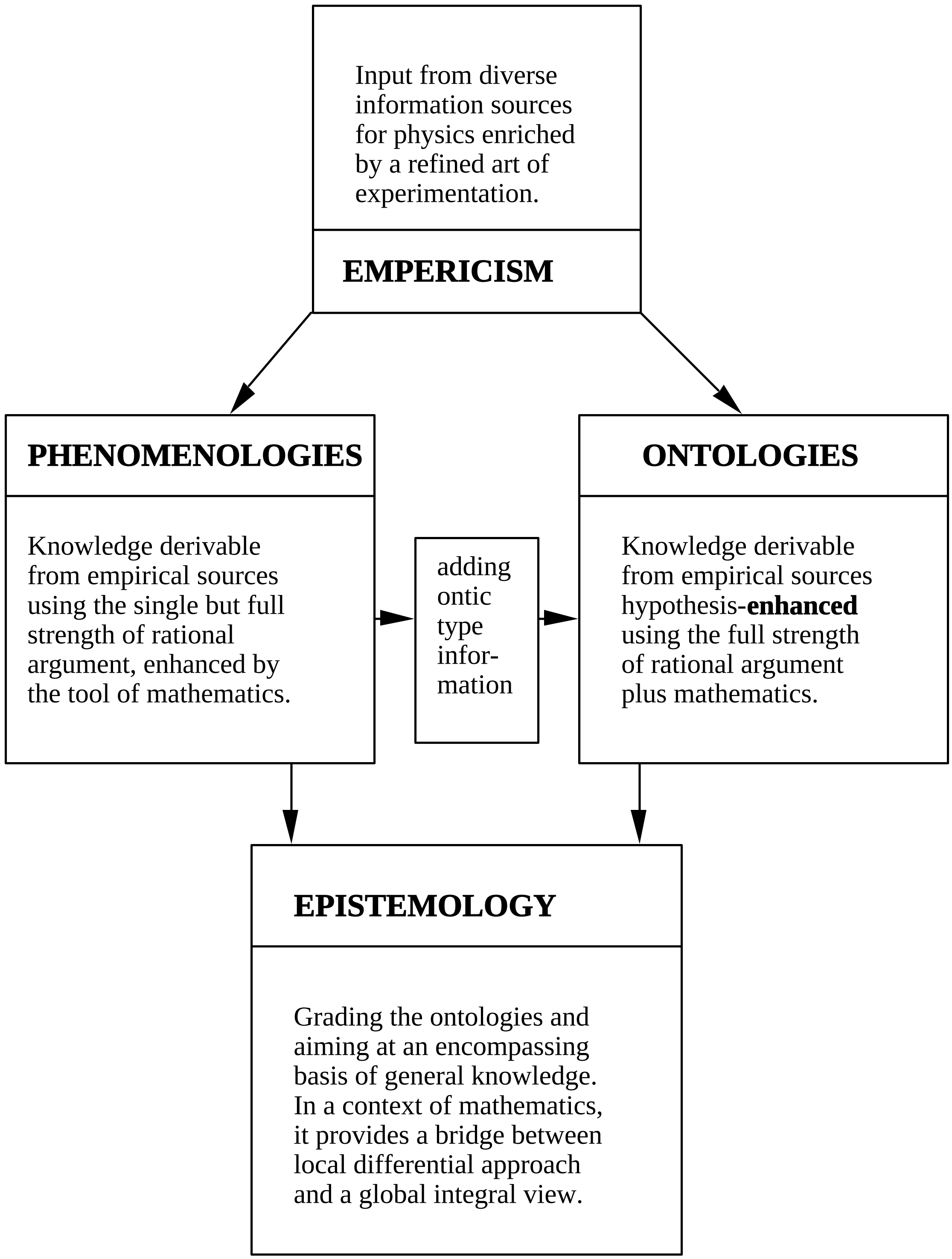
Apart from mathematical technicalities referred to in the last paragraph, processing information becomes a balance of compromise between the **rationalism** of phenomenology and the **intuitionism** of ontology.

The here-following description of interrelations between physics and philosophy is presented in an historic perspective. This has the advantage of showing how, in the course of time, a greater diversity of philosophy's subdisciplines become involved in the development of physics; irrespective of whether this philosophy involvement is recognized or accepted by the physics establishment. In the early phases of the development of physics, only the simplest and most direct aspects of the theory of knowledge needed to be called upon.

Then gradually, when physicists become more inquisitive about nature, more differentiated and sophisticated concepts in the arsenal of knowledge theory begin to play a role. Indeed, the history of physics can be rather clearly subdivided in an early era in which empiricism was the most dominant element. Made possible by concurrent mathematical developments in the18th Century, phenomenological deduction begins to have an increasingly important role. Conversely, phenomenology and technology, in turn, refine the art of empiric inquiry, thus leading to a new wave of phenomenology in the 19th Century. This new wave is accompanied by new mathematical concepts and relationships. If calculus was a prime mover of 18th century phenomenology, the theory of groups and the theory of invariants and their repercussions for calculus and geometry were beginning to make their influence felt in 19th Century physics.

Then in the 20th Century, ontology in the form of work-hypotheses is becoming more and more effective. In fact, one of the most spectacular work-hypotheses, the "action quantum concept," made its debut exactly at the beginning of this century.

For mostly practical reasons mentioned in the sequel, no explicit technical use will be made of mathematics in the form of derivations and calculations. There is merely emphasis on basic concepts. In the course of these tentative explorations, one may find that a joint conceptual development of key notions in physics and philosophy are more of a help than an extra burden on the path of improving our insight. To further that cause, let us now illustrate the deductive relation between the philosophy branches here encountered by placing them in a diagram showing their logical interconnections.



BRANCHES OF PHILOSOPHY RELEVANT TO PHYSICS

THE EMPIRIC SOURCES OF INFORMATION

Having delineated the principal branches of philosophy that constitute the modern theory of knowledge, the ingredients are now available to attempt major applications to physics. In the process of doing so, one can gradually develop judgment as to what is valuable information versus information that has no obvious scientific significance. The proof of the pudding is in the eating. There is no point in giving abstract criteria for distinguishing information quality prior to having at least some events from which to abstract.

Since there is a veritable abundance of material from which to choose, sketchy references should suffice to refresh the memory about earlier encounters with the subject matter. This has the advantage of showing the discovery from an historic perspective. Such an approach illustrates man's growing perceptiveness about the world surrounding him.

Archimedes' Principle

Going back to the ancient times of an emerging flourishing trade and shipping by sea routes in the Mediterranean, early naval architects of that time must have given thought how to determine the cargo-carrying capacity of their vessels. Over and above the general rule that bigger ships can carry bigger and heavier cargos, people needed to be able to predict in advance how much certain ships could carry in order to know how many ships or journeys would be necessary to transport a given cargo.

It was Archimedes (287-212 BC) who helped solve the problem of buoyancy of immersed objects. He somehow concluded that weight of ship and cargo had to equal the weight of the displaced water. It is probably not exactly known what type of argument he used to arrive at this result. However, all later shipbuilders lived by that rule. In the 17th Century, a Dutch physicist, Gravesande (1688-1742), came up with a simple device by means of which the correctness of Archimedes law could be tested with accuracy.

Consider hereto a cylindrical bucket and a solid metal cylinder that fits snugly inside the bucket so that both have exactly the same volume. Now suspend bucket and cylinder from a sensitive balance and bring the balance into equilibrium. While hanging from the balance, immerse the metal cylinder in a water container. The balance is now out of equilibrium, yet the equilibrium is restored by filling the bucket with water. Since the weight of this water equals the water displaced by the immersed cylinder, Gravesande's bucket experimentally proved Archimedes' contention.

This law of Archimedes, also called Archimedes' principle, is one of the first examples of a clearly stated quantitative law in physics. It goes back to the 3rd century BC. Although numerous people applied this principle through the centuries, an experimental proof came two millennia after its inception. A few decades after Gravesande's experimental proof, Euler succeeded in reducing Archimedes' principle to a special result of his fluid dynamics.

The Ptolemaic-Copernican Evolution

The next major event in the empirical recording of natural phenomena is certainly the transition from the Ptolemaic to the Copernican way of describing planetary motion. The earth-centered Ptolemaic description of planetary motion originated in the 2nd century AD. It took thirteen centuries before it was replaced by the much simpler sun-centered description of Copernicus (1473-1543). Here is an example of a choice between two empiric options. It was the sun-centered description that led, in the early part of the 17th Century, to vastly enhanced empiric detail that is now known as the laws of Kepler (1571-1630). Newton's derivation of those laws from his general principles of mechanics and gravity, decades later, vividly illustrates the essential role played by the transition from earth-centered to sun-centered description. The earth-centered description was hardly suggestive as a basis for stellar dynamics. Here we see how the quality and simplicity of empiricism can play a decisive role in later theorizing.

Snell's Law of Optical Refraction

The 17th Century saw great improvements in optical instrumentation. The telescope opened up the world in the large, and the microscope opened up the world in the small. This meant that the art of lens-making had improved steadily, made possible by an understanding of the laws of light refraction. It had been known for a long time that a light ray that enters an optical medium, say from air into glass or water, changes direction. In everyday life, we experience this effect as an apparent position change of objects under water. Snell made measurements in the early part of the 17th Century, which showed the ratio of the sine of the angle of incidence (measured with respect to the normal on the interface) and the sine of the angle of refraction (similarly measured) to be constant. It means the ratio of the sines does not change for varying angle of incidence.

The fact that reflected light shows a reflection angle equal to the angle of incidence may not be too surprising to most people, because it resembles bouncing a ball from a hard surface. However, Snell's law of refraction is at first sight surprising. It is an example of a perfectly recorded empiric law. It soon became a centerpiece of all optical design, and still is today.

The sine-ratio of refraction turned out to be a characteristic of the media meeting at the interface. Late in the 17th Century, Huygens suggested a simple explanation for the constant value of the sine-ratio of refraction. He indicated that the sine-ratio might equal the ratio of the propagation velocities of light in the two media.

Olaf Roemer's Finite Speed of Light

The idea that light might not travel arbitrarily fast between places has not been a major preoccupation of mankind, because it simply was so fast that for all practical purposes man would not have to worry about time delays caused by its finite travel time.

The Danish astronomer Olaf Roemer (1644-1710) was the first who proved that light travels with a finite velocity. In 1667, he was measuring the periods of revolution of the satellites of Jupiter. In the process, he found that the periods changed, depending on the mutual motion of earth and Jupiter. Assuming a finite velocity of light, he correctly inferred in that earth-motion towards Jupiter would give an apparent shortening of the period of revolution, whereas an earth-motion away from Jupiter would lengthen the observed periods of revolution. From the data available to him, he decided on a speed of light of ± 220,000 KM per second, which is not more than 30% off, compared to modern measurements.

Unless a finite speed of light had already become a foregone conclusion among experts, it is possible that Roemer's measurements, at the time, suggested Huygens simple explanation of Snell's law of refraction. For all practical purposes, Roemer's calculations resembled calculations later made by Doppler in the 19th century.

Empiric Ingredients of the Laws of Dynamics

The end of the 17th Century brings us close to the Newtonian laws. We examine here which empiric observations have, in part, induced the formulation of the three Newtonian laws of motion:

I Without forces acting on a body, it is either at rest or in uniform motion.
II Action equals reaction.
III Force equals mass times acceleration.

Law I, without question, was already anticipated by Galilei. Anybody making that observation, of course, had to be aware that forces are a source of motion. Stronger force causes more violent motion and big heavy bodies require stronger forces to bring about motion. Ruling out air-resistance in free fall, Galilei was aware of the fact that all bodies fall equally fast in the gravitational field of the earth. The critical point missing in Galilei's appraisal of the situation was a quantitative definition of motion differing from uniform motion. It was at this very point where the calculus became an indispensable ingredient of physics.

There is evidence that people played around with specialized definitions of acceleration. Centripetal forces and their relation to rate of rotation were, in principle, measurable. They were known prior to Newton. Huygens, who carried on a desperate struggle to make pendulum clocks work on seagoing vessels, invoked many basic principles of dynamics. It was this conglomerate of earlier experiences that is to be regarded as a diverse empirical input that led Newton to the concise formulation of his three fundamental laws.

Kepler's laws of planetary motion became part of the empirical input after Newton established **inverse square behavior of gravitational attraction.** The second law balances the centrifugal orbital force of the third law with the force of gravity here referred to as Newton's **fourth** law. A known moon orbit and extrapolation of earth's surface gravity to the moon orbit may have revealed the inverse square behavior of gravity.

The Cavendish Experiment

In the century that followed, a veritable avalanche of phenomenological explorations ensued from these **four** fundamental laws. There was so much work, there hardly seemed time for new empirical input. Yet important information in Newton's law of gravity was still uncertain. The inverse square behavior was well established as was the proportionality with the masses involved; yet the constant of proportionality that related masses and distance with force was not well known.

Henry Cavendish (1731-1810), a renowned experimenter for many years in the University of Cambridge, decided at age 70 on an experiment that would permit him to measure Newton's gravitational constant. In addition to the many other experiments he had done in his lifetime, Cavendish, in his later years, did an experiment that literally enabled man to weigh the world. For this purpose he used a torsion balance suspending large spherical weights, similarl to what Coulomb had used earlier in measuring the inverse square law for electric charges. Cavendish and Coulomb were acquainted, and the fact that Cavendish had been born in Nice, France, may have helped their communication.

The Work-Heat Equivalent

Steam engines came on the scene more or less at the same time as the French revolution. Ever since, there has been no question that devices driven by heat can do our heavy work. Conversely an American expatriate, Benjamin Thompson (1753-1814) from Woburn Massachusetts, titled by the British as Count Rumford, and later working for the Bavarian military, showed as early as 1798 that it was possible to boil water with the heat generated by the work needed for boring gun barrels. He made a preliminary determination of the work-heat equivalent. Yet it would take another half-century before physics would become thoroughly convinced about the existence of a unique equivalence between work and heat. What were the reasons for this awareness delay about an empirical fact of life that is now so easily taken for granted?

The answer to that question is that the concept of total mechanical energy had hardly been recognized as a conserved quantity in a purely mechanical context. The component structure in terms of potential and kinetic energy prevented a recognition of an energy totality. Therefore, the identification of an even less-understood quantity such as heat, with a still incomplete conception of energy and work, was at that time not as straightforward as we might now believe.

Robert Mayer is usually credited for having formulated in 1841 what is now referred to as the first law of thermodynamics. Joule also made determination of the work-heat equivalent. Heat was now definitely recognized as a new form of energy. Count Rumford went on doing useful things. He became instrumental in establishing the Royal Institution in London. Later he endowed a physics chair at Harvard College in Massachusetts, so, interestingly remaining bad feelings about not supporting the revolution had subsided.

The Discovery of "Brownian" Motion

An empirical fact that in retrospect fits the extended saga of the recognition of heat as an equivalent form of energy is certainly the discovery by Robert Brown (1773-1838) of a microscopic motion of little particles suspended in air or a fluid. His observations were a by-product of his work as a botanist exploring the flora of Australia. His microscopic investigations of 1828 revealed that pollen particles performed a never-ending random zigzag motion as if the small pollen particles were being knocked about in a random fashion by projectiles that were not visible to the microscope.

Significant for these observations was that the intensity of the observed motion appeared to increase with the temperature. Here was a beginning suggestion that heat had something to do with a random molecular motion, or whatever it was of which matter consisted. The introspective 18th Century had given way to a 19th Century suddenly bubbling with new empirical and ontological discoveries concerning the existence of atoms and molecules. A veritable quantitative theory of Brownian motion relating this diffusion displacement to temperature, medium viscosity, and particle size had to wait until a clearer picture of the structure of matter had been obtained. It was given by Einstein in 1905. The hypothesis feature places this subject matter outside the realm of empiricism. It belongs, for that reason, under the heading of ontology.

The Observation of Crystal Symmetries

Through the ages, crystals have been admired as manifestations of nature with intriguing qualities, which made them sought after as gems. In the course of time of having such objects under close scrutiny, man's interest in these gemstones began to transcend aspects of art and business. The crystal gained human attention for its very structure. The angles subtended by its natural surfaces turned out to have very specific values that seemed to be characteristic for a given type of crystal.

Further scrutiny showed that almost all crystals have symmetry axes of rotation, meaning ideally formed crystals appear the same after having been rotated about an integer fraction k of 360 degrees; if $k=2$, it is said to be a two-fold axis, $k=3$ gives a three-fold axis, k=4 a fourfold axis etc.. Soon it was noted that only *2*-fold, *3*-fold, *4*-fold and *6*-fold axes occur. These purely empirical observations seemed somehow revealing about internal crystal structure.

After these initial observations, the geometric puzzlers got to work in order to find out how different symmetry elements could be spatially combined into "closed" groups of symmetry elements. From the empirical starting point of observing symmetry elements, people were moving to complete the phenomenological picture. Let it suffice to mention here the end result of those considerations. Including mirror symmetries, there is a total of 32 distinct crystal classes, not more and not less. Where, when, and by whom this conclusion was first established has remained a somewhat vague point in the modern history of physics.

Crystals without mirror symmetry can crystallize in two mirror-symmetric forms. This phenomenon is called enantiomorphism. Hence if a crystal has no internal mirror symmetry, nature gives it a mirror companion. These two species of one and the same crystal can frequently be told apart by their left- and right-handed optical activities.

There is no question that the empiric facts of crystal symmetry carry strong ontic suggestions related to the atomic and molecular structure of crystals. The mere restriction to *2*- ,*3*- ,*4*- , and *6*-fold symmetry axes seems indicative of a space-filling aspect. When seeking a floor covering, only tiles of *2*- ,*3*- ,*4*- , and *6*-fold symmetry can be fit together; *5*-fold symmetry does not work. However, whereas crystals are restricted by the selection *2*- ,*3*- ,*4*- ,*6*- fold symmetries, molecules are not!

Dalton, the Gas Laws and the New Chemistry

Sometimes the empiric and ontic facts of life are so closely interwoven that it is next to impossible to separate the two. The gas law of Boyle-Mariotte emerged in the second part of the 17th Century. Its statement that product of pressure and volume for a given amount of gas are constant is still a reasonably straightforward empiric observation. It would take another century for Joseph Louis Gay-Lussac (1778-1850) to show how the product of pressure and volume was not constant but dependent on temperature. The next important empiric observation indicated that all gases had, at least approximately, the same coefficient of thermal expansion. This unexpected finding invited a convenient shift in the zero of the temperature scale from centigrade's somewhat arbitrary zero to Kelvin's absolute zero. The gas law now says the product of pressure P and volume V divided by the Kelvin temperature scale T is a constant; $PV/T= C$ is constant. Here we are still close to empirics.

Now we make a transition to ontics. John Dalton (1766-1844) made the observation that for a mixture of gases, the total and each component individually obey the same gas law, by splitting the total pressure into a sum of partial pressures and a corresponding split in C. Finally there is the famous hypothesis of (1811) by Amadeo Avogadro (1778-1858): the constant C in the gas law is the same for all gases of the same pressure, volume and temperature and is a measure for the number of molecules in the volume of gas considered. It was not until the middle of the 19th Century before Avogadro's hypothesis was generally accepted as a fundamental law of nature. It would become of invaluable importance for the development of modern chemistry

Chemistry, as an alchemist-type of empiricism, had been around for a long time. Similarly as astronomy changed its methods of recording after Copernicus' empiric rearrangement, so did chemistry bloom into a new phase of exactness after the work of Dalton and Avogadro. They were instrumental in guiding the path from Democritus' atoms (±400 BC) to contemporary atoms and molecules as undeniably existing constituents of matter. All through the 19th Century, new chemical elements were discovered, and by the turn of the next century, it had become clear that all of nature, as we now know it, has a little less than hundred atomic building blocks. Soon it would appear that these chemical building blocks, or "elements" as they are called, are the same from galaxy to galaxy, covering all of the visible universe. At this point, mankind had encountered a grant moment of encompassing epistemic awareness.

New Perspectives in Optics

Initiated by the famous double-slit experiment of Robert Young (1773-1829), a series of experiments now became possible which seemed to confirm Huygens' earlier contention that light would have to be considered a wave phenomenon. The objective of these experiments was to observe the interference of waves of the same wavelength coming from the same source, but from different directions. For water surface waves, one can easily demonstrate the ensuing diffraction patterns of "standing" waves. Young was able to demonstrate the existence of such patterns for light waves coming from the same source. The color of light was now identified as a matter of wavelength. The wavelength spectrum of visible light was found to be between 4000 and 8000 atomic units of Angstrom = 10^{-8} of a centimeter. The mindset at the time was very much an either/or proposition. Light was either a stream of particles, as had been favored by Newton, or it was a wave motion, leaving undetermined what would be moving. The latter, perhaps more pragmatic, point of view had been favored by Huygens. The French school of Fresnel (1788-1827) and Fizeau (1819-1896) was particularly active in pursuing the empirics following from the ontic proposition that holds light to be a wave motion.

Fundamental Empirics of Electromagnetism

It can be argued that modern electromagnetism is really determined by two fundamental empiric observations. They are: (1) the conservation of electric charge and (2) the law of induction. In chronological order Faraday (1791-1867) formulated his law of induction in 1831. This law quantifies the electric potential generated in a loop conductor as proportional to the change in magnetic flux linked by that loop conductor. As here formulated, this law calls on the two concepts: *electric potential* and *magnetic flux*.

In analogy with Newton's law of gravity and the later Cavendish experiment, Coulomb's law and the Coulomb experiments establishing, in addition, charge polarity and its relation to attraction and repulsion, an electric potential may be said to represent the work to bring a unit of electric charge from a location of zero potential to the location whose potential needs to be established. Rather than fathoming such convoluted language, readers may be more pleased to know that a familiar measure of electric potential is the "volt" delivered from house to house by your power company at the level of ±110 volt.

The *magnetic flux* is defined as the field of *magnetic induction* integrated over the surface, bounded by the loop in which *the electric potential* is being observed. Assuming that the reader does not want to go through with more convoluted definitions of a magnetic field and its surface integral, let it be known that Faraday's induction law is one of the most frequently applied laws in our modern civilization. The design of just about all electrical machinery invokes the Faraday induction law.

The next task is one of delineating what is meant by the conservation of electric charge. If current is defined as the amount of charge that passes a cross-section per unit time, then conservation of electric charge for any given volume means that the change of total charge in that volume is solely determined by the charge that goes through the boundary of that volume. In the course of time, there have been numerous experiments indicating that electric charge is conserved in this sense. Faraday's laws on the electrolytic deposits of metals by electric currents may be regarded not only as a clear indication of charge conservation, it also established the existence of a discreteness of elementary charge involved in chemical reactions. Yet, it was the elaborate study of the behavior of electric circuitry by Gustav Robert Kirchhoff (1824-1887) in 1845 that truly clinched the charge conservation statement. Kirchhoff later became active in the spectral analysis of chemical elements.

This powerful method of chemical analysis was based on the property of chemical elements to emit or absorb, in the appropriate physical circumstances, light of a very specific color-characteristic of that particular element. This method became instrumental in establishing the universality of the new chemistry. It now became clear that the existence of chemical elements as established here on earth was of a truly fundamental importance, because it retained validity throughout the universe known to man.

Coming back to the conservation properties of electric charge, the 20th Century provided a surprise in the form of charged pair creation. It should be noted that the creation of electrons and protons in cosmic rays and acceleration machinery remains compatible with the law of charge conservation, because, until now, such charges have always been created in plus-minus pairs: electron-positron, proton-antiproton.

Finally, a contact needs to be established with the laws of mechanics. A current-carrying conductor suspended in a magnetic field experiences forces that have been described in detail by Ampère (1775-1836). The force is at right angles with the direction of the current, the direction of the field of magnetic induction and proportional to their respective strengths. The force vanishes if current and magnetic field are parallel, meaning it is also proportional to the sine of the angle subtended. If Faraday's law is central to the design of electric generators, Ampère's law is central to the design of electric motors.

The fact that conversely magnetic needles are deflected by electric currents was discussed by Oersted (1777-1851). Ampère's set-up, however, led to a better possibility for precise quantitative experimentation. The mutual sense of directions of current, field and force are usually interrelated by left- or right-hand rules, depending on how the order of the triplet is taken.

Finally, the reader should be aware that the initial similarity between electricity and magnetism had led to two types of units. Weber (1804-1891) established that the ratio between the two was related to the speed of light. This fact was later borne out by Maxwell's theory.

