Where Knowledge Thrives:
The Role of the Metaphorical in Scientific Process

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Table of Contents

Introduction

My Ph. D. dissertation concerns one of the major issues in the rhetoric of science, the function of metaphors in scientific discourse. It explores the relation between knowledge production and a metaphorical kind of language that posits, specifies, and guides research on what is as yet unknown. An appropriate term for this kind of language is quasi-metaphor; its special function is to project the characteristics of a known referent within an established epistemic category to a virtual referent in a known or unknown epistemic category. My goal is to unite two opposite views about scientific language: one held by positivists and scientists, that scientific language must be clear, brief, and trope-free; the other held by post-structuralists and epistemologists, that scientific language is ultimately metaphorical, and it thus never has had the precision claimed by the positivists. I argue that scientific language is and must be simultaneously precise and open-ended in order to articulate knowledge as well as accommodate the unceasing production of knowledge. My methodology is to establish a theoretical frame in negotiation with metaphor theories and then apply it to case studies.

The initiating question of my dissertation is: What do metaphors have to do with scientific discourse? By scientific discourse, I mean the texts written by scientists and published in scientific periodicals, journals and books. I understand the important roles of metaphors in the discourse of scientific education and popularization. I have had serious doubts, however, that there could be metaphors in the discourse, because as a former scientist, I believe in the precision of scientific language, which is exactly what metaphors do not possess. Soon I realized that my idea about the precision of scientific language is the product of a positivist tradition running since Bacon and very much in question today. Current thinkers, like Burke, Foucault, Kuhn, and Derrida, basically made their names by emphasizing the metaphorical property of language, hence the scandalous status of all knowledge including the scientific. It has been hard to accept that what I learned about science and what I did as a scientist are nothing more than what a member of a black magic society does. As my distance from scientific discourse increased, however, I started to recognize that some borrowed and coined terms in science, such as the gene, are not as precise as I thought; especially in a historical scale, their present definitions are totally different from the ones initially proposed. Still, I cannot ignore the changes that science has brought into the world. As someone who studied antibiotics and the drug-resistance of antibiotics, I believed and still believe that knowledge is not scandalous, for antibiotics save lives, including my own. In treating knowledge as a scandal, the risk is undermining important discoveries like antibiotics. Such negation makes it impossible to grasp the connection between science and the humanities, and in consequence, humanists become irrelevant to science. Hence, the goal of my project is to investigate the relation between linguistic imprecision (or metaphors) and knowledge. If it is neither positivist nor scandalous, then what is the relation?

When I took a careful look at metaphor theories and terms like the gene, I found that those terms do not fit any metaphor theories, although they do have elements of this or that theory, and some scholars do identify *gene* as a metaphor. If *gene* is not a metaphor, then what kind of linguistic phenomenon is it? Furthermore, in the history of science, terms similar to the gene have had different fates. Some survived, as the gene; and other had been abandoned, as phlogiston, the overthrow of which is a famous scientific revolution. I accepted that phlogiston was rejected because of its vague meaning. Yet as long as the term *gene* is used, vague meaning threatens the soundness of its reason. What is the reason?

This group of scientific terms as a special linguistic phenomenon with interesting histories constitutes test cases in the relation of linguistic imprecision and knowledge. In order to understand the relation, what these terms are linguistically should be clearly identified, and then they should be traced through the history of science--on one hand, to examine knowledge production regarding these terms; on the other hand, to work out, if possible, a new history, especially about how knowledge emerges and changes in science. Structurally, my dissertation has three parts: the first part (Chapters One and Two) is the theoretical frame, in which I study metaphor theories in Chapter One and the anti-metaphor tradition in prose writing in Chapter Two. These two chapters are not only to establish the essential role of metaphors in knowledge production, but more importantly, to delineate that metaphors in science are a special group of metaphors that I call quasi-metaphors functioning to explore the unknown through the known. The second part (Chapter Three and Four) is case studies of the histories of the terms *phlogiston* and *gene*, respectively. The methodological reason for contrasting these two cases is that both have vague meaning, but phlogiston, though it had many supporters and still has at least one, is rejected, and the gene, though not without detractors, lives a productive life. The third part (Chapters Five and Six) are discussions on historiographical and philosophical implications of quasi-metaphor. Chapter Five takes a new look at the historiographical notion *scientific revolution* through metaphorical analysis. Chapter Six concludes that it takes both vagueness and precision in scientific language to build the intelligibility and complexity of the world. In short, part one theorizes quasi-metaphor; part two demonstrates quasi-metaphors in the history of science; and part three answers the question, why quasi-metaphor?

Chapter One surveys metaphor theories from Isocrates, who proposed the term *metaphora*, to George Lakoff. The classical studies on metaphor are in the realms of rhetoric and stylistics. Plato does not use the term metaphor, but his rational discourse contains many metaphors. He also is deeply concerned with the literal meaning of metaphor. Aristotle establishes a theory of metaphor based on recognition of resemblance and dissemblance between epistemic categories. Cicero and Quintilian develop Aristotelian theory further in stylistic studies. In particular, they endow metaphors with aesthetic values different from those of Aristotle. I. A. Richards makes the modern turn of metaphor studies by proposing a set of useful terms, and also a theory of how they function in the comprehension of metaphor. Max Black develops Richards' theory into the interactive view. Lakoff's theory treats the conventional (or dead) metaphors, "Love is a Journey" and the like, as the projections of the culture by which we live.

I examine these theories through a rather unusual perspective--the treatment of resemblance and dissemblance between categories, which, as far as I am concerned, is the coherence and key of metaphor studies. In this perspective, metaphor studies can be traced back to Plato, whose discussion on the metaphor "golden men" extends to current scholarly work. The case of "golden men" demonstrates the significance of categorical establishment in regard to the interpretation of metaphor. In Aristotle's hands, metaphor is defined by the categorization of the world. The recognition of the inter- and intra-relations of categories becomes the center of attention in modern cognitive theory of metaphor. Cicero and Quintilian, on the other hand, point out that some metaphors do not have literal paraphrases; that is to say, metaphors are more than linguistic ornaments. Contrary to the claim of some scholars, the classical studies on metaphor do go beyond poetic, rhetoric and stylistics.

In modern times, metaphor studies develop along three lines. First of all, the initiation is the acknowledgment that thinking is generally metaphorical. Accordingly, the focus of metaphor theories turns from rhetoric and stylistics to cognition. Second, metaphors as the indication of poetic genius become a part of daily language. Third, metaphor studies shift from what metaphor is to how metaphor means; in other words, from the descriptive to the methodological. The distinguished characteristic of modern theories is that resemblance and dissemblance are interchangeable; this is articulated in Max Black's interaction view. Lakovian theory suggests a metaphorical kind of thinking in which one (the target) domain is the projection of the other (the source) domain.

With respect to the pursuit of knowledge, although none of the theorists has a specific argument, their theories have some insights. Specifically, Aristotelian theory builds upon the categorization of the World, in which metaphors can be understood as a means of engaging with the World effectively. To create a metaphor, first, there must be well-established categories as well as genus and species of each category, then the thinking must cross two categories and discern the resemblance only discerned by the creator, and finally the creator represents the resemblance in the metaphor. Lakovian theory, on the other hand, underlines a general but potent way of thinking, projecting. More precisely, abstract concepts, such as love and idea, are often represented and comprehended by projecting cultural knowledge and life experience as metaphors. Implied by modern metaphors theories, moreover, resemblance can change at different stages of thinking process, especially in a knowledge-seeking process like scientific research.

Through the perspective that metaphor theories basically deals with resemblance and dissemblance between categories, the history of metaphor studies appear as a continuous development, and the modern turn of metaphor studies is not as dramatic some scholars observe from other perspectives.

Chapter Two deals with metaphors and prose. When Isocrates proposed the term *matephora*, he also initiated an anti-metaphor tradition that has been running in western thought ever since. He calls *metaphora* ornaments of language, which belongs to poetry but not prose. Plato reinforces this boundary drawn by Isocrates, although he is himself a master of metaphor in writing rational discourse. In Aristotle's studies of metaphors, he articulated that metaphors could function to bring forth new understanding and knowledge even in poetry. Meanwhile, he was also acutely aware that it was problematic to define terms with metaphors, or defining metaphors, for such attempts generally ended up arguing in metaphors.

Francis Bacon was the one who most famously urged that the plain style be the sole standard in scientific prose. One criterion of clear, concise and appropriate writing is that it is trope-free, for the content of scientific prose should be nothing more than the matter itself. A nice illustration of what can go wrong with metaphor is the interpretation in recent decades of Bacon's metaphors in a passage from *Of the Dignity and Advancement of Learning*. Three feminists scholars' readings of Bacon's passage suggest that, by the metaphors "hound, entering, penetrating, corners, and holes," Bacon delineates his new scientific methods of investigating nature as sexual intercourse, or even rape. Other scholars retort that those metaphors depict knowledge as light illuminating the dark. When a scholar goes as far as describing rape in violent and graphic language in order to ridicule the interpretations of the feminist scholars, the heated debate goes to exactly what Aristotle had warned—that the scholars are bound to argue in metaphors.

Moreover, there are cases in the history of science which are thought to demonstrate how misleading, and hence dangerous, metaphors are in research. Gaston Bachelard, for instance, quotes Dutch mathematician and physicist van Swinden's criticism of another Dutch physicist Brugmans' metaphor that "iron is a sponge for the magnetic field." My reading and analysis of Brugmans' and van Swinden's theories and texts conclude that what van Swinden considers problematic is Brugmans' conception of the interaction as well as his research methodology, rather than the use of metaphor *per se* as Bachelard claimed.

In tracking the anti-metaphor tradition, I catch an intriguing contradiction running from Isocrates, Plato, to Bacon, and to Bachelard: even though they are acutely aware the problems of metaphors, they cannot get rid of metaphors in the very texts elaborating the problems. It should not be entirely a surprise because Aristotle had recognized that metaphors can be means of conveying a new understanding and of producing knowledge. By my observation, the modern turn of metaphor studies happens at the point when what metaphor is defined as doing and what metaphor can do become irreconcilable in the lights of cognitive science. Accordingly, modern scholars turn to considering seriously the relation between metaphor and thought. Kenneth Burke gives prominence to the claim that metaphors play a role in knowledge making.

Further, Black's interaction view of metaphor articulates the dynamics of modern metaphor theories, in which resemblance and dissemblance are interchangeable depending on the context. More specifically, a metaphor can create new perceptions, the comprehension of a metaphor can bring up something new, and metaphors are open-ended.

On Black's theory, Richard Boyd develops his own regarding the significant roles that *catachresis* plays in theory change. By *catachresis*, he means "theory- constitutive scientific metaphors," employed to introduce theoretical terminology where none previously existed. Body recognizes that the terms possess an important character of metaphor, open-endedness, yet they do not permanently resist complete explication accomplished through successful scientific research. In order to establish how the metaphors function to accommodate the changing view on the casual structure of the world, Boyd approaches the terms through Hilary Putnam and Saul Kripke's theory on the reference of natural kind terms. To him, such terms refer to what is to be discovered or established, hence they are *nondefinitional* and *nonreferential*, but they have epistemic access that brings out the possibility of their existence.

I share Boyd's theoretical approach, and my study of the terminology includes the borrowed and coined terms in scientific discourse. My theory derives from the negotiations with two particular aspects of his: On one hand, in regard to *catachresis*, although they have metaphorical characteristics, the resemblance between primary and secondary subjects—or tenor and vehicle for Richards, or thisness and thatness for Burke, or categories for Aristotle, or source and target domains for Lakoff—can hardly be pinpointed, hence he discusses catachresis in terms of epistemic access rather than resemblance. When Evelyn Fox Keller criticizes the gene as a metaphor, she considers the primary and secondary subjects are amalgamated. To me, of terms like the gene, one category is the projection of the other; thus, they seem amalgamated, and the resemblance becomes vague, or dynamic. On the other hand, Boyd notes that his theory does not address the role of metaphorical thinking in theory invention, which can be considered as what Black leaves out of his theorization: how metaphors create, other than presenting, resemblance. If taking the reference of these terms as the unknown, then the role of these terms play in theory invention is to project a known reference into unknown, such that the unknown is framed and thought as the projected reference belonging to an epistemic category, existing or not existing yet. Such projection is speculative, so it is a creation; yet the projected reference as is well-established knowledge, the projection thus is a process making epistemic assumption according to epistemic access.

As a class of metaphor not intensively explored yet, I name these terms quasi-metaphor, whose special function is to project the characteristics of a known reference within an established epistemic category as a virtual reference in a known or unknown epistemic category.

Theoretically, quasi-metaphor is a combination of Aristotelian and Lakovian theories. The Aristotelian inspires the conception that metaphors are based on categories of the world, and by crossing categories, metaphors not only create new knowledge but also become effective means engaging with the world. The Lakovian theory provides the idea that a metaphor can serve as a cognitive projector to explore and understand the unknown according to the known. In Theodore Brown's *Making Truth*, he demonstrates many models and illustrations in the textbooks or publications of chemistry are indeed Lakovian metaphors whose source domains dwell in the layman's world. My ambition in quasi-metaphor theory is to go back to the time when the terms are conceived without knowing for sure if they exist, before they are written in textbooks or publications, when they are not full-blown metaphors by any definition, including the Aristotelian and the Lakovian, because the other category--or target domain, or vehicle, or secondary subject—is a virtual projection to be discovered or proved yet. Hence by "quasi," I mean this class of metaphors has a virtual part as the projection of a known reference. Functionally, quasi-metaphor is one step further from Boyd's theory. Quasi-metaphors play an important role in both theory invention and theory change. By projecting known reference, quasi-metaphor is a powerful tool to invent the resemblance between the known and unknown; or in other words, conceptualize the unknown through the known reference, such that it can guide the research to understand the unknown. The open-endedness or the dynamics of resemblance allows the conceptualization to be revised at different stage of research, until the epistemic assumption of the quasi-metaphor is proved or rejected. When proved, it is transferred to epistemic access as a part of knowledge. The growth of knowledge, therefore, roots in quasi-metaphor, in its conceptual power to frame and guide research on the unknown and its open-endedness that make continual revisions possible.

Scientific development roots in quasi-metaphors. Each quasi-metaphor is a thread in the history of science. Some run through the history, and some of them break somewhere. Chapter 3 is a case study of an abandoned quasi-metaphor, phlogiston. It was a coined word that had been referring to the material of combustibility: when something absorbed phlogiston, it obtained combustibility; as something was burning, it was releasing phlogiston. This overthrow of phlogiston is considered the major event of the Chemical Revolution, which happened simultaneously with the French Revolution. The leader of the Chemical Revolution, French chemist Antoine-Laurent Lavoisier (1743-1794) was convicted as a traitor and guillotined. The interweaving of a social and a scientific revolution, Lavoisier's tragedy, and the fact that one of the key elements of the Chemical Revolution, the discovery of oxygen, was not made by Lavoisier but Joseph Priestley (1733-1804), an English chemist, all make this episode one of the most intensively discussed cases in the history of science. For such an episode, the historiographical (or how to approach it) becomes more important than the historical. In particular, the Chemical Revolution to Thomas Kuhn is the paradigm of paradigm shift, one of the major cases from which he draws the structure of scientific revolutions. Hence, to study the history of science through a new concept like quasi-metaphor, this case can serve as a test to observe what the history is like, and more importantly, what difference it makes to study the history of science through this angle. Accordingly, my study starts from the historical, that is, to narrate the history of phlogiston as quasi-metaphor. Then, I examine what difference such an approach brings forth. Specifically, with respect to the abandonment of phlogiston, what explanation it can provide.

In the history of phlogiston as a quasi-metaphor, the most distinguished perception is that the evolution of the concept is the evolution of its epistemic assumption, which designates two levels of conceptualization: First, it was assumed that combustibility is a material; or, phlogiston is employed to frame the unknown cause of combustibility as a material. Second, this material should belong to certain epistemic category. What phlogiston referred to was rather stable compared with to what epistemic category it should belong. Especially, from German metallurgist Johann Becher (1635-1682) to his pupil Georg Ernest Stahl (1660-1734), the conceptualization of combustibility as a material did not change. For Becher, his *Terra pinguis* as the material of combustibility was one of the elements, and for Stahl, when he coined the name *phlogiston* to replace *Terra pinguis*, the change he made is that phlogiston as the material of combustibility linked the three kingdoms of nature—the air, plants and animals.

With respect to what ultimately shakes the phlogiston theory, most scholars agree on the discovery of weight-gain phenomenon--metals gained weight after burning. To Kuhn, it is a crisis for phlogiston theory. Quasi-metaphorical analysis comes to the same conclusion, although in this perspective, the weight-gain problem directly contradicts the epistemic assumption, because if combustion is a process of releasing phlogiston, a material kind, it is impossible for metals to gain weight.

The most complicated and arguable part regarding the episode, however, is why the name *oxygen* Lavoisier gave to the species of gas, first discovered and named as *dephlogisticated air* by Priestley, could be accepted, as well as what was revolutionary in the Chemical Revolution. Some scholars argue that the central feature of the revolution is the overthrow of phlogiston theory and meanwhile the establishment of the role of oxygen in combustion. The rebuttal from other scholars is that Lavoisier’s theory of combustion has serious flaws, too. Some even argue that the overthrow of phlogiston is not necessary at all, considering that oxygen was discovered under the guidance of phlogiston theory, and Priestley kept working on revising the theory.

There are also scholars, including Kuhn, who take the view that the Chemical Revolution is a competition between two rival research programs, one represented by Priestley and the other, Lavoisier. From this perspective, it is easy to explain why, until his death, Priestley never accepted Lavoisier's theory. It is hard to explain, on the other hand, why the overwhelming majority took Lavoisier's over Priestley's. One explanation is that Lavoisier ran a successful campaign to promote his program, but the problem is that Lavoisier's theory of combustion was not totally accepted. Caloric to Lavoisier was one of the most important concepts regarding combustion, but it was abandoned just as phlogiston.

From the quasi-metaphorical perspective, the problem of phlogiston is its epistemic assumption that combustibility is a material. When the weight-gain phenomenon was discovered, it should have been abandoned. Nevertheless, in the long unsuccessful process of searching for phlogiston, some scientists revise the conception, and a plausible one is to use the term to refer to the principle of combustion, and if so, weight-gain is not even a problem. Hence, the reason why phlogiston does not survive lies beyond itself. When Lavoisier replaced *dephlogisticated air* with *oxygen*, on the surface, he only used a different technical term referring to the same species of gas discovered by Priestley. Yet to him, oxygen was one of the elements, a specific and special epistemic position in another fundamental quasi-metaphor of his, (modern) chemistry. Lavoisier projected the methodology of experimental physics into his chemistry. In other words, he vigorously promoted the methods that worked well in experimental physics in chemistry. Lavoisier and his colleagues innovated instruments and established the nomenclature system, such that the experimental system and its language became precise, accurate, as well as productive. In doing so, he brought chemistry into science, and his program produced scientists, rather than scientists choosing his program, as Kuhn argues. The

Chemical Revolution, hence, is the establishment of modern chemistry as a scientific discipline, which swept away the chances for phlogiston to be revised and survive. Lavoisier, the leader of the revolution is also the father of modern chemistry.

Similar to Chapter Three, Chapter Four is a case study in the history of science—the gene as a quasi-metaphor. At present, no other scientific concept can match the celebrity status of the gene for inspiring scientific passion and public imagination. It carries the hope to cure currently incurable diseases like cancer and Alzheimer's; it makes some thus far unreachable goals seem reachable, even immortality; it hands us a scapegoat for our shortcomings as well as failures, meanwhile promising us a chance to escape the inevitable by creating designer babies. No wonder the gene captures the attention of scientists, philosophers, historians, rhetoricians, politicians, as well as business men. One of the books in the Greenwood Guide to Great Ideas in Science, Ted Everson's *The Gene,* depicts it as a historical lineage determined by a broad array of scientific, technological, and social events that span centuries.

In Hans Jorge Rheinberger's *An Epistemology of The Concrete: Twentieth-Century History of Life*, on the other hand, the gene and alike are considered to be epistemological objects, as the objects of inquiry constituted by material entities and process, including physical structures, chemical reactions, and biological functions. The most important characteristic of epistemological objects are vagueness. The quasi-metaphorical approach has the same departure point as Rheinberger's, although the vagueness in my study is the meaning of the gene as a term rather than an object of inquiry. Like Rheinberger, I also examine structures, reactions, and functions; unlike Rheinberger, however, I emphasize that it is quasi-metaphors as the stable signifiers that maintain coherence among the structures, reactions, and functions; meanwhile, it is the open-endedness of quasi-metaphor that make it possible to modify and revise the structures, reactions, and functions.

In the history of science, the gene and phlogiston barely have anything in common, except for sharing one problem--vague meaning. The gene thus should also be as doomed, but it remains active and productive. Hence in Chapter Four, the history of the gene as a quasi-metaphor first is to pursue what is vague in the gene term, then to explain how the vagueness allows the gene to be redefined and developed, while maintaining the coherence of research. The discovery of the double helix, as the last piece of evidence to prove the epistemic assumption of the gene, not only separates the gene from phlogiston, but also separates the quasi-metaphor gene from the scientific concept gene. Accordingly, Chapter Four is divided into Part I and Part II by this decisive historical moment, the confirmation of the assumption. Part I starts with a review of historical and philosophical treatments of the gene by other scholars, and Part II ends with negotiations between the quasi-metaphorical and other approaches. Part I focuses the history of the gene quasi-metaphor, or, the establishment of the material foundation of heredity through the confluence of developments in different disciplines; and Part II traces the history of quasi-metaphors derived from the gene; or, the scientific advances in various disciplines arising from the gene concept.

In Part I, the history of the quasi-metaphor gene is the process in which the epistemic assumption of the gene as a material reality is transferred into epistemic access. Generally, there are three periods: the proposal to frame the yet unknown hereditary unit as the gene; the research on chromosome revealing on which the gene locate; and the identification of the chemical nature of the gene. The quasi-metaphorical character of the gene allows the process to be dynamic. On one hand, the epistemic assumption provides guidance to scientific research; on the other hand, the results from scientific search keep modifying and specifying the assumption. It also allows the process to be a striking convergence of a few disciplines. In other words, at different stages of research, the projections of knowledge into the specifically unknown aspects about the gene are different, depending on what is known and what is unknown at that particular moment. The search for the gene thus appears as if it is handed from one discipline to another, and by participating the search of the gene, disciplines develop and enrich their own territories. It is a remarkable pattern of knowledge production, for the gene is the stable reference the knowledge of which keeps being revised, as well as accumulating; the gene is also the coherence that sustains the research over a half century and across different disciplines.

Specifically, the thinking and investigation of heredity ran through history. Systematic research on heredity began with taxonomy—the categorization of organisms. In late eighteenth century, nominalism and essentialism were both rather mature. In the development of essentialism, the theories on the evolution of species by French naturalist Jean Baptiste Lamarck (1744-1829) and English naturalist Charles Darwin (1809-1882) suggested the existence of hereditary material. Darwin, Dutch botanist Hugo de Vries (1848-1935), and Austrian scientist Gregor Johann Mendel (1822-1884) offered their hypotheses of hereditary unit as *gemmule*, *pangene*, and *factor* respectively. Furthermore, Mendel also proposed a workable experimental system to investigate heredity, as well as Mendelian laws of heredity.

Compared with others, Danish scientist Wilhelm Johannsen (1857-1927)'s proposal of hereditary unit as the gene is extremely vague. Unlike others, the gene does not attach to any hypothesis. It comes only with an epistemic assumption that the gene as a material unit of heredity exists. Such proposal is so vague that the gene is considered undefined at that point. Other than the vagueness, two important aspects in Johannsen's proposal are the epistemic category to which the gene belongs--the hereditary system that includes phenotype, genotype, and gene, and the methodological commitment to Mendelism. The vagueness makes it possible for the gene to be revised and developed; the system of phenotype, genotype, and the gene endows the gene pragmatic applicability to other disciplines; and Mendelian methodology provides a workable experimental system, not only for the gene but also for biological sciences.

The advances of microscopic and dyeing techniques in the second half of the nineteenth century contributed to the discovery and research on chromosome. American geneticist Thomas Hunt Morgan (1866-1945)'s research on the chromosomes of fruit fly concludes that the gene should locate on chromosome. In the later nineteenth century, the research of biochemistry brought out the components of deoxyribonucleic acid (DNA) were adenine base (A), guanine base (G), thymine base (T) and cytosine base (C). The development of microbiology made significant contributions in the twentieth century: at the beginning of the century, the discovery of virus, the simplest form of life; and in the 1940s, one-gene-one enzyme hypothesis and DNA as the hereditary material. In 1950s, based on American biochemist Erwin Chargaff's (1905--2002) discovery that the ration of A equaled to T, and G, to C in the cell and British biophysicist Rosalind Elsie Franklin's (1920-1958) X-ray crystallographic data, American zoologist James Watson and English biophysicist Francis Harry Compton Crick (1916-2004) suggested that the structure of the DNA as a double helix.

Watson and Crick's discovery of DNA structure is the decisive event that determines the chemical nature of hereditary material. It confirms the epistemic assumption of the quasi-metaphor gene, and the gene thus is turned into a scientific concept. Accordingly, research on heredity has moved to molecular level since then. In Part II, the staggering increase of knowledge in the post double helix era is achieved through the research on, not a single line or a single discipline, but dispersive quasi-metaphors, each of which follows the mode of the quasi-metaphor gene, drawing on advances from multiple disciplines to prove, or eventually disprove in some cases, their epistemic assumptions.

In contrast to the amazing convergence of developments from various disciplines to substantiate the material reality of the gene in the previous part, this part traces a rather striking divergence of advances departing from the knowledge that the gene is DNA, which also is the point of departure for molecular biology, as the research on DNA as genetic material and other chemicals related to DNA in *vivo* or in *intro*. The history of the gene concept between the discovery of the double helix and the completion of the Human Genome Project is often narrated through the history of molecular biology, such as Michel Morange's *A History of Molecular Biology* and Rheinberger's *A Short History of Molecular Biology*. In this part, I attempt an alternative approach--to recount this history through crucial quasi-metaphors, such as gene action, transcription, translation, information, genetic code, and genome. In doing so, those diverse understandings and also confusion regarding the gene concept are explained by the interactions of the conceptions of the unknown, the research methodology, and the advances of biotechnology.

After the discovery of the double helix, *gene action* was the quasi-metaphor responsible for the shift of research from the gene-centered to the gene-action-centered. By Crick's proposal, *gene action* was to frame the unknown functions of DNA in protein synthesis, which he theorized as the central dogma. Another two important quasi-metaphors were yielded in order to specify *gene action*, *information* and *(genetic) code*. The informer indicated what is transferred in the processes of DNA to RNA and to protein. The latter conceptualized the processes as cryptologic, which led to not only the discovery of the codons but also another two fundamental quasi-metaphors, transcription and translation framing the yet unknown processes of DNA to RNA and RNA to Protein, respectively. Ironically, the research guided by these quasi-metaphors proved that the central dogma is neither central nor dogmatic, and furthermore, the gene cannot act; rather, its activation and activities are regulated by other genes and chemicals. The gene had been studied by its function instead of action.

At present, *genome* is the most active and productive quasi-metaphor in life science by its epistemic assumption that the genomic sequence contains all of what is needed to guide the development of a functional being. One of the conditions that made *Genome* the current central quasi-metaphor is the rapid rise of genetic technology, that is, the research on what can be done on the gene. Genetic technology used to be merely a tool to study DNA and related molecules, but it has become an independent field scientifically and economically along with the advances in the research on what the gene can do, or gene function. The completion of the Human Genome Project is a landmark the establishment of genetic technology, the end of genomic sequencing phase and the beginning of genomic annotating phase.

The post double helix research, especially genomic research, reveals the widely diverse functions of the gene, which can hardly be united in a single definition. The complexity regarding the gene in this period hence is that of gene function. As to the gene, it a common sense now that the gene is DNA.

Chapter Five moves to discuss the historiographical implication of quasi-metaphor. Instead of studying quasi-metaphors in the history of science, this chapter focuses on an important historiographical notion, *scientific revolution*. Neither clear nor precise, Scientific revolution is similar to other humanistic notions, such as love, power, and reality. Studying scientific revolution as a quasi-metaphor allows to trace the changes of the conceptualization historically and to articulate the epistemic assumption and construction of conception in a particular episode.

There are three issues regarding the scientific revolution as a historiographical approach: First, do scientific revolutions exist? Some historians insist that the history of science consists by series of scientific revolution, whereas some considers that there is no such thing as scientific revolution. Second, what type of episodes can be qualified as scientific revolutions? In relation to phlogiston, the Chemical Revolution serves one of the exemplary cases of scientific revolution; in relation to the gene, some scholars take the discovery of the double helix and the completion of the Human Genome Project as scientific revolutions for their significance, but others do not, for both lack the controversy or breakdown of the typical ones. Third, if there are scientific revolutions, how should they be narrated? One approach is to treat a scientific revolution as one of series revolution in science. In so doing, scientific revolution stands as a generic term referring to a mode of scientific advance formed by a series of leaps instead of gradual additions. The other approach is to perceive the scientific revolution as a social revolution achieved by the establishment of scientific entity. In this case, The Scientific Revolution particularly points to one dramatic upheaval in science, including the first decades of the seventeenth century, but no consensus regarding when it began and ended.

American philosopher Edwin Burtt (1892 - 1989) first proposes the Scientific Revolution as a historiographical notion against the positivist perception of French theoretical physicist Pierre Duhem (1861–1916) that modern science was a medieval product. The epistemic assumption of Burtt's Scientific Revolution is that modern science is an intellectual upheaval radically different from medieval thought. To him, the work of Kepler, Galileo, Descartes, Boyle, and Newton constituted mainly the revolution. French philosopher Alexandre Koyré (1892 - 1964) pushed further to claimed that the Scientific Revolution is an intellectual mutation and a notable discontinuity achieved by Galileo and Descartes. Herbert Butterfield turned the epistemic assumption against whiggish historiography in which the history of science is perceived and presented as the ratification, if not the glorification, of the present.

The ways in which Burtt, Koyré and Butterfield construct the entity of the Scientific Revolution, however, were exactly positive and whiggish. Burtt drew a straight line to connect Kepler, Galileo, Descartes, Boyle, and Newton, for their research represented revolutionary intelligence by his standards; as to Koyré, Galileo and Descartes. Butterfield even claimed that the Chemical Revolution was the postponed Scientific Revolution—could any history get more whiggish than scheduling for an episode to happen? Hence, the Scientific Revolution is an inherently paradoxical notion: the revolutionary entity that is supposed to demonstrate the history of science is not positivist and whiggish has to be constructed in positivist and whiggish ways. This paradox produces two different narratives of the Scientific Revolution. One intends to emphasize the Scientific Revolution as the major rupture or discontinuity in western history, such as Butterfield's; the other narrative, that of the Scientific Revolution, as a part of the history of science, consists of a series of radical changes, such as Kuhn's.

The root of the paradox lies in the insistence on the discontinuity of science. By quasi-metaphorical analysis, continuity and discontinuity are not two opposite notions with respect to the history of science; rather, they are perceptions depending on what, where and when. Although some quasi-metaphors are abandoned, like phlogiston; some do run through the history, like time, element, and the gene. Moreover, some fundamental quasi-metaphors, such as physics whose methodology was the projection of mathematics; chemistry, experimental physics; and molecular biology, chemistry, they can be perceived as either upheavals or continuous development of methodologies in different fields. It seems that the boundaries of disciplines reinforced by modern institution dictates the former perception and weakens the latter. Most importantly, the significant establishments regarding the gene are excluded from scientific revolutions, thus the knowledge production patterns, much more complicated than Kuhn's, are also ignored.

To define scientific revolution as the transformation of epistemic assumption to epistemic access or the rejection of an epistemic assumption can not only resolve the paradox but also take the discovery of the double helix or completion of human genome into account. In doing so, both continuity and discontinuity are equally important to the history of science.

As the last chapter of my dissertation, Chapter Six discusses the implication of quasi-metaphor for the philosophy of natural kind language. Natural kind terms are used by philosophers to denote, or purport to denote, physical, chemical, and biological kinds of the world, which is the opposite of metaphorical kind. To me, technical kind (terms) is more appropriate than natural kind (terms) to refer to the intelligibility of the world in current scientific discourse. This chapter directly addresses what metaphors have to do with scientific discourse and particularly take scientists' view into account.

What is precision regarding natural kind language? This chapter starts with the introduction of a research case to illustrate what precision means to working scientists. In a process to identifying a piece of DNA sequence as a gene, precision means that the nomenclature system can represent as well as differentiate chemical structures. Moreover, experimental narrative can function as an instructor, and precision thus has a role in codifying technique. The nomenclature system realizes Saul Kripke's Simple Real Essence theory, the simplest version of a causal theory of realism, in which the reference of natural kind terms appeals to internal constitutions alone. On the other hand, experimental protocols and manuals illustrate Hilary

Putnam's theory, in which natural kind of terms are defined by the ostensive character of their reference.

Vagueness arises in scientific language with the metaphor-like terms that do not and cannot have fixed definitions. In Chapter One and two, it is elaborated that the metaphors in scientific discourse are not metaphors by any established metaphor theory. On one hand, they do possess metaphorical characteristics, such as vague meanings and referring to one thing in terms of another; on the other hand, they lack an established category by Aristotelian theory, the vehicle by Richards' theory, subsidiary subject by Black's theory, and target domain by Lakovian theory. Scholars like Keller and Boyd also notice this issue, although Keller only comments that tenor and vehicle meld into a single form in the terms like the gene, whereas Body calls them catachresis or theory-constitutive metaphorical expressions in this "Metaphor and Theory Change." I name them quasi-metaphor because the virtual category in them as well as the extraordinary metaphorical role they play.

From the perspective of the natural kind language, the vagueness of quasi-metaphor is the vagueness of its reference. Four approaches toward the vagueness—referential indeterminacy, reference potential, nondefinitional reference fixing, and reference as the unknown in quasi-metaphors—are discussed. Hartry Field's reference indeterminacy suggests that some concepts such as mass do not have fact of matter to what they denote if they are singular terms or to what their extension is if they are general terms. Phillip Kitcher's reference potential is the set of events that a speaker is disposed to admit as initiating events for tokens of that term. Discoveries and acceptance of new hypotheses can increase the reference potential by introducing new causal interaction or new descriptions that the reference of tokens can be fixed. Boyd's theory of theory-constitutive metaphorical expressions departs at the tricky paradox of scientific research in which scientists have to name something that they want to research before they understand that something. The terms to him are thus either nondefinitional or nonreferential. Quasi-metaphor theory shares the same departure with Boyd's theory. Nevertheless, a quasi-metaphor is conceived by framing the unknown through what is known, hence a quasi-metaphor is proposed with epistemic assumption. Quasi-metaphors are neither nondefinitional nor nonreferential. So, the projected known existence and its epistemic category are crucial, because they suggest the tentative or preliminary indication of both essence and properties.

A quasi-metaphor is the tool that makes it possible for scientists to interact with the world. By projecting the known into the unknown, a quasi-metaphor is the first step from the known to the unknown. When its epistemic assumption is confirmed, it becomes a scientific concept hence a part of knowledge; when rejected, it articulates the limitation of the projection and differentiates the unknown from the known. Scientific research, therefore, is a dynamic process mediated by language: the precision of scientific language plays a role as a technique in research, while the vagueness of quasi-metaphor allows the productivity and eventually builds our world view, as limited as it can be and as complex as it can be.

Unlike the general approaches of semiotics or language theory, in which theory guides the analysis or case studies, the approach of my dissertation is to establish a metaphor theory in regard to innovative concepts, then apply the theory to cases in the history of science, and finally come to the more philosophical question of the grounds of scientific thought and practice. In so doing, the importance of language, which has not attracted much attention in the studies of history and philosophy of science, moves into the foreground.

CHAPTER ONE

More than Words:
Metaphor Theories

Metaphors are not mere words. (Lakoff 208)

Metaphors are one of the most fascinating and inspiring linguistic phenomena. They are so much more than words that they are treated independently from words, although they are, first of all, words. What makes them un-wordy is not themselves: they are spelled and pronounced exactly the same as what they are supposed to be as mere words. Metaphors are not self-identifying; rather, their metaphorical identity is given away by those words that should not be yet still are grouped with them in semantic process. In other words, when those seemingly misplaced words survive being considered misused, they are treated as metaphors. Hence, "[a]mong the mysteries of human speech, metaphor has remained one of the most baffling" (Boyle 257). In Max Black's view, a relatively simple metaphor is "a sentence or another expression in which some words are used metaphorically while the remainder are used nonmetaphorically" (27). The key, however, is where to draw the line between the metaphorical and the nonmetaphorical. The definition of metaphor, or more precisely, the definition of metaphor in relation to nonmetaphor, becomes crucial: it can easily run into a logical trap in which a metaphor is a metaphor because it is not nonmetaphorical, and *vice versa*, if what exactly nonmetaphor is cannot be articulated. Actually, the nonmetaphorical as the literal--that is, "precise and unambiguous" (Ortony 1)--is where Andrew Ortony starts his *Metaphor, Language and Thought*, the introduction to the most influential anthology of contemporary metaphor studies. From a different perspective, George Dillon's semantic theory of metaphor suggests that reader's recognition of a metaphor is triggered not by the response to the question

"Does this passage contain a metaphor?" but by that to "Is this passage susceptible of metaphoric interpretation (i.e., by adjusting the sense of some word or words in it)?" (39), because, as Paul Ricoeur puts it, "[t]o affect just one word, the metaphor has to disturb a whole network by means of an aberrant attribution" (23). Thus, to recognize a metaphor is to single out a "deviant" (Ortony 4) word or words in an expression; or more precisely, to detect "dissonance" as one kind of the "nonliteral" caused by the "deviance" in an expression (Winner and Gardner 425). To me, though, *deviance* is a cover term indicating departures from the ordinary way of speaking, and its use implies no commitment to a specific linguistic theory.

In this chapter, I examine the theories of metaphor in Western tradition, from the proposal of the concept by Isocrates to George Lakoff. Although Plato never uses the term metaphor, his discussion about metaphorical terms suggests what the trouble may be regarding the interpretation of metaphor. Plato's student Aristotle establishes a theory of metaphor based on the resemblance and dissemblance between categories of the world. Even though his discussion of metaphor lies in the realm of poetics and rhetoric, Aristotle's conception of metaphor has the flavor of modern cognitive theory about metaphor. Roman Rhetorician Cicero and Quintilian develop Aristotelian theory further to stylistic study, meanwhile endowing metaphors with aesthetic values. The modern theories, generally acknowledged as starting from I. A. Richards' proposal of a set of useful terms as well as a theory of how they function in metaphor studies and extending to Max Black's interactive view, bring forth a dynamic view on resemblance and dissemblance. In this theoretical system, resemblance and dissemblance are interchangeable, depending on context and perspectives. As scholars point out, metaphors afford different ways of viewing the world, and something new is created when a metaphor is understood. Furthermore, the resemblance designated by a metaphor may vary in different stages of thinking, especially in a knowledge-seeking process like scientific research. Lakoff's theory, on the other hand, suggests a specific thinking process in which one (the target) domain is a projection of the other (the source) domain.

Plato and the Golden Metaphor

Aristotle first established a systematic approach for the study of metaphors, although, according to George Kennedy, it was Isocrates who first used the word metaphor (*metaphora*) in *Evagoras* 9 in the sense known today. *Metaphora* literally means "carrying something from one place to another, transference" (*On Rhetoric*, 199). Plato, Aristotle's teacher, does not use the term *metaphora*, but he makes ample use of metaphors, brief or extended, in his writing. Plato also delivers insightful analysis of a metaphor, "golden men." As the longest and most extended study of a metaphor, the interpretation and analysis of this metaphor illustrate the importance of categorization in studying metaphors, the impossibility to pinpoint the literal meaning of a metaphor, as well as the glory of a long-lived metaphor.

In the dialogue with Hermogenes regarding proper names of beings, Socrates quotes Hesiod's *Works and Days*:

> But now that fate has closed over this race
> They are holy demons upon the earth,
> Beneficent, averters of ills, guardians of mortal men.
> (Plato 92b)

He then comments on the way in which Hesiod "says a golden race was the first race of men to be born:"

> I suppose that he means by the golden men, not men literally made of gold, but good and noble; and I am convinced of this, because he further says that we are the iron race. (Plato 92b)

As vigorous and meticulous as Socrates can be, he works out a process in which an interpretive meaning of a metaphor is affirmed while the literal meaning is denied. “Golden men” actually has two literal meanings: men with golden (skin) color and men made of gold. The former is so absurd in the context that Socrates does not even take it into account, although morphologically[1], the former might be more accurate reading of the phrase. As far as Socrates is concerned, “golden” is the word out of place in the discussion of human races, because “men” and “golden” belong to two different categories of words--humans and metals, respectively. His “adjusting the sense of some word” is the rationale that “golden” should not be understood as its literal meaning, the material of which men are made, but the characters of men. To him, “golden” is a metaphorical expression of “good and noble.” As a philosopher who believes that rhetoric “requires a shrewd and bold spirit naturally clever at dealing with people” (*Gorgias* 30), he makes an intriguing move to prove his interpretation is the right meaning by using another metaphor of Hesiod’s, “we are the iron race.”

Depicted as “not handsome,” with “snub nose and protruding eyes” (*Theaetetus* 143e), Socrates and some his contemporaries believed that they were a much degraded generation, compared with the first generation “golden race.” H. C. Baldry suggests that the “connexion with gold were not the terms traditionally or normally employed in antiquity, at any rate before the Roman Empire, … but were first introduced by Hesiod” (83). His research concludes that “[i]t was Roman writers who made the transition from a golden race to a golden age, and from them the concept was handed into more modern literature” (92), in which golden age is to “describe an outstanding period of history or literature” (84). In *Works and Days* Hesiod depicts the first generation as such: “Golden was the race of speech-endowed human beings which the

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[1] In the original text, golden is χρυσούν, and gold, χρυσού (Plato 54a).

immortals, who have their mansions on Olympus, made first of all," and then "a second race, much worse, of silver." The third one "of bronze … out of ash trees" is "terrible." The fourth one is "more just and superior," as "the godly race of man heroes" who "dwell with a spirit free of care on the Islands of the Blessed beside deep-eddying Ocean" (97-101). Hesiod does not assign the fourth a correspondent metal. Regarding his generation, he laments that:

> If only then I did not have to live among the fifth men,
> but could have either died first or been born afterwards!
> For now the race is indeed one of iron.
> And they will not cease from toil and distress by day,
> nor from being worn out by suffering at night.
> (103)

Hesiod's reflection on his people in relation to their predecessors is represented by the well ranked and valued system of metals. Except for the fourth one, the hierarchy of races coincides with that of metals in the Greek society then[2]: gold is the most expensive, beautiful, pure, and "associated with the gods" (Baldry 91); next, silver and bronze. The least is iron, usually used for "tools, implements and weapons" (Richardson 570). Gold stands at the top of the list because, according to Sherwood Taylor's survey of Greek alchemical writings, the preparation of gold-like substance, as the main object of practical alchemy, "was far more difficult than for silver." The ancient Greek recipes tell that, "[f]or a metal to pass as gold [,] it had to withstand the fairly reliable tests then available" (127). Accordingly, Hesiod designates gold as the representation of the first human generation appeared on the Earth. Both gold and the first generation are on the top of their kinds; or, both have the highest value in each category. While the properties of gold are well defined as a metal, what precisely are the characters of men

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[2] According to Michael Vickers' research, the price ratio of silver to gold in the fifth and fourth centuries B.C. is between 10:1 and 14:1 (613); and Harry Richardson's, "[u]ntil about 330 B.C. prices for iron ore appear to have fluctuated between $1.25 and $1.50 (silver) per hundred pounds. Then a boom, due partly to the speculative opportunities created by the wars of Alexander, quadrupled the price" (570).

corresponding to those of gold? Socrates obviously is not sure of Hermogenes' grasp on this metaphor, so he explicitly imposes his interpretation on "golden" as "good and noble." He must have sensed that the literal paraphrase of "golden" as "good and noble" is problematic, because strictly speaking, "good and noble" are not more specific or less vague than "golden." He then tries to affirm his interpretation by another metaphor of Hesiod, "we are the iron race." Yet the latter also cannot establish what in "our" characters are corresponding to those of iron, let alone help the former. "[W]e are the iron race" can prove, however, that the hierarchy in the category of metals can be used effectively to think and talk about human races. Jean-Pierre Vernant summarizes Hesiod's myth as such:

> [T]he sequence of the races made up a complete cycle of decline. Starting with the age of gold, when youth, justice, mutual friendship, and happiness reign, all in their pure state, we end with an age that is its opposite in every respect: it is entirely given over to old age, injustice, quarrelsomeness, and unhappiness. …[T]he myth must originally have been composed of four races of metal whose value followed a line of progressive decline. (60-61)

On the other hand, Ian Morris remarks that, from Plato down to Vernant, "Hesiod's myth of the Ages of Man (*Works and Days* 109-201) has been interpreted as a complex structural homology associating different groups in the worlds of men, spirits and gods with gold, silver, bronze and iron in a rank sequence," which "is certainly not accepted by all ancient historians." For instance, Raymond Firth argues that:

> It is as if, allowing for the obvious differences, in our society gold, silver and copper were used as media of exchange in three series of transactions but there was no accurate means of rendering them in terms of each other. (341)

In other words, Firth is not convinced that the hierarchy from gold to iron exists in ancient Greek, suggested by the "spheres of exchange in a gift economy" (Morris 9). In consequence, the reading of Hesiod should not necessarily conclude that one race is inferior to the other/another[3], as Baldry posits:

> Looking back from his [Hesiod's] own day he saw the prevailing picture of man's past as consisting of three phases-the present period of the use of iron, the period of heroes, and the period of the use of bronze. Following the train of thought suggested by iron and bronze, and seeking a metal which could give its name to the happy time when men lived like gods, he chose gold, the metal associated with the gods. (91)

Nevertheless, the metaphor in this passage, "the train of thought," saves a tedious process of explaining why the age "when men lived like gods" is "happy time," and more importantly, why gold is associated with gods, which is exactly what Hesiod's myth represents. These two arguments regarding whether there was a well-ordered rank of metals at Hesiod's time do not necessarily contradict each other: the hierarchies in the categories of races and metals may not be well established before Hesiod's time. His magnificent myth represents not only the inter-relationship--that is, the resemblance-- between the two categories but also the intra-relationship--that is, the deliberately differentiated hierarchy--in each category. Or more precisely, his work establishes the ranks of each category as well as correspondence, or a mapping, between the two categories, and they eventually become common sense. Furthermore, the way in which he thinks of races in relation to metals is even extended to ages, such that "golden age" becomes a long lived metaphor. From this perspective, his "golden race" is not merely an uncanny representation. It indeed is an intelligent and insightful way of thinking, which provides him, the Greeks, the Romans, and us, an extraordinary approach of self-reflection.

"Golden race" is a golden example. From Plato to the present, the dialogues on the interpretation of this metaphor raise four major issues with regard to the studies of metaphors:

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[3] Vernant's reading of *Works and Days* suggests that relationship of the four metals is a hierarchy in two pairs: gold to silver, bronze to iron. Thus the relationship of silver and bronze is extremely crucial to the interpretation of the work (67-73).

First, how deviant a word or words should and could be in an expression to cause dissonance that signifies a metaphor?

Second, how similar the two categories, as well as how stable the similarities between two categories, should be in order to produce a metaphor?

Third, does/can every metaphor have a literal paraphrase? Or do metaphors create meanings?

Fourth and the major issue of this study, are metaphors just apt expressions of thoughts, or are they also essential tools in thinking processes?

Basically, the first and second are about mappings established by metaphors, the third is about semantics of metaphors, and the fourth, cognition.

Aristotelian Metaphors and Categories

In his *Poetics*, Aristotle defines metaphors in the section on diction within the classification of nouns: "metaphor is the application of an alien name[4] by transference either from genus to species, or from species to genus, or from species to species, or by analogy, that is, proportion" (1457b). Such a definition clarifies that categorization-shifting is the root of metaphorical deviance. The outline of the Aristotelian categorical system as an ontology of substances looks like this (following Paul Studtmann):

- Substance:
  - Immobile Substances — Unmoved Mover(s)
  - Mobile Substances — Body
    - Eternal Mobile Substances — Heavens

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[4] In Stephen Halliwell's translation, the word "name" is "word" (105).

