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PHYSICS - PARTICLE PHYSICS

AN UNSETTLED AND UNSETTLING RELATION*

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1. Motivation

It might well appear that the subject matter suggested by the title of this note has little to do with the perplexing problems' physicists are now facing or the continuously changing fortunes of the Superconducting Super Collider (SSC). These particular problems seem to be part of the role this society has assigned to science and to physics. The decisions reached and the ultimate dispositions of these questions appear to be primarily determined by political and financial considerations indicating that problems within science in particular the relations between various disciplines and sub-fields, have little or nothing to do with these issues.

Even though there is no doubt that political and financial concerns will play a major role in arriving at a definite conclusion, it is the purpose of the present discussion to call attention to a number of systemic problems within science and physics that have contributed greatly to the present unsettled and unsettling situation. It is of course nothing new that in the last 60-70 year's science has become increasingly specialized but it is not always realized how the continued operation of these centrifugal tendencies has changed the interrelations between various scientific fields and sub-fields. This extreme specialization has led to an intellectual and semantic fragmentation that makes meaningful communication extremely difficult. Ever more serious is the succeeding phase where a marked alienation between various fields and sub-fields tends to develop. Science becomes increasingly disjointed as it splits into separate sub-fields. Communication becomes almost impossible and it is also no longer felt as desirable. These tendencies are general. They do not just operate between distinct scientific disciplines such as chemistry, physics and biology but they are equally strong within the individual fields. Not only have the nature and structure of science changed radically, but so has the role of the individual scientist. Her (his) intellectual and often emotional allegiance is no longer to a particular science, but to a limited and restricted sub-field. The technical demands necessary to make scientific contributions on a truly professional level require such a deliberate limitation. The relation between science and society also has undergone enormous changes. As Society supports science to an unprecedented degree specific and visible results are expected, even demanded.

The interrelation between scientists his (her) scientific fields and specialties and the Society have become extremely tangled and complex. The precise relation between distinct scientific disciplines and between a given field and its various sub-fields is not discussed very often especially not in a "resource neutral setting."

This paper is devoted to a rather low key discussion of the scientific relations between disciplines and sub-fields. Such a discussion is not particularly simple to carry out in a fair and impartial manner. It is necessary to compare the importance and promise of one field with another. The stagnation in one area must be contrasted with the advances in another. The scientific promises in one area need to be compared to the expectations of technological advances in another. The purely scientific aspects of such a discussion are hard enough but the results achieved depend also in fact crucially on the financial support the society provides. Consequently all conclusions tend to be tentative and are always subject to revision and reconsideration. The main point of this paper is to show how the enormous and fabulous advances of science have by their very success created divisions and problems. Furthermore this development of science in separate almost autonomous effort (which in this paper will be referred to as independent scales or levels) is an almost inevitable concomitant of both the scientific advances and their social relevance. It is not claimed that the ideas expressed here are deep or a sign of profound thought; however, the issues raised are important for science, scientists and science education. They may be controversial at times but important questions should be discussed rationally not decided by fiat or default. These comments are the exclusive responsibility of the author. No person, group, organization, laboratory or educational institution deserves any blame.

2. *The Reactions to the House Action of June 17, 1992*(F.N.)

F.N.: This section was written before the full funding for the SSC was restored by the senate. As of this time the house and senate have not reached a joint decision. In spite of this change this section was not rewritten -- it would appear that the problems and concerns are not materially altered by this positive note. The subservience of scientific projects to political pressure stressed in this section is amply verified by these actions.

The house of representatives voted on June 17, 1992, to stop the further funding of the SSC. By this action taken with a vote of 232 to 180 the house effectively canceled the project. It was not a postponement or a down scaling of the project; $34 million were earmarked for a decent burial. To most physicists this came as a totally unexpected shock. Only a handful of physicists had maintained that in spite of many promises, a substantial investment and a major effort to reorient high energy physics toward SSC- related activities, the fate and future of the SSC remained in doubt. These people referred to a house vote in the summer of 1991 where a motion to cancel the SSC was defeated by 86 votes as a cause of concern. However those who expressed doubts and concerns were a distinct minority. They were regarded as professional pessimists, lukewarm supporters of the project, or behind the time. Consequently the shock and dismay about this abrupt cessation was that much greater.

Most high energy physicists were angry and deeply disappointed. There was a sense of betrayal. Many young and semi-young physicists had in good faith reoriented their careers in the firm belief that the SSC was the most promising new research direction. They were convinced that the country and the government had made a firm commitment to provide the resources commensurate with the scientific importance of the project.

Of course everybody was fully aware that a project of this magnitude would involve the government in an essential manner. That there would be criticism, debates, vacillations, delays and reconsiderations are in no way surprising. What is surprising in the house decision is its suddenness and the angry acrimonious debate that preceded the vote. Even in the justification of the cancellation by various house members given later (when tempers presumably had cooled) the snide, condescending attitude towards supporters of the SSC was maintained. One house member dismissed the arguments in favor of the supercollider as "the wailings coming from the self interested champions of the supercollider" (*New York Times*, July 12, 1992). This is hardly a statement that inspires confidence that difficult decisions are obtained by rational, thoughtful examination.

It is extremely difficult to predict the future and the future needs of science but it is precisely the long range possibilities that are at issue when decisions regarding major projects are made. So it is not too much to expect that such decisions are made with great care and circumspection and painstaking regards for the scientific and technical needs. It is disturbing that the agencies and individuals charged with making the final decisions seem to have little or no appreciation of the actual operation of science. It is impossible to predict the outcome of a scientific investigation. It must be planned and yet be opportunistic; goal directed, yet flexible and capable of change. That is why a continued thorough scrutiny of the scientific reasons for the projects is essential. The final justification for any major project must be the scientific and technical needs it fulfills, the promise of future developments, and the risks inherent in missing great opportunities.

Even so it would be hopelessly naive to believe that scientific or technological projects that require enormous expenditures, take a great deal of time and are bound to have a major impact on the economy of a region, would not become embroiled in political controversies. It is therefore understandable that many extra scientific factors will play important if not dominating roles. Many discussions, such as the location of laboratories and facilities, are determined by regional rivalries, personal animosities, political expedience and private ambitions. That is far from ideal, but probably the only way in which the society can responsibly support science on a continuing basis. To be effective, once the necessary compromises and accommodations have been made there should be a mutual understanding and a mutual respect for the commitments made. It is for that reason that the sudden termination of the SSC by the rescinding of earlier commitments is having a chilling effect on all scientists who depend on governmental agencies for support. To initiate and eliminate major projects like the opening and closing of a Broadway play is capricious and utterly irresponsible. Such actions will inevitably and very rapidly lead to a complete demoralization of the scientific community. It will undoubtedly lead to an understandable reluctance for new young people to enter as demanding a field as science when the necessary support is so tenuous and capricious and cannot be trusted. (Even if the funding for the SSC is restored, a feeling of concern and suspicion will most likely remain).

The almost inevitable conclusion to be drawn from the sudden cessation of the SSC is that, for whatever reason, the United States no longer aspires to be one of the leading scientific centers in the world. The anxiety and concerns this produces goes beyond the SSC, particle physics and physics. No science can function very effectively or for very long if the necessary support can be canceled at any time for what are presumably persuasive political reasons. This is a deeply worrisome by-product of the decision to cancel the SSC. It should be a major concern of the society, affecting as it does scientific research, technology and (although often not realized) education.

3. *The Reaction of the Physics Community*

Even though there is general agreement that the method and timing of the cancellation of SSC was deplorable, it would be quite wrong to infer that within physics there is unanimous, let alone enthusiastic support for the SSC. In physics itself the support of the SSC was not very strong. It was never very broadly based. A number of outstanding and influential particle physicists tried extremely hard and with great skill to persuade the physics community that this project was essential for the continual preeminence of the United States in particle physics. The proponents fully realizing the decisive role of national politics also embarked on an ambitious political program. They appeared to have been successful; the project was supported by the necessary agencies and included as a major item in the science budget. Even so the physics community was never totally convinced of the necessity, or even desirability, of the project. In political terms the support was soft.