Empirics of the Laws of Heat Radiation

If we consider that life on earth is contingent on heat energy radiated by the sun, it stands to reason that physics has made empiric attempts at establishing what kind of laws heat radiation obeys. To get a clear idea of how much heat energy reaches us by radiation, the radiation transport of heat has to be separated from heat transport through contact, conduction and convection. Since the light and heat of the sun reaches us through empty space, it rules out any of the other mechanisms of heat transport. Carefully controlled experiments by Stefan (1835-1893) established in 1879 that the radiation heat emitted from a hot body is proportional to the fourth power of the Kelvin temperature of that body. It was found that the constant of proportionality of this law did not depend on the constituent materials of the body. A copper sphere or an aluminum sphere would give the same constant proportionality, provided the surfaces of the spheres had been thoroughly blackened. In principle, a black surface and empty space inside (black-body) should give the same result. So, Stefan had established a universal empirical result in radiation.

Ten years later, Boltzmann (1844-1906), capitalizing on the black-body concept, gave a very elegant derivation of this law by assuming that the hollow black-body was filled with random electromagnetic radiation. This thermodynamic derivation, though, belongs under a phenomenology heading. Note, however, that the Boltzmann derivation critically depends on the material independence of the law as indeed observed by Stefan.

The next phase in the understanding of heat radiation had to do with its spectral composition. It was found that the spectral intensity approached zero at the long and short wave length end, with a maximum in between, thus resulting in a bell-shaped curve. The wave length $\neg_m$, where the maximum intensity occurs, becomes shorter and shorter for higher temperatures. Wilhelm Wien (1864-1928) became the 1911 Nobel laureate for his work on the spectral properties of the black body radiation. He found the product $\neg_m$ T to be constant. The value of the constant was determined by experiment, its value later made explicit Planck's theory of heat radiation.

The here-given account of black-body radiation had been solidly established before the end of the 19th Century. Several attempts were made to come to a deeper understanding of the black-body radiation. Wien himself was responsible for a high-frequency approximation of the spectral distribution and Raleigh-Jeans produced a low- frequency approximation using theory concepts available at that time. These efforts are, however, outside the realm of empiricism; they belong in the realm of ontology. Yet for the record, let it be mentioned here that in 1900 Max Planck (1858-1947) showed that a synthesis of the two approaches was possible by introducing a concept of **quantized action**. The action here referred to by Planck (to be distinguished from Newton's action=reaction) is of the nature of a momentum (mass x velocity) integrated over distance.

This work earned Planck the 1918 Nobel prize. The resulting law not only accounts quantitatively for the Wien displacement constant, by integration over the whole spectrum from zero to infinity, it also produces a correct value for the proportionality constant of the Stefan-Boltzmann law of total black-body radiation.

A Survey of Twentieth Century Empiricism

A scrutiny of the preceding description of empirical experiences shows an increasing difficulty of separating empiric observations from ontic injections. The fact is that once certain ontic elements have become accepted as established parts of physics, the strategy of empirics adapts to this new situation by incorporating those elements as essential features of the physical world we know.

A perhaps striking example is the observation of the photo-electric effect made at the end of the 19th Century. The experiments confronted physics of that time with the enigma that photo emission manifested an unexplained cutoff, if the wavelength of the radiation was increased. This cutoff happened to be quite sharp and the effect could not be undone by increasing the intensity of the radiation of that longer wavelength.

An answer to this enigma was given by Einstein (1879-1956) in the same year (1905), when he published his papers on Brownian motion and the special theory of relativity. Einstein showed that the answer to the emission cutoff was a consequence of Planck's hypothesis about the quantization of action. In fact, it could be used to measure an accurate value of Planck's action quantum h in good agreement with the values that could be inferred from the measurements on black-body radiation.

Another case invoking the same action quantum was the observation of the Balmer spectral series of hydrogen and the 1913 quantitative account given by Bohr (1885-1962) invoking the same action quantum.

The experiments and the corresponding ontic injections made by the theories of Planck, Einstein and Bohr straddled the transition from 19th to 20th Century. They started off a new "quantum" era.

From that time on, almost all fundamental empirics had either directly or indirectly something to do with the "phenomenon of quantization," as it was now called. There is an extensive literature dealing with quantization and its consequences. Here an attempt is made to select mostly those contributions that have opened up new avenues in that part of empirics that is somehow related to quantization. Here follow brief descriptions referred to by their year of publication or recognition.

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<u>1908</u> Rutherford (1877-1937) received the Nobel Prize for chemistry in recognition of the total outcome of his scattering experiments. His work would simultaneously open up atomic as well as nuclear physics.

<u>1911</u> The discovery by Kamerlingh Onnes that certain metals become superconducting at very low temperature. The zero resistance seemed total, because currents have been observed that go unabated for hours, days, and longer.

1912 Father and son Bragg show that X-ray diffraction patterns can be produced by having crystal lattices act as diffraction gratings. In 1916, Debye-Scherrer showed how similar results could be obtained with powdered crystals.

1913 Moseley (1887-1915) publishes his work on X-ray spectra of atoms. He established how nuclear charge (atomic number) rather than atomic weight determines chemical properties, which led to the concept of isotopes as elements of same atomic number, but different atomic weight. His work very much helped to complete Mendeleev's systematic table of chemical elements and made it possible to predict where new elements were to be expected, bringing the total number to 92. Several new ones have now been added, most of which though are unstable .

1914 Following Bohr's work on hydrogen's stationary states, Franck and Hertz devised an experiment by means of which stationary states of gaseous substances could be demonstrated to exist, and last but not least, their energy levels could be measured.

1927 Davisson-Germer and Thomson show that electron beams of uniform velocity are capable of producing diffraction patterns similar as those observed by the Braggs and Debye-Scherrer. These experiments confirm a quantum relation earlier postulated by de Broglie.

1933 Meissner and Ochsenfeld show that, in superconductors, not only the electric field vanishes, but also the field of magnetic induction is expelled from the superconductor. This simultaneous expulsion becomes understandable from a point of view of spacetime description.

1957 Mössbauer observes that nuclear resonances in the realm of X-rays can become extremely sharp at low temperatures. The phenomenon is explained as the result of a recoilless emission; instead of a single nucleus the joint nuclei of the whole crystal lattice take up the recoil impact. This phenomenon introduces a new era of precision measurements.

1960 German (Doll *et al*) and US teams (Fairbank *et al*) confirm the existence of flux quanta at half the value earlier anticipated by Fritz London. The flux quantum is found to equal h/2e, *i.e.,* Planck's quantum divided by twice the electronic charge.

1962 Brian Josephson conducts a number of experiments demonstrating the tunneling capabilities of superconducting electrons through thin insulating layers. This new effect leads to interesting new measuring devices. One of them, the Josephson ac effect, permits measurements of the h/e ratio with unparalleled precision.

1980 Von Klitzing and fellow workers experimenting with two-dimensional semiconductor samples at low temperatures and high magnetic fields discover that the ratio of Hall voltage divided by sample current assumes constant values $h/i\, e^2$ in which i is either an integer or, as later discovered, a rational fraction.

Josephson and quantum Hall effects together provide to date the most accurate e and h values approaching ten significant decimal points.

1989 The experiments by Tonomura *et al* show how diffraction patterns result from a statistical build-up of individual particle incidences. This experiment may well establish a landmark event, because after all those years it finally indicates that the mystic Newton may have been closer to the true nature of light than the pragmatist Huygens. Yet, as long as there are enough particles, they may be easier handled by waves.

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The account of work covering the Twentieth Century does not end here. A massive amount of empiric reconnaissance work has been performed in the nuclear realm. A tremendous number of particles have been generated by the accelerator machines. Most of these particles are unstable and fall apart until the remains are stable. Empiric rules have been established for particle interactions. Particles have been assigned numbers such as Lepton- and Baryon number in such a manner that charge, lepton, and baryon numbers are conserved in certain interaction processes. Only electric charge ties in with an earlier existing concept. The others are purely empiric additions. There are other assigned properties such as parity, spin, isospin, hypercharge and helicity. Some are universally conserved; for others it depends on the type of interaction. The latter are subdivided in strong, weak, or electromagnetic interactions.

Properties of these elementary particles such as charge, mass, and spin are inferred from trajectory behavior and interactions. Let us cite here the most dominant particles that occur in collision interactions of nuclei. There are charged particle-antiparticle pairs: ± electrons,± m-mesons, ±pions, ±protons. Neutral particles occurring in pairs of particle anti-particle are neutrons, neutrinos; their pairing is not determined by charge, but may, perhaps, be thought of as a form of enantiomorphism as encountered in crystals. Finally, there are the neutral pion and the photon, which are considered to be their own anti-particle.

TABLE OF ELEMENTARY PARTICLE PROPERTIES

name	**charge**	**mass**	**spin**	**magn. moment**
-electron	- e	m	¡/2	-(e/m)¡/2
+electron	+e	m	¡/2	(e/m)¡/2
+µ meson	+e	3am	¡/2	(e/3am)¡/2
- µ meson	- e	3am	¡/2	-(e/3am)¡/2
+pion	+e	4am	O	O
- pion	- e	4am	O	O
neutral pion	0	4am	O	O
+proton	+e	27am	¡/2	there are no simple
-anti proton	- e	27am	¡/2	theoretical expres-
neutron	O	27am	¡/2	sions for these
anti neutron	O	27am	¡/2	magnetic moments
neutrino	O	O	¡/2	O

anti-neutrino	O	O	¡/2	O

Table I: The purely empiric value a=68 gives, within a percent, cited masses and magnetic moment data. The magnetic moment expressions are according to the theory of Dirac.

PHENOMENOLOGY

If the 17th Century, so to say, opened up the empiric inquiry into the laws of nature, the 18th century could be called the era in which the phenomenologists became active and ruled supreme for some time. They took the legacy of Newton and examined the possibilities of what they could do by further developing the mathematical principles of the calculus. It is of interest to note that 18th Century physicists were mostly leading mathematicians, and most 18th Century mathematicians were extremely accomplished physicists.

Lagrange, Euler, d'Alembert and the Bernouillis were the people who brought Newtonian mechanics to full bloom by applying the full force of mathematical rationalism. Had they practiced the same type of professional chauvinism as, at times, has been seen in the 20th Century between physics and mathematics, they might not have gotten anywhere. All of which goes to show that, regardless of how much we may depend on good specialists, 'specialism' all by itself is not to be taken as an object of a life's dedication, because, at any time, a specialization may become obsolete. An "evil" generalist will come along and show an easier and better way. In so doing, he turns the triumph of a life dedicated to specialism into the tragedy of mastering an art no longer needed.

The explosion of mathematical ability in the 18th Century, as triggered by the calculus and Newton's laws, was accomplished by people who at all times retained a good overview of the situation. Since mathematics played a major role in these developments, the question arises: how do we explain and talk about these matters without getting into the details of mathematical development? Probably the best way of accomplishing this is by giving a description of how Newtonian mechanics branched out into a number of disciplines. To appreciate the creation of these branches, we need to be aware of some inherent limitations of Newtonian mechanics.

The major triumph of Newton's laws was a derivation of the planetary trajectories. If we know how small the planets are as compared to the immense size of their trajectories, we may realize that Newton's laws, as formulated, apply merely to mass-points. Newton was well aware of this limitation. In fact, any time we speak of a trajectory, we can only think of that trajectory as traced out by a point. So, presented with a finite body, what is the location of the point to be taken as representative of the trajectory motion?

In addition to the trajectory motion, the object can be rotating. So, taking any peripheral point of the body, we would get a trajectory with a superimposed wiggle . There is, however, a point inside the body that is at rest with respect to the rotation. It is known as the center of gravity. Newton's mass-points are therefore abstractions of finite bodies that have all their mass concentrated in the center of gravity.

How do we get from laws of mechanics that apply to point-masses to the behavior of finite bodies, or the behavior of fluid media such as gases and liquids, and last, but not least, elastic media? Could all these potential branches be completely determined by Newton's laws of point-mechanics? It seemed a rather daunting task to work from the specialized Newtonian statements to the more encompassing fields of what are now known as rigid body dynamics and fluid-dynamics.

There still are today pending questions as to whether mechanical behavior in the large can be completely governed by the behavior in the small, as cited in Newton's laws. It seems easier and more convincing to work from the general (in the large) to the special (in the small). As will be discussed later, the latter procedure is indeed possible for electrodynamics. Yet, notwithstanding gallant efforts known as Hamiltonian procedures, it has not been possible to escape the conceptual grip of a locally centered understanding of mechanics. The Newtonian breakthrough was made possible only by starting with a point-centered description.

Following roughly a chronological procedure, we now discuss new branches of mechanics and how they relate to contemporary life.

Rigid Body Dynamics

Although some knowledge of rigid-body dynamics was already around in the days of Newton, the complete laws of rigid-body dynamics were formulated by Leonard Euler (1707-1783). For the purpose of bringing out similarities with Newton's second law (force=mass times acceleration), let us reformulate the latter as force=change of momentum per unit of time, in which momentum is defined as mass times velocity.

The fundamental law of rigid body dynamics is a similar type of relationship. It says: torque = change of angular momentum per unit of time. The concept of torque is defined as force times its arm of leverage. Similarly, angular momentum is momentum times its arm of leverage.

It follows from this law that zero torque means the angular momentum vector is constant in time, hence angular momentum retains a fixed direction in space and its modulus is constant. Having devices retaining known fixed directions in space, one does not need stars to navigate on the seven seas. In 1957 the submarine "Nautilus" crossed the polar basin underneath the icecap using such *inertial guidance systems.* The trick is how to suspend spinning tops so that they are not subject to even the smallest external torques. These technical problems were satisfactorily solved by the Draper Laboratory in Cambridge, Massachusetts.

Euler's derived law of rigid-body dynamics has great similarity to Newton's first and third laws. Without external force, a body is either at rest or in uniform motion. Analogously, without an external torque, a top is either at rest or spinning uniformly.

Yet, while momentum is simply the product of mass and velocity, angular momentum is not simply the product of two things. It is instead the result of a so-called tensor operation on angular velocity. This tensor is known as the moment of inertia; it is mass related to the square of its lever-arm with respect to a body's gravity center. A tensor, strictly speaking, is a mathematical object. Tensors acquire a role of great importance in the study of anisotropic media, relativity, and noneuclidean geometry.

Every solid body thus has a moment of inertia characteristic of that body. It is shown that a rigid body has three mutually perpencicular principal directions of inertia for which angular momentum and angular velocity have the same direction. The car mechanic who does your wheel balancing, is, or should be, expert in aligning the wheel's principal axis with the direction of angular rotation.

While Euler did not have to worry about balancing the wheels of his car, he could quantitatively explain a number down-to-earth things such as the precession of a spinning top on a smooth floor, as well as less mundane things as precession of the earth's axis of rotation. In a contemporary context, Euler's equations help to design the spin cycle in your washing machine; in short, they helped in the design of the smooth vibration-free operation of numerous pieces of machinery. Who says old stuff is not worth knowing?

Fluid Mechanics

Euler not only gave us the fundamental equations of rigid-body dynamics, he also transcribed the Newton laws to serve a purpose in the study of fluid behavior. It is believed that Euler had pangs of conscience before he decided on a final formulation of the equations that are now known as the "Euler equations of fluid dynamics." They are partial differential equations that give, at any time and at any point in the fluid, a balance between inertia forces and forces due to gradients in static and dynamic pressure. Euler assumed that, for many practical purposes, the fluid could be taken to be frictionless.

There is a remarkable change in Euler's point of view in the process of transcribing Newton's laws. Writing down Newton's equations of motion, one writes down a balance of forces while moving along with the object under consideration. Euler, by contrast, assumes a stationary position with respect to which the fluid is in motion.

It is of interest to know that Lagrange (1736-1813) formulated a similar set of equations by giving a balance of forces while moving along with the fluid. One then finds that the kinetic pressure term in the Lagrange equations is missing. Yet, the two formulations are found to be compatible by taking into account mass conservation and the added (indirect) time change due to fluid motion. Euler, almost thirty years older than Lagrange, recommended him for a position at the Berlin academy. It thus seems likely that these two pioneers may have exchanged views about this subject matter.

As mentioned earlier, Euler's equations account for Archimedes' law of buoyancy, and they also account for an hydrodynamic result earlier found by Daniel Bernouilli (1700-1782). Theory and experiment illustrate the reality of the kinematic pressure and its force components. Even today, the Euler equations of fluid dynamics are called upon to account for the lift of airfoils. The force accounts for the propulsion of a strange "sailing" vessel with rotating vertical cylinders sticking out of its deck, as well as the "curve" balls in a variety of sports. All this holds reasonably well, as long as internal fluid friction can be ignored.

The equations of Navier (1822) and Stokes (1845) are attempts at modifying the Euler equations by accounting for viscosity. Stokes gave exact solutions for spheres and cylinders moving in viscous fluids. The ensuing quantitative relations have been used to measure viscosity coefficients of fluids and have given results in good agreement with other methods of measurement not critically dependent on a full scale use of the Navier-Slokes equations. The conspicuous Stokes result for spheres was later used by Einstein in his theory of Brownian motion and in Milikan's famous oil-drop experiment.

Elastic Media

Gases, liquids, and solid media are capable of supporting waves. The component of kinematic stress can be neglected for this purpose, because there is no appreciable flow. The design of musical instruments, during the 18th Century, owes a great deal of insight to the mathematicians and physicists of its days. They provided wave equations and solutions for strings, membranes, air columns: in organs, trumpets, horns, trombones and woodwinds. The existence of a wave equation and the discovery of its solutions go back to that time. D'Alembert (1717-1783) and last, but not least, the Bernouillis were instrumental in these matters.

Cauchy (1789-1857) extended the derivation and use of wave equations to the elasticity of crystals. He established that, for every direction of propagation there are, in general, three distinct velocities of propagation. In isotropic media, this number reduces to two distinct velocities of propagation. The latter are longitudinal and transverse modes of propagation. By measuring the propagation velocities, one can measure the two elastic constants that characterize an isotropic solid; they are either cited as Young's modulus and Poisson contraction, or they are cited as bulk moduli. Media that have direction-dependent properties are called anisotropic. They have many more elastic parameters: their measurement is a much more tedious process. For instance, while isotropic media have two independent elastic constants, quartz crystals have as many as six independent elastic constants.

Lagrangean and Hamiltonian Methods

Apart from creating his version of the equations of fluid dynamics, Lagrange became known for his very incisive work in transcribing Newtonian mechanics. His very mathematical methodology greatly improved the invariance structure of mechanics. At the same time, he delineated in different ways the physical content of the laws concerned.

While the Newtonian formulation of mechanics had been closely tied to the use of orthogonal Cartesian frames of reference, the Lagrangean approach liberated mechanics from this apparently unnecessary restriction. The underlying idea was that the laws of nature should not be unduly biased by subjective individual decisions favoring specific frames of reference. Here we are confronted with a first attempt at formulating laws of nature independent of specific frames of reference.

In mathematical language, this means one seeks a formulation that is invariant (remains unchanged) under the widest group of transformations that interrelate frames of reference. Later, this was found to be a wise decision because it facilitated transitions to continuum mechanics as well as relativity and quantum mechanics. To appreciate these methods, it is necessary to develop a feel for invariant mathematical expressions, plus knowledge under what group of transformation such invariance features will be retained. Ensuing improvements are found to be contingent upon rather fundamental changes in the starting point of law formulation.

Since the Newtonian formulation demands a balance of forces, explicit frames of reference are needed to delineate force components. The Lagrangean formulation first introduced the concept of kinetic energy* and subsequently introduced the concept of generalized forces. A more complete frame-independence was accomplished with the introduction of the notion of potential energy. The latter is the work done against the external forces; only if that work is independent of the path has it acquired a unique meaning and can it be called a "potential energy."

At this point, there are two energy-like components: the kinetic energy T and the potential energy P. The sum of the two became recognized as total energy E=T+P. The difference of the two is now referred to as the Lagrangean action L=T-P. It appears from these considerations that the notion of energy was by no means a well-defined concept in Newtonian days. Bits and pieces of energy components really arose from solving Newton's equations. Energy was first known as a mere integral of these equations, it also was distinguished as having invariant properties. While this awareness was around at the end of the 18th Century, it would take another four decades before the notion of energy would receive the wider recognition it received in the first law of thermodynamics. It was a great variety of empiric inputs, their rational and ontic processing that finally brought out a much wider range of energy manifestations encompassing electric- magnetic-, heat- and nuclear-energy.

However, prior to the era of thermodynamics, Lagrange recognized that the action function L would take a central place in his formulation of particle dynamics. Subsequently, Hamilton (1805-1865) considered L as the integrand of an integration over time. Examining the extremum features, of that integration, according to a procedure developed by Euler, the Lagrangean equations are found to be exactly the condition for an optimum value of this integral. It has become known as "Hamilton's action principle for particle dynamics." It also led to equivalent formulations known as "Hamilton's equations of motion" as well as a

* Earlier used by Leibniz under the name "living force."

"Hamilton-Jacobi equation," which is a partial differential equation whose characteristic equation are the Hamilton equations.

Another remark in order here has to do with geodetic (shortest) lines in Riemannian geometry. The Lagrangean equations, when written in terms of the metric coefficients, assume the form of so-called geodetic equations. This structural resemblance would later play an important role in the general theory of relativity: objects exposed only to inertia and gravity follow a geodetic path in the Riemannian spacetime continuum.

The Hamilton principle for particle dynamics has been successfully extended to continua. The time integral then becomes a spacetime integration of a Lagrangean density which is the difference between kinetic and potential energy densities or, if you will, the difference between magnetic and electric energy densities. The word *density* refers to the fact that energy components are defined per unit volume. Here is an early invitation to consider spacetime formulations instead of purely spatial formulations with time as a parameter telling us how things evolve.

It should be mentioned that Maupertuis (1698-1759) had earlier forms of an action principle. It appears that the Hamilton principle emerged as the result of finally recognizing the very gradual evolution of the energy concept. There are principles and concepts that take a long time before they can be seen in the full context of their ramifications. They are examples of initially narrow and preliminary knowledge acquiring a wider scope in the process of reaching for their epistemic goal. It illustrates the drawn-out transitions from phenomenology and ontology to epistemology.

The Science of Thermodynamics

Thermodynamics deals with the many ways in which heat and mechanical energy can interact. It developed, so to say, in the wake of the industrial revolution brought about by the steam engine. As such, it has become one of the beautiful examples of how systematic pursuit, starting from empirical facts, can lead to a grand phenomenological structure with widespread ramifications in physics, chemistry, and technology.

Once the Mayer work-heat equivalence of 1848 had become fully accepted, it became known as the first law of thermodynamics. The second law of thermodynamics was a recognition of the need and existence of an additional thermodynamic description parameter. This unknown entity would have to be complementary to temperature, say, similarly as pressure is complementary to volume, and potential is complementary to charge. This new thermodynamic variable was given the name entropy, meaning "referring to a state of change."

It was understood that heat and temperature could not function as a duo of variables for this purpose; their product had the wrong dimension. It had to be temperature and this newly created variable; together they made up heat. The entropy concept is one of the very subtle inventions of 19th Century phenomenology. It opened windows to new horizons. In mathematical terms, it meant entropy had *a priori* integrable qualities, heat as an energy component did not have that quality. The integrability of entropy is now frequently referred to as the first part of the second law of thermodynamics.

The second part of the second law of thermodynamics claims that entropy can only increase; it cannot decrease. The latter statement is equivalent to the statement that heat can flow only from higher to lower temperature. It was pointed out by Maxwell that intelligent tiny creatures (Maxwell devils) capable of separating fast and slow molecules could violate the second part of the second law. Without those smart creatures the universe would be destined to die the death of an ever-increasing entropy. Later, entropy was found to be a measure of disorder.

The two laws of thermodynamics here discussed much improved the understanding and the limitations of all thermal engines. The maximum obtainable efficiency of the steam engine was shown to be governed by the differential between input and output temperatures. The heat process in chemical reactions became better understood through thermodynamics, as well as the coexistence of gaseous and liquid phases.

Boltzmann made a spectacular applications of the laws of thermodynamics with his derivation of the Stefan law of heat radiation.

The Emergence of Spacetime Descriptions

Spacetime descriptions are usually associated with the relativity era, the truth of the matter is, however, some of it emerged prior to relativity. The fact is that the more dramatic aspects emerged during the development of relativity. A good plea can be made that if not Lagrange, it was Hamilton who, at least indirectly, called attention to spacetime description features. Here is a possible view of the situation, provided a little mathematical detour is permitted.