- Destructible Mobile Substances — Sublunary bodies
  - Unensouled Destructible Mobile Substances — Elements
  - Ensouled Destructible Mobile Substances — Living things
    - Incapable of Perception — Plants
    - Capable of Perception — Animals
      - Irrational — Non-Human Animals
      - Rational — Humans

In Aristotle's methodology of categorization, "genera are co-ordinate and different, differentiae will differ in kind," such as the genera of "animal and knowledge" (*Categories*, 1b16). While the "secondary substances—those within which, being species, the primary or first are included, and those with which, being genera, the species themselves are contained," such as "a particular man we include in the species called 'man' and the species itself in its turn is included in the genus called 'animal'" (*Categories*, 2a 15-19).

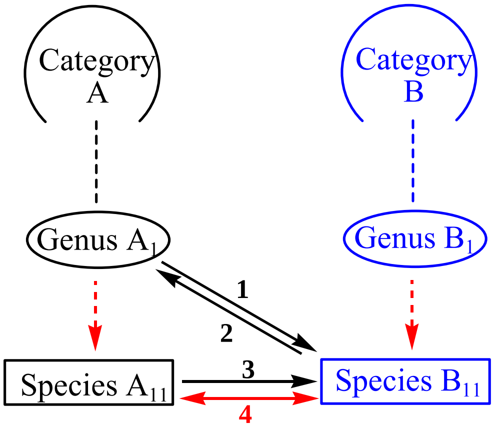

Figure 1-1: Aristotelian Metaphorical Transference

Aristotle's example from genus to species is "there lies my ship[5]," because lying at anchor is "a species of lying." Thus, the transference is from the general to the specific. (1 in the Figure 1-1)

The example from species to genus is "verily ten thousand noble deeds hath Odysseus wrought," because "ten thousand is a species of large number, and is here used for a large number generally." Here, the specific is used to imply the general. (2 in the Figure 1-1)

The examples from species to species are "with blade of bronze drew away the life and cleft the water with the vessel of unyielding bronze," in which *draw away* and *cleave* both are a species of "taking away." The transference is that one kind of specificity substitutes another. (3 in the Figure 1-1)

Metaphors as analogy or proportion are more complicated. Aristotle explains that such kind is "when the second term is to the first as the fourth to the third. We may then use the fourth for the second, or the second for the fourth. Sometimes too we qualify the metaphor by adding the term to which the proper word is relative." (4 in the Figure 1-1) One of the examples is: "As old age is to life, so is evening to day. Evening may therefore be called 'the old age of the day,' and old age, 'the evening of life,' or, in the phrase of Empedocles, 'life's setting sun.'" In such a metaphor, the relations within a genus and its species, or within one category are transferred to another genus and its species, or another category. In *On Rhetoric*, Aristotle points out that "[o]f the four kinds of metaphor, those by analogy are most admired" (1411a). Richard Janko also believes that "it corresponds to 'metaphor' in our sense, and depends on discerning likeness

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[5] In Ricoeur's summary Aristotle's metaphor theory, "[t]he first characteristic is that *metaphor is something that happens to the noun*" (17, emphasis his). This example is not so. See footnote 4 on page 31 about the translation discrepancy. Moreover, Halliwell's translation of this example is: "'my ship stands here': mooring is a kind of standing" (105). Heath's: "'Here stands my ship'; lying at anchor is one kind of standing" (34). Aristotle is discussing genus and species of a verb that describes making objects stationary.

between pairs of things" (129). What Aristotle establishes at this point is exactly what Plato says about "golden men," although instead of seeking literal meaning of the metaphors, Aristotle turns to discuss the effects of metaphors: one is to bring "an unfamiliar quality" or "strangeness" into subjects that "escapes notice;" another is to bring the subjects "before-the-eyes," namely, to "signify things engaged in activity" (1405a; 1411b).

Aristotle also discusses the way in which a metaphor is conceived. To him, making metaphorical expressions is a unique and innate talent that "cannot be imparted by another," and "it is the mark of genius, for make good metaphors implies an eye for resemblance" (*Poetics*, 1459 a5). He repeats this point again in *On Rhetoric*, "metaphors should be transferred from things that are related but not obviously so, as in philosophy, too, it is characteristic of a well-directed mind to observe the likeness even in things very different" (1412a).

As Figure 1-1 visualizes, the most striking aspect of Aristotle's theory is that a metaphor establishes a mapping between two unrelated categories. Max Black comments that "every metaphor may be said to mediate an analogy or structural correspondence," which is "the correct insight behind the classical comparison view of metaphor as elliptical or truncated simile" (30). The mapping is not simply one-to-one relationships—that is, species to species or/and genus to genus relation; rather, the metaphor crosses the boundaries of categories, and the hierarchies of each category as well. The boundaries and hierarchies are set as the order of the world. Ricoeur claims that metaphor "'redescribes' reality," thus "the category-mistake is the de-constructive intermediary phase between description and redescription" (24). Every metaphor, in this sense, is resistance to the order, an insurrection. Ricoeur praises that "Max Black's integration of model and metaphor… allows us to exploit thoroughly this idea, which is completely opposed to any reduction of metaphor to a mere 'ornament'," but Aristotle's contribution should not be ignored, either. Ironically, modern metaphor theories like that of Ricoeur, which intend to promote metaphor's roles beyond the classics, often describe category transference of metaphors as "category-mistake," "categorical transgression," and speak of metaphors as "deviant" or "deviation." The negative connotation hints a confirmative attitude toward boundaries and hierarchies.

More importantly, the Aristotelian Mapping contains an intriguing twist: A word or words does not attract recuperation by metaphor unless it or they deviates from other words in an expression, whereas making a metaphor requires the perception of resemblance between different categories. As Ken Baake indicates, "[m]etaphors, …, assist in cognition by drawing attention to similarities between things or phenomena. But, in calling attention to similarities, metaphors implicitly also call attention to differences" (65). That is, to be transferable, a word or words must possess a certain resemblance with that which it/they is/are transferred to; to signify transference, a word or words must deviate from which it/they is/are transferred to. From an author-reader perspective, on one hand, the creator of a metaphor attends to the resemblance of two subjects belonging two different categories; on the other hand, the receiver of a metaphor recognizes the dissonant word or words in an expression. For instance, when Hesiod glorifies the "golden men," he perceives the resemblance as "good and noble" in gold and the first human race; when Isocrates reads "golden men," his discerns that it must not mean "men literally made of gold," therefor, "golden" is dissonant and may be metaphorical. "[M]etaphors involve similarities between dissimilar concepts," Verena Haser remarks, "[t]his *fundamental tension* has far-reaching implications for the nature of the features shared by tenor and vehicle" (19, emphasis mine). The tension is manifested by the establishment of the mapping between two categories. It is precisely what makes metaphors "the most baffling" thing in human speech, and

I consider it is the key issue to metaphor theories (so also Boyle 257).

Aristotle emphasizes that to make a metaphor is beyond linguistic skills, and he mystifies this talent as a kind of genius. Nevertheless, the questions are: if it takes a genius to perceive the unlikely likeness in order to make a good metaphor, then does it take another genius to understand the metaphor? What about the majority of readers as non-geniuses? To make a metaphor, as showed in Figure 1-1, is about crossing categories; or more precisely, to think subjects beyond their own categories. The categories of the World are knowledge acquired through education. In Aristotle's theorization, as Ricoeur recognizes, "[d]eviation appeared to belong to a purely lexical order, but now it is linked to a kind of deviance that threatens classification itself" (23). So, in order to make a metaphor, does one need a good sense of categorization, or more precisely, one's contemporary categorical system, and then break the well-established order of things in the thinking process? By "the mark of genius" as producing good metaphors, does Aristotle mean the rare and unique talent is that the creator has to think beyond what is taught; or, the creator even does not need to be taught about categories yet still produce good metaphors? No matter what the answers are, Aristotle's insights of metaphor making go beyond stylistic study, although he does place these discussions in the section on *Style*.

Metaphors and Roman Rhetoric

Roman rhetoric, overall, is a further development of Greek rhetoric. Cicero also talks about metaphors (*translatio* in Latin) within his discussion of the three styles. The first is the "restrained and plain" style. He comments that the "plainness of style seems easy to imitate at first thought, but when attempted, nothing is more difficult," because, "although it is not full-blooded, it should nevertheless have some of the sap of life, so that, though it lack great strength, it may still be, so to speak, in sound health" (*Orator*, 76). The second style is "fuller and more robust" than the first one. It has "a minimum of vigour, and a maximum of charm;… all the ornaments are appropriate to this type of oration, and it possesses charm to a high degree" (*Orator*, 91). The third style is "magnificent, opulent, stately and ornate." It is "the kind of eloquence which rushes along with the roar of a mighty stream, which all look up to and admire, and which they despair of attaining. This eloquence has power to sway men's minds and move them in every possible way" (*Orator*, 97). In the second style considered as "moderate and tempered," Cicero continues the property of transference by defining metaphors as "words transferred by resemblance from another thing in order to produce a pleasing effect, or because of lack of 'proper' words" (*Orator*, 92). Cicero adds a part to the definition of metaphors--a reason for using metaphor can be "lack of 'proper' words," which implies that metaphors do not always have literal paraphrases. In this sense, metaphors are not mere stylistic choices, or ornaments of language, but essential expressions. If so, those words at the positions lacking "proper" words should belong to the first style, for they function as plain words. The way in which Cicero categorizes metaphors in his styles hints that his attention for metaphors focuses on their "pleasing" effects, rather than their possible use as vehicles of new knowledge.

Furthermore, Cicero elucidates that, under Aristotle's category of metaphors, there are more nuanced categories or subcategories. He comments that the metaphor, "Dread Africa trembled with terrible tumult," is named "hypallage" by the rhetoricians, because words are exchanged, and "metonymy" by the grammarians because nouns are transferred. Aristotle also puts catachresis and allegory as metaphors. These categories and subcategories suggest that the nuances among linguistic uses or stylistic skills are much more elaborated, and the intra- and inter-relationships of these categories and subcategories have been established.

Quintilian takes metaphor as "the most beautiful of *tropes*," and says it is "so natural a turn of speech that it is often employed unconsciously or by uneducated persons." His definition of metaphors is similar to Cicero's: "A noun or a verb is transferred from the place to which it properly belongs to another where there is either no literal term or the transferred is better than the literal (*Institutio Oratoria*, VIII, v, 35-vi, 29, emphasis in the original).

Quintilian divides metaphors into four classes. The first is to substitute one living thing for another. Such as:

"The steersman then

With mighty effort wrenched his charger round."

Secondly, inanimate things may be substituted for inanimate, as in:

"And gave his fleet the rein,"

Or, inanimate may be substituted for animate, as in:

"Did the Argive bulwark fall by sword or fate?"

Or, animate for inanimate, as in:

"The shepherd sits unknowing on the height
Listening the roar from some far mountain brow."

He marks that the effects of extraordinary sublimity are produced when the theme is exalted by a bold and almost hazardous metaphor in which inanimate objects are given life and action, as in:

"Araxes' flood that scorns a bridge,"

Quintilian clarifies his method of classification as such:

> These four kinds of metaphor are further subdivided into a number of *species*, such as transference from rational beings to rational and from irrational to irrational and the reverse, in which the method is the same, and finally from the whole to its parts and from the parts to the whole. (*Institutio Oratoria*, VIII, v,

35-vi, 29, emphasis mine)

He provides more kinds of metaphors, as well as methods of how to create more according to Aristotelian categorical systems. He uses "species" to describe kinds of metaphors, which implies that metaphor becomes a formal category, a genus, designating a linguistic phenomenon and a stylistic skill.

What Quintilian regards as the "extraordinary sublimity" of the metaphorical effect coincides with Aristotelian aesthetic view of metaphor, "to observe the likeness even in things very different," although they state it from different angles. Aristotle comes from the perspective of resemblance (or the poet)--the most difficult to perceive the likeness, the most admired; whereas Quintilian, dissonant (or the reader)—the most unlike to be used, the most sublime. There is one more turn on the criticism of poetic metaphors between Aristotle and Quintilian. As a feature of poetry and oratory, Aristotle emphasizes the effectiveness; whereas Quintilian, sublimeness. To the former, a good metaphor should bring a subject "before-the-eyes;" and the latter, arouse noble feelings. Both, however, emphasize the affective rather than cognitive power of metaphors, and this emphasis has proven to be pervasive and very lasting.

As Cicero, Quintilian also has concerns about metaphors in relation to the "proper" words. He juxtaposes metaphors to proper words, and states that words "are 'proper' when they signify that which they were first designed to name; metaphorical, when they have one meaning by nature and another in the context" (*The Orator's Education*, 1.5 v. 71). He explains in *Institutio Oratoria* that, "for metaphor should always either occupy a place already vacant, or if it fills the room of something else, should be more impressive than that which it displaces" (VIII, v, 35-vi, 29). Both Cicero and Quintilian make it clear that some metaphors are used where there are no proper words to express the meanings. From Quintilian onwards, context becomes an important part of metaphor theory. Context is a convenient concept for the discussions on the dissonance and resemblance between categories, as well as the literal meaning of a word /words and its/their metaphorical interpretation.

At this point, basically, metaphor is established as a stable category of stylistic study. Isocrates first designates *metaphora* as a category to indicate a distinctive linguistic phenomenon, and as Kennedy points out, *metaphora* itself is a metaphor (59). The very trouble with *metaphora* as a metaphor is that, at that moment, there was not such a category. *Transference* as the literal meaning of metaphor belongs to a category of means that can "carry[ing] something from one place to another," but where is the other category for the mapping? Presumably, metaphor should be the one; however, it was not yet. *Metaphora* indeed could not establish Aristotelian Mapping. Aristotle drew the boundary for such a category by defining it, dividing it into sub-categories, and providing rich species to fill the category. Cicero and Quintilian refined the definition by adding, as well as elaborating, more categories, subcategories, and species into the category. In this historical process, when *metaphora* the term was invented, it was an empty category projecting the characters of a linguistic phenomenon of displacement in which the meaning of words were transferred. After the studies of Greek and Roman scholars, *metaphora* becomes a well-defined stylistic concept within their system of thought.

The contributions from these three great rhetoricians set up the "systematic and formal" (Osborn 123) theory of metaphor. Michael Osborn indicates that the emphasis on the linguistic aspect is because, historically, "metaphor was settled securely with the jurisdiction of Style, it remained there, perpetuated in a condition which spotlighted its linguistic attachments, and threw in shadow its basic mental components" (124). In defense of the classical treatment on metaphors, John Kirby argues that Aristotle “by no means restricted metaphor to poetic or extraordinary contexts,” as his response to one of the alleged distinctions between cognitive and classical treatments on metaphors--that the former “sets off a putative ‘figurative’ or ‘poetic’ language in contradistinction to everyday usage” (539). It is undeniable that classical studies on metaphors do offer more that linguistic insights, although these rhetoricians do tend to study metaphors within the realm of rhetoric and style. Kirby emphasizes Aristotle’s remark that “everyone converses using metaphors,” but it cannot change the fact that the dominant uses of metaphor in the classics are artistic. The “good eye,” lauded by Aristotle for unifying the most remote categories and praised Quintilian for achieving the “effects of extraordinary sublimity,” is the eye of an artist. As in all forms of arts, the first criterion is creativity or uniqueness, which is well put by Salvador Dalí, that “the first man to compare the cheeks of a young woman to a rose was obviously a poet; the first to repeat it was possibly an idiot.” One of ways that e. e. cummings metaphorizes rose is:

“the voice of your eyes is deeper than all roses[6].”

No one had ever written so before, and no one will (barring an episode of imitative idiocy). Similarly, William Butler Yeats is lauded for his “gift… for finding striking metaphors” (Parisi 9). This small bouquet will have to stand for the great body of literary uses of metaphor; the current project traces the uses of metaphor in the pursuit of knowledge.

Modern Metaphor Theories

Since the eighteenth century, neo-classical theorists have started to consider that metaphors involve the actions of thought. Osborn declares that “it was surely a dramatic

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[6] Strictly speaking, “roses” here is catachresis.

moment" (127) when Lord Kames wrote, "A metaphor is defined above all to be an act of the imagination, figuring one thing to be another" (572). Nevertheless, it is generally acknowledged that I. A. Richards' metaphor theory initiates a new era. He "not only proposed a set of useful terms for talking about metaphors (the 'topic' or 'tenor,' the 'vehicle,' and the 'ground'), he also proposed a theory about how they function" (Ortony 3). Richards starts his metaphor theory from his negotiation with classical aesthetics of metaphors. At the beginning of his discussion of metaphor in *The Philosophy of Rhetoric*, he attacks the three Aristotelian assumptions of metaphors:

The first classical assumption is that "'an eye for resemblances' is a gift that some men have but others have not." Instead, he assumes that all men are more or less alike with respect to perception, "though some may have better eyes than others, the differences between them are in degree only and may be remedied, certainly in some measure, as other differences are, by the right kinds of teaching and study."

The second classical assumption is that the gift "alone cannot be imparted to another." Richards argue that "[a]s individuals we gain our command of metaphor just as we learn whatever else makes us distinctively human. It is all imparted to us from others, with and through the language we learn, language which is utterly unable to aid us except through the command of metaphor which it gives" (89-90).

The third and "worst" classical assumption is that "metaphor is something special and exceptional in the use of language, a deviation from its normal mode of working." Richards proposes that metaphor is "the omnipresent principle of language" (92).

In so doing, Richards takes metaphors from the hands of Muse and passes to ordinary men, for whom metaphors are "constitutive form," instead of "a grace or ornament or added power of language" (90). He demystifies the process of making metaphors by taking the stand that everyone is capable of making metaphors. He also de-glorifies metaphors by considering metaphor as a normal use of language—although, ironically, most metaphors he analyzes are poetic metaphors. His theory radically shifts metaphor studies from the linguistic to the cognitive: "In the simplest formulation, when we use a metaphor we have two thoughts of different things active together and supported by a single word, or phrase, whose meaning is a resultant of their interaction" (93). He emphasizes that "*Thought* is metaphoric, and proceeds by comparison, and the metaphors of language derive therefrom" (94, emphasis in the original). In this perspective, metaphorical language is just the tip of an iceberg that indicates much more profound thinking processes. Yet, there were some considerations of cognitive activity with respect to making metaphors in the classics, one of which is actually attacked by Richards, that "'an eye for resemblances' is a gift that some men have but others have not." In his rebuttal, Richards takes "eye" as a literal word, because he says "we all live, and speak, only through our eye for resemblances. Without it we should perish early" (89). In the context of Aristotle, however, "eye" is a metaphor referring to the mental activity of extracting the likeness between two categories with genera and species. Aristotle states that "metaphors should be transferred from things that are related but not obviously so;" in other words, metaphors should reveal the likeness that usually cannot be "seen." Thus, a metaphor as "two thoughts of different things active together" is not really a radical shift, just as Osborn claims in his "The Evolution of the Theory of Metaphor in Rhetoric" that, "[f]rom the very beginning, Richards' charge that traditional rhetoric dealt with metaphor only as the shifting of words is not justified" (123).

Another radical shift regards the identity of those who have "two thoughts of different things active together." In classical treatment, the line between metaphor creation and comprehension, or the poets and readers, is clear. Aristotle elaborated both well; while Cicero and Quintilian discussed the latter more. In other words, regarding the tension, in which a metaphor must possess resemblance with and meanwhile also deviate from the rest of an expression, the classical treatment always deals with one relationship each time, either from resemblance and poets, or deviance and readers. Richards approaches the different categories as two different thoughts. When he talks about "when we use a metaphor we have two thoughts of different things active together," he does not articulate what he means by "use," nor does he specify who is "we." Using a metaphor is different from creating and comprehending a metaphor. By "use," if he means to repeat a metaphor on occasion, then his charge on the classical treatment of metaphors is misplaced. Meanwhile, the line between the creation and comprehension of a metaphor diminishes because "we" are users. In his words:

> Our world is a projected world, shot through with characters lent to it from our own life. "We receive but what we give." The processes of metaphor in language, the exchanges between the meanings of words which we study in explicit verbal metaphors, are super-imposed upon a perceived world which is itself a product of earlier or unwitting metaphor. (109)

Indicated in this passage, the fundamental turn from the classics is the relation between "we" and the world. From Aristotle to Quintilian, metaphors are an effective tool that "we" created and through which "we" can engage with the real World, so their discussion more or less has to do with the categorization; whereas to Richards, the world itself is a metaphor of our perception upon the World, thus metaphors are mainly about "us," "our" thoughts of different things, instead of "we" and the World. Accordingly, "we" are users rather than creators of metaphors. Richards proposes his tenor-vehicle model as such:

> At present we have only some clumsy descriptive phrases with which to separate the two ideas [them]. 'The original idea' and 'the borrowed one'; 'what is really being said or thought of' and 'what it is compared to'; 'the underlying idea' and

> ‘the imagined nature’; ‘the principal subject’ and ‘what it resembles’ or, still more confusing, simply ‘the meaning’ and ‘the metaphor’ or ‘the idea’ and ‘its image.’
>
> How confusing these must be is easily seen, and experience with the analysis of metaphors fully confirms the worst expectations. We need the word ‘metaphor’ for the whole double unit, and to use it sometimes for one of the two components in separation from the other is as injudicious as that other trick by which we use ‘the meaning’ here sometimes for the work that the whole double unit does and sometimes for the other component — the tenor, as I am calling it — the underlying idea or principal subject which the vehicle or figure means. (96-97)

He clarifies that the conceptualization of metaphor should handle both thoughts at once with the capacity of distinguishing the two thoughts. The dilemma of such an approach is represented by this passage in which Richards attempts to define tenor and vehicle. On one hand, how can the meaning of the vehicle be pinpointed without the meaning of the tenor being articulated? On the other hand, how can the meaning of the tenor be known without knowing what the vehicle means? The definition forms a logical circle in which tenor and vehicle define each other. Peter Macky remarks that “the problem with Richards’ definition is its ambiguity,… his formulation is not precise enough to be a good working definition” (44). David Douglass notes that even Richards himself uses tenor and vehicle in a confusing way:

> In some cases, he treated tenor as a literal paraphrase of the metaphor, which may be extracted as a statement (p. 135); in others, tenor appears to be restricted to the topic of discourse (p. 121) and in still others may extend to the implications of its subject (p. 120). In at least one case (p. 117-118), both tenor and vehicle are treated as the things to which the words themselves refer—an apparent contradiction of Richards’ primary thesis. (411)

By the same token, the tension between making and interpretation, or deviance and resemblance, becomes difficult to discuss. The Classical writers deliver very clear elaborations on deviance and resemblance by dealing with each through one specific perspective; Richards, nonetheless, just vaguely comments that “[i]n general, there are very few metaphors in which disparities between tenor and vehicle are not as much operative as the similarities” (127). Given that

Richards' theory is about metaphor users, deviance should be more important than resemblance.

Interactions, therefore, "do not work through *resemblances* between tenor and vehicle, but depend upon other relations between them including *disparities*" (107-108, emphasis in the original). Richards also notes that "[a]s the two things put together are more remote, the tension created is, of course, greater… what seems an impossible connection, an 'impracticable identification,' can at once turn into an easy and powerful adjustment if the right hint comes from the rest of the discourse" (125-126). By tension, he means the degree of dissonance; or in classical discussion, how remote two things are. I apply tension in the way Haser does, that "metaphors involve similarities between dissimilar concepts. This fundamental tension has far-reaching implications for the nature of the features shared by tenor and vehicle" (19).

Just like *metaphor*, the terms of Richards' approach, such as *interaction*, *tenor* and *vehicle*, are also metaphors. The ambiguities of these terms, however, make Richards' model extremely productive. Douglass argues that the Richards' model owes its prevalence to its ambiguity: "[i]f anything, Richards' inconsistencies and ambiguity seem to have been exploited as advantages by authors wishing to stretch the boundaries of his original conception" (414). In this regard, the ambiguities become strengths that keep metaphor studies going.

Another turn of Richards' theory is that his metaphor study shifts from seeking "what metaphor is" to "how metaphor means" (Douglass 407). An even more important turn, as discussed above, is that, although Richards' approach turns away from the classical categorization of metaphor studies, it meanwhile inaugurates a new category in metaphor study: Richards does create new concepts in order to engage with metaphors. Since Richards, metaphor studies have shifted from the descriptive to the methodological.

Black develops Richards' theory into the interaction view, which is of "importance in philosophy" if metaphors are classified "as instance of substitution, comparison, or interaction," because the interaction view is "free from the main defects of substitution and comparison views and [attempts] to offer some important insight into the uses and limitations of metaphor" (1962, 45 and 38). In Black's classification, a substitution view of metaphor holds that "a metaphorical expression is used in place of some equivalent *literal* expression," and a comparison view of metaphor holds that "a metaphor consists in the presentation of the underlying analogy or similarity" (*Models and Metaphors* 31 and 35, emphasis in the original).

The main assumptions of interaction view are:

> (1) A metaphorical statement has two distinct subjects—a "principal" subject and a "subsidiary" one.
> (2) These subjects are often best regarded as "systems of things," rather than "things."
> (3) The metaphor works by applying to the principal subject a system of "associated implications" characteristic of the subsidiary subject.
> (4) These implications usually consist of "commonplaces" about the subsidiary subject, but may, in suitable cases, consist of deviant implications established *ad hoc* by the writer.
> (5) The metaphor selects, emphasizes, suppresses, and organizes features of the principal subject by implying statements about it that normally apply to the subsidiary subject.
> (6) This involves shifts in meaning of words belonging to the same family or system as the metaphorical expression; and some of these shifts, though not all, may be metaphorical transfers.
> (7) There is, in general, no simple "ground" for the necessary shifts of meaning—no blanket reason why some metaphors work and others fail.
> (*Models and Metaphors* 44-45)

Assumption (1) is a further development of Richards' terminology. Black notices that "it is significant that Richards himself soon lapses into speaking of 'tenor' and 'vehicle' as 'things,' while by definition, tenor and vehicle should refer to thoughts. Assumption (2) makes it clear that Black prefers to study metaphors through things instead of thoughts. Also, by putting subjects to "systems of things," Black turns back to classical approach; that is, categories of things. Assumptions (3) to (7) delicately elaborates the tension of deviance and resemblance in an interactive approach. Unlike the classical scholars, Black does not treat deviance and resemblance separately; unlike Richards, Black does not tangle with the metaphor user's thoughts. Instead, Black goes after the semantics of metaphors through what can be established about two "system of things." From this perspective, Eileen Way's observation that "Black's claims are more moderate than Richards'" is germane (6). Deviance and resemblance become relative and dynamic in Black's theory, and they largely depend on what can be counted as "associated implications," "commonplaces," and "deviant implications" in a metaphorical expression. Hence, Richards' interaction is the interaction of two different thoughts; Black's interaction is interaction of transference and transferability. Or, more precisely, the tension between transference and transferability is considered transferable. This insight puts metaphors as a far more powerful thinking tool than what scholars thought at that time. Almost thirty years later, Black revisits his work. He clarifies and defends his earlier work, as well as pursues a few points further. In particular, he "intend[s] to defend the implausible contention that a metaphorical statement can sometimes generate new knowledge and insight by changing relationships between the things designated (the principal and subsidiary subjects)," because metaphors are "cognitive instruments through which their users can achieve novel views of a domain of reference" (35 and 38).

The complication of the "relationship between the things designated" can be illustrated by Edwin Black's discussion of the metaphor, "communism-as-cancer." He suggests that the misconception of the subsidiary subject "saves" the metaphor. As he elaborates, on the surface, this metaphor places communism in the category of natural phenomena. Logically, a cancer is not created by a person, so no one is supposed to be responsible for any cancer. If communism is a kind of cancer, then no one should develop a moral attitude toward its agents. Such perspective constitutes “a difficulty with the metaphor” (117). Yet according to psychological study done on cancer patients, “an extraordinarily high proportion of people who have cancer are disposed to blame the cancer on a morally responsible agent” (117). If one holds a more medically-informed notion of cancer as a natural process, the metaphor breaks down as a means of heaping loathing on communism. Once it becomes clear that cancer has nothing to do with the morality of cancer patients, “the cancer of communism” will not be considered proper. The established “commonplace” as epistemic commitments of the principle and subsidiary subjects are extremely important to metaphors.

Richards initiates the turn of metaphor studies away from the classical; Black’s work completes the turn. Mark Johnson concludes that “Black’s essay, ‘Metaphor,’ is perhaps the landmark by which we may orient ourselves in attempting to understand recent work on the subject” (19). Nonetheless, Douglass considers that “Black achieved something less than success in his endeavor,” for “Black’s terms are generally treated as synonyms for Richards’, and the two models are conflated” (416). While classical studies focus on how a metaphor establishes an Aristotelian Mapping through the resemblance of two non-related categories, Richards and Black’s theories emphasize dissonance, as Black puts it, “a metaphorical statement appears to be perversely asserting something to be what it is plainly known not to be” (21). Moreover, they treat the tension of Aristotelian Mapping at different levels: Richards locates the sources of the tension as tenor and vehicle, and Black studies the dynamics of the tension.

Metaphors and Thoughts

If Richards is the one who take metaphors from poets and give them to ordinary people, then George Lakoff is the one who strips metaphors of their poetic color by declaring that “ordinary everyday English is largely metaphorical” (204). Lakoff is “a cognitive scientist and linguist," different from the scholars discussed above who are either rhetoricians or philosophers. His cognitive approach is considered "a challenge to traditional Western thought from Aristotle to Descartes, as well as many philosophical assumptions and linguistic theories" (Dirven 78). Lakoff re-defines metaphor as "a cross-domain mapping in the conceptual system," while "metaphorical expression" is "a linguistic expression (a word, phrase, or sentence) that is the surface realization of such a cross-domain mapping" (203). He stresses that "[i]t is important to keep them distinct" (209).

Lakoff and Johnson argue that the "human conceptual system is metaphorically structured and defined" (6). More specifically, "the locus of metaphor is not in language at all, but in the way we conceptualize one mental domain in terms of another" (203). As for "[t]he conceptual domain from which we draw metaphorical expressions to understand another conceptual domain," Kövesces names that the source domain, "while the conceptual domain that is understood this way is the target domain" (4).

One of Lakoff's golden metaphors, or in his own word mnemonics, is LOVE IS A JOURNEY, which "inherits the structure of the LIFE IS A JOURNEY metaphor" (223). Of this metaphor, the target domain is "Love;" source domain, "Space." The correspondences are: "the lovers are travelers, and the love relationship is a vehicle" (224). Through this metaphor, "an internally consistent JOURNEY structure" is imposed on the concept LOVE, "including a beginning, a destination, a path, the distance you are along the path, and so on" (219). Therefore, "[o]ur conventional ways of talking about [love] presuppose a metaphor we are hardly ever conscious of. The metaphor is not merely in the words we use—it is in our very concept of love… We talk about [love] that way because we conceive of [it] that way" (5). Love is understood through a journey, a process that one experiences when one goes through a journey.

Or, love, a rather abstract and personal concept, is portrayed by projecting the features of a journey into it. LOVE IS A JOURNEY accurately represents one conception of love in American culture.

Based on such analysis, theoretically, the structure of metaphor is:

> Metaphors are mappings across conceptual domains.
> Such mappings are asymmetric and partial.
> Each mapping is a fixed set of ontological correspondences between entities in a source domain and entities in a target domain.
> When those fixed correspondences are activated, mappings can project source domain inference patterns onto target domain inference patterns.
> Metaphorical mappings obey the Invariance Principle: The image-schema structure of the source domain is projected onto the target domain in a way that is consistent with inherent target domain structure.
> Mappings are not arbitrary, but grounded in the body and in everyday experience and knowledge.
> A conceptual system contains thousands of conventional metaphorical mappings which form a highly structured subsystem of the conceptual system.
> There are two types of mappings: conceptual mappings and image mappings; both obey the Invariance Principle. (245)

The Invariance Principle is:

> Metaphorical mappings preserve the cognitive topology (that is, the image-schema structure) of the source domain, in a way consistent with the inherent structure of the target domain. (215)

I define such mapping system as Lakovian Mapping, which is visualized as part of Figure 1-2. In Lakoff's theoretical frame, *metaphor* is "a cross-domain mapping in the conceptual system;" and *metaphorical expression* is "what the word 'metaphor' referred to in the old theory" (203), in which "the word 'metaphor' was defined as a novel or poetic linguistic expression where one or more words for a concept are used outside of their normal conventional meaning to express a 'similar' concept" (202). The meaning of the term metaphor differs for him from that of classical theories. He redefines the concept metaphor, and further simplifies the classical theories of metaphors. His charge that "[i]n classical theories of language, metaphor was seen as a matter of language, not thought" cannot be totally justified because what he discusses is indeed a different realm from the classical theories, and further, as elaborated above, classical theories do more than he says (202).

Figure 1-2 is a comparison of Aristotelian Mapping as classical treatment of metaphor with Lakovian Mapping. As shown, in the Aristotelian Mapping, the metaphorical transference happens between two categories referred to the World. In Richards and Black's theories, the two systems are substituted by the vague word "things." In that of Lakoff, they are "domains." The fundamental difference between domain and classical category is the former is the cultural and the latter, natural. To put it another way, categorization in domains is fluid and negotiable in a way that it is not in Aristotelian metaphysics. In the elaboration of the differences between their theory and "the classical and still most widely held theory of metaphor, namely, the comparison theory," Lakoff and Johnson write:

> (1) [Classical] [m]etaphors are matters of language and not matters of thought or action. There is no such thing as metaphorical thought or action.
> (2) A [classical] metaphor of the form "A is *B*" is a linguistic expression whose meaning is the same as a corresponding linguistic expression of the form "A is like *B,* in respects *X,* Y, *Z* ...." "Respects *X,* Y, *Z,* ..." characterize what we have called "isolated similarities."
> (3) A [classical] metaphor can therefore only describe preexisting similarities. It cannot create similarities. (153)

To the first charge, as is discussed above, the classical studies do offer inexplicit discussion about mental activities in metaphor creating, although the studies do not take cognition as their focus. To the second charge, the type of metaphors that Aristotle and Quintilian admire most is far beyond it. To the third charge, again, classical categorization derives from the World, while

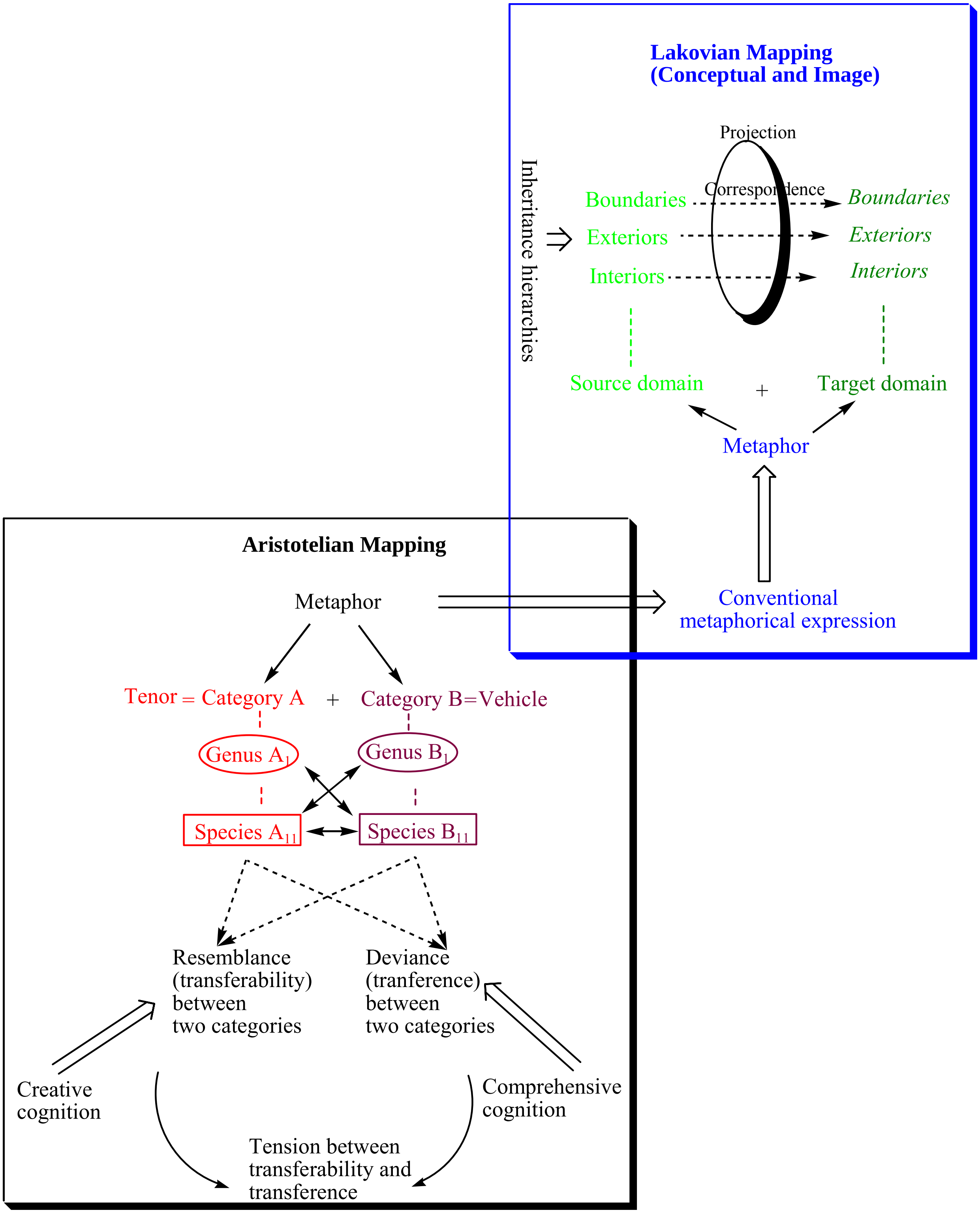

Figure 1-2: Aristotelian and Lakovian Mappings

Lakoff's domain derives from the culture; therefore, to the former, the similarities are preexisting, while the latter, similarities can be created through projection.

Most importantly, for most of Lakoff's conceptual metaphors, such as "an argument is war," "love is a journey," and "ideas are food," the domains are cultural phenomena. Lakoff also calls them "conventional metaphors," which indeed are dead metaphors. There are two problems revealed by Figure 1-2 with respect to Lakoff's criticism of classical theories:

First, Aristotelian metaphor and Lakovian metaphor are two different concepts dealing with two different phenomena; therefore, Lakoff's criteria should not be applied to the former, not to mention taking them as "standard view" or "standard theory" (Haser, 8).

Second, unlike what Lakoff marks, Aristotelian metaphor is much different from (conventional) metaphorical expression. It is an extremely complicated social process to turn a metaphor into a conventional metaphor expression. For instance, there have to be at least two cultural conditions for LOVE IS A JOURNEY: first, a culture that perceives love as partnership; second, a culture that has vehicles as the most popular means of travel. Hence, to apply a theory based on the analysis of some conventional metaphor expressions to all metaphors will be biased.

Comparing the theoretical treatments of "golden men" and "LOVE IS A JOURNEY," the difference is obvious. The classical discussion focuses on the creation and establishment of the metaphor; Lakoff's discussion is about the cultural influence of the metaphor. Nevertheless, the discussions on "golden age" bear many similarities with that of Lakoff: both focus on the conceptual functions of metaphor if the metaphors become conventional or dead metaphors. So classical and Lakovian treatments are indeed about two stages of the life of metaphors: the creation of metaphors, and the cultural function of dead metaphors (if they survive to leave the fossil remains that are dead metaphors).

Accordingly, while Aristotelian metaphors are powerful tools to help “break an old categorization, in order to establish new logical frontiers on the ruins of their forerunners” (Ricoeur 233), Lakovian metaphorical expressions are the linguistic manifestation of metaphors as cultural conceptualization, as he puts it: “[t]he system of conventional conceptual metaphor is mostly unconscious, automatic, and used with no noticeable effort” (245).

During almost three thousand years from the time when Hesiod invented “golden men,” while wishing, as an “iron man,” that he could have been living in a different time, either earlier or later, to the present when Lakoff points to LOVE IS A JOURNEY and says that we are living by such metaphors, how far have human beings gone? Maybe far; maybe not. As Fig 1-2 shows, Lakovian Mapping is not that far away from Aristotelian Mapping, unlike what some scholars may have thought. Nonetheless, Lakoff does greatly develop and enrich metaphor studies. Although none of these theories have much to say about using metaphor more or less consciously as a tool for exploring the unknown, the elements in their various perceptions provide a new tool to explore scientific research through a metaphorical approach—a approach so unusual that one must start from the anti-metaphor tradition to construct it.

CHAPTER TWO

From the Surprising to the Unsurprising: Metaphors and the Production of Knowledge

It is surprising that the use of metaphors in science is so unsurprising.
(Boyd, 496)

Parallel to the long and rich tradition of metaphor studies, however, is the anti-metaphor tradition. The common sense used to be that metaphors had little to do with the discourse of knowledge, so to talk about metaphors in science was surprising. Since metaphor was first called out and named by Isocrates, it has been regarded as problematic for the discourses of knowledge. Isocrates declared it and other irregularities of language to be inappropriate for "prose," and Plato was largely of the same opinion, though he did not use the word *metaphora* while using many of them. From them has flowed a long tradition of distrust of metaphors in serious discourses of knowledge making, a tradition that focuses on their indeterminacy of meaning as well as their lack of truth. Along the way, they provided Bacon and many other early modern scientists with a whipping boy to expose the errors of previous writers. Yet Aristotle, though Plato's student, also articulated a positive function for metaphor so that it could been seen as a means of conveying a new understanding of the matter under discussion, and this more positive view has in recent years been adopted and developed by a number of philosophers of science who will be represented in this discussion by Richard Boyd. This is a substantial step beyond the *tu quoque* arguments of Derrida in "White Mythology: Metaphor in the Text of Philosophy" (1982), for it suggests that the inescapability of metaphor need not be a scandal, and I further argue that the process of knowledge making involves a metaphorical kind of language that I name quasi-metaphor. Basically, quasi-metaphors help specify and conceive, as well as to guide research on what is considered unknown.

The Start of the Trouble: Isocrates, Plato, and Aristotle

The concern about metaphorical language in the discourse of knowledge starts as early as the beginning of metaphor studies, which can be represented mainly by Isocrates, Plato, and Aristotle's views on metaphors.

Isocrates proclaims in his discussion on prose *vs*. verse that:

> To the poets are granted numerous ornaments (*kosmoi*) [of language], for [*they can represent gods as associating with men, conversing with and aiding in battle whomsoever they please, and* [7]] they can express themselves (*delosai*) not only in ordinary language (*tois tetagmenois onomasiri*), but also by the use of foreign words (*xenois*), neologisms (*kainois*), and metaphors (*metaphorais*)... but to writers of prose (*tois de peri tous logous*) none of such [resources] are permitted; they must strictly (*apotomōs*) use both words (*onomatōn*) and ideas (*enthumēmatōn*) [of a certain category:] [1] of words, only those that are in the [ordinary] language of the *polis* (*tōn onomatōn tois politikois*); [2] of ideas, only those that are closely relevant to the matter at hand (*tōn enthumēmatōn tois peri autas tas praxeis*). (*Evagoras*, 8-10)

The Greek version of this passage contains the earliest recorded use of *metaphora* in the current sense of metaphor, referring to one of the "numerous ornaments of language." Isocrates grants the right to use metaphors to poets and bans them from prose. Yet as scholars later point out, *metaphora* is itself a metaphor, which originally means "carrying something from one place to another, transference" (Kennedy 59, Kirby 525). Until Isocrates wrote this passage, *metaphora* as a metaphor was neither in use nor considered an actual fact. Moreover, there is an intriguing contradiction in this very passage: Isocrates in his own prose has to employ metaphors to enunciate the inappropriateness of metaphor in prose.

As for Plato, he does side with Isocrates with respect to stylistic differences between poetry

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[7] The translation of this passage is from Kirby's article and done by himself. This particular sentence, however, is from Isocrates III, 9, because Kirby omits it in his translation.

and prose:

> But you will know that the only poetry that should be allowed in a state is hymns to the gods and poems in praise of good men; once you go beyond that and admit the sweet lyric or epic muse, pleasure and pain become your rulers instead of law and the rational principles commonly accepted as best. (*The Republic*, 606e to 607a)

Plato's passage echoes Isocrates on the generic functions of poetry and prose: poetry is supposed to represent gods, as well as the men who reflect them; prose, on the other hand, matter, law, and reasons. While Isocrates sets linguistic rules for both, Plato "has less to say about the compositional aspects of language and rhetoric than about their metaphysical and ethical ramifications" (Kirby 531). In his own writing, however, Plato employs metaphor frequently and "will go back to a metaphor, as a dog goes back to a bone, when one thinks he has done with it" (Robinson and Denniston 14). Indeed, he is himself "a master in the use of metaphor" and also "the mastermind coiner of a number of new metaphors (e.g., *eidos*, *idea*, *paradeigma*, *methexis*, *phantasia*, *methodos*, *theoria*) which subsequently had quite successful philosophical careers as dead metaphors" (Trabert 25).

In his fundamental work on the concept of knowledge, *Theaetetus*, Plato puts forwards "three metaphor complexes" in connection with his "metaphilosophical statements":

> 1. Metaphors of being on one's way, of travel and of voyage stand for philosophical inquiry.
> 2. Metaphors of battle, hunting, competition, and fighting are employed for philosophical argument, especially the metaphor of wrestling.
> 3. The dialogue features one of Plato's most famous metaphors: Socrates' description of himself as a philosophical midwife (Trabert 27-29).

Plato is able to elaborate his theories on knowledge through those metaphors. In particular, he delicately differentiates knowledge and perception, knowledge and true judgment, knowledge and true belief. Knowledge, thus he concludes, is none of the three. More importantly, those metaphors employed suggest Plato's conception of knowledge. For instance, he depicts his interlocutors who get confused during philosophical discussion as "sea-sick passengers" (191a); a less relevant discussion on a subject as "going a long way around" (197c); a philosopher as "a hunt after kinds of knowledge" (198a); and Socrates as "a philosophical midwife" (149a, 157c–d, 160e, 184b, 210b). These metaphors portray knowledge seeking as philosophical inquiry in which knowledge is an objective entity--static, distant, waiting to be discovered, and philosophers have to find their approach to obtain knowledge. Hence, metaphors are not only linguistic tools to articulate his theory of knowledge but also elements of the theory, or more precisely, the fundamentals on which he conceptualizes knowledge. In Bernhard Debatin's words, "Plato's metaphors anticipate a more rational way of thinking and thus constitute 'rational anticipation'" (25). The same contradiction that Isocrates has with metaphors also runs in Platonic writings. It implies that metaphors are inevitable in understanding and reflecting on knowledge.

Aristotle is the first one who systematically studied metaphor, in both *Poetics* and *On Rhetoric*, and his insights on metaphors were never limited to metaphors as linguistic ornaments. For instance:

> N[e]w glosses are unintelligible, but we know words in their prevailing meaning [*kyria*]. Metaphor most brings about learning; for when he calls old age "stubble[8]," he creates understanding and knowledge through the genus, since old age and stubble are [species of the genus of] things that have lost their bloom. (*Rhetoric*, 1410b)

As revealing as Hesiod's "golden men" to Isocrates, Homer's "stubble" to Aristotle brings

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[8] Robert Fagles' translation is "husk":

But now my heyday's gone—
I've had my share of blows, yet look hard at the husk
And you'll still see, I think, the grain that gave it life (243-245).

"before-the-eyes" the last stage of a hero's life, such that learning becomes easy and pleasant (*Rhetoric*, 1410b). "Stubble" immediately imposes a negative connotation to old age because it associates with emptiness, bleakness, and uselessness. Particularly in the *Odyssey*, as the hero's self-depiction in comparison with what he is in his prime, this metaphor sets up a tragic tone by bringing forth the similarities between the late stages of a man and a crop and emphasizing the sadness in both. In his meditation on Aristotle's analysis of this metaphor, Paul Ricoeur concludes that "this transgression is interesting only because it creates meaning" (23). Here comes the core question about metaphor: can the meaning of a metaphor be defined, like a word? Aristotle does not think so, for in his discussion on definition, he comments that, because definition demands clarity, it should not "become involved in equivocation." Moreover, "[i]f we are to avoid arguing in metaphors, clearly we must also avoid defining in metaphors and defining metaphorical terms; otherwise we are bound to argue in metaphor" (*Posterior Analytics*, 97b30-35). The point is that the meaning of a metaphor is ambiguous, and consequently arguable. On the other hand, Aristotle grasps that, even in poetic context, metaphors are not ornaments; rather, they create "understanding and knowledge." Specifically, Homer's metaphor *stubble* creates a new category in which men of old age and stubble belong together. Such are the so-called *ad hoc* categories, temporarily established to achieve goals through violating the correlation structure of natural categories (Barsalou 211, Rosch 382). By putting men of old age into a category of things that have lost their bloom, *stubble* delineates a hero's loss in aging. What makes the category *ad hoc* is exactly what makes the metaphor uncanny, that such a tragic perspective of a hero's life as stubble is unique and personal but can still be perceived and sympathized with. In context, Aristotle does not talk about the discovery of new knowledge but what makes a perspective striking and novel. The bearing of these remarks on scientific language was not discerned for two millennia. In their discussions about metaphors and knowledge, modern scholars often quote a proverbial translation of the second sentence in Aristotle's passage above:

> Midway between the unintelligible and the commonplace, it is metaphor which most produces knowledge.

