ready in the *Living Forces* we find a clear statement that if Newton's empirical laws were different, space would not behave in a Euclidian way, but non-Euclidian geometries would have to be developed. It is in this sense that the transcendental criticism has indirectly influenced the development of science.

In the M.A.d.N. and even more in the opus posthumum, Kant expressed doubts in the entire intelligibility of nature and tried to trace the limit between what in science is derived from deduction and what is due to experiment. He found reality partially intelligible and tried to formulate a rule of demarcation. This again became the source of the Popperian problem of demarcation between science and metaphysics which Popper called Kant's Problem. Yet the place accorded by Kant to pure deduction seems to have been over-estimated. His many followers among the «Naturphilosophs» and their absurd mistakes (like Schelling's deductions of a theory of condensation and evaporation, or Hegel's of polarization and the nature of light) will testify to that. Finally, he started out from conservation laws and ended up in the opus posthumum with an even stronger affirmation of an underlying principle of conservation of matter and forces in Nature. The concept tying together all these ideas: conservation, impenetrability, space, time and cause, contact forces as against action-at-a-distance is the concept of the ether which fills all space in nature and many of the pages of the opus posthumum. In the M. A. d. N. Kant had started from empirical concepts and went on to establish an a priori scientific metaphysics. Now he went back in order to establish empirical detail in conformity with his metaphysics.

*Kant's Influence on His and the Next Generations.*

Kant died at a time when several philosophical, scientific and social movements were being formed. Most of the greatest intellectual figures of the time had been directly or indirectly his disciples. The philosophy and literature of German Romanticism was spreading rapidly, and all its chief representatives claimed Kantian parentage. They saw in him only Kant the idealist or transcendentalist. Even Goethe who resented the fact that Kant had ignored him did consider his own science (Metamorphose der Pflanze) and his philosophy as Kantian. In science, and in the scientific metaphysics of the «Naturphilosophie», Kant was the great conservationalist who elevated forces to become the action principles of matter and by their conservation a way was found to unite Nature with Man, body and mind. Kant's

M. A. d. N. was considered the death-blow to all kinds of atomism. The new dynamics was built purely on forces of attraction and repulsion acting from centers which did not occupy any space. The Kantian formulation of the ether as the carrier of various forces was the triggering point of all field ideas which spread from Germany to France and England. The Kantian influence on Coleridge and his contacts with Davy and then Faraday are known; Kant's influence on Thomas Young and the fact that Young came to get acquainted with the Eulerian optics through his work is less known.

Fichte, Herder and Hegel all started out from the Kantian philosophy: each of them created an image of what is the gist of Kant and then took up a position for or against the gist. Kant's famous last attack on Fichte is more of a defense against being misisterpreted than a full-scale attack on Fichte's philosophy. The Kantian element both in Hegel and Fichte again took two different directions: a scientific position which is often claimed to have led to failure, and a socio-political philosophy which led up to Marx and his followers. Those who claim that Kant's approach led to failure in science generally presuppose that Kant was an antipositivist both in the scientific and in the sociological sense, and that science, in order to succeed, had to be positivistic while the social sciences, in order to lead to truth, had to be antipositivistic. These views are all half-truths. Kant's antipositivistic influence in science was of utmost importance in creating a metaphysical framework on which Helmholtz's work on the conservation of forces was formulated, with which Einstein's realism and search for new conceptions of time and space could harmonize, on which the complimentarity principle of Niels Bohr could thrive. On the other hand, J. S. Mill and A. Comte were not less Kantians than were Whewell and Duham, Weber and Mannheim or Wundt and Freud.

As a result of the breadth of Kant's «Weltanschauung» and of both the correct and the misleading interpretations of his work, Kant provided the point of departure for many diverse schools of philosophy: the idealistic school in Germany, the Naturphilosophie, the Newtonian natural philosophies of the late 18th and early 19th centuries, of the neo-Kantian revival in science around 1850 (du Bois-Reymond, Virchow, Ludwig, Brücke, Helmholtz, and others), and in philosophy in the late 1870's, in psychology (Wundt) and again in our century in physics (Boltzmann and Bohr) and in philosophy the critical approach of Popper, Agassi, Lakatos, Feyerabend and others.

While Kant was a typical hero of the Enlightenment, he differed from them in one important aspect: Kant carried into all his theories, whether scientific or socio-political, an element of development (i.e. change) to the most fundamental level: here his «enlightened» contemporaries saw only fixed and eternal principles of natural or moral laws. In other words, from his early *A.N.U.Th.H.* (1755) through his *Critiques*, and again in his posthumously published *Vom Übergange*, Kant introduced everywhere the idea of evolution in time and sought after the lawfulness of the changes rather than of the fixed states. This became very important in 19th century biology and in the physics of Boltzmann and his numerous followers. Whether Kant had any influence on Darwin is not yet established, but that the Kantian mode of thought should have been congenial to Darwin is more than probable. Kantian dynamic philosophy influenced greatly also the 19th century theories of the electromagnetism mainly through the Kantian element in the Naturphilosophie, and one of his leading disciples became the famous Hans ϕrsted, known for his discovery of the creation of a magnetic field by the change in electric current, and for his scientific metaphysics expressed in his *The Soul in Nature.* Already Ampère in 1814 rejected the Newtonian atoms as small rigid impenetrable spheres and preferred the Boscovichean-Kantian mathematical points serving as centres of force embedded in an all-pervasive ether.