Consider hereto Hamilton's action principle. Assume the equations of motion are met, the remaining contributions to the integral occur at the spacetime beginning and end of the spacetime trajectory. This boundary contribution assumes the form of a spacetime displacement with coefficients that are found to be energy-like, as a coefficient of the time displacement, and three momentum-like components as coefficients of the spatial displacements. In modern lingo, this linear expression is said to define a differential one-form in the four dimensions of spacetime. Its conditions of integrability are the Hamilton equations of motion, which happen to be equivalent to the Lagrange equations. This transcription from Lagrangean L to the action one-form of energy and momentum is known as a Legendre (1752-1833) transformation.

The more spectacular examples of invitations for entering into spacetime description occur in Hamilton-based descriptions of the continuum realms of electromagnetism, elasticity and, despite claims to the contrary, quantization.

The name Minkowski (1864-1909) is perhaps most indelibly connected with the introduction of spacetime description. Yet, simultaneously, also Hargreaves had been making similar observations. Whereas from the beginning, Minkowski considered a spacetime metric, Hargreaves' work seemed more oriented towards a perception of spacetime as merely a differentiable manifold.

Without Minkowski's metric spacetime manifold, there might be some real doubt whether Einstein could have developed the General Theory of Relativity as we know it today. The basic idea of spacetime description is really a connotative evolution in the time concept. Whereas Newton had insisted on a connotation of the time as a parameter that was said to be absolute, Minkowski added the feature of a coordinate quality that could not be absolute, but had to admit a *frame contingency*.

A number of mental exercises related to the so called "Doppler effect" and its connection to the phase invariance of wave propagation had long ago invited an option of attributing time with a coordinate quality. Waldemar Voigt of crystal physics fame, was one of the first to consider that angle. In so doing, he ended up with a spacetime transformation that today would be referred to as a first order Lorentz transformation. Inversion of that transformation leads to asymmetry that would contradict "Galilean" relativity. An exercise in symmetrization can now be shown to lead from the asymmetric Voigt transformation to the actual Lorentz transformation, which is symmetric under inversion.

Once the Lorentz transformations are known and we consider the many ways in which they can be derived, one wonders whether a thorough assessment and comparison of these varied approaches can bring us closer to an improved epistemics of these transformations.

The Phenomenology of Electricity and Magnetism

In collecting the empiric input material for a phenomenological theory of electricity and magnetism, it was noted that basic information was obtained through experimentation involving globally complete systems. There was no need to extrapolate observation made on finite bodies to point-like objects such as Newtonian mass-points. An exception seemed Coulomb's law dealing with the polar nature of point charges The Cavendish gravitation experiment was patterned after Coulomb's experiment. Its sources of gravitational force were rather large heavy spheres. Newton, however, had already confronted this predicament by mathematically showing that those spheres could be replaced by mass-points.

Faraday's induction law should be mentioned as a unique example of a global observational input. The collective information culminating in Kirchhoff's charge conservation has more of an inferred character. Then there is Gauss' global law of electrostatics as an alternative of sorts to Coulomb's force law. Finally there is the Ampère force law that leads to a cyclic integral measuring current. Since current is charge per unit time. Ampère and Gauss laws become united under the heading of spacetime description..

The current and charge aspects of the Ampère-Gauss law establish contact with the mechanical world of forces. In continuum mechanics, Lavoisier's (approximate) law of mass conservation may only be regarded as a law statement that is nearly global in nature. Major questions are associated with the implications of the mathematical machinery used in making transitions from local mass-point events to events requiring a global characterizations.

The phenomenology of mechanics has taught us how to work from the local observation to a globally encompassing point of view. There were two stages in that development. First, a transition had to be made from the Newtonian particle-based formulation to Euler, Navier-Stokes and Cauchy equations governing the local behavior of continua. The next step in the single particle as well as the continuum case is the integration of the differential equations that are said to govern those situations.

In electrodynamics it is the other way around; experimentally we start off with a global integral result and from there we work our way around towards a local equivalent in terms of differential equations. The latter program was most completely achieved by James Clerk Maxwell (1831-1879). There are reasons to assume that local happenings don't uniquely determine what happens in the large and, conversely, knowing what happens in the large has more of a chance determining what happens in the small. So our problem is to convey the essence of these mathematical techniques without getting involved with all the technicalities of the calculus.

It should be a source of hope that Faraday, notwithstanding his limited training in calculus, became a prime force in discovering one of the most fundamental global laws of nature. He was instrumental in creating the concepts that made Maxwell's mathematization possible. Faraday, with his lines of force and tubes of force, became one of the eloquent creators and supporters of a field concept outside of matter. In continuum mechanics, we saw already the emergence of a field concept in matter.

The Newton gravity and Coulomb's electrostatic laws initiated vacuum fields. These laws were cast in integral form by Gauss (1777-1855). Later, Ampère, but especially Faraday, extended these field concepts to magnetism. Faraday enriched the field concept with geometric illustrations of his tubes and lines of force. They were called "fields" or more specifically "vector fields." In fact Faraday distinguished four mathematically distinct vector fields in space, and so did Maxwell! There were line vectors (parallel elements of surface) and surface vectors (tubes), each of which could be given directive (arrow) or circular orientation.* The electric field E is a line vector with arrow, the magnetic field H is a line vector with circulation direction. The dielectric displacement D is a tube with arrow, and the magnetic induction B is a tube with inner circulation.

The great simplifiers of modern times have done away with Faraday's mathematical inventions and Maxwell's substantiation thereof, by boldly replacing the four Faraday species by one mathematical vector-type. This identification is

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* The inverse distance of the parallel surface elements is measure of field strength (e.g., gradient field) of a line vector, whereas the inverse cross section of a tube-type vector measures field strength.

permitted within the orientation preserving orthogonal group. As a result, attempts at using more general coordinates require the erection of local orthogonal frames to retain the now generally accepted vector identifications. A text by Schouten has preserved Faraday's field inventions and his physically important field distinctions by putting these features in the mathematical perspective for which Maxwell had been reaching. Although this text has been around for almost half a century, there, so far, are no real signs in the general text book literature of keeping Faraday's discerning judgment alive. Only ad hoc tidbits appear in those branches of physics where such distinctions are essential.

All of this brings us to some major mathematical developments that became instrumental in dealing with these fields. They have to be delineated prior to a more encompassing discussion of global integral situations and their relations to a local differential point of view.

The Theorems of Local-Global Field Relations

The subject matter to be discussed in this section is one of those structural developments in mathematics that took a long time before most of the ins-and-outs had come to a level of mature awareness. Pioneering in this realm goes back to Gauss' *divergence* theorem of integral calculus, which was then followed by Stokes (1819-1903) theorem, which said something similar, yet seemingly using altogether different attributes. Where Gauss was using a differential operation known as "divergence", Stokes was using a differential operation known as "curl". These structurally very different-looking differential operations have played an important role in the early development of the mathematical (field) theory that was to be erected on Faraday's field concept. Yet, by the same token, their seemingly different appearance obscured a perspective that would become relevant later in future developments.

For the sake of simplicity, the early preachers of the Maxwell gospel reduced Faraday's four distinct vector species into one species that was to serve all purposes of electromagnetism and all purposes of physics: period! Faraday and Maxwell might have had second thoughts about these decisions, because the earlier mentioned distinctions of several vector species had been rather instrumental in Faraday's thinking. After all, this type of thinking led to the very fundamental induction law.

Here is one of those instances where the originator of a law had the epistemic perspective to bring in the correct ingredients, yet followers felt they could dispense with those ingredients. Hence followers took out elements that remind us *where they came from* . In so doing they forced a good epistemic result into a state of ontic isolation. Therefore, in speaking about these matters, it is better to describe the concepts without use of standard mathematical symbols and formalisms, because they bring back some old habits we wish to avoid here.

Let us now briefly sketch how the Gauss-Stokes initiatives have fared later in the 19th Century and thereafter. In the 19th Century, Cauchy made an important application of Stokes' theorem for his residue theorem of Complex Analysis.

Then, in the course of time, a number of authors extended the awareness of a diversity of features of these integral theorems. Poincaré (1854-1912) and Brouwer (1882-1966) gave outlines of dimensional generalizations. Cartan (1869-1951) and Schouten (1882-1971) stressed conditions for which these theorems exhibit properties of frame- as well as metric-independence. De Rham, capitalizing on these extended invariance features, then realized their potential for purposes of topology. In so doing, he reactivated a feature Gauss must have sensed already, in witness of what in physics is known as Gauss' theorem of electrostatics: *i.e., the prototype of a period or residue integral*.

So, in the course of time, people have become increasingly perceptive about the Gauss and Stokes theorems. It should now be remarked that these laws are generalizations of the principal theorem of the integral calculus, in which boundary values of the integral function equals a one-dimensional line integral between those boundary points. The derivative thus appears as the inverse of integration. In the general Gauss-Stokes case, a *cyclic integration* has a uniquely defined inverse operation that is called *exterior derivation.*

In the middle of the 19th Century, mathematicians became interested in geometries of higher dimensions than three. These geometries don't have to be metric or Riemannian.* So let it be known that the fundamental theorem of the integral calculus is not tied in with a metric specification either, it therefore stands to reason that neither Stokes' nor Gauss' versions of such theorems tie in with metric structure. Unfortunately textbooks used for the purposes of physics do not bring out these metric-independent features. The reader may now sense some remarkable generalizations. Let us give this general formulation, normally referred to as "Stokes' generalized theorem" in a "nontechnical" language as much as possible.

Given an n-dimensional differential manifold with a p-dimensional scalar- or pseudo scalar-valued integral over a closed (cyclic) p-dimensional domain (0< p< n). This p-integral equals an (p+1)-integral taken over a (p+1)-domain bounded by the cyclic p-domain. The integrand of the (p+1)-integral obtains from the integrand of the p-integral through the process of exterior derivation.

For the case p=0, it is the principal theorem of the integral calculus depicting a line integral of the exterior derivative, the latter is here a gradient of a scalar function in an n-manifold. This makes the line integral only dependent on the end points of integration, independent of the path between the two points.

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* Riemannian manifolds are metric, but they are commonly understood to have positive definite metrics. The metric of spacetime is said to be indefinite. It is important that both theorems hold regardless of whether a metric exists or has been defined; manifolds without metric are referred to as differential manifolds indicating that differentiability is retained.

For n=3 and p=2, it is Gauss' theorem, the exterior derivative becomes the divergence of standard vector analysis.*

For n=3 and p=1, it is Stokes theorem, the exterior derivative becomes the curl of standard vector analysis.

Note that for n=3 and p=1, the 2-dimensional surface bounded by the one-dimensional integration loop is not unique. An infinity of surfaces is bounded by that loop and all these integrals over distinct surfaces are equal, because they equate to the same line integral.

Now consider the case n and p=n-1, or specifically for ordinary space n=3 and p=2. In this case there are only two domains bounded by the closed surface in space. In a Euclidian n=3, we say there is an inner and an outer domain. (Compare footnote)

In Euclidian space, mathematical conscience dictates assumptions to the effect that there is a behavior at infinity to make integrals over the outer domain vanish. Such assumptions are permissible when concentrating solely on what exists in the inner domain. Yet a sustained ignoring of things in the outer domain would be an arbitrary act of convenience to study only things close at hand. It is now important to consider here that Gauss' law for electric charge has residue polarity, whereas gravity is nonpolar, their ramifications for the outer domain become vastly different.

A Euclidian outer-domain is a rather unmanageable thing, because its extension is unlimited. A compact closed manifold is more manageable; let it be visualized as equivalent to a three-dimensional sphere. A two-dimensional sphere imbedded in a three-sphere gives a Jordan-Brouwer separation between two domains, similarly as a circle on a sphere separates two domains. Now the question arises: what is outer- and what is inner-domain. Just having it depend on the mutual size would not be an absolute distinction that is usable in this mathematical context.

Assuming the surface normal is pointing *out* of what is originally meant to be the inner domain, then that same normal points *into* the outer domain. Hence, according to the generalized Stokes recipe, Gauss' theorem in a compact closed three-dimensional manifold leads to the difference between the two contributions of inner and outer domains respectively.* So in a purely spatial context we have:

An integral of a (surface) vector field taken over a 2-boundary in a compact closed 3-space equals a volume integral over the interior minus a volume integral

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* In the traditional 3-manifold that is Euclidian, the outer domain is normally ignored as "empty." For a compact and closed 3-space, it becomes necessary to become specific about inner and outer domain.

* Gauss' law extended to a compact physical space permits a remarkable application to Mach's principle: see hereto E J Post and M Berg, *Epistemics of Local and Global in Mathematics and Physics* (p.324 of the Proc.of the Sept. 1996 London Conference on *Physical Interpretations of Relativity theory*, British Society for the Philosophy of Science.

over the exterior. Whatever is interior or exterior is solely determined by what is defined to be the normal direction.

A similar theorem holds for p=n-1 in an n-dimensional closed manifold. Unlike a past that has shown itself blind to orientability distinctions, in modern physics it is now mandatory to discriminate between pseudo-scalar-valued integrals (e.g., Gauss' law of electrostatics) and scalar-valued integrals (Gauss' law for gravity).

Formalism Choices for Global Assessment

A promise was given to avoid undue technical expositions of mathematical matters related to the phenomenological build-up of physics. Yet, if some details are at variance with sound principles, it is mandatory to be alerted to inadequacies in contemporary mathematical processing of physical laws. A detailed technical account using contemporary notations would exactly obscure the points that are worth making. So, rather than obscuring those details, a way has to be found to delineate alternatives, and that is exactly what is being attempted here.

Resurrecting in modern garb the lost Faraday details can be achieved by calling on Cartan's frame-independent mathematical discipline of differential forms. Much of the work shown in this vein has been paying lip- service to something that is said to be better, while carrying on with old identifications of the past, or worse. Violations against the clean mathematical spirit injected by Faraday are simply not mentioned. Such are token efforts of using something new, merely for the sake of *newness*. In lacking the proper physical motivation, such efforts cannot take full advantage of perspectives created by the differential form discipline of Cartan (1869-1951).

Let us summarize what these full advantages imply. They are frame-independent and for the most part metric-independent. It should be mentioned that the Gauss-Stokes, or as they now say, the generalized Stokes theorem* is totally frame- and metric-independent. The implication of metric-independence is at first puzzling from an angle of physics. A stereotype objection suggests that features of metric-independence can hardly have any physical meaning at all, because all measurements in physics are tied in with units of length and time.

Metric-independence became a discovery of temporary interest in the Twenties and Thirties, but was then abandoned for what seemed to be a lack of realistic physical perspectives. Yet, during the late Thirties in articles, and then in the Fifties in a monograph, de Rham shows how exactly these metric-free features of the Stokes theorem make it an ideal tool for topological investigation. Topology isolates structure that is preserved under deformations and for that reason should not depend on metric size.

* For unknown reasons Gauss' name in this context has been dropped in the recent literature.

Now suddenly it is time to remember our good old friend Carl Friedrich Gauss, who left us his law of electrostatics. This law says: a closed surface integral of the dielectric displacement **D** equals the net charge enclosed by its integration surface. In the perspective of present experimental knowledge, all charges are multiples of an elementary charge. Hence **Gauss' integral of electrostatics is seen to count net charge.** Counting should not depend on whether we have or whether we use units of length and time. Suddenly old work on metric-free physics does not look all that silly.

Here we see the importance of not going into technical details, because had we done so, some finer points would have been totally missed. Hundreds of textbooks have discussed Gauss' law of electrostatics, yet none call on metric-independent features. All of which goes to show how effectively the mathematical techniques used in physics, throughout the past century, have made those nonmetric virtues invisible.

There is another intriguing aspect associated with metric-free laws of physics. To the best of presently available knowledge, metric references are the only ones that can tell us whether physical objects are large or small in terms of human judgment. So what is a possible implication if certain physical laws are found to be metric-independent? The answer is that there is a good chance that such laws are applicable in the macro- as well as in the micro-domain. Please note, it says: "There is a good chance! It does not necessarily follow." Yet, by the same token, it is hard to find a metric-free law that applies in the macro- and not in the micro-domain.

Ever since the birth of quantum mechanics, it has been hammered into the heads of impressionable young students that the laws of physics in the large cannot be simply extrapolated to the micro-domain. Large scale Newtonian mechanics had to make place for quantum mechanics. The latter is taken to assume special validity in the atomic and subatomic realm. The question is now whether these words of wisdom of our old quantum teachers are really compatible with the earlier observations made about Gauss' laws of electrostatics. This metric-independent law has been found to be a counter of microscopic net charge quanta!

Contemporary quantum teaching seemingly implies that laws about "charge quanta" may have micro- and macro-applicability, yet it is not so sure about laws invoking "action quanta." Here is a lack of equal treatment, perhaps because Schroedinger's equation looks so different from Newton's equations; in fact, it does not even come close to the Hamilton-Jacobi equation. The latter can claim an equivalence with the Newton equations of motion. We do know, however, Schroedinger did use the Hamilton-Jacobi equation to obtain his wave equation.

Fortunately there is a structural equivalent of the Gauss integral, it is known as the Aharonov-Bohm integral. The latter counts flux units h/2e. Also this quanta counter can be shown to be metric-independent; how else could it have become a quanta counter? Under the heading ***empiricism***, it was mentioned that these flux units were observed by Doll and Fairbank and their fellow workers. The action quantum ±h appears here jointly with the charge quantum ±e. In the meantime, numerous experiments have become known that confirm the existence of these quanta. In fact, it was mentioned that, together, flux quanta and quantum Hall effect impedance quanta have yielded the most accurate data (9 decimal places) of e and h presently available. These are facts of observation that should be weighed when assessing fundamental aspects.

In the course of a nontechnical exposition, as promised earlier, it became necessary to develop a critical eye for details of the sundry technical potential associated with different procedures available. This puts us in the seemingly awkward position of assessing technical potential in a nontechnical way. A simple example shows that one does not have to be a specialist in all the competing techniques to decide which one is better and which is the best.

For instance, when buying an automobile it is possible to compare car performance without getting into detail how one make succeeds in burning less fuel than the other, while going the same speed over the same distance. The choice would not be difficult if the vehicles are of comparable size and durability.

Students of electrodynamics are in a similar position when making decisions how to study the subject. Say, one formalism gives you an overview of a major part of the discipline, but another formalism gives a wider and encompassing view. You might opt for the wider view, even if it is more difficult, because it avoids greater difficulties later on in the game.

We can now become specific about the choice in question. It is the choice between learning the mathematical discipline of vector analysis, which was developed in the second part of the 19th Century as a shorthand compromise for physicists, versus the method of differential forms such as developed by Cartan in the Twenties. Yet, to bring an added perspectives in focus, it will be necessary to cover part of what is known as "de Rham cohomology." The latter is a branch of topology developed during the Thirties in which cyclic integrals are used to probe topological structure. The second choice invokes more mathematics, yet, unlike vector analysis, these new Cartan-de Rham skills are not restricted by compromise.

In the past, vector analysis used to be taught by mathematicians who mostly had a feel for physics. Today, vector-analysis is mostly taught by physicists as part of a physics course. Few physics lecturers are able or inclined to teach physics using methods of differential forms. The simple excuse for not taking such an initiative is that a majority of students and colleagues would have no understanding of what would be going on. All the journal articles appear in vector-analytic garb.

Any prospective student, convinced that the wider perspective of the form language is bound to pay off in the long run, will have to shop around at mathematics departments if any such course is available. Several mathematics departments may be able to offer such possibilities, but without much of a chance that such lectures are going to accommodate a commensurately adapted physics point of view. An effective adaptation of the material to the needs of physics, however, is a sine qua non. Exaggerated specialism in mathematics has created reluctance to take on such involvement.

De Rham's original work reveals much inducement from concepts of electrodynamics. Unfortunately this tradition has not been continued. For instance, for the purposes of physics, de Rham made a very essential distinction between two different types of differential forms: those that change sign under inversion, and those that don't. This feature is essential to deal with the differences between magnetic fields and fields of electric polarity. Mathematical follow-ups of de Rham Cohomology have abandoned this distinction. Modern mathematics texts restrict their forms to those that don't change sign under inversion. So here again a mutilated version is presented as a mathematical aid to physicists. All of which goes to show how mathematics is guilty of omissions comparable to those in physics. The moral of the story: not the discipline is at fault, but rather petty chauvinist specialism within the disciplines.

At first sight, all of this may sound discouraging to those who seek enlightenment in these matters. On the other hand, it also means there is work to be done and chances are that **the wider panorama of a quanta counting formalism** will be a rewarding extension of the traditional teachings of Maxwell theory. Therefore, do not despair. Just keep in mind how Gauss' traditional theorem of electrostatics already reflects all the principal ingredients of cohomology procedures; de Rham's generalizations of that process will then come quite naturally.

Right from the beginning, it seems, Gauss sensed the full potential of what his theorem could do. The closed-surface integral assumed the role of a *sensor* that can tell what is inside the surface. After the discovery of the discrete unit of elementary charge, this integral became a counter of net elementary charge.

Now knowing that Gauss' theorem is just a special case of the generalized Stokes theorem, one may now expect generalizations of Gauss' law of electrostatics in the following manner: Since Gauss' law of electrostatics pertains to a 2-dimensional closed (*i.e.*, cyclic) integral, the question arises whether there are 1- and 3-dimensional cyclic integrals that have a bearing on physics? The answer is: yes, there are!

The 1-dimensional cyclic integral is known as the Aharonov-Bohm integral, it is known to count quantum units of flux h/2e. A 3-dimensional companion has been suggested by R M Kiehn as a product structure of the previous two; it counts units of action h. Action can manifest itself as angle integrated spin or angular momentum along with the Bohr-Sommerfeld integrals. These integrals are called period- or residue-integrals (*e.g.*, Complex Analysis).

Similarly as the Cauchy theorem of complex analysis shows how the analytic domains of a function can tell what kind of nonanalytic domains are inside, the here cited three residue integrals in physical spacetime reveal what kind of quantum residues are inside. Here they are once again:

I Aharonov-Bohm's 1-dimensional cyclic integral counts residues that are flux quanta h/2e.

II Gauss-Ampère 2-dimensional cyclic integral counts net charge quanta ±e, either at rest or in synchronous motion.

III Kiehn's 3-dimensional "cyclic" integral counts units of action ±h that can be perceived as angle-integrated spin, angular momentum or a cyclic time-integrated energy.

Since the here delineated matters call for incisive changes in existing customs, an alternate exposition of these subjects is presented in the last chapter for the purpose of getting an historic mathematical perspective on these topics.

During the time when Maxwell codified what is now generally known as Maxwell theory, Cauchy and Navier-Stokes gave finishing touches to continuum mechanics. The laws of mechanics had been successfully reduced to the art of solving differential equations. The available laws of electrodynamics, by contrast, favored an integral rather than a differential rendition. Yet, the experience of the day resided in the realm of differential equations, so work got underway to translate the integral relations in a set of differential equations. The names of Laplace (1749-1827) and Poisson (1781-1840) come to mind, because each had a differential equation named after him that seemed to fit in a more extensive plan.

It was Maxwell who gave the complete transcription of the global integral laws of electrodynamics in differential form. In the process of doing so, he had to add a term to secure mutual consistency. This term is known as the "Maxwell displacement current" and the final equations, eight in total, are now known as the Maxwell equations.

Maxwell's work resulted in one of the most perfect phenomenologies of physics. Through the years, the theory did not really need corrections nor additions. In fact through the years, people discovered new facets of the theory. Right from the start Maxwell theory complied with requirements of relativity. Maxwell had used the Stokes and Gauss laws to create his eight differential equations. The knowledge gained in the 18th Century in solving differential equations could now be used to full advantage.

All of this shows how Maxwell gained information by working from the global integral point of view towards the local differential point of view. The local approach led to the discovery of the immense spectrum of electromagnetic radiation from radio waves to X-rays. At the time, the frame of mind was solely focused on local pursuits. The results achieved by this most perfect theory of physics were enormous. The development of vector analysis had brought this part of the theory at the finger tips of physicist and engineer both.

The global emipiric law statements that had spawned all this local beauty of Maxwell theory would temporarily recede in the background. Even if global applications of electrodynamics were amply supported through their usefulness in the design of electric machinery, the last century gave no inkling that an integral superstructure could be of relevance to quantum theory. There were at the time indications that gave evidence of the existence of a charge quantum, yet that was not seen as sufficient reason to elevate Gauss' law of electrostatics to the level of a quantum law.