The high energy and particle physicists are organized as a division (called Particles and Fields) of the American Physical Society (APS). The APS has about 46,260 members, the Division of Particles and Fields has about 2800 members (actually 2100 members who are paying their dues). By contrast there are about 6000 members in the Condensed Matter Division while nuclear physics, chemical physics, atomic and optical physics has 2400, 2300 and 2600 members respectively. The number of physicists directly involved with the SSC is at most 7%, not quite half as many as in the largest division and comparable to three other divisions. It is therefore not surprising that the American Physical Society reacted in a rather guarded and tentative manner to the possible cancellation of the SSC. At a meeting of the Executive Committee of the APS in Seattle (July 4-6, 1992) they took the position that the SSC should be continued. They argued cogently that stopping the SSC now would mean that the United States had given up the leadership role in particle physics, casting serious doubts on its interest in supporting basic science. This appears as thoughtful and balanced support by the representatives of the physics community. However, in objecting to the abrupt cessation, they urged that the project should be continued "without diminishing the support of other physics projects." This is precisely the problem within the physics community: Obviously the scenario suggested cannot be implemented without new money. This weakens the APS endorsement to such an extent that it becomes practically useless in a political confrontation. This was duly noted by a member of the house (a democrat from Kansas). Weak as the support of APS for the SSC was, it is most likely the strongest statement the Executive Committee could confidently make. If the 46,000 members of the APS were polled individually it is not certain that even this sympathetic if somewhat equivocal statement would pass.

The actual situation is much more tenuous. Two respected, previous presidents of the APS (Nicholas Bloembergenn, a Nobel Prize winner, and James Krumhansl) were less than enthusiastic about the SSC. Bloembergenn objected to what he considered as exaggerated claims of the U.S. Department of Energy (DOE) that high energy physics had been instrumental in producing magnetic resonance imaging spin-offs. (This is unfortunately a rather typical occurrence that will be analyzed in more detail later. The high energy physicists making these claims should have known that other physicists are sensitive about receiving proper credit and recognition for their contributions).

In this most sympathetic mode Mr. Krumhansl referred to the SSC as "a heroic but specialized experiment." At other times he was considerably more negative. As thoughtful and brilliant a physicist as Freeman Dyson argued strongly against "big science" in general and against the SSC in particular in a lecture given in 1988 ("Six Cautionary Tales for Scientists"). While Bloembergenn, Krumhansl and Dyson were either lukewarm or opposed to the SSC, they are by far not the most militant opponents. Some eminent physicists, including several Nobel prize winners, angrily and violently opposed the construction of the supercollider.

Independent of the validity, wisdom or foolishness of these views, such widespread and public antagonism against an extremely expensive project, is a crippling handicap in a highly visible tough political fight. This immediately raises some obvious, important and rather puzzling questions.

Why is there, or why does there appear to be, such a deep schism within the scientific community? Why do scientists in specific areas have so little interest in — sympathy for — tolerance of — different scientific activities? Why do presumably intelligent, even brilliant, scientists exhibit such antagonism and anger in dealing with different and contrary scientific views?

Is this long standing separation a systemic problem of contemporary science (as was suggested in the first section) so that the conflicting views toward the SSC merely represent an exacerbation of a general situation?

The answer to the second question is certainly yes, the first question is the central one. The next section (probably the most important one in this paper) will attempt to explain the reasons for the deep rifts which separate one science from another and which cause such deep divisions and crass dissonances in the scientific community.

4. *The Gradual Loss of Coherence*

<u>A) The Self Image of Physics</u>

All sciences exhibit a complicated mixture of two distinct and complementary trends. One is the tendency to study specific phenomena, to investigate particular systems. The other is the search for broader, more general laws, more universal principles. It was in physics that the general laws made their first appearance. Through the studies of Copernicus, Galileo and especially Newton the laws of motion were shown to be of great universality, applying to terrestrial motion in exactly the same way they govern celestial motion. Although well known and totally accepted now, this generality and universality came as an extraordinary surprise to the public of the 17th and 18th century.

This search for general and universal regularities continued in the 19th and 20th century. The discoveries of general laws of thermodynamics, electrodynamics, quantum mechanics and relativity all stressed the existence and importance of general laws. At the same time, the properties of individual specific systems continued to be investigated. The conductivity of solids, the flow of fluids, the characteristics of gas discharges and spectra of particular atoms or molecules is examples of such studies.

There is a continual, almost contrapuntal relation, between the development of general laws and specific phenomena. Sometimes a particular result was found to be a manifestation of a general law. Sometimes a particular effect was found to contradict a law that previously had believed to be universal. This dichotomy between the search for generality and the understanding of highly specific behavior occurs in all sciences. It was by the study of the properties of particular organic molecules that the notion of stereo isomerism developed in chemistry. The detailed comparison of the anatomical features of many different species was instrumental in suggesting a general law of evolution in biology.

Another generally unarticulated feature of this dichotomy is the implication that the search for general universal laws is more important and certainly more fundamental than the investigation or discovery of individual phenomena. (Unless a new phenomenon contradicts a general law or leads to a new general law!)

The discovery of the laws of chemical equilibrium by Gibbs was presumably more important than the measurements of the heat of formation of a class of compounds. The calculation of the viscosity of a particular gas, while possibly difficult, is not accorded the same scientific importance as a derivation of the Maxwell-Boltzmann distribution.

There is now a widespread belief that in physics most, if not all, of the general universal laws is known. All that remains to be studied to make physics a presumably complete subject is to identify, classify and enumerate the entities to which these general laws apply. This was (and is) primarily an experimental study that has been spectacularly successful during the last 60 years. With the discovery of atoms, electrons, nuclei, protons, neutrons, mesons and quarks these investigations have not only led to a deep understanding of the world, but have produced a remarkable technology in the process. Obviously certain important questions remain. For example whether new basic principles exist which would explain the repetitive occurrence of very similar particle families. There is a widespread belief that the basic outline of physics is well known (possibly even completely known) and well understood. The general laws and the basic organizing principles are believed to be known. They need to be applied to the observed objects to obtain an understanding of the world and a satisfactory explanation of most phenomena. It might even be possible that a theory exists (or can be constructed) which allows the understanding of the properties of the basic particles themselves. Such ideas are taken for granted by most physicists: a knowledge of the constituents, their properties and interactions implies (by known or knowable laws) the complete behavior of the system and should yield the explanation of all phenomena. This set of ideas, to which many physicists subscribe, even if not explicitly articulated, constitutes a view and approach to physics that might be called the self image. This self image is the foundation for many of the attitudes' physicists exhibit toward their science. It is the purpose of the next section to demonstrate that this self image, while not exactly wrong, is seriously incomplete and often extremely misleading.

B) The Level Structure of Physics

It might well appear curious that while in physics and in science generally, the tendency toward unification and unity has been pronounced and successful. The comments made in Section I stressed the extreme specialization, fragmentation and even alienation between fields and sub-fields. An understanding of the basis of this conflict is essential for a rational discussion of the goals and future of scientific research. An instructive example of how this dichotomy of generality and specificity evolved is afforded by an off hand remark made by Dirac in the preface of his book on Quantum Mechanics (first edition 1930). In explaining why it was so important to write a book on quantum mechanics Dirac could hardly contain his enthusiasm: "Quantum Mechanics explains most of physics with the possible exception of nuclear physics and cosmic ray physics and all of chemistry." With this pronouncement Dirac delineated the problem. Dirac clearly meant that with the known constituents of the atom and the laws of quantum mechanics, all molecular properties should be calculable. So one may well ask, how come chemistry still exists as an independent, enormously active, thriving field with its own concepts and methods?

The basic underlying reason is that to be at all manageable any scientific investigation requires choices, limitations and restrictions. The initial step is the choice of the system to be considered and the class of phenomena to be examined. This amounts to a deliberate restriction to a chosen set of questions to certain topics, to certain levels of accuracy. In any such study, certain properties are stressed and treated in detail, while others are treated approximately and others are neglected altogether. Other choices, whether made by intent, default or instrumental limitations, are the numerical ranges of the relevant physical limit.