In biology itself Kant's influence has been found both in experimental biology and in philosophical (speculative) biology. In the one direction, it has been argued persuasively that Kant's critical approach, applied to scientific ideas in the early and mid-19th century, enabled Berzelius and Liebig, Schwann and Schleiden, Johannes Muller and du Bois-Reymond to dispel the older confused notions and establish a mechanical-reductionist view of life. On the other hand, the greatest «Naturphilosophs» and even Hans Driesch at the end of the 19th century claimed Kantian parentage for their unashamed vitalism.

The Kantian conception of the world as real and of time and space as frame of reference became the chief influence behind all 19th century scientific theories of vision, perception, psycho-physics and even mental energies. The bearers of this influence were Johannes Muller (his theory of specific energies), Helmholtz (in theories of vision and perception), Fechner (psycho-physics) and Freud (mental energies). Moreover, the neo-Kantian epistemology developed by Helmholtz influenced also the new theories of the interaction between mathematics and the real world.

In philosophy of science, William Whewell based both his *Philosophy of the Inductive Sciences* and his *History of the Inductive Sciences* on the Kantian theory of knowledge, and exercised direct influence on all branches of science. This line was taken up by the epistemology and philosophy of science of mid-19th century philosopher-scientists like Helmholtz, du Bois-Reymond, Virchow, Fechner and even the fundamental Freudian philosophy that there are universal developmental laws of the human psyche, and that their comprehension is somehow centered in the concept of energy. The latest development of the critical approach is the critical philosophy of science which has been mentioned already.

Coming to evaluate Kant's direct influence on the development of science - namely, the influence on his own scientific work, we get a different picture. It is known that in Kant's later life Euler's influence gained the upper hand again. Kant abandoned his early view on the influence of tidal function on the retardation of the earth's rotation and accepted an opposite view from Euler's. As seen above, he supported now another theory, a force of impenetrability as an inherent property, he opposed action-at-a-distance and he drew the appropriate conclusions for light and heat. When these views became accepted in the early 1820's, nobody ascribed their victory to the influence of Kant - but rather to Young and Freud, to Euler who was considered (albeit erroneously) as the arch-Newtonian and even to Newton himself whose theories of the ether and of contact forces («the other Newton») became known at this time. Newton's phenomenal success took care of that. Every experimentally acceptable new idea had to be traced to Newton. The result was that Kant himself became known as a staunch Newtonian.

The early works in physics and astronomy were rarely mentioned during the time when the critical works were at the peak of fame.

Kant's direct influence, however, was revived at least in astronomy when the great French astronomer, Jean Arago, acknowledged in 1842 Kant's importance. Yet the main difference between Laplace and Kant, namely the evolutionary emphasis of Kant, was not noticed until after Darwin and has not been properly appreciated even today.

Kant himself was aware of his future impact. He is supposed to have said to Stägemann in 1787, «I have come with my writings a century too soon; after a hundred years people will begin to understand me rightly, and will then study my books anew and appreciate them». (Tagebücher von Varnhagen von Ense, Leipzig, 1861, vol. I, p. 46). How right he was! And he could have easily said, that he had come two centuries too soon.

*18th Century Image of Science.*

The image of science that emerges from the critical dialogue is outlined here.

The conclusion drawn from Newton's unprecedented success was that certainty of non-revealed knowledge is possible. This was the starting point of that quest for certainty which exemplifies our science, philosophy, literature and politics ever since the 17th century and which since Freud has been described as an innate (i.e., non-cognitive and non-environment-influenced) need shared by all human beings (if not by all living creatures). Due to the Baconian influence this certainty centered around the concept of 'fact' in England; in France (due to Descartes!?) it looked to mathematics to provide that certainty; Germany, since Leibniz, sought certainty in consensus: such metaphysical synthesis which would harmonize between the differing schools and would discover underlying unitary principles (conservation laws in nature) which apply to everything. This became the basic task of the scientist-philosopher, and no problem looked worthwhile to be worked upon if it was not directly contributory to the broad metaphysical view. This image of science and this view of the role of the scientist kept alive the Leibnizian tradition through Euler, Kant, Hegel, Fichte, Helmholtz, Fechner, Freud, Marx, Einstein and Bohr. This is the tradition of the critical dialogue between the competing Newtonian and Cartesian traditions as shown above.