A mixture of theoretical and experimental evidence then focused attention on the one-dimensional integral of the vector potential. First London in the Thirties and subsequently Aharonov-Bohm in 1964 showed that said integral could sense magnetic flux going through the interior of its integration loop; say similarly as the Ampère law senses current going through its integration loop.

Aharonov and Bohm effectively demonstrated experimentally the residue (period) status of the loop integral of the vector potential, it is now commonly referred to as Aharonov-Bohm integral. While Aharonov-Bohm focused on electron beam phase changes brought about by intensity changes in a linked flux loop, London, as early as the Thirties, suggested the possible existence of a flux quanta.

Actual flux quanta were first observed in 1961 by Fairbank *et al* and Doll *et al.* They turned out to be half the size of London's prediction. The discrepancy with London's prediction was later traced back to the electron pairing in superconducting samples used for their experiments. A year later, the Josephson ac effect reaffirmed its close relation to flux quantization.

So, all in all, the possibility of a quantum superstructure of standard Maxwell theory was gaining ground. This superstructure counts flux units, charge units and action units. The counting feature requires these integrals to be completely frame- and metric independent; in the jargon of mathematics it is said they are metric-independent, Diffeo-4 scalar- or pseudo scalar-valued integrals. The latter property is in perfect harmony with the metric-free general invariance of the Maxwell equation discovered by Kottler, Cartan and van Dantzig (KCD) in the Twenties and the Thirties.

The vector analytic formalism, as presently used, can neither be adapted to requirements of general invariance nor made capable of dealing with pseudo scalars changing sign under orientation changes of the frame of reference. An ad hoc dealing with all these features, which have now acquired very specific physical meaning, has been proven too tedious and cumbersome.

Therefore once more, a plea is in order to change from vector-analysis to a de Rham style differential form language as the appropriate vehicle of mathematical communication in physics. In order to keep available a need for local frame-related work, a form of tensor analysis is essential that permits a one-one correspondence with differential forms as defined by de Rham. Presently only the Schouten version of tensor analysis appears capable of providing a tensor equivalent of de Rham's pair and impair differential forms.

Classification of Crystals, the Point Groups

Under the heading of empiricism it was noted that regularities of crystal structures had not escaped the attention of early observers. The singular occurrences of 1-,2-,3-,4-, and 6-fold symmetry axes, and no others, gave rise to a suspicion of space-filling options with different types of molecules. The number 1 here is added as the identity operation for the purpose of mathematical completeness.

In the course of time, people wondered in how many different ways 1-,2-,3-,4- and 6-fold axes can be combined in space so as to exhibit the group property of *closure.* The word symmetry implies that the object under consideration physically appears the same after a symmetry operation. So. it should be the same after any sequence of symmetry operations for which it individually remains unaltered. This property is called the *closure* of a combination of symmetry elements and the totality of all symmetry elements that can be so generated is called a symmetry group. This group is a uniquely determining characterization of the crystal class.

The next question is how many of those closed (group) combinations can be constructed out of he basic set 1-,2-,3-,4-,6-. The answer is that there are 11 such groups. First there are the five isolated cases 1,2,3,4,6. Next four of those can be combined with 2-fold axes at right angles, thus accounting for a total of nine distinct symmetry groups. The two remaining are the tetrahedral and octahedral groups, which permit new spatially oriented arrangements of symmetry axes; *i.e.,* angles _ 1/2.

The 11 distinct pure rotation groups combined with the inversion group creates another set of 11 groups of crystals that have a center of symmetry. Finally 10 crystal groups have been counted that have mirror symmetry combined with rotation symmetry but no inversion symmetry. So the total of crystal symmetry classes is found to be 11+11+10=32 crystal groups consisting of rotation, inversion and mirror symmetry elements. All crystals can indeed be placed in a one of these 32 enumerated mathematical possibilities. It has also been found that crystal classes that have pure rotation symmetry elements (*i.e.,* neither mirror nor inversion symmetry) can occur, and mostly do occur, in two modifications that are mirror images of one another. This phenomenon is called **enantiomorphism**. Through enantiomorphism Nature uses up options of symmetry manifestation. Nature also has a preference for groups with many, rather than few symmetry elements. Triclinic crystallizations seem relatively rare.

In the earlier discussions of phenomenology, a major item of concern surfaced in relation to the question whether the compromise vehicle of mathematical communication, called "vector-analysis", will be adequate to meet future demands. It is perhaps not a coincidence that the beautiful phenomenology of crystal symmetry is, for all practical purposes, absent in contemporary physics textbooks. Of course, textbooks on crystallography and crystal physics can not avoid these issues; they have to make ad hoc adaptations. In the long run such stop gap methods can be expected to have their limitations, raising again questions about a transition to a methodology of differential forms accommodating inversions and mirror operations.

The differential-form pilot-projects, that did occur, have consistently failed to discuss matters of metric-independence and orientability. So, they cannot accommodate enantiomorphism and options related to the counting laws of modern physics. Bad habits of the past are blocking a complete transition.

For purposes of local work, as frequently required in technology, transitions to coordinate language are mandatory. Whatever is available in that respect is, of course, laced with the bad identification habits of the past. To avoid falling back into old identification habits, tensor procedures, called upon in making transitions, should remain equivalent to the (Cartan)-de Rham language (see Schouten)

Crystal Physics

Most physical media are assumed to have properties that are the same for all directions. As an electromagnetic medium, free space manifests the best-known approximation to such properties. Similarly air, gases and liquids are assumed to have propagation properties of sound that don't depend on direction. Physical media of this kind are said to be isotropic. In mathematical language it means, the properties of the medium remain unchanged (invariant) under the group of rotations.

As early as in the days of Huygens, it was observed that certain crystals can have properties that cannot be produced by isotropic media. A famous example is Icelandic Spar. A single ray of light does not stay single after refraction; the single ray splits into two light rays, each with their own index of refraction. The medium is said to be double-refractive. Huygens correctly inferred that the two resulting light rays had different velocities of propagation.

The example of double refraction shows that crystals can manifest a physical behavior that is more complicated than that of isotropic media. Since a crystal exhibits a symmetry that is reduced with respect to that of an isotropic medium, the manifestation of double refraction seemed related to the reduced symmetry of the Icelandic Spar crystal with respect to an isotropic medium.* Pierre Curie (1859-1906) used to say: "it is the asymmetry that creates the effect."

So the next question was: What other kind of effects can be expected that might exist in some crystals and not in isotropic media. The most familiar effect with which the world became acquainted through crystals is probably the piezo-electric effect. This effect is absent in isotropic media. In fact, piezo-electricity cannot exist in media that have a center of symmetry. As the name says, if you squeeze certain crystals, they develop an electric field, and conversely, if you submit them to an electric field, the crystal undergoes an elastic deformation. Keep in mind that the intensity of this effect is highly direction dependent.

Quartz crystal elements are today the piezo-electric heart of modern clocks and watches. These crystal elements are made to vibrate at very high frequencies with the help of an electric driving circuit. Electrical circuitry then divides the

* In mathematical language the crystal group is always a subgroup of the isotropy group of all proper and improper rotations.

vibration rate into seconds, minutes, and hours. These elements are cut from quartz crystals in such a manner that their frequency of vibration is almost independent of the ambient temperature. So far it has been impossible to make electric resonance systems of comparable stability, temperature independence and with the small size of a crystal element.

There are many other effects occur in crystals which cannot occur in isotropic media. Once the relation between effect and symmetry (Curie) is established, the next question is: How can we predict in a systematic manner which crystal exhibits what effects? This monumental task was performed, almost in its entirety, by Waldemar Voigt. To do this, he had to establish carefully the transformation properties of the arrays of coefficients that describe the effects for which one is looking. In mathematics, these arrays of coefficients are known as tensors. The procedure for Crystal Physics is then the following:

The **effect tensors** are subjected to the symmetry operations permitted by a given crystal symmetry group to find out what arrays of coefficients remains invariant under those operations. If no coefficients are left, the effect cannot exist in a crystal of that symmetry. If some coefficients remain, then the remaining array is said to be invariant under the operation of that crystal group. The effect can now exist according to that reduced array.

Voigt performed the enormous task of checking many conceivable crystal effects for the 32 crystal classes. In addition, he measured the coefficients of many of the effects that were so investigated for a considerable number of substances.

To go about the task of establishing the reduced tensor arrays, Voigt had to make many mathematical adaptations to undo vector identifications commonly made in physics. These adaptations were absolutely mandatory, because standard traditions leave behavior under inversions unspecified. In crystal physics, inversions become distinguishing operations describing medium properties.

Since here-mentioned matters tie-in with presently neglected distinctions between pair and impair differential forms, sooner or later (perhaps better sooner) decisions will have to become necessary to drop the old method of *vector analysis, which has a built-in feature of ignoring inversions.* Compare hereto what has been said in previous sections.

One of the reasons why standard physics texts don't discuss the 32 crystal classes and their consequences for crystal physics is undoubtedly the inadequacy of the standard methods of mathematical communication in contemporary physics. The specialists, *i.e.,* those directly concerned with crystal physics, know how to accommodate these inadequacies by appropriate specifications. So, it can be done! Yet, special needs of crystal physics are deemed insufficiently pressing to be made part and parcel of everyday physics.

Previous sections have mentioned how very fundamental issues about local and global are also involved in the methods of mathematical communication. All these things together make it desirable to incorporate these distinctions as permanent features of physics description. In teaching new generations, it will, in the long run, be too cumbersome postponing those essentials for later specialism.

Voigt's procedures are brought in alignment with the general procedures of a universal tensor calculus in the earlier mentioned Schouten text (see notes & refs.). Hence well tested procedures are available, it may well be a matter of departmental policy decisions whether these things become standard curriculum items.

Cause and Effect Relations

Crystal physics places an unusual focus on the nature of physical interactions in different types of physical media. The interactions, whatever their nature, when regarded from a spatially local point of view, are taken to be instantaneous. This assumption is no longer true if internal mechanisms in the medium, whatever their nature, are causing a time delay of response: *i.e., nonlocal* behavior in time. Such delays clearly happen if the causing disturbance has to travel over some distance in space. Yet, over and above, it is the internal mechanisms in the medium that plays a role.

Causality simply demands that the response cannot precede the onset of the initiating disturbance. How would one cast this causality condition in a mathematical form valid for all passively responding media, independent of the shape of the effect-initiating disturbance?

Kramers, taking a rest from the *ontics* of early quantum mechanics, took on this task of pure phenomenology in the mid-twenties. He found that the modification of phase and amplitude of any response system has to obey a set of integral transforms. At about the same time, Kronig and later Bode in communication engineering, arrived at a similar conclusion on the basis of analyticity considerations, without Kramers' explicit appeal to causality. The integral transforms now known as the K-K relations have found wide applications ranging from high energy particle physics to communication engineering.

Since matters of cause and effect relate to very basic human experiences, one would have thought that causality might have played an earlier role in the general description of physics; say in the 18th Century, which saw so many highlights of phenomenology. An understanding of this apparent omission of the classic workers goes back to the strictly local nature of early physical descriptions. In fact, this preoccupation with local methodology even extends into the contemporary realm. It was given to Kramers to assume a global point of view in he time domain, and in doing so he overcame the barrier of identifying implications of causality for modern physics.

If the 19th Century was the great era of phenomenology, the 20th Century may well be called the century of ontologies. Some of the ontological victories already started in the last part of the 19th Century. So let us summarize some of the happenings, it makes it easier to retain an overview of what has been taking place.

The emergence of the kinetic theory of gases pioneered by Maxwell and Boltzmann showed what the molecular hypothesis could do in understanding the gas laws. Van der Waals added hypothetical extensions by assuming a finite molecular volume plus a general volume-based interaction between molecules. The resulting van der Waals equation of state would play a key role in the subsequent experimental programs that led to the liquification of gases.

Statistical mechanics greatly helped a deeper understanding of thermodynamics. The work of Gibbs solidified the conviction that the second law of thermodynamics reflected a statistical behavior of nature. Boltzmann further helped these insights by showing how molecular collisions could assume the role of a mechanism that would contribute to entropy increase.

The emergence of the quantum at the turn of the century would lead to a string of ontic propositions that interestingly were mostly related to prescriptions of how to work with it; its nature was regarded as something weird and off limits. The idea of first trying to know how to work with quanta is well taken. Philosophizing about its nature was regarded as premature, at least for the time being. From that time onwards, physicists became hard-nosed ontologists. Their motto: Tell me how it works and what it does and don't give me philosophy. I don't care where it comes from, *i.e.*, for the time being at least!

All of this shows how the realm of ontologies can cover the whole gamut from hard-nosed pragmatism to over-idealistic romantic idealism. Quantum mechanics has gone overboard in either direction. The Copenhagen interpretation represents a good deal of idealistic belief that quantum mechanics, as presently known, is nature's last word to man. Others, on the other hand, can be heard voicing opinions that none of that interpretive stuff is essential to successfully use those tools.

From Democritus' Atoms to Dalton's and Avogadro's New Chemistry

One of the most striking ontic inventions of antiquity was certainly the idea of atoms by Democritus (460-370 BC?). Rarely had a good idea to wait this long to witness a measure of verification. The founders of the new chemistry Dalton and Avogadro substantiated the reality of the concepts of atoms and molecules around the time of the French revolution. Atoms and molecules, so to say, became a work hypothesis that did very well, although nobody had seen atoms or molecules. For some people the edifice of chemistry was enough to believe in their reality. The kinetic theory of gases and the Einstein theory of Brownian motion then made these concepts from a physical point of view even more realistic.

Yet, notwithstanding these triumphs of chemistry, even at the beginning of the 20th Century there were still physicists who said they were not at all convinced about the reality of atoms and molecules. Ernst Mach supposedly was one of the nonbelievers. If we realize that Boltzmann was Mach's successor at the University of Vienna, one wonders whether Mach was just doing his best to give poor Boltzmann a hard time. Or perhaps, mindful that adversity might breed stronger conviction, Mach was just trying to bring out the best in Boltzmann.*

The depicted situation casts an unusual light on the personalities of Mach and Boltzmann. It illustrates epitome attitudes in phenomenology and ontology. Boltzmann's thermodynamic derivation of the Stefan law of radiation shows him as an ontologist adept in phenomenology. Mach may have been more of a single-minded phenomenologist. Einstein much admired Mach's consistency in these matters, yet his work on Brownian motion showed him to be an accomplished ontologist as well.

Mach supposedly was not comfortable with the direction in which Einstein had taken some of his thinking. If these human interactions make us aware how important people have been battling with fundamental ideas, it may help as a reminder never to go blind by siding up with one "ism." Finding niches for "isms" is what is being attempted here. Do not despair if your particular "ism" has not been mentioned here; there may always be a place for another little niche, provided the vehicle of communication is not too restrictive.

An Avalanche of Quantum Propositions

These are just reminders that unlike phenomenology, the world of ontology is full of controversy. Nowhere has that been more conspicuous than in the realm of the theory of quanta. The action quantum was originally introduced by Planck in 1900 as a necessary concession to common sense; a need to interpolate between the low- and the high-temperature end of the spectral distribution of the black body radiation.

In the beginning, Planck's proposition was most likely received as an ad hoc thing that just happened to lead to the right result. From an angle of philosophy one might argue good ontology, but perhaps at that time without too much of a promise for a successful tie-in with epistemics.

Then five years later, Einstein identified the essential role of the same quantum of action in the photo-electric effect. The very sharp and characteristic wavelength threshold for releasing electrons out of metals revealed the action quantum as more than just an ad hoc proposition.

After the photo-electric effect, the quantum of action had become respectful. It would take another half decade before Bohr was to make the proposition that the angular momentum of orbitally circulating electrons should equal a multiple of h. On the basis of that proposition Bohr was able to calculate the stationary states for hydrogen. The spectral observation of hydrogen again confirmed the same value of h as earlier obtained through the photo-electric effect and black-body radiation. Physics was now homing on a real universal constant that was anything but ad hoc, even if it had been discovered in a slightly ad hoc fashion.

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* An assesment of this controversy by L Tisza is forthcoming.

At this point there seemingly were two disconnected applications of the action quantum: the *energy* quantum played a role in the black body radiation and in the photo-electric effect, versus the *angular momentum* application in the Bohr atom. Physics was here confronted with two separate and seemingly independent ontic propositions. A next train of thoughts explored inquiries as to whether these two ontic propositions could be somehow united into a more encompassing principle.

A possible solution to this question was anticipated by Sommerfeld (1868-1951) and Wilson. They proposed in 1915 a royal road to quanta as Sommerfeld called it. The action integrals of analytic dynamics might only assume values that were multiples of the action quantum, instead of the arbitrary values permitted under the rules of standard classical mechanics. It appeared a smooth and most inconspicuous transition to incorporate quantum features. In fact, in the perspective of relativity, the Bohr-Sommerfeld relations, as they are now known, assumed the form of cyclic spacetime integrals of the four-vector known as energy-momentum; cyclic in time relates here to a periodicity of the system under consideration, cyclic in space to an orbital periodicity.

This new encompassing quantum formulation was successfully applied by Schwarzschild (1873-1916) and Epstein to a splitting of spectral lines by electric fields discovered by Stark (1874-1957). Sommerfeld himself showed that an application of the B-S relations to hydrogen, taking into account relativity effects, would account for an indeed observed fine structure of Hydrogen and ionic Helium. In a now mostly forgotten paper, Einstein showed the following year that the Bohr-Sommerfeld relations had a topological connotation for the orbitals that were being selected by these conditions. So towards the end of the war-torn Tens, the description of quantum phenomena seemed in good shape. This peace of mind was not meant to last for a long time.

Applications to the Stark effect were followed by applications to the Zeeman effect. There were no problems accounting for the normal Zeeman effect, as Lorentz had already done by a different line of reasoning. However, the anomalous Zeeman effect remained a challenge for any of the approaches that were around at that time. Efforts to disentangle spectral multiplet structures, as exhibited by more complicated atoms, also yielded marginal results. The new theoretical extensions of standard mechanics had done nice things, it was, however, by no means a last word in a new atomic mechanics. Mindful that many body problems had been haunting Newtonian mechanics for a long time, there was no reason to assume that atomic analogues of the planetary situations would be any easier to solve.

In the beginning of the Twenties, some differences surfaced that did not quite have an analogue in terms of celestial mechanics. Unlike planetary bodies, the bodies of atomic mechanics were electrically charged and their orbital motion had to be expected to generate magnetic fields. Compton in 1921 and Kronig*

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* At the time Kronig worked with Pauli in Vienna, his spin proposition was less than enthusiastically received by Pauli, whereas Uhlenbeck and Goudsmit received positive support from

experimented with the idea that electrons might have a spin and a magnetic moment of their own. In 1925 Uhlenbeck and Goudsmit brought this spin hypothesis in a form that was accepted as a possibility for sorting out the anomalous Zeeman effect.

In the same year Heisenberg proposed a new basic quantum procedure and in 1926 Schroedinger made a similar proposal that in retrospect turned out to be equivalent to the Heisenberg proposal. Yet neither one dealt with a possible electron spin. Then in 1927 Dirac reconciled in his way the Schroedinger approach with principles of relativity and behold the solutions confirmed the electron spin propositions of Uhlenbeck and Goudsmit. The Twenties were just loaded with ontic propositions. It was a slight abuse of language, when Einstein called it an epistemological orgy; today, we would say an ontological orgy. The latter is in better accord with definitions given by Bunge and Zerin cited in the preamble.

This was not all. More things had happened between 1920 and 1925 that had helped trigger the work of Schroedinger. In 1923 de Broglie and Duane came up, more or less simultaneously, with a proposition that was very closely related to de Bohr-Sommerfeld relations. In fact Duane sort of derived it from the B-S relations and de Broglie postulated the relation as an extension of the original Planck postulate, as suggested by relativity. De Broglie's relation is, in fact, a proportionality between two 4-vectors: *i.e.*, the energy-momentum 4-vector and the frequency-wave 4-vector with the quantum of action ¡ as the constant of proportionality. This relation was to have a major role in implementing the Schroedinger wave equation; the latter and matrix mechanics set off the second quantum revolution.

However, before getting involved in the 1925 quantum revolution, let us first discuss what seemed to be a last triumph of the Bohr-Sommerfeld relations. Duane was concerned about the particle-like behavior of X-rays as registered by Geiger-Müller counters. He, therefore, wondered whether a wave description of X-rays, as used by father and son Bragg, might really be appropriate for hard X-rays. He made an attempt at a particle description using Bohr-Sommerfeld conditions to calculate a discrete momentum exchange between particle and crystal lattice. By identifying his particle diffraction, so calculated, with the wave diffraction of Bragg, Duane obtained the same relation between momentum and wavelength as de Broglie. Even if Duane, in principle, did his analysis for photons, there is, in retrospect, no difficulty extending the same to massive objects, slower than the speed of light. One thus confirms de Broglie's proposition as a Bohr-Sommerfeld contingency.

The Duane argument is now all but forgotten, which is somewhat sad and inconsistent. If we realize that the photo-electric effect cannot be argued without being explicit about a discrete exchange of energy; *so why not a discrete exchange of momentum?*

Ehrenfest in Leiden. When, many years later, Pauli was interviewed by Uhlenbeck about his lack of support for Kronig, Pauli answered: "I was so dumb when I was young."

Then, in the middle of this bewildering avalanche of ontic propositions, a new ontic proposition emerged, it is known as the Schroedinger equation. This equation, and Dirac's relativity counterpart, became to be regarded as the basis for the mid-Twenties quantum revolution. They were presumed to supersede all earlier ontic propositions. In fact, some of the earlier ontics was conveniently placed in a category of approximations. Such was the case for the Bohr-Sommerfeld conditions, which had rendered such good services just before the anomalous Zeeman effect came around to prove their limitations. Even Sommerfeld himself, an originator of a royal road to quantization, became an ardent supporter of the wave equation cult now emanating from Copenhagen.

It will take some philosophical calm to create order in this overwhelming array of ontic material. It gives a feeling of discomfort if one ontic recipe is prematurely lifted above others, before it even had been established as to what the old recipe meant. Now, after three quarter century, the Copenhagen view of Schroedinger's equation is still cause of dispute and disagreement.

The never ending objections and alternate propositions, generated by Copenhagen, testify that physics is still faced with a situation swamped by too much ontic diversity. There is presently not enough of an epistemic lead to make reliable decisions. Yet, many of these ontic propositions hold sufficient promise of truth to justify more epistemic scrutiny.

To create some order in this over-abundance of well-intended creative suggestions, it pays to look for a common denominator that almost all propositions have in common. It appears that past interpretations of the 1925 quantum mechanics have been focusing on primarily one picture of physical reality: *i.e.*, **the single system,** whereas a very small minority has been favoring **ensembles of systems**.

These two options have been silently coexisting for sixty years. Neither one explicitly excludes the other as inadmissible; it has been mostly "either one or the other" and sometimes "one and the other." This unresolved situation deserves further examination.

Ensemble Implications of Schroedinger's Recipe

Since the Schroedinger equation became one of the most frequently used tools of physics, it makes Schroedinger's rationale for obtaining this result one of the most famous ontic recipes of physics. The factual ingredients that went into this recipe were de Broglie's proportionality of energy-momentum and frequency-wave vectors* and the Hamilton-Jacobi equation of particle dynamics.

The dependent variable S of the Hamilton-Jacobi equation is an action function that relates to the phase of something that has the mathematical appearance of a wave, say, of amplitude ×. The ensuing logarithmic relation between S and × is used to change the dependent variable from S to ×. The

* Without an intended effort on their part, de Broglie's relation was strikingly confirmed by the Davisson-Germer experiment of 1927.

resulting expression is taken as a Lagrangean integrand of a Hamilton principle. Extremizing this integral yields as condition for optimization the "Euler-Lagrangean differential equation." This Euler-Lagrangean became one of the most effective quantum tools of the century; it happens to be the Schroedinger equation!

For conservative systems, its application in physics becomes an *eigen*value equation. Schroedinger made the solutions subject to conditions of single valuedness and square integrability, which can only be met for discrete energy states (eigenvalues) and corresponding eigen functions. Results of the first applications mostly coincided with earlier Bohr-Sommerfeld work, yet there were some remarkable differences.