These choices, which precede or accompany the initiation of any research effort, select the systems to be studied; the methods, procedures and instrumentation to be used; the properties to be investigated; the accuracy required; the domains of the parameters; and the conceptual, theoretical framework presupposed. Together these define the "scientific scale" or the "scientific level." This notion is an extension, a generalization of the numerical scale, which specifies the values of the parameters. It will be clear that making all these selections is a conscious and purposeful limitation of the area of the problems to be studied.

Such levels provide a rather broad, diffuse and partially overlapping categorization of science. The basic assumptions, the conceptual setting, the type of arguments made, the nature of the explanations and interpretation, the purposes and the methods are all much the same within a level while they differ sharply in distinct levels. Of necessity each scientific level further involves simplifications, schematizations, idealizations and approximations that delineate the level still further.

A very simple example of two distinct levels is provided by the contrast between the study of hydrogen in the temperature range of 10^{-1} — 10^{-3} ^{o}K at the pressure of 1 atmosphere and another study of hydrogen at temperature 10^{12} — 10^{15} ^{o}K and densities of 10^{16} grams per cubic centimeter. The techniques, purposes, features and theoretical framework of studies in liquid hydrogen and neutron stars are wildly different, illustrating the enormous differences between distinct levels of the same system.

Similarly nuclear spectroscopy, high pressure physics, high temperature chemistry, femtosecond chemistry, dilute gases, pion proton scattering and quark physics are all sub-fields distinguished by different parameter values, different conceptual structures, different notions and different laws (regularities) with altogether different goals.

It is particularly important that in scientific practices these levels have acquired a considerable degree of autonomy. The research questions considered are phrased using the language and notions pertinent to that level.

This means that the approximations and idealizations implied by that level are automatically contained in the treatment and formulation of the problems to be considered. An "atomic level" can be defined in a rather natural way by using the customary ideas of atomic physics where both the electrons and nuclei are treated as point particles. The theoretical framework is non-relativistic quantum mechanics. This level gives an excellent description of optical spectra; the only nuclear property involved is the nuclear spin. The finite size of the nucleus is rarely included. Its neglect is inherent in the use of this particular level. (It would be necessary to include this finite size when dealing with a μ mesonic atom).

This example illustrates the idealization the use of a level automatically entails, such as the representation of a finite nucleus by a point particle. Another feature of a level is the approximate closure of a level. This means that the analysis of problems phrased in the context of a level can be carried out with the laws and concepts of that level. Thus in the analysis of optical atomic spectra the influence of the nucleus can be summarized with a few simple parameters (nuclear spin and the nuclear magnetic moment) but no further detailed nuclear features are needed.

It might appear that this concept of a level is automatically incorporated in the choice of research and the manner in which it is carried out. Furthermore the idea possesses an inevitable vagueness so that a further examination might appear unnecessary and even a little pointless; this is actually incorrect. The concept of a level, or scale, is essential for the understanding of the structure of science. It is of special importance in analyzing the intricate relations between distinct scientific disciplines and the equally subtle relationship between sub-fields of particular fields.

In an ironic twist, the level structure is responsible for both the extraordinary advances scientific specialization has produced and the loss of scientific coherence with the concomitant fragmentation and alienation between fields and sub-fields.

C) Some Examples Some General Properties of Scales

1. The Hydrodynamic and the Kinetic Scale

One of the simplest instances in which the level structure becomes particularly clear is in the analysis and description of the classical behavior of continuous media. The general class of phenomena considered is the motion of fluids; the forces those fluids exert on bodies immersed in the fluids. There are two distinct levels on which these phenomena can be analyzed. One is the hydrodynamic level that employs concepts such as densities, pressures and velocity fields. The behavior of the system is governed by the equations of hydrodynamics that are adaptations of Newton's equations. The characteristics of the individual fluids (such as density, viscosity, thermal conductivity) have to be determined from experiment. The hydrodynamic description as such does not fix the values of these parameters. Using such data and the solutions of the hydrodynamic equations, a remarkably accurate description of the behavior of fluids has been obtained. These questions can be discussed on the molecular, kinetic (or Boltzmann) level. On this level the concepts entering the description are the molecular properties (such as masses, sizes, sometimes shapes) of the molecules and their mutual interactions.

These molecular properties can not be derived within the Boltzmann level, but must be inferred from experimental information. The dynamic behavior of such a kinetic system is determined by the Boltzmann equation, which itself depends just on the assumed forces between the molecules and Newton's laws. For given molecular properties the Boltzmann level determines all possible behaviors of the fluids made up of these molecules (such as the flow patterns, the forces exerted on bodies, etc). Applying Dirac's dictum to this situation would suggest that it is possible, even desirable, to dispense with the hydrodynamic level altogether and describe all the fluid flow phenomena in purely molecular terms. It is indeed possible to derive some of the hydrodynamic features with the Boltzmannian idea. For example the viscosity of a gas can — making certain approximations — be obtained from the Boltzmann equation. This shows that the kinetic level contains more information (once the molecular forces are known)! than the hydrodynamic level. Another example illustrating this same aspect is the computation of the forces an object experiences when moving through a continuous medium. This is a standard problem on the hydrodynamic level — it can also be computed on the Boltzmann level. However the Boltzmann level also allows the calculation of the fluctuations in the forces. This is impossible within the hydrodynamic level where, strictly speaking, the forces should not fluctuate at all.

These observations do seem to indicate that the kinetic level is indeed more fundamental than the hydrodynamic level so that Dirac's dictum appears to have substantial validity. This is extremely misleading. Dirac's suggestion that the molecular level explains and describes all fluid flow behavior is reminiscent of oracle-like maxims that give no indication of just how such derivations should be accomplished.

To appreciate just how subtle the relationship between the hydrodynamic and the kinetic scales actually is, consider the phenomenon of shock waves. This is usually analyzed on the hydrodynamic level, supplemented by the equation of thermodynamics, without any reference to the molecular level. It is extremely doubtful that a purely mathematical analysis of the Boltzmann level (which should in principle contain both the hydrodynamic and thermodynamic laws) would have predicted shock waves and their properties. It is also far from obvious that turbulence is a conspicuous feature of fluid flow and thus the hydrodynamic level is interpretable or would have been predictable in exclusively Boltzmannian terms.

On the other hand there are observable manifestation of the molecular level that cannot be described on the hydrodynamic level at all. The Brownian Motion of a mirror suspended in a gas or liquid is an example. There are other rather delicate phenomena associated with the diffusion of a mixture of gases near a wall that can only be handled using the Boltzmann equation.

Even these brief remarks indicate already that the relationship between levels is rather delicate and quite difficult to asses in full generality. Each scale has a role to play in the elucidation, analysis and prediction of new phenomenon. Knowing from experiments and the hydrodynamic discussion that turbulence exists, it becomes an interesting (so far unsolved) question to understand on a deeper molecular level. Quite possibly such an understanding might point towards new, related but different phenomena.

On the other hand if it becomes necessary to describe the fluid flow in a more precise manner, by the introduction of surface tension's effect, it is much easier and probably much more effective, to start from the hydrodynamic level. (Surface tension itself is of course a molecular phenomenon.) It is not even clear how surface tension could be introduced on a strictly Boltzmannian level. (Even if possible it would be very clumsy).

A more extensive investigation shows that the level structure generally introduces two distinct, almost complementary features. The more fundamental level (in this example the kinetic level) can introduce novel, often unexpected, features and unanticipated interrelations, while the more macroscopic level (in this case the hydrodynamic level) is a more effective starting point for generalizations and the introduction of a new qualitative concept. This further stresses the complementary aspect of the level properties. Both the autonomies of the level and their interrelation are essential characteristics.