In the Introduction to Euler's «De Curvis Elasticis»[1] we find a passage which is most typical of his approach to physical problems

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[1] Additamentum I to his «Methodus Inveniendi Lineas Curvas Maximi Minimive Proprietate Gaudentes» (This is the work where Euler solves the isoperimetric problem and invents the calculus of variations which became so famous during the fight between Jacob and Johann Bernoulli), Lausanne and Geneva 1774.

and casts also some light on his image of science and methodology:

«Wherefore there is absolutely no doubt that every effect in the universe can be explained as satisfactorily from final causes as it can by the aid of the method of maxima and minima from the effective causes themselves. Now there exist on every hand such notable instances of this fact, this, in order to prove its truth, we have no need at all of a number of examples; nay rather one's task should be this, namely, in any field of Natural Science whatsoever to study that quantity which takes on a maximum or a minimum value, an occupation that seems to belong to philosophy rather than to mathematics. Since, therefore, two methods of studying effects in Nature lie open to us, one by means of effective causes, which is commonly called the direct method, the other by means of final causes, the mathematician uses each with equal success. Of course, when the effective causes are too obscure, but the final causes are more readily ascertained, the problem is commonly solved by the indirect method; on the contrary, however, the direct method is employed whenever it is possible to determine the effect from the effective causes. But one ought to make a special effort to see that both ways of approach to the solution of the problem be laid open; for thus not only is one solution greatly strengthened by the other, but, more than that, from the agreement between the two solutions we secure the very highest satisfaction. Thus the curvature of a rope or of a chain in suspension has been discovered by both methods; first, *a priori*, from the attractions of gravity; and second, by the method of maxima and minima since it was recognized that a rope of that kind ought to assume a curvature whose center of gravity was at the lowest point».

Thus Euler's methodological demand is for theoretical proliferation; moreover, his image of science is such that whatever we do in our mathematics in a way of revealing actual reality in Nature and by necessity whether we proceed by the direct method which is the method of minima and maxima from efficient causes or by the indirect method of proceeding from final causes, i.e., from a priori principles, we must reach the same conclusions. This mutual reinforcement is the real satisfaction for the natural philosopher. The sharp distinction between final, a priori causes, and effective causes is Leibnizian rather than Newtonian and in this Kant turns out to be a few decades later Euler's most faithful disciple.

These views on the role of science and metaphysics combined with the strongly Puritan view of German Pietism which stressed the habit of selfrenouncing labour of singleness of purpose. This was a sine qua non for developing scientific metaphysics on unshakeable foundations on which a whole system of science, ethics, social theory and an anthropology had to be built.

In addition, Euler, and later Kant, developed a theory of growth of knowledge which rejects the view of knowledge-by-accumulation and comes much nearer to the view that knowledge grows by a continuous critical dialogue between competing metaphysics. The connecting 19th century link in this tradition was William Whewell. Its 20th century main representatives are Popper, Agassi, Lakatos and their followers.

Euler's realistic attitude and his opposition to the growth-by-accumulation theory of the growth of scientific knowledge is beautifully illustrated by his systematic, detailed description of trials and experiments that failed, and allows us to gain a true insight into his method of discovery. In all the literature of Euler, the only source that I know that draws attention to this at all, without however drawing any conclusions as to Euler's theory of the growth of knowledge, is an essay by Fuchter in 1948[1]. All this in complete harmony with the above-mentioned theoretical pluralism.

The Eulerian-Kantian view of science and the resulting scientific-metaphysics determined for the next generations a choice of problems dealing with general principles in Nature. Their work complemented the Newtonian achievement and the French mathematization of mechanics: the principle of conservation of energy, the continuum approach which gave birth to the field concept, the new, specifically biological approach, both in its vitalist formulation and the 'hard-approach' cell-theory-reductionism grew on Kantian soil.

I did not enter into the problems of the influence of the general intellectual environment on the image of knowledge. Some questions that must be raised are the following:

What was the impact of the fact that the Berlin Academy consisted mainly of French, Swiss, Russians and other non-Germans? How did Frederick the Great's German nationalism (created by him, its charismatic leader) harmonize with the cosmopolitan 'Enlighten-

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[1] Dr. R. Fuchter, «Leonard Euler» - Basel, January 1948 pp. 1-24. On page 14 here it is noticed that Euler's style of topics starts with a thorough analysis of the problems at hand. This characterizes his mathematical, physical and epistemological-philosophical work.

ment' attitude, to science and culture in general? Why did Euler write his 'Anleitung zur Naturlehre' in German (instead of the usual French) [1] and why was it never published? What was the institutional difference between the Berlin and St. Petersburg Academies and how did this influence Euler's work who was in contact with both? Why was Germany the centre of growth of the strong synthesizing and reconciliatory tradition best represented by Leibniz, Euler and Kant? How did Kant deal with his own German nationalism, pro-French revolution attitude and the changing Prussian - Russian occupation of his beloved Königsberg? Finally, how was the social and political Rousseau - influenced philosophy of Kant (constructed on his scientific metaphysics) influenced by the absolute autocratic rule of Frederick the Great's successor?

### *Helmholtz*[2] *and Conservation of Force.*

The central concepts in the physics of Newton were space, time, mass and force. By the end of the nineteenth century the central concepts in physics were space, time, mass and energy.