For the harmonic oscillator, a so-called zero-point energy had to be added to the original Planck energy states. Yet, unbeknown to many authors of contemporary quantum texts, a concept of zero-point energy had been introduced by Planck in 1912. Planck, at that time, had shown how **an ensemble of harmonic oscillators has to retain a zero-point energy if it is to retain a state of phase randomness.**

For quantum rotators, Schroedinger's equation brought another change in quantum number. The Bohr number n appeared replaced by $\sqrt{n(n+1)}$; the latter was found to fit better spectral results. In the early Sixties, Kompaneyets reported about a calculation that shows how starting from quantum states 1,2,3,...n, a randomly oriented ensemble of rotators ends up with a statistically averaged quantum number $\sqrt{n(n+1)}$ for the modulus of angular momentum. The Feynman Lectures later presented the same ensemble-based derivation of the angular momentum quantum number. Since the Schroedinger equation full-automatically produces this statistical result, *it should have generated some suspicion that the equation launched by Schroedinger might possibly be an ensemble tool.*

The two examples of zero-point energy and angular momentum quantum number together present compelling evidence of Schroedinger's equation describing ensembles that are in appropriate random states. This state of affairs excludes Copenhagen's single-system interpretation, unless the ensemble is taken to be a Gibbs ensemble of conceivable manifestations of one and the same system. Neither Kompaneyets nor Feynman cite their ensemble averaging as contradicting Copenhagen's dictum of nonclassical statistics, nor do they cite a possible violation of Copenhagen single-system precepts. Hence only an abstract Gibbs ensemble can be a saving grace for continued support of Copenhagen single-system now obeying a **classical statistics**. Now we see how Copenhagen got the idea of the *fuzzy nonclassical orbits*.

Planck did his zero-point work well before the birth of the 1925 quantum revolution; he had no obligation to reconcile his work with Copenhagen's view of the Schroedinger equation. There is little doubt that Planck had in mind a real ensemble not a Gibbs abstraction. Neither Kompaneyets nor Feynman may have been aware of Planck's zero-point ensemble, because had they been aware it should have initiated right then and there a revolt against Copenhagen's single-system views and ensuing nonclassical madness.

The choice between abstract and real ensemble becomes once more a serious obstacle on the road to quantum electrodynamics. Neither Feynman and co-authors nor Kompaneyets have submitted the here cited predicament as an ontic alarm for theory development. So it must be assumed they approved of the abstract ensemble as representing a single-system. This reading of the results indeed gibes with Copenhagen's picture of blurred washed out orbits. Remember how in those early days, and presumably still today, we were told: it is a fallacy to think of true and precise orbits of atomic systems! Individual orbits are to be seen merely in terms of a probability presence.

The point is well taken; in a real ensemble, there is a perfectly **classical** orbital randomness of phase and orientation between the single-systems in the ensemble. ***Modern physics owes Planck an apology.***

Quantum Electro-Dynamics QED

After the early Thirties had proclaimed the Schroedinger and Dirac equations to be the exact tools for quantization of mechanical systems, the interest shifted to quantization of electrodynamic systems. The first developments of this discipline showed a structure plagued by infinities. This type of QED could not really be applied anywhere.

In the meantime, the development of microwave technology made it possible to refine the art of spectral observations. It was found that there were two electron levels in hydrogen that were supposed to be the same according to the Dirac theory, yet they did not quite coincide. The level difference could be measured with the help of micro-wave methods and it became known as the Lamb shift after its discoverer.

The Lamb shift was a little bit of a blow for the Dirac theory, which had until then had a rather sacrosanct reputation. It was felt that the deviation had something to do with hydrogen states of zero angular momentum. Subsequently a small anomaly of the electron's magnetic moment was suspected as another cause of deviations. It meant that the Dirac theory prediction of the electron's magnetic moment might not be exact. Independent spin flip measurements of electrons in strong magnetic field indeed confirmed the existence of an anomaly of about one in thousand. The anomalies for electron and muon were found to be almost the same but not quite. Again microwave measurements saved the day for measuring even such tiny differences in electron and muon anomalies.

The next question was whether the infinity-plagued discipline of QED could shed light on these experimental observations. Bethe first showed that a Lamb shift could be calculated by performing a balancing act with the infinities. Subsequently Feynman, Schwinger and Tomonaga reconstructed QED to deal with these new challenges. Schwinger first indicated a value for the magnetic moment anomaly, which initially appeared to be the same for electron and muon.

The fact that finite answers could be extracted from a discipline that from the onset had been beset with infinities was nothing less than an achievement of pure ontological magic. Yet, despite such heroic results, people want to know: **are infinities necessary?**

In retrospect, the infinities are a consequence of Copenhagen's single system premise, because it provides every individual harmonic oscillator in the vacuum of free-space with an ever present and indelible zero-point energy. The real physical ensemble of harmonic oscillators, as considered by Planck, has no trouble accommodating a zero-point energy as a statistical average of the the lowest ensemble energy state compatible with random phase between the oscillators. **So the choice of a Gibbs ensemble as an ensemble of states for one harmonic oscillator must be the culprit responsible for the QED infinities.** The simple reason is that every oscillator has been assigned a zero-point energy, which integrated over a free-space spectrum is divergent.

Brownian Motion

Faced with the spectacle of an extremely light particle bouncing around on the surface of a liquid, how would one build a theory about that? That is exactly what Einstein did early this century. It goes to show that his ontic talents matched his phenomenological talents called upon in developing relativity. The theory of Brownian motion is a striking example of penetrating into the micro-physical domain with the help of some straightforward ontic propositions concerning microscopic observations. While the molecule itself is too small to be seen, the small particle being bounced around can be seen. So, looking at displacements suffered by that visible particle through the bombardment in time by molecules, one assumes to have a measure for inferring something about the molecular constituents of the medium that is doing the bombarding.

The result of these considerations led to a conclusion that the square of the displacement average is proportional to the time of observation, the temperature as measured in degrees Kelvin, and inversely proportional to the fluid viscosity and the radius of the observed particle. Since particle radius, temperature, and viscosity are measurable, the factor of proportionality can be determined. This leads to information about Avogadro's number* and Boltzmann's constant. These results have been cross-checked against results from the kinetic gas theory; they have shown good compatibility. So the array of microscopic ontic propositions that go into this sort of theorizing seem reasonably well justified to give even Ernst Mach peace of mind about the existence of atoms and molecules.

Unlike his work on the general theory of relativity in which Einstein stood largely alone, work on Brownian motion, especially the experimental observations, has been pursued by several others. Smoluchowski pursued comparable objectives and arrived at similar results about Brownian motion.

* The number of molecules in a gram molecule of any substance.

Crystal Classification, The Space Groups

The symmetry properties earlier considered are merely rotations, inversions, and mirror operations that don't combine into inversions. The groups determined by this conglomerate of symmetry operations are called the point-groups of crystal symmetry. They are the result of a purely phenomenological development, directly deducible from observed symmetry elements. Although these symmetries are understood to be implied by the lattice structure of crystals, it is not absolutely necessary to make the lattice hypothesis.

Making a lattice hypothesis brings us into the realm of ontology. Let us just briefly ask the question how the lattice idea affects the earlier discussed 32 crystal groups. It means over and above the 32 point-groups, there are now also translation operations that transform the actual lattice into itself. In 1848 Bravais (1811-1863) concluded on a total of 14 basic lattice configurations, each with their own translation group. The next horrendous task was sorting out what different type of translation groups combine with point-groups to form new groups. In 1892, Schönflies (1852-1928) and von Fedorov enumerated the total of combinations of point and translation groups to be 230; they are called space-groups.

For many years now, X-ray examinations of crystals have gone hand-in-hand with the here-cited conclusions about crystal classification. There have been recent attempts at extending these classification schemes to include physical features related to magnetism. There remain questions, though, whether the ensuing extensions are as fundamental as the ones derived by Schönflies and Fedorov. The 32 point-groups and the 230 space-groups constitute what appear to be the first, and most successful, applications of the mathematical discipline of group theory to classification problems in physics.

If it had not been for the lattice hypothesis, the classification of crystals into point- and space-groups would have been a purely phenomenological achievement. So, formally speaking the crystal classification is an example of a successful ontology. Yet, its work hypothesis appeared to be so close to the reality of observation that it might by now have been argued as almost the equivalent of an empiric implication. In this respect the classification of crystals by Bravais, Schönflies and Fedorov could be regarded as a worthy conclusion of the century of phenomenology.

As classifications go, the crystal classification is up to this point probably one of the best physics has offered. It seems closer to physical reality than the group classification for elementary particles. The reasons are clear, in elementary particle theory we miss something as tangible and simple as the lattice hypothesis.

Remaining Ontologies

Shortly after Maxwell theory reached the world of physics, Lorentz saw a chance of capitalizing on the structural similarity of the Maxwell equations in free space and the equations in matter. Free- and bound electrons were found to be responsible for many of the electric properties of matter ranging from conduction to dielectric permittivity and magnetic permeability. In the course of time, this initial Lorentz theory of electrons went through many changes to accommodate findings in the quantum behavior of matter. The ensuing work has mushroomed under the heading of what is now called solid state physics.

Other developments, where the contact between ontics and epistemics still remains weak are the quarks of Quantum Chromo-Dynamics (QCD) and perhaps, in a much more distant way, Strings. These disciplines have become mixtures of empiricism, phenomenology, and ontology that are hard to disentangle. It shows when in despair, try everything. One may assume when the time has come, also here a measure of order is bound to prevail.(Compare next chapter)

EPISTEMOLOGY

The end of the preamble has a diagram of logic interconnections that are an attempt at imparting a strong suggestion that all of man's *deductive* and *inductive* efforts ideally should come together under the heading of epistemology. This has been posed as a conceivable ideal, as something for which one strives and hopes. Clearly, mathematics comes closer to that ideal than physics. Let us examine for either discipline in more detail the possibilities of living up to that goal.

The mathematics about which most physicists have knowledge comes about by a deductive process of reasoning. Yet, occasionally mathematics uses an inductive argument, but if that is done, it is preferably restricted to what is called *complete* induction. Without the completeness, the inductive process retains a question mark that mathematics can ill afford. So, in mathematics an inductive argument starts out with a conjecture, or hypothesis, but it remains on the intuitive level unless the conjecture can be proven. The completeness of induction is part of such proof.

In physics, much of the time we are inclined to be content with less. We must be, if we want to push forward. Moreover, whenever a conjecture becomes solid knowledge, meaning the conjecture is regarded as a truth statement, it is never with the degree of absoluteness that one likes to see in mathematics. In physics, we got accustomed to being satisfied with less. It is the very reason why some mathematicians see physics as an endeavor that is hazardous to their peace of mind.

There is some justification for this attitude. In the course of these discussions, several instances have been mentioned. Ironically a major example with which we have been concerned holds for both: *i.e.,* physics and mathematics. It is the neglect of the pair versus impair distinction of differential forms. Here physicists and mathematicians are both at fault for ignoring the pair-impair feature originally introduced by de Rham.

This tacit removal of an essential characteristic has interfered with several attempts at mathematical modernization in physics. It has reduced these efforts to a sort of lip-service to new principles, without really adapting those principles to the physical reality from which they came. Keep in mind that de Rham's work, in many ways, derives from a then already-existing description of electromagnetic fields. The pair-impair difference comes from basic distinctions between electricity and magnetism. It is the polarity of electric charge versus the absence of a similar polarity for a conceivable magnetic source. The next chapter argues why a so-called magnetic charge and the *observed* magnetic flux quantization are mutually exclusive!

The expectation and hope is that epistemology will be forthcoming in putting such things in perspective, so that mathematicians as well as physicists and philosophers can be pleased. Mindful that philosophy, as the wisdom of language, is engaged in defining and redefining words, we now need to evaluate or reevaluate what has been achieved at this point.

The new realms of physical experiences opened up by a sophisticated art of experimentation has been widening our empirical experiences. Those empirically well established new experiences have created new avenues for a deductively strong phenomenology. The latter, combined with a forever fertile ontic imagination of physicists, provides a sea of information from which high points can be lifted for value judgment.

Some Epistemic High Points

One of the high points of epistemic glory is undoubtedly Maxwell's synthesis of the laws of electrodynamics and the subsequent verification of electromagnetic waves by H. Hertz (1857-1894). Contingent on these discoveries was the extension of the electromagnetic spectrum reaching from radio waves, heat waves, infra red waves, light waves, all the way to ultraviolet waves, X-rays, and hard X-rays.

Prior to Maxwell, optics had been a successful isolated ontology operating on Huygens' premise of wave motion. At the time, radio waves and X-rays were still unknown and there was no clear realization whether heat and light waves could be possibly related.

The Planck theory of black-body radiation subsequently became a crowning glory of the electromagnetic spectral interpretation. Its integration over the whole spectrum from zero to infinity produced, as it should, not only the already known Stefan law of heat radiation but also the correct value for its constant of proportionality.

The Stefan law itself had already been made a subject of interesting theorizing by Boltzmann. Combining the Maxwell theory of radiation pressure with the laws of thermodynamics, he obtained the Stefan radiation law with a proportionality factor as an undetermined constant of integration. The latter was later correctly reduced to other physical constants through the integration of Planck's radiation law: *i.e.*, Planck constant, speed of light and Boltzmann's constant. The latter is a close relative of Avogadro's constant and the (ideal) gas constant.

It cannot be denied that the turn of the century from 19th to 20th was an era of unexpected enlightenment for the physical sciences. A multitude of previously disconnected or poorly connected realms of physics suddenly permitted a unified deductive picture. That is what epistemics is about.

The here-depicted situations show a beautiful blending of ontic and phenomenological principles. One could say pure phenomenology is almost epistemology. Yet if pure phenomenology cannot always exist in wider contexts, then, by the same token, pure epistemology is the thing to which to aspire. In the preceding discussions the word "epistemic" has, for that reason, been favored over the more pretentious "epistemology."

The cases here discussed have given insight in some unusual epistemic beauty, yet much is overshadowed by a strange duality, which, for many people, mars the ideal of what an epistemology should be. It is the duality of particle and wave.

The Epistemic Puzzle of Particle and Wave

The above picture seems contingent on an ontic proposition that light is a wave phenomenon. Many people may not be inclined to call this an ontic proposition, because diffraction and interference seem to leave no doubt about Huygens' wave proposition. Finally, Maxwell theory itself comes up quite naturally with wave equations for the propagation of electromagnetic disturbances. The cards seem stacked against Newton's original idea of light particles by favoring Huygens' position of waves. The evidence seems so absolutely overwhelming that many people are unwilling to consider alternatives.

The first experimental evidence pleading against the universal wave proposition was the photo-electric effect. Releasing photo electrons from a cathode appeared to be controlled by the wavelength of light rather than by intensity. Above a certain wavelength, no electrons could be released regardless of intensity. Einstein's explanation of this effect was based on a proposition that light could only give off energy parcels that were ruled by Planck's constant. For years, and still today, though, this feature may have disturbed the teachings of wave-monism. Yet, it has not discouraged the use of the wave paradigm, because the amount of evidence for which this works so well is simply too overwhelming.

In recent times, Tonomura *et al* have done experiments in which electron diffraction patterns are shown to come about by single electron incidences. Now, similarly, optical experiments have been done showing how light diffraction comes about as a result of single photon absorption events. Even before the Tonomura experiments, the interpreters of quantum mechanics have confronted this issue with the proposition of a particle-wave duality. This proposition is of the nature of a work recommendation by stipulating, depending on the problem at hand, either particle or wave picture applies, but never both at the same time. This Copenhagen work-suggestion had a temporary effectiveness in dealing with the photo-electric effect, yet Tonomura-type experiments are presenting a new confrontation challenging the tenability of this duality .

How do we explore a way out of this obvious predicament? First of all there is no such thing as a single wave. Every wave known to man consists of numerous Fourier components. These components have a rather abstract meaning. They have been invented by our mathematical brethren. Yet the argument suffices to convey an impression that the wave concept, known to us, has an inherent plurality connotation.

By contrast, there is such a thing as a single particle. It now follows that the Copenhagen wave-particle duality suffers from an inconsistency. It compares a single object with a plurality object. The presented predicament can at least be partially resolved by replacing the (single) particle-wave duality by a **many particle-wave duality**.

It shows the Copenhageners as over-preoccupied with single systems. Later, it will appear that the Heisenberg exchange forces reveal a rather schizophrenic feature of Schroedinger's "wave solution." They cover an interesting hybrid aspect of joint single system and ensemble behavior. So, Copenhageners were right, at least in part.

Assessing Phenomenological Pathways

A good phenomenology based on solid physical observation is the closest we can come to a good epistemology. In fact, such phenomenology may be said the practical equivalent of epistemology.

Taking a general look at the developments underlying the present state of knowledge about mechanics and electrodynamics, one immediately sees two diametrically opposed procedures.

The branches of mechanics have been developed from *the small* towards *the large*, or using another form of presently customary language it said: from *the local* towards *the global*. Anyway, that is how nature has revealed itself to us in the ream of mechanics. There are ab initio global law statements in the form of Hamilton's principle, yet the ingredients going into the Lagrangean of the Hamilton integral have been conceptually developed from the local point of view. We know building from the inward to the outward is no simple matter. It has been a matter of deep concern to Euler who did much of this work.

On the other hand, the fundamental laws of electrodynamics, as initially formulated by Faraday, Ampère and Kirchhoff, by contrast, were all observations tying in with global observations. That is how nature revealed itself to man in the realm of electrodynamics, or perhaps that is how man was able to see nature.

In electrodynamics, it was not so much a building inwards, but rather an exploring inwards. Who opened up the outward‘inward process in electrodynamics? Faraday's tubes and lines of force were instrumental in developing the field images of current electromagnetism. Maxwell's codification ties in consistently with the images developed by Faraday and the subsequent application of the Gauss-Stokes theorems. Let it be said that Maxwell was keenly aware of the importance of maintaining those Faraday distinctions.

What Maxwell did, mathematically speaking, was defining integrands for scalar- and pseudo-scalar-valued integrals. All this ties in directly with de Rham's definitions of pair and impair differential forms with properties of either *exactness* or *closedness.* De Rham theory makes the exploring inwards a better defined mathematical process, which in turn specifies its physical nature through the physical character of the defining scalar- and pseudo scalar valued-integrals. Note hereby that the physical dimensions of all the integrals occurring here are given by the units (e) of "electric charge" and units (h/e) of "flux". The latter, *i.e.,* flux, is measured by action (h) units over charge (e) units; ***all of these are good global spacetime general, metric-independent, invariants.*** The ensuing physical field dimensions are then uniquely determined through length (l) and time (t) as integration variables.*

Now compare this outward‘inward process of electrodynamics with the inward‘outward process used by Euler, when he created the branches of mechanics. Euler did not have available to him the equivalent of invariant integral expressions that are so very characteristic of electrodynamics. Regardless of whether or not a clean global‘local process here exists, the fact is: so far the empirics of mechanics has not revealed it to us. So, the procedure followed by Euler became one of smearing out point-masses leading to a definition of mass density plus velocity and acceleration fields for that mass density.

There is no denying that the outward‘inward process, Maxwell was able to use, is conceptually superior to the procedures that Euler had to follow. Euler was well aware of an unavoidable measure of conceptual looseness of his development of continuum mechanics.

Now let us see what Maxwell did with his translations of Faraday, Ampère and Kirchhoff in integral form. Mindful of the earlier d'Alembert and Bernouilli experiences in solving partial differential equations he proceeded to obtain differential equations. Using the Gauss and Stokes integral theorems he converted his global integral law statements into local statements. The results were the Maxwell equations and from the Maxwell equations and the constitutive properties of the medium under consideration he obtained d'Alembertians describing the wave propagating features of his fields. Keep in mind precious information can get lost in the conversion global‘local.

Yet, notwithstanding this loss of information, the development of Maxwell theory was spectacular, and above all it was rigorous compared to the earlier developments in continuum mechanics. Maxwell had lifted out the at-that-time still somewhat isolated observations of Faraday, Ampère and Kirchhoff and joined them into an encompassing structure that has been the standby for technology and physics for well over a century.

Well-meaning pedagogues, spreading the new gospel, soon began looking for a mathematically more perspicuous form of the theory. This led to a shorthand now known as *"vector analysis*."

Some of the original vector distinctions made by Faraday and Maxwell now became mostly invisible by the compromise of vector identification on which vector analysis is based. Yet the compromise worked wonders, many more people

* see notes and references, mathematical sources: texts by Schouten and Post.

were now able to take advantage of the new insights. At that time, the local insights were dominant and more important than the global aspects. The latter had not as yet received the needed observational evidence for which an integral point of view would assume a role of preeminence.

The quantum-counting properties of cyclic integrals now change all that. The Ampère-Gauss, the Aharonov-Bohm integrals, and the integral proposed by Kiehn testify to the existence of a quantum superstructure of standard Maxwell theory. Information lost in the global‘local reduction has so been retrieved and can now help us confront a new quantum reality of physics!

Two of these integrals have been around for a long time. The third is of more recent vintage in the sense that it has been around for two decades. Kiehn's integral was introduced as an action quanta counter with all the metric-independent, generally invariant features that go with counting. The Aharonov-Bohm integral acquired operational significance as a phase-shifter in the Sixties. Yet, in the Thirties it had already been identified as a potential flux quanta counter. Its capabilities for that function were confirmed in the early Sixties. The Gauss-Ampère integrals has been around longer than any of the others. Their combined specifics as legitimate charge quanta counters were emphasized and verified in the Eighties.

These are just reminders that there are three cyclic integrals of 1,2, and 3 dimensions that meet all the requirements of being able to function as counters of quanta of flux, charge and action. These integrals constitute a quantum superstructure of contemporary local Maxwell theory. Yet, to make these characteristics explicit, the vehicle of vector analysis is less than inviting, because it obscures the very features that are crucial to verify those characteristics.

Without a formalism that stresses the metric-independent features, it is difficult to appreciate macro- as well as micro-applicability of those integrals. People familiar with some of those integrals through standard vector procedures are naturally inclined to question micro-applicability. Applications envisioned by Aharonov and Bohm reflect a macro-realm.

PROPOSITION: **The global superstructure of quanta counters constitutes a valid epistemic extension of traditional Maxwell theory and has macro- as well as micro-applicability.**

Applications to quantum Hall effects and the electron's anomalous moment illustrate an applicability of these global laws to macro- meso-micro-scopic and sub-microscopic situations. The anomalous moment application avoids the infinities of standard QED.

Questions now arise how these three integrals relate to Schroedinger results and last, but not least, how do these integrals relate to the famous Bohr-Sommerfeld integral that had rendered such excellent services in the earlier days of quantum mechanics? At this point it is necessary to recall that the

Copenhageners had reduced the status of the Bohr-Sommerfeld integrals to mere approximations of the presumed "exact" Schroedinger results.

All of a sudden there are too many questions deserving a better answer. The problem is that physics has at least in part been driving in a wrong direction. It has exhausted itself in finding reasons to believe that the wrong direction is the right direction. This has led to an accumulation of invalid doctrine. All these teachings have to be undone, one by one. Here is at least a preliminary overview.

First of all, the Bohr-Sommerfeld conditions follow from the Kiehn integral by assuming point-charges. Since the three field integrals as quanta counters have a measure of unsurpassed exactness, it follows that the Bohr-Sommerfeld conditions share some of that exactness. The familiar investigations by Wentzel-Brillouin and Kramers thus reveal a mathematical rather than a physical asymptotics between results of Schroedinger and Bohr-Sommerfeld.

Copenhagen has circulated the unsubstantiated claim that the results of Schroedinger were exact and the Bohr-Sommerfeld results were approximate. This conclusion tacitly assumes the two methods address the same physical situation. So, an apparent conflict is precipitated by the undocumented Copenhagen insistence that the Schroedinger equation would be describing a single physical system.

Since the flux quantization experiments indicate single-system relevance for the quanta counters, it follows that the Bohr-Sommerfeld integrals have a single system connotation. So the asymptotic relation between Bohr-Sommerfeld conditions and Schroedinger equation leaves open the option that the latter describes an ensemble of identical systems and not a single-system. These conclusions are strongly supported by a more detailed study of their statistical interrelation.

Copenhagen

Almost all of the predicaments associated with Copenhagen views of quantum mechanics can be resolved by admitting to the ensemble nature of the Schroedinger approach. There are no known disadvantages associated with this change of venue, and it may be said that it protects users from making wrong and unpromising applications of the Schroedinger method. One will find that the Schroedinger equation has been written down many times for physical situations that really are not suitable for a Schroedinger treatment. Writing down this equation is believed the thing to do in any quantum situation. Most of the time, one can rest assured of at least getting an asymptotically close result.