Although I cannot trace it to Aristotle's text according to the reference lists of various sources, I think this translation elegantly captures Aristotle's philosophy of metaphor and knowledge production, such a remarkable start.

Bacon's Attempt to Banish Metaphors from Scientific Prose

The stylistic tradition of scientific prose can be traced back to Francis Bacon, who "is frequently hailed as the father of modern scientific prose" (Zappen, "Francis Bacon and the Rhetoric of Science" 244), although John Harrison argues that the role Bacon assigned to poetry in satisfying the needs of human nature is underestimated (107). Bacon does maintain the distinction between poetry and prose, for he considers poems are lies made for pleasure (*Of Truth*, 1). Historians establish "a more or less direct relationship between Bacon and later attempts by members of the Royal Society of London to establish the plain style as the standard in scientific prose" (Zappen, "Francis Bacon and the Historiography" 75). Bacon, therefore, is a precursor to both positivistic science and the plain style. Kate Aughterson argues that plain style is a tradition developed through "Wyatt, Cheke, Ascham, Donne, and Jonson rather than being simply a phenomenon of mid- to late seventeenth-century and Enlightenment scientific writing" (98). If so, then the Greeks should be counted in, as Morris Croll claims that, "[a]ccording to the sketchy and untrustworthy reports of ancient literary historians, Gorgias was its 'inventor'" (82). Croll also points out, on the other hand, that Muret, Lipsius, Montaigne, and Bacon inaugurated "the successful Anti-Ciceronian movement" through giving European prose-style "a new direction," and those men were "all Aristotelian at first hand" (79, 103).

Bacon criticizes his contemporary Ciceronians for prizing style at the expense of content:

> [M]en began to hunt more after words than matter—more after the choiceness of the phrase, and the round and clean composition of the sentence, and the sweet failing of the clauses, and the varying and illustration of their works with tropes and figures, than after the weight of matter, worth of subject, soundness of argument, life of invention, or depth of judgment. (*The Advancement of Learning* 32)

The opposite of this Ciceronian ornate style was the cluster of Aristotelian ideals, including clearness, brevity, and appropriateness associated with positive science (Croll, 74-78). The plain style is recommended for scientific prose, the features of which are "a lack of ornate metaphors; simple syntactic patterns; a logical progression of sentence and meaning; and a preference for well-tried words rather than neologisms: a transparent, trope-free language" (Aughterson 98). As an advocate[9] of the plain style, Bacon proposes these criteria:

> First then, away with antiquities and citations of authors; also with disputes and controversies and differing opinions, everything in short which is philological. Never cite an author except in matter of doubtful credit; never introduce a controversy unless in a matter of great moment. And for all that concerns ornaments of speech, similitudes, treasury of eloquence, let it be utterly dismissed. Also let all those things which are admitted be themselves set down briefly and concisely, so that they may be nothing less than words. For no man who is collecting and storing up materials for ship-building or the like, thinks of arranging them elegantly, as in a shop, and displaying them so as to please the eye; all his care is that they be sound and good, and that they be so arranged as to take up as little room as possible in the warehouse. And this is exactly what should be done here. (*Works*, IIIV, 359)

Even in this very passage, as Robert Schuler notices, "Bacon himself fails to meet these criteria," by "[h]is own penchant for analogous thinking and metaphor—exemplified here in the ship-

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[9] A vigorous plain style is "not a general prescription applicable to all scientific communication but a style peculiarly suitable for the writing of natural and experimental histories" (Zappen 1989, 80). More significantly, as scholars of science history and scientific rhetoric from the 1960 to the present argue, the plain style as a prescriptive style is part of the institutionalization of science. Bacon's role, accordingly, is not only a precursor of the plain style, but more importantly, a precedent in the development of scientific community. (Zappen 1989, 76-77)

builder" (49). Again, the same contradiction in Isocrates' and Plato's writings runs in this passage: in order to elaborate inappropriateness of metaphors, Bacon has to employ metaphors, and more explicitly indeed.

Yet his concerns about the problems of metaphorical language are absolutely germane to semantic clarity and precision. As a matter of fact, the best example to display the problems with metaphorical language comes from the interpretation one of his own metaphors. In 1980, Carolyn Merchant analyzes one of his passages from *Of the Dignity and Advancement of Learning*, the second book. She again analyzes it in her *The Death of Nature: Women, Ecology, and the Scientific Revolution* through feminist theory:

> *For you have but to follow and as it were hound nature in her wanderings, and you will be able, when you like, to lead and drive her afterwards to the same place again.* Neither am I of opinion in this history of marvels that superstitious narrative of *sorceries, witchcrafts, charms*, dreams, divinations, and the like, where there is an assurance and clear evidence of the fact, should be altogether excluded… howsoever the use and practice of such arts is to be condemned, yet from the speculation and consideration of them… a useful light may be gained, not only for a true judgment of the offenses of persons charged with such practices, *but likewise for the further disclosing of the secretes of nature. Neither ought a man to make scruple of entering and penetrating into these holes and corners, when the inquisition of truth is his whole object—as your majesty has shown in your own example*. (Italics added by Merchant)

Merchant also provides the historical context. In 1603, the first year of his English reign, James I replaced the milder witch law of Elizabeth I, which evoked the death penalty only for killing by witchcraft, with a law condemning all practitioners to death. In 1612, the trials of Lancashire witches of the Pendle Forest brought up the sexual aspects of witch trials in England for the first time. The confessions of the accused women, the stories of fornicating with the devil, were planted in the mouths of those women by a Roman Catholic priest who had emigrated from the Continent, because those women recently rejected Catholicism. These social events, Merchant believes, must have "influenced Bacon's philosophy and literary style." Furthermore, "[m]uch of the imagery he used in delineating his new scientific objectives and methods derives from the courtroom, and, because it treats nature as a female to be tortured through mechanical inventions, strongly suggests the interrogations of the witch trials and the mechanical devices used to torture witches" (168). She argues that "the strong sexual implications of the last sentence can be interpreted in the light of the investigation of the supposed sexual crimes and practices of witches" (169).

Evelyn Fox Keller quotes a part of the passage in her book *Reflection on Gender and Science* published in 1985:

> For you have but to follow and as it were hound nature in her wanderings, and you will be able, when you like, to lead and drive her afterwards to the same place again.

She remarks that, for Bacon, the goal of science was "the restitution and reinvesting of man to the sovereignty and power… which he had in his first state of creation." Yet the question is "[b]y what means, and from what sources was science to acquire such power?" She argues that "Bacon's answer to these questions is given metaphorically—through his frequent and graphic use of sexual imagery," and one of them is the metaphor in the quotation, meaning "[n]ature may be coy, but she can be conquered." Such metaphors, she further comments, are "important… to see how deeply Bacon's use of gender is implicated in his conception of mastery and domination" (35-36), and "[n]ot simple violation, or rape, but forceful and aggressive seduction leads to conquest" (37).

Another feminist scholar Sandra Harding quotes some part of Merchant's quotation of Bacon twice in her book *The Science Question in Feminism*, which was published in 1986:

> For you have but to follow and as it were hound nature in her wanderings, and you will be able, when you like, to lead and drive her afterwards to the same place again… Neither ought a man to make scruple of entering and penetrating into

> these holes and corners, when the inquisition of truth is his whole object—as your majesty has shown in your own example.

The first time is to demonstrate that "the political and legal metaphors of scientific method originated at least in part in the witchcraft trials of Bacon's day" (115). She notes that Bacon employs "bold sexual imagery to explain key features of the experimental method as the inquisition of nature," although "it might not be immediately obvious to the modern reader that this is Bacon's way of explaining the necessity of aggressive and controlled experiments in order to make the results of research replicable" (116). Moreover, she concludes that "[b]oth nature and inquiry appear conceptualized in ways modeled on rape and torture—on men's violent and misogynous relationships to women—and this modeling is advanced as a reason to value science" (116). The second time, in which she omits the line, "as your majesty has shown in your own example," is to suggest that "metaphors are, of course, one way of expressing value judgments." She interprets this metaphor as to "*recommend* that similar benefits can be gained from nature if it is conceptualized and treated like a woman resisting sexual advances" (237, emphasis hers).

Some scholars are disturbed by the interpretations of these three scholars. In his article published in 1998, Iddo Landau points out that, first, the three quotations of these three scholars are "misquotations;" and second, the conclusions are "far-fetched." Landau's interpretation of the metaphor starts from the overall contents of the book, in which Bacon "distinguishes (a) the course of nature; (b) what he calls 'the wanderings of nature' (i. e., uncommon natural events); (c) art (i. e., cases in which human beings intervene in the course of nature to produce effects that Bacon believes to be beneficial)." Bacon also "recommends attention be given to natural irregularities, and regrets that 'a substantial and methodical collection of the Heteroclites or

Irregulars of nature well examined and described I find not; especially not with due rejection and as it were public proscription of fables and popular errors'" (49-50).

The passage below is in Bacon's discussion of irregularities in relation to witchcraft and charms:

> *The end of this work [i.e. researching irregularities carefully] honoured with a precedent in Aristotle, is nothing less than [i.e. not] to gratify the appetite of curious and vain wits, as the manner of mirabilaries is to do; but for two reasons, both of great weight; the one to correct the partiality of axioms and opinions, which are framed for the most part upon common and familiar examples; the other, because from the wonders of nature is the most clear and open passage to the wonders of art.* For you have but to follow and as it were hound nature in her wanderings, and you will be able, when you like, to lead and drive her afterwards to the same place again. Neither am I of opinion in this history of marvels, that superstitious narrative of sorceries, witchcrafts, charms, dreams, divinations, and the like, where there is an assurance and clear evidence of the fact, should be altogether excluded. *For it is not yet known in what cases, and how far, effects attributed to superstition participate of natural causes; and therefore* howsoever the use and practice of such arts is to be condemned, yet from the speculation and consideration of them *(if they be diligently unravelled)* a useful light may be gained, not only for the true judgment of the offences of persons charged with such practices, but likewise for the further disclosing of the secrets of nature. Neither ought a man to make scruple of entering and penetrating into these holes and corners, when the inquisition of truth is his sole object,-as your Majesty has shown in your own example; *who, with the two clear and acute eyes of religion and natural philosophy, have looked deeply and wisely into those shadows, and yet proves yourself to be truly of the nature of the sun, which passes through pollutions and is not defiled. I would recommend however that those narrations which are tinctured with superstition be sorted by themselves, and not mingled with those which are purely and sincerely natural. But as for narrations touching the prodigies and miracles of religions, they are either not true or not natural; and therefore impertinent for the story of nature.* (Italics is the parts omitted by Merchant)

Landau argues that, in the context, "wanderings" is a metaphor for irregularities, "entering and penetrating into holes and corners" is not part of a sexual metaphor, but of a "knowledge as light" metaphor, "where light is taken to illuminate dark corners, i.e. ignorance." As far as he is concerned, this passage "does not satisfactorily demonstrate Merchant's claims," for Merchant does not clarify either how hounding “nature in her wanderings” so that one can afterwards “lead and drive her afterwards to the same place again” is related to torturing witches with mechanical devices; or where the passage employs imagery taken from the courtroom or from the torture chamber; or how the last sentence of her quotation is interpreted as intercourse or rape, as she argues it is the devil who is supposed to have intercourse with the accused witches, not men. Keller’s quotation is partial, and Landau considers that “there is nothing in Bacon’s passage about the coyness of nature.” Harding’s citation is even worse, in which her rape interpretation “does indeed seem more plausible, but only under these bogus conditions” (51-52).

Alan Soble’s “In Defense of Bacon” was first published in 1998, and he furiously attacks Harding’s interpretation, especially one of the conclusions that she draws, that “Francis Bacon appealed to rape metaphors to persuade his audience that experimental method is a good thing[10].” He rebukes the feminist interpretation as “damning criticism” and “abusive accusation” (453). He speculates that Harding, according to Keller’s reading of the passage, “might have concluded… that the conquest of nature implied by ‘hound’ is more accurately described as rape.” He argues that, for Bacon, rape and seduction are different, and Bacon “advises that science would be more successful by patiently wooing nature than by raping her.” Furthermore, “‘penetration’ needs not to be taken as have any sexual implications. And even if ‘penetrating’ is sexual for Bacon, ‘penetration’ does not entail or suggest rape” (454). Soble mocks the rape interpretation by pushing it further:

> So, thinking that his audience found rape desirable, attractive, permissible, or at least that it would be, even if despicable, Bacon champions experimentalism by drawing an analogy between it and rape. Bacon says to them [his audience]: Think of doing science my way as forcing apart with your knees the slender

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[10] This sentence appears in her *Whose Science? Whose Knowledge? Thinking from Women's Lives* on page 43. She alludes to it in her *The Science Question in Feminism* on page 237.

> thighs of an unwilling woman, pinning her under the weight of your body as she kicks and screams in your ears, grabbing her poor little jaw roughly with your fist to shut her mouth, and trying to thrust your penis into her dry vagina; that, boy, is what the experimental method is all about. (453)

At this point, one has to ask: where will these arguments on the metaphor not go?

The battles on the reading of Bacon are much broader and deeper than the interpretation of this single metaphor, but this case is revealing. Bacon could have never expected that the interpretations of his own metaphor set the best example of what could go wrong with a metaphor. Both sides agree that the source of the dissonance in the pas sage is "hound," together with "entering, penetrating, corners, and holes." The differences, however, are the perception of resemblance. Landau takes the resemblance between those words and light; whereas the feminists, sexual activity, or even rape. Soble is strongly against interpreting the metaphor as rape, although it can be perceived that rape in the feminist scholars discussion means force, conquest and control represented in rape; whereas in that of Soble, graphically violent intercourse. The feminist interpretation is not as ridiculous as Soble remarks, that "Harding's philosophy itself should be rated X: she injects sex or rape where there is none to begin with" (456), since Bacon often refers to nature in the feminine gender. Arguably, Soble's metaphor *inject* can be interpreted as an accusation that Harding rapes Bacon's passage, perhaps a virgin, through such a forced interpretation. Landau maintains that nature as *she* is a "dead metaphor" already, which "loses its 'freshness' and original connotation" (55). In other words, Bacon may use the metaphor without being aware the connotations made explicit by the feminist scholars, because the connotations are normal to the ideology of his time, so normal, and the metaphor and other similar ones are used so frequently, that they become dead metaphors. Nevertheless, is it not one of challenges of the feminist movement, "to wake up sleeping metaphors by becoming aware of their implication, will rob them of their power to naturalize our social conventions about gender”? (Martin 501).

As an important part of Alan Sokal’s recent criticism on feminist science-criticism, he points out that the core of the debate is not about waking up dead metaphors, or even not about whether there are sexist metaphors in scientific discourse—admittedly, there are; but whether modern science contains any sexist assumptions, or in a broader context, what sexist metaphors entail for the history and philosophy of science. To him, on one hand, the feminists are not able to locate and criticize any sexist assumptions in modern science, independently of any argument from history. On the other hand, there is ample evidence, independent of any allegedly sexist imagery, for the epistemic value of modern science. Hence, “the fundamental flaw in Merchant and Harding’s metaphor-hermeneutics is not exegetical but logical,” which, as the ‘generic fallacy’: evaluating an idea on the basis of its origin rather than its content” (122).

With regard to Harding’s reference of Newton’s law as “Newton’s rape manual,” which, to her, is as “illuminating and honest” as “Newton’s mechanics,” Sokal is obviously agitated, but he responds in a different way from Soble. Sokal asks, “[d]oes Harding realize what she is saying?” As a scholar who has the intelligence to appreciate fully the significance of Newton’s law and the opportunity to enjoy the products made by the technology based on Newton’s law, like the computer she uses to compose, “[d]oes Harding *really* contend that rape and torture metaphors *helped* to bring about this cognitive and material progress?” (121, emphasis in the original)

Bacon’s metaphor proliferates not only arguments on itself but also more arguments about the arguments on it. These arguments on interpretation are exactly the concerns of those thinkers from Isocrates to Bacon. What happens to Bacon’s metaphor is what Aristotle has said, that “we are bound to argue in metaphor.” The trouble is, in Black’s words, that “there can be no *dictionary* (though there might be a thesaurus) of metaphors” (*More about Metaphor*, 24 emphasis in the original). In other words, metaphors do create meanings, but the meanings are ambiguous, imprecise, and arguable. It therefore seems a logical conclusion that metaphors should be avoided in prose for the sake of clarity, brevity and appropriateness.

Further Doubts about Metaphor in Science

Following Bacon, positivists draw a sharp boundary between metaphorical and literal statements, and plain style is the standard for scientific prose, from which metaphors are especially excluded. Furthermore, some scholars even think that metaphorical thinking is dangerous in science. In Gaston Bachelard’s classic book *The Formation of the Scientific Mind*, he provides an instance of a misleading metaphor in scientific research.

In 1785, Dutch mathematician and physicist Jean Henri van Swinden (1746 – 1823) published his *Recueil de Memoires*, giving it the title *Analogie de l'electricite et du magnetism*, in which he recounts the objections to the many analogies claimed to unify electricity and magnetism in one theory. A physicist preceding him, Dutch physicist Anton Brugmans (1732 – 1789), proposed a hypothesis that a magnet contained minute invisible particles of iron, each of which possessed by itself the properties of a separate magnet. In each particle of iron resided two distinct fluids, the austral and the boreal, which were inert and neutral when combined as in ordinary iron, but the austral attracted the boreal and *vice versa*, and the boreal repelled the boreal, so did austral, when decomposed. This two-fluid model of magnetism is essentially the symmetry of the two-fluid model of electricity, in which inverse-square attraction or repulsion between portions of different or the same kinds of fluid accounted for the phenomena (Purrington 34). Brugmans thought that, “[j]ust as a sponge transports water through its whole mass, with more water being transported as the volume of the sponge increases, so iron, which has the greatest mass or volume, seems to attract and extract (abducere) a greater quantity of Fluid than the Iron of lesser volume." After conducting a few experiments, van Swinden believed that Brugmans' conception was wrong. He wrote:

> This expression "iron is a sponge for the magnetic Fluid" is therefore a metaphor that turns away from the truth, and yet all explanations are based on this expression which is taken in its literal sense. In my view, though, it is not correct to say that all Phenomena can be reduced to this, that Iron is a sponge for the magnetic fluid, and then to argue however that appearances deceive us here. It is not correct to think that reason shows these expressions to be erroneous while nevertheless using them to explain Experiments. (Bachelard 85, emphasis in the original)

Based on this statement, Bachelard concludes that:

> Van Swinden's thought is very clear: it is not as easy as we make out to confine metaphors to the realm of expression alone. Like it or not, metaphors seduce reason. They are particular and distant images that imperceptibly turn into general schemata. A psychoanalysis of objective knowledge must therefore take great care to remove all the colour from these naive images even if it cannot erase them. Once abstraction has gone through this process, it will be time to illustrate rational schemata. To sum up: primary intuitions are an obstacle to scientific thought; only an illustration that works beyond the concept and brings back a little colour to essential features can help scientific thought. (85)

Bachelard scolds Brugmans for the use of the sponge metaphor, yet he does not explore another possibility that what really goes wrong is the conceptualization of the interaction between iron and magnetic fluid. Or further, there is no such thing as magnetic *fluid* but instead magnetic *field* in the current view. Brugmans conceptualized the interaction between iron and magnetic fluid as sponge and water, so he employed the metaphor. van Swinden is against the conception that electricity and magnetism are alike. His treatise on this issue won the gold medal in the contest on Analogy Between Electricity and Magnetism organized by the Munich Academy of Sciences in 1776, that "there is no analogy between electricity and magnetism, or, at least, if there is one, it is very weak" (Kipnis 216-7). Brugmans' thinking, as far as van Swinden is concerned, is limited by the very conceptualization that he expresses in the metaphor. Brugmans still could have had a more reasonable theory if only he had thought beyond his original conception, and designed experiments accordingly. van Swinden pointed out the problem in Brugmans' approach towards experiments: to van Swinden, a conception should be tested and modified by experiments, but to Brugmans, a conception is to explain experiments. Methodologically, Brugmans' approach is a logical circle in which scientists design experiments according to a conception(s), and then use the data from the experiments to illustrate the conception(s). By "'iron is a sponge for the magnetic Fluid' is therefore a metaphor that turns away from the truth," van Swinden suggests that the conception expressed by the metaphor contradicts observations from the experiments of his design. Thus contrary to Bachelard's claim, Brugmans' problem is not his employment of the metaphor to express such a conception; it is the conception itself that is wrong, and further Brugmans' research methodology makes it impossible to correct it.

Intriguingly, in the passage of Bachelard's criticism on the inappropriateness of metaphors in scientific discourse, he employs a series of metaphors. Metaphors to him are particular, distant, and naive images, or colors of such images. This definition echoes the classic view on metaphor, that metaphors are "ornaments of language." He is acutely aware that metaphors cannot be confined to "the realm of expression alone." In order to delineate how hard it is to resist metaphors in reasoning, he employs a metaphor, metaphors *seduce* reason. The word *seduce* not only personifies metaphors and makes metaphors active agents who, by themselves, not by the choice of scientists as the users, can act to corrupt reasoning process, but it also vividly indicates that the usage of metaphors always goes beyond the boundary drawn for them. Ironically, even those who draw the boundary, like Isocrates, Plato, and Bacon, are seduced by metaphors. Bachelard, too, steps right into the same trap, in which he has to use a metaphor as the best expression of what he means, while maintaining the belief that there are literal, or even more precise, substitutes for metaphors. On the other hand, Bachelard grasps that metaphors have something to do with "primary intuition." In other words, metaphors are somewhat signifiers of cognition. This insight is beyond metaphors as "ornament of language," and it glimpses the reason for the contradiction running since Isocrates, that what metaphors do is much more than what metaphors are defined to do. Bachelard, however, does not question the definition of metaphor.

The Recovery of Metaphor as a Source of Scientific Knowledge

The modern turn of metaphor studies start precisely from reexamining the conceptualization of metaphor, when what metaphors are defined to do and what metaphors do become so obviously irreconcilable. Classical studies on metaphors are mainly placed within poetic style, although, again, their contributions do exceed rhetorical and stylistic scope. Modern metaphor studies intend to go beyond literature, and this turn makes two advances compared with classical studies: first is to explore more specifically the interactions between metaphor and thought; Second and consequently, to introduce cognitive science into metaphor studies. "The shift of emphasis from metaphors as an occurrence in language to metaphor as an occurrence in thought," as Michael Osborn observes, "is not an abrupt change, creditable to the genius of one man, but rather is a slow, growing realization, the development of which can be traced through various periods of rhetorical theory" (122). When Kenneth Burke proposes the four master tropes, he emphasizes that his "primary concerns with them… will be not with their figurative usage, but with their role in the discovery and description of 'the truth'" (503). The heuristic function of metaphors has been long recognized, so there is no doubt that metaphors play major roles in describing "the truth," or more precisely, knowledge. Aristotle first points out this function of metaphors, and it is still a very important aspect of modern metaphor studies. The nuance is that, to Aristotle, metaphors make knowledge intelligible; while to modern scholars, metaphors popularize science. The radical idea in Burke's conception is that metaphors have a role in "discovery" of knowledge. In doing so, "[m]etaphor, instead of being selected after and apart from the discovery of ideas, occurs anterior to, and actually generates, the discovery of ideas. Metaphor, instead of being subservient to thought as the vehicle of its expression, becomes the master of thought as determiner of its nature," thus, Burke "essentially has opened a new area of emphasis which may become a vital phase of development for the theory of metaphor" (Osborn 130). Metaphors become a processor of thinking rather than a linguistic tool to express thoughts, the final products of thinking. To Burke, metaphor is a "perspective," "a device for seeing something in terms of something else. It brings out the thisness of a that, or the thatness of a this" (503).

Burke's conception of metaphor falls with Max Black's interactive view. The strength of Black's theory is, as discussed in Chapter One, its dynamic view of the tension between resemblance and dissonance of a metaphor; that is, resemblance and dissonance are themselves transferrable or interchangeable, depending on context and perspectives. Or, in Burke's terms, thisness or thatness depends on who is seeing, what is intended to be seen; or, in Andrew Ortony's words, "metaphors permit us to see aspects of reality that they themselves help to constitute." This claim is related to two themes. The first is that metaphors "afford different ways of viewing the world" (Ortony 5). The second is the idea that something new is created when a metaphor is understood. I add that the third implication of Black's theory is that the resemblance designated by a metaphor may vary in different stages of a thinking process—that is, thisness or thatness also depends on when to look—as in a knowledge -seeking process like scientific research. This implication in Boyd's word is the open-endedness of metaphors.

In his elaboration on the problems with the comparison view, Black seems inspired and states that "[i]t would be more illuminating in some of these cases to say that the metaphor creates the similarity than to say that it formulates some similarity antecedently existing." Yet he moves right away from this brilliant point, only leave a footnote saying "[m]uch more would need to be said in a thorough examination of the comparison view" (37). Black is also worried about the indeterminacy of open-ended and interactive metaphors that he limits their proper role to heuristics and the pretheoretical scouting up of the terrain as well as possible explanations.

Boyd goes further, arguing that metaphor has a place in the very constitution of theories themselves. Boyd's study of metaphors in science focuses on explaining the role of metaphors in the articulation of new scientific theories, or in theory change. He believes that "there exists an important class of metaphors which play a role in the development and articulation of theories in relatively mature sciences. Their function is a sort of *catachresis* – that is, they are used to introduce theoretical terminology where none previously existed" (482). *Catachresis* is not usually used in this sense in rhetoric. According to *A Handlist of Rhetorical Terms* by Richard Lanham, *catachresis* means abuse or *abusio*. Primarily, it refers to implied metaphor, or using words wrenched from common usage, as when Hamlet says, "I will daggers to her." Secondarily, it refers to an extravagant, unexpected, farfetched metaphor, as when a weeping woman's eyes become Niagara Falls (31).

Perhaps, Boyd uses *catachresis* to highlight the extension of the term into a novel area, for he treats it as the kind of "theory-constitutive scientific metaphors." Though these metaphors may be "fundamentally pretheoretical, and lack the explicitness and precision" (486), using them can "accomplish the task of accommodation of language to the causal structure of the world" (483). More specifically, these metaphors can introduce terminology and modify usage of existing terminology, such that "linguistic categories are available to describe the causally and explanatorily significant features of the world" (483). The linguistic categories can "cut the world at its joints." How so? Because the theory-constitutive scientific metaphors serve as a *nondefinitional*[11] mode of reference fixing, which is especially "well suited to the introduction of terms referring to kinds whose real essences consist of complex relational properties, rather than features of internal constitution" (483). Boyd's conception of reference heavily relies on Hilary Putnam and Saul Kripke's theory, but he proposes to discuss reference in terms of epistemic access, which is "between language users and features of the world" (485). Epistemic access functions through avoiding "the necessity for idealized reference to *dubbing*[12] and for an implausible emphasis on the role of speaker's referential intentions" (508). As far as Boyd is concerned, such linguistically mediated epistemic success, including modification of linguistic usage to accommodate language to newly discovered causal features of the world, is the very core of reference. Furthermore, epistemic access becomes handy in some processes, like that of scientific research: when referential precision is unavailable, then "reference is continuous if the term in question continues to provide epistemic access to the same kind" (520). Epistemic access through metaphors, on one hand, maintains continuity of the inquiry; on the other hand, it allows theories to be revised.

There are two problematic aspects regarding Boyd's theory. First, Boyd himself is aware

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[11] *Nondefinitional* means the metaphoric term is not given necessary and sufficient criteria to define its reference. Natural kind terms are not definitional either: they do not work by listing the necessary and sufficient criteria that something must have to be gold, or an oak tree.

[12] *Dubbing* refers to an alternate model for determining reference, which is to treat the term as in essence a proper name fixed by some institutional act of naming ("dubbing").

that one of the problems with the concept theory-constitutive metaphors is that "we are typically unable to define the relevant respects of similarity or analogy between the primary and secondary subjects of these metaphors" (519). He argues that, on the epistemic access account of reference, there is no reason to doubt "the relevant metaphorical expressions refer," at least, they are "referring to features of the world delineated in terms of those – perhaps as yet undiscovered – similarities and analogies," which wait for scientific research to discover. Then semantically, what exactly do these metaphors contain except for the epistemic access? More precisely, what can these metaphors access? Boyd has an example to illustrate epistemic access:

> Consider, for example, the case of cries issued by sparrows to warn others of approaching predators. Such crying "extends the senses" in a perfectly straightforward sense. Sparrows, hearing such cries, are able to detect indirectly the presence of predators outside their line of sight through the efforts of others. The detection of predators takes on a social character: sparrows have, in such cases, socially coordinated epistemic access to certain kinds of predators. Even though it may be inappropriate to talk of "reference" in such cases, a "warning cry" is a warning cry rather than a mating call precisely because sparrows (a) can detect predators by sight with fair reliability, (b) typically issue warning cries only when they do so, and (c) typically respond to hearing warning cries in much the same way that they respond to seeing a predator. (505)

In this case the warning cry signifies danger but not the particular predator seen. That is to say, what the predator precisely is does not matter, as long as (a), (b), and (c) are established as a collective understanding—in Boyd's words, "socially coordinate epistemic access." The key issue, however, is not the epistemic access in the warning cry as danger, but rather how to establish "socially coordinate epistemic access" to predators. In scientific inquiry, particularly, the situation is more like such: the one who sees the predator is the only one who sees the predator and knows that what is seen is a predator, while the rest of the school can neither see nor understand the meaning of such seeing. Kuhn thus remarks that "Boyd seems simply to assume that the term's adherents of a given theory somehow or other know to what their terms refer. How they can do so ceases to concern him" (536).

Second, Boyd emphasizes that theory-constitutive metaphors are devices of the scientific community to "accomplish the task *of accommodation of language to the causal structure of the world*" (483, emphasis in the original). Boyd's view on language is quite different from that of the positivists. He recognizes that metaphors are necessary in scientific inquiry precisely because their imprecision allows theories to be modified as research progresses. Nonetheless, his view of knowledge as the objective waiting to be discovered echoes that of Plato. In this perspective, language passively *accommodates* the changing view on the causal structure of the world, made by scientific discoveries, and the discovery of the causal structure of the world, as implied, has nothing to do with language. Inevitably, he concludes that "[t]here is no purely *linguistic* precision, no mere following of *linguistic* rules, which accounts for precision in the use of theoretical terms" (521, emphasis in the original). If the language that presents the causal structure of the world is not precise, then knowledge cannot be knowledge because it is hardly a shared belief. Boyd justifies that "there are no distinct principles of linguistic precision in science, but rather that linguistic precision is one of the consequences of methodological precision of a quite general sort" (523). By "methodological precision," he means "precision in reasoning, careful experimental design, diligent reporting of data, proper control of experimental variables, precision in measurement, and so forth." According to the empiricist understanding of scientific practice, methodological precision is "precision in scientific practice" (521). In methodological precision, however, at least three aspects, precision in reasoning, careful experimental design and diligent reporting of data, must be achieved through linguistic precision. The core of methodological precision is indeed linguistic precision, but it is not through any linguistic hygiene for regulating metaphors that precision is obtained. Hence, Boyd's conclusion that scientific language generally lacks precision should be reconsidered. Kuhn asks, “What is the world, …, if it does not include most of the sorts of things to which the actual language spoken at a given time refers?” Or more specifically, “[i]s what we refer to as ‘the world’ perhaps a product of a mutual accommodation between experience and language?” (541-542).

The answers may lie in a different approach. In the notes to this article, Boyd comments that he does not address the question: what role does metaphorical thinking play in theory invention? It is inspiring because before theory change there is theory invention. What if the investigation of metaphors in science starts from the beginning of research, theory invention?

Metaphors and the Unknown

The tricky point in scientific discovery is that the truth is the unknown to be discovered. It indeed is an age-old paradox, as Socrates asks Cratylus: “but if things are only to be known through names, how can we suppose that the givers of names had knowledge, or were legislators before there were names at all, and therefore before they could have known them?” (142) In other words, how can one name the unknown? Yet without naming the unknown, how can one talk and think about the unknown in order to get to know it? This paradox has never been an obstacle to science because a part of scientific thinking is how to bypass it. Robert Oppenheimer says:

> Whether or not we talk of discovery or of invention, analogy is inevitable in human thought, because we come to new things in science with what equipment we have, which is how we have learned to think, and above all how we have learned to think about the relatedness of things. (129)

Metaphors, analogies, and models have similar functions in scientific research. Formal analogies are propositions of the form A is to B as C is to D such that the relationship between A and B is similar in direction and proportion to the relationship between C and D. A metaphor is a form of analogy in which an identity is suggested or implied in the general form A is C (Gilbert 315).

With respect to their functions in science, "analogical transfers of 'ideas' as well as metaphors are routine features of scientific as well as everyday reasoning" (Knorr 204). A model, generally speaking, is "an object of imitation." In science, since 1901, *model* refers to "[a] simplified or idealized description or conception of a particular system, situation, or process, often in mathematical terms, that is put forward as a basis for theoretical or empirical understanding, or for calculations, predictions, etc.; a conceptual or mental representation of something" (*Oxford English Dictionary*). Black takes *models* as "speculative instruments," which "bring about a wedding of disparate subjects, by a distinctive operation of transfer of the implications of relatively well-organized cognitive fields" (237). There also are warnings against models. In French theoretical physicist Pierre Duhem's (1861–1916) discussion of the establishment of physical theories in 1906, several decades before Black, he writes:

> Let us admit frankly that the use of mechanical models has been able to guide certain physicists on the road to discovery and that it is still able to lead to other findings. At least it is certain that it has not brought to the progress of physics that rich contribution boasted for it. The share of booty it has poured into the bulk of knowledge seems quite meager when we compare it with the opulent conquest of abstract theories. (99)

To him, thinking through models is much less intelligent than abstract reasoning. A model might help at the very beginning, but forming a theory needs a lot more than a model. This is, indeed, an insight on successful scientific research. Compared with the efforts to establish a theory eventually, the contribution of a model might not be significant at all. Yet how do scientists start to conceptualize possible objects of research? Exploring the relation between models and science is important because science is always thought of in terms of facts and precision, and "the imaginative aspects of scientific thought have in the past been too much neglected." Such a relation fills, though partly, "the gap between the sciences and the humanities." Science is, to Black, "an affair of the imagination" (243).

What metaphors do in scientific discovery, therefore, is to bring "thisness" or "thatness" into the unknown. For instance, Thomas Young used the metaphor, "light is the undulation of an elastic medium," to suggest that light is a material similar to waves, which is different from Newton's idea that light is a stream of particles--"Rays of Light are very small Bodies emitted from shining Substances." The unknown about light at that time (A.D. 1802) was the physical nature of light, though light itself was a well-known phenomenon. The point of the argument is how to define light as a physical existence, rather than whether light itself is an existence. The tenor of both Young and Newton's metaphors is the physical characteristics of light, and the vehicle of Young is the wave, and of Newton, the particle. Thus, two different perspectives on the physical characteristics of light are proposed through two metaphors.

In the history of science, however, words are not always this handy, especially when scientists cannot yet determine the existence of their supposed referents. So scientists often create new words to refer to what they are trying to discover. Some research fails, so the words die; some succeed, and these terms become scientific concepts, landmarks of human understanding and civilization. Scientific textbooks provide precise definitions of them; scientists use them day in and day out; mass media writes them and writes about them frequently; ordinary people use them to explain things that they do not understand before. In short, they become knowledge; they become public inspiration; they become culture. The fact that they are coined words, however, makes it hard to pinpoint what and how they function in scientific research. Are they are models, analogies, metaphors, or catachresis?

Then the key question comes back: How to define the unknown? "The need to define is a persistent characteristic of the human mind. Man constantly seeks to circumscribe the unknown. These attempts to define the unknown must begin with the known" (Thomas 8). What is considered as known must be articulated; otherwise, the unknown cannot be specified.

Boyd discusses the most important aspect of the known, *reference*, through Putnam's realistic theory[13] of meaning. Putnam proposes to specify "a normal form (or, rather, a type of normal form) for the description of meaning," including a syntactic marker, a semantic marker, a stereotype, and an extension (269). For instance, water is:

Syntactic markers: mass noun, concrete;
Semantic markers: natural kind; liquid at room temperature;
Stereotype: colorless; transparent; tasteless; thirst-quenching; etc.
Extension: H_2O (give or take impurities)

As to H_2O, it means that "the extension of the term 'water' as *they* (the speakers in question) use it is *in fact* H_2O" (269). *Fact* is an interesting choice of word, for this fact does not exist outside the establishment of modern chemistry. Strictly speaking, the molecular formula of water is H_2O; Or, H_2O is the molecule that constitutes water. Water and H_2O are the same "thing." By using H_2O, one does not refer to something different from water; rather, one puts water in the epistemic category of modern chemistry and signifies one's commitment to it. In other words, Ophelia was never drowned in H_2O, for Shakespeare did not know that water was H_2O. In John Dupré's discussion of "natural kind" general terms naming biological organisms, he points out that "no such sameness relations suitable for Putnam's theory can be found in it" (70). The reason is that the theory of evolution has undermined the belief in the fixity of species, the traditional assumption since Aristotle. According to the theory, organisms could be

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[13] In *Mind, Language and Reality*, Volume 2, mainly, Chapter 11, Explanation and Reference, and Chapter 12, The Meaning of 'Meaning,' Ian Hacking comments that "he [Putnam] has since become increasingly anti-realistic" (74).

unambiguously sorted into discrete kinds on the basis of overt morphological characteristics (84). Reference, therefore, is not only about what a word refers to, but also, more importantly, the position of what is referred to in the epistemic categories of the world, although the way to categorize the natural world has changed much since Aristotle, and there is more than one way to categorize the world.

In Putnam's theory, the "essential gist" of two claims about reference is: first, "concepts which are not strictly true of anything may refer to something;" and second, "concepts in different theories may refer to the same thing" (197). He specifically notes that the first one is more controversial and less realistic, for "the idea that concepts provide necessary and sufficient conditions for class membership has been attacked" (197). Indeed, his normal-form description of *water* (a natural kind) does not enumerate necessary and sufficient conditions for something to be properly called water. Putnam insists that the first claim can be supported by Bohr's case, that even though *electron* is not exactly what Bohr meant when he used it, the term *electron* still refers to a fact-- today an electron is a member of the lepton class of fermions. He points out that some metaphorical terms can survive, if they can grow from the metaphorical to the factual; or, their references can be established in the process of scientific research. In so doing, the metaphorical terms acquire meaning, or reference, and a stable position in the epistemic categories of the world as well. The fundamental difference between the early and later stage is the difference between metaphorical and literal, non-referential and referential, non-definitional and definitional. From my view, at the beginning of the inquiry, the reference of the borrowed or coined terms is the unknown that is not discovered yet, so the borrowed or coined terms do not have a real reference or a position where their referents belong within the epistemic categories. Rather, they have a virtual reference, speculated to have some place within the established epistemic categories; or, in some cases, such a virtual reference comes with a virtual epistemic category. This kind of virtual reference is a projection of exiting reference.

Boyd's notion of *epistemic access* becomes extremely important to discuss a projection of reference because epistemic access is more flexible than the membership of any epistemic category. Through epistemic access, the epistemic organization of the world can be, though not necessarily will be, revealed and accordingly discussed. To him, for any particular general term, the question of reference is to be understood as the question: "to which kind (or kinds), or property (or properties), or magnitude (or magnitudes), ... and so on, does our use of this term afford us epistemic access? When we conduct rational inquiry intended to discover facts about the referent of this term, about what kind(s) do we in fact gather information?" (505) That is to say, as long as the epistemic access can be verified, the reference is identified. When it comes to a projection of reference, epistemic access should be considered as *epistemic assumption*, which may or may not be proved as real. If proved, it becomes epistemic access; if not, it should be abandoned.

For instance, Young observed "strong resemblance between the nature of sound and that of light" in terms of wave characteristics, and he conceived that the physical characteristic of light (the unknown) is like wave (the reference), thus light should possess the characteristics of a wave (epistemic access). In his "Experiments and Calculations Relative to Physical Optics," Young described the experiments that he wanted "to lay before the Royal Society," as "so simple and so demonstrative a proof of the general law of the interference of two portions of light" (1), which can only occur to waves. Young projects the characteristic of the reference, wave, into that of light, the unknown. Since the physical characteristic of wave (interference) is well established, as epistemic access of the reference wave, it helps Young frame and speculate the characteristic of light. When it comes to light, the characteristic is an epistemic assumption before Young's experiments prove that light does possess wave characteristics.

Quasi-metaphor

By thinking of a borrowed or coined term as a projection of known reference, including the epistemic category to which the reference belongs, it becomes clear that such terms are a metaphorical kind. First, these borrowed or coined terms fit Black's view of metaphors, that "a metaphorical statement can sometimes generate new knowledge and insight by changing relationships between the things designated," because metaphors function "as cognitive instruments through which their users can achieve novel views of a domain of reference" (35 and 38). In the case of a borrowed or coined term, the user perceives the unknown through a known reference, considers the relation between the two, articulates the probable resemblance that the unknown may bear with the known reference (which is the process to set up the epistemic assumption) and finally frames the unknown in a borrowed or coined term. In Young's case, the wave metaphor provides a new perspective, different from the Newtonian, that eventually produces new knowledge about light.

Second, those terms are not real Aristotelian metaphors, for there is no such Category B with which the Category A can cross (Ref. Chapter 1, Figure 1.1). The substitute for category B is a virtual category to be discovered and then fulfilled, hopefully. Still, categorization is the fundamental way to engage with the natural world, the methodology and system of which was first established by Aristotle. In doing so, the world is made intelligible and thus comprehensible. Even today, it is a basic approach in science, although the methodology and systems have been developed far beyond where Aristotle left them. For instance, the wave characteristic of light is a well-established category today, but for Young, he could only speculate, according to the wave characteristic of water and sound, a virtual category of wave characteristic that light may possess.

Third, these terms are not Lakovian metaphors, either, for the target domain does not even exist yet (Ref. Chapter 1, Figure 1.2). As cognitive instruments, however, they echo the function of Lakovian metaphors. For instance, the Lakovian metaphor that "IDEAS ARE OBJECTS" is a "projection of entity status upon mental phenomena via an ontological metaphor," and "THE MIND IS A CONTAINER" is "a projection of entity status with in-out orientation onto our cognitive faculty." Those are "interactional properties" of ideas and of the mind, which "reflect the way in which we *conceive* of mental phenomena by virtue of metaphor" (*Metaphors We Live by* 214). Similarly, a borrowed or coined term projects the reference with the epistemic access. Take again, the wave theory of light, as an example: Young's conception of light's wave characteristic is based on the knowledge of waves, particularly sound waves in his time. The maturity of wave theory, especially of water and sound, and Young's mastery of the theory are the ontological and mental conditions through which Young can use wave as a quasi-metaphor to think of, or project, the characteristics of light.

Boyd names the borrowed words *catachresis*, which is to introduce theoretical terminology where none previously existed. I propose another name, quasi-metaphor instead, including borrowed and coined terms that refer to what is to be discovered (Figure 2.1). By using quasi-metaphor, I want to emphasize that these terms in scientific research, first of all, function to define and specify the unknown. They may or may not fit in a theoretical frame, especially in empirical sciences. Moreover, quasi-metaphors do possess all of the characteristics of Black's interaction metaphors, and their projection of the known conveys Boyd's theory on reference and epistemic access. I use specifically the term epistemic assumption for quasi-metaphors when they are proposed, for in the history of science, not every quasi-metaphor survives. When epistemic assumption is proved, it is transferred to epistemic access, and the quasi-metaphor becomes a properly scientific term. The users of quasi-metaphors bear in their mind a very specific characteristic as an epistemic assumption of what to be discovered according to the projection of the reference's epistemic access. By keeping the term metaphor in the quasi-metaphor, I want to focus the metaphorical instead of rhetorical function, cognitive instead of linguistic functions of the quasi-metaphors, which, through the efforts of generations of scholars, have been well-established.

Shown in Figure 2.1, quasi-metaphor includes reference with its epistemic access belonging to a source category A, as a representation of the known. Through this source category A, the unknown is conceived as a virtual category A' that projects the source category A, and the correspondences are: reference of the source category corresponds to the projection of reference; the epistemic access corresponds to the epistemic assumptions. Such a virtual category A' may be proved nonexistent through research; or be developed into an existing category B. In the history of science, cases of both kinds have occurred, and one of each will be discussed in later chapters. There are different stages in the development of a virtual category into an existing category, and the resemblances--the specific similarities between epistemic access and epistemic assumption in a quasi-metaphor--are different at different stages. Through quasi- metaphors, the history of science can be examined from a new perspective, in which different narrative, understanding, and criteria of continuity and discontinuity—and especially a different view of scientific revolutions—can be provided.

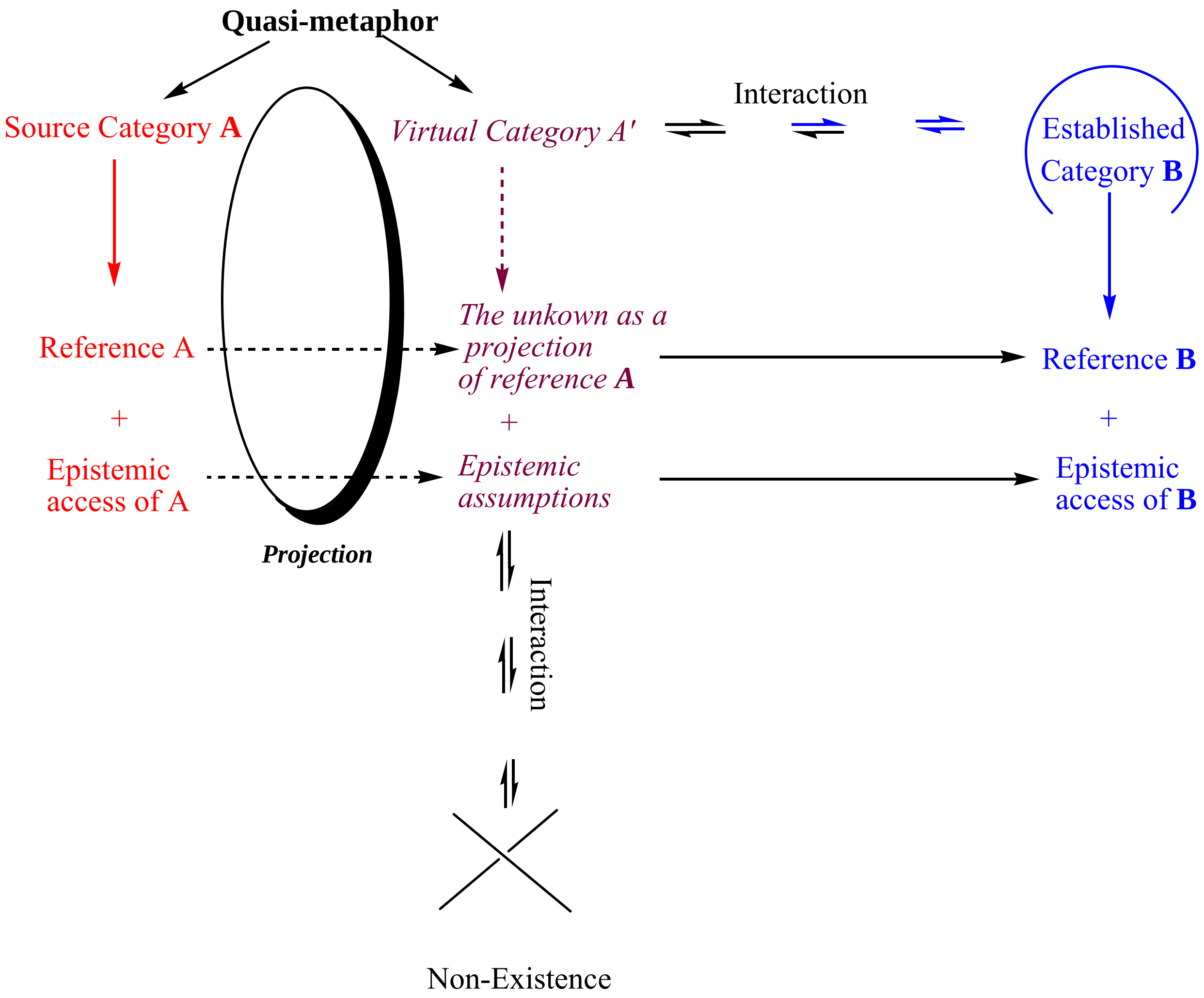

Figure 2-2: Quasi-Metaphor

When Dedre Gentner discusses the course of development of a scientific analogy, she quotes George Polya's advice to those who wish to develop mathematical insight: "And, remember, do not neglect vague analogies. Yet if you wish them respectable, try to clarify them." She then comments that "it is precisely in the process of turning an initially vague, rich, multi-purpose feeling of analogy into a well-clarified model that much of the creative process in science takes place" (128 -129). The development of a quasi-metaphor follows this path, too. Take metaphor studies as an example: At the very early stage, the conceptualization of metaphor is vague: the beginning of metaphor study is an empty stylistic category with a simple definition. At this level, the source category is figurative language, and the assumption is that there is a particular kind of use of words by transferring them, and these words have a particular function by which they may be identified. In Isocrates' words, metaphors are linguistic "ornaments." Then Aristotle provides a much more specific definition, dividing the category into sub-categories and collecting rich examples (species) to fill the sub-categories. He also proposes critical approaches. At this point, metaphor as a category is formally established. Aristotle mainly contextualizes his metaphor study in poetics, so the source category is poetic study, and epistemic access/assumption, poetic criticism. The Roman rhetoricians refine the definition. They add more categories, subcategories, and species into the category, and elaborate more on the nuances and effects of the sub-categories. Metaphor studies owe this to the great development of rhetoric during Roman time. The source category therefore is rhetorical studies, and accordingly, the epistemic assumption, rhetorical effects. The modern turn of metaphor studies is mainly cognitive. Metaphor becomes a concept approached through cognition. The source category becomes cognitive science, and epistemic assumption, the research results of the field.