The general concept of energy became meaningful only through the establishment of the principle of conservation of energy in all its generality; thus the story of the emergence of the energy concept and the story of the establishment of the conservation laws are difficult to disentangle. The man who formulated the principle for the first time mathematically, in all its generality, was Hermann von Helmholtz. His was a towering scientific personality, and his lifework has left its mark on all branches of nineteenth century science, from theoretical mechanics to applied physiology. The concept of energy, as we know it today (by 'today' classical, prerelativity physics is meant), has emerged from Helmholtz' 1847 paper «Über die Erhaltung der Kraft», and up till then nobody, including Helmholtz himself, had a clearly defined concept of energy.

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[1] The directives of Frederick the Great were that a) all publications of the Academy should appear in French and b) parallel to the French translation the work could be printed in any other language as desired by the author.

[2] This subject was developed by me in two articles: «Helmholtz's Kraft': an Illustration of Concepts in Flux», Historical Studies in the Physical Sciences, vol. 2, 1970, University of Pennsylvania Press, pp. 263 - 298; «The Conservation of Energy: A Case of Simultaneous Discovery?», Archives Internationales d'Histoire des Sciences, Vingt-troisième année, No. 90 - 91, Janvier-Juin 1970, pp. 31 - 60; and in my book «The Discovery of the Conservation of Energy», Hutchinson (in press).

But the problem is not a purely scientific one. In view of the prerequisites for the establishment of the principle of conservation of energy, this final event had to take place in nineteenth-century Germany, and not possibly in England or France.

In Germany the universities were such that, whatever the student studied, he could not avoid facing sooner or later the great metaphysical problems posed by the various 'Weltanschauungen'; while in England 'science' was pursued, or in the traditional spirit 'natural philosophy' was taught, German 'Philosophie' covered the whole of the human intellectual enterprise. Speculation was encouraged; even today, after the great, late-19th century battle to erase the last remnants of an influence of the 'Naturphilosophie', or rather of a degenerated, ridiculously trimmed-down version of it, 'speculation' does not cause such contempt in German as it does in English. In Germany the 'schools' or 'laboratories' of the great scientists represented a complete philosophical system, and every student had to take a stand towards them. One could not have worked in Weber's physical laboratory, or in Liebig's laboratory, without making a considered philosophical approach to Kant's epistemology or to the question of the mechanistic-vitalistic controversy. In this atmosphere one could not very well separate experimental data from highly speculative hypotheses.

This atmosphere, which was unfavourable to many other scientific projects (like, for example, to the first formulations of electrodynamics: indeed all German theories failed, while the English, French and the isolated Ørsted did the work) was indispensable for the establishment of the conservation of energy principle, i.e. the emergence of the concept of energy. In these universities the 'schools' of Liebig, Woehler, Johannes Müller, Weber and Gustav Magnus were founded. And here it was that the further prerequisite — namely the crossfertilization between physical and physiological thought — was made possible. Moreover, it was here that biology was born. In these universities matured men like Schwann, Bruecke, DuBois-Reymond and Helmholtz. Their shared background consisted of a vast reading in the philosophers and a readiness to face their questions, an awareness of a deep-seated connection between all the sciences, and a tendency to look for coherence between their philosophies and their scientific knowledge. Helmholtz's father was a close friend of Fichte's son, himself a professor of philosophy, and Helmholtz himself, according to his own testimony, was deeply influenced by both Kant and Fichte. It was of crucial importance that, in addition to this, Helmholtz's education in physics and his mathematical ability made him the ideal man for the task awaiting him.

This was the stage on which the physical concepts in use in the early 19th century had to act out their roles.

The confusion between 'force' and 'energy' (as we use these terms) in the works of Helmholtz and some of his contemporaries was not simply a terminological one, as most of the commentators on this topic tend to assume, but rather a necessary prerequisite for the final clarification of the concepts. Only an undefined entity could have been the subject of a general belief in principles of conservation in nature, and such a belief was one of the major factors in the actual establishment of the conservation of energy principle in its final, mathematical, that is, correct and well-defined form.

One of the most cherished beliefs of inductivist historians of science is that the principle of conservation of energy grew directly out of the realization of the impossibility of a perpetual motion machine. This realization is indeed one that was arrived at inductively, and it dates back to the seventeenth century, if not to earlier times. Certainly Stevin already drew physical conclusions from it, and the 1775 declaration of the French Academie not to consider any more suggestions for the construction of such a machine covered not only mechanics but all the branches of physics. Thus, at least seventy-five years before the establishment of the conservation principle, the alleged 'intellectual father' had been established beyond doubt.