It must be emphasized that the level notion is helpful in analyzing the structure of science in understanding and appreciating the marked differences between distinct and separate investigations. In dealing with a particular set of questions a scientist will generally select ideas and methods often belonging to quite distinct levels. The choice of level is pragmatic and opportunistic. No scientist swears eternal allegiance to a particular scale. For a rational scientific comparison between rather different areas and for an overall analysis the level (or scale) notion is indispensable.

2. *The Chemical Scale*

With the ideas presented in this paper the magic words of Dirac's famous dictum are equivalent to the statement that the molecular level is completely derivable from the more fundamental atomic level. In other words, all the molecular properties and all the interactions between molecules can be derived from the known structure of atoms and (non-relativistic) quantum mechanics. As the earlier discussion already indicates, the relation between distinct scales is rarely that simple or unequivocal.

It is true that there was great excitement (in 1927) when Heitler and London showed very soon after the development of quantum mechanics that the new formalism gave a persuasive explanation of the covalent bond between two neutral hydrogen atoms. This gave the first explanation of the formation of a hydrogen molecule. Many physicists and chemists (van Vleck, Slater, Pauling) believed that this would signal a major change in chemistry (some called it mathematical chemistry) which would consist exclusively of a mathematical numerical treatment of the Schroedinger equation, appropriate to a particular molecule. All the chemical properties could presumably be obtained from such a numerical analysis.

These hopes and expectation have been realized in part, especially in recent times, through the extensive use of computers. Still there can be no doubt that chemistry has maintained its autonomy, it is manifestly no sub-field of physics. The reason for this separation is that each level evolves its own qualitative concepts, general ideas, language, approximations and schematization. In chemistry notions such as valence, coordination, number, directed bonds, resonance and structural formulas are of special interest and of great importance. The professionals (and this is true in all fields) have developed a remarkable ability to manipulate such concepts in a partially intuitive and partially qualitative manner that gives each level its distinctive style.

Some of those concepts eventually received an approximate formulation in terms of the quantum theory. This is satisfactory but even the incomplete qualitative concepts and the regularities that connect them are of extraordinary importance in the interpretation, understanding and prediction of chemical properties. The procedures and methodology based on these ideas gives chemical investigations their own particular cachet, which is what ultimately defines the chemical level (actually a series of such levels).

The approximate derivation of concepts from atomic physics is often not particularly enlightening and rarely relevant. The remarkable geometrical arrangements of large organic molecules (with long chains, side chains, rings and often intricate three dimensional configurations) are fundamental in analyzing the chemical behavior. However a derivation of these structures from the Schroedinger equation (which would have to be a *tour de force)* is unlikely and hardly relevant. It is extremely difficult to believe that such a derivation would give new chemical insights. (Such derivations would unquestionably be of intellectual and logical interest). It is the contrast between utility and applicability of the concepts of the chemical scale and the arcane derivation from a more fundamental theory that characterizes the separation between the levels.

Even this facile phrase "derivable from the Schroedinger equation" is fraught with ambiguity. What should the proper starting Schroedinger equation be? Should all the spin-spin, all the spin-orbit, all the orbit-orbit interactions be included? Can one neglect all relativistic effects? Are there instances where a quantum field theoretic treatment is needed? To show that such questions are neither silly, obvious or trivial, it should be noted that a non-relativistic calculation of ionization energy of iron leads to errors of about 100 eV (out of 1500 eV). For heavier atoms the error is substantially greater. In the most commonly used chemical (atomic) scales relativistic effects are usually omitted so a 7% error in such an ordinary physical (chemical) quantity is somewhat surprising.

This is only one instance that indicates great care must be exercised in declaring those certain features (relativity in this case) can safely be neglected. Another such unexpected result is that neglecting the retarded interactions can lead to serious errors in the calculations of absorption spectra in the 1μ range. Perhaps the least expected result is the quantum electrodynamics effects which have an influence on the vander Waals forces between neutral molecules. These examples illustrate an intermingling of levels. Relativistic or field theoretic features are not usually included in the chemical levels; however, these instances demonstrate that these features can infringe on a purely atomic chemical scale.

It will be evident that in a field as large and diverse as chemistry there will be many separate and distinct scales (all different from physics)! The treatment of molecules, containing anywhere from 1 to 5 atoms, is necessarily different from those containing 5 — 50 atoms, or 50 — 500 atoms. These distinct classes also exhibit very different kinds of behavior. For the large molecules the three dimensional geometric features are especially important. A representation as a rigid, or partially rigid, body with all the properties this entails (moments of inertia) is a much better starting point than a numerical study of the Schroedinger equation *a la* Dirac.

This brief and incomplete description of the chemical scales illustrates and confirms the complementary nature of scales. The qualitative notions impart a certain degree of independence and autonomy to the scales. Different scales are separated by different quantitative and qualitative features. The complementary aspect of the scales arises from the unexpected reappearance of effects that are manifestations of another usually (but not always) more fundamental scale. It is this complementary aspect which suggests that it is wise to use extreme circumspection in deciding categorically that one level can not affect and is not affected by another level.

3. *The Quark Level General Properties*

The investigation of the structure of matter has led over a rather prolonged time, through some curious detours, to the well known sequence of levels: the molecular, atomic, nuclear, particle (proton-neutron) and now the quark level. These successive investigations required progressively higher energy along with more refined detection apparatus. The last two or three levels are usually referred to as high energy or particle physics.

At one time each of these objects (molecules, atoms, protons, neutrons, mesons and quarks) were considered fundamental. "Fundamental" means that these objects are identifiable on their appropriate level; exhaustively described by a few parameters (mass, charges, spins, strangeness and a few others). Geometrically these "presumed fundamental" objects are treated as points. The internal structure is completely summarized by the values of the particle parameters. In terms of the ideas discussed in this paper, each one of these new layers of constituents corresponds to a new level. On the macroscopic scale a molecule is treated as a point. On the molecular scale the nucleus is a point, etc. Currently the only objects not assigned an extended geometrical structure is the quarks and leptons. They are assigned definite quantum numbers.

It will not come as a surprise that the different levels defined by the successive layers in the structure of matter have very different characteristics. The concepts, laws and the dynamics all vary sharply from one level to the next. But the radical changes which were revealed through the continued examination of the structure of matter came as a series of major surprises.

The discoveries of the proton-neutron force (the strong force) and the weak force were unanticipated events. Although nowadays all physicists are familiar with the four (independent) forces, many quite eminent scientists (Einstein, Dirac and Eddington) were very reluctant to accept the strong force and the weak force as coequal with the gravitational and the electromagnetic forces. That neither the weak force nor the strong force could be understood as a combination of the electromagnetic and gravitational force was a deep disappointment. This clearly demanded a radical revision of the unification philosophy (which in fact was never attempted in a systematic manner by these physicists).

Unexpected as the discovery of the new forces was, it is fair to describe the discovery of the non conservation of parity in weak interactions as a major shock which totally confounded the physics community. These novel ideas and new results could be incorporated in the framework and formalism of physics, but it is important to stress that they were neither anticipated nor predicted. Each of the new regimes (the nuclear regime for example) produced new features which required a new level of description.

In the early studies it was widely believed that nuclear physics was and would remain totally disjoint from the rest of physics. Consequently it was not expected to lead to any practical results in technology let alone have applications outside of physics, such as chemistry. Rutherford declared as late as 1937, quite emphatically that nuclear studies would never lead to any release of energy and any other (so called) useful results. It is important to recall these reactions and predictions of highly competent scientists of that time not because they were wrong but to stress how difficult it is to foresee the future directions and new applications of basic science. Predictions of inevitable new advances, or the impossibilities of important new results, are equally unreliable.

In this context it is important to emphasize that these are compelling scientific reasons to study the quark level in meticulous detail. A first reason mentioned only rarely is the need to make a careful study of the validity of the laws of quantum mechanics on the quark level. Even though at this moment no serious consideration is given to the possibility that quantum mechanics would need fundamental alterations or might break down at the quark level, this is not something that can be assumed dogmatically. It is instructive to recall that Bohr seriously contemplated the possibility of a breakdown of quantum mechanics on the nuclear level. So the possibility that as unusual a level as the quark level would require dynamics of its own is not *a priori* excluded. If that was the case, the consequences are difficult to survey but they might well be of major significance, not confined to the quark level.

