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Paradigms, Puzzles and Root Metaphors: Georg Christoph Lichtenberg and the Exact Sciences

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Pepper's root metaphor theory supplies a means of analyzing different conceptions of science. Pepper's work shows that scientific theories and what he calls unrestricted world hypotheses originate in metaphors. The career of the eighteenth century physicist, Georg Christoph Lichtenberg, illustrates the metaphorical nature of the act of scientific discovery. Pepper's root metaphor theory offers an explanation for Lichtenberg's inability to incorporate his discoveries in the eighteenth century's mechanistic framework.

The study of the history of science has attracted new interest in recent years. Much has been learned about the development of the individual sciences. Simultaneously, our understanding of the relationship between scientists, scientific communities, and society has deepened. There are, however, a number of contested issues which divide historians and philosophers of science. None of these questions is more fundamental than determining the status of scientific knowledge. Since 1962, much of this debate has centered on Thomas Kuhn's theory of *The Structure of Scientific Revolutions* (Kuhn, 1970). Kuhn and his supporters contend that science represents more than a system of laboratory procedures. To Kuhn science indicates a commitment to certain problems as well as the means of resolving these questions. Kuhn draws the different elements of his theory together in his notion of paradigms. Kuhn maintains that paradigms make normal science possible. Normal science is charged with resolving the puzzles that appear in the everyday business of conducting the scientific enterprise. Thus to Kuhn the history of science becomes the history of the succession of paradigms which have served to determine the form and content of science.

Many have critized Kuhn's position. Karl Popper speaks for some of Kuhn's opponents when he contends that Kuhn has misunderstood the nature of scientific investigations. According to Popper, science represents a gradual overcoming of error through a rigorous examination of nature. Popper maintains that science possesses a distinctive epistemology which is premised upon experimental verification. Individual scientists propose theories. The issue is, then, put to nature. If a theory meets its test, it is provisionally accepted until such time as either sufficient anomalies arise which force scientists to abandon it, or a better theory is proposed which replaces it (Popper, 1970).

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Kuhn has met his critics on several occasions. In 1970 he conceded that his use of the term 'paradigm' had been vague (Kuhn, 1970, p. 181). Nevertheless, Kuhn remains convinced that his critics are wrong in charging that his theory of science is subjective and irrational. Frederick Suppe, who organized one of the discussions on Kuhn's work, judged Kuhn the loser in the proceedings. Suppe concluded that once again the basis of scientific knowledge has returned to 'the correspondence between theories and reality.' The debate, however, has not ended with Suppe's announcement of the victory of neo-positivism. What is needed is a clearer means of formulating the problem (Suppe, 1977).

Forty years ago Stephen Pepper raised many of these issues in his *World Hypotheses*. I will argue in this paper that Pepper's root metaphor theory supplies both an explanation for the origin of different conceptions of science and a framework for discussing the history of science. Moreover, Pepper's root metaphor theory avoids many of the pitfalls involved in Kuhn's theory. My remarks will break into three parts. First, I will summarize Pepper's conception of the relationship between scientific thought and what he calls structural world hypotheses. Second, I propose to examine two episodes in the life of the eighteenth century scientist Georg Christoph Lichtenberg. Pepper's root metaphor theory will be used to analyze Lichtenberg's work in the exact sciences. Finally, I will return to the problem of the status of scientific knowledge and consider the implications of Pepper's root metaphor theory for the history of science.

Near the beginning of *World Hypotheses*, Pepper asks "In what ways, if at all, does a structural world theory differ from a scientific theory?" (Pepper, 1970, p. 82). His answer is as simple as his question is direct. There is, Pepper maintains, no difference between these modes of thought. Pepper recognized, however, that in practice there did appear to be a difference. He explained:

> The latter (a scientific hypothesis) is admittedly artificial and clearly distinguishable from the evidence it systematizes. The greater the refinement of the data and multiplicative corroboration the more unmistakable the distinction between evidence and hypothesis . . . . In contrast, a structural world theory is not clearly distinguishable from much of the evidence it organizes, and the more highly developed it is, the less can the distinction be made. It follows that a structural world hypothesis is not conceived as artificial but as the natural and inevitable reflection of the structure of the evidence organized, as if the reference of the symbols passed directly out into the natural structures symbolized or suggested the immediate intuition of them. It acquires, accordingly, a cognitive value indistinguishable from that of the evidence it organizes. (Pepper, 1970, p. 82-83).