It is also often assumed that the principle of conservation of energy was a direct generalization of the law of conservation of mechanical energy, as formulated, for example, in Lagrange's 'Mécanique Analytique'. But here the use of the modern name of mechanical or kinetic and potential energy leads us into hindsight; it is implicitly assumed that the concept of energy was extant, and that the scholars in this field thought in these terms and worked with this notion. Actually it is only now that we view all sorts of terms, 'vis viva', 'Spannkraft' and the many others, as instances of the all-embracing concept of energy. At the time, when in mechanics the sum of 'vis viva' and of 'potential function' (under this or any other name) was found to be conserved, nobody thought of the necessity or possibility of generalizing this any more; this was general enough. The notion of energy as something so general that all the special forms are only instances of it, emerged only in 1847; and then in precise mathematical language. Before that, it was a vaguely understood entity, which was conserved and related to mechanical energy and even served very fruitfully as a working concept; it was used also by Faraday, or Mayer, or any of the twenty odd 'simultaneous discoverers' of the principle. The proof that, due to exact mathematical and dimensional considerations, the conserved entity must be related to mechanical energy by simply being reducible to it, is the work of Helmholtz.

One often finds in historical or even physical literature that, at the beginning of the nineteenth century, two theories of the nature of heat were still in vogue: the mechanical, or rather, dynamical theory (the name mechanical is really justified after the work of Clausius), and the material-caloric theory. It is implied, or sometimes explicitly stated, that Carnot had a clear conservation law in mind and was only misled by his use of the caloric theory, and that the mechanical theory had to be established in order that the principle of conservation could be finally enunciated. Actually, the connection between the actual development of the early thermodynamics and the theory of the nature of heat was very weak; even a year after Helmholtz's proof, some scientists held to the caloric theory of heat (as, for example, Clausius, although he knew of Helmholtz's work); the conversion processes which were available due to the work of many physicists from Carnot to Joule, did not really point to a general conservation law; this again would seem to us so natural, because we already view heat as one of those many instances of a general concept of energy. Moreover, the famous supporters of the mechanical theory: Rumford and Davy, did not entertain any conservation ideas. As to Rumford, what he did show was the exact opposite: his argument was that, the heat generated being inexhaustible (that is, clearly not obeying any conservation law), it cannot be material (as material substances do obey a conservation law); Davy, if his experiment teaches anything at all (which was doubted by Andrade), had nothing to do with conservation. In short, the historical development was again the only, logically possible one: first the establishment of the principle of conservation of energy, and through it, the emergence of the concept of energy, and after that the formulation and separation of the two laws of thermodynamics, and the mathematical formulation of a true, mechanical theory of heat. It was only after these developments of thermodynamics that the actual processes of nature took an important place along the possible ones, and a new interest arose in the extremum principles which now became an integral part of the new energy-centered mechanics.

The following are the factors which, in my view, constituted a solid basis for the enunciation of the conservation principle:

1) An a priori belief in general conservation principles in Nature.

2) Realization that it is not enough that two formulations of mechanics: the vectorial Newtonian and the scalar analytical Lagrangian, are mathematically equivalent, they must also be conceptually correlated.

3) An awareness of the physiological problem of 'animal heat' or, more generally, 'vital forces', and a belief that these are reducible to the laws of inanimate nature.

4) A mathematician's certainty that, whatever is the entity which is conserved in Nature, it must be expressible in mathematical terms, and a mathematician's skill to perform the task.

Helmholtz had read at a very early age the works of Newton, Euler, d'Alembert and Lagrange (though not Hamilton) and was aware of the double tradition of mechanics, and that something had to be done about it. It was clear to him that the Newtonian-vectorial mechanics was the concept of force, and at the same time he saw that the only quantity conserved in scalar-Lagrangian mechanics was the sum of 'vis viva' and the 'potential function'. By temper and intellectual heritage he was a disciple of Kant and thus committed to a belief in the great unifying laws of nature; this took the form of conservation laws, and naturally the conserved entity had to be that vaguely defined entity 'Kraft' or 'force' (in the Faraday-sense). All this was in complete harmony with his mechanical philosophy: a belief that all phenomena of nature are reducible to the laws of mechanics. By training he was a physician and he spent several years in the laboratory of the famous physiologist Johannes Müller. There he came to face the problem of 'vital forces' and especially that of 'animal heat', and his first works were in this field. Again his approach was that 'vital forces' are like other forces conserved in Nature and, as all phenomena are reducible to mechanics, so 'vital forces' must be reducible to mechanical forces. On top of all that, Helmholtz was a mathematician of the first rank. He saw very clearly that, if 'Kraft' is conserved in Nature, and mechanical energy is conserved in mechanics, then all 'Kraft' must have the same physical dimension as mechanical energy and must be, moreover, reducible to it. That is exactly what he did in his 1847 paper.

What this story teaches us is how our use of the past in the service of the 19th century image of science distorts history. The chapter of the conservation of energy can be viewed as an important critical dialogue between the competing traditions of the Newtonian and the Leibnizian Scientific Research Programmes and the greatest failure of the Newtonian or, if one wishes, the greatest success of the Leibnizian. It will be interesting to see how this success was again jeopardized with the 1924 Bohr, Kramer, Slater papers.