Given the asymptotic closeness, it will be very hard to change those old habits. Using the old stuff, one can't go too wrong, because approximate correctness seems assured. So, why bother and do something else? The point is well taken, provided we don't complain about being bothered by Copenhagen contradictions or something else not right in the state of Denmark. Is physics just a means to do something successfully or do we also want to understand what we are doing?

Human effort at understanding should not be smothered by a widening stream of interpretative difficulties.

Let us repeat grounds earlier-given as to why the Schroedinger process is suited for ensembles, whereas methods based on three cyclic integrals are better suited for assessing *ordered* single systems; the cyclic integrals include Bohr-Sommerfeld as a special case of the Kiehn integral.

Experimentally, the Aharonov-Bohm and the Ampère-Gauss integrals have unmistakable single-system connotations. Since the Kiehn integral is an exterior product formation of the differential forms of these two, it shares in those single-system connotation.

A claim that these integral law statements must be approximate is no longer valid, because quantum Hall effect and Josephson ac effect directly relate to applications of these integrals. These two effects have produced today the best reproducible (to 9 decimal places) measurements of the elementary quanta of charge e and action h. None of the e and h determinations obtained solely through the use of either Schroedinger-Dirac or/and QED results come even close to that reproducibility. These are facts of observations. They should put to rest much, if not all, undocumented talk about as to what is exact and what is not. (see footnote p.99)

Extremely low-temperature conditions and high magnetic fields create states of single-system order for the quantum Hall effect. In the plateau states all Landau states are presumed to be operating in phase order, thus forming a synchronous joint lattice of those states. The single-system situation for the Josephson effect is perhaps most evident from the experimental set-up and end result.

Having established a single-system and exactness status for results obtainable through cyclic integral applications, a previously unasked question now presents itself: Can the Copenhagen claim be correct that the Schroedinger equation would also be describing a single-system feature? The answer is: yes, in an approximate manner! Unfortunately this question has never been adequately confronted. There is evidence, that Slater expressed such opinions to Bohr, which led to an unfortunate distantiation between the two. We shall see later that Heisenberg exchange forces are a case in point where Schroedinger gives in addition a statistic-type information about internal single system structure.

The Schroedinger equation thus leaves a convoluted trail of attempts at understanding. The new situation shows Schroedinger's equation as describing an unordered ensemble of single systems in which the latter can still have a measure of internal chaos.

Ironically a partial proof of the ensemble connotations of the Schroedinger equation is spelled out in the Feynman lectures (Vol.II as well as Vol.III). It is a statistical derivation of the typical angular momentum quantum number $\sqrt{n(n+1)}$. This Schroedinger angular momentum is the result of averaging a randomly oriented ensemble of identical single-system quantum rotators in quantum state nh.

It is another and even greater irony that the other part of that proof was given by Planck more than a decade prior to the 1925 quantum revolution. Planck showed how, per system, an average zero-point energy ¡ø/2 has to exist in a randomly phased ensemble of harmonic oscillators. Hence, **zero-point energy is not a single-system manifestation and neither is uncertainty.** The single system aspect was an unsubstantiated Copenhagen invention. Also, the source of QED infinities is due to the same impermissible single-system extrapolation of the Copenhageners.

The question is: (why) would these matters have escaped Feynman and co-authors? The, at-the-time, heavy investments in QED procedures may have been a factor, why there was reluctance to cite this as a beginning of a (counter) revolution. Another factor is that they may have been unaware of Planck's ensemble-based introduction of the concept of zero-point energy. One of the surviving authors of the Feynman Lectures might be able to cast light on this enigma.

So, Copenhagen's nonclassical primordial single system state of phase and orientation randomness, continued in washing out the reality of actual orbits, which, in turn, led to zero-point infinities and the concept of single system quantum uncertainty. All this nonclassical stuff can be replaced by a much less nonclassical primordial ensemble of random phase and orientation. Its zero-point energy is a minimal condition holding off phase and orientation ordering such as happens in the plateau states of the quantum Hall effect. **It is now a natural ontic proposition to identify quantum uncertainty as an inherent manifestation of the zero-point ensemble.**

Modern physics is confronted with a choice between Heisenberg's a priori single system-uncertainty versus a rather more natural uncertainty of phase-and orientation randomness of Planck's primordial ensemble. At that time, the absence of contrary evidence still permitted Copenhageners to focus on the paradigm of a Gibbs ensemble of conceivable manifestations of one-and-the-same single-system. They were lured by a wishful romantic desire to see the Schroedinger-Dirac process as a last word for an all encompassing single-system solution. Even so, at the time, voices were heard not to go overboard in having such unwarranted expectations.

It was a matter of weighing respective relevances of two ontic options. The vast majority went for a decision of romanticism versus an option needing fewer nonclassical escapes to make matters work. Not altogether unexpected, through the centuries, mankind has had a track-record of taking recourse to magic romantic solutions. Mythology so testifies. Also the quest for scientific reality itself can fall victim to the lure of magic. It is easier to overcome marginal scientific opinions than religiously held magic convictions, because the latter cannot admit to any vulnerability.

Contemporary physics is rather unanimous in its opinion that quantum mechanics is among the most effective tools available. This prowess of its practical potential stands in flagrant contrast with the prevailing ideas about its inner workings. The latter are shrouded in pure magic. So, we have here a wonderful instrument, but we don't really know why it does what it does.

The situation manifests a superficial similarity with contemporary life. In every day activities we quite effectively use electronic devices without knowing how they work. The difference is that some people designed those devices and they **do know** how they work.

By contrast, in physics nature "has" the answers and man sets the problems in his perennial quest for self-awareness. Man tries to find out what nature knows and, as part of nature, man has been making tools to explore nature's reality. In the past most tools that were developed had a direct rational relations to empirically observed facts. In time, the relations between facts and tools became stretched: e.g., in the Bohr-Sommerfeld case as well as in the Schroedinger-Dirac methodology. It calls for justifying the means by the end product they deliver. If the latter amazingly is of an unexpected relevance, it would be a sin to reject the results it gives, because the tools were obtained via some stretched ontic leaps of faith. Instead, one should attempt integrating these ontic propositions in a wider epistemology.

Notwithstanding ardent searches, seven decades of quantum history have not led to a consensus of epistemic unity. Those not concerned with details of tool design can afford assuming the pragmatic attitude of leaving things as they are. Yet, those concerned with tool design need to give thought why tools do what they do.

In these discussions experimental results have been emphasized as setting the tone to minimize distracting ontic infusions. The extremely reproducible Josephson- and quantum Hall effects indeed conspicuously contrast with the virtual ineffectiveness of the *statistical* Schroedinger procedure to predict or even explain and justify, in retrospect, the utterly *nonstatistical* precision of the simple relations obeyed by these effects. It just adds to a staggering evidence now favoring a two-tier approach to quantum mechanics: **Schroedinger-Dirac for ensembles and cyclic integrals for the simpler single systems.**

It should be understood that ensemble views of Schroedinger solutions have been considered since the early Thirties. The ensemble and single system have been sort of coexisting ever since. The truly classical statistical options engendered by the × function have not been part and parcel of these two groups that have now been tolerating one another for sixty years. Nonclassical statistics still rules supreme in the textbook literature, despite classical counter examples mentioned in that very same literature (*e.g.*, Feynman).

All these years classical statistical options have been available to serve single system- and ensemble view both. It is, however, the single system zero-point energy that does Copenhagen's single system point of view in. So, in retrospect, it ironically appears that the most popular view of quantum mechanics is the least viable from a point of view of physical reality.

Copenhagen's point-particle with its probability presence ingeniously circumvents the obvious predicaments of Schroedinger's smeared-out particle model. Yet, in the long run Schroedinger's and Copenhagen's single system interpretations both are untenable. It only took so much longer to come to that realization for Copenhagen's point-particle. Both procedures presume far too much about what × functions can do. In retrospect, it is unrealistic to expect detailed single system structural and behavioral implications from a × function process that has limited built-in capability to serve that purpose. The magic of Schroedinger's recipe made the monumental Hilbert legacy of eigenvalue procedures available to problem solving in quantum mechanics. This overwhelming windfall experience was bound to create an atmosphere of endowing × with magic capabilities, way beyond its potential for delivery.

As an alternative to such overambitious × function dreams, which in some theories include ×'s for the world and even the universe, one may now consider more modest aspirations for × as mostly an ensemble tool. This possibility can be assessed as a perhaps inadvertent consequence of Schroedinger's very own recipe that led to his wave equation, because this recipe optimizes, in essence, the solution manifold of the Hamilton-Jacobi equation.

Once alerted to these options, it is not all that hard to show that phase and orientation emerge as statistical parameters of an averaging process of solutions. This solution manifold, for given energy and angular momentum, represents in effect an ensemble of *randomly phased* & *-oriented* solutions. This leaves exactly two interpretive options: (1) The Copenhagen choice, which is now an ensemble in the sense of Gibbs, in which solutions are taken to be conceivable manifestations of one and the same single system or (2) The solutions individually correspond to the identical single system elements of an ensemble.

Option (2) is by far the more realistic choice. It avoids the ultra-violet catastrophe precipitated by spectral integration of *individual* zero-point energies. Option (2) also allows interaction between ensemble elements; the Copenhagen option (1) does not! Here we see how close Copenhagen's choice was to a physically realistic option. Let it provide understanding for the many decades it took to come to this realization. In retrospect, one could say, it had to be that way; how else could the results have been so close to a physical reality we knew about all along? The Schroedinger equation thus remained an object of perennial questioning, because not knowing its limitations, one can't quite know its defects.

The Delusion of A Priori Absolute Uncertainty

The rejection of single systems as a sufficiently realistic object of Schroedinger-Dirac description creates a two-tier situation in the approach to quantum mechanics. On the one-hand, an evolution in cyclic integration methods permits an exploration of single system topology, on the other-hand, phase and orientation, pertaining to ensemble elements, permit order-disorder exploration casting light on phase changes of matter.

The situation resembles the two-tier situation of Newtonian mechanics and traditional statistical mechanics. Slater, at one time, made suggestions to this effect to Bohr; they were not well received. There is a difference though:

traditional ensemble parameters are dynamic; they relate to thermal states. By contrast, Schroedinger-type descriptions pertain to a statistical state governed by orientation and phase. The latter are not dynamic by themselves, but set the stage for the dynamics of thermal behavior.

Planck has considered harmonic oscillator phase as an order-disorder parameter in ensembles thereof. These mutual position variables hold a key to understanding transitional states of matter between solid, liquid and gas. Mindful of the stationary temperatures during such transitions, order parameters are regarded as setting the stage for subsequent intervals of thermal change. Knowledge about modification changes in condensed states of matter is still full of open questions.

Remarkable about the zero-point energy state, as introduced by Planck, is its primordial nature as a first transition state in the temperature run going upwards from the absolute zero. The transition between quantum Hall plateaus and normal Hall effect states are indicative of an order-disorder transitions in which zero-point energy is instrumental. Further inquiry raises many more questions about other near zero-point transitions in which zero-point energy has a determining role.

The inherent ensemble nature of zero-point energy immediately raises questions whether the closely related concept of *quantum mechanical uncertainty* can be a single system attribute. Its postulated a priori nature has prevented a development of detailed pictures of single system structure. Traditional Heisenberg uncertainty shrouds micro objects from further inquiry, thus limiting man's knowledge about these matters.

These traditional uncertainty conclusions are all contingent on their presumed but mistaken single system association. They are no longer compelling for the ensemble. An early ensemble point of view, pursued by a small minority[*] since the Thirties, has unfortunately been unable to forge ahead, because they were hesitant to reject Copenhagen's nonclassical statistics that goes with the single system thesis. They did accept single-system Heisenberg uncertainty, which impeded further ambitions to independently explore single system structure. It shows the overwhelming impact of uncertainty in the Heisenberg vein. It seemed, the Gods were reminding man of his limitations; this sentiment did gibe favorably with the times of collective guilt following the era WW I.

Heisenberg's 1927 uncertainty paper transcribed an imprecision due to truncating Fourier expansions. To understand its relation to Heisenberg matrix mechanics, consider how the latter invokes products of Fourier expansions, in which Cauchy's rule for product series translates into matrix multiplication. The fact is truncating Fourier expansions is an act of man, the question to be asked is whether a multiplication with Planck's constant makes it an act of God?

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[*] After Slater's first attempted revolt, Popper, Kemble and Groenewold acquired prominence in this minority. Popper received conditional support from Einstein. During that time Russia also had an active school of ensemble supporters.

In the same year, Kennard confirmed a near identical uncertainty restriction as a valid consequence of Schroedinger's formalism. *This clinched the famous relation, not Heisenberg's interpretation.* An ensemble view marks uncertainty as a manifestation of a primordial ensemble disorder. The latter sits easier with our sense of reality.

The ensemble movement, started by Popper, Kemble and others in the mid-Thirties, somehow bought into the idea of Copenhagen's nonclassical statistics. In doing so, they sacrificed single system inquiry and placed themselves in a no-win situation. Copenhageners could tolerate such ensemble pursuits, because the conflict was merely a formal one between a real- ensemble (Popper-Kemble) or a Gibbs ensemble (Copenhagen).

Identifying uncertainty as a manifestation of Planck's primordial ensemble eliminates uncertainty as a single system phenomenon. It reopens single systems to the semi-new tools of cyclic integration. The latter invite topological modelling. This *quantum cohomology* is in its initial phases. In a give and take of some of the presently distorted measures of physical reality, the reader may consider whether *Quantum Reprogramming* (see notes), in its account of *quantum cohomology* presents alternatives with less of a premium on accepting magic solutions.

The moral of the story is that in the mid-Twenties physics became confronted with some wonderful new tools that did not quite permit an *ab initio* justification by derivation. Perhaps more than ever before, tools had to be justified in retrospect, by showing what they could do. Add to this the potentially misleading asymptotic proximity between single system and ensemble results and the stage is set for gullibility in theory.

At the time, an avalanche of strikingly new results invited commentaries gushing with praise for the wonders of the *new* nonclassical physics. Yet the history of Heisenberg uncertainty is living testimony that even the best could not escape measures of flawed and careless reasoning. It shows too well how hiding behind the label *nonclassical* can be used not to face reality.

There is a physics consensus floating on the belief: If magic worked once, it may work again! It gets harder and harder to judge how much of the contemporary production falls in that category. To make sure, let us check what other reasons Heisenberg could have had to favor a single system aspect for his new quantum mechanics.

Helium Spectrum and Heisenberg Exchange Force

It was Bohr's monumental step forward to provide a theoretical justification of the Hydrogen spectrum, which offered promising perspectives beyond the discoveries of Planck and Einstein. When during world-war I, Kramers came to work with Bohr in Copenhagen, one of his first assignments was an attempt at a similar delineation of the intricacies of the spectrum of the next chemical element of the periodic chart: *i.e.,* helium. It soon became clear that the early tools, which had accounted for the hydrogen spectrum as well as its fine structure, were no match for problems presented by the helium spectrum.

A series of new discoveries were necessary before a new attempt could be made at delineating the peculiarities of the helium spectrum. In fact, an altogether new quantum methodology had to be developed before anything could be done in this direction.

Almost a decade later, Heisenberg (Kramers' successor) initiated the first steps that led Born and Jordan to matrix mechanics. The ensuing algorithm was superseded the next year by the equivalent Schroedinger method. In the meantime, Pauli added the exclusion principle as an independent proposition. Then the anomalous Zeeman effect spawned the electron's spin and magnetic moment. Armed with these new tools, Heisenberg reexamined the helium spectrum.

In approaching this problem, Heisenberg abandoned the matrix methods, he had helped to initiate. He right away availed himself of Schroedinger wave function language, reinforced with the exclusion principle and the notion of spin. The results were spectacular. Not only did this new approach qualitatively account for the observed two independent spectral systems of helium, it also gave a reasonable quantitative account of the term levels. They were later refined by higher order perturbation calculations.

From the victory over helium's spectral peculiarities, a new transcending insight emerged. It is now known as the Heisenberg **exchange force** and its associated energy. This exchange force is not really a new force category. It is a Coulomb-type interaction, which develops a new angle through the superimposed process of quantum mechanical averaging. The exchange energy for helium is a dynamically weighted integral of the Coulomb interaction between the two orbiting electrons; low order weight functions are the one-particle solutions prevailing in the absence of Coulomb interaction.

Heisenberg's analysis reveals some uncanny single system capabilities of the "many" particle Schroedinger equation. This went well beyond the early tools available to Kramers. These experiences may also give a measure of understanding how Heisenberg became a convinced supporter of a single system based interpretation. So, when in the next year (1927), the concept of quantum uncertainty was launched, the success of the helium analysis suggested to make uncertainty a single system attribute. Only later was it discovered that this very step would produce vacuum infinities. The latter were to acquire a near-fatal, yet crucial role, in later QED procedures.

This interlude about exchange forces raises the most serious challenge to a compatibility with the unmistakable ensemble connotations earlier identified for Schroedinger's method (see section on Copenhagen). While it is true that classical statistical processing of early single system quantization can give typical Schroedinger results, that evidence does not suffice to derive and obtain the whole Schroedinger method in that manner.*The Schroedinger process here reveals capabilities of dealing with compound structures not presently covered by the earlier Bohr-Sommerfeld procedures.*

Heisenberg's exchange perturbation process gives Schroedinger's method indeed a better handle on many particle situations than the Bohr-Sommerfeld process. A semi-classical *orbitally-based* perturbation would have trouble yielding these results, because typical Heisenberg exchange effects follow from electron spin and wave function stipulations imposed by the Pauli principle. The latter has no obvious counter part in classical mechanics! Heisenberg's exchange process thus reveals, over and above the ensemble statistics, an internal statistics of compound single system structure. These matters have been the subject of new semiclassical investigations, recently reviewed by Wintgen *at al*.*

The previous section on *Phenomenological Pathways* mentions **quanta counters** as superseding the Bohr-Sommerfeld relations. Aharonov-Bohm takes precedent over Bohr-Sommerfeld (as it should).** Compound many particle single systems may well be governed by a mixture of flux closure and and orbital topology. The latter somehow translates into observed electron shell formations.

Presently, these are at best options for future investigation. The question is whether and how global approaches can cast light or duplicate Heisenberg's exchange procedures. The exchange integral procedure has also been successfully applied to molecule formation and chemical bonding. As for helium, also here single system aspects demand a new forum, because a statistical looseness (chaos) remains in the internal structure of compound single systems.

Applications to magnetism further illustrate how exchange forces convert ensembles of random spin into extended single systems with long range spin order. Here is a giant single system with minimal internal looseness compared to the micro-loosenes in the helium atom. Internal looseness again prevails near the Curie temperature.

It now becomes clear why it is difficult to derive Schroedinger's equation from first principles. Planck showed that zero point energy ¡ø/2 can have a classical statistical origin and later the angular momentum quantum number $\sqrt{n(n+1)}$ was shown to be reducible to a classical statistical origin. When these two items later were found to emerge automatically from the Schroedinger equation, physics gave in to the temptation of accepting a first principle status for this equation. These two items are indicative but not sufficient for a conceivable reduction of Schroedinger's equation to the first quantum principles used in the older quantum mechanics of Planck, Einstein and Bohr-Sommerfeld.

The preliminary picture transpiring here has a potential of yielding better understanding of the complex nature of Schroedinger's equation. This quest is contingent on two factors:

(A) The quanta counters earlier referred to in the previous sections give a better first principle foundation of early forms of quantum mechanics. Phase and

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* D Wintgen et al, Chaos **2**,19 (1962)

** The *Aharonov-Bohm* integral must be regarded as more fundamental, because it is independent of specific particle assumptions. In fact, the 3-dimensional *Kiehn integral* yields Bohr-Sommerfeld after a point charge application of the*Ampère-Gauss* integral.

orientation statistics of quanta counter statements pertaining to *elementary* single systems can lead to Schroedinger statements.

(B) The semiclassical studies pursued by Wintgen *et al* and others hold promise of also covering ensembles of compound single systems. In this vein Schroedinger's equation covers ensembles as well as internal statistical matters of compound single systems. **Hence the Copenhageners were at least partially right.**

At about the time when Heisenberg solved the helium problem, Kramers, who had started out with that problem when he came to Copenhagen, achieved a significant victory in the phenomenological realm. He established a set of analytic conditions that need to be obeyed if physical interactions are subject to the dictates of causality. It is interesting to compare the nature of these two achievements.

Heisenberg expertly juggled an array of ontic propositions that finally led to an understanding of the helium spectrum. The input came from the discovery of electron spin, the Pauli principle and the methods of the new quantum mechanics. It is a beautiful example how ontic combinations can ultimately home in on epistemic truth.

When Kramers started out with the *helium* problem, all of the just mentioned ingredients were still absent. His efforts stranded on insurmountable difficulties. Then in later joint work on dispersion with Heisenberg, it was Kramers who supplied the mathematical structure of dispersion that enabled Heisenberg to make his ontic proposition that would cast new light on quantization. Then armed with Heisenberg's ontic proposition, the more rational part was completed by Born and Jordan. They worked out the mathematical procedures that later became known as *matrix mechanics*.

The realm of dispersion theory has a strong phenomenological aspect that must have appealed to Kramers, because he returned to a missing element in his earlier work. He namely succeeded in giving a rigorous mathematical formulation of how causality is obeyed in the process of dispersive response. We see here how the development of quantum mechanics has been an almost ideal playing field for recognizing the complementary potential of intuition and rational effort in making meaningful steps forward possible.

MATHEMATICS; VEHICLE FOR PHYSICAL THOUGHT

Introduction

Man's need for mathematics may well have coincided with his awakening sense of property. There was an early need to count herds of sheep and cattle. So, animal husbandry may well have ignited man's fascination with numbers.

From counting herds to the operation of addition was in many ways an abstraction waiting to happen. It arose as an act of convenience. How else could a herd owner obtain an impression of his total herd, if his several shepherds were taking their part of the herd to different places? In case man had not yet invented addition, the chances were he would do it soon.

For a perspective on these matters, consider that animal husbandry may have started as little as thirty to fifty thousand years ago. We know little about how those ancestors dealt with their numbers. We do know, however, that the Romans used a notation for numbers that was not very convenient for the performance of arithmetic operations.

It was late in the middle ages that Roman numerals in Europe were being replaced by the Indian-Arabic number system. At that time, arithmetic with the new number system became a university specialty. It is said a professor at the university of Padua let it be known that people could now overcome their fear for big number arithmetic by taking his courses.

This little detour in the history of mathematics carries an important message. It shows, no matter how simple or lofty the mathematical objective, the endeavor can stand and fall with a wrong choice of notation. Mathematical notations may be said to represent an engineering aspect of mathematics. The transition from Roman to Indian-Arabic numerals had a tremendously liberating effect, not only on the development of arithmetic but also on the development of mathematics in general. Many things in arithmetic that, until that time, seemed impossible or cumbersome, now became possible. The ensuing liberation led to the emergence of new mathematical disciplines, each with their own aspects of notational engineering.

A familiar example in calculus is the fluxion notation of Newton versus the d notation of Leibniz. The latter won out, because it explicitly identifies the variables inducing the change. In so doing it permits a helpful distinction between partial and total derivatives.

From these earlier experiences it is clear that appliers of mathematics had at least as great a stake in notational engineering as the practitioners of pure mathematicians. In fact, the latter may take a more neutral position vis a vis notation, where the applier of the art may show very specific preferences.

By the same token, it also happens that appliers of the art of mathematics are so anxious for their results that they take shortcuts. They take just that what they think is needed to serve them for the moment of need, yet later such shortcuts may come back to haunt them. Undoing premature shortcut decisions is a very hard thing to do once a pattern of shortcut habits has been established. As an example we shall discuss here the Saga of the use of mathematical field theories in Physics.

The Mathematical Field Concepts of Physics

The velocity field of fluid flow is a traditional physical field and so is the associated field of momentum density, which comes about after multiplying the flow velocity by the local mass density of the fluid. The forces in a fluid are taken to be transmitted by contact across a surface area of given direction. The contact forces in a fluid remain somewhat academic entities, because they are contingent on a choice of surface. In a more general sense the force conditions in a continuum are described by a so-called stress field. The latter is a tensor field, a local force acting across a surface area is then obtained by having this tensor operate on the vector of the surface element as a linear vector function. It means the components of the force field are linearly related to the components of the vector defining the surface across which the force is supposed to work. The components of the stress tensor are the coefficients of this linear vector relation.