The evolution of metaphor studies demonstrates that research is fundamentally metaphorical by constantly creating and developing quasi-metaphors. Such activity, on one hand, requires the known to be defined and articulated through precise language; on the other hand, the linguistic reference to the unknown requires vagueness to allow modification. Thus, vagueness is exactly the strength makes it possible for a quasi-metaphor to grow up, as Ricoeur puts it, "a family resemblance first brings individuals together before the rule of a logical class dominates them. Metaphor, a figure of speech, presents in an open fashion, by means of a conflict between identity and difference, the process that, in a covert manner, generates semantic grids by fusion of differences into identity" (234). The "open fashion" that Ricoeur grasps is what Boyd names as "open-endedness," which is the consequence of vagueness or imprecision. The open-endedness allows constant endeavor to pinpoint the precise meaning of the quasi-metaphors. As long as knowledge searching and discussion go on, the language must keep both vagueness and precision simultaneously. This is the very reason why, even Plato, even Bacon, could never get rid of metaphors in their writings. Hence, the view that metaphors should be excluded from scientific prose because of their vagueness is, as in Boyd's words, "an extremely plausible but mistaken understanding of precision in science" (483). Scientific developments root in quasi-metaphors, in the vagueness that leads to endless production of knowledge, which, ironically, is always expressed in precise language. Accordingly, the history of science can be narrated through quasi-metaphors. Although Stephen Hawking never mentions the metaphorical characteristics of universe, time, and gravity, his *The Brief History of Time* can be read as the history of the quasi-metaphors. There are numerous cases, and two particular cases are presented in next chapters.

CHAPTER THREE

The Word PHLOGISTON Had to Die:
A Quasi-metaphors and a Scientific Revolution

> In the final analysis, what Lavoisier had discovered was a word. (Bell 108)

During the years when I was a graduate student of chemistry at the University of Washington, every June in the graduation ceremony of the department, the chair and his assistants entertained the parents and guests with a few experiments, one of which was the reserved explosions of balloons. In particular, two balloons with flashy colors, purple, yellow, or red, filled with hydrogen and oxygen, ideally in the ratio of two to one, were floating above the rostrum, accompanying the speeches by those who were eager to speak and those who did not seem so. At the end, the chair held a torch lit by one of his assistants and approached a balloon. Quickly, a loud sound of explosion filled the room before anyone could see the torch being close to the balloon. While the parents and guests recovering from the shock, the same happened to the second balloon. Then all applauded, which sounded much weaker than the explosions. I always knew that the graduates could not be impressed. As a Teaching Assistant of the introductory classes, I observed the professors also amusing the freshmen with the same demonstration in the first day's class, although only with one balloon, perhaps because it was matriculation instead of graduation, or it was the parents who, more or less, paid for the classes. The sound of the explosion, however, had an unusual effect on the chaotic and noisy classroom holding more than two hundred students at the first day of the quarter. It immediately silenced them. To me, this experiment was first demonstrated on the very first day I started to learn chemistry in my junior high school. It was conducted in a regular test tube, and the sound was weak, but I believed that I caught a glimpse of blue flame, gentle and pure yet so transient.

Until I studied the history of science, I scarcely realized how important the experiment was in the history of chemistry and science—so important that it is considered one of those "crucial experiments," defined as those that decisively prove or disprove a hypothesis or theory. The greatest claim for the experiment is that it pronounced the death of an old theoretical system as well as marks a scientific revolution, the Chemical Revolution:

> …he [Lavoisier] transformed the demonstration of water's composition into a historic national event. On the twenty-fourth of June, 1783, the king, a minster, the English chemist Charles Blagden, and some academicians took their places in front of the combustion apparatus to witness and agree that Laplace and Lavoisier, having turned on the faucets for the two gas reservoirs, collected several drops of water in the tube of a funnel.
> …Lavoisier, who had been given the task of producing inflammable air, was able to give an even more spectacular demonstration of the composition of water, as the result of a big experiment requiring analysis and synthesis that lasted two days. … After twelve years of research that multiplied doubts on the foundations of the chemistry of elements, it was a "drop of water" that extinguished phlogiston. (Bensaude -Vincent and Stengers 86)

Dramatic as it can be, this so-called "the last nail in the coffin of phlogiston" in the whole episode of the Chemical Revolution is merely a ride on a huge wave (Jackson 223). The revolution "has been recognized as revolution by all historians and scientists, just as it was seen to be a revolution in its own time" (Bernard Cohen 236). It has created as much of a bang in histories of science as in the lecture hall and has become a classic nexus attracting key terms like revolution and paradigm shift: it is, as it were, the paradigm of a paradigm shift. Moreover, it happened at the same time as the French Revolution. Its leader, French chemist Antoine-Laurent Lavoisier (1743-1794) was convicted as a traitor (for other reasons) and guillotined. The tragic hero, the interweaving of two revolutions, together with the fact that one of the key elements of the Chemical Revolution, the discovery of oxygen, was not made by Lavoisier but Joseph Priestley (1733-1804), an English chemist, all these make this episode the Hamlet of the history of science--there are a thousand versions of the revolution in a thousand people's eyes. The important and the intriguing thus turn into how to approach the episode rather than the episode itself. Approaching the revolution through quasi-metaphor is rather specific, for the question to be answered is: why did the term *phlogiston* die? The answer, on the other hand, brings up other answers to questions in a broader sense: what was the revolution about? What was revolutionary, if anything? Hence, first of all, the history of phlogiston as a quasi-metaphor should be carefully examined. Then the relation between *phlogiston* and *oxygen* are discussed, especially regarding what is in it for Lavoisier to rename a species of gas *oxygen*? Finally, what is *modern* in the *chemistry* conceived by Lavoisier that made him "the father modern chemistry," as claimed by almost every entry textbook in chemistry nowadays? In this case of metaphorical analysis, I argue that the abandonment of phlogiston is determined by the formation of modern chemistry.

Approaches toward the Chemical Revolution and Lavoisier

To Thomas Kuhn, the Chemical Revolution is one of the major cases that illustrate what he calls as a "paradigm shift," which occurs when "[a]fter the pre-paradigm period the assimilation of all new theories and of almost all new sorts of phenomena has in fact demanded the destruction of a prior paradigm and a consequent conflict between competing schools of scientific thought" (96). He considers the core of the revolution is the "emergence of Lavoisier's oxygen theory of combustion" and the "rejection of the phlogiston theory" (69, 71). Henry Guerlac, on the contrary, is against taking the Chemical Revolution as the overthrow of the Becher-Stahl phlogistic theory. Such an approach, as far as he is concerned, "says at once too much and too little," because first, it over-emphasizes the break from the past; second, it neglects much of the accumulated body of knowledge that was absorbed by the newer chemistry; and third, it overlooks that something more fundamental has occurred during that period than the mere substitution of one theory of combustion for another, certainly important though the latter proved to be (xvii). Furthermore, Frederic Holmes argues that Priestley's conceptualization of phlogiston was indeed so loosely connected with the older phlogiston theories descended from Stahl that the events at the heart of the Chemical Revolution should be viewed more as a competition between two rival new research programs than as the replacement of a reigning paradigm (735).

Arthur Donovan summarizes that there are basically two approaches towards Lavoisier and the revolution. One focuses on Lavoisier through detailed and carefully documented studies in order to reconstruct Lavoisier's career as a chemist and the progress of the revolution he led; the other attempts to locate Lavoisier's achievement in the larger context of the development of science in the latter half of the eighteenth century. In the contemporary studies with respect to Lavoisier, he considers that there is a serious inadequacy in studies that link the two approaches (214-215). Holmes agrees with him and adds that there is also a serious inadequacy in our contemporary understanding of eighteenth century chemistry (*Eighteenth-century* 103). In his establishment of such a link, Donovan emphasizes again that "Lavoisier himself conceived of his revolution as one that brought chemistry into science, rather than as a revolution in science" (215). The view that Chemical Revolution is a revolution in science focuses on the continuity in science by regarding the revolution as one more step forward in scientific progress; whereas Donovan's argument emphasizes the dramatic change in many aspects of chemistry that finally made it possible to distinguish chemistry from alchemy after the revolution.

Quasi-metaphor offers a new perspective. Max Planck proposed in 1912 that the subject of physics history should be the faith in the unitary intelligibility of the world, which prompts "the physicist" instead of this or that particular individual physicist (Bensaude -Vincent and Stengers 45 – 46, 272). By such historiography, a scientist, or more precisely, the achievement of a scientist, is defined by the difference between the known and unknown before and after his discovery or theory—how, in other words, his achievement refined the known and the unknown of his time. Quasi-metaphors are poised right between the intelligibility of the world and the unknown. Their epistemic assumptions are the projection of specific intelligibility at given historical moments. The evolution of a quasi-metaphor is the process of transferring epistemic assumption to access, namely, redrawing the boundary between the known and the unknown. Tracking the history of a quasi -metaphor thus is the articulation of its knowledge production, or, the intelligibility contributed by the scientist. Moreover, while the transformation of epistemic assumption to access can be done as "detailed and carefully documented studies" regarding any scientist's career, such as that of Lavoisier, the relations of quasi-metaphors, like that of phlogiston, oxygen, alchemy and (modern) chemistry, can contextualize Lavoisier's achievement in the development of science.

Quasi-metaphor and Paradigm

Studying the history of science through quasi-metaphors shares the same orientation with Kuhn's paradigm, which is to study the well-established through its process of changing. Kuhn is mainly interested in the cases in which the well-established is overthrown. To him, the pre-condition of change is breakdown. Quasi-metaphors can illustrate Kuhn's cases, like phlogiston; they also can be continuous developments of a term, such as the gene, that goes through various changes with respect to its epistemic assumptions until it is turned into access. Hence to me, the precondition of change is scientific data suggesting new understanding, and this data could but not necessarily would result in a breakdown.

One problem of Kuhn's paradigm is its meaning. As Masterman observes, Kuhn uses *paradigm* in no less than twenty-one different meanings:

(1) As a universally recognized scientific achievement.
(2) As a myth.
(3) As a 'philosophy', or constellation of questions.
(4) As a textbook, or classic work.
(5) As a whole tradition, and in some sense, as a model.
(6) As a scientific achievement.
(7) As an analogy.
(8) As a successful metaphysical speculation.
(9) As an accepted device in common law.
(10) As a source of tools.
(11) As a standard illustration.
(12) As a device, or type of instrumentation.
(13) As an anomalous pack of cards.
(14) As a machine-tool factory.
(15) As a gestalt figure which can be seen two ways.
(16) As a set of political institutions.
(17) As a 'standard' applied to quasi-metaphysics.
(18) As an organizing principle which can govern perception itself.
(19) As a general epistemological viewpoint.
(20) As a new way of seeing.
(21) As something which defines a broad sweep of reality.

Masterman further divides these definitions into three groups: a group of "metaphysical paradigms or metaparadigms" that refer to a metaphysical notion or entity, as (2), (3), (8), (17), (18), (19), (20), (21); a group of "sociological paradigms" with sociological sense, as (1), (6), (9), (16); and a group of "artifact paradigms or construct paradigms" that operate in a more concrete way, as (4), (7), (10), (12), (13), (14), (15), each of which operates differently in the history and historiography of science. In her attempt to reconcile these definitions, Masterman suggests that "a paradigm draws a crude analogy," which has three logical characteristics:

(a) a crude analogy is finite in extensibility.
(b) it is incomparable with any other crude analogy.

> (c) it is extensible only by an inferential process of ‘replication’, which can be examined by using the computer programming technique of ‘inexact matching’, but not by normal methods of examining inference. (79)

She remarks that the problem of saying something philosophical and exact about a paradigm is the same problem Max Black has when he formalize the ”interaction view” of metaphor used in language, and the new “way of seeing” produced by Black’s metaphorical interaction is an alternative form of Kuhn’s “gestalt-switch” (80). To her, the shift of paradigm could indeed be perceived as a shift of resemblance and dissemblance, or the dynamics of the tension in a metaphor. Paradigm shift happens when the finite extensibility is exceeded.

As discussed in Chapter Two, however, the important feature of the interaction view is the open-endedness of metaphors. If a paradigm is roughly an analogy, it should possess infinite rather than finite extensibility. Paradigms, then, should not shift. While claiming that “Kuhn’s own account of the limits and extensibility of a paradigm is both sketchy and faulty, for which fact he himself apologizes,” she admits that her own logical elaboration about paradigm-extensibility failed, which leads her to ask, in view of obvious difficulties of handling such an entity as Kuhn’s crude paradigm, “what happens if we drop his whole paradigm idea?” If yes, then “we run the risk of totally disconnecting the new-style realistic history of science from its old-style philosophy: a disaster” (88). So, she does not provide a solution.

In quasi-metaphor theory, the extensibility of a quasi-metaphor is determined by its own epistemic assumptions. If experimental data keep turning out to be consistent with the assumptions, a quasi-metaphor can keep extending; otherwise, it must be abandoned.

When Kuhn parallels scientific revolutions with social revolutions, he creates another problem, which is how to explain the drive of a scientific revolution. Masterman attempts to solve this problem, but she cannot. Kuhn suggests that the cause of the crisis prior to the scientific revolution is the "malfunction" of the scientific community. Accordingly, a scientific revolution is the revolution of scientific community, as he states:

> This genetic aspect of the parallel between political and scientific development should no longer be open to doubt. The parallel has, however, a second and more profound aspect upon which the significance of the first depends. Political revolutions aim to change political institutions in ways that those institutions themselves prohibit. Their success therefore necessitates the partial relinquishment of one set of institutions in favor of another, and in the interim, society is not fully governed by institutions at all. Initially it is crisis alone that attenuates the role of political institutions as we have already seen it attenuate the role of paradigms. …
>
> The remainder of this essay aims to demonstrate that the historical study of paradigm change reveals very similar characteristics in the evolution of the sciences. Like the choice between competing political institutions, that between competing paradigms proves to be a choice between incompatible modes of community life. (*The Structure* 93)

The drive of paradigm shift, to Kuhn, is the choice of the scientific community at given time between competing "modes of community life." He implies that *oxygen* is called *oxygen* because Lavoisier is a better politician than Priestley, with which some scholars agree, but from which quasi-metaphor draws different conclusion: as said above, the drive is the inconsistency between epistemic assumptions of quasi-metaphors and experimental data. When the reliability of the experiments can be proved, then fate of a quasi-metaphor is determined by the data. Compared with paradigm, the particular strength of quasi-metaphor in the studies of the history of science is that it can preserve, as well as articulate, continuity in the development of science. Through quasi-metaphors, scientific revolutions can be perceived as either replacement of one quasi-metaphor by the other or the validation of a quasi-metaphor. Thus, both continuity and novelty can be studied. In doing so, the shortcomings pointed out by Guerlac in the studies of Lavoisier and the Chemical Revolution, in which the break with the past is exaggerated, and the assimilation of knowledge and practice is neglected, can be avoided. Through quasi-metaphor, the history of eighteen-century chemistry will not be narrated as “waiting for Lavoisier to arrive on the scene;” or, “according to the punctuation of before Lavoisier and after Lavoisier” (Bensaude -Vincent and Stengers 46 - 47).

The Conception, Productivity and Problems of Phlogiston

Mostly in the studies of the Chemical Revolution, it is ignored that there are a few revolutions prior to it, and phlogiston theory itself indeed is a revolution. In 450 B. C., Greek philosopher Empedocles initiated the idea of four elements: fire, earth, air, and water. Aristotle further defined the four elements with qualities: fire is hot and dry; water, cold and wet; air, hot and wet; earth, cold and dry. The element of fire possesses absolute levity, which later was considered as the levity of phlogiston. Moreover, according to *Anonymus Londienesis*[14], Aristotle also introduced the idea that, when the soul leaves a body, the latter becomes heavier, since the soul tends upwards (Partington and McKie 2). This idea later comes in quite handy in the defense of phlogiston. Aristotle had had influence on phlogiston, as well as science, so profound that in 1624, almost two thousand years later, a law was passed in France to compel the chemists of the Sorbonne to conform to the teaching of Aristotle on pain of death and confiscation of goods. However, this was a last-ditch stand. “Adherence to the teachings of Aristotle hindered progress in science for about 2000 years” (Jones 147).

Paracelsus, the best known of the sixteen-century chemists, discarded the four-element theory in favor of his *tria prima* of mercury, sulfur, and salt, “the primary bodies of the alchemical tradition” (Bell 66). Paracelsian alchemy also had a frantic struggle with traditional Galenic medicine. It is not surprising that Paracelsian alchemy proclaimed itself revolutionary,

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[14] Perhaps a student’s lecture notes, incorporating material from Menon, a pupil of Aristotle in the Empiric School founded by Serapion of Alexandria in first half of the second century B. C..

and was thought to be so. In the article “Chymie” appeared in Diderot’s *Enclopédie*, the eighteen century French chemist Gabriel François Venel (1723–1775) called for a “new Paracelsus” to elevate chemistry to the rank that she deserved among the sciences (Bensaude - Vincent and Stengers 24 – 25).

Jan Baptista van Helmont (1577-1640), a Paracelsian disciple, modified the Paracelsian doctrine into a theory in which water was primordial substance, the fundamental principle and the base of material transmutations.

Johann Becher (1635-1682), who was praised as “a German metallurgist of great sagacity, and perfectly acquainted with all the chymical facts then extant,” developed Paracelsian ideas in a different direction than van Helmont (Kirwan 2). He replaced the *tria prima* with air, earth, and fire, but considered air as an agent or instrument for chemical mixtures rather than an element. To Becher, all solid substances consisted of three types of earth: *terra fluida* was the mercurial, which determined fluidity and volatility; *terra lapida* was the solid, which contributed to fusibility or the binding quality; and *terra pinguis* was fatty earth, which provided material substance with its oily and combustible qualities. *Terra pinguis* was the principle of inflammability: a piece of wood originally consisted of ash and *terra pinguis*; when the wood is was burnt, the *terra pinguis* was released, and the ash, left (Strathern 206).

Becher’s pupil, the “celebrated” Georg Ernest Stahl (1660-1734), modified Becher’s theory. Becher’s *Physica Substerranea* circulated widely throughout Europe, and went through several editions. By 1703 the third edition was being prepared in Germany by Stahl, a professor of medicine at the newly founded University of Halle. Stahl wrote an introduction, in which he coined the word *phlogiston* (setting on fire) from a Greek word *phlogios* (fiery) to replace Becher’s *terra pinguis* (Strathern 207 -208). Stahl assumed that air was chemically inert, incapable of entering into chemical combinations. It could, on the other hand, absorb limited amount of phlogiston, and phlogiston was absorbed by plants, which were eaten by animals. Thus, there was a phlogiston cycle in Nature, and phlogiston was the link among the three kingdoms of Nature (Brock 83).

Since Aristotle, there had always been attempts to understand the irreducible elements[15] constituting the material universe; water, air and fire were thought to be elements. By the early eighteenth-century, however, the general accepted doctrine regarding them was less the elemental than the instrumental, in which they were understood as agents rather than components, of physical change. Becher and Stahl went a step further to treat fire as a phenomenon with an unknown cause that Becher names *Terra pinguis*, and Stahl, phlogiston. Both Becher and Stahl's quasi-metaphors had the same reference, as well as the same epistemic assumption that combustibility was a material released during combusting. Yet Stahl did not agree with Becher with respect to the virtual category that Becher assigned to *Terra pinguis*, in which *Terra pinguis* was considered as an element the same as the other two terra-elements. Stahl's idea that phlogiston linked the air, plants and animals suggested his thinking about phlogiston had gone beyond fire and elements, beyond materials that could be seen.

Stahl's conception of phlogiston set off a search for the material. Chemists had very various candidates. Phlogiston has been regarded as elementary fire, as the matter of heat, as the matter of light. Phlogiston as elementary fire was soon dismissed because it was inadequate—no one really knew what elementary fire was. Chemists had to make a choice between heat and light, for they observed that combustion released both. Phlogiston thus was thought by some

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[15] Element is also a fundamental quasi-metaphor, and I decided to keep the study of its evolution out of my dissertation.

chemists to be a compound of the matters of heat and light; when uncombined it was fire or free matter of light and matter of heat. On the other hand, in order to circulate in the three kingdoms, phlogiston could be the matter of heat contained in cells or interstitial spaces in the combustible body and set free when these cells were broken; or as a component intimately and chemically combined with the remaining parts of the combustible body that were of a different nature and dissolved by them (Partington and McKie 12). Those speculations were so confusing and even contradictory that it was impossible to give phlogiston a clear and unified definition. The lecture notes of Professor Samuel Williams, the Hollis Professor of Mathematics and Natural Philosophy from 1780 to 1788 at Harvard, present a phlogiston theory to a class in these words:

> By Phlogiston we mean no more than the principle of Inflammability; or that by which bodies become combustible or capable of burning.—And that there is such a principle or element as Phlogiston, and that common Air may be charged with large quantities of it may be easily represented (Conant 15).

A principle and an element are logically incompatible. If phlogiston was an element, than it could never be a principle, and *vice versa*. By listing *principle* and *element* simultaneously as the possible candidates of phlogiston, Professor Williams hinted his serious doubts on the idea of phlogiston as an element. Carleton Perrin points out, "[i]n essence, the doctrine of Becher and Stahl embodied the defining properties in a set of hypothetical, quality-bearing material 'principles'" (266). The set of principles, such as "vegetation absorbed phlogiston from atmospheric air, dephlogisticated air was produced from all kinds of earth mixed with spirit of nitre, and inflammable air was pure phlogiston," indicated the precise knowledge with respect to pneumatics and botany, or part of the intelligibility of the world then (Barrotta 165). The phlogiston theory thus proposed not only the existence of phlogiston but also a stable epistemic category in which phlogiston fitted well.

Phlogiston as a quasi-metaphor functioned as research guidance. Although the outcome seemed somewhat chaotic, it was productive. Chemistry became "a recognizable discipline with the beginnings of a uniform methodology and of a widely accepted body of theoretical ideas" (Coley 65). Under this discipline, pneumatic chemistry was able to develop. The race to discover different airs started across all of Europe, from Uppsala to Paris, London to Berlin. As a consequence, European chemists created a social network through exchange of correspondence, visits, and journals. For instance, German chemist Lorentz Crell's (1744 -1816) *Chemische Annalen* solicited contributions from German chemists, as well as others from all over Europe. It indeed united chemists of different status: Henry Cavendish (1731 - 1810), like Boyle, was a wealthy member of British nobility; Lavoisier was an academician and administrator who equipped his laboratory at his own expense; Priestley was an English Unitarian minister to whom a noble protector had donated a laboratory; German-Swedish Karl-Wilhelm Scheele (1742-1786) was a self-taught apothecary who worked in poverty and obscurity without institutional affiliation and who wandered from Stockholm to Uppsala and from Uppsala to Köping, where he finally bought an apothecary (Bensaude -Vincent and Stengers 79). Even Lavoisier, the one who cast out the phlogiston theory, commented that, with phlogiston, "for the first time in the history of chemistry, a theory was embodied in the facts it aimed to explain" (Bell 50). There had been a time when many European chemists began to consider that phlogiston theory might be the answer to all chemical changes. During the fifty years following Stahl's death, phlogiston theory was accepted by the most respected European chemists and became dominant (Wisniak 733).

One of Stahl's great contributions was the establishment that the corrosion of metal and the burning of wood or charcoal are both related to combustion. In other words, corrosion was a process of slow burning. This discovery modified the commonsense category of combustion by the knowledge that combustion did not necessarily come with immediate and intensive release of heat and light, and most importantly, fire is merely one of the phenomena related to combustion. In 1787, Immanuel Kant applauds this discovery as one of the major landmarks of scientific development: "When Galileo caused balls, the weights of which he had himself previously determined, to roll down an inclined plane; when Torricelli made the air carry a weight which he had calculated beforehand to be equal to that of a definitive volume of water; when Stahl changed metals into oxides, and oxides back into metals, by withdrawing something and then restoring it, a light broke upon all students of nature" (108 -109). As a significant development of Stahl's category, this discovery implied that the research was on the right track, especially when phlogiston has not yet been found.

Yet another phenomenon related to the reactions of metals and air produced what Kuhn calls the crisis of phlogiston theory, which led to the complete abandonment of phlogiston. According to Stahl's theory, when metals were heated, phlogiston was released and calce was formed. The calce then should weigh less than the metal because by Newton's gravity theory, weight means quantity of materials, and the release of a material should be accompanied by weight loss. As the accurate balance became widely used, it was demonstrable that metals gained weight when heated. Weight-gain of heated metals became a problem only when Newtonian theory was well accepted and only for phlogiston theory. Phlogistonists passionately pursued the resolution. Guyton de Morveau (1737-1816) hypothesized that, because phlogiston was lighter than the air, its presence in a substance should make that substance lighter (Bensaude -Vincent and Stengers 82). Venel considered that phlogiston should have negative weight, as he stated in his lecture of chemistry at Montpellier, "[p]hlogiston is not attracted towards the center of the earth, but tends to rise ; thence comes the increase of weight in the formation of metallic calces and the diminution in weight in their reduction" (Partington and McKie 380). Several phlogistonists harked back to Aristotelianism and ascribed the Aristotelian term *levity*, meaning negative weight, to phlogiston. Scheele, Priestley, Cavendish and Kirwan thought matter with negative weight was ridiculous, and refused to be associated with this idea (Musgrave 189).

The Discovery of Oxygen

If phlogiston could be isolated, the weight-gain issue should not be difficult to work out. According to the theory, the air was the most accessible material from which phlogiston could be extracted. The studies of air under the guidance of phlogiston theory were fruitful. Before 1750, it was not recognized that actual air was a mixture, and the general belief was that "the atmosphere might be loaded—more in some places than in others—with more or less obnoxious alien effluvia" (Butterfield 213). Historians customarily labeled the air studies of Great Britain as "pneumatic chemistry," participated in mainly by those who were not identified principally as chemists. They employed commonly known chemical operations and apparatus in their experiments, but they were not deeply experienced in the chemistry of salts that characterized the forefront of chemical investigation on the continent. Stephen Hales (1667- 1761) was often acknowledged as the founder of pneumatic chemistry. As a Newtonian natural philosopher, he included chemical operations among a broader spectrum of phenomena that he examined by "weight and measure," (Holmes, *The Revolution* 738). His book *Vegetable Staticks*, in which he applied Newtonian principle to plant life, basically changed the view of air in the eighteenth-century. Particularly based on Newton's discussions of the attractive and repulsive forces between particles of matter, he defined the concept "fixed air" as air of "unelastic" state but which could resume its elastic state when released. Violent and fatal effects of very noxious vapors on the respiration and life of animals was due to the loss of the air's elasticity. Joseph Black (1728 – 1799), in his Ph. D dissertation published in 1756, wrote about his discovery of the "fixed air" (carbon dioxide), which is unbreathable and did not enter into combustion, fermentation, or respiration. Cavendish isolated and defined "inflammable air" (hydrogen) given off by metals as they reacted with acid in 1766.

Priestley, whom some historians view as a physicist, some as a philosopher and theologian, engaged with air study since 1760, and wrote a series of books named *Observations and Experiments on Different Kinds of Air*. In March 1772, Priestley presented his research on fixed air in a lecture to the Royal Society, which was published in his *Directions for Impregnating Water with Fixed Air* in July 1772 and was forwarded to Lavoisier within a week. On July 18, Lavoisier presented Priestley's work to the Academy.

Lavoisier himself also had worked intensively on fixed air, for he thought fixed air could be the key to solve the weight-gain problem. Lavoisier adapted Hales' apparatus and used it to do experiments on *minium* (lead oxide) (Figure 3-1). His design made it sure that the experimental system was isolated, especially from the atmosphere, such that nothing could be incorporated into or released out of the system. In so doing, combustion happened with only the presence of air enclosed in the glass jar. In what historians regard as Lavoisier's "Easter memoir" of 1772, as the first public announcement of his theory in the title "On a New Theory of the Calcination and Reduction of Metallic Substances, and on the Cause of the Augmentation of Weight That They Acquire by the Fire, Whether through Calcination or Through Other Analogous Processes," he reported his observation:

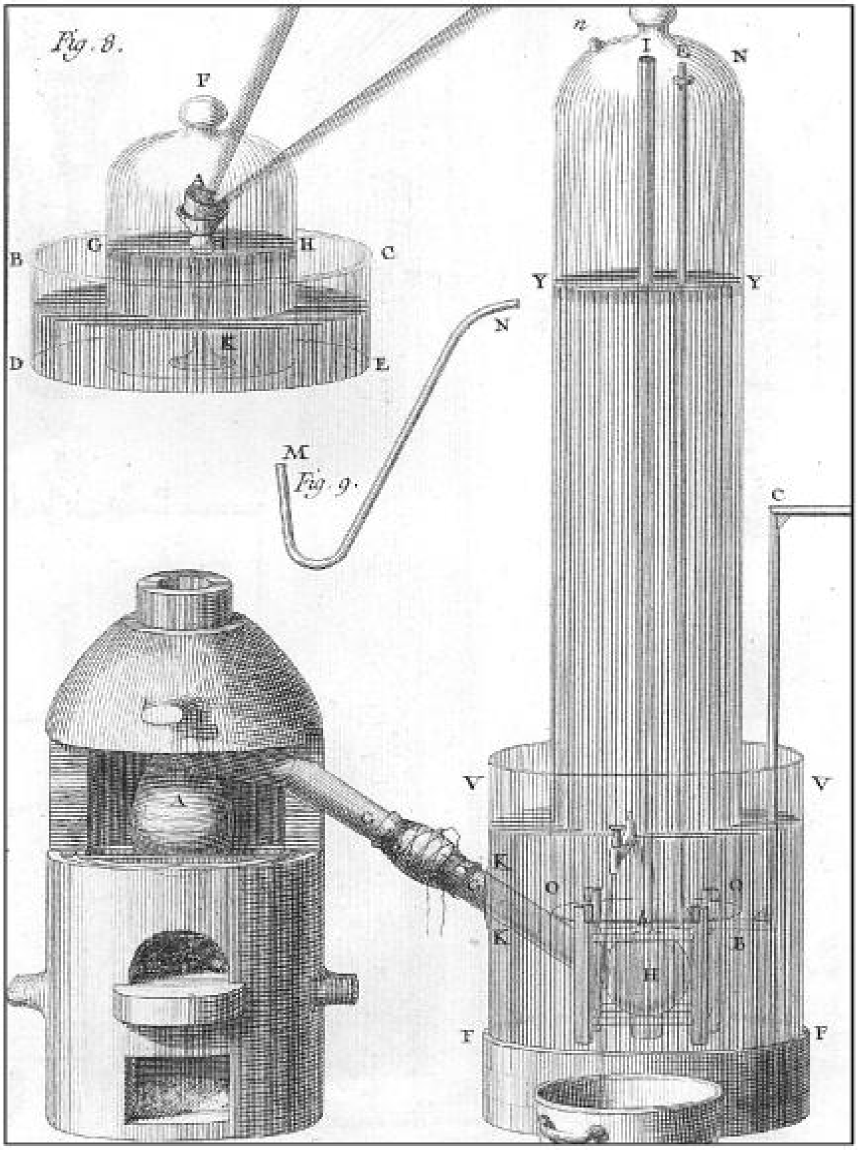

Figure 3-3: Lavoisier's apparatus for his experiments on minium, as drawn by Madame Lavoisier. Upper left, retort fashioned from four pieces of iron. Lower and right, retort in place in furnace, and connected to modified apparatus of Hales. (Holmes, *The Evolution*, 142)

> [T]hese metals are reduced to a calx[16] the volume of the air diminishes, and the augmentation of the weight of the metal is found to be almost equal to the quantity of the air absorbed. … one is able to achieve the reduction of these metals, that is, to cause them to change from the state of the calx to that of metal, they immediately restore all the air that they had absorbed and lost at the same time all the augmentation is weight that they had acquired. (Holmes, *Antoine Lavoisier* 32)

In this passage, not only Lavoisier's "vaunted balance sheet method suddenly emerge[s] in its full glory," as Holmes marks, but also Lavoisier's confidence that weight-gain problem was nothing but the combination of the metals with fixed air had been established (*Antoine Lavoisier* 33). He concluded that "[i]t obviously results from these experiments, 1st that a metallic calx is nothing other than the metal itself combined with fixed air, 2nd that the metallic reduction consists only of the disengagement of the air from metallic calces. 3rd that the metals owe the weight gain to the fixed air contained in the atmosphere" (*Antoine Lavoisier* 35). At this point, Lavoisier did not know the role that charcoal played in his experimental system as we do today, although he noticed the experimental data with and without charcoal added were different, and the inconsistency made him feel uneasy. Nevertheless, he fired his opening shot across the bows of Stahl's phlogiston-based chemistry:

> This theory is destructive to that of Stahl adopted by almost all chemists, and the circumstance would be suitable to put me on guard; however, I could not refuse the evidence; above all decisive experiments have assured me that it is possible to reduce almost all metals without the addition of phlogiston.
> …
> Finally, all the experiments that I have made on this subject lead me to believe that almost all of the phenomena that one attributes to phlogiston derive only from the absence of fixed air, and reciprocally, as I have shown for the metals.
> I have even come to the point of doubting if that which Stahl calls phlogiston exists, at least in the sense that he gives to that word, and it seems to me that in every case one could substitute the name of matter of fire, or light, and of heat. (Holmes, *Antoine Lavoisier* 35-37)

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[16] *Calx* are named *oxide* in modern chemistry.

Lavoisier denied the existence of phlogiston, and he emphasized that his rejection was on Sthal's definition of the term. Or more specifically, he thought he had proved that the epistemic assumption of phlogiston that combustibility was material was wrong. Rather, combustion was reactions of materials with fixed air. This specificity in Lavoisier's statement implies that Lavoisier was aware of the revisions on the term phlogiston, and some could explain his experiments.

Priestley, on the other hand, systematically worked on reactions in a mercury trough and collected the gases released, and he obtained a gas by reducing a calx of mercury. During a trip to Paris in October 1774, Priestley dined at Lavoisier's home with other French chemists. As Priestley recalled six years later, he talked about his experiments of the reduction of mercury calx and the gas product, "saying that it was a kind of air in which a candle burned much better than in common air, but that I had not yet given it a name. At this, all the company, Mr. and Mrs. Lavoisier included, expressed great surprise" (Bell, 98). Lavoisier and other French chemists indeed had done the same experiment; however, they thought the gas was fixed air. In other words, they did not realize that the gases produced, with and without added charcoal, were two different spices. Priestley's candle experiment separated them. Priestley first called the gas "*mercurius percipitatus per se*," and later, "dephlogisticated air."

In parallel with Priestley, German-Swedish apothecary Karl-Wilhelm Scheele also produced *Feuerluft*, "fire air," by at least four chemical reactions, most likely in 1771. He demonstrated that the atmosphere air consisted of fire air. Fire air was thought to be capable of absorbing the phlogiston from bodies like iron filings, which were rich with phlogiston, and its union with phlogiston allowed it to escape from the container leaving only *Verdobeneluft*, the corrupt, vitiated air in which mice could not survive. Scheele claimed that he wrote Lavoisier a letter in September 30th, 1774 to express his gratitude, for Lavoisier had sent him a copy of Lavoisier's book; Scheele described in the same letter the ways of preparing fire air. Lavoisier never replied and later denied having seen the letter, which would have established Scheele as the true discoverer of oxygen (Severinghaus 13)[17].

To conduct *mercurius calcinatus*, Lavoisier redesigned his apparatus, which "as simple as it is, is all the more exact, because there is neither solder nor lutes, nor any passages across which air can be introduced or escape" (Holmes, *The Evolution* 145). (Figure 3-2) Such design is good for both qualitative and quantitative analysis. The gas produced in the reduction of *mercurius calcinatus* with charcoal could dissolve in water, precipitate limewater, extinguish

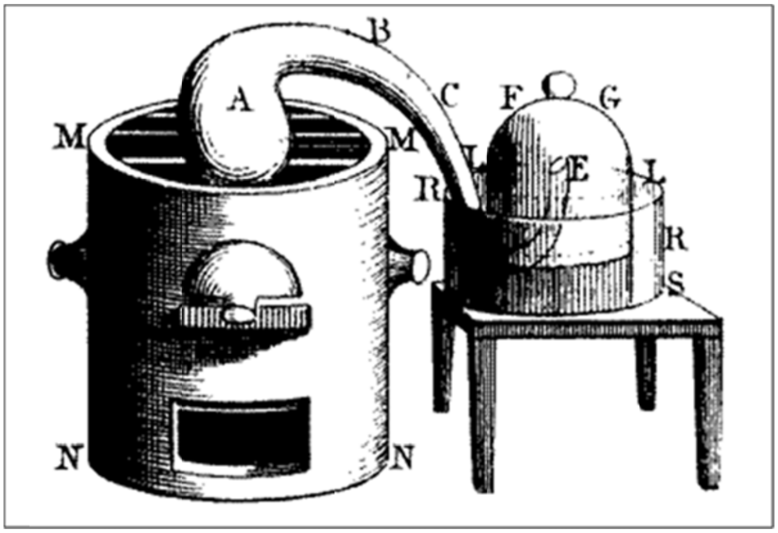

Figure 3-4: Lavoisier's apparatus for calcination and reduction of mercury. (Holmes, *The Evolution* 145)

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[17] The existence of the letter was uncertain until 1992 when the original letter came to light in a donation to the Archives de l'Acadèmie des Sciences, Lavoisier Collection, in Paris from the holders of Mme Lavoisier's artifacts (Severinghaus 17).

candle flames, and suffocate mice and birds; it was clearly the “fixed air” obtained previously by Black and Priestley. On the contrary, the gas produced without the presence of charcoal did not dissolve in water or precipitate limewater, yet supported respiration; it was “dephlogisticated air” of Priestley and “fire air” of Scheele.

On September 5, 1777, Lavoisier presented a paper to the Academy, in which he devoted a paragraph to enunciate his terminology, “I shall hence forward designated dephlogisticated air or eminently respirable air in the state of combination and fixity, by the name of acidifying principle, or if one like better the same meaning in a Greek word, by that of *le principe oxygine*” (Bell 108).

In 1789, R. Kirwan published *An Essay on Phlogiston and the Constitution of Acids* in the defense of phlogiston. He called Lavoisier’s theory as “Antiphlogistic hypothesis” that “reversed the ancient hypothesis.” According to “numerous and ingenious experiments of Dr. Priestley,” “inflammable air, before its extrication from the bodies in which it exists in a concrete state, was the very substance to which all the characters and properties of the phlogiston of the ancient chymists actually belonged” (5). With respect to “many strong prejudices…favour the new opinion,” Kirwan seemed to forget that Stahl’s theory was itself a revolution, because his defense came to a defense of the origin instead of rationality of the theory, as he said “[i]t is to Germany that all modern nations must resort, to improve in mineralogy and metallurgy, as the ancient did to Greece to improve oratory” (8). Nevertheless, the part of his defense based on Priestley’s experiments is strong. Holmes’ research and reinterpretation of Priestley works indicates that, as his experiments and thinking progress, Priestley was moving “further and further beyond the traditional definition of phlogiston as the inflammable principle” (*The* *evlution* 750). In other words, Priestley, too, is working toward a new theoretical frame, a reformed phlogiston theory with the epistemic assumption revised.

Lavoisier's establishment of modern chemistry, however, did not leave room for the revision. Until today, the question is still debated: who did discover oxygen first? Scheele, who first isolated it? Or Priestley, who first defined its properties? Or Lavoisier, who first named it oxygen? Kuhn argues that this question is misleading because the word "discover" has the connotation that "discovering something is a single simple act" often thought to be "unequivocally attributable to an individual and to a moment in time" (55). Thus, even if Scheele is ignored, neither Priestley nor Lavoisier can be the answer, because in such a complicated event, observations and theoretical conceptualization of the observations, facts and the hierarchy of the facts regarding the intelligibility of the Nature cannot be united within an individual scientist's research at a certain historical moment. Pierluigi Barrotta, on the other hand, thinks that the question is sensible and legitimate as soon as the ontological level appropriate to the give situation is identified. If one believes that the appropriate ontology is given by pneumatic chemistry, then oxygen is Priestley's discovery; if one accepts the ontology determined by Lavoisier's natural interpretation, then oxygen is Lavoisier's. Alan Musgrave thinks such a question is naïve because it is asking "an all-or-nothing affair," which is not the case of the discovery. To him, no one really discovered oxygen, for no one got it totally correct: Scheele thought what he got was fire air; Priestley, dephlogisticated air; not even Lavoisier, who thought what he got was "an igneous combination of an 'oxygen base' and caloric." He claimed that, with regard to the answer to who is the discoverer of oxygen, "I do not know and I do not care" (195). If viewed through a quasi-metaphorical perspective, such a question is indeed a question impossible to ask. The discovery itself is a species of gas. To Scheele and Priestley, it is a member belonging to the phlogiston category, a relative of phlogiston. To Lavoisier, it is a solid base on which he conceives a new quasi-metaphor thus a new category, which is "what-was-soon-to-be-called the 'New French Chemistry'" (Bell, 109), or modern chemistry. The power of Lavoisier's establishment is that today, anyone who wants to talk about the species of gas that supports aspiration and combustion has to use the word *oxygen* to refer to it. By using *oxygen*, one is already in the intelligible world constructed on Lavoisier's conceptualization of chemistry, though modifying it in certain respects. That is to say, anyone who asks "who did discover oxygen?" is already on Lavoisier's side, so the answer is not important at all. On the other hand, if one asks "who did discover dephlogisticated air?" Or, "who discover fire air?" Then one cannot get an answer, because no one, except some historians and philosophers of science, and you, my reader, knows to what dephlogisticated air or fire air refers. Hence, who discovers the gas is not important compared with what the gas has to do with the intelligibility of the world. Guerlac comments that what Lavoisier discovered regarding combustion theory was "the role of oxygen" instead oxygen itself (xiii).

Nevertheless, the puzzling factor is, in Bell words, "what Lavoisier had discovered was a word" (108). Why the contemporaries of Lavoisier and Priestly had taken Lavoisier's oxygen over Priestley's dephlogisticated air despite the fact that it was Priestley who discovered dephlogisticated air? Lavoisier's combustion theory was not flawless, either. Hasok Chang's study of phlogiston suggests that "[w]hat Lavoisier considered the most definitive characteristics of oxygen are not regarded as characteristics of oxygen at all by modern chemists" (*The Persistence* 415). More precisely, Lavoisier's *oxygen* consisted of *oxygen base* and *caloric.* To

Lavoisier, *Oxygen base* was the principle of acidity[18], and acids were made through the composition of oxygen. *Caloric* was the material fluid of heat, with an even stronger affinity for combustible substances. When oxygen base unites with a combustible substance, it releases the caloric (and light) that it was previously combined with. According to modern chemistry, however, I do not consider *Oxygen base* is a flaw. Until John Dalton (1766-1844) developed atomic theory[19], in which the difference of oxygen the atom and oxygen the gas as elementary substance[20] can be articulated, Lavoisier's terms at least differentiated them. *Oxygen base* as the principle of acidity was limited but not totally wrong, either. For instance, the combustion of sulfur with oxygen yields sulfur dioxide (SO_2) and sulfur trioxide (SO_3); when these two react with water, then sulfurous acid (H_2SO_3) and sulfuric acid (H_2SO_4)[21], as well as much heat, are produced. Carbon and phosphorus have the similar processes, in which oxidation is part of acidification. The oxygen in those acids was not the same as oxygen the gas, which was made clear by Lavoisier. Yet this is not the only process of acidification, not, for example, for acids like hydrochloric acid (HCl).

In retrospect, caloric is the troublesome concept of Lavoisier's, and just like phlogiston, this term was eventually abandoned. In his "Table of Substances," Lavoisier defined caloric as "Heat, Principle of element of heat, Fire, Igneous fluid. Matter of fire and of heat." Similar to phlogiston, the epistemic assumption of caloric is that heat is material as one of the basic material units, an element. Also similar to phlogiston, when it was proved that heat was not a

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[18] In Bell's translation, Lavoisier's word was "acidifying." (Ref. page 17)

[19] Atom again is a fundamental metaphor.

[20] That is the different between O_2 as oxygen the gas and O oxygen the element in modern chemistry.

[21] The two oxides, (SO_2 and SO_3) and the two acids (H_2SO_3 and H_2SO_4) could not be distinguished until Scottish chemist Thomas Thomson's (1773-1852) *A System of Chemistry of Inorganic Bodies* published in 1831 (270).

	Noms nouveaux.	*Noms anciens correſpondans.*
Subſtances ſimples qui appartiennent aux trois règnes & qu'on peut regarder comme les élémens des corps.	Lumière	Lumière.
	Calorique	Chaleur. Principe de la chaleur. Fluide igné. Feu. Matière du feu & de la chaleur.
	Oxygène	Air déphlogiſtiqué. Air empiréal. Air vital. Baſe de l'air vital.
	Azote	Gaz phlogiſtiqué. Moſete. Baſe de la moſete.
	Hydrogène	Gaz inflammable. Baſe du gaz inflammable.
Subſtances ſimples non métalliques oxidables & acidifiables.	Soufre	Soufre.
	Phoſphore	Phoſphore.
	Carbone	Charbon pur.
	Radical muriatique.	Inconnu.
	Radical fluorique	Inconnu.
	Radical boracique	Inconnu.
Subſtances ſimples métalliques oxidables & acidifiables.	Antimoine	Antimoine.
	Argent	Argent.
	Arſenic	Arſenic.
	Biſmuth	Biſmuth.
	Cobolt	Cobolt.
	Cuivre	Cuivre.
	Etain	Etain.
	Fer	Fer.
	Manganèſe	Manganèſe.
	Mercure	Mercure.
	Molybdène	Molybdène.
	Nickel	Nickel.
	Or	Or.
	Platine	Platine.
	Plomb	Plomb.
	Tungſtène	Tungſtene.
	Zinc	Zinc.
Subſtances ſimples ſalifiables terreuſes.	Chaux	Terre calcaire, chaux.
	Magnéſie	Magnéſie, baſe du ſel d'Epſom.
	Baryte	Barote, terre peſante.
	Alumine	Argile, terre de l'alun, baſe de l'alun.
	Silice	Terre ſiliceuſe, terre vitrifiable.