The discovery of a number of new particle attributes on new levels shows that these new domains are not just miniature versions of the older levels. New particle characteristics do appear. Although there were good reasons, mainly theoretical, to believe in such properties as color and charm, it clearly takes experimentation which probes this level to establish such properties. Further investigation is badly need to check whether the particles do — or do not — have additional intrinsic properties.

Often additional objects are discovered in a new experimental regime. This happened with a vengeance in the "particle level," with the discovery of K, $\overline{\mathrm{K}}$, λ and σ. The discovery of the λ, the first object found heavier than a proton, caused a now completely forgotten stir. The bottom quark was found pretty much as expected. However, the τ was a complete surprise. The high energy physics community was at first most skeptical about the reality of the τ meson. By now the existence of three lepton families, of which the τ is the third massive member, is believed to be a major feature of the particle spectrum. So much so that the discovery of yet another lepton would seriously threaten the confidence in the existing structure. It is remarkable that an object initially maligned became indispensable in a few years.

The interaction between quarks is well understood in terms of a gauge field theory. It is a brilliant generalization of local quantum field theory, but it maintains the basic principles of relativity and quantum theory. The theory is not particularly elegant, but when tested the agreement between theory and experiment is extraordinary. Even so there remain many untested domains and many puzzling questions. The so called standard model provides a remarkable unification of the weak and electromagnetic interaction. The theory provided brilliant predictions for the existence and the masses of W and Z. Even so that theory can not be anywhere near the final story — the unification efforts of weak electromagnetic and strong interactions have not been markedly successful so far.

Many questions must be answered before anything approaching a complete understanding of the quark level can be achieved. It is generally agreed that for a variety of reasons (including the expense and duration of experiments) the theoretical ideas tend to play a dominant if not decisive role in particle physics. This is not a particularly healthy situation. In spite of extraordinary originality and enormous mathematical sophistication, physics (as distinguished from mathematics) needs a continual, steady influence of experimental information. Not all experiments have to be of profound importance, but for theory to remain effective requires the constant monitoring of all aspects of a given level. Without such constant scrutiny there is a serious danger that physics will become too ingrown, too remote from its source. Clearly to obtain the necessary experimental information, equipment, apparatus and accelerators are needed.

A very important issue confronting the quark level is whether further detailed examination might lead to new insights in particle physics or in other parts of physics and science. As argued throughout this paper, it is unreasonable to expect definite and specific answers to such questions. As a general principle if precise, concrete answers already known for a piece of research, it is possibly valuable as a check or as a topic for a Ph.D. thesis but certainly not as a major research effort for an accomplished scientist, let alone a large collective effort. It is in the very nature of significant research that its outcome is only partially predictable. It is most important to wonder and reflect about the type of information, the analysis and study of a novel level might yield. The succeeding comments should be regarded as ruminations about the possible directions and results of quark physics. Such considerations are usually speculative and often vague but they are interesting and an essential prerequisite for thoughtful future planning.

Just as it was reasonable to conjecture that the nuclear level would be effectively isolated from atomic, molecular physics and certainly from chemistry and material science, it is equally rational to be skeptical about the impact of quark physics. That this conjecture was wrong for the nuclear level has obviously no bearing on the quark level. That so many of the useful results were unexpected and unanticipated, since nuclear physics then (1940) was not understood in great detail. The understanding of quark physics at this moment, although probably better than nuclear physics, is still far from complete. Again this doesn't guarantee that important results on the quark level will be forthcoming, but is suggests the need for further investigation.

It is well to stress the degree of novelty and the unusual features of the quark level. The occurrence of fractional charges is strange enough, the color notion and the strict confinement via color forces are strikingly different from anything else in physics. If single quarks are intrinsically unobservable or if the color is in principle unobservable this raises questions to some people about the applicability of quantum mechanics to these levels. Such questions deal with the precise applications of the quantum probability postulates which require the "in principle" measurability of the particle attributes. These problems are of deep fundamental significance and eventually must be addressed. There was an active group of physicists that suggested a "quark liberation front" so that single quarks or colored objects could be observed. This is no longer a very active area of investigation but this would change instantly if a single quark was observed or suspected.

There are now impressive computer studies indicating that quantum chromodynamics (the theory of quarks) might well lead to quark confinement. This is still no compelling proof. Even if correct this result is somewhat incomplete and disappointing. It would parallel the explanation (on the chemical level) of why HF is chemically more stable than HBr by producing a computer printout showing that the numerically obtained solutions of the Schroedinger equations led to larger binding energy for HF than HBr. This is interesting and impressive but does not give much qualitative or intuitive insight. It is perhaps naive to hope for a more conceptual insight in as fundamental a result as quark confinement. Such a basic result should not emerge exclusively as a numerical accident. Thus the question of the existence of single quarks cannot be considered as definitely settled. Before it is questions of the utility and applicability of the quark level remains extremely tentative. It often happens that the fluctuations on one level have a marked impact on another. This is the mechanism whereby the vacuum fluctuation of quantum electrodynamics influence molecular forces. The same mechanism is via the Casimir effect responsible for the macroscopic force between metal plates. So there is a reasonable possibility that should be taken extremely seriously and explored in detail, that quark fluctuations have pronounced and pervasive effects.

There are a number of indications which suggest that features of the quark level might well show up in many different guises. The increasing conceptual similarity between condensed matter physics and particle physics hints or suggests that there might well be collective properties on the quark scale that are novel. It is impressive how many studies in quantum chromodynamics involve phase transitions that are so characteristic for statistical mechanics and condensed matter. There might be new quark states of matter, a quark-gluon plasma might exist, both might have interesting properties. These possibilities certainly cannot be excluded. They must be investigated before such studies are carried out. It is premature to be too sure about a lack of relevance.

These are conjectures and speculations, but it important to realize how rapidly and unpredictably science and physics changes even in fields that were studied extensively. The Bohm-Aharonov effect is a relatively straightforward but dramatic consequence of non-relativistic quantum mechanics. It took more than 30 years before it was discovered and then it came as a major surprise. The Berry phase could have been discovered right after Born's papers on the adiabatic theorem in quantum mechanics in 1927, but it took Berry's incisive insight almost fifty years later to recognize its deep and pervasive significance. The quark level is neither explored nor understood in the detail and depth of non-realistic quantum mechanics, so it is quite reasonable that new and delicate insights will emerge.

In addition the last ten years have seen a spectacular development in instrumentation and experimental technique. This makes it possible to carry out experiments of extraordinary sensitivity. It was an accepted dogma of physics (and chemistry) that single atoms could not be observed let alone be manipulated. In 1980, a single Barium atom was photographed using a cleverly constructed trap. In 1987 a single positron was kept in a trap for three months. Individual quantum jumps were observed in 1986 by Hans Dehmelt. These feats were unheard of before the techniques were unanticipated. Even a few years ago, such results would have been considered impossible and even inconceivable.

In the face of such astounding advances, it is hard to be too categorical about the impossibility of major innovations in the quark level. Of course none of these advances proves or guarantees that new, important or useful results will emerge from investigations of the quark level. The only thing which is absolutely certain is that if all the implications and properties of the quark level are not studied, nothing new will happen. Even given the uncertainties and unpredictability of the future, the sketchy suggestions given here as well as all unexceptional examples of the past demonstrate that pursuing the investigations of the quark level is not only scientifically justified but actually required by a responsible concern for the future of science. It is not a whimsical, capricious pursuit for the sake of international scientific public relations or personal aggrandizements. As a coda to this section, it should be stressed that describing the importance of quark studies does not mean that this level is more important than others, nor that its practitioners are the most gifted or most deserving scientists. The discussion does not address the social, political and financial issues involved in supporting large scale scientific enterprises either. The discussion does show, that this is an area of great interest and great importance, which eventually must be investigated.