Here Pepper has differentiated between scientific theories and structural world hypotheses on the basis of the former's "artificial[ity]" as compared to the latter's "natural[ness]." A scientist claims that his theory is only a convenience. He will abandon it when he finds a better means of representing nature.

A metaphysician considers a structural world hypothesis as more than an implement. A world hypothesis presents truth. It cannot be discarded without altering the nature of things.

Pepper amplified on this distinction in later publications. A year after publishing *World Hypotheses* he explained that the best way to understand the difference between a scientific theory and a world hypothesis is to recognize the degree of restriction present in science. A biologist examines a certain order of problems and can always say that a given problem lies outside the biological sciences. A metaphysician cannot make this claim. Structural world hypotheses have as their definitive characteristic their claim to unrestricted scope.

There is, nevertheless, a fundamental similarity between scientific and structural hypotheses. "Cognition, after all," Pepper concludes, "is cognition . . . and in general, there is no difference between a scientific and metaphysical hypothesis, except that the former is restricted and the latter is not" (Pepper, 1943, p. 253). Clearly, Pepper considered scientific hypotheses as restricted instances of the more general structural hypotheses. Pepper never tired in his attempt to establish the priority of structural world hypotheses in cognition. These statements raise two serious questions. First, where do world hypotheses originate; and, second, how do they develop?

Pepper resolved these questions in his root metaphor theory. Pepper explained his theory of the origin of world hypotheses when he wrote:

> The suggestion is that world hypotheses get started like any man's everyday hypothesis framed to solve some puzzling practical problem. The man looks back over his past experience for some analogous situation which might be applicable to the present problem. Similarly, a philosopher, puzzled about the nature of the universe, looks around for some pregnant experience that appears to be a good sample of the nature of things. This is his root metaphor. He analyzes his sample, selects its structural elements, and generalizes them as guiding concepts for a world hypothesis of unlimited scope. This set of concepts becomes the set of his categories of his world hypothesis. (Pepper, 1973, pp. 197-198).

Science and metaphysics arise when men and women are faced with problems. Individuals turn to their past experiences and find 'some analogous situation' which offers them the possibility of solving their difficulties. What they have discovered is a root metaphor which becomes generalized in time. Pepper contended that there had been a series of possible root metaphors which had been developed into world hypotheses. Four relatively adequate world hypotheses exist in the present world: Formism based on the root metaphor of similarity; mechanism based on the root metaphor of the machine; organicism based upon the root metaphor of the historic event; and, contextualism based upon the root metaphor of the purposive act.

Can Pepper's root metaphor theory clarify the development of the history of science? I believe it can. One way to show this is to examine a specific problem in the development of science. As is often the case, failure is sometimes more instructive than success. I propose to describe two episodes in the career of the eighteenth century scientist Georg Christoph Lichtenberg. Lichtenberg's work illustrates the dilemma facing a scientist who grows disillusioned with the available conceptions of science. When Lichtenberg chanced upon a set of facts that could not be explained within the confines of the eighteenth century mathematics or experimental physics, he refused to sacrifice his discoveries. Lichtenberg retreated from the practice of science. He became one of the earliest critics of the mathematical-deductive and mechanistic conceptions of science. Before examining these episodes, it will prove useful to briefly sketch the essentials of his life.

Georg Christoph Lichtenberg

Georg Christoph Lichtenberg was born on 1 July 1742 near Darmstadt in the village of Ober-Ramstadt. As a child Lichtenberg developed a progressive spinal disorder that was to leave him a hunchback for life. His father was a well respected pietist minister who was known for his experiments in mechanics. Lichtenberg's father died when his son was nine years old. During these years his education followed the normal course. Lichtenberg entered a local Latin school and later enrolled in the Darmstädter Paedagogium. He remained at this gymnasium for nine years. Lack of money forced Lichtenberg to return to his mother's home upon graduation. Fortunately, his mother succeeded in convincing the Hessan state authorities to award Lichtenberg with a scholarship for the University of Göttingen.

Georg August University was hardly a quarter of a century old when Lichtenberg arrived in Göttingen. He chose to study mathematics and physics. Between 1763 and 1767 Lichtenberg distinguished himself as a mathematician, experimenter, and a devoted student of English culture. His passion for England served him throughout his life. Unlike many who were forced to leave the University after graduation, Lichtenberg found immediate work as a tutor for visiting English students in Göttingen.