Figure 3-2: Simple Substance belonging to all the kingdoms of nature, which may be considered as element of bodies (Lavoisier 175).

material but a form of energy[22], this quasi-metaphor lost its right to exist. Ironically, the rejection of Lavoisier's *caloric* is the best rebuttal to the argument that the rejection of phlogiston is merely a political campaign successfully executed by Lavoisier; for if so, caloric should have survived. Fleck notes that "it was this same Lavoisier who introduced such imponderable elements as heat and light in addition to the ponderable elements and who thus 'contradicted his own idea'" (123). By "ponderable," he means weighable. *Oxygen*, on the other hand, is a technical term that referred to the gas that Priestley first isolated, and on which Lavoisier also experimented himself. What was the gain in changing the name of this substance, especially considering it is the key of the Chemical Revolution?

The Chemical Revolution

Then it comes to the core question regarding the episode: What is exactly revolutionized? An older view, as Bernard Cohen describes, is that the "central feature of the Chemical Revolution was the overthrow of the reigning 'phlogiston' theory and its replacement by a theory based on the role of oxygen" (231). Priestley, who first identifies the properties of dephlogisticated air, never accepted oxygen theory till the day he died. He steadfastly sticks to the "doctrine of Stahl, which was at one time thought to have been the greatest discovery that had ever been made in the science" (Holmes, *The Revolution* 753). So, "old theories never die, their supporters just fade away" (Schofield 287). Is not this what Max Planck meant, that "a new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it" (Kuhn 150)? Considering that Priestley lived ten years longer than Lavoisier, then what did Lavoisier's program have that summoned the "new generation" to participate, even when he had

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[22] Energy is also one of the fundamental quasi-metaphors, too.

died and his opponents were still alive?

Kuhn suggests that it is the choice of the scientific community between competing "modes of community life," which includes "law, theory, application, and instrumentation together." His argument is not very convincing, considering that Lavoisier's *caloric*, very important to him, was rejected. Chang's argument includes two reasons. First, Lavoisier and his colleagues ran an effective and well-coordinated campaign for their new chemistry, including the spreading of their new nomenclature and the controlling of institutional spaces such as the Paris Académie and the new journal *Annales de chimie*. Second, the rejection of phlogiston makes much more sense when it was contextualized in the very gradual establishment of the building-block ontology of chemical composition (*The Hidden* 18-19). He implies that the Lavoisier's real battlefield was not merely combustion theory. Rather, Lavoisier's intention was to establish a new chemistry.

In the historical studies, there are "two images of Lavoisier—as leader of the Chemical Revolution, and as founder of modern chemistry" (Holmes 9, 1995). Should these two Lavoisiers be constructed one independent of the other? Or, should the Chemical Revolution be separated from the founding of modern chemistry? Given that oxygen theory and phlogiston theory are two rival theories each with their own strengths, then Lavoisier's was not necessarily the one to keep. The fatal obstacle to the extension of the phlogiston quasi-metaphor is its epistemic assumption that phlogiston is matter. The mercury experiment strongly suggests, at least to Lavoisier, that phlogiston does not exist; that oxygen is the matter of combustion. Yet to Priestley and other phlogistonists like Kirwan, the experiment can still be explained, or explained away, within the theoretical frame of phlogiston, especially when phlogiston is used to refer to a principle. This is the very reason that Priestley and some phlogistonists were never convinced.

Priestley declared that “no man ought to surrender his own judgment to any mere authority however respectable.” The irony is that phlogiston had been the authority, and he had been at “the position of leadership and adulation that he had enjoyed for a decade during which his doctrine dominated Europe” (*The Revolution* 753).

What is Lavoisier’s authority? All most all current chemistry textbooks name him “the father of modern chemistry.” In the history of science, the development of chemistry before Lavoisier is held to lag the others sciences by almost a century. While natural philosophy (physics) had been advanced under the Newtonian ideal of a rigorous quantitative and experimental methodology, chemistry remained qualitative and imprecise. “It has often been a matter of surprise that the emergence of modern chemistry should come at so late a stage in the story of scientific progress,” comments Herbert Butterfield (203). Lavoisier did not become a chemist by the traditional routes of medicine, pharmacy or metallurgy. Instead, he received a broad exposure to mathematics and the natural science through formal education at the collége des Quatre Nations and the family connections that introduced him to leading Parisian men of science (Perrin 266). It was not surprising that, in joining the main current in chemistry in the 1770s, Lavoisier provoked many controversies because he distinguished himself from his English colleagues by a tendency toward theory (Bensaude -Vincent and Stengers 84). Lavoisier’s ambition was to introduce what worked successfully in experimental physics into chemistry.

Lavoisier’s *chemistry* in the Chemical Revolution is a quasi-metaphor, similar to physics in the Scientific Revolution and molecular biology in the molecular revolution of biology[23],

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[23] Physics and the Scientific Revolution are discussed in Chapter 5, and the molecular revolution, Chapter 4 Part II.

whose conception was the projection of the methodology belonging to another discipline. In Donovan's words, Lavoisier took "as his model for chemistry the vigorous new approach to physics developed and championed by the experimental physicists" (Donovan 221). Lavoisier's new chemistry is a virtual category that he wishes to fill by transforming the old chemistry through new practice. The epistemic assumption is that, by applying the methodology, chemistry should be transformed to a scientific discipline as precise and accurate as experimental physics. In this perspective, the Chemical Revolution is the one that brought chemistry *into* science rather than as a revolution *in* science, very much along the lines that Donovan's argues. As such, Lavoisier's program produces chemists/scientists, unlike what Kuhn argues that scientists choose his program.

A Revolution of Methodology, Instruments and Language

Most chemists and historians agreed that Lavoisier had chosen wisely in making phlogiston the point of attack in his campaign for the formation of modern chemistry. In doing so, he was able to articulate first the law of conservation of mass:

> We may lay it down as an incontestable axiom that, in all the operations of art and nature, nothing is created; an equal amount of matter exists both before and after the experiment. Upon this principle, the whole art of performing chemical experiments depends: We must always suppose an exact equality between the elements of the body examined and those of the products of its analysis.
> (130 -131)

The balance remained Lavoisier's "favored instrument for precision." He invested considerable expertise and capital in the manufacture of balances of almost unrivalled accuracy (Golinski 202). In chemical reactions, the balance could provide accurate weight of reactants and products. The balance was so important to Lavoisier that it became his metaphorical way of thinking. It inspired him to invent the "balance sheet," on which chemical reactions could be represented in the way to follow the law of the conservation of mass.

Lavoisier designed and produced quite a few novel instruments. Historians generally believe that Lavoisier probably spent his fortune on the best scientific apparatus that money could buy at that time. Lavoisier's most elaborated instruments were the calorimeter, the gasometer, the apparatus for the synthesis of water, and the precision balance (Holmes, *The Evolution* 137). These instruments brought precision and accuracy, the essence of quantitative methodology, into chemistry. Since then, Chemistry has been a scientific discipline in which, "as in experimental physics, precise experiment was the methodological key to acquiring reliable knowledge of causal relations" (Donovan 228).

Lavoisier's other endeavor to bring chemistry into science is his nomenclature project. Madison Smartt Bell comments that the new chemical nomenclature and the French translation, together with refutation of Kirwan's challenge to oxygen theory were the two great propaganda tools of the Chemical Revolution. Around 1780, Louis-Bernard Guyton de Morveau (1737-1816), who was associated with Lavoisier, complained that the names of chemicals then was a jungle: many names were traditional and meaningless (powder of Algaroth, turhith mineral, colcothar); some substances were named after a person (Glauber's salt) or a place (Epsom salt); still other names were superficially associated with the appearance (oil of vitriol, butter of arsenic, liver of sulfur) or the effect of such substances on the sense organs (sugar of lead). Meanwhile, he also realized that using the names of substances used in the kitchen were especially objectionable, for many of these products were corrosive or toxic (Ihde 77). Lavoisier could not agree more with the necessity of reforming this field. Joined by Claude Louis Berthollet (1748 – 1822) and Antoine Francois de Fourcroy (1755 - 1809), the four men set out to formulate a nomenclature based on the framework of Lavoisier's theories of chemistry. Lavoisier's philosophy of scientific language sided with Étienne Bonnot de Condillac (1715-

1780). He quoted Condillac's writing at the beginning of his *Elements of Chemistry*:

> We think only through the medium of words. Languages are true analytical methods. Algebra, which is adapted to its purpose in every species of expression, in the most simple, most exact, and best manner possible, is at the same time a language and an analytical method. The art of reasoning is nothing more than a language well arranged. (viii)

To Lavoisier, the "right" language of science is the language of mathematics, and the line of reasoning should be presented as how mathematicians do it--in equations, or, in the form of balance sheet. Lavoisier is right on the point, because the "open", the "dark", or the "figurative" had been the styles of the sixteenth and seventeenth century alchemy writing. It was complained that in 1675 in a book *Cours de chymie* that "most of the authors who have spoken of chemistry have written about it with such obscurity that they seem to have done their best not to be understood" (Golinski 376). Lavoisier attacked the use of the term *phlogiston* as a "veritable Proteus," and words like *phlogiston* reflect the kind of "metaphorical expression" endemic in alchemy (Crosland 179). Lavosier's criticism echoes the voice of Bacon regarding plain style, particularly about metaphors, that the vague meaning of metaphors caused confusion. The difference was that Lavoisier did not propose to purify or clean up ordinary language; instead, he proposed a new "language" for chemistry with Condillac's algebra character, like mathematics, like that so triumphantly employed by Newton.

Their efforts culminated in the publication of the *Méthode de nomenclature chimique* in 1787. The adoption of Latin and Greek words made the terms internationally useful, and a systematic pattern of suffix made it possible to distinguish a great many compounds very easily. Salts formed from *aside sulfurique* were called *sulfates*. The *-ique* (Engl. *-ic*) suffix on a name indicated acids saturated with oxygen; the *-ite* suffix referred to salts of *-eux* (*-ous*) acids. The *-ure* (*-ide*) ending indicated compounds not in an acid state, i.e., *sulfure* (*sulfide*) used for sulfur-containing compounds; like liver of sulfur (K_2S). The *-ide* ending was oxides of the metal (Ihde 77). In doing so, it was secured that one chemical had only one name; while one name referred to only one chemical. Moreover, the name of a chemical indicated its chemical composition and structure. The chemicals belonging to the same category had the same signifier in their names. Yet many chemists were reluctant to adopt the new language, because the validity of Lavoisier's anti-phlogiston concepts and theories remained open to question. Lavoisier launched a campaign for the new nomenclature: correspondence with chemists everywhere; invitations to dinner; the publication of a new journal, *Annales de chimie*, in 1789. The triumph of his nomenclature was so complete that, fifty year later in the mid-nineteenth century, pre-Lavoisier works on chemistry were incomprehensible. The language used by chemists of the Academy exalted them from druggists and artisans, who would long continue to speak of something like "spirit of salt" and "vitriol" (Bensaude -Vincent and Stengers 89).

In 1786, when Guyton de Morveau contributed the first of his chemistry volumes to the *Encyclopédie méthodique*, he was a phlogiston loyalist. By 1789, his conversion to the anti-phlogiston theory and terminology was so complete that in the introduction to the second volume he made a claim that the whohttp://www.google.com/le nomenclature project had been his idea in the first place. He also wrote that phlogiston theory had been useful in its day—before it was definitively refuted by advances in pneumatic chemistry in the late 1770s and early 1780s. Guyton's conversion suggested that in the battle of phlogiston vs. oxygen, Priestley and Lavoisier perhaps were roughly even. It was Priestley who, under the guidance of phlogiston, discovered oxygen. The new chemistry, however, was the battle Priestley could not win. By bringing new chemistry into science, Lavoisier also brought his combustion theory and the name oxygen, which used to be fire air and dephlogisticated air, into science. To re-name dephlogisticated air as oxygen is thus not about the material or the referent itself; rather, it was about its epistemic position—to which epistemic category it should belong. Dephlogisticated air designated it to phlogiston theory, hence to alchemy. Oxygen put it in the elementary table of the new chemistry. It had to be oxygen. In the perspective of language and methodology, "the Chemical Revolution was more the creation of a new science than a change in an existing one.

Before 1750, chemistry could not be regarded as an independent discipline. It had long antecedents, but they were ancillary to other fields. Alchemy was a source for many of the recipes and much of the apparatus of chemistry, but this information was concealed in intentionally ambiguous and allegorical language" (Hankins 81).

In the root, the success of Lavoisier was the success of his epistemic assumption, that the methodology which works in experimental physics should work the same in chemistry. By the law of conservation of mass, he restricted the practice of chemistry within only the material realm. His nomenclature established one to one correspondence between chemicals and their names, hence secured a precise and practical language for the principle as science. The most important feature of such a system was to produce and facilitate the exchange of data. Since 1699 the new rule of the French Academy required scientists to contribute directly to the advancement of the science through their work and to communicate new results regularly to the Academy. The publication of an annual volume of the *histoire et mémoires de 'lAcadémie* began 1700. Under such circumstances, academics could no longer be satisfied by producing didactic works or rearranging established facts in a more rational order. Academic value shifted to emphasis on originality, and originality had to be supported by data. After almost a century, when Lavoisier proposed his chemical system, it was immediately adopted and tested. The new generations of chemists were attracted to his program, precisely because of its productivity. Lavoisier's quasi-metaphor chemistry provided productivity; and data production, in return, sustained and eventually validated the quasi-metaphor as a scientific discipline.

The Chemical Revolution is the death a quasi-metaphor phlogiston and birth of much more fundamental quasi-metaphor chemistry. Without the establishment of modern chemistry, phlogiston cannot be overthrown. Without phlogiston, the revolution might be a silent one. Lavoisier, accordingly, should be approached as both the leader of the Chemical Revolution and the founder of modern chemistry in the studies of the episode. Had he not been the founder, he could not have been the leader; had he not been the leader, he could never have laid the foundation for the revolution.

CHAPTER FOUR--Part I

GENE, Survival of the Materialized:
Before a Quasi-metaphor Met a Chemical

> And where the meaning of most four letter words is all too clear, that of gene is not.
> (Pearson 399)

In the quasi-metaphorical perspective, the gene is a perfect counterpart to phlogiston. The gene has had a dramatically different fate than phlogiston, while sharing one major issue, the vagueness of meaning. Since Lavoisier, the vague meaning of phlogiston has been blamed for its death; with respect to the gene, however, its vague meaning has been intensively explored for its significance. The irony is that, despite the suggestion to abandon the gene term for exactly the same reason as phlogiston, the gene maintains strongly and persistently vital. Scientifically, the gene not only has been the central organizing theme in many venues, such as molecular biology, genetics, and biochemistry, so that the twentieth century seems the century of gene, but it has also brought fundamental revolutions to biology, pharmaceutics, and medicine. Socially, the gene has become part of common knowledge. "Gene talk" is as ordinary as political debates and daily conversations, even into personal deliberations as well (Duden and Samerski 167). A little girl says in a diaper commercial, for instance, "I wet bed, but it's not my fault—it's in my genes." What concerns some scholars, though, is that everyone--"from evolutionist to dairy farmer," in Kenneth Waters' words (*Genes Made Molecular* 163)--knows and talks about the gene, but no one, including the scientists, can tell what exactly it is. Yet Elof Axel Carlson points out that the gene is indeed not alone regarding this problem of meaning. Terms like *life*, *love*, and *reality* "defy adequate definition when relentlessly pursued through all their subtleties and complex exceptions" (*Defining the Gene* 475). Another case in the history of science is the term mass, whose meaning is no stabler or clearer than the gene. As philosopher Max Jammer notes that “[the definition of the] mass is a mess” (3). These cases bring up a controversy: if the positivists are right that vague meaning is a fatal flaw for scientific terms, the gene and other terms alike should have been rejected, yet its vitality and productivity challenges the positivist assumption.

From quasi-metaphorical perspective, on the contrary, vague meaning makes it possible to conceptualize and further develop a conception. The history of the gene as a quasi-metaphor explains how its vague meaning allows the gene to be redefined and developed, while maintaining the coherence of research. The decisive historical moment, as the epistemic assumption of the gene is confirmed, not only separates the gene from phlogiston, but also separates the quasi-metaphor gene from the scientific concept gene. Accordingly, this chapter is divided into Part I and part II by the discovery of the double helix, the last piece of evidence that proves the existence of the gene. The history of the gene in this chapter begins with questions regarding heredity since the Greeks and extends to the discovery of the double helical structure of deoxyribonucleic acid (DNA) (i. e., the classical/Mendelian period) and then from the research springing from the gene as DNA to the completion of the Human Genome Project (i.e., the molecular period). Part I starts with a review of historical and philosophical treatments of the gene by other scholars, and Part II ends with negotiations between quasi-metaphor and other approaches. Part I focuses the history of quasi-metaphor gene, or, the establishment of the material foundation of heredity through the confluence of developments in chromosomal studies, biochemistry, cell biology, microbiology, virology, and biophysics; and Part II traces the history of quasi-metaphors derived from the gene; or, the scientific advances in various disciplines arising from the gene concept.

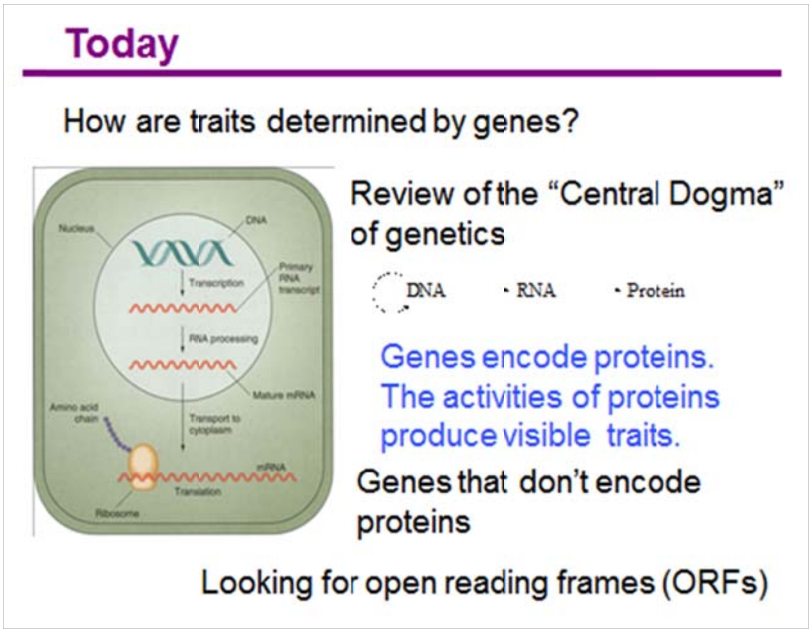
In Part I, particularly, the history traces the process in which the epistemic assumption of the gene as a material reality is transferred into epistemic access, to pinpoint what is known and unknown at every step where important discoveries take place and build the understanding of heredity. It mainly has three phases: initial work beginning with Darwin leading to an accepted conception of hereditary material as the gene, a period directed toward locating the gene somewhere in the cell, and then an intensive search to determine its chemical nature. Such a narrative passes over day-to-day and year-to-year detailed accounts like those carried out by Kay and others. It follows neither the approach of the scientific textbook in which the gene is a passive object waiting to be discovered nor the claim of Ludwig Fleck and Bachelard that something like the gene is technologically produced. The history of the gene as a quasi-metaphor is a dynamic process in which, on one hand, the epistemic assumption provides guidance to scientific research; on the other hand, the results from scientific search keep modifying and specifying the assumption. Unlike the evolution of phlogiston in the history of alchemy, that of the gene--the “little word” referring to the unknown cause of heredity--is a striking convergence of a few disciplines as a remarkable pattern of knowledge production. Quasi-metaphor gene is the stable reference to the body of knowledge kept being revised as well as accumulating; therefore, it is also the coherence that sustains the research over a half century and across different disciplines.

Discourse on the Definition of the Gene

Since the term was coined in 1909, definitions of the gene have been constantly revised; in the process, all the attempts so far to reach a unified and stable definition have failed. Notwithstanding the significance and popularity of the gene term in contemporary biology and society, its definition remains a “matter of controversy” (Griffiths and Neumann-Held 656). The problems with the definition are two-fold or two-dimensional: the historical and the disciplinary. Historically, the gene has been considered a unit, a factor, a segment, a molecule, a code, a long continuum, an encode, and a discrete genomic region; the modifiers describing those nouns, further, have also been revised from time to time, for example, the gene once was defined as "an undefined unit," another time, "a spherical unit defined by target structure," and another time, "a dynamic functional quantity of one specific unit" (Carlson, *Gene* 259; Pearson; Gerstein et. al; Pesole). A century after the term was introduced, "its meaning has been transformed almost beyond recognition" (Karola Stotz and Paul Griffiths 7).

Different scientific practices have their own definitions of the gene, although undergraduate students in life science majors are taught what philosophers call "the classical molecular gene concept" (Stotz, Griffiths, and Knight 649). In the Winter Quarter of 2010 at the University of Washington, when I took *Genome 371: Introduction to Genetics* offered by the Department of Genome Science, the gene concept first appeared when the teacher introduced the "Central Dogma." He remarked that "how gene is defined depends on what you are doing," and then went through this particular slide below (Figure 4I-1).

Among more than 190 students in that class, (assuming the attendance was 100%,) I am not sure how many students had grasped what the teacher said about the gene concept depending on the practice. I do not even remember if my teacher made such a comment when I took a similar class almost twenty years ago. In this context, the gene concept is presented with an implied definition of the classical molecular gene, in which the gene is a particular segment of DNA sequence that encodes a protein. Therefore, in a questionnaire study of how the gene is conceptualized by biological scientists at the University of Sydney, Australia, it is no surprise



Today

How are traits determined by genes?

Review of the "Central Dogma" of genetics

DNA • RNA • Protein

Genes encode proteins.
The activities of proteins produce visible traits.

Genes that don't encode proteins

Looking for open reading frames (ORFs)

Figure 4I-1: The PowerPoint slide of *Genome 371: Introduction to Genetics*

that 92% of the scientists identify the classical molecular gene concept as the biological function of a gene. Yet the same study shows that, to developmental biologists, the gene concept is used to investigate the complex developmental pathways through which genes are expressed; to evolutionary biologists, more or less, it is about the effects of the gene on phenotypes (Stotz, Griffiths, and Knight 671).

A definitive definition of the gene is not available either historically or disciplinarily. To some scholars, the difficulties of unifying the Mendelian/classical gene, the molecular gene, and especially the highly complex and diverse genes revealed by the genome studies signify the "crisis of the gene concept" (El-Hani 297). This problem of meaning echoes that of phlogiston; for this very reason, Lavoisier abandoned the word. Keller and David Harel invoke Lavoisier's words, that "we cannot improve the language of any science without at the same time improving the science itself; neither can we, on the other hand, improve a science, without improving the language or nomenclature which belongs to it," when they propose to replace both the concept and the word *gene* with *funtor* or *genitor*. As far as they are concerned, either of the proposed word "appears to be faithful to the findings of contemporary biology, encompassing many of the recently emerging—and surprisingly complex—links between structure and functionality," whereas the gene concept has been "stretched beyond the limits it can tolerate" regarding the scientific research of the current century, "the century beyond the gene" (1, 2). They suggest abandoning the gene term as Lavoisier did phlogiston.

Yet what makes the gene dramatically different from phlogiston is that, scientifically and socially, the gene is still extremely active and important. How to approach such a phenomenon? From the historical perspective, some scholars take the gene as an evolving concept, because "a distinguished lineage of research strategies postulates a one-to-one correspondence between a gene and some meaningful development unit" (Griffiths and Neumann-Held 657), even though there are "numerous instances of independent discovery, periods of obscurity, and spurious philosophic attitudes" in its history (Carlson, *Gene* vii). Peter Portin suggests that the gene concept is and has always been continuously evolving, such that the concept becomes not only "rather abstract, open, and generalized" but also increasing in "complexity" (173).

Where does the increasing complexity come from? When Raphael Falk carefully examines the causes and consequences of the evolution of the gene concept, he contemplates whether the gene is a "concept in flux," deriving from Yehuda Elkana's analysis of the concept of *Kraft*, German for force/energy, which suggests "a general feature of the way in which scientific concepts develop: they are in a state of flux while the scientist is struggling to clarify his thoughts; that is, while the discovery is being made" (264). Falk decides that what Elkana talks about is different from what happens to the gene, because the research in molecular genetics is “largely anchored in theories at the biochemical and biophysical levels. As long as these provide for the ‘experiment boom’ that goes on, there is no pressure on the investigators to become reflective and to ‘struggle to clarify thoughts’” (*What Is a Gene* 167). He is acutely aware of the paradox in taking the gene as “a concept in flux”: although the meanings of the gene have kept changing such that the concept seems in flux, as if formulating a theory at an early stage, the research regarding the gene is so productive that mature theories are established, and that complex knowledge is produced. Moreover, the scientists do not seem to worry about the meaning at all.

Instead of untangling the meanings of the gene concept, some philosophers pursue how the conceptualization of the gene functions, scrutinizing what the conceptualizations of the gene can or cannot do in sciences. To Moss, there are “two distinctly different senses of the gene,” gene-P and gene-D. With the P standing for performationist, gene-P refers to the genes that have a predictable relationship to a phenotype. BRCA1, the gene for breast cancer is a gene-P. With the D for developmental resource, gene-D refers to a segment of DNA sequence that is indeterminate with respect to phenotype. NCAM (for the neural cell adhesion molecule) as a gene-D is expressed in many tissues, at different developmental stages, and in many different forms (*What Genes* xiv, 45). The line between gene-P and gene-D, for Moss, is that between Mendelian/classical and molecular gene, although he specifically points that “Gene-P is no longer limited to classical methods” (*The Future* 5).

Differing from Moss, Paul Griffiths and Karola Stotz divide the functions of the gene concept into three aspects: instrumental, nominal and postgenomic. The role of the instrumental gene is to construct and interpret experiments, through which the relationship between genotype and phenotype is explored. In doing so, moreover, it also serves as a disciplinary foundation, as in quantitative genetics and population genetics. That of the nominal gene is pragmatic, which allows stable communication between scientists "in a wide range of fields grounded in well-defined sequences of nucleotides," but does not embody major theoretical insights into genome structure or function. The post-genomic gene embodies the continuing project of understanding how genome structure supports genome function but deflates, at the same time, the function of the gene as a structural unit (499).

From the perspective of empirical science, Thomas Fogle perceives that the identification of a molecular gene does not stem from definition; rather, it is "a methodological process." In other words, in scientific research, "genes are recognized by formally or informally comparing elements of structure, expression, and function to those previously documented," and there are sets of well-established physical elements and properties for the molecular genes. Forcing those diverse molecular phenomena into "a single Procrustean bed," that is, the molecular gene concept, implies a universal construction. Fogle argues that the gene as a molecular vehicle for causation is an ambiguous referent, so the gap between the Mendelian/classical gene as a "unit of inheritance" and the molecular gene is impossible to bridge. In order to maintain the coherence of the molecular gene concept, the Mendelian/classical gene concept should be abandoned. Ultimately, what remains is at best a "consensus gene" concept, "a collection of flexible applied parameters derived from features of well-characterized genes." For example, the production of ribonucleic acid (RNA) is a strong indication of transcription generated by a gene; so, RNA is one of the consensus features in the characteristics of the gene. Furthermore, a study by Stotz and Griffiths done among the American scientists reveals that, when they have been exposed to the cases violating the characteristics in practice, they intend to ignore those problems and revert to the consensus gene concept by considering the particular cases as exceptions (*Genes* 8).

Contrary to Fogle, Kenneth Waters argues that a "fundamental concept" that "genes are for a linear sequence in a product at some stage of genetic expression" underlies the application of the gene, despite inconsistent and ambiguous usage of the term *gene* (*Genes Made Molecular* 178). Further, he emphasizes that some philosophers like Fogle generally confuse the ambiguities of application for ambiguities in the underlying concept. The ambiguity of the gene stems from the incapacity to identify a particular linear sequence or stage of genetic expression when the fundamental concept is applied in scientific practices, not from the fundamental concept itself. In eukaryotes, for example, the gene, a segment of DNA that codes a protein, is interrupted by some other small segments of DNA considered as "junk DNA" (so far as we currently know). The ambiguity in this case is that the gene's expression as protein cannot be thoroughly discussed unless the terms such as introns, exons, promoters, and operators[24] are employed. The fundamental gene concept, however, unifies the understanding of a wide variety of phenomena at the molecular level, including the studies by classical genetics; so there is no gap between the Mendelian/classical gene and the molecular gene (*Molecules Made Biological* 543-544).

Hans-Jörg Rheinberger extends the discussion further to argue in defense of the vagueness regarding two aspects in experimental systems. He identifies the gene as an "epistemic object/thing." Epistemic objects are "material entities or process—physical structures, chemical reactions, [and] biological functions –that constitute the objects of inquiry." The irreducible characteristic of epistemic objects is vagueness, because they embody what one does not yet know. The other element contrasting with epistemic objects is "technical objects,"

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[24] More discussion of these terms is in the second part of this chapter and Chapter Five.

which are epistemic practices and material cultures through which epistemic objects become entrenched and articulated, including “instruments, inscription devices, model organisms, and the floating theorems” (*Toward a History* 28-29). Rheinberger’s argument concerns the fundamental problem in scientific research: how to deal with the unknown? It is a tripartite question: How to conceptualize the unknown? How to express and communicate the conception? And how to construct experimental system(s) to explore the conception? The first is a mental process, the second, linguistic, and the third, pragmatic. To draw lines and set these three in order may be naïve; nevertheless, such differentiation provides a hint on how to approach phenomena like the gene. Rheinberger’s approach is the third--the pragmatic, a view from bench-work that emphasizes how instead of what and why: how data are produced to constitute an epistemic object through technical objects. So, epistemic objects are epistemically unknown and constructed objects, the very property of which is vagueness (*Gene Concepts* 220).

Quasi-metaphors take the same departure point as epistemic objects--that is, the vagueness--to explore how the gene term and alike function in the history of science. Yet the vagueness in the quasi-metaphorical approach is the meaning of the term that refers to the unknown. This chapter will demonstrate that at every step of the development of the gene term, even when what the gene is remains unknown, its status as epistemic object is clear. Furthermore, the vagueness of the meaning allows scientists from different disciplines at different stages in different historical periods to work toward one goal--what the gene is, such that it maintains the coherence of the research.

Nevertheless, to Philip Kitcher, the conceptual changes of the gene are the changes of reference potential. More specifically, the reference potential of the gene for the scientific community is a compendium of how the tokens of the term are fixed for the members of the community. Over time, various adjustments in reference potential of the gene occur (340). The reference potential of the gene may share no common element with another reference potential at an earlier time. Yet the evolution of the concept may be continuous, in that, at intermediate times, the reference potential of the gene may contain elements from both the non-overlapping classes. For instance, genes are taken to be the units of function, the units of recombination, and units of mutation (349). Change of meaning, therefore, is perfectly normal, for it is acknowledged that the gene has multiple referents sorted by mutually understood cues among scientists. Kitcher argues that reference potential, as an alteration in the mode of reference, is necessary because “the acquisition of an identifying description paves the way for a different means of fixing the reference of the old expression” (340). Suggesting the opposite of Rheinberger’s approach, his does not assume that the “old expression” is important to the referent potential. That implies, in the case of the gene term, that how it is proposed does not affect what happens to it; or, the sense need not determine the reference. The important thing is that the scientific community agrees to keep fixing the reference of the term until such time as the term does acquire an identifying description (a sense) that does determine its reference. As to quasi-metaphorical approach, the way in which the reference of the gene is proposed differs from Kitcher but sides with Rheinberger: it is the unknown cause of heredity. More importantly, viewing it as a quasi-metaphor endows the gene with open-ended characteristics but epistemic assumptions, and methodological commitments--these are the reasons why it has been kept among many similar terms.

Historically, the gene term came first and then the gene object(s), not the other way around; hence the gene concept could be approached through a linguistic perspective. In order to do so, the first question to answer is: what category does the word *gene* belong to? Keller takes a metaphorical approach to explore the gene term:

> From its very beginning then, the gene was already something of a monster—not quite a metaphor, or at least not in any strict sense of the term, but a neologism that has the potential of even greater force for it builds on the work of two or more metaphors that are between or among them not only disjoint but in active tension, and conjoins these into a new and apparently seamless unity. Where a metaphor says "this is that," inviting us to see both this and that in new ways, here we have a linguistic construction that amalgamates this and that. It melds into a single form two entities with the disparate properties of atom and organism, and contains the incoherence of such a melding under the protective wrap of a new word. In effect, it offers a resolution of the riddle of life by invoking an entity that is a riddle in and of itself. (*Making Sense of Life* 131)

She criticizes the gene term at two levels. At the scientific level, she considers the concept almost useless to explain any aspects in developmental biology, because "[g]enes display neither the stability nor the clarity expected of the explanatory elements upon which the physical sciences have come to rely" (*Making Sense of Life* 117). That is, from an atomic/molecular perspective, the gene does not have a fixed molecular formula--as water is H_2O; from a morphological perspective, no one can tell which part of an organism develops from what specific gene(s). So, the failure of the concept gene is that it should, yet cannot, answer to both genotype and phenotype. More disturbing, the gene still "freely oscillates between atom and organism;" in other words, the gene is still counted as explanations of biological development at both molecular and morphological levels, although to Keller the concept "could no longer serve so readily both as the fundamental unit of heredity and, at the same time, as the pilot of life's developmental journey" (*Making Sense of Life* 134). Keller's opinion echoes Lavoisier's philosophy in the language of science: if a term lacks precision, it should not be employed in a line of reasoning. At the rhetorical level, Keller thinks that the word *gene* is a bad metaphor, for it lacks well-defined tenor and vehicle. Nevertheless, Keller does grasp that there seem to be "amalgamated" categories in the conceptualization, though the concept itself cannot articulate them. Keller’s standpoint is intriguing. If she holds a positivist position like Lavoisier's, she should reject the perspective that gene is a metaphor—even though a bad one. On the other hand, if she takes *gene* as a metaphor through a rhetorical view, then she should not criticize it for its imprecision by a scientific standard; rather, what should be carefully examined is what makes gene, a coined word, a metaphor. She argues that the term *gene action* is an effective metaphor because the word *action* represents a clear vehicle and, in the meantime, suppresses the structural in favor of the functional. Nonetheless, how clear can *gene action* be if the modifier, *gene*, is already a blunder?

Studies on Heredity and the Conception of the Gene

Although the term *gene* was coined at the beginning of the twentieth century, questioning, thinking, and theorizing about heredity in the western history can be traced back as early as biblical times. During the eighteenth century, the studies on heredity became relatively mature, and two approaches to the natural system emerged, nominalism (or instrumentalism, or conventionalism) and essentialism (or realism). In nominalism, taxonomy is a humanly devised system to control natural variability through morphological, anatomical and physiological investigations regarding species boundaries. New species were added to taxonomic lists according to the nominalist’s decision on where they should be. Essentialism, on the other hand, perceived each species as a given and well-defined entity with distinct characters. Newly discovered species were thought previously unknown. To essentialists, hybridization, understood as cross-breeding that had its roots since antiquity in the practices of animal and plant breeders’ domestication, was the ultimate research tool to determine taxonomic status, for they emphasized the specific characters that differentiated species. Swedish botanist Carl Linnæus (1707-1778) introduced an essentialist taxonomy based on the reproductive characteristics of a plant's organs, which laid the methodological foundation for biological inheritance through examining hybridability of organisms. Linnæus' contemporary, French naturalist George-Louis Leclerc, Comte de Buffon (1707-1788) proposed a nominalist taxonomy that took the structures and functions of living creatures, as well as their utility for men, into consideration. Starting from Buffon's pragmatic classification, the morphogenist approach emphasizing diversity led to French naturalist Jean-Baptiste Lamarck's (1744-1829) and English naturalist Charles Darwin's (1809-1882) theories of evolution of the species. The taxonomies that found the epistemic categories of the biological world, together with the experiences of the differences and similarities of inter-species and intra-species, lead to the ultimate question: what is the cause of heredity?

Nine years after the publication of *On the Origin of Species*, Darwin published in 1868 *Variation of Plants under Domestication*, which, as Frederick Churchill points out, was arguably the most fundamental work on heredity in the middle third of the century, save that of Mendel (*From Heredity Theory* 343). Darwin proposed a new theory, the "provisional hypothesis of pangenesis." The fundamental building blocks of pangenesis were microscopic hereditary particles that Darwin named "gemmules." He described that each cell constantly shed gemmules at each stage of development. The gemmules circulated throughout the body and found their way to the reproductive cells, where they could be assembled into germ cells, the sperm and egg cells, capable of developing into new organisms. Dutch botanist Hugo de Vries (1848-1935), who was thirty nine years junior to Darwin and only met Darwin once briefly, published *Intracellular Pangenesis* in 1889. The title of his work paid homage to Darwin, as he said that "I was led to my study of heredity by my love for Darwin;" nevertheless, his theory had little in common with Darwin's. De Vries coined the word *pangenes* to replace Darwin's gemmules, meanwhile honoring and benefiting from his association with Darwin. Pangenes referred to enzymes and the most basic components of the cell. In doing so, De Vries altered Darwin's gemmules concept and basically rejected the central idea of the pangenesis hypothesis, namely, gemmules. He continued to work toward establishing "the law of pangene" through hybridization, during which he encountered the remarkable work of an earlier scientist, Gregor Mendel (Schwartz 73).

Quite different from Darwin's theory of Natural Selection, which created a great stir, Mendel's is an instance of what Carlson calls "a modest discovery… announced under modest circumstance; …slow to gain recognition, and may await independent rediscovery before the significance is realized" (*Gene* 1). Gregor Johann Mendel (1822-1884) was eventually acknowledged as the father of genetics[25], to whom the discipline wholly owes its origin (Olby, *Mendel no Mendelian* 53), and with whom every modern scientific textbook of/about genetics starts. Mendel's paper "Experiments in Plant Hybridization" was read at February 8th and March 8th, 1865, meetings of the Brünn Natural History Society to report his work on hybridization of peas. In 1900, suddenly this article was cited and discussed simultaneously in the same volume of the *Proceedings of the German Botanical Society* by de Vries and two other scientists, Carl Correns and Erich von Tschermak. Some scholars like Keller and Portin consider the event as the rediscovery of Mendel's rules of inheritance (Keller, *The Century of the Gene* 1; Portin 175), whereas R. A. Fisher and later Falk dispute such a simple-minded view of science. Fisher argues

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[25] The term *genetics* was coined by Bateson. In 1905, he was a fellow at the University of Cambridge and wrote a letter to urge the university to establish a Chair in the field:

> If the Quick Fund were used for the foundation of a Professorship relating to Heredity and Variation, the best title would, I think, be 'The Quick Professorship of the Study of Heredity'. No simple word in common use quite gives this meaning. Such a word is badly wanted, and if it were desirable to coin one, 'Genetics' might do. (Harper 143)

that, often, the so-called rediscoveries do not examine the complete work of Mendel "with sufficient care to prevent its many extraordinary features being overlooked and the opinions of its author being misrepresented" (137). Falk demonstrates that none of the proclaimed rediscoveries of Mendel's paper identified with his claim, although de Vries and English biologist William Bateson (1861–1926) immediately and fully appreciated the impact of Mendel's idea on the wider scope of their theories of heredity. Thus, Mendel is recruited rather than rediscovered (*Struggle* 221). No one, however, will deny that he offers an experimental system in which hybridization becomes a powerful analytical tool with three operational principles: First, choose materials proper for experiments. In Mendel's own words, "the value and utility of any experiment are determined by the fitness of the material to the purpose for which it is used" (2). Second, decide on the pairs of differentiating characters with which to experiment. Mendel chose to observe the difference in the form of ripe seeds and pods, the colors of the seed albumen, seed-coat and unripe pods, the position of flowers, and the length of the stem, as the characters of different pea plant species, in order to make the clearest observation and obtain the most accurate data (5). Third, collect quantitative data and process data in ways that best reveal the relationship of observational parameters. In so doing, the experiments yield the well-known Mendelian laws: the law of segregation and the law of independent assortment.

Such a system of experiments allows scientists to "build on… and come up with a workable theory of heredity" (Monaghan and Corcos 268). Mendel further speculates that there are "factors" determining the characters:

> So far as experience goes, we find it in every case confirmed that constant progeny can only be formed when the egg cells and the fertilizing pollen are of like character, so that both are provided with the material for creating quite

> similar individuals, as is the case with the normal fertilization of pure species. We must therefore regard it as certain that exactly similar factors must be at work also in the production of the constant forms in the hybrid plants. (20)

Mendel knew that the characters were determined by something beyond the characters themselves, vigorously based on the evidence of his well-designed experiments, although he could not tell what and how. Olby notes that this was the only place where Mendel used *factor* as *determinant*; elsewhere he either used *element* or simply refers to the characters in the sense of the determinant (*Origins of Mendelism* 192). English geneticist Cyril Dean Darlington (1903-1981) comments that "[t]o us it is now evident that the great revolutionary moment was when Mendel referred to 'elements which determine'" (97). Fisher, however, remarks the recognition of Mendel's work in the history of science as such: "[e]ach generation, perhaps, found in Mendel's paper only what it expected to find" (137).

To further specify and define the rapidly developing research on heredity, Danish scientist Wilhelm Johannsen (1857-1927) in his article, "The Genotype Conception of Heredity," published in 1911, proposed two new concepts: genotype and phenotype. To him, "[a] genotype is the sum total of all the 'genes' in a gamete or in a zygote," whereas "phenotype [is] real things; the appearing (not only apparent) 'type' or 'sorts' of organisms are again and again the objects for scientific research." Moreover, phenotype could be changed or obtained by the selection of genotype. Johannsen thought that, "for the descriptive-morphological view, the manifestation of the phenotypes in different generations" is better than the "characters" of Mendel, and the genotype concept was proved by "pure line breeding and hybridization" (*Genotype Conception* 133-134,137, 135). In his *Elemente der exakten Erblichkeitslehre*, he described the relation of phenotype and genotype in this way:

> This constitution we designate by the word genotype. The word is entirely independent of any hypothesis; it is *fact*, not hypothesis that different zygotes arising by fertilization can thereby have different qualities, that, even under quite similar conditions of life, phenotypically diverse individuals can develop. (Dunn 92, emphasis in the original)

Thus, phenotype is free from environmental influence, and phenotypical diversities are caused by the change of genotype. Or, genotype determines phenotype. Such a relation can only be revealed through altering the existing relationships between phenotype and genotype. Genotype and phenotype concepts are "a conceptual breakthrough," "one of the most fundamental distinctions in all of biology" (Falk, *Genetic Analysis* 63; Churchill, *William Johannsen* 28). The significance of the proposal is that it claims the relation as well as the distinction of the cause and effect regarding the characters and their determiners in the same being. When characters are referred to as phenotype, the implication is that they are determined by phenotype made up by genes. Still, what is the gene? In 1909, Johannsen proposed the gene term in his book *Elemente der exakten Erblichkeitslehre*:

> There has been no doubt about that the gametes contain 'something' which is responsible for, or influences the character of the newly founded organism. The zygote, which results from the fusion of the gametes during fertilization contains the 'something' supplied by each of the gametes. This 'something' in the gametes or zygote, which is accountable for the character of the organism, is usually referred to as 'Anlage' – a very ambiguous term. Many other terms have been proposed which are unfortunately all associated with assumptions. The most commonly used term instead of 'Anlage' is 'pangen', which is derived from Darwin. However, the word 'pangen', is an unfortunate choice, since it is a combination of two Greek words (pan, meaning all, everybody; gen, meaning to become). Only the meaning of the latter is suitable to describe that 'something' in the gametes can or might be able to contribute to a trait of the developing organism. No hypothesis concerning the nature of this 'something' shall be put forward thereby or based thereon. Therefore it appears as most simple to use the last syllable 'gen' taken from Darwin's well-known word pangene since it alone is of interest to use, in order to replace the poor, and ambiguous word, 'Anlage'. Thus, we will say for 'pangen' and 'pangenes' simply 'gene' and 'genes'. The word gene is fully free from every hypothesis; it expresses only the safely proven fact that in any case many characteristics of the organism are conditioned by

> special, separable and hence independent ‘conditions’, ‘Grundlagen’, ‘Anlagen’ – in short what we will call just ‘genes’–which are present in the gametes. (Translation from note of Wu and Morris)

In short, the gene is something in a gamete that is responsible for or influences the characteristics of the organism. Carlson’s remarks that the gene is “undefined” at this point (*Defining the Gene* 475), which is another way of saying that the Johannsen proposed a quasi-metaphor rather than a definitive concept.

Johannsen is very careful, rather than hesitant as Roll-Hansen argues, not to provide his conception “a particular physical content” (202). As Churchill comments, Johannsen is a chemist and statistician first (*William Johannsen* 28), and he was aware that the nature of the gene is unknown and believed it could only be determined through further experiments. L. C. Dunn marks Johannsen’s ability to “see more clearly than most of his contemporaries how necessary it was to restrict oneself to what we now call operative definition, and to avoid conceptions not derived from experimental evidence” (93). In Johannsen’s own words:

> The conception of the gene as an organoid, a little body with independent life and similar attributes, is no longer to be considered. Assumptions which would make such a conception necessary, fail utterly. Putting a horse in the locomotive as a cause of its motion—to use Lance’s classical example—is just as “scientific” a[n] hypothesis as the organoid “explanation” of heredity. (Dunn 93, 1991)

What Johannsen saw more clearly than his contemporaries who also proposed similar terms perhaps was that, if the scientific-sounding organoid completely begged the question, his own knowledge in the same matter was not much better, so why not leave the term to future experiment?

Johannsen’s definition of the gene can better be taken as the definition of a quasi-metaphor. He conceives the unknown cause of phenotype as genotype, and then derives the gene term from the genotype. It is an insightful move, in retrospect, to separate the genotype from the gene. In so doing, an anti-reductionist idea is embedded in the proposal, that the phenotype develops from genotype, not from genes, although the gene is the component of the genotype. Briefly, genotype cannot merely reduce to one gene after another. As vague as the little word *gene* could be, its epistemic assumption of the hereditary hierarchy was rather accurate. Falk notes that, by deriving the gene from genotype, Johannsen provided "new legitimization to heredity of particular entities," and by dissociating transmission from development, "the unit character became superfluous" (63, 2009). In other words, Johannsen's proposal reduces the complexity of heredity to three different levels of research: the hereditary materials as the gene; the transmission of the whole genotype from one generation to the next; and the development of the genotype to phenotype in an individual. The proposal was not merely for terminology; it also functioned as a proposal for research programs. As did de Vries, Johannsen rejects other similar terms because of their epistemic assumptions; with respect to the gene, its assumption is that "the notion 'gene' covers a reality evident from Mendelism" (Johannsen, *The Genotype Conception* 133), which is as simple as that the gene exists. He continued to hold this epistemic assumption and rejected any hypotheses of the physical nature of the gene till the end of his career in the early 1920s (Johannsen, *Some Remarks* 136; Falk, *Genetic Analysis* 63).

Just as Stahl's phlogiston projected precisely the intelligibility of the natural world at his time, Johannsen's conceptualization of the gene does exactly the same to the intelligibility of the heredity of his time. To function as quasi-metaphors is to conceive the unknown. Phlogiston and the gene cannot be specific and precise because they are speculations. The basis of such speculations, their epistemic assumptions, on the other hand, is the precise projection of what is known, as well as the methodology which leads to the known. In Stahl's case, phlogiston is proposed together with Becher's understanding of substances, which consist of three types of earth: *terra fluida* (the mercurial), *terra lapida* (the solidifying) and *phlogios* (the fiery)/phlogiston (Strathern 207 -208); in Johannsen's case, heredity is understood through the phenotype, genotype and gene. Both phlogiston and gene are coined to refer to a virtual existence to be discovered, and their epistemic assumptions are almost identical: that something, the phlogiston or gene, exists that has such effect, combustibility or heredity. Furthermore, as quasi-metaphors, both function to guide the research to discover them. Or more precisely, both come with methodological commitments: phlogiston's is alchemical, and the gene's, Mendelian.

American geneticist George Harrison Shull (1874-1954) was the first to propose the gene term in the English language in his "Presence and Absence Hypothesis" published in 1909, for "[t]he word 'gene' has the advantage that it does not assume by its form or derivation any hypothesis as to the ultimate character, origin or behavior of the determining factor" (414). Compared with other similar terms, the epistemic assumption of Johannsen's gene is extremely conservative--in his own words, "[t]he 'gene' is nothing but a very applicable little word" *(Genotype Conception* 132); however, the gene term is the one that survives. If Mendel's is a "modest discovery," then Johannsen's is a modest proposal, which, in Carlson's words, "gave the gene concept an opportunity to evolve" (*Gene* 22).