D) The Organizational Aspect of the Level Structure

The existence of disjoint autonomous levels has important psychological and organizational consequences. To discuss the organizational aspects it needs to be recognized that in the last fifty years the technical encumbrances necessary to carry out significant research has become so enormous that they have begun to dominate, even overwhelm many of the scientific activities. At one time a scientist could reasonably expect to think about interesting questions, devise an experiment, construct the necessary apparatus, do the experiment, analyze data and with some luck relate the results to the existing theoretical ideas. Nowadays this sounds like an adolescent fantasy, or a science fiction description of a scientist.

The extraordinary technical knowledge and the special skills required in contemporary research make such demands on individuals that they must devote practically all their time and most of their efforts maintaining and refining their viability as professionals in a narrowly restricted area of science.

The unpredictable advances in science and the rapid changes in technology demand a continual monitoring for new and better applicable methods. Little if any time (or energy) is left for scientific activities not directly related to current research. Detailed familiarity with other subjects rarely is seen on a professional level. This heavy and unavoidable emphasis on specific techniques sharpens the separation between the fields and the levels within a field. The time, effort and expertise required for the technical implementation of a major research project is one of the outstanding characteristics of contemporary science. The execution of such research programs with there varied, special need generally demands very large research groups.

These features are especially pronounced in high energy or particle physics. An enormous effort is necessary to design and built accelerators. Accelerator physics has developed into a highly sophisticated independent specialty. It is a typical example of a scale, or a level, as these concepts are used in this paper. The construction of detection equipment, equally essential for high energy physics, is another separate specialty. This again is an example of another, distinct scale. There are many such scales within particle physics and innumerable separate scales in the rest of physics. It should be noted that the technical specialization is not restricted to particle or high energy physics but pervades all of science. Producing chemically pure, perfect crystals of gray tin; constructing a guidance system for a rocket; designing a refrigerator for temperatures of a micro degree; or developing measuring devices for femto second chemistry all require specialized knowledge superb, instrumentation and a large supporting staff of technical experts.

Even though the size of the research groups in high energy physics is very large and the duration of an experimental study is very long — other areas are developing larger and larger groups, with investigations taking more and more time, not infrequently from three to ten years. The extensive astronomical surveys of the distribution of galaxies; the measurements of the microwave background; the human genome project in biology; are all enormous enterprises requiring highly specialized equipment, delicate instrumentation and meticulous planning. These examples and many others demonstrate that the ancillary technical needs have become an essential factor in the execution of any major research project.

It might appear that the increasing technical demands are primarily, if not exclusively, a feature of experimental studies. However, even in theoretical studies there are increasing technological demands. The mathematical level now routinely expected from a theoretical physicist is vastly more advanced than it was twenty years ago. Sophisticated computer techniques and symbol manipulations are essential (new) tools for an aspiring theoretical physicist. It is an open question whether it is more demanding to design and builds a particular tunable laser than to obtain a working knowledge of algebraic topology; but to the respective scientists who have these needs, both represent formidable technical challenges that must be met so they can pursue their research. Because of the enormous and continuing demands the technical control of a scientific level makes, it becomes increasingly difficult to change from one level to another. It is extremely difficult to acquire new technical skills and a new scientific orientation. Thus inevitably the increasing technical demands cause an increasing individual adherence to a level. This is often by necessity or design, occasionally by default. As a consequence the levels become more separate, scientifically more disjointed and the practitioner becomes scientifically more insulated.

It is hardly surprising that the existence of these diverse levels has important organizational consequences. The American Physical Society has at this moment thirteen distinct divisions which cover such diverse and barely related fields as particle physics, high polymer physics, plasma physics, atomic, molecular physics and six others. In addition there are six topical groups dealing with multiparticle dynamics, computational physics and four others. Even that does not exhaust the enormous variety of topics such as surface physics, non-linear optics, laser cooling traps and (so far) about thirteen others. The divisional meetings of the Society are well attended. Those of the topical groups are smaller but relatively better attended. The attendance of the general meetings has been dropping steadily. Much of the administrative and organizational business of the Society is carried out at these general meetings. Retiring addresses, press conferences, job placement registers and award ceremonies all make the general meetings important events. Most of the serious scientific discussions take place at the divisional meetings (or at meetings of topical groups). New scientific results are sometimes announced at the general meeting, but there is rarely an opportunity for an incisive discussion. It is no doubt clear that this splitting in divisions, and the subsequent splitting in topical groups, corresponds very closely to the splitting in distinct scientific levels.

A glance at these separate divisions and topical groups shows that they deal with completely different questions in different ways, employing distinct methods and procedures. The rationale for the formation of the divisions and topical groups is precisely that the members share a joint scientific language and a common interest in a class of problems. This creates a degree of scientific congeniality between the members that distinguishes that group from all others. It is not impossible that there are occasional overlapping ideas, methods or phenomena between these groups. However, it is unlikely that the physics of beams overlaps significantly with the physics of multiphonon processes, or that crystallographic ideas are relevant in the nuclear reaction of heavy ions. Such overlaps are usually minor and probably coincidental rather than conceptual. As time goes on there is an increasing proliferation of such topical groups showing a continual need for — and certainly a desire for — further specialization and more independence from the rest of physics. The members of individual groups do not seem to have a great deal of interest in communicating with other groups. Very few of the members of the topical groups or the divisions bother to attend the general meetings. The science discussed at those meetings is clearly perceived as less relevant to their scientific interests. Their scientific scope and concerns are determined quite narrowly by their immediate practices and problems. The evolving organizational structure of the American Physical Society (and many other scientific societies) is an accurate reflection of the current scientific fragmentation.

E) The Psychological Consequences of the Level Structure. The Popularity Inequalities

The discussion presented so far describes the separation of science in distinct levels and its eventual fragmentation into non-communicating sub-fields. These developments appear as an almost inevitable consequence of the magnitude complexity and diversity of contemporary science. But it might still seem puzzling why there is so little sympathy, in fact so much antagonism between different levels, often leading to unpleasant exchanges and acrimonious arguments.

It is important to recognize that active participation in scientific research requires an intense personal involvement. Research is often extremely competitive; it demands persistent dedication in the face of continual frustration. To function in such an environment, an individual must be convinced of the importance of (his) her efforts, but to withstand the unavoidable disappointments, delays, misdirections, there needs to be a very strong emotional commitment. The great physicist and brilliant teacher Ehrenfest used to admonish his students that in research "the problem must become your own, you must give it all your skill and energy." While investigating this research problem this must be the most important thing in the world. It is yours to solve — and nobody else's. Ehrenfest's admonition shows that research was indeed and should be an intensely personal activity.

At one time a scientist tended to be a "natural philosopher" interested in — and responsible for — a large general area. With the increasing specialization, the domain for which a typical scientist accepted personal responsibility was greatly diminished. With the enormous technical demands of highly specialized areas in the current climate the personal commitment is no longer to a general field but to a limited, restricted level. This in no way diminishes the importance the individual scientist attributes to his activities, nor does it in any way alter the emotional attachment to the research effort. The level structure inevitably introduces a constriction of the domain of personal involvement.

It is obvious that such a heavy concentration on a limited area and investing this with so much emotional importance influences how a scientist view other fields, other levels. There is a tendency to order other fields or levels with the utility they might possess for the investigation of a particular scientist. That would be a highly personal ordering. A somewhat more objective ordering and comparison of levels that is both interesting and of some importance in assessing the relative importance of distinct levels be carried out in terms of the notion of "fundamentality."