These stress force concepts were beginning to develop already in the days of Euler, yet the final codification of the field description in material media was due to Cauchy. Here we see how major parts of physics were developed by leading mathematicians of the time.

Quite different from these traditional contact forces was the gravity force concept introduced by Newton. The force of gravity in effect reaches across empty space, without the need of a physical contact to transmit the force; *they were said to be acting at a distance.* Their intensities were perceived as depending on the mutual distance of those gravitating bodies and their masses.

In the beginning of the 19th Century, other examples of such forces became known as also acting at a distance. They were the electric and the magnetic forces. Faraday explicitly initiated the description of the realm of influence of those forces with the help of an influence field. As the gravity field, these new electric and magnetic forces also were identified as vector fields of influence. Two are defined as forces per unit mass and per unit electric charge respectively. The magnetic induction field is now defined per unit electric charge and velocity, since *extensive searches have not led to an identification of magnetic charge!*

It is hard to find fault with the 19th Century pioneers who gave in to the temptation of lumping all these vectorial quantities together under one unified mathematical vector concept. Even if Maxwell, in practice, may have succumbed to the pressure of adopting a unified vector concept, it is known that in the spirit of Faraday's original geometric conceptions, Maxwell was well aware that the physical distinctions between these vector species were also explicitly reflected in subtle mathematical differences.

Yet notwithstanding the important sentiments of Faraday and Maxwell, the vector unification nevertheless became a fact of convenience and led to the system of vector analysis, which is today the most widely used shorthand of mathematical communication in physics. Here we want to find out more about the price that had, and still has, to be paid for this convenience?

Closer scrutiny shows that the system of vector analysis remains valid provided rectangular frames are being used that are all of the same handedness. It means the frames are mutually related by the special orthogonal group SR(3). It soon appeared that the restriction to frames of one handedness made it difficult to deal with phenomena that have mirror image features such as occur in crystals. The system of vector analysis is therefore unusable in crystal physics. A point of later concern became the presumed arbitrary extensions of these frames to infinity, which is possible if, and only if, physical space is taken to be Euclidian in nature.

Even if this proposition has locally a good measure of validity, the fact is that the Euclidian thesis of indefinitely extendable frames of reference to infinity is an idea that is much less clear than we were made to believe in school. Assumptions of a Euclidian physical space are so highly specialized, that the chances of their verification in the long run must be taken as slim.

To the Euclidian predicament we may add that physical reality imposes, over and above, a distinction between inertial and noninertial frames. The latter are mutually related by transformations that are not linear in the time. By contrast, note that Euclidian rectangular frames are mutually related by the strictly linear transformations of the group SR(3). This unbalance between space and time properties for particle dynamics was removed by Lagrange. These analytic ramifications were further developed by Hamilton and Jacobi. The existence of a (spacetime) differential one-form of energy-momentum was so established. The integrability of this form gives Hamilton's equations of motion, going back to early 19th Century.

At that time, however, the continuum theory of the electromagnetic field had not yet developed. Perhaps for reasons of simplicity, the attractiveness of the shorthand of vector analysis won out in the early mathematical codification of electrodynamics. Yet, this tie-in with the restricted system of vector analysis justifies questions whether this connection imposes undue restraints in contemporary situations.

Even from this brief overview it is clear that the interaction between physics and mathematics on the subject of field theory is very convoluted. To disentangle this situation we do well to obtain first a time table of mathematical developments that seems relevant to the emergence of physical field theory.

Survey of Mathematics in Its Relation to Physics

17th Century
Descartes' analytic treatment of geometry. His notion of "Cartesian" frames of reference, mutually related by linear transformations are strictly Euclidian.

end 17th Century
The development of calculus, Leibniz and Newton and beginning the concurrent development of Newtonian mechanics against the backdrop of a strictly Cartesian tradition.

18th Century
The development of continuum mechanics by Euler, continuing Cartesian traditions. At the end of this century his successor Lagrange lifts the Cartesian traditions in particle dynamics.

first part 19th Century

Beginnings of **global** formulations in physics, in contrast to the differential traditions of Newton and Euler.

Examples are:

1 Hamilton's action principle
2 Gauss'law of electrostatics
3 Ampère's law
4 Faraday's law of induction

Concurrent with these developments came the extension of the field concept as capable of conveying an action at a distance. This period of global formulations in physics was made possible by two major integral theorems ascribed to Gauss and Stokes. Both theorems convert an integral over a closed boundary to an integral over the interior of that boundary. It became clear by the turn of the century, that these theorems have validity way beyond their initial Euclidian origin. They are now seen as dimensional generalization of the principal theorem of the integral calculus. Since notions such as enclosing and linking are crucial for their formulation, they are also beginning to invoke ideas about topology and global physics.

1854

Riemann breaks with the exclusive Euclidian-Cartesian tradition in geometry. The Riemann-Christoffel condition is a tensor criterion providing information how much a Riemann manifold locally deviates from Euclidian. With Riemann and followers begins the development of a metric-based tensor calculus, holding up under **general** transformations; *i.e.*, Diffeo-n, instead of the SR(n) or SR(3). This metric-based apparatus of generalized calculus was further honed to perfection in the next century by the work of Ricci, Levi-Civita and Schouten.

Last part 19th Century

Since the needs of crystal physics were purely local, where curvature has no major role, there was reason to retain Cartesian frames. This simplified need, led in first parttime to disciplines restricted to Cartesian tensors.

20th Century

Since the primary laws of electrodynamics and many other fields invoke mostly tensors of valency one (called vectors), the slumbering discipline of Cartesian tensors ultimately gave birth to the famous shorthand that later became known as vector analysis.

Better than any very detailed mathematical discussion, this brief history illustrates the sequence of compromises underlying physics' most widely used vehicle for communicating essentials of physical field theory.

1915 For the purpose of delineating the principles of his general theory of relativity, **Einstein had to revive the basics of noncartesian tensor analysis.**

His efforts only met with partial success, because his principle of general covariance, meant to adapt the Riemannian procedures to the compelling needs of physics met with major opposition. Contemporary texts on relativity tend to pay lip service to this criticism, which leaves this matter unresolved.

1920 Cartan develops the shorthand of differential forms, which meets the condition of **general**, *i.e.*, a **diffeo** invariant frame independence, in contrast to the orientation preserving orthogonal group in three dimensions SR(3) of vector analysis. It is an invariant shorthand for antisymmetric covariant tensors. The (invariant) differential operator replacing the gradient, curl and divergence of vector analysis is the **exterior derivative.** The latter is a metric-independent differential operator, identical to the differential operator in the generalized laws of Stokes and Gauss.

The Cartan differential form method thus manifests itself as a subcategory of a generalized tensor analysis, that is distinguished by a metric-free feature.

Mid Thirties

As earlier observed, the Stokes and Gauss laws with their closure and linking features have topological de Rham implications. Since topological assessment precedes metric assessment, the metric-free connotations here mentioned invite an exploration of the Stokes-Gauss laws and Cartan's method of differential forms as tools in topology. De Rham has done exactly that, over and above he reintroduced with his pair-impair forms the orientability features that could not be covered by the usual methods of vector analysis. De Rham's procedures for topology exploration are now known as **de Rham Cohomology.** Since de Rham made little or no use of the option created by impair forms, later mathematical discussions have unfortunately remained restricted to pair forms.

Do these Cartan-de Rham methods relate to a truly frame- and metric independent **global** mathematical formulation of major physical laws? Here is a preliminary answer to that question, which is, in fact, a **spacetime** invariant quantum reformulation of earlier mentioned 19th Century laws:

I The energy-momentum **closed** one-form has an integral that can assume the role of the Bohr-Sommerfeld conditions.

II The Aharonov-Bohm integral is the integral of a *closed* one-form, which can assume the role of counting units of flux. This one-form is also said to be **closed.**

III The Faraday induction law can be seen as an integral of a two-form expressing the conservation of flux, if the integral is taken over **any** closed two-dimensional domain in spacetime. This two-form is now said to be **exact.**

IV The Ampère-Gauss law is the integral of a two-form that can assume the role of a counter of a net number of charge units. This two-form is now said to be **closed.**

V Charge conservation is expressed by a vanishing integral of a three-form, which is taken over **any** closed three-dimensional domain in spacetime. This three-form is now said to be **exact.**

VI There is an integral of a **closed** three-form defined by Kiehn, which may be said to be the field counter part of the particle-based relation I. The three-form of the Kiehn integral is the exterior product the differential forms of II and IV.

Integration of the Ampère-Gauss part IV reduces the field statement VI to the particle statement I of the Bohr-Sommerfeld integral.

Statements II,III,IV,V are all familiar field laws. Thanks to the Cartan-de Rham formalism, they can now be seen in a different perspective capable of adding new insight of a topological nature. Specifically the differential forms II, IV and VI now testify to **a fundamentally global, Diffeo-4 invariant, aspect of quantization,** which cannot come through at all in the traditional SR(3) way of viewing these laws

Pondering the Implications of this History

The just given overview of mathematics as it relates to physics testifies about elements of philosophy and reoccurring trends in the mathematical treatment of physics.

There is a basic philosophy of making fundamental physical laws as much as possible independent of the frame of reference. After all, laws are given by Nature, frames of reference are chosen by man. Throughout this history of physical mathematical interaction between physics and mathematics, the theme of frame-independence reoccurs with different demands of rigor. The trend in early exploration first attempts weaker requirements of rigor, which initially may enhance chances of making progress faster. From time to time, this process is interrupted to give the conscience a chance for reconsideration. The purpose is one of probing the rigor of earlier decisions. Once it appears that the existing ways of life are mostly adequate, traditions retake their course, provided certain restrictions are kept in mind. Thus the old methods become reestablished with minimal adaptations to account for new insights.

The history as here accounted vividly illustrates this state of affairs. From Descartes to Newton and Euler, the Euclidian frame background was found to be as quite adequate. Then at the turn of the century from 18th to 19th, there was a sudden trend to extend the frame independence of particle mechanics. It was initiated by Lagrange and then continued by Hamilton and Jacobi. Extended frame-independence, only in part, motivated this work; in fact, it may have been a byproduct.

The intensified exploration of the planetary system called for more accurate predictions of planetary objects. This need in part precipitated the sophisticated techniques of Lagrange and Hamilton-Jacobi. Their work was much enhanced by an extended and improved transformation theory for celestial mechanics. Surveyors, navigators and chart makers needed the improved data.

Continuing the path of physical development, the next laws to emerge are Gauss' law of electrostatics and then the Faraday law of induction. Neither the net charge count inside an enclosure nor the induction law seem to demand a specialized choice of rectangular inertial frames of reference, yet invariably, the run of the mill text book always seems to imply exactly that. Rotating electric generators operating on accelerating vehicles vividly indicate that Faraday's law fortunately knows how to ignore unnecessary inertial frame restrictions. If restrictions remain, they are due to man's acts of convenience to carry on with formalisms that are known to be inadequate. Many workers in the field know these shortcomings, but don't feel called upon to make changes.

Gauss' law, as a counter of net charge, manifests an obvious frame-independence in static situations. Counting moving charges requires a combining of Gauss' law with Ampère's law. This combining of the two calls for a *spacetime* frame-independent global rendition. Texts giving local-oriented presentations can be expected to avoid those issues.

At the beginning of the Twentieth Century, the study and applications of electrodynamics had flourished, notwithstanding the restrictive nature of the system of vector analysis in which it had been cast. All of which goes to show that people are smarter than the systems they devise. Even if today better mathematical systems are available, there is no rush to make changes. Apart from some reluctant experimentation with differential form methods, the overall impression is that teachers and students of physics are not buying into this.

The only way of changing that stalemate is to discontinue the use of SR(3) restricted systems, because they stand in the way of perceiving promising new opportunities. Since the promise of new opportunity has a different ring to different people, let the pursuit of *new* not be just for the sake of an indiscriminate pursuit of *new*.

Frames and Metric

One of he hazards of life is to be confronted with situations in which we seem compelled to use words not knowing what their impact will be, because we are not sure how others perceive them. In physics, the words *frame* and *metric* belong in that category. To serve an objective of having these words generate a consistent picture, here is an attempt at delineating their role in mathematics and physics without too many technicalities.

Descartes first introduced the concept of frame of reference in geometry as an aid in the analytic description of geometric objects. His coordinate axes, in effect, were rulers with a metric subdivision. All rulers are given the same metric subdivision. It thus appears that frame and metric were really introduced jointly as almost inseparable entities. The Pythagorean theorem then provides a basis for transferring the metric notion to arbitrary directions.

It was the combination of identical rulers at right angles with respect to one another, which, in the Cartesian tradition, made the metric next to invisible. It was this *tradition of the invisible metric* that directly carried over into physics with great success. In fact Descartes' choice ruled supreme in physics until the beginning of the Twentieth Century. In geometry, Descartes' tradition succumbed to a more general point of view in the middle of the 19th Century.

Minkowski was the first to make the metric visible as a separate entity by extending spatial physical description to the level of explicit spacetime description. The spatial expression for distance $x^2+y^2+z^2$ so became replaced by the time affected expression $x^2+y^2+z^2 - c^2t^2$, which boldly transforms the invisible spatial metric {1,1,1} into a visible spacetime metric $\{1,1,1,-c^2\}$ with $-c^2$ disturbing the pristine character of the {1,1,1} part of the metric. So strong was the tradition of working with an invisible metric that people started making the rather unphysical substitution $c^2= -1$, for the mere purpose of also establishing for spacetime a metric {1,1,1,1}, just to maintain similarity with the space metric {1,1,1}.

Unfortunately for the magicians who in their amazing juggling act had made the metric disappear again, Einstein at just about the same time was walking around with a strong suspicion that the speed of light might not be a constant. This point of view has since been observationally supported by three effects (gravitational red shift, bending of light rays and perihelion shift of Mercury). More recently one can add to these a time delay anomaly measured by Shapiro. This anomaly occurs for radar signals bounced from the surface of the planet Venus when travelling through the sun's gravity field. While the bending of light in gravitational fields should already give some time delay, the bulk of the Shapiro effect would have to come from an actual decrease of the speed of light in strong gravitational fields. Until now astronomy observations of singular twin star formations has in general been supportive of the existence of the effect observed for the Mercury orbit. It should be mentioned that an acceleration red shift has been accurately verified in the laboratory with the help of the Mössbauer effect. Experiments by Moon and Champeney* have brilliantly verified the acceleration red shift as exactly what it says it is; *i.e.*, not a purely kinematic effect explainable in terms of Lorentz tranformations.

In the light of this accumulating evidence, the motivation for a continued hiding of the metric is beginning to lose its justification. Perhaps one might still continue in hiding the space metric, but not the spacetime metric. Since the spacetime metric components are tied together in a single tensor quantity that was postulated by Einstein as the spacetime metric, there is, in the light of the general theory, no alternative but to give this metric an independent physical existence. All this is based on Einstein's initial suspicion that the speed of light might be subject to change according to the gravitational profile existing in the universe.

Precision laboratory experiments give the speed of light in about ten decimal places. These measurements need to be regarded as measuring the speed of light in the gravitational field of the earth. The correction suggested by the general

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* Proc. Phys. Soc. (London) **77**,350(1961)

theory to extrapolate those measurements to a hypothetical gravity-free space are presently still escaping a direct verification.

All of this brings out the boldness of the conceptions underlying the general theory. Even if some major parts remain unverified by direct experiment, the body of indirect evidence supporting its premises is by now rather impressive. In terms of a field description of physics it means the metric tensor has been making a transition from being a purely geometric quantity to becoming a field with rather exclusively large scale physical implications. This metric tensor no longer should remain hidden in a special choice of frame of reference.

Let us now consider the implications of this metric emancipation for the bulk of physics that deals with every day, smaller scale, physical phenomena. It means a rewriting of standard physics using an explicit metric tensor and arbitrary frames of reference obeying Diffeo-4 instead of SR(3) conditions. If that seems a lot of work, it may come as a relief that much of that work has already been done in Twenties and Thirties by Kottler, Cartan and van Dantzig.[**]

The most striking result of these mathematical transcriptions is that the metric tensor again disappears form the scene as a primary physical quantity, but now in a Diffeo-4 context. Many pundits of the time could be heard saying: *"Of course, we knew this all along. You just proved that we can continue using the SR(3) frames of our beloved vector analysis. So what you are doing is just mathematics."*

It may be historically relevant to recall that the time of the KCD publications roughly coincided with the Kretschmann-Bridgman(KB) criticism of Einstein's principle of general covariance. They (KB) claimed the principle to be without compelling physical implications. Most relativity texts mention the KB criticism, yet few mention the explicit KCD results. It seems Einstein remained unaware of KCD developments despite his correspondence with Cartan.

The positive variant on this theme may lead us to observe how the KCD outcome is in keeping with the relativity conclusion that metric influences only show up in very large scale gravity effects. So, the next question should have been: what is the meaning of a Diffeo-4 frame-independence of the KCD statements? Such inquiry would have been more positive than seeking premature justification to dismiss the KCD results. Here is an answer why KCD distinctions are needed:

First: the metric is the one and only criterion that gives us a measure of large and small in a physical sense. Hence, if the metric does not explicitly occur in the law statements, they may be expected to hold in the macro- as well as in the micro-domain.

Second: prior to questions about metric properties of physical objects, a knowledge of their topological structure is a prerequisite. Topological properties are not affected by changes in the category of Diffeo-4. Hence the typically

[**] This KCD process imposes an obligation to give noncartesian definitions of physical fields previously only known in a Cartesian context. Physics has consistently evaded such decision making, its method of "curvilinear" coordinate procedures testifies to that.

metric-free Diffeo-4 invariance of KCD law statements permits and really invites a topological exploration of microphysical objects.

Most modern physicists will vehemently deny the possibility of probing into the topology of microphysical structure, because with their thesis of a universal single system quantum mechanical uncertainty they had made the domain inaccessible. So, to the extent that there is at all an awareness of these matters, physics has been assuming a stance of taking quantum uncertainty as an excuse not to delve into the KCD aspects of physics.

On the other hand, if quantum mechanics, as it stands today, describes, say, phase randomized ensembles of identical systems, the excuse not to take cognizance of KCD related matters no longer holds. Then there are no longer objections against uncovering mathematical machinery capable of casting light on the topology of microphysical structures. Therefore, standard SR(3) field theory shortchanges physics by missing out on valuable topology insight.

The earlier cited integral law statements I,II-VI, are all metric-independent and Diffeo-4 invariant. Over and above, the statements I,II ,IV and VI are honest to goodness Diffeo-4 invariant quantization statements that are in general use today. So, what is all this talk of quantum theory not being reconcilable with requirements of the general theory of relativity? Presumably, physics got caught in interlocking inferences that can only be disentangled by KCD !

On the one hand, there is the SR(3) restricted field theory. Extended to standard quantum theory, it goes hand in hand with Copenhagen's single system view of Schroedinger solutions and the single system universal quantum uncertainty thereof. The latter totally blocks a straightforward assessment of microphysics.

On the other hand, if a Diffeo-4 field theory is being pursued, it reveals metric-free aspects of existing quantum integral laws. This opens up the microdomain for individual assessment, if and only if, quantum uncertainty is now viewed as an ensemble phenomenon. That makes the Schroedinger equation an ensemble and not a single system tool as claimed by the Copenhageners. Since single system laws may be considered as more fundamental than ensemble laws, **the SR(3) restriction of the Schroedinger equation is an ensemble symmetry and not a single system property.**

In the Thirties and even in the late Twenties several leading physicists and mathematicians made very serious attempts at transcribing the Schroedinger and Dirac equations in a Diffeo-4 invariant manner. Their objective was making them compatible with the general theory of relativity. This work did not lead to new physics.

In the light of the topology perspectives created by the KCD work, it can now be said: those early attempts at Diffeo-4 transcriptions of the Twenties and Thirties were incorrectly addressed. Since there had been many other unpromising efforts at Diffeo-4 transcription of other equations of physics, some people said: "any equation can be written in covariant form, if you set your mind to it." All these indiscriminate and unsuccessful attempts at generalization brought a measure of discredit to the principle of covariance.

A few words are now in order about the physical dimensions associated with a global Diffeo-4 field treatment on the basis of the known integral laws I,II,...VI. All of them have physical dimensions exclusively expressible in terms of the units of electric charge [e] and action [h]. Flux has the dimension [h/e]. The universal impedance of the quantum Hall effect has the dimension [h/e^2], it emerges as the quotient of two integrals. Nature is telling us that h, e, h/e and h/e^2 are good Diffeo-4 invariants, as they should be, if the integral laws I,II...VI are deserving of the name scalar-valued integrals.

These cited facts of life in physics suggest minor modifications in the standard custom of citing physical dimensions. The standard MKS system of mass [m] charge [e], length [l] and time [t] is advantageously changed into a system [h,e,l,t]. The latter has two invariant dimensional units [h] and [e], the customary system [m,e,l,t], instead. has only one invariant unit [e].

Mappings between Mathematics and Physics

Since all mathematics derives from the physical being of things, in a technical sense, all mathematics is relevant to physics; meaning it can have an applicable counterpart in physics. In this sense mathematics and physics can be said to be related by some sort of a "mapping."

The map from physics to mathematics is in a grosso modo of a one to one nature. In fact, the formulation of physical fundamentals can lead to very specific mathematical fundamentals; calculus being a prime example. Conversely, the map form mathematics to physics is bound to be multivalued, because basic mathematical concepts are a subcategory of basic physical concepts. It is harder to get from mathematics to physical fundamentals than from physics to mathematical fundamentals. It is the principal reason why mathematics lends itself better to abstraction than physics.

Yet, during a better part of this century, physics has been taking recourse to abstractions much more so than in the past Such occurrences have notably coincided with events referred to as transitions from classical to nonclassical physics. The terminology classical‘nonclassical is here taken to refer to an attempted extension of existing classical procedures to the realm of microphysics. Such extensions were found to be subject to not earlier noted limitations. Hence a discipline, initially thought to be complete, turned out be incomplete. If a more rigorous test of completeness were to emerge, one might hope that the artifact of a classical-nonclassical division might become a thing of the past.

At this juncture Planck, Einstein, Bohr and Sommerfeld had given sundry formulations of those restrictions, which finally culminated in what are now known as the Bohr-Sommerfeld relations. The latter imposed discrete quantized values for the action integrals of analytical mechanics, whereas before they could assume a continuum of values. This scholarly and logical approach met the best traditions of working from physics to mathematics. Yet this codification of the nonclassical quantum conditions imposed by nature was still believed to be wanting.

Others, Heisenberg, Schroedinger and Dirac now decided on a far more radical path. This new generation of pioneers followed the seemingly less promising, yet somehow inspired, process of seeking maps from the mathematical realm into the realm of physical fundamentals. Two major themes can be identified as having dominated this transition.

Heisenberg started out with a mathematically most demanding proposition of seeking a radical transition from the realm of continua to that of discrete objects. It took the vast mathematical experience of Born to translate this program into a mathematically feasible *eigenvalue* procedure for calculating stationary states of real physical situations.

By contrast, Schroedinger, older, with a wider realm of mathematical experience, found an *eigenvalue* continuum that could produce the discreteness needed for the problem at hand. Here is a rare case of two mathematical procedures describing the same physical situation. A result which seemed to question the earlier proposition of a one‘one map from physics to mathematics. A subsequently proven mathematical equivalence rescued the thesis of a single path from physics to mathematics.

The Heisenberg-Schroedinger eigenvalue process became one of the most widely applicable calculational procedures known in physics. It gave an answer to many pressing questions, without really yielding a commensurate insight into the fundamental nature of the quantum conditions. The procedure retained the status of a recipe. In fact, it was one of those rare occasions in which a mathematical proposition created a fundamental piece of physical machinery, without really revealing the fundamental physical counterparts that are believed to be needed for a derivation of that machinery from entities that have a more obvious first principle character. In all those years hat derivation has not been forthcoming.

After this inverse procedure of working from mathematics to physics had shown such extremely promising results, it could be expected that this path would be tried again. From the infinities of QED to the quarks' eight-fold group structure all the way to String theory, modern physics testifies to a trend of abstractly adapting all of physics to existing mathematics. While there have been results, there are also signs of a diminishing return from this reversal of the path of inquiry.