Discovering Chromosome and the Advances of Cell Biology

The gene had its first opportunity with the chromatin and chromosome. In 1844, Swiss botanist Karl Wilhelm von Nägeli (1817-1891) observed nuclear division, but he thought it was exceptional. After more than forty years, however, it was established that cell formation was initiated by nucleus division and followed by cell self-division (Dunn 52). The progress on the studies of cell biology, particularly cell division, owed a great debt to the developments of microscopic technology as well as organic chemistry during the second half of the nineteenth century. Two Dutch eye glass makers, Zacharias Janssen (1587-1638) and his son Hans Janssen (1534-1592), invented the microscope by placing multiple lenses in a tube, through which they observed magnified images. The Dutch merchant, Antoni van Leeuwenhoek (1632-1723) made single lens microscope. Before British physicist Joseph Jackson Lister (1786-1869) invented the achromatic objective in the later 1820s, the two kinds of microscopic objectives, the single lens and the compound, suffered severe shortcomings. The former had lower resolutions, and the latter yielded low-quality images due to the multiplication of the spherical optical aberrations and coma of each lens. The achromatic overcame the problems (Masters 2).

Another technique to improve image quality was the specimen staining techniques that increased specimen contrast. The golden age of German organic chemistry began in the second half of the nineteenth century, and the synthetic dye industry reached maturity twenty years later. The field became scientifically eminent and played a dominating role in the German chemical industry (Beer). The newly synthesized and industrially produced dyes could be used both on textile fibers to create more colorful women's fashions and on cellular structures. With the help of microscopes and dyes, the various stages of mitosis were thoroughly studied. 1882, German biologist Walther Flemming (1843-1905) published his seminal book, *Zellsubstanz, Kern und Zelltheilung* (*Cell Substance, Nucleus, and Cell Division*), "the foundation for all further research into mitosis," in which he described what the new dyeing techniques had proved concerning the cell nucleus (Paweletz 74). He also coined the term *chromatin*:

> [I]n the view of its refractile nature, its reactions, and above all its affinity to dyes, is a substance which I have named chromatin…. The word chromatin may stand until its chemical nature is known, and meanwhile stands for that substance in the cell nucleus which is readily stained. (Translation from Olins and Olins 809)

Chromatin means stainable material. In a review of chromatin history published in *Nature* in 2003, more than a century later, the author declares: "so the name 'chromatin' still stands, and is likely to remain into the future" (Olins and Olins 809). In 1889, German anatomist Heinrich Wilhelm Gottfried von Waldeyer-Hartz (1836-1921) published his "Karyokinesis and Its Relation to the Process of Fertilization," in which he reported "[t]he phenomenon which is indicated by the name 'Karyokinesis' depends essentially on the appearance of distinctly visible and readily stained thread-like structures in the cell nucleus, which change their form during its division" (159). Meanwhile, he proposed to name "those things in which there occurs one of the most important acts in karyokinesis, viz. the longitudinal splitting," as "chromosome" (181).

After years of tedious research, it was established that mitosis had three major stages, prophase, metaphase, and anaphase. The resting nucleus between cell divisions did not stain well, and much of the chromatin was organized in one or several thin threads, or network of threads. As cell division approached (the onset of prophase), the membrane around the nucleus disappeared, and the chromatin became condensed and more easily stained by appropriate dyes. Then the treads contracted into a few heavily staining bands, designated as chromosome. It was known then that every member of a species had in each cell a constant number of chromosomes, forty-six in man, for instance. They seemed to arrange themselves during nuclear division in an equatorial plane, and then each chromosome split into two, longitudinally. The split occurred before metaphase when the chromatin was still in the uncondensed stage and almost invisible to observation. At the next stage, the metaphase, the two halves of the split chromosomes separated from each other and moved to the opposite poles of the dividing nucleus. At anaphase, around each bundle of chromosomes, that is, at the two poles, a new nuclear membrane was formed, and the bands reverted back to their threadlike and largely invisible resting condition (Mayr 675).

The development of cell biology, particularly in the changes of chromosome during cell division, suggested that chromosome was significant for heredity. Chromosome research became an important theme from then on.

Locating the Gene on Chromosome

Far away from the biological research of Europe, a young American from Kansas, Walter Stanborough Sutton (1877-1916), collected lubber grasshoppers from his parents' ranch and then studied them with passion, first at the University of Kansas and then at Columbia University. In 1902, Sutton published "On the Morphology of the Chromosome Group in *Brachystola Magna*," in which he concludes that a chromosome pair was joined by the maternal and paternal member in the spermatocyte. In synapsis, the entire chromatin basis of a certain set of qualities inherited from two parents was localized for the first and only time in a single continuous chromatin mass. In the second spermatocyte division, the two parts were again were separated, one went to contribute to the daughter-cells the corresponding group of qualities. At the end of which he stated "the central insight of biology" (Schwartz162):

> I may finally call attention to the probability that the association of paternal and maternal chromosomes in pairs and their subsequent separation during the reducing division as indicated above may constitute the physical basis of the Mendelian law of heredity. (39)

Only two months later, he published "The Chromosome in Heredity," in which he claimed that the data of his cytological research "completely satisfy the conditions in typical Mendelian cases." He furthermore elaborated that "many of the known deviations from the Mendelian type may be explained by easily conceivable variations from the normal chromosomic processes" (231). In Sutton's research, chromosomal theory converges with Mendelian laws, though there is a thirty-eight year gap in between.

The one mainly responsible for neglecting Mendel’s work, ironically, was Nägeli who first observed cell division, as far as quite a few historians are concerned. As the foremost botanist and hybridizers of his time, he corresponded with Mendel regarding Mendel’s work, but he failed to even mention Mendel or Mendel’s work in any of his publications. The explanations on why he neglects Mendel’s work are still debated. (Dunn 14 to 20, Schwartz 96 to100, Falk 36). Nonetheless, Johannsen rejected chromosomal interpretation of the genotype (*Some Remarks* 140). American geneticist Thomas Hunt Morgan (1866-1945), in particular, opposed Sutton’s speculation that the accessory chromosome ((i.e. X chromosome) was involved in sex determination. Yet Morgan’s research turned out against his deepest wishes, as well as Johannsen’s. In a biographical memoir of Morgan, one of his favorite and most accomplished students, A. H. Sturtevant (1891-1970) remarked on his mentor’s research that “[t]he results were of importance in serving to demonstrate the role of the chromosomes in sex determination, at a time when that importance was seriously questioned by many biologists,” and “[t]his was one of Morgan’s most brilliant achievements” (289). In 1917, Morgan claimed that the genes were “hereditary materials” in his article “The Theory of the Gene” (514), and furthermore, his data “has played a role in persuading us that the genes postulated for Mendelian inheritance have a real basis and that they are located in the chromosomes” (520). Morgan reflected in his 1934 Nobel Lecture:

> What are genes? Now that we locate them in the chromosomes are we justified in regarding them as material units; as chemical bodies of a higher order than molecules? Frankly, these are questions with which the working geneticist has not much concern himself, except now and then to speculate as to the nature of the postulated elements. There is no consensus of opinion amongst geneticists as to what the genes are whether they are real or purely fictitious - because at the level at which the genetic experiments lie, it does not make the slightest difference whether the gene is a hypothetical unit, or whether the gene is a material particle. In either case the unit is associated with a specific chromosome, and can be

> localized there by purely genetic analysis. Hence, if the gene is a material unit, it is a piece of a chromosome; if it is a fictitious unit, it must be referred to a definite location in a chromosome - the same place as on the other hypothesis. Therefore, it makes no difference in the actual work in genetics which point of view is taken. (315)

On one hand, Morgan had always been the defender of the gene. To him, *element*, *gene*, *genetic factor*, and *Mendelian factor* are synonyms, as he defined that "[t]he germ plasm must, therefore, be made up of independent elements of some kind. It is these elements that we call genetic factors or more briefly genes" (*The Theory of the Gene* 515). On the other hand, his experimental approach was to study the phenotypes of fruit flies through hybridization so as to understand the chromosomes; what the gene exactly was therefore did not affect the design and the outcomes of his experiments.

This discovery of the location of genes is a major step in the development of the concept, because the epistemic assumption of Johannsen is partially confirmed as epistemic access that the location of the gene is at chromosomes. It is ironic that even Johannsen, the creator of *genotype* and *gene* terms, did not believe that they had anything to do with chromosomes. Both terms survived because Morgan's work proves that genes are located at chromosomes. A quasi-metaphorical explanation is that the epistemic assumption of the gene term is too modest to specify where the gene should or should not be. As long as its discovered nature does not conflict with the epistemic assumption, the term survives. Clearly, Johannsen had in mind where the gene should not be, which included where the gene actually turned out to be--located at chromosomes. Yet the epistemic assumption of his quasi-metaphor did not rule it out, so the gene had its chance. More importantly, Johannsen's methodological commitment was also significant: had the chromosomal research not turned out to be Mendelian, the gene might have been nothing but a little word, forgot as other similar terms. At this point, genetic research moves into the post-Morgan era, a new phase in which the emphasis will shift to determining the physical and chemical nature of the gene.

Mutations on Chromosome and Advances in Physics

One of Morgan's most gifted students, Hermann Joseph Muller (1890—1967), brought the *Drosophila* research to another level. Muller was indeed Morgan's disfavored student, for they had different perceptions regarding objects and methodology of research, but both became highly influential in the history of genetics. When Muller took Morgan's graduate course in experimental zoology in his first year as a graduate student in physiology, he quickly became wary of what appeared to him as Morgan's sloppiness and lack of precision as well as "chaotic and undeveloped" thinking (Schwartz 196). The exact nature of the gene, assumed not to make slightest difference to current genetic research by Morgan, was the crux of the matter to Muller. Muller believed that the best research strategy in genetics should be the study of mutations:

> The subject of gene variation is an important one, however, not only on account of the apparent problem that is thus inherent in it, but also because this same peculiar phenomenon that it involves lies at the root of organic evolution, and hence, of all the vital phenomena which have resulted from evolution. It is commonly said that evolution rests upon two foundations--inheritance and variation; but there is a subtle and important error here. Inheritance by itself leads to no change, and variation leads to no permanent change, unless the variations themselves are heritable. Thus it is not inheritance *and* variation which bring about evolution, but the inheritance *of* variation, and this in turn is due to the general principle, of gene construction which causes the persistence of autocatalysis despite the alteration in structure of the gene itself. Given, now, any material or collection of materials having this one unusual characteristic, and evolution would automatically follow, for this material would, after a time, through the accumulation, competition and selective spreading of the self-propagated variations, come to differ from ordinary inorganic matter in innumerable respects, in addition to the original difference in its mode of catalysis. (*Variation* 35; emphasis in the original)

If Morgan gave a home to the gene, then Muller provided a portrait of what the gene should be, even though what he knew about the nature of the gene at that moment was only that "these genes exist as ultramicroscopic particles; their influences nevertheless permeate the entire cell, and they play a fundamental role in determining the nature of all cell substances, cell structures, and cell activities" (*Variation* 32). His analysis reduced evolution and heredity to the level of the gene and meanwhile demonstrated the importance of the gene concept: it is only on the gene that the variation could happen, and it is only through the gene that the variation could be inherited. The epistemic access of the gene was specified into four aspects in Muller's hands: first, the gene had independent primary functions; second, the gene is located at specific sites along chromosomes, third, the gene was capable of producing and maintaining, in some cases, mutations, and fourth, the gene was capable of duplicating itself (Falk, *Genetic Analysis* 132).

Moreover, Muller had profoundly changed the empirical methodology of genetic research. Instead of using breeding or hybridizing to understand the genotype, he was the first one who was capable of directing the experiments toward the gene itself, "the root of life" in his own words (Schwartz 240), so that he could manipulate the gene and then obtain the phenotype, altered or not. Both Morgan and Muller were empirical scientists. If Morgan was the master who could get the best out of the tools available, Muller was the apprentice who attempted to get what the tools could not handle yet. Muller succeeded even though Morgan disliked his research and distrusted his results (Schwartz 240). In 1927, Muller published in *Science* his "Artificial Transmutation of the Gene," a report of research that dealt with creating a series of artificial races in their chosen organisms by the use of X-rays, which could not obtained otherwise. In 1927, Muller addressed to the Fifth International Congress of Genetics in Berlin regarding the detailed account of his X-ray work. It was recognized as "a major turning point in the history of science. Man had for the first time willfully manipulated the genetic material" (Schwartz 241).

The Gene and the Research of Microbiology

When Muller struggled to obtain *Drosophila* mutants, he became acutely aware of how difficult it was to work with the fly for his purpose. Had not Mendel declared that "the value and utility of any experiment are determined by the fitness of the material to the purpose for which it is used" (2)? In his 1921 lecture, Muller predicted that future genetic research would make geneticists "grind genes in a mortar and cook them in a beaker. Must we geneticists become bacteriologists, physiological chemists and physicists, simultaneously with being zoologists and botanists? Let's hope so" (Falk, *Genetic Analysis* 232).

Genetic research went exactly where Muller predicted it would, and it happened when the development of microbiology offered wonderful organisms for the research. Microbiology started from as early as the late seventeenth century. Anton van Leeuwenhoek was the first one who saw single bacterial cells and other microorganisms that he called "wee animalcules," as he described in one of his letters:

> I have had several gentlewomen in my house, who were keen on seeing the little eels in vinegar: but some of them were so disgusted at the spectacle, that they vowed they'd never use vinegar again. But what if one should tell such people in the future that there are more animals living in the scum on the teeth in a man's mouth, than there are men in a whole kingdom? (Stanier et. al. 3)

The Royal Social invited Leeuwenhoek, a merchant who had little education and never attended a university, to communicate his observation to the members. A few years later, in 1680, he was elected as a Fellow.

Newton's contemporary Robert Hooke (1635-1703) published *Micrographia* in 1664, which attracted widespread attention to the microscope and its fantastic capacities. Hooke appreciated that the "use of *Microscopes*, and some other *Glasses* and *Instruments* that improve the sense," and "our senses were able to furnish us with an intelligible, rationall and true one [phaenomena of Nature])" (14, 193 emphasis in the original).

By the early nineteenth century, some scientists came to acknowledge the causal relationship between the growth of microorganisms and the chemical changes taking place in the same organic infusion. In 1837, French physicist Charles Cagniard-Latour (1777-1859), German physiologist Theodor Schwann (1810-1882) and German botanist Friedrich Traugott Kützing (1807-1893) independently proposed that the yeast was a microscopic plant, functioning to convert sugars to ethyl alcohol and carbon dioxide as the characteristic of alcoholic fermentation. This theory was bitterly attacked by the leading chemists of the time. It was French chemist Louis Pasteur (1822-1895) who convincingly demonstrated that all fermentative process are the results of microbial activity in his work *Memoir on the Organized Bodies which Exist in the Atmosphere* published in 1861. Later, during his studies on the butyric formation, Pasteur also discovered the existence of forms of life that could live only in the absence of free oxygen.

The study of anthrax, a serious infection of domestic animal and transmissible to human, led to the discovery that bacteria can act as specific agents of infectious diseases. In 1876, German country doctor Robert Koch (1843-1910) demonstrated the bacterial causation of anthrax by a series of experiments on mice.

Pasteur used simple, transparent liquid media of known chemical ingredients for the selective cultivation of fermentative microorganisms. Koch, on the other hand, adopted meat infusions and meat extracts as the basic ingredients in his culture media. Nutrient broth and its solid counterpart, nutrient agar, become the most widely used media in general bacterial work. This technique made it very easy to grow bacteria, especially at small scales, as well as controlling the growth conditions. Bacterial generation times vary from 12 minutes to 24 hours, much shorter than animals and plants. In addition to their relatively simple forms of life, all these made bacteria one of the best experimental objects.

In the 1940s, American geneticists George Wells Beadle (1903--1989) and Edward Lawrie Tatum (1909--1975) worked on bread mold *Neurospora crassa*. They used X-ray to induce wide variety of mutations that made *Neurospora* unable to carry out specific biochemical processes (1941). The genes controlling different steps of metabolism were mapped to different chromosomal locations, and they concluded that "The results also support the view that a one-to-one relation exists between gene and enzyme" (129, 1945). Fifty years later, the renowned American geneticist Norman Harold Horowitz (1915--2005) praises Beadle and Tatum's work as "one of the pivotal works of modern biology" and "a *Neurospora* revolution" that "change the genetic landscape for all time." Methodologically, they established "a new kind of experimental organism-a microorganism that was ideally suited for classical genetic studies but which differed from the classical organisms in that it grew readily on a medium of defined chemical composition" (127). Theoretically, the one-gene-one-enzyme relation was established, which exhibits the direction relation between genes and proteins—"a key to understanding the organization of living matter" (Horowitz 634). This is critical to the epistemic assumption of the gene term because at this stage, the gene is shown to be not only a physical but also a functional unit. In Beadle's words, "protein specificities are gene directed" (226, 1951).

The Chemical Nature of the Gene and the Research of Biochemistry

In 1944, American scientist Oswald Theodore Avery (1877-1955) and his two colleagues, Colin MacLeod and Maclyn McCarty, published their work on transforming a nonvirulent *pneumococci bacterium* strain into a virulent form by treating it with purified extract of nucleic acid extracted from virulent cells. They concluded:

> If, however, the biologically active substance isolated in highly purified form as the sodium salt of deoxyribonucleic acid actually proves to be the transforming principle, as the available evidence strongly suggests, then nucleic acids of this

> type must be regarded not merely as structurally important but as functionally active in determining the biochemical activities and specific characteristics of pneumococcal cells. (155)

Avery was reluctant to declare that DNA was the transformative molecule indicated by their experiment, because there was general acceptance that genes were a special type of protein molecule (Watson 12).

In fact, DNA was not a new discovery then. It had been discovered as early as 1869 by Swiss physician and biologist Johannes Friedrich Miescher (1844-1895), the first one working on the various types of proteins that made up the leucocytes, because it was believed then that proteins were the most promising targets for understanding how cells functioned. In the process of isolating and purifying proteins from pus on fresh surgical bandages, Miescher obtained a new, non-protein substance, which he described as “a material made up of only cells, such as this one, would above all finally call for a serious study of the chemical constitution of the cell’s nucleus” (Dahm 277, 2005). Two years later, he isolated the same material from salmon caught in the river Rhine. Due to this material presence in the nuclei, he named it “nuclein.” It was impossible to conceive, including by Miescher himself, that nuclein is the very material responsible for the heredity. He and many of his contemporaries favored the notion that that fertilization and subsequent embryonic development was achieved by the sperm cell, which upon contact with the egg, transmitted a motion stimulus intrinsic in the sperm cell’s molecular constitution. Miescher thought that nuclein might be the molecule that transmits this motion stimulus (Dahm 574, 2008). In 1882, Flemming suspected that “possibly chromatin is identical with nuclein, if not, … one carried the other” (Olins and Olins 809). Miescher’s student, German pathologist and histologist Richard Altmann (1852-1900) published his *Ueber Nucleinsäuren* in 1889, in which he described how he obtain protein-free nuclein, and based on his analysis, renamed nuclein nucleic acid (Choudhuri 306).

Regarding the discovery of DNA, German biochemist Ludwig Karl Martin Leonhard Albrecht Kossel (1853-1927) is regarded as "the inheritor of Miescher" (Lagerkvist 75). He was the first to isolate and identify adenine base (A) from extracts of pancreas in 1885, and then from yeast nuclein. Altmann's method helped Kossel advance his research. Kossel purified and identified guanine base (G) in 1891, thymine base (T) and cytosine base (C) in 1893, while he also developed a new method for nucleic acid purification (Jones 35). Kossel's research focus soon shifted to proteins, although he was still working on nucleic acids with a few of his students. He coined the word *Bausteine* (meaning building block) referring to his conceptualization of cell chemistry, that "the metabolism of the cell can be resolved into the linking together and the separation of a number of primary molecules, the *Bausteine*, by which the secondary complex molecules are synthesized and degraded" (Olby 76, 1974). In his own words:

> The multiplicity of the proteins is determined by many causes, first through the differences in the nature of the constituent *Bausteine*; and secondly, through differences in the arrangement of them. The number of *Bausteine* which may take part in the formation of the proteins is about as large as the number of letters in the alphabet. When we consider that through the combination of letters an infinitely large number of thoughts may be expressed, we can understand how vast a number of the properties of the organism may be recorded in the small space which is occupied by the protein molecules. *It enables us to understand how it is possible for the proteins of the sex-cells to contain, to a certain extent, a complete description of the species and even of the individual.* We may also comprehend how great and important the task is to determine the structure of the proteins, and why the biochemist has devoted himself with so much industry to their analysis. (1912, from Olby 77, emphasis mine)

Particularly, in the embryo more and more varied *Bausteine* were linked together to yield the complex proteins; whereas in the sperm these were removed, leaving the basic proteins. Kossel had profound influence on the research of hereditary material: he was the one who played the major role to establish the chemistry of nucleic acids and thus settled the relationship between nucleic acids and protein; on the other hand, he brought forth the conception that nucleic acids just did not appear sufficiently varied to account for biochemical significance. Based on what was known then that the *Bausteine* of nucleic acid contained one base and one sugar for each phosphorus atom, he thought "nucleic acid appears as a complex of at least 12 building blocks, but in the living cell the structure is probably larger, because some observations suggest that in the organs several of these complexes are combined with each other" (Kossel's Nobel lecture, 1910).

Kossel dominated nucleic acid chemistry until the entrance of Russian-American biochemist Phoebus Aaron Theodore Levene (1869-1940) at about the turn of the twentieth century. Levene discovered that there are two types of nucleic acid: one was obtained from yeast, and the other, calf thymus. His research indicated that the sugar moiety obtained by hydrolysis of the former is *D*-ribose, and that of the latter, desoxyribose. Moreover, the former had one base different: uracil instead of thymine (Levene and Bass 260). Since then the former was referred as ribonucleic acid (RNA), and the latter, deoxyribonucleic acid (DNA). Regarding the structure of DNA, Levene had the tetranucleotide theory--most historian and scientists call it tetranucleotide hypothesis today (Olby, *The Origins* 783)--that one nucleic acid contained equal quantities of the four bases, A, G, T, C (Levene 262). In retrospect, the hypothesis was criticized as a "scientific catastrophe," which not only "buried Miescher's work for decades more," but also "had the unfortunate effect of greatly restricting the possible variations of the nucleic acids" (Glass 229). In an interview in 1973, one of the authors of Avery's paper, McCarty did recall that "I was told by the late Colin Macleod that when he and Avery consulted Dr. Levene about the possibility that nucleic acids might be involved in the biological activity he discouraged them by citing the essential invariability of nucleic acids on the basis of the tetranculeotide theory of their structure" (Protugal and Cohen 84). Gunther Stent comments that Avery's discovery of the genetic role of DNA in 1944 was premature, similar to Mendel's. The delayed recognition of the significance is due to "the difficulty of comprehending how the monotonous molecule envisaged by the 'tetranucleotide' structure of the DNA, the only structure formulation available in the early 1940s, *could* be the carrier of hereditary information" (173, emphasis in the original).

At the end of the 1940s, a new purifying and quantitative analytical technique, paper chromatography and also a better experimental method to hydrolyze nucleic acid were developed in the lab of biochemist Erwin Chargaff (1905--2002), who emigrated to the US during the Nazi era. His data proved that the tetranucleotide hypothesis was "impossible" (Chargaff 28, 1950); meanwhile, it suggested a strong tendency that the ratio of A equaled to T, and G, to C (Zamenhof and Chargaff a 9, 1950). Moreover, the proportion of the two groups varied with the species (Zamenhof and Chargaff b 207, 1950).

The Structure of DNA, Virus and Biophysics

In 1951, When American zoologist James Watson, as a post-doctoral fellow, joined the research of English biophysicist Francis Harry Compton Crick (1916-2004), then a graduate student at Cavendish Laboratory at the University of Cambridge, both believed that "DNA would have to provide the key to enable us to find out how the genes determined, among other characteristics, the color of our hair, our eyes, most likely comparative intelligence, and maybe even our potential to amuse others" (Watson 13). As far as Watson concerned, those scientists who thought that the gene was protein molecules "were cantankerous fools who unfailingly backed the wrong horses" (Watson 13). In 1952, Watson got a long letter from American bacteriologist Alfred Day Hershey (1908--1997) to inform him "a powerful new proof that DNA is the primary genetic material" (Watson 72, 1980), that "one of the first steps in the growth of T2 [phage] is the release from its protein coat of the nucleic acid of the virus particle, after which the bulk of the sulfur-containing protein has no further function" (39). That is to say, the replication of viruses, to which bacteriophages belong, only relies on DNA rather than protein. At this point, virus became the crucial material for the research.

In the studies of bacteria, it was noticed by Pasteur and a few scientists that certain pathogens were able to pass through filters that otherwise stop bacteria. In 1886, German agricultural chemist Adolf Eduard Mayer (1843-1942) named a disease in tobacco capable of spreading among plants causing great loss of yield as "tobacco disease." Russian botanist Dmitry Iwanowski (1864-1920) demonstrated that the agent caused tobacco mosaic could pass through the bacteria-proof filter. One of Mayer's colleagues at the Agricultural School in Wageningen, Dutch botanist Martinus Willem Beijerinck (1851-1931) conducted the same experiment as Iwanowski, and reached the same conclusion. Still considering the agent bacterial, he further did many painstaking experiments that failed to identify any bacterial characteristics of the agent. Nevertheless, the agent could multiply in the living tissues of infected plants, especially rapidly in young tobacco leaves. It could also be stored for months, even in oil, or be precipitated with alcohol, without losing its infectious properties. Beijerinck eventually was brought to the conclusion that the cause of tobacco mosaic disease was not a bacterium but a *contagium vivum fluidum* (soluble living germ), as tobacco mosaic virus (TMV) in particular, which defined a liquid, soluble, self-reproducing, and subcellular entity (Kammen 1-5). Beijerinck's concept encountered strong resistance. In 1935, American biochemist Wendell M. Stanley (1904 -1971) published his "Isolation of a Crystalline Protein Possessing the Properties of Tobacco-Mosaic Virus," which ended up with a remark that "Tobacco-mosaic virus is regarded as an autocatalytic protein which, for the present, may be assumed to require the presence of living cells for multiplication" (162).

The discovery of bateriaphages, however, supported Beijerinck. In 1915, English bacteriologist Frederick William Twort(1877-1950) recorded that an agent causing colonies of *bacterium micrococcus* to become "glassy," as a bacterial disease, could be transmitted for an indefinite number of generations by successive passages from glassy to normal colonies, yet could not grow by itself in any media, nor could it cause the glassy transformation of heat-killed micrococci. French-Canadian microbiologist Félix d'Hérelle (1873-1949) studied another bacterial disease, diarrhea of locusts, the results of which was published in 1917 in the title of "An Invisible Microbe that is Antagonistic to the Dysentery Bacillus." He stated that:

> [I]n the absence of the dysentery bacilli the antimicrobe does not grow in any media. It does not attack heat-killed dysentery bacilli, but is cultivated perfectly in a suspension of washed cells in physiological saline. This indicates that the anti-dysentery microbe is an obligate bacteriophage. (374)

This was the first time this group of viruses hosted by bacteria were named *bacteriophage*. Later usage shortened it to *phage* (Stent, *Molecular Biology* 6). Beijerinck's conception of virus was proved, yet viruses were not liquid as he thought.

Virus as the simplest form of life provided a great convenience for research. When German physicist Max Delbrück (1906-1981) went to the Biology Division of California Institute of Technology with a Rockefeller scholarship in 1938, he was introduced to bacteriophage. Delbrück recognized immediately that phage would make ideal objects for studying the mechanism of biological self-replication. At outbreak of war in 1939, Delbrück remained in the United States in instead of returning to Germany. He became the head of new school of phage researchers, the phage group, who, in Stent's words, "not only changed the orientation and intellectual climate of phage research but also provided one of the main fountainheads of the then nascent molecular biology" (*Molecular Biology* 19). Rheinberger indicates that Delbrück's influence sprung from "his technical and organizational innovation which gave phages their place in the history of molecular biology: the introduction of quantitative techniques in the analysis of virus replication, the 'standardization' of the phage systems, … and the establishment of a network of international cooperation and exchange of information" (*A Short History* 10). Delbrück stated that:

> It is likely that its solution will turn out to be simple, and essentially the same for all viruses as well genes…. The study of the bacterial viruses may thus prove the key to basic problems of biology. (Rheinberger 9)

So, As Watson and Crick attempted to solve the structure of DNA, they knew that they were racing others who worked on the same project, including the prominent American chemist Linus Carl Pauling (1901-1994), for the Nobel Prize, because the structure of DNA would not only provide the most crucial chemical property of the gene but also suggest how the gene functions as hereditary material (Watson 106). Fortunately, Watson and Crick had the most important two pieces of data they needed: Chargaff's research hinted that DNA could have two strains, and British biophysicist and experimentalist Rosalind Elsie Franklin's[26] (1920-1958) X-ray crystallographic data suggested the structural should be helical (Watson 115, 45). In 1953, eventually, they published their proposal of the structure of DNA as a double helix with "novel features which are considerable biological interest" (a 737). Within a month, they publish

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[26] Crick, Watson, and Maurice Wilkins shared the Noble Prize of 1962 in Physiology or Medicine "for their discoveries concerning the molecular structure of nucleic acids and its significance for information transfer in living material." Dr. Franklin died of ovary cancer in 1958 at age of 37. Dr. Chargaff "wrote to scientists all over the world about his exclusion and became a bitter polemicist against molecular biology" (Judson *No Nobel*). More details about the story can be obtained mainly from Watson's *The Double Helix: A Personal Account of the Discovery of the Structure of DNA* (Norton Critical Edition), Branda Maddox's *Rosalind Franklin: The Dark Lady of DNA*, Wilkins' *The Third Man of the Double Helix: The Autobiography of Maurice Wilkins*.

another paper to elaborate the genetic implication of the double helix. The structure suggested that "spontaneous mutation may be due to a base occasionally occurring in one of its less likely tautomeric forms"; second, regarding the mechanism of genetic replication, "the template is the pattern of bases formed by one chain of the deoxyribonucleic acid and the gene contains a complementary pair of such templates" (b 737). It was evident at this point that the gene is a chemical named DNA with the structure of a double helix.

Stent applauded Watson and Crick's line of reasoning that they "for the first time introduced genetic reasoning into structural determination by demanding that the evidently highly regular structure of DNA must be able to accommodate the informational element of arbitrary nucleotide base sequence along the two poly nucleotide strands" (xvii). At this point, the significance of Avery's discovery also became evident. To put Carlson's notes of Mendel and Stent's note of Avery together, then Avery's is a "modest discovery," too. The epistemic assumption of the gene concept was wholly confirmed; thus it became epistemic assess—a part of knowledge, and accordingly the gene was a fact. In Falk's words, it is a "profound epistemological change" (*Gene* 326); to Stent, it is the birth of molecular biology, the time to think about genetics "in terms of large molecules that carry hereditary information" (xi).

Here is also the place where the phlogiston and the gene's fates radically diverge, even though as language these two terms shared the same problem with their meanings. In phlogiston's case, the search for its material reality fails; the concept is abandoned. In gene's case, on the contrary, its material reality is DNA; it survived. The discovery of the gene as DNA does not come from a single or simple line of research, so different disciplines, such as genetics, cell biology, biochemistry, and physical chemistry, have different perceptions as to how it was achieved. The history of the quasi-metaphor gene provides a new view through which it can be clearly described when and how the advances of the disciplines contribute to the discovery, such that a new coherence of scientists and disciplines emerges. Moreover, quasi-metaphors as a special category of language play a crucial role. Only as a quasi-metaphor can the gene term, on one hand, refer to the unknown cause of heredity as a virtual reality, and on the other hand, precisely project the epistemic assumption. Further, the discoveries are marked as the modifications or partial confirmations of the assumption. The vague meaning of the gene as a quasi-metaphor makes it possible to accommodate the continuous changes on the specification of its assumption. Every discovery makes the assumption more specific till the chemical structure of the gene is definitively established, which, in return, confirms the assumption and thus turned it into epistemic access. Boyd elaborates that the important epistemic access of the term *DNA* is:

> First, its use permits scientists to report to each other the results of studies of DNA.
> Second, its use permits the public articulation, justification, criticism, debate, and refinement (in the light of justification, criticism, debate, and experimentation) of theories concerning DNA, thus making the interpretation of data and the evaluation of proposed theories – as well as the reporting of results – into a social enterprise.
> Third, the use of the term "DNA" makes possible verbal reasoning concerning DNA with respect to questions of data interpretation, theory evaluation, experimental design, and so forth. That is, the use of language makes possible not merely the formulation of theories and publicity and cooperation in their assessment; it makes it possible for reasoning (whether individual or public) to be verbal reasoning: to take place in words. (Boyd 506)

Nevertheless, it should be particularly emphasized that, based on the analysis above, DNA is known through and only through the gene. More accurately, the articulated epistemic access regarding DNA should be that DNA is the chemical that makes the gene; or, the chemical nature of the gene is DNA. Without the gene, DNA is merely a chemical; without DNA, the gene is just a vague and abstract concept, nothing but a little word. Given the gene as DNA, then the gene makes sense, and DNA possesses a genetically specific role.

During a half century, *gene*, the little word yet a fundamental quasi-metaphor proposed by Johanssen, had acquired full epistemic access, from one discipline to another, from one hand of a scientist to another hand of another scientist, experiment by experiment, data adding data, until it becomes a scientific term referring to a fact. "So, what really happened to classical genetics? It went molecular" (Waters, *Genes Made Molecular* 184). From this point forward, the gene is not an abstract concept anymore. It has a physical content; it becomes material; it is DNA. The gene would live another life, a life that phlogiston never had a chance to live.

CHAPTER FOUR—Part II

Fruits of the Gene:
Productivity of a Quasi-metaphor

Long live the genome! So should the gene. (Falk 105, 2004)

The discovery of DNA structure as a double helix not only concluded the search for the chemical nature of the gene but also suggested a probable mechanism of heredity at a molecular level. It is considered one of the greatest moments in the history of science, although some historians like Olby argue that there were other contemporary discoveries just as important as that of the double helix, and it was only in retrospect this particular one is aggrandized, at jubilee celebrations especially, because of its "remarkable iconic value that has contributed significantly to its public visibility," as well as "a degree of notoriety attaching to the manner of its discovery and the characters involved" (*Quiet Debut* 91). Ironically, the notoriety arises from the recognition that its tremendous significance would guarantee a Noble Prize even before the discovery was made; to some of the characters, the research became a race to the prize, such that later the integrity and fairness of the process were questioned. As the last solid piece of evidence to prove the existence of the gene, the discovery decisively divides the quasi-metaphor and the scientific concept. It thus marks the end of search for the gene and the start of research on DNA, the end of the quasi-metaphor and the beginning of the scientific term.

Post double helix research in the past half century has resulted in "the unpredictable production of knowledge and the diffusion of practices" (Rheinberger, *A Short History* 5). After a half century, 2003 was "the year of clone": Dolly, the first cloned sheep, died in February, and whether human beings should be cloned was heatedly debated because the techniques and the whole human genome had become available (Clayton and Dennis 75). Meanwhile, the research methodology has been fundamentally altered. After the establishment of the chemical nature of the gene as DNA, the gene became an available material, like salt, sugar, or butter. To experimental scientists, their objects became the gene itself rather than phenotype, which had been the object since Mendel. Muller dreamed and attempted to work directly on the gene, though at his time what the gene was remained unknown. He and his colleagues could only use X-ray bombardment to obtain random mutations, and then screen the highly abnormal ones unlikely to have been obtained through hybridization. By analyzing the abnormal characters, they figured out what was exactly changed on the chromosome and further constructed chromosomal maps. It was massive and tedious work. Inasmuch as the gene had been proven to be DNA, and DNA could be easily obtained, research on the gene became straightforward and efficient. Scientists could experiment on DNA as creatively as they had conceived and then measured or observed the changes of the phenotype. In order to do so, they needed to develop new experimental techniques, as well as design proper instruments. Techniques and instruments became the key elements in research. The attention of the Noble Prize also turned to the inventions of new techniques, such as Polymerase Chain Reaction (PCR), DNA recombination, and DNA sequencing methods. In consequence, biotechnology became a profitable industry. On October 15, 1980, for instance, the biotechnology company Genetech experienced the fastest increase in the value of any stock in the history of the New York Stock Exchange: within 20 minutes of the start of trading, the price of shares in the company went from $35 to $89 (Glick and Pasternak 3).

Historians narrate the scientific research of this period mainly through the history of molecular biology. It is, however, “a multi-layered and complex process” that cannot be adequately described by either the merger of the disciplines that have contributed to the process, or the simple addition of yet another new biological discipline, or the isolated results of a few geniuses and their well-equipped teams belonging to separated research institutions (Rheinberger, *A Short History* 3-4). Nor can the process be characterized by a central theory but rather by "the use of an immense battery of techniques and a general approach to explaining—and altering—organismic function by reference to, and use of, an *omnium gatherum* of detailed molecular mechanisms" (Burian 67, emphasis in the original). The challenge of tracing the history of molecular biology thus is how to escape from narrowing the history down to the sociology of a few research institutions, or turning the history into that of a few ideas of great men, or wrapping the history in technological determinism. Rheinberger's *A Short History of Molecular Biology*, which covers its beginning in the early 1930s to the first steps into the age of genomics during the late 1980s and early 1990s, provides a dynamic interaction of ideas, instrumental innovation, and techniques. Michel Morange, on the other hand, approaches the history through what he defines as the "molecular revolution" of biology, including "the new conceptual tools for analyzing biological phenomena" forged between 1940 to 1965, and "the consequent operational control" acquired between 1971 to 1980 (Morange, *The History* 2). An alternative strategy is to pursue the history of the "unpredictable production of knowledge" instead of that of molecular biology. In Rheinberger's *An Epistemology of the Concrete: Twentieth-Century Histories of Life*, he elegantly argues that the productivity of the gene lies in "its epistemic and operational plasticity in the search for unprecedented knowledge rather than in its rigorous and unambiguous definition" (151). Here I will extend his claim further to argue that the productivity of the gene has two distinctive modes, the quasi-metaphorical and the scientific, divided by the discovery of the double helix. The staggering increase of knowledge in the post double helix era is achieved through the research on, not a single line or a single discipline, but dispersive quasi-metaphors, each of which follows the model of the quasi-metaphor gene, drawing on advances from multiple disciplines to prove, or eventually disprove in some cases, their epistemic assumptions. In contrast to the striking convergence of developments from various disciplines to substantiate the material reality of the gene in the previous part, this part traces a rather amazing divergence of advances departing from the knowledge that the gene is DNA, which also is the point of departure for molecular biology. Between the double helix discovery and the completion of the Human Genome Project, the history of the gene concept is recounted through crucial quasi-metaphors, such as *information*, *gene action*, *gene function*, *genetic code*, and *genome*, which derive from the gene, as well as *transcription* and *translation* from *gene action*. In doing so, those diverse understandings and also confusion regarding the gene concept are explained by the interactions of the conceptions of the unknown, the research methodology and biotechnology.

Molecular Biology and the Gene

Many scholars recognize that molecular biology is "a hybrid science combining experimental systems from biophysics, biochemistry, and genetics, among others" (Rheinberger, *An Epistemology* 157). When the European Molecular Biology Organisation (EMBO) was established, the executive committee members and their disciplines were: crystallography (Max Perutz and John Kendrew from the United Kingdom), microbiology (François Jacob from France and Ole Maaløe from Denmark), biochemistry (Hans Friedrich-Freksa and Adolf Butenandt from Germany), biophysics (Edouard Kellenberger from Switzerland, Charles Sadron from France and Arne Engström from Sweden), physical chemistry (Alphonso Liquori from Italy and Ephraim Katchalski from Israel), embryology (Jean Brachet from Belgium) and genetics (Adriano Buzzati-Traverso from Italy) (Strasser 516). Such an impressive combination makes the question even more important: what is molecular biology that it can provide coherence to such a wide range of diverse disciplines?

The definition of molecular biology has never reached a consensus. Although Morange writes *A History of Molecular Biology* as "a history that was as complete as possible," he is aware that "the content and the history of molecular biology are difficult to define" (Morange, *The History* 4, 2). He describes molecular biology thusly:

> Molecular biology is not merely the description of biology in terms of molecules — if this were the case, it would include not only biochemistry, but also all those nineteenth-century studies in chemistry and physiology that led to the characterization of biological molecules. With such a broad definition, even Pasteur would have been a molecular biologist. Rather, molecular biology consists of all those techniques and discoveries that make it possible to carry out molecular analyses of the most fundamental biological process—those involved in the stability, survival, and reproduction of organisms. (1)

Morange's main concern with respect to molecular biology is to what the modifier *molecular* refers. Yet *molecular analyses* is not a clear concept, either, because analysis at the organic or biochemical level is molecular. In addition, "fundamental biological process" covers almost all topics in biochemistry, cell biology, and genetics. For example, the activity and mechanism of enzymes, undeniably "fundamental biological process," belonged and still belongs to biochemistry. Such description, as Rheinberger observes, is broad to "the point of a tautology" (*What Happened*, 304). Nonetheless, Morange puts chronological boundaries on what he and others call the "molecular revolution," which are "the new conceptual tools for analyzing biological phenomena [that] were forged between 1940 and 1965" and "the consequent operational control [that] was acquired between 1972 and 1980" (2). In this context, it is understood that molecular biology is the revolutionary approach in biological study that formed between 1940 and 1980.

To Olby, there are broad and narrow conceptions of molecular biology: the broad one was introduced in the 1930s and 1940s, in which "biological function was to be accounted for in terms of structure going right down to the molecular level" and the narrow one, after 1950s, which was about the "association between structural and genetic mechanisms" (*The Molecular Revolution*, 503-504). Similarly, Rheinberger holds that molecular biology has "a double status": "on one hand, it represents a specialized field (molecular genetics) within the framework of the other biological disciplines; on the other, it is a general experimental and theoretical paradigm which is spreading throughout all of biology" (*A History* 2).

With respect to the developmental stages of molecular biology—"shifts" is Rheinberger's word—there are a few major benchmarks on which scholars basically agree: the establishment of the double helical structure of DNA, the deciphering of the genetic code, the capability of biotechnology, and the prevalence of the genomic projects, especially the completion of the Human Genome Project, although they do not necessarily divide the period strictly according to them. Rheinberger is the one who promptly grasps that molecular biology "has ceased to exist as a narrowly confined discipline, leading to a quasi-dispersed existence as an arsenal of methods pervading all of the life sciences"; in other words, molecular biology dissolves as a discipline after the completion of the Human Genome Project (*Recent Science*, 9).

The origin of the term molecular biology was once credited to William Thomas Astbury (1898-1961), English biophysicist, who had used the term since 1945. He did pioneer research on the X-ray diffraction of proteins[27] and DNA, saying that he did so "if only for the sake of molecular biology, where perhaps more than anywhere else the great future of X-ray analysis

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[27] *Protein* is a macromolecule composed of one to several polypeptides. Each *Polypeptide* consists of a chain of amino acids linked together by peptide bonds (Snustad and Simmons 735).

lies" (Hess 664). Yet Warren Weaver acted quickly to set the record straight, that it was he himself who first used the term (582). The term appeared in his annual review of Rockefeller Foundation of 1938. As the Director of the Natural Sciences Division at that time, he acutely observed the emergence of "[a] new biology, …small but significant," in which the techniques and instruments well developed in a chemical or physical laboratory opened "a door to a biology of molecules," such that biological phenomena can be described and analyzed "not in terms of cell as units, but in terms of genes and other critical and important subdivisions of cells, … and even in terms of molecular structures and forces" (35). His conception of molecular biology is a projection of research methodology--successful techniques and instruments--of physics and chemistry to biological materials, assuming that they could bring advances to the field[28]. Weaver's conception of molecular biology was fairly vague, and the gene is the only kind of "molecular" that he specified as the "critical and important" objective of molecular biology. What the gene exactly was still remained unknown at that time, and the popular speculation was that genes were proteins, one of the objects of study of biochemistry (Tanford and Reynolds 40). Robert Kohler indicates that the scientists whose grants were approved by Weaver did not engage in DNA research; rather, most of his projects of molecular biology indeed belong to biochemistry, like the research applying ultracentrifuge to proteins (Kohler 345). Weaver provided a list of techniques and instruments that he thought important to molecular biology: X-ray, ultracentrifuge, electrophoresis, electronic microscope, electronic diffraction, "tagged atom," and double stream refraction. X-ray diffraction turned out to be the technique by which the structure of DNA was elucidated. Nowadays, centrifuge and ultracentrifuge of difference sizes

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[28] Abir-Am identifies *molecular biology* as a metaphor (*The Discourse* 346), and *molecular* as a "metaphoric adjective (*The Discourse* 396).To me, Weaver's conception of molecular biology echoes Lavoisier's chemistry, and both are quasi-metaphors.

and rotating speeds are regular instruments used for biomaterial separation, and DNA gel electrophoresis is a routine experiment. "Tagged atom," namely labeling one or more atoms by its/their radioactive isotope/s in a chemical, is also powerful method, like in DNA-DNA or DNA-RNA hybridization. As useful as they are in other disciplines, electron and double stream diffraction on the list have not had much to do with molecular biology.

According to the *Oxford English Dictionary*, however, even Weaver was not the first to use the term. It first appeared in 1884 in the *American Journal of Science*, in the review of a book, *The Law of Heredity: A Study of the Cause of Variation, and the Origin of Living Organisms* written by William Keith Brooks[29]. The reviewer brilliantly summarized the essence of the book:

> Heredity is the leading word of the title; but the second part of the title gives the key to the essay. It is a supplement to Darwinism on the speculative side, a contribution by a trained zoologist and comparative anatomist, with a genius for speculation, to what may be called molecular biology. The author understands natural selection—its weak points as well as its strong ones—is naturally attractive to pangenesis, and has built upon it his new theory of heredity; we should say rather of the cause or origin of variation. (157)

In this context, *molecular* means "molecular level." Influenced by the advances of chemistry, Brooks came to the idea that Darwin's theory could be well explained at a molecular level. Though the earliest, this meaning was rejected in the light of the twentieth century science, and Morange clearly explains why in his conception of molecular biology. The linguistic difference between Brooks' molecular biology and Weaver and Astbury's is that Brooks' is a noun modified by an adjective describing the specificity of the noun; whereas the latter is quasi-metaphor as a projection of the operational mode dealing with molecules to biology. In Weaver and Astbury's conception, *molecular* refers to molecules which are essential to living organisms,

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[29] William Keith Brooks (1848--1908) was an American zoologist, as well as mentor of Thomas H. Morgan.

particularly the gene before any other molecules. The conception of molecular biology emerged when the conception of the gene became relatively sophisticated. Astbury's prediction with regarding to the usefulness of X-ray as an analytical tool is accurate. Only eight years later, Franklin's X-ray diffraction of DNA provided Watson and Crick the most important data to establish the double helical structure. Hence to a historian like Morange, the formation of molecular biology occurred because geneticists "no longer satisfied with a quasi-abstract view of genes, focused on the problem of the nature of genes and their mechanism of action" (*The History* 2). To Rheinberger, the gene has been "a fluctuating object of molecular biology" (*Experimental Complexity* 247).

As long as what the gene is stays unknown, however, molecular biology at most is a plausible speculation. Once the gene is proved to be DNA, then all the marvelous techniques and instruments on Weaver's list find themselves a chemical to work on, the very one that determines, more than any other countable elements, every living being. It is the reason why Stent dates the beginning of molecular biology as April 25, 1953, the day when Watson and Crick's paper of the double helix was published in *Nature* (Watson, *The Double Helix* xi). Olby devotes his book, *The Path to the Double Helix*, to the process in which the clear knowledge of DNA comes to exist. To him, DNA is the principle focus of molecular biology. His approach has been criticized of "seeking to reduce the rise of molecular biology to a single event" ever since the book was published in 1974 (Abir-Am, *"New" Trends* 170). To the rise of molecular biology as to the history of gene, the establishment of the double helix is a decisive event, rather than a single event, because since then, molecular biology had gained its operational or material objective. "The Watson-Crick structure was, if not the most important, at least the most revolutionary for it provided the basis for coherently explaining gene specificity, gene replication and gene mutation," remarked S. Brenner at 1959 (804). Molecular biology coheres in all techniques and instruments by working on DNA, as well as uniting various disciplines through their research on DNA. A story told by Crick himself and quoted by Stent perhaps is convening annotation:

> I myself was forced to call myself a molecular biologist because when inquiring clergymen asked me what I did, I got tired of explaining that I was a mixture of crystallographer, biophysicist, biochemist, and geneticist, an explanation which in any case they found too hard to grasp (Stent 160).