*The Science, Technology and Industrial Revolution Problem.*

The eighteenth century brought with it the industrialization of Europe, with the accompanying development in technology and the growing social unrest and dissatisfaction in spite of the relative betterment of human destiny. For our understanding of the growth of knowledge we must face the problem of the kind of interaction that took place between science and technology. Are those who claimed that the steam engine did more for thermodynamics than vice versa correct, or are their violent opponents in the right? The literature on the question is huge, and the controversy was vociferous. Here I shall not go into this problem in detail but shall only suggest a new way of looking at the question.

Those who dealt with this [1] generally presuppose that only well-formulated scientific ideas in the body of knowledge can influence technical development, and they go on to ask whether or not these specific ideas did in fact influence technical development. Also, it is generally presupposed that pure science, applied science and technology form a continuous spectrum. Both presuppositions, in my opinion, are mistaken.

First of all, the cross-fertilization between science and technology does not have to go into specific ideas. It depends on the image of knowledge accepted in a society at a given stage of its cultural development -

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[1] To mention only a few recent publications:
Landes, «The Unbound Prometheus», Cambridge U. Press, 1969.
Mathias, «The First Industrial Nation», Methuen & Co., 1971.
Mathias, «Science and Society 1600 - 1900», Cambridge U. Press, 1972.
Hobsbowm, «Industry and Empire», Penguin Books, 1969.
Cardwell, «From Watt to Clausius», Heinemann, 1971.
Hill, «Reformation to Industrial Revolution», Penguin Books, 1969.
Musson, «Science, Technology and Economic Growth in the Eighteenth Century», Methuen & Co., 1972.

whether knowledge is open speculation, revelation, results of disputation or any idea which can be expressed in something palpable. In the eighteenth century this last became the accepted form of knowledge for those who had only the idea, as well as for those who had only the palpable results: they all had a sharad conception of knowledge, at least in England, and they interacted fruitfully: these were the people who inquired into the nature of heat, the nature of light, the nature of various imponderable fluids, the nature of minerals, the nature of plants, etc. Some others inquired into the nature of machines and technical devices and, irrespective of what they were engaged in personally, they accepted this wide range of activities as relevant to what they considered knowledge. Whether in this general mood there were specific influences of some specific ideas is much less important. The critical dialogue in which both these 'scientists' and these 'technologists' were involved was with the different images of knowledge of the German Kantians and the Scottish metaphysicians or French mathematicians: and the interaction between these different groups was one of mutual disapproval exclusively. In other words: the first group were Baconians with respect for both experimenta *fructifera* and *lucifera*, as long as they were *experimenta*.

The other presupposition is connected with the above point: technological innovation does not result from the direct extrapolation of applied science; on the contrary: technological innovation generally results from a rethinking of the most basic science with a different aim and thus a different angle of view. Whether these fundamentals which have to be reviewed are seen in primitive commonsensical terms of a well-established basic scientific truth or in terms of the most sophisticated latest scientific results based on them, is unimportant. Thus all technological innovation presupposes knowledge of some level of fundamental science, but not necessarily what is called science. To illustrate my point here: think only of the mazer and the lazer.

The industrial revolution is the result of the interaction between the various socio-economical factors which are well known and the image of knowledge which gave equal importance to experimental science and technology without distinguishing sharply between the two.

## *The Victorian Image of Science.*

German Naturphilosophie, with its search for underlying unitary principles and great (mystically potent) conservation laws, was the greatest antithesis to the English matter-of-fact, commonsensical, inductivist philosophy of nature. The one was speculative, idealistic, broad, and very often vague. The other was empiricist, materialistic, narrow, and often precise down to meaningless pettiness. These two descriptions are two competing images of knowledge which dominated the 19th century. Both had great influence on scientific problem-choice in their cultural environment - all over Europe there were representatives of both conceptions. The influence of the German Naturphilosophie - an influence which combined the Hegel-Fichte-Schelling romanticism with Kantian metaphysics and search for underlying principles - spread to England through the work of Coleridge and Thomas Young. It introduced the acceptability of the Eulerian wave theory as a legitimate area of research, and in the hands of Young and Fresnel it became triumphant. Though Young tried to look Newtonian [1], his was the first direct proof that Newton had been wrong in the critical dialogue between the Newtonian and the Leibniz-Euler-Kantian Scientific Research Programmes.

On the other hand, the German scientists — physicists and biologists alike — who turned away from Naturphilosophie with disgust, instituted what practically amounted to a reign of terror of empiricism.

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[1] Thomas Young, «On the Theory of Light and Colours» (Phil. Trans., 1802, p. 12) says:

«A further consideration of the colours of thin plates, as they are described in the second book of Newton's optics, has converted that prepossession which I before entertained for the undulatory system of light, into a very strong conviction of its truth and sufficiency; a conviction which has been since most strikingly confirmed by an analysis of the colours of striated substances. The phenomena of thin plates are indeed so singular, that their general complexion is not without great difficulty reconcileable to any theory, however complicated, that has hitherto been applied to them; and some of the principal circumstances have never been explained by the most gratuitous assumptions; but it will appear that the minutest particulars of these phenomena are not only perfectly consistent with the theory which will now be detailed, but that they are all the necessary consequences of that theory, without any auxiliary suppositions; and this by inference so simlpe, that they become particular corollaries, which scarcely require a distinct enumeration.