The here depicted method of creating machinery, without finding out what makes the machinery tick, is a major dilemma in the state of modern physics. A sentiment has been spreading in the physics community that man has approached natural limits to what he can know about nature. The Copenhagen view of the Heisenberg uncertainty relation definitely reflects such sentiment and has created an attitude that from now on physics ought to be prepared to settle for those limitations.

While there is no question that Schroedinger's equation constitutes a fundamental piece of machinery, the very recipe Schroedinger gave for obtaining his wave equation still tempts beliefs in an ultimate reducibility to simpler first principles; even if there is presently no consensus as to how. The fundamentals are believed to be hidden. The Copenhagen attitude sort of intimates that that is all man can hope for. Others, not convinced that Copenhageners have an exclusive option of reading the mind of God,* are not settling for such compromise. They hold out for a further reduction to fundamentals.

In carrying further this assessment, at least some sort of criterion is needed as measure of fundamentality of physics-related assertions. It is well to consider a measure of irreducibility of basic law statements under consideration, because irreducibility reveals some sort of primordial quality. Such irreducible elements lend themselves best to be used as general building blocks from which more involved reducible entities, say the Schroedinger process, can be constructed.

Since the need for nonclassical quantum corrections first showed up after extrapolating macroscopic laws to microphysical situations, a structural feature signalling the absence or presence of incompleteness in macro-micro transition would be most helpful. In the light of what has been said in previous sections, the metric is found to have a major role in making those distinctions, because it is the one and only criterion in physics for making an effective macro-micro distinction.

Mindful of the earlier discussed extensive efforts in the traditional mathematical descriptions of physics of making the metric invisible, it is now evident how this trend has obliterated aspects of the classical-nonclassical schism. Since such distinctions can't be denied, they take us by surprise and come in through the back door to haunt us. Having thus shown some of the ways in which we can mislead ourselves by going the inverse path, let now be the time to make a long story short. How do we disentangle the labyrinth encountered as a result of using the inverse probing from mathematics to physics?

Reversing the path, it becomes necessary to conclude that the Schroedinger process describes an, in experiments frequently occurring, compound situation: a randomized ensemble of identical elelments. The latter does not qualify for the irreducible status of a basic primordial law. Schroedinger statements now become reproducible as averages of randomized (perfectly classical) statistical ensembles of elements that are found to be irreducible. The discrete quantum states of the irreducible elements are governed by the earlier discussed (primordial) integral statements I,II..VI encountered in the second section of this chapter; they cover the Bohr-Sommerfeld relations as a special case for point-particles. The easily establishable Diffeo-4, metric-independent status of these integrals justifies their applicability in macro- and microdomains.

By contrast, the Schroedinger equation, or the Dirac equation for that matter, can be given a Diffeo-4 status; as has been done several times. This Diffeo-4 status is, however, contingent on retaining the metric structure. Since the avoidability of metric structure in establishing quantization for the ensemble

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* Ironically Bohr accused Einstein of having such exclusive information (God does not play dice). All of this shows: such risks do come with the territory!

elements has now assumed a crucial position for extrapolations to the micro-domain, the remaining role for the metric would have to relate to the ensemble nature of objects described by the Schroedinger process.

A Frank Assessment of Mathematical Physics In this Century

The preceding has been a preliminary assessment of the consequences of certain customs lingering in contemporary physics. A major point of concern is the remaining reference to physical fields on an SR(3) basis as compared to an *ab initio* Diffeo-4 referral. The outcome of these discussions is mostly a change of emphasis as to what is fundamental and what is derived or implied in physics.

Customary renditions of quantum physics present Schroedinger's equation as a fundamental and primary source of information and the law statements I,II,.....VI are regarded as inferred.

The option defended in these chapters proposes an alternative that **the quantum law statements I,II,...VI are fundamental, whereas the recipe origin of the Schroedinger equation suggests a derived ensemble status in physics.** There are good arguments to consider the Schroedinger equation as a tool exclusively describing phase and orientation random ensembles of identical quantum systems.

An experimental justification for this reversal in emphasis is to be found in the ten decimal place precision of h and e measurements ensuing from Josephson and quantum Hall effects.

By contrast, measurements of h and e inferred from Schroedinger and quantum field theory have shown a scatter of values affecting the fourth and fifth decimal places.[*] These are facts of life that cannot be denied. The SR(3)-based field tradition has been a factor in maintaining an indecision of priorities as to what is fundamental. Notwithstanding this uncertainty of what comes first, the Lamb shift and the anomalous magnetic moment of the electron and muon became the very important triumphs of SR(3)-based field theory.

From that time onwards, physical theory of the particle domain assumed a change of course. Group theory became a guiding principle to create order in the abundance of particles discovered in the world's high energy accelerators. This led to an hypothesized existence of fundamental building blocks named quarks. **At this time, it is not clear at all how many quarks have been hypothesized and how many have been discovered.**

Starting in the Seventies, a next wave of physical theorizing has invoked the concept of *strings.* It has now become known as String theory, perhaps later superseded by Superstring theory. **So far neither version of string theory has led to a contact with experimental physics.** It should be mentioned that the concept metric-free has also surfaced in String theory. Yet, no contacts are made with earlier work of the Twenties and Thirties by Kottler, Cartan and van Dantzig (KCD), which now is *physically relevant* .

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[*] Cohen and Dumond, Revs. Mod. Phys.,**37**,593(1965)

The Cartan-de Rham view is crucial in unveiling the metric-free, Diffeo-4, global features of integral law statements I,II,....VI, thus **leaving quantum gravity in a very illusionary position.**

One would have hoped that string theory could have made the KCD connection. Instead, string theorists have been adding insult to this injury of omission they inflicted on their own topological aspirations. String theory has been reviving the experimentally unsubstatiated magnetic monopole. **Magnetic monopoles have been incompatible with established laws, ever since flux quantization has been experimentally proven to exist in the Sixties.** Flux quanta are described by the (London) Aharonov-Bohm law II. The 2-form ensuing from this L-A-B one-form by exterior derivation is by definition **exact**, hence no magnetic charge residues: *i.e.,* no magnetic monopoles!

Moreover, electric monopoles can be shown to be enantio-morphic pairs, which means their 2-form is **impair** in the sense of de Rham. However, the alleged magnetic monopoles derive from a 2-form that crystal physics has identified as definitely **pair,** hence it misses this feature of enantiomorphic "pair" production. It is now incumbent on the magnetic charge people to invent a new pairing principle; they never did! Since de Rham's notions of **pair** and **impair** are contingent on manifold orientability, we are here confronted with ramifications of the orientability neglect of physics' favorite vehicle of mathematical communication: vector analysis.

Here is direct and clear evidence how unawareness of the physical implications of distinctions such as **closed** and **exact** as well as **pair** and **impair** leads to situations of wanting one's cake and eating it too; even among those who should have known better.

The sequence from quantum fields to quarks and strings unfortunately manifests an undeniable decline of physical relevance. A partial answer *why* has been alluded to in previous discussions. The perpetuation of SR(3)-based field concepts (even in the Diffeo context of relativity) has become a major obstacle standing in the way of bringing into cognizance physical implications of the global concepts conveyed by ***closed*** and ***exact*** as well as ***pair*** and ***impair*** distinctions between differential forms.

Experimental physics can claim several high points over the last half century from Mössbauer effect to Josephson and quantum Hall effects.The Josephson effects were the fruit of systematic exploration, somehow anticipated. The other two were products of serendipity. Their descriptions can be strikingly accommodated by the integral laws II, IV and VI.

The contemporary mathematical descriptions of physics reveal shortcomings of communication in physics itself and with the neighbor discipline of mathematics. It is rather amazing that there are not more published accounts of mathematicians pinpointing deficiencies of prevailing mathematical treatments in physics. Somehow mathematicians today either throw up their hands in despair about what physicists are doing or they passively accept and elaborate on their concoctions if funding comes through physics channels. Things were different at the beginning of this century. The following true story illustrates how years ago mathematicians were less afraid of physics related initiatives and criticism.

In 1915, Einstein consulted Hilbert about problems he had in obtaining gravitational field equations to replace the Newton-Poisson equation. After having been briefed by Einstein, Hilbert went to work and went ahead publishing an elegant solution to his visitor's problem. A few weeks later Einstein found the same result in a much less elegant manner. However, Einstein also published his first order solutions to the field equation. Unlike Hilbert, the physicist knew its solutions before he had the equation.

Hilbert, by contrast, drew a faulty conclusion from the equations he had so well derived. After having been called to task about his error by the mathematician Felix Klein, Hilbert admitted to his error, and then proceeded making a *famous conjecture restricting the law of energy-momentum conservation.* The existence and nature of this restriction was later substantiated by another mathematician, Emmy Noether. Quantum field physicists later took a liking to quoting Noether's result, yet without mentioning Hilbert's restriction.

Another example of interdisciplinary crossfertilization was given by an astronomer by the name Schwarzschild. He found an exact solution of the new field equations of gravity.

These examples testify how people of diverse background can do more together than in isolation. In comparison, the current scene conveys more chauvinistic attitudes. Over the past decades too many mathematicians could be heard priding themselves on their ignorance of physics. Physics can't afford this snobism. No independent initiatives, say comparable to those of David Hilbert and Felix Klein, can be expected on that basis. The for ever recurring **magnetic monopole** is living testimony that such narrow specialism condems mathematics to an unworthy subordinate role obscuring rather than opening new horizons. Better levels of mutual inquiry and criticism are needed to restore a more balanced exchange between the queen and king of sciences.

Schroedinger and the Copenhagen View

A preliminary word, about a good option on a more promising reassessment of contemporary quantum mechanics, is now in order. It pertains to the question how the present observations affect the interpretation of quantum mechanics:

I. The lack of hospitality shown by Schroedinger and Dirac equations to accommodate, in a meaningful manner, Diffeo-4 requirements of frame-independence rules out a fundamental status based on first principles.

II. The three quanta counters of flux, charge and action do have perfect, Diffeo-4 frame- and metric-independent properties. They qualify to assume a primary status as fundamental laws. *Their close association with high precision measurements of the quanta of action and electric charge strongly supports that position.*

Let the Schroedinger recipe for obtaining his wave equation be the guide in the now following exposition. Consider the Hamiltonian H of a compound system, with particle positions given by the radius vectors $(\mathbf{r}_k)_l$, with k numbering the particles in a single system and l numbering systems in an ensemble. The independent variable S be the action of the Hamilton-Jacobi equation, it relates to ¡ through the Aharonov-Bohm one-form. The logarithm of S is defined as the wave function ×. Then optimize this modified hamiltonian with respect to × and behold, Schroedinger's wave equations emerges.

A major question is the role played by the variational procedure. The S‘× modified Hamilton-Jacobi equation has a solution manifold spanned by the integration "constants" of its general solution. The variational procedure selects a family of integration constants that give H an optimum value. The quantization ensues from square integrability and single valuedness of ×. Can this picture bring us closer to an interpretation of the Schroedinger equation?

Now consider the closed orbits of a single system planetary motion:*i.e.*, k=1. The quantization of energy and angular momentum selects elliptical orbits tangent to two concentric circles in a plane. The orientation of the plane of this family of ellipses, and the position and phase of the latter, constitute remaining integration constants. The Schroedinger angular momentum number $\sqrt{n(n+1)}$ can be shown to be an orientation average of an ensemble of planetary orbitals (compare the *Feynman Lectures* for a proof).

The next simplest case of a two electron compound system (helium) k=1,2. Instead of a simple spatial averaging, there is now an averaging in a six-dimensional phase space for both particles. The impact of Heisenberg exchange is assessed by first regarding the orbiting electrons as independent, which reduces orbital behavior to the previous case. Electron spin taken in the perspective of the Pauli principle already delineates para-ortho helium features. Next a Coulomb interaction between the two electrons is injected as a perturbation to approach quantitative data in successive steps. This procedure has been one of the major triumphs of the (Pauli and spin) extended Schroedinger process, yet by the same token, it also shows that it is not purely an ensemble tool, in addition to quantization, Schroedinger rules on other internal behavior of compound systems.

For isotropic radiation one may consider the harmonic oscillators (photons) of vacuum. They also correspond to a situation equivalent to k=1. The remaining parameter of interest is the phase of each oscillator. An averaging over phase, as indicated by the Schroedinger recipe, yields an ensemble of harmonic oscillators that has to retain an average energy ¡ø/2 per oscillator if the ensemble probability is to remain positive (Planck, 1912).

The Schroedinger process is in these examples revealed as a hybrid statistical tool pertaining to an ensemble and the mutual particle relations inside the single systems of that ensemble. Seen in the cited Hamilton-Jacobi perspective, there are integration constants pertaining to the ensemble aspect of the identical systems, whereas other integration constants pertain to the system itself in case the latter is of a compound nature. The variational recipe so becomes a structural optimization pertaining to an ensemble and the single systems therein, similar to the more familiar dynamic extremum creating a most probable velocity distribution.

In recent times there have been efforts of reassessing the helium atom again from a semiclassical point of view. These methods have been reviewed by Wintgen *et al.*[*] There now is an overriding picture emerging from these discussions indicating that all statistics invoked by Schroedinger's process are essentially classical in nature. In fact, identifying the nature of this hybrid statistics helps greatly in understanding the physical nature of the recipe that led to the Schroedinger equation. That understanding is now getting closer to completion, which means **one of these days the Schroedinger recipe may be graduating to become a real derivation.**

The Schroedinger equation viewed as a derived object elevates the Aharonov-Bohm integral into a first principle position., because the Hamilton-Jacobi action function S is the phase of × and relates to the differential one-form of Aharonov-Bohm and the Bohr-Sommerfeld energy-momentum one-form. Since single valuedness of × governs quantization, a relation to cyclic integration is evident. Pauli and the "build-up" principles now challenge new perspectives in flux closure (Aharonov-Bohm) and orbital closure (Gauss-Ampère).

An extreme case, different from any of the previous ones, is the quantum Hall effect. A two-dimensional lattice of Landau states is moving through the Hall sample. In the plateau states there is total order of orientation and mutual phase. So, *there is no statistics,* it means the quanta counting laws can now be applied without the intermediary of the Schroedinger equation. Outside the plateau state is a realm for Schroedinger applications, with an ensuing zero-point energy. *The transition from plateau- to normal state in the Hall effect is contingent on a build-up of zero-point energy and vice versa transitions to plateau states eliminate zero-point energy disorder.*

It appears **the statistical implications of the Schroedinger process indicate a perfectly classical origin.** This may help in staying away from some of the more fantastic wave function options that have been made in the course of time: *e.g.,* wave functions of the world and even of the universe. They are by-products of an undefined concept of nonclassical statistics.

More extensive discussions of a "first principle" status of the cyclic integrals are given in a recent monograph entitled: *Quantum Reprogramming* (Notes and refs.). The same text also covers detailed discussions of phase and orientation randomness in ensembles.

Physics Role in Kindling Mathematical Concepts

The Newtonian breakthrough in physics prompted the creation of calculus as a new mathematical discipline. So, it was the wider physical problem that brought on an emergence of a more narrowly specialized new branch of mathematics. Subsequent developments in calculus by the Bernouillis and Euler and many others were, in just about every step, accompanied by a multitude of physical applications of the Newtonian laws of mechanics.

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[*] D Wintgen *et al*, Chaos **2**,19 (1992)

The development of musical instruments in the 18th Century, from organs, woodwinds, horns and string instruments, is greatly indebted to a mathematical physical analysis of those instruments. The Fourier decomposition of wave motion is a direct offshoot of joint endeavors in the physical mathematical realm.

The next century saw new mathematical discoveries, many of them again prompted by questions asked in the realm of physics. Would Gauss have arrived at his ***divergence*** theorem, if he had not been preoccupied with the collective behavior of electric charges such as expressed in Gauss' law of electrostatic? His divergence theorem became necessary to establish the residue integral status of Gauss' integral law of electrostatics.

Similarly, hydrodyamics and especially Ampère's studies about the relation between currents and magnetic fields have led to what is now known as Stokes' ***curl*** theorem. Since moving charges are currents, the Gauss and Stokes theorems must be expected to be closely related. An awareness of this kind emerged in the last part of the 19th Century. It led to an extension of these early theorems, which is now known as the ***generalized Stokes theorem***.

The latter theorem can be viewed as a natural dimensional generalization of the principal theorem of the integral calculus. In the hands of de Rham the generalized Stokes theorem and its associated residue integrals have become a mighty instrument in topology. De Rham's early papers and some of his ensuing terminology are vivid reminders that Maxwell's theory of elctromagnetism was midwife to a sophisticated method of assessing topological structure. All of which brings to the 20th century groups of cohomology as a dual counterpart of the earlier Poincaré groups of homology.

Yet, speaking about groups what more natural illustration of the group concept exists than the groups of crystallography. The 32 crystallographic point groups may have been enumerated already in the 18th Century. The question is: could these diverse mathematical perspectives have come into focus at all without the early stimulation of assessing the physical world around us?

It may now no longer be necessary to venture an answer to the last question. Let us just be thankful to those earlier generations for not unduly limiting their fields of perception and specialism. Just a reminder: Discipline chauvinism is no help in widening horizons!

A Need for Imperfection

While we may be inclined to give some laws of nature a label of perfection, there are others that are known to be approximations. Is it possible to come to a more instructive subdivision of the two?

A delineation between the two categories can be made by assessing how they relate to the ordering principles of nature. Some laws are valid no matter what the conditions of temperature and no matter in what medium the phenomena take place. As long as we stay away from the realm of quanta, the Newtonian laws of mechanics and the Faraday law of induction seem to have that sort of quality. They are valid at any time and any place.

Depending on their denominational convictions, some physicists may even say these laws are still valid in the quantum domain, provided certain integration constants are permitted to assume only discrete values. If for purposes of classification the latter point of view is allowed to prevail, these laws can said to belong in the category of ***field laws***.

By contrast, there are the so-called ***constitutive laws*** of physical media ranging from free space to gases, liquids, solids and the particle, atomic and molecular constituents thereof. The theory of groups has plaid a role in classifying and identifying their properties, because the **medium properties remain invariant under all the symmetry operations of the medium.** By contrast, the **field laws have to remain unaffected by symmetry operations**, because if they were affected they could not belong in the field law category,

The field laws apply anywhere, any time, and the goal is to have them either in perfect or near-perfect form so as not to have them unduly restrict the constitutive laws. The constitutive laws only apply to situations as specified by that law. They can at best be expected to be near-perfect in their stipulated realm. The laws of crystal physics come perhaps closest to near-perfection.

Nature is home for the perfect as well as the near perfect. Faraday's law of induction may well be an example of **perfect** information; even if we don't know as yet its quantum ramifications. By contrast, the laws of crystal physics provide examples of **near-perfect** descriptions. Man, as a constitutive part of Nature, belongs himself by necessity in the category that can never be perfect.

This condition of being less than perfect is not to be regarded as a necessarily regrettable state of affairs. Without imperfection man would not be able to grow. Similarly as crystals grow around their imperfections, so does man.

Music provides probably one of the most elevated examples of how man can grow through imperfection. Unbeknown to many who make music, the creation of the Western musical scale and the ensuing emergence of polyphonic orchestration of symphonic musics became possible as the result of a purposeful introduction of a slight, but inviting, numerical imperfection. It became known as Johann Sebastian **Bach's well tempered clavier**. The piano key board is designed so that 12 quints equal 7 octaves. To achieve this compromise, every quint needs to be tuned lower by about one part in thousand.

The great compositions of Western music have all been erected around these imperfections of the musical scale. In fact, these great compositions became possible by virtue of its well chosen tuning imperfection. It is based on a general experience that appropriate combinations of tonal intervals appear to be capable of carrying a gamut of different emotional sentiments. This general experience seems to be universal for all people, except perhaps those who are tone deaf.

The composing genii of the world discovered how combining rhythmic beats and polyphonic sound became a mighty instrument for reaching out to the soul of mankind. It was said, Franz Liszt conducted subtle flirtations with his female students by changing the key of musical themes he played on his piano. His transpositions of key were aimed at adapting to the personality of the girl to whom it was addressed. Bach, of course, tapped entirely different realms of human emotion and then so again did Beethoven.

Music shows how great things can be created around a compromise that is essentially an imperfection. Let this example not be an invitation to deify imperfection. It is the special character of imperfection that remains after valiant attempts of aiming for the perfect. Imperfection so acquires an intrinsic quality. If perfection is the goal, imperfection provides the dynamics in approaching that goal. Somehow man is always simultaneously dealing with a mixture of these inseparable concepts. In music, it was the decision of learning to live with the unavoidable imperfaction of he musical scale that opened up a wonderful world of emotions that was much wider than what could possibly have been evoked by the monophonic musicianship of the middle ages.

EPILOGUE

What has been learned now that this exercise of viewing physics in a philosophical perspective is coming to an end?

Those, who say they are now more confused than before, let them be reminded that most of us have been addled all along. Man has capabilities of adapting to his own states of confusion, while fiercely condemning those of others. In that vein, these discussions may help in creating fuller awareness of how addled we all are. Problems need to be recognized, before they can be worked on.

Those who, after reading these lines, claim they have not learned a thing, may consider that they have at least gained a mnemonic device for the purpose of retaining parts of that vast body of knowledge, called modern physics. An overview of the totality is needed to pass judgment on details. Once an overview exists, the process of reevaluation can easier take its course.

Finally there is, we hope, a category admitting to pholosophy's potential for ordering, if physics is viewed against the background of its subcategories. Whatever has been learned is then found to be contingent on the use of a more discerning mathematical machinery, which makes crucially needed distinctions explicit from the start. **Existing methodology in physics is predicated by three almost silent assumptions**:***i.e.*****, all of physics can be constructed working from local towards global, SR(3) field definitions suffice, orientability can** ***mostly*** **be ignored.**

Despite proven relevance of the general theory of relativity, existing mathematical vehicles remain prone to hidden assumptions testifying to a universe dominated by residual elements of Euclidian uniformity. While *locally* this can be very close to the truth, such presumed uniformity in the large remains a stumbling block for gaining viable *global* perspectives on cosmology. By contrast, ***in the small,*** **the quantum laws of physics yield global insight into the topology of microphysical structure.** Unlike mathematics, in physics the concepts *local* and *global* are not quite identical with the expressions *in the small* and *in the large*.

There is virtue in being reminded, at every step, that **orientability counts** and that **Euclidian uniformity cannot be taken for granted**. The use of formalisms that compel confrontation with those issues helps in reassessing decisions of the past that might have been obscuring a more discerning future.

If questions do arise whether here suggested reassessments are worth the effort, keep in mind, thanks to those built-in mnemonic devices, it has been possible to track down details of Copenhagen's obscure ontology and the ensuing premature rejection of a truly royal road to quantization. Even Sommerfeld, a principal originator of this royal road, abandoned his own creation, so great was the magic lure of the Schroedinger-Dirac methodology. Einstein, who saw the topological features of the Bohr-Sommerfeld integrals, also made an effort to reconcile the Schroedinger-Dirac equations with the precepts of general covariance.

Transitions from local to global description invariably place emphasis on degrees of frame-independence in physical description. These requirements need to be applied discerningly. Past experience with Dirac's equations remind us how easily indiscriminate application of frame independence can lead into a cul de sac. The degree of frame independence has to fit the object of description at hand.

Good judgment is needed to make the calls where and when to demand Diffeo-4 treatment. It would be silly to demand it for the description of crystal symmetries, yet the Faraday induction's universal applicability clearly demands full Diffeo-4 independence. Yet, the induction law is rarely mathematically treated that way, even if its local applications clearly support a Diffeo-4 relevance.

If promises of earlier attempts at replacing SR(3) vector analysis by Cartan's Diffeo-4 differential forms have not met with expectations, the reason is frequently that Diffeo-4 status cannot be dictated. **Nature, instead, has to be asked under what circumstances a Diffeo-4 status is appropriate.** Increased 'specialism' in mathematics as well as physics have compounded the possibilities of bridging a widening gap in communication.

Just having a better vehicle of communication in mathematics alone can't swing the balance. If meaningful results are to be expected, a more subtle understanding of how this vehicle relates to physics needs to go hand in hand with knowledge of the vehicle. Differential forms have been taught in a manner of learning the outward manipulations of a Diffeo-invariant system without effectively touching on the deeper needs of physics. It is a reality of life that having a better violin is a necessary but not a sufficient condition for making better music.

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