In 1771, Lichtenberg's English connections won him a commission from the Royal Society. The Royal Society requested Lichtenberg to determine the geographical position of several northern German cities. He completed his calculations in 1773 and was rewarded with a second assignment. Lichtenberg was named to edit the notebooks of the self-educated Göttingen astronomer, Tobias Mayer. Mayer had won recognition for his careful observation of the moon's orbit. He had hoped that he would be able to translate this data into a means of determining longitude at sea. Lichtenberg faced a tremendous task. Mayer's notes stretched over two decades. Lichtenberg completed only one volume. It was published in 1774. The book was a success. Astronomers payed tribute to Lichtenberg's careful organization of the data by naming one of the moon's minor craters after him (Hahn, 1927).

The Petersberg Problem

Throughout his career, Lichtenberg sought for a method that would provide a unity between the phenomenal world and science's representation of that world. During the 1770's Lichtenberg wrestled with a number of important scientific problems. In two separate episodes Lichtenberg called a halt to his researches. The first episode concerned a problem in the theory of probability; the second involved the discovery of a new characteristic of electricity. An examination of each of these episodes will suggest the main features of Lichtenberg's conception of science.

Lichtenberg's first important scientific publication came in 1770. In that year he was appointed *professor philosophiae extraordinarious*. It was customary for newly appointed junior professors to present a paper describing some part of their work they intended to discuss during their lectures. Lichtenberg chose to speculate on the Petersberg problem.[1]

The Petersberg problem arises out of an imaginary wager. "Peter" promises to pay "Paul" two half dollars if a heads does not appear on the first toss of the coin and 2^{r-1} half dollars if no heads occurs until the r^{th} toss. The problem is to determine the amount of "Paul's" expectation; and, secondly, the amount "Paul" should give "Peter" before the first toss if the odds are to be fair (Keynes, 1929).

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[1]See: Keynes, J.M. A *Treatise on Probability.* London: 1929, p. 316. Todhunter. I. *A History of Mathematical Theory of Probability from the Time of Pascal to Laplace.* London: 1865. Todhunter credits Daniel Bernoulli (1700-1782) as being the first mathematician to notice the problem which appeared in the *Comentarii* of the Petersberg Academy. The mathematical solution is presented in Keynes *Treatise on Probability.* The formula reads:

$$\sum_{1}^{N} (1/2)^{R} 2^{R-1}$$

if the number of tosses is not in any case to exceed N in all, and

$$\sum_{1}^{\infty} (1/2)^{R} 2^{R-1},$$

if this restriction is removed. That is to say, "Paul" should pay $N/2$ half dollars in the first case, and an infinite amount in the second. Keynes concluded that nothing could be more paradoxical than this. Keynes does not discuss Lichtenberg's attempt at a solution.

The Petersberg problem had a distinguished history. Lichtenberg's contemporaries, Poisson and Condorcet, tried to solve it through *a priori* mathematics. They failed. They claimed that "Peter" had accepted an unfulfillable obligation. If by chance, heads does not occur until the hundredth toss, "Peter" will owe "Paul" a quantity of silver greater than the mass of the sun. Lichtenberg chose to follow a different track. He set out to solve the problem empirically.[2] He proposed to toss the coin a certain number of times and log the results. After a number of tosses the coin landed on its edge. This is a mathematically inconsequential event. Lichtenberg, however, took it seriously and ended his experiment. There was no way to tabulate this data. No theory of probability could account for such a discrepancy.

Lichtenberg (1770) used his essay to explain his dissatisfaction with what might be called the mathematical-deductive approach to science. He explained his conception of mathematics in a long passage which appeared in his private diaries. He wrote:

> In applying his inferences to Nature, the mathematician often finds considerable deviations from what he has been led to expect by his calculations. It is not very difficult to give the reason for this in a general way and to perceive that it cannot be the fault of mathematics. He abstracts from our world a world of his own, whose lawbooks lie in his own hands (so to speak). No force can be at work in his world before he himself has placed it there: He knows what happens anywhere in that world, and he reads prophecies from his formulas: He annuls laws without working miracles: Decrees others; and gives his world any shape he likes. So far mathematics can take him, and everything in it is as certain as the eternal truths upon which it rests. . . . When, therefore, the mathematical expert makes an inference about our system on the basis of his own, he must observe the differences arising on all those occasions when the general law in HERE modified by special circumstances which THERE were not taken into account. If a bomb which, according to calculation, should reach its goal by describing a parabolic curve, neither reaches its goal nor describes a parabola: If a force which should lift a certain weight is hardly sufficient to set the lifting gear in motion: Then the fault lies not in the calculation, for in the world as the mathematician conceived it this force would in fact have lifted that weight, and the bomb have reached its target by a parabolic curve. Nor can the fault lie in general laws wrongly abstracted: should he, because of these occurrences reject the laws of Galileo, or establish new laws for the action of levers: No—the fault lay in his believing that his system was already wholly at one with ours. (Cited in Stern, 1959, p. 295)

Mathematics cannot be blamed for the coin landing on its edge. The fault lies in the scientist's misplaced faith which led him to assume that there is an identity between 'his system' and the universe. What Lichtenberg called *a priori* mathematics or the mathematical-deductive method is premised on the assumption that nature is reducible to number. Lichtenberg believed that reality was different than the mathematicians' description of it. Thus, Lichtenberg concluded that mathematics is a useful tool for organizing some kinds

[2]Keynes, J.M. *Treatise*, (1929, pp. 316, 361-362). Lichtenberg's contemporary Georges Louis Leclerc, Comte de Buffon hired a child to perform the tosses while he logged the results. Buffon published his findings in 1777 in an article entitled *Essai d'arithmetique morale*. He tallied 1792 tails and 2040 heads.

of knowledge. Nature, however, remains elusive. The goal of science is to represent the universe with all 'of its considerable deviations.'

The events of the next six years have been traced. They include Lichtenberg's geographical observations; his editorship of Mayer's papers; and, a second journey to England. Lichtenberg remained in England for fifteen months in 1774 and 1775. He used his time to visit the observatory at Richmond; tutor the King on astronomy and physics; and, collect an assortment of experimental equipment. He returned to Göttingen shortly before Christmas in 1775. During his absence he had been nominated *professor ordinarius*. He remained in Göttingen for the rest of his life.

The Lichtenberg "Figures"

While in England Lichtenberg was struck with the idea of repeating a number of experiments that he had seen on a grander scale. Today, his theory of how to go about making discoveries seems curious. "Microscopes should be invented," Lichtenberg speculated "for every kind of investigation, and where that is impossible, experiments should be conducted on a large scale. This is the only direct road to new discovery" (cited in Stern, 1959, p. 294). Lichtenberg theorized that if he increased the size of his equipment sufficiently he would come upon a new discovery. Perhaps the strangest thing is that his ideas worked.

In 1777, Lichtenberg launched a series of experiments dealing with electricity. He instructed his assistant to prepare a huge electrophorus nearly two meters in diameter. Lichtenberg's mechanic, Klindworth, neglected to clean the laboratory after preparing the dielectric plate. Ten months later Lichtenberg publicly reported what happened in his "De nova methodo naturam ad motum fluidi electrici investigandi" (cited in Hahn, 1927, p. 36) [On a new method to investigate the motion of electric fluid.]. Lichtenberg explained:

> About the beginning of spring 1777 my electrophorus was just finished. In my room all was still covered with very fine resin powder that had risen during planning and polishing of the cake and the metal disk, and later it lay on wall and books. When air motion occurred, it deposited on the metal disk of the electrophorus, to my great annoyance. However, it was not until I had hung the disk on the ceiling of the room many times, that the powder deposited on the cake; then I could not move it uniformly as had occurred on the metal disk, but to my great joy it was arranged like small stars at certain points. These were dull in the beginning and difficult to see; when I sprinkled more dust intentionally, however, they became very clear and often resembled embossed work. Sometimes innumerable stars, the Milky Way and bigger suns appeared. The bows were dull on their concave side, and decorated manifoldly with rays on their convex side. Marvellous small twigs emered; the twigs produced by frost and grades of shading and finally different figures of particular shape were seen . . . . However, a very pleasant play occurred to me, when I saw these figures could scarcely be destroyed. Even if I wiped off the dust carefully with a feather or a hare's paw, I could nevertheless not prevent that the figures, which were destroyed just before, quickly developed again to some extent anew and still wonderfully. Therefore, I painted a piece of black paper with adhesive paste, laid it down