A more extensive examination of Newton's writings has shown me that he was in reality the first that suggested such a theory as I shall endeavour to maintain; that his own opinions varied less from his theory than is now almost universally supposed; and that a variety of arguments have been advanced, as if to confute him, which may be found nearly in a similar form in his own works; and by no less a mathematician than Leonard Euler, whose system of light, as far as it is worthy of notice, either was, or might have been, wholly borrowed from Newton, Hooke, Huygens, and Malebranche».

Scientific papers which contained even the slightest attempt at a theory not yet proved by hard experiment were rejected (Helmholtz's famous paper of 1847, rejected by Poggendorff, is a case in point). Their influence was such that in 1905 Einstein's relativity paper was almost rejected - it was Max von Laue, who happened to be assistant editor in charge, who saved it.

The controversy around the Scientific Research Programmes was no less violent than that about the images of knowledge. The Newtonian scientific metaphysics, according to which the world consists of discrete particles with *central forces* (and only such) acting between them at a distance, was crumbling in light of the other metaphysics of the later Newtonians and of the wave theory. Yet it was dominant enough for many years to prevent ϕrsted's discovery (1820) of the magnetic lines of forces of a current-carrying wire, because he was only looking for lines of force radially pointing out of the wire. ϕrsted's Newtonian research programme did not disturb his Naturphilosophie-influenced image of knowledge: romantic, mystical and speculative[1].

Two remarks should be made here: it is often claimed that the Naturphilosophen were anti-scientific and even anti-intellectual in their approach, and it is also often implied, if not directly stated, that they knew no science. This again is a myth, fed by the inductionist historiography of the Victorian image of knowledge. It is enough to read Hegel's works on natural philosophy, or Schelling's 1832 appraisal of the state of electromagnetism, to realize that this view has no basis. We must get used to the idea that, in spite of their full awareness of the great achievements of science, their image of science was unlike ours and unlike that of our Victorian grandfathers.

The Victorian image of knowledge was a vulgarized Baconianism of great extremity and power[2]. It is knowledge triumphant: certain, inductively collected, down-to-earth knowledge. And the Victorians, in their thoroughness, documented their view of knowledge by rewriting the history of the accumulation of scientific knowledge[3]. This huge undertaking was then supplemented by creating the tradition of typical 19th

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[1] ϕrsted's great literary work: «The Soul in Nature» (Dawson Reprints, London, 1966) is a revealing document of his image of knowledge.

[2] On this see Arnold Thackray, «The Industrial Revolution and the Image of Science» in A. Thackray and E. Mendelsohn (eds.) «Science and Values» (Humanities Press) - in press.

[3] See H. Butterfield, «The Whig Interpretation of History» (London 1932).

century biographers of great scientists, all of whom had been ardent experimentalists and hard workers from early childhood.

It is the 19th century Victorian image of knowledge which also created the myth of the Protestant ethic as a cause for the development of modern science. The self-image of the 17th century, as we have seen, was that the theologically non-partisan, latitudinarian gentlemen of leisure were the source of all new ideas. This image was now systematically replaced by the hard-working, preferably lower middleclass boy, who lives up to God's expectations from the genius with which he had been endowed. The biographies started with Macauley's Bacon and went through Brewster's Newton, Pouso's Davy, Peacock's Young and countless others.

The Victorian image of science was sustained by another new tradition: the tradition of writing textbooks - no longer treatises written from the personal point of view of the authors, but rather impersonal reports of the organized and well-ordered progress of scientific success. The need for textbooks arose from teaching science on a vast scale in the newly institutionalized scientific profession. The result was the final establishment of the Victorian image of knowledge.

The first such typical textbook, of enormous influence, was Thomson and Tait's «Principles of Mechanics and Dynamics» which first appeared under the title «Treatise on Natural Philosophy» in 1856. The greatest influence in spreading the vulgar-Bacon image of science was Sir John Herschel's «A Preliminary Discource on the Study of Natural Philosophy[1].

The nineteenth century neared its end with a slow self-assured arrogance. The greatest secrets of nature had been deciphered, the building of classical physics almost completed, a method of science discovered. The Victorian spirit of smug self-satisfaction relying on frugal Puritan ethics was triumphant. At first only in England, but then among the scientific establishments of all Europe, it was dominant. The great classical inductivist histories of physics, chemistry and biology will attest this. They all show that science is growing by accumulation and that, ever since the 'right', i.e. empirical, method banished all speculative fancy from science, mistakes have almost become extinct. After the discovery of the cell theory in biology, the periodic table in chemistry, and the electro-magnetic character of light in physics, all

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[1] Johnson Reprint, 1966. See also J. Agassi's, «Sir J. Herschel's Philosophy of Success» (HSPS, 1, 1969, p. 1).

that had still to be discovered were details. The future scientist would mainly be responsible for putting exactness and order where it was still missing: «look after the next decimal place, and physical theories will take care of themselves»[1] or Kelvin's famous saying: «I often say that when you can measure what you are speaking about and express it in numbers, you know something about it; but when you cannot measure it, when you cannot express it in numbers, your knowledge is of a meager and unsatisfactory kind; it may be the beginning of knowledge, but you have scarcely in your thoughts advanced to the stage of SCIENCE, whatever the matter may be».