If a field "a," with all its laws and concepts, imply all the concepts and laws of another field called "b," this is written as a=>b. Furthermore if "b" does not imply "a" the field "a" will be called more fundamental than "b." Going from field "b" to field "a" will yield new information, new concepts and new laws. Presumably going from field "a" to field "b" will generally not yield new information, just a recasting or reformulation of already known laws and concepts. It was stressed in this paper, that the derivation of the laws of the less fundamental fields (such as "b") from the more fundamental field (such as "a") is actually a difficult and subtle process and not always possible in detail. It is more in the nature of an exhortation of what science should be able to do than an implement able procedure. This does not seem to diminish its psychological appeal. Many scientists are pleased and proud to be a part of a "truly fundamental enterprise," vague and inarticulate though this notion may be. Thus the "fundamentality" classification is one of the important ingredients that produces strain and irritation between the levels. A common ordering which should not be taken all that seriously, but which nevertheless is often implied, has this general character:

quarks => particles => nuclei => atoms =>molecules

molecules => matter => aggregates of matter => planets

=> macromolecules

=> biological materials

=> cells

=> organs

=> organism

It is well to reiterate that this ordering completely ignores the autonomy and closure of the levels, which has been emphasized so much in this paper. As outlined here the quark level is the must fundamental, however, it would be wise not to bet on the medical therapeutic effects of the discovery of the top quark. (Even this author wouldn't bet on that)! It is also wrong to ignore the more fundamental levels altogether. Manifestations of these levels show up at unexpected places and can lead to unanticipated phenomena. The heating of the earth, a geological effect, is due to nuclear processes; superconductivity, a macroscopic material phenomenon has its roots in quantum mechanics. Other examples presented earlier in this paper demonstrated these complementary features of the level structure.

The idea or the illusion of fundamentality has a powerful psychological impact on scientists and laymen alike. It suggests depth, profundity and finality. There is something deeply intriguing about an ultimate "theory of everything." It is thus not surprising that many scientists and the general public retain an interest in the fields considered as fundamental such as: particle physics and cosmology. For the professionals in or near those fields, the intense personal commitment (which is of course present in most fields) is combined and reinforced by the perceived fundamentality of the area making the commitment that much stronger.

The idea that the fundamentality of an investigation is regarded as extremely significant by the physics community is corroborated by the large number of Nobel prizes (and other awards) these areas have received. In the years 1943-1991 ninety-one individuals received or shared in the Nobel prizes. Eleven prizes were awarded for the construction or development of apparatus or instruments. Optical and atomic physics together received fifteen prizes (10+5) condensed matter and superconductivity accounted for nineteen prizes (10+9) while nuclear physics and particle physics together received thirty-five prizes (14+21). Thirty-eight per cent (38%) of the prizes awarded by the Nobel Committee went to subjects believed to be "fundamental" (or nearly so) at the time of the award. By receiving so many of the highest scientific awards, these fields acquire a high degree of visibility and glamour. By the same token scientists in other areas might well feel slighted and unappreciated. Their commitment to their respective fields is surely as great as that of more visible scientists, their technical and intellectual achievements are not markedly different from those in the more glamorous fields.

Clearly if one area receives more than its share of awards and recognition, while another feels underrepresented and neglected, this will inevitably produce irritation, anger and most likely will lead to serious competition. The scientists in these fundamental areas are habitually pictured as individuals of the highest intellectual caliber. Furthermore a large number of books have appeared intended for the general reader all extolling the brilliant advances and spectacular new insights in these new fundamental areas, such as the standard model in particle theory, the inflationary universe, the "theory of everything." It is undoubtedly not true that there is an organized, well-orchestrated campaign to publicize just these fundamental theories. There is little doubt that the public image of science is inaccurate and blurred.

For the last forty years, this author has made a habit of asking Ph. D. candidates on their final exam (when their knowledge of the field is arguably as broad as it will ever be) to name and describe three major advances in the preceding ten years in fields definitively different from their thesis. One of the advances should be in theory, another experimental. The third could be anything, but not in the field of their thesis. The initial results were rather poor and they gradually got worse. As time went on the three advances requested were reduced to two, then to one. The ten years were extended to twenty years. Very few students could have a coherent explanation, or even a qualitative description, of a "recent" advance not in their specialty. Furthermore most didn't care. The conclusion appears inescapable: the technical knowledge of most physicists (indeed most scientists) of subjects not immediately relevant to their own research is so sketchy and limited, that they cannot render a responsible technical judgment in other areas. (At times this author was tempted to request the same information from his colleagues, but for reasons left to the reader to guess it was never done).

Consequently a serious discussion of topics requiring detailed knowledge of widely separated area becomes virtually impossible. Instead the fragmentation continues. The proliferation of new specialties continues and a combative mood of competition and alienation emerges. Another disruptive feature, especially important in a volatile political environment, is the hierarchy of views with which science is regarded not only by scientists, but by a broader public constituency. Just as the attitude toward fundamentality could be summarized by a sequence of "fundamentality inequalities" this social ordering can be summarized by a sequence of "popularity inequalities." The symbol A > B combines a number of meanings: activity A is felt by a majority to be more important than B; there is more willingness to spend money on A than on B; A is overall more popular than B. The meaning A > B is not terribly precisely defined by these stipulations; it does convey a general attitude. The symbol at the extreme left is the most popular; the one on the extreme right is most unpopular. As an example consider:

Music, sports > medical science > ecology > Science > biology > chemistry > organic chemistry > physical chemistry > physics > Particle physics > quark physics.

This is of course a very crude characterization, but the general tendency is correct. It is certainly true that science is not a very popular enterprise. Science teachers in high school for example often complain that their colleagues consider them incomprehensible, difficult, demanding and weird, students feel very much the same way. Principals and superintendents are usually not too understanding of science teachers either. Typically they complain of just two of the four negative characteristics. In addition there is a general dislike, fear and suspicion of theory as being too mathematical and too obtuse leading to the universal inequality: Experiment > Theory.

The particle theorists at the extreme right of the popularity inequalities are the most unpopular. They (probably not coincidentally) have the hardest time getting jobs. It is noteworthy that the popularity index sequence is almost the exact reverse of the fundamentality index sequence. Particle physicists are presumably the most fundamental, but also the least popular. No doubt in part because of the heavy, difficult formalism employed by the theorists, in part because of the not infrequent hyperbole used in announcing new results, in part because of their frequent intolerance of theorists for qualitative, intuitive explanations.

Astronomy, astrophysics and cosmology occupy anomalous positions in these popularity inequalities. Observational astronomy has always been very popular as the existence of many amateur astronomy clubs show. Ever since its inception there has been and there is a mystique about space travel that has especially captured the imagination of young people. Recently the combination of cosmology and particle physics is not only of fundamental importance it also has created an extraordinary interest outside the scientific community. This is evident from the inexhaustible stream of popular books on quasars, the universe, the Big Bang and the incredible success of Hawking's difficult and sophisticated book " brief history of time."

Perhaps there is an element of mystery, a sense of magic, which causes so many people to be fascinated by theories and speculations about the beginning and end of the universe. Perhaps the perceived proximity of eternity and theology in cosmological consideration stimulates a public interest in that abstract and difficult field even though its immediate applicability and instant relevance for other fields is hardly greater than that of particle physics. The combination of these factors, the technical and emotional adherence to autonomous and independent scales, the uneven and unpredictable appreciation of distinct scientific achievements, the almost contradictory reactions to fundamentality and popularity, all these have conspired to produce an inordinate lack of coherence. There is no longer a single scientific method, there is little agreement about what problems are most important, what questions are urgent. Instead of one frontier, there are many. One scientist profound insight has no interest for another. The fullerenes of chemistry, surely an exciting new development, is a matter of supreme indifference to the string theorist. The remarkable results of the Cobe space craft on the primordial black body radiation have little interest for students of the spinodal decomposition. So one could go on matching up pairs of subjects, each one of vital interest to one scientist and of excruciating tedium to another. The science community is split up in parochial groups, each one intent on establishing and maintaining its own scientific identity. Even mutual appreciation has become difficult.

5. *The Dangers of a Green Curtain*

It may appear that the lengthy discussions in this paper have little or nothing to do with the initial arguments for and against the construction of the SSC. In addition to the scientific aspects there are many different non-scientific factors that influence the decision to support or not support major scientific projects. It is particularly important to analyze the social and scientific consequences of such actions.