Hence, molecular biology is the research on DNA as genetic material and other chemicals related to DNA in *vivo* or in *vitro*. There is an ironic twist in the conception of molecular biology that, strictly speaking, neither a gene nor DNA is a molecule. DNA is "a macromolecule composed of a long chain of deoxyribonucleotides joined by phosphodiester linkage" (Snustad and Simmons 725). The collection of DNA that is characteristic of an organism is named its genome. Both do not have a fixed molecular formula, or do not have the smallest physical unit; they are themselves units. Nature outwits human imagination and conception again. The *molecular* of *molecular biology*, epistemologically, serves as a reminder of the limitation of human knowledge regarding life at the beginning of the twentieth century, and also serves at the same time as a celebration of the breaking of the obstacle by the work of numerous scientists. In short, we know more now because we are able to see the limitation when we guessed that the gene was a molecule.

Alternatively, to re-narrate the history between the discovery of the double helix and the completion of the human genome project is to exhibit the mode of knowledge production. In this new, productive, and unpredictable mode of dispersion of quasi-metaphors, moreover, there are two interweaving lines of research on DNA concerning what can gene do and what can be done to the gene. As early as 1927, Muller applied X-rays to fruit flies to study chromosomes, when "most classical geneticists were for at least the first three decades of the twentieth century busy selecting spontaneous mutations and performing extensive crossing and breeding experiments" (Rheinberger, *An Epistemology* 159). Weaver's proposal for molecular biology could be considered as an extension of Muller's approach. They mainly pursue what could be done to the gene; in other words, to manipulate the gene so as to understand what gene can do. Nonetheless, what can be done to the gene is inconceivable without the knowledge of what gene can do. Hence, one cannot develop without the other, and together, they bring up the glory as well as controversies regarding the gene.

Gene Action

After 1953, the gene was seen as a chemical with the structure of a double helix. On the first page of the national British newspaper *News Chronicle* at May 15, 1953, twenty days after Watson and Crick's paper, the report regarding the discovery of the double helix ends this way:

> On four groups of elements, according to their arrangement, depend the characteristics passed from generation to generation.
> No one suggests these groupings can yet be arranged artificially. Discovering how these chemical 'cards' are shuffled and paired will keep the scientists busy for the next 50 years" (Olby, *Quiet Debut* 402).

The four groups refer to the four bases of DNA, A and T, G and C. The doubts on how four simple chemicals together could function as hereditary material were still lingering, and the journalist surmised that it should take scientists a half century to figure it out. It indeed took less than ten years.

In 1958, Crick published his *On Protein Synthesis*[30], which was first delivered as a

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[30] By synthesis, Crick meant biosynthesis, namely the synthesis in the cell or *in vivo*. The first totally chemically synthesized protein is crystalline bovine insulin, accomplished by Chinese scientists in 1966 (Zou, "Chemical Synthesis").

lecture at University College London in September 1957 (Strasser 492). Lily Kay aptly comments that, "[i]n a single masterly stroke, Crick encapsulated the imperative logic of the genetic code and the ideology and experimental mandate of the new biology: genetic information, qua DNA, was both the origin and universal agent of all life (proteins)" (30). Crick's paper began with Watson's remark that "[t]he most significant thing about the nucleic acids is that we don't know what they do.' That is to say, even after it was proved that the gene was DNA, how DNA functions as hereditary material was still largely unknown. Crick wanted to "persuade you [the reader] that protein synthesis is a central problem for the whole of biology, and that it is in all probability closely related to gene action" (161). This paper clearly marked the turning point from searching for the gene to searching for gene action; or, from gene-centered research to gene-action-centered research. In the context, the gene was a scientific term interchangeable with nucleic acids; whereas *gene action* was a new quasi-metaphor. Similar to the quasi-metaphor gene, *gene action* was a term to express the conceptualization of the unknown mechanism of nucleic acids, in Crick's words, that "the nucleic acids are in some way responsible for the control of protein synthesis, either directly or indirectly" (144). Through framing the mechanism of gene function as gene action, the genetic material was perceived as an autonomous agent, "the gene acts to produce its end product," so to speak in 1929 (Gewon 360). In this conception, the gene is "capable not only of animating the organism but of enacting its construction" (xiv, 1995), as Keller observes, although she takes *gene action* as a metaphor.

The term *gene action* had appeared in academic publications since 1926 according to a *Google Scholar* search. Before the gene was identified as DNA, it was a general term referring to gene products, chromosome product, phenotype, or gene effects. For instance, Morgan talked about gene action in these terms: "[s]o far as we can judge from the action of mutated genes, the kind of effect produced has as a rule no relation to location of the gene in the chromosome" (314, 1934). It was only after the determination of the genetic material that *gene action* could be a quasi-metaphor, which specifically refers to the unknown mechanism of DNA as genetic material.

In Werner Maas' historical account of gene action, DNA-centered research from 1940 to 1965 is presented as the period of "building a theoretical framework of gene action" (71). To Crick, gene action mainly is to control the synthesis of protein, while "[t]he main function of proteins is to act as enzyme" (*On Protein Synthesis* 138). During 1951 to 1953, English biochemist Frederick Sanger published a series of papers in which he established the complete amino acid sequence of insulin, the smallest protein, as well as the method for determining the amino acid sequence of any protein. So the chemical nature and order of the building blocks of proteins were well understood. It had been more than three decades, however, since American biochemist James Batchellor Sumner (1887-1955) first claimed in 1926 that the urease (an enzyme) he purified and crystallized from the jack bean possessed the properties of protein (Sumner 435); it was later proved by another American biochemist John Howard Northrop's (1891-1987) work on crystalline pepsin (an enzyme) that "the enzymes are proteins" (Northrop 755; Tanford and Reynolds 164-175). There were no doubts that enzymes were a miraculous kind of protein that could catalyze chemical reactions, yet to state that the main function of proteins was to "act" as an enzyme suggests that a protein could choose whether to be an enzyme; or, more precisely, a protein can control its own function. It was discovered later that the activity of enzymes was controlled by outside conditions. The use of *act* reflects the very limited knowledge about enzyme activity then.

Regarding how the synthesis of protein was controlled by the gene, Crick proposed a theory, the central dogma, in which "the transfer of information from nucleic acid to nucleic acid, or from nucleic acid to protein may be possible, but transfer from protein to protein, or from protein to nucleic acid is impossible" (*On Protein Synthesis* 138). In his elaboration of the central dogma, Crick outlined the most updated experimental data regarding protein synthesis, and more importantly, what still should be pursued: first was the relation between the DNA sequence of a gene and the amino acids sequence of the protein from the gene; and then the role of RNA in protein synthesis, because it was known that RNA was involved in protein synthesis and carried at least part of the information that determined the composition of a protein.

Information

Information is the most far-reaching term, if not the most important, in the post double helix era. Similar to *molecular biology*, *information* is a profound quasi-metaphor that defines a discipline as well as a way of thinking. It is also a concept that keeps evolving. The relation between information and the gene are complicated: the word *information* has been so involved in the research of the gene since the double helix structure was proposed that information seems a part of molecular biology. Along the process, newly developed theories of information have been applied to the research of molecular biology from time to time. Further, the way in which information shapes the conception and research of molecular biology is a unique and revealing angle to study the history, as Kay does in her *Who Wrote the Book of Life*.

The informational way of thinking in biology, to philosopher John Maynard Smith, can be dated back to as early as German biologist August Weismann (1834 -1914). In his *The Evolution Theory* published in 1904, Weismann wrote:

> As these primary constituents are quite different from the parts themselves, they would require to vary in quite a different way from that in which the finished parts had varied; which is very like supposing that an English telegram to China is

> there received in the Chinese language. (63)

Smith comments that Weismann's thought is remarkable because he draws "an analogy with a specific information-transducing channel, the telegram," and also "recognizes that heredity is concerned with the transmission of information, not just of matter or energy" (182). In the context, however, Weismann's "argument is in a sense fallacious," because he could not articulate the relationship between acquired characteristic and mutated characteristic. To him, if a blacksmith develop strong arm muscles, such a trait transfers to his sperm cell as well as the egg fertilized by the sperm, so that his son also has muscles.

Sahotra Sarkar credits Watson and Crick for the explicit usage of *information* in molecular biology for the first time when they proposed the double helix structure:

> The phosphate-sugar backbone of our model is completely regular, but any sequence of the pairs of bases can fit into the structure. It follows that in a long molecule many different permutations are possible, and it therefore seems likely that the precise sequence of the bases is the code which carries the genetical information. If the actual order of the bases on one of the pair of chains were given, one could write down the exact order of the bases on the other one, because of the specific pairing. Thus one chain is, as it were, the complement of the other, and it is this feature which suggests how the deoxy-ribonucleic acid molecule might duplicate itself (*Genetical Implications* 965).

This is one of the most elegant and succinct deductions from molecular structure to biological mechanism, according to my professor of Advanced Organic Chemistry in graduate school. When no one had any idea how DNA functioned as hereditary material, Watson and Crick projected that, like any information system, DNA must make it possible for heredity to be kept in code as well as transmitted through duplication. *Genetical information* as a quasi-metaphor helped frame and depict the yet unknown hereditary function of DNA, and another quasi-metaphor *code* further specifies how. The epistemic assumption is the ubiquity of information systems, even in biology. *Information* is itself a quasi-metaphor. Imported into molecular biology, *information* becomes a quasi-metaphor of a quasi-metaphor, even vaguer and more flexible as well. Hence, on one hand, discussions on information and the gene have to specify which information theory and which definition of the gene. For instance, with respect to the gene and information in developmental biology, the discussion goes to which concepts of information--the causal or the semantic/intentional[31]--the gene belongs. Moreover, the term *information* is so accommodating as to be used anywhere, from hereditary information to genetic information, phenotype information, sequence information, and genomic information.

The dawn of the information age, moreover, is Kay's "novel vantage point" for recounting the works on the genetic code between 1953 and 1970 (xv). Through "the metaphorical nature of information theory," she discerns that "[w]hen applied metaphorically to biological phenomena, 'information' becomes even more problematic," because "it becomes a metaphor of a metaphor, a catachresis, and a signifier without a referent" (21, 24). Here is a clear illustration of the difference between the metaphorical and quasi-metaphorical approaches. Treating *information* as a metaphor of a metaphor, Kay's studies comes to the conclusion that information discourse in molecular biology is "the linguistic hall of mirrors" from which even she cannot escape. As a quasi-metaphor of a quasi-metaphor, however, what *information* brings into molecular biology is the methodology. In the case of genetic code, cryptology particularly was applied to frame the unknown relationship of DNA and heredity and helped to solve the

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[31] *Causal information*: Information flows over a *channel* connecting two systems: the *receiver*, the system that contains the information, and the *sender*, the system that the information is about. There is a channel between two systems when the state of one is systematically causally related to the state of the other—when we can infer the state of the sender from the state of the receiver. *Semantic/intentional information*: many thoughts possessed by intelligent beings are about things with which they have only the most tenuous causal connection or about things that do not exist. The relation between thoughts and things is called *intentionality* or *aboutness*. Thoughts contain intentional information (*intentionally content*) about the objects of thought. (Sterelny and Griffiths 100 to 105, emphasis orignal)

problem. The referent of *information* is the unknown relationship rather than nothing, as it would be if treated as a metaphor in Kay's fashion. With regard to those I take as quasi-metaphors, there is always this difference basically between Kay and me.

When Crick proposed the central dogma, he realized that "information means here the precise determination of sequence, either of bases in the nucleic acid or of amino acid residues in the protein" (*On Protein Synthesis* 153). In other words, heredity is DNA sequence, although how DNA sequence determined protein sequence and other phenotypic features was still unknown. DNA therefore does not carry genetic information; rather, a DNA sequence is the information. At this point, the quasi-metaphor information should be replaced by the scientific term sequence. Yet *DNA sequence* is a term too specific and technical to be grasped by the ordinary audience right away. *Information*, on the contrary, is so easy and flexible by its enormous vagueness that "the flow of information" can be used to substitute any explanations in the process from DNA to protein. For instance, transcription is the flow of genetic information from DNA to RNA, and translation, that from RNA to protein. Predrag Šustar's close reading of *On Protein Synthesis* and related works of Crick concludes that "the 'central dogma' and other related generalizations are assertions about particular causal regularities, generalizations which, strictly speaking, do not require a reference to the notion of genetic information" (23). The word *information* is used in the article because the intended readers are "non-specialists," as Crick himself claims (138, 152). Waters agrees. To him, the syntheses of RNA from DNA and protein from RNA can be precisely delineated in any advanced textbook of molecular biology by "causal terms" "without essential appeal to notions of information" (*Molecular Made Biological* 542). That is to say, *information* after Crick's definition becomes a metaphor which can be used to bridge the specialists' and laymen' understandings.

Such is the case in general. The confirmation of the epistemic assumption of a quasi-metaphor validates its position in the epistemic categories, sometimes even establishing new epistemic categories, such as Lavoisier's chemistry and Weaver's molecular biology. With a valid position in an epistemic category, these scientific terms like other words can be used as metaphors. Along with (genetic) information, the gene is also an excellent example. Richard Dawkins' master piece *The Selfish Gene*, published in 1976, takes the gene as a social being, and narrates the characteristics and functions of the gene though its social life. In *The Meanings of the Gene*, Celeste Condit analyzes hundreds of stories from American magazines and television news from the 1910s to the 1990s and identifies three groups of gene-based metaphors: the gene as determiner of fate, that gene as a means for discriminatory ends, and the gene as a tool to produce perfect children.

Genetic Code

According to Morange, the Austrian physicist Erwin Schrödinger (1887 -1961) was the one of the first who used *Code* to describe the role of the gene (75). In *What Is Life* published in 1944, he wrote:

> It is these chromosomes, or probably only an axial skeleton fibre of what we actually see under the microscope as the chromosome, that contain in some kind of code-script the entire pattern of the individual's future development and of its functioning in the mature state. Every complete set of chromosomes contains the full code; so there are, as a rule, two copies of the latter in the fertilized egg cell, which forms the earliest stage of the future individual. (20)

To Schrödinger, the gene was "the hypothetical material structure underlying it [chromosome]" (28-29). The word *contain* suggests that while Schrödinger knew that chromosomes (or their axial skeletal fibres) were where the hereditary material was, he did not know yet that chromosomes carried or were the material. Further, without knowing what the gene was, "code-script" was quasi-metaphor describing his speculation of the relationship between chromosomes and the individual developed from the chromosomes. Schrödinger's conception had profound influence, as Watson recalled:

> …[A] widely-read book entitled *What Is Life*?, written by the physicist Erwin Schrodinger, then already very famous. In retrospect, the most important point made by Schrodinger was that the gene is to be thought of as an information carrier. And the only reasonable way in which genes could he imagined to carry their hereditary information is by embodying a succession of a small number of different repeating elements, or symbols, whose exact pattern of succession represents an encoded genetic message. Schrodinger illustrated the vast informational capacity of such a coding system with an example that used the two symbols of the Morse code—dots and dashes. (xiii)

Watson clarified that Schrödinger projected the methodology of cryptology in order to understand how heredity functions. Watson also mentioned that "[a] major factor in his [Crick's] leaving physics and developing an interest in biology had been the reading in 1946 of *What Is Life?*" (13). Morange praised Schrödinger because he "dared to say what no geneticist would have said," and his "physicist's farsighted vision, allowing him [Schrödinger] to see genes merely as containers of information, as a code that determines the formation of the individual" (75). Kay, however, insists the influence of the book is largely Watson's reconstruction done in the 1960s, in which "Whig mythologies spun around Schrödinger's *What Is Life?* as *the* precursor to the genetic code have obscured the historical nature of his own preoccupations and the scientific and social context of the 1950s: the new world picture within which 'genetic decoding' took place" (4-5, emphasis in the original). Yet regarding the quasi-metaphor code, Schrödinger's text is the best at articulating what is known, what is unknown, and what can be known through cryptology, which is exactly the inspiration that Watson and Crick drew from it.

The knowledge that the gene was DNA with four different bases narrowed the epistemic assumption of the quasi-metaphor code, and cracking the code of life became feasible. The "definite proposal" was first made by Russian born American physicist George Gamow (1904-1968) in 1954, who suggested solving the relation between DNA and proteins through the double helical structure, and later, through a cryptographic approach. His conception of the quasi-metaphor code was the unknown relation between "the four-letter DNA alphabet and the twenty-letter protein alphabet [of basic amino acids]" (Gamow 63, 1956). The epistemic assumption was that there must be a fixed relation between the sequence of DNA and that of proteins. He visualized the relation as "key-and-lock" in protein synthesis: each amino acid was inserted into a diamond-shaped cavity formed by a pair of nucleotide bases, and then polymerized along and around the axis of the double helix, showed in Figure 4II-1.

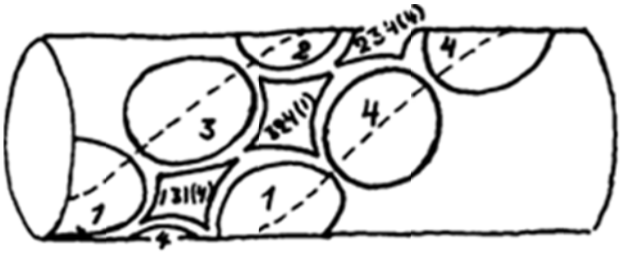


Figure 4II-1: Gamow's diamond system

As a physicist, Gamow's biology was "ludicrously patchy" (Judson 258). The first obvious problem with the diamond system was that, against experimental data, RNA became unnecessary.

In 1952, British biochemist Alexander L. Dounce suggested in his paper *Duplicating Mechanism for Peptide Chain and Nucleic Acid Synthesis*, based on his analysis of the chemical bond formation and energy flow to peptide chains, that "nucleic acid would function as a template or distributing agent in governing the order of arrangement of amino acid in peptide chains as well as the order of nucleotides in nucleic acids" (257). In the next year, he published another paper *Nucleic Acid Template Hypotheses*, in which he stated that there was "the template sequence deoxyribonucleic acid (DNA) – ribonucleic acid (RNA) – protein" in protein synthesis (541). That is to say, the amino acid sequence of a protein was determined by the specific sequence of its corresponding RNA, which, in turn, was determined by the specific sequence of its corresponding DNA. RNA therefore must play an important role in protein synthesis. The second problem was that the four nucleotide bases did not seem to be capable of structuring twenty different diamond-shaped cavities accommodating twenty amino acids. Third, the diamond system lacked a control of the orientation of protein sequence. Lastly, the site of synthesis contradicts experimental data. By Gamow's hypothesis, protein had to be synthesized in the nucleus where the diamond system was supposed to be, but experimental data showed that proteins were produced in both cytoplasm and nucleus, depending on the cell type (Brachet and Chantrenne 335). Nonetheless, Gamow's diamond system contributed two crucial conceptions for solving the coding problem: the nucleic acids sequence provides a template for assembling amino acids into proteins; or, more specifically, three bases coded one amino acid.

When Watson and Crick examined Gamow's hypothesis, they spotted one more problem. They thought Gamow's list of twenty amino acids was partially wrong, so they came up with a list of "magic twenty" amino acids from which natural proteins were synthesized, although when asked how they knew the magic twenty were right, Crick said, "we didn't know! Just time has been shown we guessed correctly" (Judson 260). In 1957, Crick published "Codes without Commas." Differently from Gamow, Crick worked on the "coding problem" through a mathematical approach to answer "how a sequence of four things (nucleotides) can determine a sequence of twenty things (amino acids)" (416). He explained that only triplets of nucleotides, that is, 64 (4 x 4 x 4) sets, could be possible codes. Moreover, non-overlapping codes were the most probable, whereas overlapping codes were highly unlikely, and partial overlapping codes were not impossible according to experimental data (Figure 4II-2).

The next problem was that 64 non-overlapping triplets were too many to code 20 amino acids. Crick proposed that only some triplets were to code the twenty amino acids, or have "sense," while the rest never got "sense" if they were read. He wrote:

> We shall assume that there are certain sequences of three nucleotides with which amino acid can be associated and certain others for which this is not possible. Using the metaphors of coding, we say that some of the 64 triplets make sense and some make nonsense. We further assume that all possible sequences of the amino acids may occur (that is, can be coded) and that at every point in the string of letters one can only read "sense" in the correct way.

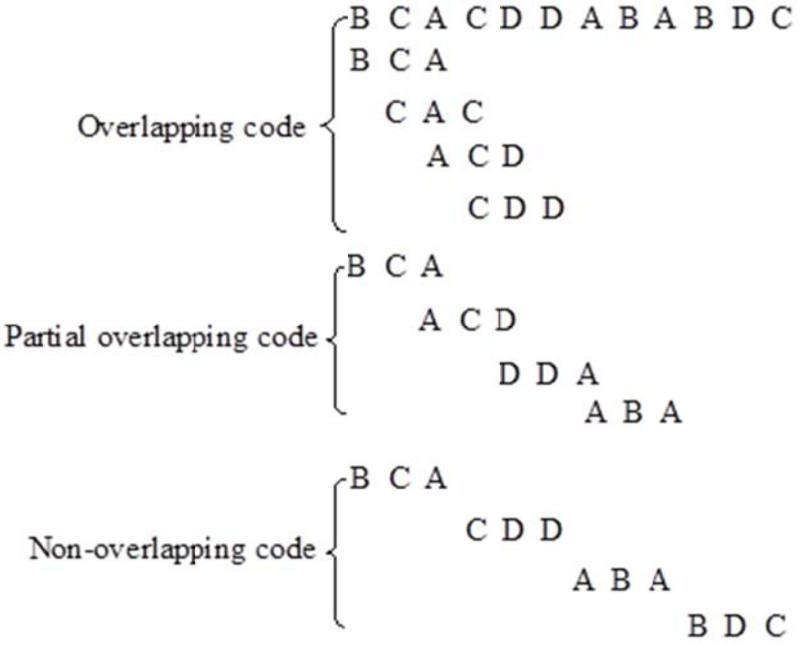


Figure 4II-2: Crick's three classes of codes. The letters A, B, C, and D stand for the four bases of the four common nucleotides. The top row of letters represents an imaginary sequence of them. In the codes illustrated here each set of three letters represents an amino acid. The diagram shows how the first four amino acids of a sequence are coded in the three classes of codes. (*On Protein Synthesis* 160)

Illustrated in Figure 4II-3, any two triplets which make sense can be put side by side, but the overlapping triplets so formed must always be nonsense.

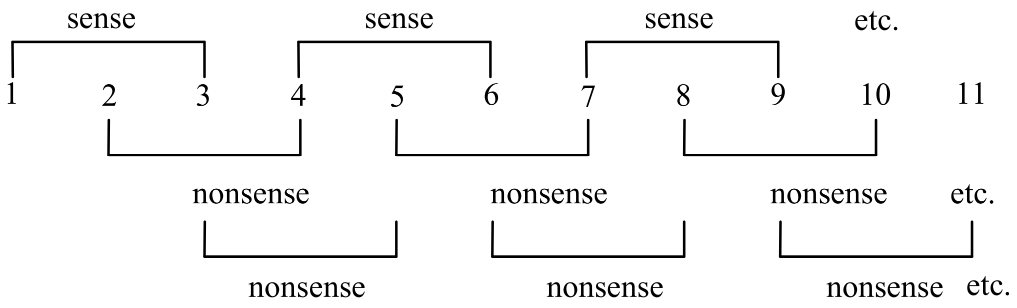


Figure 4II-3: Crick's sense and nonsense codes. The numbers represent the positions occupied by the four letters A, B, C, and D. It is shown which triplets make sense and which nonsense. (*Codes* 418)

Then one of such code of twenty triplets could be written compactly as:

$$A \quad B \quad \begin{matrix}A\\B\end{matrix} \qquad \begin{matrix}A\\B\end{matrix} \quad C \quad \begin{matrix}A\\B\\C\end{matrix} \qquad \begin{matrix}A\\B\\C\end{matrix} \quad D \quad \begin{matrix}A\\B\\C\\D\end{matrix}$$

$A \quad B \quad \begin{matrix}A\\B\end{matrix}$ means that two of the allowed triplets are ABA and ABB. In such a coding system, "no sequence of these allowed triplets will ever give one of the allowed triplets in a false position," so it is comma-less (Crick, *On Protein Synthesis* 160). Moreover, the four single-base triplets—AAA, CCC, GGG, UUU—had to be nonsense.

Crick borrowed Gamow's quasi-metaphor code to conceptualize the correlation between nucleic acid and protein sequences because both he and Gamow agreed with the epistemic assumption. By what he called *coding*, he projected the known categories of cipher codes to the unknown relationship of nucleic acid and protein sequences. He spoke of *coding* as a metaphor (*Code without Commas* 214), but I consider it a quasi-metaphor, for the coding system that he tried to work out then was the unknown, or virtual. Through the analysis of chemical bonds of DNA and protein as well as energy flow, "[w]ithout invoking metaphors of coding, information transfer, or even numerological logic," Dounce came to conclude not only that a minimum of three bases could determine one amino, but also that the synthetic process of DNA to protein should work via RNA, which was basically Crick's central dogma (Kay 70).

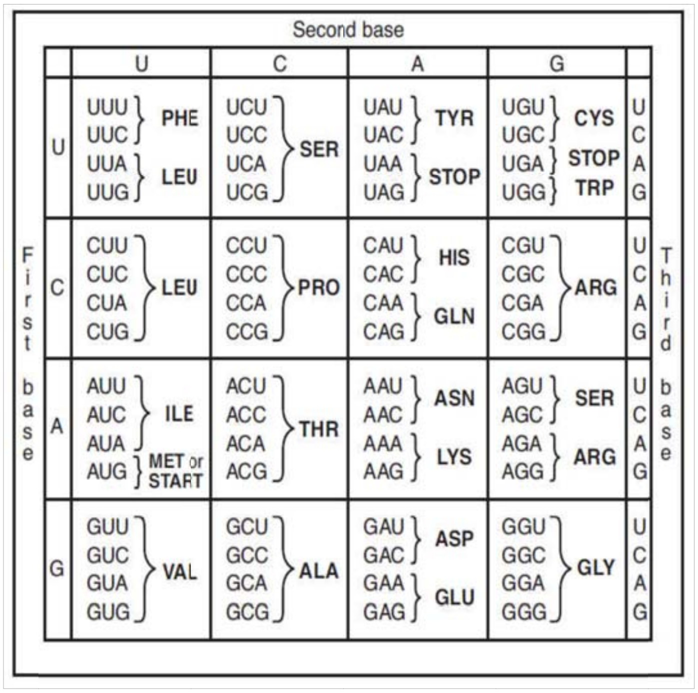
To me, the major difference between Crick and Gamow, is that Crick's approach is quasi-metaphorical and Gamow's, systematic. More specifically, Crick's deduction is based on cryptology, and Gamow's, the bonding relations of DNA and amino acids (peptide bond). Gamow got the 3:1 ratio right, even though the synthetic process that he imagined was totally wrong due to lack of more empirical data, for he did not know that RNA was certain to be involved in protein synthesis. Gamow didn't do anything more on DNA function after that paper, but Crick stuck to it, so no one knows how Gamow's system would have worked, or self-corrected.

The parallel approaches of Gamow and Crick demonstrate that there was more than one approach to conceptualize and solve the unknown relationship between DNA and protein sequences. Crick's is quasi-metaphorical, namely, employing cryptology, a system outside molecular biology; whereas Dounce focuses on the logic within molecular biology, which does not involve any quasi-metaphors at all. That Dounce's approach worked out the same conclusion suggests that Crick's had to be quasi-metaphorical. A code is a rule for converting or representing a form into another. For instance, 2516190119091422110 in the rule of the numeric order of the 26 English is "ypsasinuj." Without the code, no one could ever work out what this number represents. In other words, the logical coherence of the number exists outside its form such that its form does not give away another form "ypsasinuj." Similarly, if Crick's *code* had been a metaphor, namely, the relationship between DNA sequence and protein sequence exists as 251619011909142110 and "ypsasinuj," then the approach should have been only Crick's—to crack the code. Dounce's approach, in which he reached the same conclusion as Crick's by the structure of proteins, that is, the logical/theoretical coherence of protein system itself, without introducing the rule, should have not worked. Moreover, that *coding* is a quasi-metaphor was exactly why some of Crick's speculations are proved to be right and some wrong by experimental data. By uses *coding* instead of *code*, Crick hints his conception is more about approach, and admittedly, Crick's quasi-metaphorical approach has its advantages. Dounce's only produces a specific hypothesis; whereas Crick's suggests a system that produces a constellation of new concepts and terms, including the central dogma. From a virtual projection of the relation between DNA and protein as a coding system to the establishment of the concept code and the epistemic category belonging to it, such as codon, transcription, translation, and so on, *coding* is one more excellent example of a quasi-metaphor's productivity.

In 1961, American biochemist Marshall Warren Nirenberg (1927-2010) and German biochemist J. Heinrich Matthaei, a post-doctoral fellow in Nirenberg's lab then, published their new experiment in their *E. coli*-cell free protein-synthesis system, a stable *in vitro* system invented to conduct experiments that could only be carried out in living cells (*in vivo*) in the past. In order to understand how the gene determined its protein, they worked out a new and efficient system to do things to the gene. Their result was that "polyuridylic acid [that is, UUU] stimulated the incorporation of L-phenylalanine alone" (1596). In other words, UUU coded for L-phenylalanine. UUU is the first cracked code. It proved that Crick's coding system was the right solution; but his nonsense codes were not totally correct, because Crick deduced that UUU is senseless so it should not exist. Following the experimental system of Nirenberg's lab, more codes were discovered in different labs. In 1963, Crick published a review of the progress, *The Recent Excitement in the Coding Problem*, in which he named the codes codons:

> This is a group of bases that code for one amino acid. In simple codes a codon is a fixed number of consecutive bases, e.g., in a 'triplet' code it is three consecutive bases, but it is possible to conceive codes in which, for example, some codons consist of two bases and others of three. Again it is not certain that the bases making up a codon are adjacent on the polynucleotide chain. (166-167)

The newly coined word *Codon* had very precise meaning as a triplet of nucleotides that code for an amino acid, or an initiation, or a termination of such a signal. Similar to the case of DNA for the gene, the codons are the material reality of the coding for Crick or code for Gamow. By 1965, all most all of the genetic codons had been determined, showed in Figure 4II-4.



	Second base								
First base	U		C		A		G		Third base
U	UUU	PHE	UCU	SER	UAU	TYR	UGU	CYS	U
	UUC		UCC		UAC		UGC		C
	UUA	LEU	UCA		UAA	STOP	UGA	STOP	A
	UUG		UCG		UAG		UGG	TRP	G
C	CUU	LEU	CCU	PRO	CAU	HIS	CGU	ARG	U
	CUC		CCC		CAC		CGC		C
	CUA		CCA		CAA	GLN	CGA		A
	CUG		CCG		CAG		CGG		G
A	AUU	ILE	ACU	THR	AAU	ASN	AGU	SER	U
	AUC		ACC		AAC		AGC		C
	AUA		ACA		AAA	LYS	AGA	ARG	A
	AUG	MET or START	ACG		AAG		AGG		G
G	GUU	VAL	GCU	ALA	GAU	ASP	GGU	GLY	U
	GUC		GCC		GAC		GGC		C
	GUA		GCA		GAA	GLU	GGA		A
	GUG		GCG		GAG		GGG		G

Figure 4II-4: The complete table of codons. *Start* means start codon, and *Stop*, stop codon, the triplets as the initiation and termination of translation, respectively.

The establishment of the code system as the codons in the table is the end of the quasi-metaphor code or coding, and the beginning of the scientific term of (genetic) code, which has a clear and precise definition: "the correspondence between triplets in DNA (or RNA) and amino acids in polypeptide" in the words of a textbook of *Genes* (774).

Kay does not distinguish the quasi-metaphorical and the scientific status of the terms; rather, she treats them as metaphors. Regarding genetic code, she argues that:

> From linguistic and cryptanalytic standpoints, the genetic code is not a code: it is simply a table of correlations, though not nearly as systematic or predictive as the periodic table, for example, because of contingencies, degeneracies, and ambiguities in the structure of the so-called genetic code. These culturally animated imaginaries, nevertheless, have persisted, making it now seem inconceivable that genes did not always transfer information, or that the relation between DNA and protein could be something other than a code. Yet, there were (and probably could be) other ways of knowing. These particular representations were historically specific and culturally contingent. The genetic code is a "period piece," a manifestation of the emergence of the information age. (2)

In brief, she argues that the scientific concept *genetic code* is far too simple as *code* in a cryptic system, and the relation between DNA and protein sequences is also far more complicated than the genetic code, so that *genetic code* is incapable of representing the complex relationship between DNA and protein, which, as she is aware, still has to be represented in the word *code*[32]. The quasi-metaphor code, as a projection of a cryptic system, obviously is influenced by the information age, yet what is projected is a fixed relation between "the four-letter DNA alphabet and the twenty-letter protein alphabet" (Gamow 63, 1956). The epistemic assumption was that there must be a fixed relation between the sequence of DNA and that of proteins so that DNA as

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[32] "[W]hile engaging in a critique about the language of DNA, I have tried, whenever possible, to avoid using the very terminology scrutinized in this study—"information," "language," "code" —or at least to signal its metaphorical status and historical usages. Ultimately, these overlapping resonances set a limit on linguistic resolution. So my book, too, as with the genomic "Book of Life," is subject to the same complications of discursive origins and their context-dependence. It has not always been possible to escape from this linguistic hall of mirrors." (xix)

hereditary material predetermines the sequence of protein. The codons turn the *code* to *genetic code* as a table of correlations, which is what Gamow and Crick wanted to know. The simplicity of the table should not be a negative criterion to judge genetic code—codons are only supposed to tell the correlation between four DNA bases and twenty amino acids—nothing more and nothing less. (The periodic table is produced through another fundamental quasi-metaphor *element*, which should not be put in comparison with genetic table—the two *tables* have different functions[33].)

Further, Kay knows that research quickly found cases in which the pairings of the table were imperfectly realized, especially in eukaryotic cells. The experimental system used to establish the genetic code was *E.coli* (Rheinberger 22), a prokaryote whose translation and transcription differ from that of a eukaryote (cell with nucleus). So DNA that is not coded to protein or its relation with protein is not precisely coded, such as an intron[34] of a eukaryote, is not a scandal but rather reveals the limitation of the experimental system of working only with prokayrotes rather than a limitation of genetic code per se. By treating quasi-metaphor *code* and scientific concept *genetic code* as metaphors, Kay over-emphasizes their vagueness and overlooks their capacity to acquire precise meanings. In a dynamic view, those identified as quasi-metaphors do have a stage of vague meaning, but once their epistemic assumptions are approved by empirical data, they are turned into scientific terms, as precise and clear as scientific language can be. Her inability to escape from these terms while arguing their contingencies, degeneracies, and ambiguities is not because those terms belong to "the linguistic hall of mirrors;" rather, it is because they are not metaphors as she held them to be but scientific

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[33] I will discuss the functions of *table* in science in later work.

[34] Its definition is on page 31.

concepts that once were quasi-metaphors.

Gene Activity and the Central Dogma

Ironically, research on the synthesis of protein from DNA turned out to be much more complicated than Crick's conception. When Gamow proposed his hypothesis of the diamond system, he also suggested another quasi-metaphor, translation:

> [T]he enzymes (proteins), the composition of which must be completely determined by the deoxyribonucleic acid molecule, are long peptide chains formed by about twenty different kinds of amino-acids, and can be considered as long 'words' on a 20-letter alphabet. Thus the question arises about the way in which four-digital numbers can be translated into such 'words' (318).

Gamow conceived the process of protein synthesis, in which the amino acid sequence of a protein was predetermined by DNA, as a "translation procedure" of one language to another during which the meaning or information remained. The epistemic assumption was that the amino acid sequence of a protein must be made out of its corresponding DNA sequence as heredity. That is, in some way, one sequence equaled the other, although one had twenty building blocks, and the other, four.

Further research gradually uncovered the far more complex process, mainly regarding the involvement of RNA. The work of American virologists Eliott Volkin and L. Astrachan on bacteriophage in 1956 demonstrated that, after *E. coli* was infected by a phage, a new class of RNA was synthesized with the similar base composition to the bacteriophage's DNA (149). After 1957, the American biochemist Arthur Pardee collaborated with two French biochemists, François Jacob and Jacques Monod, to research the genetic control of galactosidese[35] synthesis. Later, their work was referred as the "PaJaMo Experiment," famous for not only the

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[35] Galactosidase is an enzyme that digests galactosides; that is, it catalyzes the hydrolysis of galactosides into monosaccharides.

fundamental change that it brought to the view of gene action, but also the delicate design of the series of experiments that has served as model for experimental scientists ever since. In 1961, Jacob with English molecular biologist, Sydney Brenner, and American molecular biologist, Matthew Stanley Meselson, identified "an unstable intermediate carrying information from genes to ribosomes for protein synthesis," which they named "messenger" RNA (576).

In the same year, Brenner reported his research in the Cold Spring Harbor Symposium, and he re-defined translation as such:

> [T]he information encoded in DNA must somehow be transmitted to the ribosomes where it is translated into the amino acid sequence of a polypeptide chain. … Hence the notion emerged that each gene is responsible for the synthesis of specific RNA copies of itself, which, after being incorporated into ribosomes, act as working templates for protein synthesis (101).

Translation became from RNA instead of DNA sequence to protein sequence. RNA has four bases just as DNA, except that it has a U where DNA has a T; in other words, the sequence of DNA and its corresponding RNA were practically the same, and RNA to protein maintained the four letters to twenty letters conveyance of heredity information. The epistemic assumption remained the same.

Regarding the relation between DNA and RNA, Jacob and Monod proposed another quasi-metaphor, transcription in their presentation:

> According to modern concepts, the deoxynucleotide sequence which constitutes a gene participates in two distinct chemical processes. In the first, for which the term *replication* should be reserved, free deoxyribonucleotides are linearly assembled by specific base-pairings, forming an identical sequence or replica of the original sequence; the second process, which we shall call *transcription*, allows the gene to perform its physiological function, i.e., to specify the molecular structure of a certain protein or polypeptide chain. …..Two stages may then be distinguished in transcription, the first of which is presumably closely similar to replication, involving, however, ribonucleotides instead of deoxynucleotides, and resulting in an RNA "transcript" of the original DNA sequence. In the second

> transcription stage, the RNA transcript in turn directs the assembly of amino acids into the polypeptide (193, emphasis in the original).

The transcription quasi-metaphor was apt, as well as consistent with translation. The epistemic assumption was that the hereditary information was conveyed from DNA to RNA through copying one sequence to the other, as if to the transcription of a text. At this point, the process from DNA to protein was understood as four bases DNA to four bases RNA and then to twenty amino acids.

Research on transcription and translation has developed further complexities for the Central Dogma. In 1970, two independent research labs published in the same issue of *Nature* about a reverse transcription: Howard Temin and Satoshi Mizutani's data demonstrated that "an enzyme that would synthesize DNA from an RNA template might be present in virions of Rous sarcoma virus" (1212); while David Baltimore's research showed that such an enzyme existed in the virions of both Rauscher mouse leucaemia virus and Rous sarcoma virus. They named it "RNA-dependent DNA polymerase" (1209). On the same issue, the editorial review *Central Dogma Reversed* introduced the significance of this discovery. Mainly, it "has a profound implication for the whole of molecular biology, as well as for the mechanism of cancer induction by RNA viruses." It was also "an extraordinary personal vindication for Dr. Howard Temin" for he put forward the hypothesis as early as 1964. Overall, it proved that the central dogma was "a considerable over-simplification" (1198).

For his own defense, Crick published a paper on next issue of *Nature* titled *Central Dogma of Molecular Biology*. As a direct response to the editorial review, he pointed out that "[t]his is not the first time that the idea of the central dogma had been misunderstood in one way or another." The he explain why the term was originally introduced:

> The central dogma was put forward at a period when much of what we now know in molecular genetics was not established. All we had to work on were certain fragmentary experimental results, themselves often rather uncertain and confused, and a boundless optimism that the basic concepts involved were rather simple and probably much the same in all living things. In such a situation well constructed theories can play a really useful part in stating problems clearly and thus guiding experiment. (561)

This little passage could stand as a manifesto for theoretical scientists. Trained as a physicist, Crick's approach is that of theoretical science in which he theorizes on experimental data of other scientists. According to Olby, Crick's not being Mendelian was exactly his strength among his contemporaries who worked on biology at molecular level, for "[m]any ideas which were already floating around were picked up by Crick, who recognized the really important ones and made these precise, then presented them as fundamental generalizations" (*Francis Crick* 978). In the midst of experimental scientists who were trained to follow data, Crick was the one who tried to come up with theories capable of summarizing what is going on, and his best was the diagram for the central dogma (Figure 4II-5).

Through the central dogma, Crick subtly shifted gene- action-centered research to protein-synthesis-centered research. Crick proposed the central dogma as a theory, which is promised as epistemic access and held for its generalization and accuracy. His defense, that the central dogma does predict the two special transfers, DNA to DNA and DNA to RNA, should occur, and that he never suggests RNA to DNA cannot occur (562, 1970), does not appear strong. Had scientists followed the central dogma faithfully, had Howard Temin not "stuck to his gun in the face of widespread skepticism," it could have been exactly what Johannsen was against when he proposes the gene term, "putting a horse in the locomotive as a cause of its motion" (*Central Dogma Reversed* 1198). In other words, Mendelian scientists would have not defined any terms precisely without experimental data, because they might have gotten cause and effect totally wrong. Without further research, what Crick called “the central dogma” should be the possible synthesis pathway from DNA to protein. The main point of Mendelian methodology, that experiments should produce data to articulate facts rather than being driven by theories, makes the quasi-metaphor especially important. In the course of his research in molecular biology, Crick seemed to become conscious of the problem of his approach, as he stated in an interview in 1968, “I have always had great difficulty in publishing theoretical ideas in a vacuum of evidence, partly because if you are a professional theoretician as I am, you cannot afford to publish too many wrong ones” (Olby, *Francis Crick* 965). If a theory is quickly and frequently proved to be imperfect on this or that issue, then it loses its status as theories. The vagueness of a quasi-metaphor disqualifies its proposal as a theory, but gives an open end for

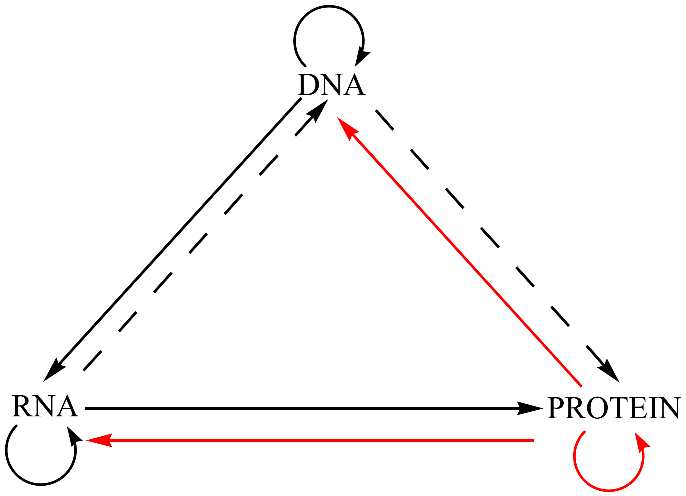

Figure 4II-5: Crick’s central dogma.

> The arrows show the situation as it seemed in 1958. Solid arrows represent probable transfers, dotted arrows possible transfers. The red arrows[36] represent the impossible transfers postulated by the central dogma. They are the three arrows starting from protein (561, 1970).

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[36] Crick had another diagram to indicate all possible transfers, in which the impossible transfers are absent arrows. I used the red color arrows instead.

revision, as long as what discovered is not against the epistemic assumption.

Today, the central dogma is neither central nor dogmatic, but still important, because protein synthesis is still one of the major activities in living cell. It thus also renders a "classic molecular gene" concept that "identifies a gene with a stretch of DNA that codes for one of the polypeptide chains that goes to make up a functional protein" (Stotz, Griffiths, and Knight 649). In this definition, the structural unit is also the functional unit of the gene, and together with the simple and elegant diagram of the central dogma, they are an effective start to teach undergraduate students genetics and gene function. The central dogma of gene action led Crick to hold the epistemic assumption of a fixed relationship between DNA and protein sequences, so that he participated in the research with great passion. Despite the errors and simplifications, the fast progress on cracking the codes does owe its success to Crick's prediction of triplet code from DNA bases to amino acids. The quasi-metaphors helped articulate and communicate his conceptions.

Quasi-metaphor gene action became problematic in 1960 when it was discovered that some genes coded repressor or activator proteins that could control one or more genes, usually at the transcription level. Such genes are now named "regulatory genes." To differentiate, any other genes that code a protein are named "structural genes" (Holtzer, Abbott and Holtzer 1541). This discovery profoundly changed the understanding of gene action. In their presentation of *On Regulation of Gene Activity*, Jacob and Monod concluded:

> The discovery of units of coordinated genetic activity and of regulator genes which control the activity of structural genes,… appears to offer precisely the type of elements needed to build the complex and precise chemical networks of information transfer upon which the development and physiological functioning of organisms must rest. (208)

The gene could not act. The epistemic assumption of gene action that the gene mainly functions alone to produce proteins, the most important biological material, turns out to be not wrong, but too simple, even naive upon reflection with what is known today. This simpleness, however, faithfully recorded how little was known about gene function when the double helix was discovered.

Instead, the gene exhibited activity that was regulated by other genes as “biochemical coordination” (208). The last drop of mystery, the least residue of vitalism, the weakest sense of agency hinted in gene action faded away with the term. Life could be depicted through “chemical networks” in which every chemical, even as quintessential as the gene, cannot be active and functional without working in coordination and cooperation with other chemicals. The term has gradually been replaced by “gene activity.” Henceforth, the gene is studied in term of its function.

Keller also makes the same observation; as mentioned in part I, she considers both *gene* and *gene action* metaphors. She remarks that, though a bad metaphor and useless concept, the gene introduces more metaphors into the disciplines, one of which is gene action, which is a “guiding metaphor” that “bracket[s] the question of development” and “help[s] define the approach” for the American school of Morganian genetics, because the metaphor is “capable not only of animating the organism but of enacting its construction” (1995, xiv). In my view, the quasi-metaphor gene was productive as research was focused on nailing the chemical nature of the gene, meanwhile *gene action* was a convenient synonym of phenotype, or the effects of the gene in the organism. Only after the gene was known to be DNA could *gene action* be a quasi-metaphor to specify a virtual category of gene’s role to control protein synthesis. With respect to the questions of what lent the discourse of gene action such persuasiveness in developmental biology for so many years, and why is it now giving way, Keller's states:

> The simplistic answers might go like this: embryology languished because it was bad and unproductive science; we talked about gene action because we didn't know better; indeed, developmental phenomena are so difficult to study that real progress was impossible until the advent of techniques of recombinant DNA that molecular biology has brought (*Refiguring Life* 32).

Discussing gene action in embryology provides an excellent opportunity to study the developmental interactions between concepts like the gene, gene action and the disciplinary knowledge. The period Keller identifies in the history of embryology when "we talked about gene action because we didn't know better" are two periods under the lens of quasi-metaphors: the period when we did not know what a gene was, such that we used gene action to refer to hereditary traits, and later when we knew what a gene was, such that gene action was a quasi-metaphor to indicate gene's sole control of protein synthesis, which was too narrowly specific to bring better understanding to embryology. The reason to abandon the term *gene action* is pragmatic to Keller--the availability of gene manipulative techniques; whereas to me, it is the better understanding of the gene by its activity, which leads to an even more profound quasi-metaphor gene function, along with the epistemic assumption that the activity of a gene is related to or dependent upon other chemicals, including other genes. The rapid pace of DNA research on genetic code and gene action by the end of 1960s brought the gene another definition, "a code residing on nucleic acid that gives rise to a functional product" (Gerstein et al 670). This is clearly a broadening of the scientific concept of a gene, since what the gene "gives rise to" is any functional product, not just a polypeptide. The conception basically summarizes the research on the role of the gene rather than guiding it.