The few dissenting voices were heard from a small number of representatives of the great synthetic tradition, a minority of whom were natural scientists - the others being epistemologists or social sci-

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[1] This is by Richtmyer «The Romance of the Next Decimal Place» (Science, 75 (1932) 5) and is quoted by Lawrence Baldash in his «Completeness of Nineteenth Century Science» (ISIS 63 (1972), pp. 48 - 58). Here Baldash asserts that the feeling of completeness of science «was more of a low-grade infection but nevertheless very real» as against S. Brush who in his «Thermodynamics and History» (The Graduate Journal 7 (1967), pp. 477 - 565) maintains that there was no such feeling. I am firmly on the side of Baldash. Actually the typical case to whom the Victorian calm in physics does not apply is Boltzmann, on whom Brush heavily relies, and who indeed represents the Vienna of the late 19th century where the Victorian image of science was rejected even before the great changes in the body of knowledge took place with Einstein. On this see my «Boltzmann...» quoted above, and «The Austrian Mind» by W. M. Johnston (U. of Calif. Press, 1972).

Some of the typical 19th century historians of science to whom I was referring are Rosenberger: «Geschichte der Physik», Knopp: Geschichte der Chemie», Nordenshjöld: «History of Biology», Damppier: «History of Science».

It is interesting to note that the Victorian image of knowledge became fortified by the great number of scientific biographies written in England in the 19th century. They all carry the standard image of the hard-working experimental scientist who objectively approaches the hard facts and thus cracks open Nature's secrets one by one. It is enough to recall the delightful biographies of Newton by Brewster, Davy by Pouso, Young by Peacock, Faraday by Tyndall, Tait by Knott, Maxwell by Garnett, etc.

The painstaking activity of making the measurement more and more accurate, and considering this the scientist's chief occupation, became a mere repetition of what went on in Newtonian astronomy in the early 19th century; it should have been a warning to the thousands of mathematical physicists today who are busy sharpening their analytical tools or devising better approximation methods, presupposing that the basic theory (Quantum mechanics in the Copenhagen interpretation) is final and here to stay.

entists: Marx, Weber, Freud, Boltzmann. A dissenting culture as against individual voices existed only in Vienna: for Freud, Boltzmann, Karl Kraus, von Hoffmannsthal, Schumpeter, Hans Kelsen and many of their friends and co-citizens in the crumbling Austro-Hungarian monarchy it was questionable whether knowledge was indeed triumphant. And it was here that the foundations of the twentieth century image of knowledge were laid: that achievement does not constitute progress, that science cannot give certainty; that no theory describes reality, and that theories are mere instruments of prediction. The only certainly from now on is in the positive, palpable, immediate production of detail, not in understanding in a deeper sense. Instrumentalism, that symbol of broken-down relevance, revived, and with it came positivism. That positivism at its peak —logical positivism— flourished in Vienna is small wonder. It is my historical thesis that instrumentalism causes degenerative problem-shifts in scientific research programmes, and that we are in a period of stagnation in basic research because of it. However, it had a positive by-product: since understanding ceased being the raison d'être of science, the Victorian problem that «the sum of knowledge is at present, at any rate, a diverging, not a converging series» disappeared [1]. For a while, sum of knowledge was no issue - the issue was the next prediction.

This, however, brings my story to the twentieth century, and this will be taken up by my friend and colleague S. Jaki. But before giving the floor to him, permit me to draw a few lessons from the above history: knowledge grows dialectically in the form of critical dialogues between competing scientific metaphysics and competing social images of knowledge. In order to ensure progress, such dialogues have to be encouraged and even supported. With each dogma establishing itself as a result of success in a given area of the spectrum of knowledge, a view opposite to it has to be encouraged also. Value should be attributed to the existence of the open dialogue, and not to indoctrination of knowledge as seen by the temporarily winning side. This will keep the limelight on problems and not on solutions, and thus the continuous shift in the division of the spectrum of knowledge according to the major problem-situations will be assured: this will take care of relevance. On the transmission side this can be achieved by teaching science in

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[1] J. J. Thomson in his presidential address to the British Association 1909, quoted by Badash, op.cit.

its historical context: cultural history will become the history of competing scientific research programmes with their metaphysical core and the competing social images of knowledge. Whether this will solve the moral crisis also I do not pretend to know. But at least this would keep us on the side of the rationalists. I believe we have not reached a stage where we must return to seek wisdom in revelation - fundamentally our course for the past two thousand years does constitute progress, and in order to avoid bankruptcy we must stick to knowledge.