> on the figures and pressed it lightly. So I succeeded in making several copies of the figures. I have presented these six copies to the Royal Society. This new variety of printing was very favourable for me in order to progress further quickly, because I had neither pleasure nor time to sketch or destroy all the figures. (Cited in Takahasi, 1979, pp. 3-4)

This passage offers an opportunity to examine the process of scientific discovery. What is most striking in Lichtenberg's description is his use of metaphor. He begins with a simple narration of the events leading up to his discovery. Then, the new phenomenon appears. Lichtenberg tells us that the resinous dust "was arranged like small stars at certain points." The figures were difficult to make out. Lichtenberg "sprinkled more dust intentionally" and, suddenly, the little dust figures were transformed. They no longer resembled "stars." They were "the Milky Way and bigger suns." They possessed marvelous qualities. They were "decorated manifoldly with rays .... Small twigs emerged; the twigs produced by frost on the glass resemble them." Lichtenberg described his discovery in a series of poetic images. Each of his metaphors asserts a correspondence between his figures and the rest of nature. To Lichtenberg there is a hidden likeness between the way the stars shimmer in the sky, the manner in which ice crystals form on window panes, and shape that dust takes on a dielectric plate after it has received a charge. Lichtenberg did not use these metaphors to merely embellish his report. Rather, he chose them because they intimated the underlying quality of his discovery. He hoped that he had found the clue which would lead to an understanding of the relationship of light, electricity, and heat.

The Nature of Scientific Discovery

Stern commented on this passage in his study of Lichtenberg's aphorisms. Stern remarked:

> This is not the language of late eighteenth-century science; and what recalls an earlier, pre-Newtonian style is not the abundance of analogy but the abandonment to it, the readiness to follow analogy beyond the point where it is merely useful explanation into that wide imaginative realm where it lives in its own right and for its own ends, a realm which Newtonian science (though not Newton himself) had proscribed as "nonscientific." (Stern, 1959, p. 42)

Stern is both right and wrong. Certainly, Lichtenberg was unwilling to reduce his findings to the eighteenth century mechanistic framework. In this sense, Lichtenberg had "entered a realm which Newtonian science had prescribed as 'nonscientific'." Perhaps, however, what makes Lichtenberg's description "nonscientific" is not its metaphorical quality but that the kind of metaphors that Lichtenberg created fell outside the mechanistic conception of physics.

Bronowski discussed this theme in his short book *Science and Human Values* when he argued that all science "grows from a comparison. It has seized a likeness between two unlike appearances" (Bronowski, 1965, p. 15). Bronowski does not use the word metaphor. Nevertheless, it is clear that when he speaks of science as discovering the shared "hidden structure" between two apparently dissimilar phenomena he is talking about the metaphorical character of science. It was, in fact, the perception of just such a metaphor that prompted Newton's discoveries. The circumstances of Newton's discovery of the inverse square law are well known. In 1665, the rector of Cambridge University closed the school because of an outbreak of the plague. Newton was compelled to spend the next eighteen months at his mother's cottage. "I was," Newton later recalled, "in the prime of my age for invention, and minded Mathematics and Philosophy more than at any time since" (cited in Gillispie, 1973, p. 120). What Newton "minded" was the relationship between terrestrial and celestial physics. One day he noticed an apple fall. There is nothing remarkable in this. Apples have been falling from trees for a long time. What was remarkable was that Newton grasped the hidden similarity between the falling of an apple and the orbiting of the moon around the earth. Newtonian physics was born in this instant. "The apple in the summer garden and the grave moon overhead," Bronowski observed, "are surely as unlike in their movements as two things can be. Newton traced in them two expressions of a single concept, gravitation: and the concept (and the unity) are in that sense his free creation" (Bronowski, 1965, p. 15). The *Principia* or the *Mathematical Principles of Natural Philosophy* originated in a metaphor in which the moon and an apple are found to be one. Newton was a genius. He succeeded in translating a metaphor into a scientific tradition.

Lichtenberg, however, refused to reduce his discovery to number and place it within the dominant mechanistic conception of physics. His contemporaries praised him for producing a novel demonstration for Franklin's newly discovered positive and negative electricity. Lichtenberg would not use the words "positive" and "negative" to describe the mysterious electric fluid. After this episode, Lichtenberg lived for another twenty three years. There were no more major discoveries. He devoted himself to teaching and to editing a scientific journal and a popular almanac. He engaged in a polemical exchange with Johann Lavater over Lavater's theory of physiognomy. During these years Lichtenberg's reputation steadily grew. Lichtenberg's contemporaries judged his edition of Johann Erxleben's *Anfangsgründe der Naturlehre* the finest available general introduction to physics. The book was used throughout Germany and was translated for use in foreign universities (Kleinert, 1980).