The problems all revolve around one central issue: the function, role and general importance of science in contemporary American Society. The popularity inequalities show that science isn't a particularly favorite subject for many people; but the pervasive scientific illiteracy is worrisome, deeply frightening and approaches a national scandal. Science has effectively disappeared as a common element in the culture. There are no physical concepts, no laws that can safely be assumed to be part of the common intellectual heritage. If scientific ideas make a rare appearance in the popular culture this is in a distorted manner making science appear ridiculous, arbitrary and pointless. In a recent movie

"Little Man Tate" with Jody Foster, her son is supposedly a young genius. He used "lepton" as a swearword to insult his mother. The mere use of "lepton" presumably showed how precocious he was — unfortunately his later explanation was totally incorrect. This is a trivial example, but it illustrates the low regard in which the society holds science.

Like science, education is suffering from its multiple entanglements with political, organizational and financial problems. Even though there are some excellent high schools, some interesting innovative programs, some extraordinary colleges and universities where science and physics are given their due, the general understanding of physics is close to a national disaster. In spite of many plans, programs, for changing and improving science education and science literacy, up and down the governmental bureaucracy, very little has been accomplished. If anything these efforts have exacerbated the bifurcation, by making good students better and leaving a larger number of average students behind, worse than before.

It is not helpful that the dominant figures in our culture – the government, the media, the president, cabinet members, the newscasters and the anchorman (woman) – all appear to be afraid of science and exhibit an abysmal ignorance of science. This makes any rational discussion of science policy (whether public or high level) extremely difficult. This in spite of the heroic attempts of many scientists to advise and inform governmental agencies. It is not surprising that there is little understanding of the close connection between the vitality of a national research program and the general scientific ambiance of a society. If research is perceived as open, with broadly based opportunities and recognized as an important ingredient in the functioning of the society, no special programs to entice young people into science are necessary. In the present climate of mistrust and misunderstanding of science, no educational restructuring can be expected to be more than marginally successful. It is this mixture of fear and ignorance that makes the effective, education of science so difficult. It is essential to recognize that the status of science in a society is determined by the perception of its research potential, the promises of an exciting scientific future. The commitment of a society to research, determine the educational significance attributed to science—one can not chop off the scientific hopes and expectation and expect a new generation of young people to enter the field.

Science needs a continual influx of young people with new ideas and novel approaches. If science stops changing and growing it becomes uninteresting and it degenerates in endless repetition of sterile ideas — it is dead or dying. This demands a recognition by the society that resources must be provided, interest must be demonstrated, to maintain a vital scientific ambiance. There is an extraordinary danger in limiting the intellectual aspirations, the creative curiosity of a new generation for any one of a set of reasons, be they, social, political, religious or financial. To lower a "green curtain," a rigid financial prohibition of large scientific enterprises will initially lead to a serious constriction of scientific goals and inevitably to a serious deterioration of science. The interest and excitement necessary for a scientific career can only be maintained if there is an atmosphere conducive to the free exploration of ideas, serious speculation and wild dreams.

It is not often realized how fragile and vulnerable a structure science actually is. It doesn't take a long period of neglect, political or bureaucratic control, or well meaning misdirections to change a thriving scientific enterprise into a marginal backwater activity. The example of German science in a period of not quite ten years (1931-1941) is a grim reminder. The departure of a handful of scientists, the closing of a few laboratories or the cancellation of a few projects can have a devastating effect on a nation.

For a contemporary society to function technologically, economically and intellectually, scientific illiteracy has to be seen as a major social problem. Science needs to be reestablished as a significant element in the culture. This will require a close cooperation and understanding between the governmental, scientific and educational communities. The research climate determines the scientific aspirations of the culture. The educational establishment should provide the information, skills, the enjoyment of understanding and knowledge. The government is obliged to provide the framework and resources to meet these essential, needs to reintegrate science in the culture. The scientist in turn should realize that even though it is important to be enthusiastic, even evangelical about a particular line of research, it is unwise and counterproductive to couple the excitement for one effort to a disparaging, demeaning criticism of another study. No matter how different the scientific domains might seem to the individual researcher, to the outsiders these differences are all small perturbations. The success or failure of the Hubble telescope has a direct impart on the fortunes of the SSC; the discovery of the J/Ψ had a salutary affect on all of physics. Scientific differences are essential but acrimonious public debates or an arrogant dismissal of other fields will do immeasurable harm. They quite possibly could destroy the whole scientific enterprise.

Particle physics has been and is a most exciting sub-field of physics. It has provided surprises, totally unexpected phenomena and new insights. Everything that one might reasonably hope for has been realized, often in altogether unanticipated ways. Particle physics is not the only sub-field of physics. Other fields have had and are having developments, novel results and new insights that are as unexpected and promising as any in particle physics. The chaotic phenomena, high temperature superconductivity and atomic traps are some examples. But physics is not all of science. The recent developments in chemistry astronomy and biology are as spectacular and impressive as any obtained in the long history of science.

It would not only be unwise and pointless but malicious and dangerous to construct a linear ordering of the fields and sub-fields. It is fine for an individual to be more interested in, or have more talent for, one area than another, but a rigid, one dimensional enumeration is arbitrary and quite dangerous. It is reminiscent of making a list, ordered by number of such diverse creations and discoveries: a Mozart piano concerto, Columbus trip to America, the proof that π is transcendental, a Rembrandt painting, a Shakespeare play and the invention of the printing press. Clearly all are extraordinary achievement's – the world needs them all and all must be recognized. Such orderings only serve to foster jealousies, to irritate, to anger and divert effort to pointless controversies where everyone loses. The one conclusion to be drawn is that the human scientific originality is unbounded and can lead to the most marvelous results. It is the obligation of science to see to it that any one who wishes can share in the understanding and enjoy the pleasure that comes with knowledge.

All these comments may appear reasonable and sensible, but it might well appear that they do not touch or even avoid the central issue. There is an understandable almost inevitable tendency to direct and drive research in applied practical and useful directions. In times of financial stringency it might seem irresponsible, even foolish, not to stress promising technological developments and spend large sums of money on undirected scientific ventures of any kind, no matter how interesting. It is certainly necessary that a certain amount of mission directed, technically oriented research and development be supported and maintained. It is crucial to recognize that genuinely novel, momentous, society changing discoveries are very rarely the result of directed, planned and organized investigations. X-rays, the photocell, antibiotics and lasers are a few examples. To insist that only those research efforts which have immediate, practical utility are worthy of social support presumes that certain individuals (in government or funding agencies) possess detailed knowledge and information of just where new developments are likely. No single individual is that wise, let alone a single agency. If the basic, underlying science is understood and controlled, it is possible to use that information for practical applications and usable technology. If not investigations become less effective. That is why it was possible to promise that a man could make a trip to the moon and return, while it is even now impossible to promise a cure for cancer, or aids or accurate prediction and control of earthquakes.

The current political trend to concentrate on studies having visible, instant applicability is short sighted as well as dangerous. It ignores the essential and irreplaceable role of basic scientific understanding. Without a continual expansion of fundamental knowledge, applications and innovations will cease. How, in what way and what kind of science is supported is a decision that Society has to make. The discussion presented in this paper should identify some elements, which must be considered in such decisions. The various scales, their approximate autonomy, the frightening prospect of internecine scientific disputes and the often unpredictable relations between scales, must all be analyzed and evaluated to arrive at a thoughtful conclusion. Closely related is the absolute need to make science understandable and enjoyable for the general population. It would be increasingly difficult to believe that science education is an essential element in Society, if this same Society refuses to allow science to prosper.

The Society sets the science policy. This implies that this Society is also responsible for the successes and failures this policy produces. It better be well thought out, for it is difficult and time consuming to reorient such policies. It is finally well to recall that more societies have failed and collapsed because of a lack of scientific foresight, than because of any other reason.

Even though science is not all of life or all of Society, the beauty, excitement and thrill of science can be transmitted, making this educational transmission process a most important social function. It is imperative that society be so organized that the search for scientific beauty and its transmission can continue unimpaired by unnecessary, artificial constraints. Results of interest, importance and use will be forthcoming although nobody can say where or when.

One does not give up the search for hidden treasures because it isn't known where or when they will be found. Without the human curiosity that stimulates and demands such searches, life would inevitably degenerate in the boring repetition of pointless activities.