There remains one final element of Lichtenberg's work. Early in his student days Lichtenberg began to keep a series of notebooks in which he recorded his observations on life. Lichtenberg called these diaries his "Waste-Books." His heirs discovered these notebooks after his death. Their publication won a place for Lichtenberg in German literary studies. Today, Lichtenberg is remembered as an aphorist. His scientific work has been all but forgotten.

Root Metaphors in Science

How does a historian of science make sense of Lichtenberg's career as an experimental physicist? Here is where Pepper's root metaphor theory is of particular value. Lichtenberg rejected what Pepper described as formism and mechanism. This can be seen in his handling of the Petersberg problem and his discovery of his electrical "figures." Clearly, Lichtenberg's disenchantment with the mathematical-deductive method goes deeper than the landing of a coin on its edge. Lichtenberg described mathematicians as individuals who acted as if they held the "law books" of nature in their hands. In Pepper's terminology, the mathematicians' law books might be described as a set of *characters* which the mathematician uses to interpret nature's *particulars*. Lichtenberg hoped that a theory of probability would bridge the gap between the abstract characters and nature's concrete particulars. He discovered, however, that no formist system could account for a chance event such as a coin's landing on its edge. Mathematicians claim universal scope for their approach to nature. They fail when they are confronted with what cosmologists call "naked singularities." Lichtenberg reserved his final judgment on the formist or mathematical-deductive method for his notebooks. He wrote:

> What makes the explanation of natural phenomena so difficult is just this, that we count too much on that which has universal occurrence but make too little allowance for the vigorous exception, and alas, we cannot do otherwise. (Cited in Stern, 1959, p. 87)

The mathematical-deductive system possesses tremendous scope. It proves itself inadequate, however, through its imprecise treatment of "vigorous exceptions."

Experimental physics held the possibility of solving Lichtenberg's difficulties with mathematics. "The mathematician," Lichtenberg explained, "is satisfied when he has shown mere probability, the physicist's job is to determine which among the countless possible hypotheses is the only one chosen by the creator" (cited in Stern, 1959, p. 83). Lichtenberg, however, was unable to place his discovery within the eighteenth century's mechanistic framework. "It isn't," Lichtenberg observed, "as irritating to explain a phenomenon by means of some Mechanics and a big dose of the Incomprehensible as to do so entirely by means of Mechanics; which is to say *docta ignorantia* is less reprehensible than *indocta* (cited in Stern, 1959, p. 88). What Lichtenberg criticized in "Mechanics" parallels Pepper's analysis of mechanism. The mechanist has no difficulty with the precision of his conceptual framework. His difficulties arise when he chances upon a new order of phenomena. Mechanism suffers from an inadequate scope. The "Lichtenberg figures" could not be assimilated within the existing mechanistic conception of physics. They remained "nonscientific" until the twentieth century when the discovery of the ion and the shift to the field theory of electricity provided a means of understanding Lichtenberg's discovery.

Lichtenberg was an extraordinary scientist. Throughout his career he reflected on the status of scientific knowledge. He was convinced that science originated in the perception of a similarity between dissimilar objects. "The way to determine the secret workings of Nature," he maintained, "is from analogous cases where one has caught her in the act" (cited in Stern, 1959, p. 293). To Lichtenberg science represents the progressive extension and refinement of a series of metaphors. "I believe," he explained in his Waste-Books,

> that no heuristic lifting-gear is more useful than what I have called PARADIGMATA. I really don't see why we should not take Newton's optics as a model for a theory of calcination of metals. For even when dealing with the most firmly established facts—or those, at least which appear so—we must nowdays start by trying out quite new approaches . . . Of course, this rule for making inventions by means of paradigms is of no use to a fool—he is not fit for making inventions precisely because he is a fool. But even the good mind has to be prodded into seeing something new; indeed, it is almost only by such means that new things can be found in a novel manner. If (as Kastner once conjectured) Newton arrived at the Law of Gravity by way of his interest in light, then that is a paradigm. In considering the value of this expedient we should always remember that, where it makes use of it, the good mind still retains its natural freedom, and that therefore the use of paradigms does not obstruct approaches. (Cited in Stern, 1959, p. 296)

These words cost Lichtenberg dearly. He has explained his theory of scientific discovery. The inspiration comes in the perception of hidden likenesses. Paradigms articulate these metaphorical discoveries. Lichtenberg's teacher, Kastner, suggested that the Law of Gravity derived from Newton's "interest in light." Lichtenberg was not a fool. He discovered a hidden likeness between light, heat, and electricity. He was not, however, Newton. Clearly, Lichtenberg was far in advance of his contemporaries in his concern for the relationship of language, metaphor, paradigms and science. His failures in the pursuit of the exact sciences may, in fact, derive from this. As Lichtenberg grew older he lost the ability to practice science. It is as if his sensitivity to the problem of science made it impossible for him to practice science.

Lichtenberg came to distrust any attempt to systematize nature. He believed that language distorted reality. "What Bacon says about the harmfulness of systems," Lichtenberg wrote, "might be said about every word, namely, that not much more will be invented in a science once it is put into a system" (cited in Stern, 1959, p. 156). Lichtenberg felt it impossible to objectively describe any event. "It is an entirely unavoidable error in all languages," he explained, "they express only the Genera of concepts and rarely say adequately what we want to say" (cited in Stern, 1959, p. 158). Lichtenberg fell into a kind of skepticism which made it impossible for him to succeed in his scientific work. Instead, he cultivated his aphorisms. The external world interested him less and less. He left what might be considered his farewell advice to his scientific colleagues in his Waste-Books. "Someone," he wrote,

> remarked to me once: Physicians should not say I have cured this man, but, This man didn't die in my care. In physics too one might say, For such and such a phenomenon I have determined causes whose absurdity cannot finally be proved, instead of saying, I have EXPLAINED it. (Cited in Stern, 1959, p. 297)

Lichtenberg chose to give himself "to the world of words."

Lichtenberg was rediscovered in the nineteenth century. Schopenhauer praised him. Nietzsche called him one of the three German authors who deserved a second reading. (The other two were Goethe.) In 1872, Ernst Mach described his debt to Lichtenberg in the Preface to his *Anatomy of Sensation*. In the twentieth century philosophers such as Carnap, Schlick, and Wittgenstein have identified Lichtenberg as one of their inspirations. The group of individuals who have named Lichtenberg as one of their precursors differ in many ways. Nevertheless, they share a commitment to a scrutiny of language and the role of language in representing and distorting reality. Some such as Wittgenstein went even further. Wittgenstein's conception of paradigm bears a striking similarity to Lichtenberg's theory of "paradigmata."

Pepper discussed many of these themes in one of his last essays, "Metaphor in Philosophy." This essay is noteworthy in several respects. First, it offers a clear exposition of his root metaphor; and second, Pepper examined the relationship between Kuhn's idea of paradigm and his own notion of root metaphors. Pepper concluded:

> If some form of root metaphor theory for unrestricted hypotheses is combined with a form of paradigm theory like Kuhn's for restricted hypotheses, it would suggest that the basis of all productive empirical theory is in principle metaphorical. It comes down simply to being realistic about what theories are as products of human creativity. (Pepper, 1973, p. 62)

The scientific career of Georg Christoph Lichtenberg confirms Pepper's theory of the relationship between science and unrestricted world hypotheses. Both derive from metaphors. They differ in two respects: world hypotheses are unrestricted and contemporary scientific hypotheses are conceived as artificial. Lichtenberg's work suggests that science proceeds on two different levels. On one level, scientists and technicians are immersed in the everyday business of puzzle solving. At another level, scientific work depends upon what Butterfield called an "adequate intellectual framework" (Butterfield, 1962, p. 205). It is this framework which defines what a fact is and how facts are to be linked together. Pepper's root metaphor theory offers a powerful instrument for showing where these "frameworks" originate and the manner in which they develop. Pepper's work holds the promise of leading students of the history of science to a deeper understanding of the meaning of scientific thought and the relationship of scientific thought and other forms of cognition.

Lichtenberg's career in the exact sciences is more troubling. He was in the vanguard of eighteenth century science. He was an original thinker. Today, his scientific work is all but forgotten. If remembered it is considered "nonscientific." Perhaps there is a lesson to this. When scientific theories cease to be scientific they return to their origins. They become poetry.

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