Genome in Sequencing-Centered Period

In 1961, Jacob and Monod published another PaJaMo experiment paper, *Genetic Regulatory Mechanisms in the Synthesis of Proteins*, in which they reported "[t]he synthesis of enzyme in bacteria follows a double genetic control. The so-called structural genes determine the molecular organization of the proteins. Other functional specialized, genetic determinants, called regulator and operator genes, control the rate of synthesis." The chemical networks of information transfer became more complex and precise. It was the time to ask, what exactly was or contained the whole set of DNA in a living being? They ended their paper with these two paragraphs:

> The fundamental problem of chemical physiology and of embryology is to understand why tissue cells do not all express, all the time, all the potentialities inherent in their genome [that is, in the totality of their genes]. The survival of the organism requires that many, and, in some cases most, of these potentialities be unexpressed, that is to say *repressed*. Malignancy is adequately described as a breakdown of one or several growth controlling systems, and the genetic origin of this breakdown can hardly be doubted.
>
> According to the strictly structural concept, the genome is considered as a mosaic of independent molecular blue-prints for the building of individual cellular constituents. In the execution of these plans, however, co-ordination is evidently of absolute survival value. The discovery of regulator and operator genes, and of repressive regulation of the activity of structural genes, reveals that the genome contains not only a series of blue-prints, but a coordinated program of protein synthesis and the means of controlling its execution (354, emphasis in the original).

The word genome was used to refer to "complete single sets of chromosomes" in 1926 for the first time (Huxley 580). In the context of Jacob and Monod's passage, it meant the complete set of DNA sequences of the genetic material of *E. coli*, or any organisms. While what the genome is like and how exactly the genome functioned to make the cellular constituents as a living system were unknown, they projected the structure of a living cell as that of a building and the whole sets of cellular chromosome sequence as the blue-print of the building, which not only contained the information of the building blocks but also instructions for making these building blocks in timely order. In their conceptualization of the genome, they embedded a fundamental idea that the two dimensional genomic sequence stores a five dimensional system, including a three dimensional functioning system, a timing system, as well as regulatory system.

It was precisely because of this idea that the genome was a quasi-metaphor instead of metaphor. The epistemic assumption of the quasi-metaphor *genome* was that the genomic sequence contained all of what was needed to guide the development of a functional being. Had not timing and regulatory systems been included, genome as a blue print would have been a metaphor, because the cell was a building. There were blue prints that could indicate the order to put building blocks; however, there had never been any blue prints could tell the timing of building each part, not to mention regulating each part by the real conditions. This somewhat awkward conceptualization of the unknown function and mechanism of the genome indicates the limited knowledge on the relation between an organism and its genome at that time.

In 1977, a Belgian research group identified a small genome region of a viral DNA sequence, which consisted of triplets that can be translated into amino acids starting with an initiation codon and ending with a termination codon. They named it an “open reading frame” (ORF). (Contreras *et al* 1001). ORF became a useful concept in DNA research, especially in relation to another term, *gene expression*. The use of *gene expression* can be traced back to 1930, according to Oxford English Dictionary. In a short article studying *Drosophila*, gene expression is the synonym of phenotype (406). When the synthesis from DNA to protein had been understood, the term acquired a definition as the process by which genes produce RNAs and proteins and exert their effects on the phenotype of an organism. Not all ORFs can be expressed, but all proteins must be the expressions of ORFs. An ORF is a gene if expressed. In genomic research, a sequenced genome is usually first put in comparison with other known genes[37]. If a piece of DNA sequence in the genome is identified as an annotated ORF and can be expressed, in *vivo* or *vitro*, it is named a gene. ORF is a pragmatic way to define the gene[38]given ready, reliable sequence.

Also in 1977, almost twenty five years after his establishment of an amino acid sequencing method, Sanger published his DNA sequencing method, *Nucleotide Sequence of Bacteriophage ΦX 174 DNA*, which brought him a second Nobel prize and made him the fourth and only living person in the world to have been awarded the prize twice. The abstract of this article is below:

> A DNA sequence for the genome of bacteriophage ΦX 174 of approximately 5,375 nucleotides has been determined using the rapid and simple 'plus and minus' method. The sequence identifies many of the features responsible for the production of the protein of the nine known genes of the organism, including initiation and termination sites for the proteins and RNAs. Two pairs of genes are coded by the same region of DNA using different reading frames.

The significance is clear and succinct: bacteriophage ΦX 174 genome is totally sequenced as the first complete genome in history. The length is about 5, 375 nucleotides, and the method applied is "plus and minus." The use of the adjectives *rapid* and *simple* suggests the more important achievement, the establishment of the "plus and minus" method--it not only leads to the sequencing of the genome, but also is rapid and simple in comparison with the existing methods. That is to say, what is really significant is how the sequencing is done, although the sequencing of the genome is already a great achievement. The reliability of the sequencing data, as well as the sequencing method, is also verified by their consistency with the empirical data of other

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[37] The Gene Bank, namely a database that contains the DNA and RNA sequences of all proteins identified. Software that can annotate any DNA sequences or genomes is also available.

[38] A detailed process will be discussed in Chapter 6.

scientists. Moreover, the sequencing data reveals a magnificent natural phenomenon previously unknown, that the "striking feature of the ΦX 174 sequence is the way in which the various functions of the genome are compressed within the 5, 375 nucleotides." More specifically, the DNA sequence that codes gene B is contained within gene A, and E within D. Or, two ORFs were coded by the same regions of DNA through different reading start points. Sanger then concludes that "the genome has more coding capacity than had been originally supposed on the assumption that each gene was physically separate." Once more, Nature surprised the scientists. Although it was widely assumed that the codons are non-overlapping, yet the linear sequence of ORFs/genes could be over-lapping. This phenomenon hinted that at the genomic level, genes could be organized in unexpected ways. The editorial review in the same issue, entitled *ΦX 174 DNA Sequenced*, explicitly described the value of Sanger's discovery, that " [f]or the first time it is possible to look at a viral genome as a giant molecule of known structure, all function of which should be accessible to interpretation in terms of its molecular structure (Szekely 685). Sanger has been highly regarded in the scientific community, as "a man who worked with hands, at the bench," and whose work is "elegant and surprising" (Judson, *Frederick* 21). His approach echoes that of Mendel and Johannsen: he "began to explore the genome of diverse organisms without any prior conception on which to base his discoveries and without any clear idea of how he was going to find out what he wanted to know" (González Quirós and González Villa 3 to 4). This is the Mendelian counterpart to Crick's claim of the utility of strong models.

In 1978, American biochemist Walter Gilbert, another giant in DNA sequencing who shared the Nobel Prize with Sanger, claimed that "[o]ur picture of the organization of genes in higher organisms has recently undergone a revolution….in general the coding sequence on DNA, the regions that will ultimately be translated into amino acid sequence, are not continuous but are interrupted by ‘silent’ DNA.” He suggested name “a transcription unit containing regions which will be lost from the mature RNA” as intron, whereas any segment of an interrupted gene that is transcribed into mature RNA, exon (501). In prokaryotes like bacteria, whose genes have the simplest form, the DNA sequence accurately corresponds to the amino acid sequence of its protein; or, DNA bases number is exactly three times of amino acids number of the protein. In eukaryotes, however, a gene contains extra pieces of sequence, introns, interrupting the sequence code of the protein. Structurally, the intron differentiates eukaryotic and prokaryotic genes, and the function of introns remains a mystery.

Sanger’s method practically opened the door to the era of genomic-centered research, in which gene function is explored both as individual activity and its relation with other genes. In 1995, the first sequence of the genome of a free-living organism, *Haemophilus influenzae*, was published. The length of genome is 1.8 million base pair (mpb).

In 1997, the genome of *E. coli* was determined as 5 mpb.

In 2000, the genome of fly *Drosophila melanogaster* was sequenced, 180 mpb

In 2001, a 2.91 billion base pair consensus sequence of the euchromatic[39] portion of the human genome was generated (1305).

After quasi-metaphor gene and quasi-metaphor gene action, quasi-metaphor genome became the central theme of research in life science.

Genetic Technology

In 2001, Sanger published a commentary article in *Nature*, “The Early Days of DNA Sequences.” In retrospect, Sanger states: “when we started working on DNA I don’t believe we

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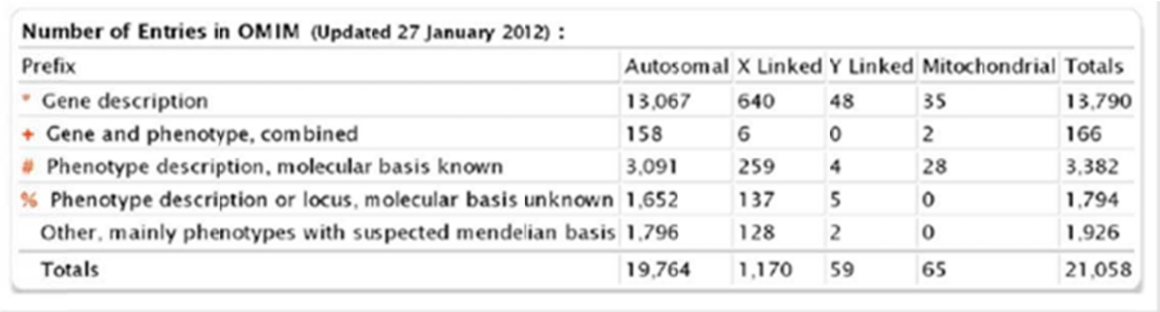
[39] Euchromatin is the fraction of the nuclear genome that contains transcriptionally active DNA and that adopts a relatively extended conformation.

were thinking about sequencing the entire human genome—perhaps in our wildest dreams but certainly not within the next 30 years" (267). His remark alludes to a crucial point that should receive serious attention both scientifically and socially: in the Mendelian approach, scientists, even the best, often do not and cannot have a whole picture regarding what the effects of their research will be if combined with that of others, because each of them only focuses on solving one or a few particular problems. The development of molecular biology serves a vivid example.

Along with research on what gene can do—or, gene function, there is another line of research on what can be done on the gene—or, genetic technology. The latter was supposed to serve the former, as conceived by Weaver. After the genetic code was resolved, the latter became more and more independent of the former. In 1970, American microbiologist Hamilton Othanel Smith with his first graduate student Kent Wilcox discovered an enzyme that was "capable of recognizing [DNA specific sequence] and degrading ('restricting') foreign DNA" (379). Further research revealed that there was a group of restriction enzymes/endonucleases, each with its specific recognizing or cutting sequence. These enzymes made it possible to "cut and paste" DNA fragments. In 1973, American geneticist Stanley Cohen and Herbert Boyer constructed "biologically functional plasmids" whose DNA fragments were from different resources (3240). In so doing, "it may be possible to introduce in *E.coli*, genes specifying metabolic or synthetic functions such as photosynthesis or antibiotic production indigenous to other biological classes" (Glick and Pasternak 5). In 1976, Boyer became a co-founder of *Genetech* company, where the first human protein (somatostatin) was produced in *E. coli* in 1977, and human insulin, 1978. Another obstacle for genetic technology was the supply of DNA, which was often too little to be workable. In 1985, Kary Mullis and his colleagues invented a technique named polymerase chain reaction (PCR), through which the target sequences in genomic DNA exponentially increase (220,000 times) (1350).

The combination of these techniques, together with the advances in computing and engineering, accelerated the pace of molecular biology. The completion of the human genome project not only successfully obtains the whole human genetic sequence, but also establishes a domain of genetic technology that can operate on the gene--to analyze or manipulate it. As the zenith of molecular biology, it has accomplished what can be done to DNA. Yet it is not the end of the gene. According to OMIM[40], only 0.7% (166/21058) of human genes is known by both its sequence and its phenotype; the rest of genome is still under investigation at various stages (Table 1). 99.3% of genes remain to be annotated, which is carried forward by scientists of other disciplines according to their research interests. Genomic sequencing has become a mature and commercialized technique. A genome of interest, like a virus or parasite, can be sequenced by a lab or biological company. Hitherto, the research of molecular biology established systematic knowledge of DNA, as well as techniques and instruments to work on DNA. It is the point of, in Rheinberger's words, molecular biology's "dissolution as a discipline in its own right" (*What*



Number of Entries in OMIM (Updated 27 January 2012) :

Prefix	Autosomal	X Linked	Y Linked	Mitochondrial	Totals
* Gene description	13,067	640	48	35	13,790
+ Gene and phenotype, combined	158	6	0	2	166
# Phenotype description, molecular basis known	3,091	259	4	28	3,382
% Phenotype description or locus, molecular basis unknown	1,652	137	5	0	1,794
Other, mainly phenotypes with suspected mendelian basis	1,796	128	2	0	1,926
Totals	19,764	1,170	59	65	21,058

Table1: OMIM statistics of January 28, 2012

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[40] OMIM stands for Online Mendelian Inheritance in Man, a database as a catalog of human genes and genetic disorders authored and edited by Dr. Victor A. McKusick and his colleagues at Johns Hopkins and elsewhere, and developed for the World Wide Web by NCBI, the National Center for Biotechnology Information.

Happened 303).

Moss insists that the gene concept cannot conflate performationist gene-P as "predictor of a phenotype" and developmental gene-D as "molecular sequence along the chromosome," and they "can be used responsibly within their domains" (*What Genes* 48). Although perceiving the gene as a methodological process[41], Fogle's argument that the gap between Mendelian/classical gene as a unit of inheritance and the molecular/DNA gene is impossible to bridge is similar to that of Moss. This gap between gene-P and gene-D, or classical and molecular genes, is precisely the annotation of genomic sequence so as to understand the relationships of genes, genome/genotype, and phenotype. To the Human Genome Project, as shown by the data, only a little has been done. Some scholars define the time after the completion of Human Genome Project as a post-genomics, and some scholars even consider "the post-genomic approaches are nothing more than a way for biologists to prolong the influx of money generated by the sequencing programs" (Morange, *Post-Genomics* 358). Yet Moss' perception of the gene concept suggests that it is important, if not more than obtaining the sequence, to annotate a genome. Hence, the so-called post-genomics should be genomics as the annotation of genomes, for if the relations of genes, genome, and phenotype are still unknown, how it can be claimed that genomic research is completed? Genomics should include sequencing of a genome and annotating the genome. Without sequencing data, a genome cannot be annotated; without annotation, the functions of the genes in the genome, as well as the relationship between the genome and its phenotype, cannot be understood. Therefore, the genome-centered research includes two phases: the one focusing on sequencing, which basically ends at the completion of

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[41] In Chapter 6, such a process will be illustrated in detail.

the Human Genome Project, and the other on gene annotating, on which the major part of current research of life science dwells. It is an insightful move that Griffiths and Stotz specifically elaborate one of the functions of the gene concept as “postgenomic,” preferably as the *genomics of sequencing* period by me for the reasons above. The research on gene functions in the context of the genome should lead to the understanding of the gene and the genome in terms of developmental systems. The time when the annotation of the genes in genomes is accomplished should be the time of genomic manipulation. In fact, the pioneer work of sheep and other animal cloning had already been done. That time should be defined as *post-genomics*, as after the *genomics of sequencing* period and the *genomics of annotating* period. Keller takes the twentieth century as “the century of the gene.” Perhaps, it is more realistic to be considered as the century of DNA; instead, the century of the gene should be the twenty first century. With the research of annotating and elaborating of genes in the genomes, better understanding of gene function is on the way.

Conclusion

With the completion of the Human Genome Project and other genomes sequenced, a new definition of the gene was proposed in 2008: “a gene is a discrete genomic region whose transcription is regulated by one or more promoters and distal regulatory elements and which contains the information for the synthesis of functional proteins or non-coding RNAs, related by the sharing of a portion of genetic information at the level of the ultimate products (proteins or RNAs)” (Pesole 1). Such a definition implies that, at the genomic level, the knowledge about the gene is so complicated that defining gene is painstaking: On the level of transcription, a region of DNA sequence can or cannot be transcribed into RNA--for instance, introns cannot; even if it can, it may or may not be transcribed because the process is controlled by more than one factors and conditions. As the product of transcription, RNA is the activity component in maintaining and expressing heredity. As to translation, only one kind RNA, message RNA can be translated into proteins; other kinds of RNA function to assist the process. In a brief version, the gene is defined as “a union of genomic sequences encoding a coherent set of potentially overlapping functional products” (Gerstein et al 669). While successfully packing the complexity into *coherent*, *potentially*, *overlapping*, and *functional*, this definition inevitably loses the specificity and clarity in what these words refer to in the terms of DNA, RNA, and proteins. Both definitions agree, however, that the gene is a unit within genomes. After 1953, the definitions of the gene are derived from the developments of DNA research, which brings out the deeper and broader understanding of the relation between DNA and gene function. Often, what scholars attempt to unify is not what the gene is chemically, which is already answered, but how the gene functions with relation to DNA sequence, as the two definitions. This is a tremendously difficult task due to the complexity of heredity and diversity of the biological world, yet nothing has turned up so far that would require us to reject the gene as a unit of heredity.

As discussed, gene research has three major different periods. First is the classical/Mendelian period from 1860s to early 1950s, when the gene was a quasi-metaphor. The main research methodology about heredity was pure line breeding and hybridization that allowed the observation and analysis of phenotype in order to understand genotype. Meanwhile, the advances in biochemistry and physics revealed more and more about the chemical and physical nature of the gene till the structure of DNA was determined. It should be mentioned that Muller’s X-ray-induced mutations on chromosome research made a significant contribution, although his methodology belongs to the second period. The second is the gene action/function-centered research period from 1950s to 1980s. While the gene had become a scientific concept, more quasi-metaphors related to the gene action or the central dogma one way or the other were introduced in the research on protein synthesis, as well as gene activity and it regulation. The research methodology was DNA operation with two directions: what the gene can do and what can be done to the gene. These two interact such that one promotes the other. In doing so, heredity was understood through gene manipulation or DNA recombination and DNA expression. The third period is the genome-centered research since 1980s. It should have two periods, the genomics of sequencing and the genomics of annotating. The genomics of sequencing period is from 1980s to 2000, during which genomes from viruses to bacteria, plants and animals were sequenced. Gene function was contextualized in genomic research; genetic technology grew matured and industrialized. For instance, the first knockout mouse, of which a specific single gene, a target gene, was disrupted for the purpose of understanding the function at the organism level, was made in 1989, and later was commercialized (Gondo 806). The climax was the accomplishment of the human genome project, and meanwhile, molecular biology dissolved into other disciplines. Scientists are working in the genomics-of-annotation period, in which they elucidate gene function according to the interest of their field, such as medicinal chemistry, biochemistry, or pharmaceutics.

The research on gene as quasi-metaphor tracks the redefinitions of what gene is, and the research on the gene as scientific concept brings about redefinitions of how the gene works. It can be observed that the definitions of the quasi-metaphor gene gradually move toward greater precision; whereas the definitions of the scientific concept gene become more and more complex. The vagueness of the quasi-metaphor gene is the vagueness regarding which chemical category the gene should belong to; that of the scientific concept gene is regarding how gene functions in different aspects of heredity. Falk’s difficulty in treating the gene as a “concept in flux" is understandable, for the concept in flux before the discovery of the double helix is the gene; and after that is gene function. Along the process, the confirmation as well as rejection of epistemic assumptions keeps redefining the boundary between the known and unknown, such that the territory of knowledge expands. Strictly speaking, the increasing complexity of the gene concept, discerned by Carson and Portin, is complexity of epistemic access, or knowledge.

The gene as Rheinberger's epistemic object at first was "a formal entity that made it possible to explain in the context of ever more ingenious experiments in cross-breeding, the emergence or disappearance of certain characters in subsequent generations," which was transformed into a "physic-chemical substrate or substance," and then "into an entity with informational properties" (Rheinberger, *An Epistemology* 154). The vagueness of the epistemic object allows the transformations. These three stages correspond with the quasi-metaphor gene, the scientific concept gene, and the genomic gene, marked by the "three great milestones in genetic[s]"—the Mendelian rules and research methodology, the molecular basis of heredity as DNA, and the Human Genome Project (Snustad and Simmons 2-5). Although at the third stage, the gene is far more than an information carrier in the quasi-metaphorical approach. The gene becomes the tool to analyze the genome—that is, to understand the genome by studying the genes, as both individuals and interactive entities, in the genome. As a well-established scientific concept, its functions at the genomic level are the ultimate goal of genomic research. Tim Lenoir points out that the epistemic object is "situated at the interface, as it were, between the material and conceptual aspects of science" (Rheinberger, *An Epistemology* xiv). The discussion about quasi-metaphors pushes this argument further, considering how scientists communicate to agree on the existence of such an object: that the interface is the signifier of the epistemic object, the language as quasi-metaphor, rather than the epistemic object itself. On one hand, the vagueness of a quasi-metaphor accommodates the transformations; on the other hand, the quasi-metaphor remains as the same word signifying stability, coherence and consistence in transformations.

The referent of the quasi-metaphor *gene* had always been the material foundation of heredity. What changes in the process of searching for the gene is the epistemic assumption, which increases in specificity to the point that DNA is proved to be the gene. With respect to different hereditary aspects and species, the quasi-metaphor *gene function* has had multiple referents, which can hardly be unified.

Waters' perception of the distinction between classical and molecular genes is that the former is "centered on the idea that genes are unities whose mutations result in phenotype difference;" whereas the latter, "genes are for a linear sequence in a product at some stage of genetic expression," which is what he calls the fundamental concept of the gene. To him, "the differences in phenotypic form identified by classical geneticists were not viewed as fundamental units of development; they were understood to be phenotypic quirks caused by differences in the real units of heredity, the genes" (*Genes Made Molecular* 183). In classical genetics, however, genes are units of genotype that determines phenotype. Mutation as a research tool to understand the relationship of the gene/genome and phenotype started from Muller and became systematic and productive after the gene was proved to be DNA. Hence, what Waters considers as the classical gene concept is indeed the molecular gene concept through mutagenesis. Either genetics or developmental biology, the ultimate question with respect to the gene, genome and phenotype is the same: What and how do genes determine phenotype on the genomic level? In the newest edition of the *Principles of Genetics*, the gene is defined as "a hereditary determinant of a specific biological function," namely, the classical gene as hereditary material; or, "a segment of DNA encoding one polypeptide and defined operationally by the *cis-trans* or complementation test," namely, the molecular gene that controls the synthesis of protein; or, "a unit of inheritance (DNA) located in a fixed position on a chromosome," namely, the genomic gene (Snustad and Simmons 727). Again, "how gene is defined depends on what you are doing," as the teacher of Genome 371 at the University of Washington says. If there is a fundamental concept underlying the gene, it should be the epistemic assumption that Johannsen has for the gene, which eventually resulted in epistemic access: the gene is material reality. Or, in the words of Benjamin Lewin, the founder of *Cell* journal and also the author of the textbook *Genes*:

> Genes are DNA

CHAPTER FIVE

At the Edge of Knowledge:
A Quasi-metaphorical definition of Scientific Revolution

There was no such thing as the Scientific Revolution. (Shapin 1)

On May 8th of 1794, at the height of the French Revolution, Lavoisier was guillotined at the age of fifty. Only one and a half years later, however, he was exonerated by the new French government that declared him to have been "falsely convicted." The next day following Lavoisier's death, Joseph-Louis Lagrange, one of Lavoisier's collaborators on the Advisory Board for Arts and Trades, remarked that "[i]t took them no more than a moment to make that head fall, and a hundred years may not be enough to produce another one like it" (Bell 184). It was told, though probably apocryphally, that in Lavoisier's trial by the Revolutionary Tribunal, the judge said: "The Revolution has no need of scientists" (Duveen 62). Either this chilling message, which represents the worst form of social revolutions—irrational, chaotic, and bloodthirsty, or this ironic tragedy, in which the leader of the chemical revolution was executed during the revolution of his society, suggests an incompatibility between the social revolution and the scientific revolution. Or more precisely, two probably incompatible notions might be forced into the term *revolution*. Hence, the historiography in which the history of science is portrayed by series of scientific revolutions is worth a careful examination.

The preceding chapters have traced how a quasi-metaphor like the gene, as well as the metaphors deriving from the gene, provided continuity and guidance over a century of work during which the understanding of what they referred to has been changed a number of times. The question arises of how they operate in a period of radical change in thinking—the sort of thing Thomas Kuhn discusses mainly with regard to the physical sciences. Clearly there have been substitutions and deflections of key quasi-metaphors, like gene action, but scholars like H. F. Judson maintain that there have been no scientific revolutions in the life sciences like those described (controversially) by Kuhn. Lavoisier's work, however, in which phlogiston and (modern) chemistry figured prominently, has been called revolutionary. Indeed, there are numerous events in the study of the natural world in the last four hundred years that some scholars have narrated as ruptures and other scholars as continuous development, including the grandest event of all, the Scientific Revolution itself (or not). It may be that describing a revolution rests squarely on a paradox that this chapter tries to articulate through perceiving *scientific revolution* as a quasi-metaphor.

In this chapter, quasi-metaphorical analysis of *scientific revolution* traces the variations of the concept, as well as the changes of its epistemic assumptions, from Burtt and Koyré's anti-positivist to Butterfield's anti-whiggish stance. The inherent paradox in the quasi-metaphor *scientific revolution* resides at the contradiction between its epistemic assumptions and its construction. In other words, *scientific revolution* as a quasi-metaphor is contrived to demonstrate that, in the history of science, there are episodes during which scientific research are dramatically different, so dramatic that it can be considered as ruptures or discontinuities. Nevertheless, the ways in which the historians depict the episodes are exactly positivist and whiggish. In short, the scientific revolution is an entity constructed in positivist and whiggish ways in order to illustrate the history of science is not positivist and whiggish. Such paradox is reflected by two extreme narrations of the Scientific Revolution. One treats the Scientific Revolution as a social revolution caused by the upheaval of modern science, and the other, a special revolution in science. Some scholars are against the former because it dramatizes the process. Kuhn, as the most influential scholar who engages with the latter approach, has theoretical difficulties because some cases do not fit what he theorizes as the structure of scientific revolutions. To define *scientific revolution* as the confirmation or rejection of epistemic assumptions resolves the paradox. In such perspective, continuity is the nonstop transformation of epistemic assumption to access, and discontinuity, rejections of epistemic assumption, which may lead to abandonment of quasi-metaphor. Furthermore, the accumulation of scientific knowledge is much more complicated than Kuhn portrays, and quasi-metaphors are the records of human knowledge growth.

Emergence of Scientific Revolution as a Quasi-metaphor

Scientific revolutions, in Butterfield's words, are the "landmarks" and "epochs of crucial transition" without which the history of science is like "an ocean without fixed points" (*The Origin* 192). Yet not all scientific revolutions are equally evaluated. *Scientific revolution*, or *revolution in science*, is a generic term referring a mode of scientific advance made up of a series of leaps rather than gradual additions. *The Scientific Revolution* specifically indicates one dramatic upheaval in science, which always includes the first decades of the seventeenth century, though the beginning and end are still debated.

The meanings of *revolution*[42] have evolved. There was no single word corresponding to revolution for the Greeks and Romans, although a number of events that happened then were treated as revolutions by scholars. The origin of the word *revolute* can be traced back to late Latin, a substantive derived from the verb *re-volvere* as *to roll back*, hence also *to unroll*, *to read over*, *to repeat*, and *to think over*, and further, *to return* and *to recur*. In the seventeenth century,

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[42] The sociopolitical *revolution* is one the most fundamental quasi-metaphors in the humanities. For the purpose of my dissertation, I leave out the discussion.

after the Renaissance, the substantive *revolutio* is a technical term in astronomy and mathematics, interchangeable with *rotatio*--*rotation* in English. Gradually, *revolution* acquired a meaning as a cyclical process or an ebb and flow, implying a return to some antecedent condition. It then became associated with the process of radical change or overturning in a sociopolitical or other sense, and consequently, the cyclical connotation diminished. When the Glorious Revolution happened in England in 1688, the notion that a revolution was also happening in sciences developed. By the beginning of eighteenth century, *revolution* was used to indicate affairs related not only to the state but also those in the intellectual and cultural, especially the scientific domains. In the last quarter of the eighteenth century, the American and French revolutions stirred up passion and hope, the promise of a shortcut to a much brighter future. Lavoisier meanwhile announced a revolution in science, the Chemical Revolution, without any suspicion that the French Revolution could gulp down his own life eventually (B. Cohen 52-54). During the nineteenth and twentieth centuries, *revolution* was used to label numerous social and political events, in both positive and negative senses. In addition, theories of revolution were established as a distinctive body of knowledge, among which is the theories about scientific process.

Scholars who study the Scientific Revolutions, like David Lindberg and Robert Westman, are aware of *revolution* as the prevailing metaphor for scientific change. B. Cohen's *Revolution in Science* traces the evolution of the concept. The first change in science to be called a revolution was the "Scientific Revolution."

The Scientific Revolution

In his Ph. D dissertation, *The Metaphysical Foundations of Modern Physical Science*, first published in 1924, American philosopher Edwin Burtt (1892 - 1989) attacked Duhem's claim that modern science was a medieval product as “positivist menace”. More specifically, during the rehabilitation of medieval science, Duhem’s study came to the conception that science moved steadily as an unbroken, providentially directed continuity from primitive beginnings to mature understanding of nature, free of metaphysics and the quest for underlying causes (Lindberg 14). Against Duhem’s view, Burtt was the first who perceived a “radical shift” or “vertical upheaval in the main current of intelligent thought” during the sixteenth and seventeenth centuries in the work of Copernicus, Kepler, Galileo, Descartes, Boyle, Newton, and a few other contemporaries (11). He also is the first to use the quasi-metaphor *revolution* to frame what happened in this period, and the epistemic assumption is the opposite of Duhem’s claim. Burtt claims that the establishment of modern science starts from a radical change in metaphysical assumptions. The outcome of the revolution is that “men began to think about the universe in terms of atoms of matter in space and time instead of the scholastic categories;” “teleological explanations, accounts in terms of use and the Good, become definitely abandoned in favour of the notion that true explanations, of man and his mind as well as of other things, must be in terms of their simplest parts” (16). In regard to how the influence of Gilbert and Boyle differs from that of Kepler, and Galileo, Burtt writes:

> Back in the days of Kepler and Galileo, besides the exact mathematical movement in science, so powerfully advanced by their achievements and bringing in its train the remarkable metaphysical revolution which it seemed imply, there was another scientific current under way, flowing by slower and more tentative steps, but none the less scientific in interest and fruitfulness. Its method was wholly empirical and experimental rather than mathematical, and it was primarily in connexion with this other current that attempts to give science a correct metaphysical ground work made a quite positive and definite ‘spirit of nature,’ or, as it was more commonly called, ‘ethereal spirit.” (156).

To Kepler and Galileo, the assumption is that the application of mathematics in science should make a new science. As far as Burtt is concerned, this mathematization of nature was completed with the work of Isaac Newton. But for Gilbert and Boyle, the assumption is that empiricism should deliver the true understanding of Nature. Considering these two assumptions metaphysical, Burtt finds them in the replacement of one by another. Most importantly, the aftermath of the revolution is the successful and productive science that brings forth a change in the "prevailing conception of reality, of causality, and of the human mind." By reality, he means that the world becomes an entity of atoms with lawfully and stably mathematical characteristics. By causality, he means that God ceases to be regarded as a Supreme Final Cause but the First Efficient Cause, held by Albert Einstein and Stephen Hawking still. By the human mind, he means that the relation of the human mind to nature changes to the form of the Cartesian dualism—its doctrine of primary and secondary qualities, its location of mind in a corner of the brain, and its account of the mechanical genesis of sensation and idea (303-304).

French philosopher Alexandre Koyré (1892 - 1964) took the same stance with Burtt against Duhem's claim. He celebrated the scientific revolution as "one of the profoundest, if not the most profound, revolution of human thought since the invention of the Cosmos by Greek thought" (400). His perception of the revolution, however, was that of "a radical intellectual 'mutation'," or, of a notable discontinuity. To him, the major contributors are Galileo and Descartes--the former first initiated and the latter fully articulated the geometrization of space and the dissolution of the cosmos, both contained in the principle of inertia.

As far as B. Cohen is concerned, it was Herbert Butterfield who "was largely responsible for making the Scientific Revolution a central issue in the mind of every reader" (390). To Butterfield, the scientific revolution is "a particular historical transition, a particular chapter of intellectual development" (*The Origin* 8). Its chronological frame was from the fourteenth to nineteenth centuries, a remarkably long period. By claiming the Chemical Revolution in the late eighteenth century as a "postponed" one, however, Butterfield still maintains the center of the Scientific Revolution in the sixteenth and seventeenth centuries. Butterfield eloquently summarized the consequences of the Scientific Revolution:

> Since the revolution overturned the authority in science not only of the middle ages but of the ancient world--since it ended not only in the eclipse of scholastic philosophy but in the destruction of Aristotelian physics--it outshines everything since the rise of Christianity and reduces the Renaissance and Reformation to the rank of mere episodes, mere internal displacements, within the system of medieval Christendom (*The Origin* 7).

Whiggism in Historiography of Science

Although Butterfield joined Burtt and Koyré to shape profoundly the scholarship on the Scientific Revolution, as a "general historian," he steered the discussion in another direction, historiography. Of the approaches toward the history of science, two are often put in comparison. One, labeled by Butterfield as "the whig interpretation of history," is "the tendency in many English historians to write on the side of Protestants and Whigs, to praise revolutions provided they have been successful, to emphasize certain principles of progress in the past and to produce a story which is the ratification if not the glorification of the present" (*The Whig* v). One extreme form of whiggish history is the history of science usually found in scientific textbooks. The other approach is to "revive the past, to enter into the minds of our predecessors, to imagine the political, social, and cultural aspects of their environment" (Hooykaas 21). The major conflict between these two approaches is how "we" as historians of science at present with current knowledge perceive "they" as scientists in the past with relatively limited knowledge, as well as the process bridging their knowledge and ours.

This problem has three aspects. The first regards how historians deal with the past in relation to the present. Nowadays, historians of science "have grown to condemning" the whiggish, "present-oriented" history, for "reading the present back into the past" often distorts what the past was like in its own term (Shapin 7; *The Origin* 201). Another is to separate "us" as these who study from "them" as those who are studied. In Peter Dear's words, the central goal of historians is to "understand why particular people in the past believed the things they did about the world and pursued inquiries in the way they did" (2). The last is to differentiate what we know and what they knew, and more importantly, to resist the belief that the truth of the present is more rational, more natural, and less in need of causal explanation than that of the past (Sismondo 12). With the same quasi-metaphor The Scientific Revolution, Burtt and Koyré work to confirm the epistemic assumption through selectively analyzing the historical figures whose achievements became a part of the modern science yet which differed dramatically from the beliefs of medieval science; Butterfield narrates the quasi-metaphor the Scientific Revolution delicately as a specific historical period during which great scientists work on building modern science.

However straightforward or biased the whiggish historiography seems, logically, the anti-whiggish is the paradoxical one. How can historians be in the present yet escape from the present? David Hull states that "[a]ny historian completely ignorant of the present could not begin to discover what happened in the past. After all, his evidence is all in the present (5)". What the present provides is historical visibility, a vantage point with more, if not better, knowledge by which the past is defined; or, the past is past exactly because it lacks what essentially defines the present. Such "burden of superior knowledge," hence, cannot be avoided by any historian (Hall 57). Moreover, as far as Steven Shapin is concerned, "there is inevitably something of ‘us’ in the stories we tell about the past,” so “it is foolish to think there is some method, however well intentioned, that can extricate us from this predicament” (10). In other words, when conscious choices are made, as about whom or what to study, which events or materials to pick up, our historical visibility and interests are already embedded in the research. Butterfield contemplates that “[t]he art of the historian is precisely the art of abridgement: this problem is this problem” (*The Whig* 102). This then is an account of a “moderate” anti-whiggish position: one can never absolutely escape the views and concerns of the present when writing history.

Nevertheless, anti-whiggish historiography provides important guidance in studying individual scientists. For instance, historians not only depict Isaac Newton’s great contribution in applying mathematical methodology to natural philosophy, but also situate them within Newton’s extensive writing in alchemy, exegeses of the Scripture, and sacred and secular chronologies, as well as the social and cultural conditions of Newton’s time (Osler 15). The historian’s Newton hence is Newton the person, not Newton the symbol only associating with Newton’s laws in scientific textbooks. The history of ideas or disciplines written by scientists, on the other hand, is generally thought to be whiggish. It is understandable that the development of a discipline or an idea from the past to the present has to be made by the successful contribution. Consequently, such history is retrospective, and the present epistemological standards of right and wrong are applied to all scientists, including scientists in the past. Anti-whiggish historians object to such a whiggish approach, because it constructs a past with the reference of the present and accordingly, presents a linear history in between. In addition, it is unfair to judge unsuccessful research of past scientists as error or wrong by present epistemological standards. In their defense, whiggish historians accuse their opponents of having “the erroneous assumption that a sequence of theory changes in science is of the same nature as sequence of political change” (Mayr 302). Furthermore, the anti-whiggish historians like Kuhn “are manufacturing problems where none exist” by pretending “abject ignorance” of the present. (Hull 4).

Ironically, it seems even Butterfield cannot escape from whiggism. As John Henry points out, Butterfield became a whig historian when he wrote *The Origin of Modern Science*. Henry argues that the whiggish indicators of “the concept of the Scientific Revolution is inherently whiggish” are the continualism and the concept of science itself. By “continualism,” he means the narrative in which Scientific Revolution appears as a linear intellectual relay. For instance, Galileo is presented as one “who prefigured Newtonian inertia.” The word *science*, on the other hand, was not coined until the nineteenth century. To apply a modern concept to a context prior to its establishment is nothing but whiggism (4).

Henry’s argument reveals the very subtle logical paradox in treating the Scientific Revolution as a historical process. It is first a revolution, namely, an assumed discontinuity, a perceived rupture in the linear, continual, unidirectional timeline of history, thus requiring an anti-whiggish approach. Yet, in depicting who does what during this period to constitute the revolution, whiggism kicks in, which presumably is what Henry means by “inherently whiggish,” though “inherently paradoxical” is more accurate from my observation. In brief, the paradox is that, to establish the Scientific Revolution, an anti-whiggish conception, one has to narrate a whiggish history. Henry’s resolution is:

> It is possible to acknowledge a whiggishness in one’s reasons for looking at the history of science without, however, allowing aspects of whiggism to intrude into our historical narratives. Rather than imposing our own views, our aim as historians should be to strive for as full an understanding as possible of the

> contemporary context. For example, if we wish to understand the contemporary response to a little book like Galileo's *Siderius Nuncius* (*Starry Messenger*, 1610), in which Galileo presented the discoveries he had made by turning the newly invented telescope to the night sky, it is obvious that we cannot simply read Galileo's text. Nor will it be enough to familiarize ourselves with the technical astronomy and cosmology of Galileo's time. ... A really full account would also require some knowledge of contemporary understanding of who Galileo was: his reputation, his presumed or perceived motivation, and whether he could be held to speak disinterestedly, and so forth (6).

As discussed above, the strength of anti-whiggish history is its treatment of historical figures as the persons who lived their own times, as to Newton and to Galileo. To write the Scientific Revolution, the question posting to a historian is: once Galileo is done, as richly contextualized as possible, who is the next? Although a decision of the historian to make, either Descartes or Newton, the next must have something to do with Galileo in a revolutionary way, and so does the one after the next, if any. The history then begins to appear whiggish. To avoid such fate, Henry makes a move to approach the Scientific Revolution by its relation to Renaissance, magic, and religion, as well as the mathematization of the world, experiment, and the mechanical philosophy. Some of the most salient changes of the Revolution in his account—namely, "the increased use of mathematics to understand the working of the natural world," "the experimental method, ... derived largely measure from the natural magic tradition," and "that religion and theology played a major part in the development of modern science," almost return to the very ideas against which Burtt conceived the quasi-metaphor revolution (13, 67, 97). He escaped from a whiggish approach, such as Butterfield's, to present the history of the Scientific Revolution, yet fell into another whiggish trap that the very conception of the Scientific Revolution proposed by Burtt was determined to avoid. Total anti-whiggish, or a non-whig history, thus seems a well-intended but unpragmatic ideal in dealing with the Scientific

Revolution. What historians can do, to put in Henry's words, is to "try to avoid overt forms of whiggism, revolutionists and continuists alike" (4). Such paradox in the concept of the Scientific Revolution forces the historian to choose which whiggish side to engage or avoid narrating "the Revolution" entirely.

The Scientific Revolution and Scientific Revolutions

By analyzing "the great tradition" of the Scientific Revolution, Floris Cohen also observes "two radically different conceptions of the Scientific Revolution." On the one extreme, he states, the revolution comes down to a fundamental upheaval in cosmology and mechanics, which consequently affected other areas of inquiry into nature, without itself being affected by them. Inside this overall framework, however, there is room left for varying definitions of what the essence of the upheaval should be taken to be. At the other extreme, the Scientific Revolution is thought of as an overthrow of earlier approaches to nature, which essentially affected all sciences alike. Henceforth, all disciplines at the time were caught in a process of accelerated change and growth. In this view, the distinction of early modern science from Aristotelian or magical predecessors is found to be no sharper than that from the newly won rationalism. At this extreme, the unique events in cosmology and in the emerging science of mechanics can only be brought out by some emphasis in the course of the overall story, and the question remains of whether that is enough to do them justice (121).

In agreement with two extremes, the analysis of the paradox yields two different perceptions of what the extremes are. On one extreme, to emphasize the Scientific Revolution as the radical change, the major rupture, the overt discontinuity in the history of either science or western civilization, the essence of the upheaval is modern science itself. Butterfield declares the scientific Revolution to be "THE PART played by the sciences in the story of our Western civilization," which "outshines everything since the rise of Christianity and reduces the Renaissance and Reformation to the rank of mere episodes" (*The Origin* 7, emphasis in the original). The Revolution obvious has gone beyond science itself. He is affirmative on the social establishment of the Revolution: the independence of science, and the cultural influence of science, which is no less than that of religion or the Renaissance. Such a view echoes what Boyle expressed in his letter written in 1656: "I do with some confidence expect a Revolution, where by Divinity will be as much as Losser, and Real Philosophy flourish, perhaps beyond men's Hopes" (B. Cohen 89). The greatest significance of the Scientific Revolution is that it brings science into social life; therefore, it is a social revolution through the emergence of modern science. When Butterfield identifies the Chemical Revolution which happens at the later Seventeenth century as the postponed Scientific Revolution, he hints that modern science is supposed to be an entity made up of different sub-disciplines of which the developments should be roughly the same. Nothing could be more whiggish than judging a historical event by when a historian thinks it should happen. Butterfield's ideal about the Scientific Revolution suggests that he thinks modern science as an enterprise debuts in the society through the Scientific Revolution, as historian Richard Westfall eloquently delivers in the opening passage of a brief teaching guide:

> The Scientific Revolution was the most important 'event' in Western history, and a historical discipline that ignores it must have taken an unhappy step in the direction of antiquarianism. For good and for ill, science stands at the center of every dimension of modern life. It has shaped most of the categories in terms of which we think, and in the process has frequently subverted humanistic concepts that furnished the sinews of our civilization (F. Cohen 5).

At the same time, it also implies that the Scientific Revolution is more or less a product of dramatization. Thus, as Shapin comments, "historians have become increasingly uneasy with the very idea of 'the Scientific Revolution'" (3). For instance, Hellyer criticizes that the word "scientific" is "misleading and anachronistic," for the various disciplines that studied nature in the early modern period had many terms, such as natural philosophy, mathematics, mixed mathematics, and natural history, yet not a single term encompassed them as "science" does today. He also questions whether *revolution* is appropriate for a process that lasts at least a century and a half or, or even as long as three centuries by Butterfield's account. To Shapin, moreover, "there was not any singular and discrete event, localized in time and space, that can be pointed to as 'the' Scientific Revolution" (3). He concludes that the Scientific Revolution is no more than "some pervasive *stories* we tend to be told about science." As "the most respected component of our modern culture," science does not need "to be defended through perpetuating fables and myths cobbled together to pour value over it" (165).

Scientific Revolutions as Change in Science

The other extreme is to emphasize that the history of the Scientific Revolution, as a part of the history of science, is constituted by a few radical changes. In so doing, the Revolution is disassembled into a series of revolutions--for instance, from Galileo's mathematization, to Descartes' mechanical explanation of nature, and to the Newtonianism, as in Dear's *Revolutionizing the Sciences*. Kuhn's *The Structure of Scientific Revolutions* had profound influence on the scholarship of this approach. His analysis of the revolutionary pattern is mainly based on these cases of the Scientific Revolution, especially the Chemical Revolution, which has a tight connection with the French revolution. In Kuhn, a revolution in science implies "a break in continuity, the establishment of a new order that has severed its links with the past, a sharply defined plane of cleavage between what is old and familiar and what is new and different" (B. Cohen 6).

As a reconciliation of the two extremes, B. Cohen conceptualizes revolutions in science as “a complex, historically changing entity—affected in turn by revolutionary theory and events in the realms of politics—rather than as a single and simple idea of how scientific change comes about” (27). While successfully shunning the problems of both extreme, such conception is too broad to tell which event is or is not a revolution in science. He thus provides a series of criteria. First, he differentiates the historical and the historian’s perspectives of revolution in order to separate “us” from “them.” Accordingly, the former as “the objective facts or historical data” is composed of the judgments made at the time of the revolution and during succeeding ages; the latter is current subjective judgments (xii). Next, he elaborates the four stages of a revolution. At the first stage, an individual or a group of scientists works to create a fundamental transformation of current scientific ideas. At the second, the scientist or the group comes up with convincing results, usually recorded in the form of an entry in a diary or notebook—row data, that is. At the third, the well-formed ideas are disseminated to the scientific community and the public. At the last, a sufficient number of other scientists become convinced and beginning to follow the revolutionary way (28 – 39). Yet every scientist who has a formally academic publication can claim that they have gone through the first three stages. So the last stage is crucial: how many scientists should be convinced to qualify a revolution? It is a question impossible to answer. B. Cohen then suggests a series of four tests that can be universally applied all revolutions. The first is the testimony of witnesses: the judgment of scientists and nonscientists of that time when the revolution happens. The second is the existence of later documentary recorders, like textbooks or later scientists’ statements, about the revolution. The third is the judgment of competent historians, notably historians of science and historians of philosophy. The last is the living scientific tradition, to the mythology that that is part of the accepted heritage of practicing scientist.

B. Cohen's methodology is descriptive. He extracts the most common features that revolutions in science have as an answer to how to recognize a revolution, but not how a revolution happens or what happens during a revolution. Unlike B. Cohen, Kuhn's methodology is prescriptive. He attempts to extract a general pattern, or "structure" for him, that can apply to all. Scholars like Alexander Bird call it the Kuhnian revolution. If Kuhn's pattern is reasonable, then it should agree with all the cases that B. Cohen studied, but B. Cohen recognizes Kuhn's "scheme seems to work out better for the physical sciences than the biological sciences" (27). What is the problem with Kuhn's structure?

The profound influence of the models and theories of sociopolitical revolutions have on Kuhn's conception of the scientific revolutions is obvious. As he states:

> One aspect of the parallelism must already be apparent. Political revolutions are inaugurated by a growing sense, often restricted to a segment of the political community, that existing institutions have ceased adequately to meet the problems posed by an environment that they have in part created. In much the same way, scientific revolutions are inaugurated by a growing sense, again often restricted to a narrow subdivision of the scientific community, that an existing paradigm has ceased to function adequately in the exploration of an aspect of nature to which that paradigm itself had previously led the way. In both political and scientific development the sense of malfunction that can lead to crisis is prerequisite to revolution. (92)

Kuhn's quasi-metaphor "scientific revolution" has a much narrower epistemic assumption than Burtt's , for it assumes that a scientific community functions the same as a society. By the assumption, a scientific revolution should have the similar cause—crisis in his word, sequence, and effects as a social revolution. Yet Mayr, a biologist and a historian of science, maintains that such assumption is erroneous, because the sequence of theory changes in science is not of the same nature as the sequence of political changes (302). As well as B. Cohen, Birds and Olser also notice that the revolutions in biology do not fit Kuhn's model. In Judson's words, "[i]n biology, no large-scale closely interlocking, fully worked out, ruling set of ideas has ever been overthrown" (*Reflections* 420). Those revolutions happen without crisis; it thus seems no discontinuity, no sharp cleavage between before and after the revolutions. Here comes the "crisis" of Kuhn's theory. To Kuhn, discontinuity is the key criterion defining scientific revolutions. In order to maintain this criterion, he brushes the non-crisis cases into the development of "normal science" at its "accumulative" stage, which does work for the ones that he lists in *The Structures of the Scientific Revolution*. Nevertheless, the consequence of scientific revolutions has to be paradigm shift in Kuhn's theory. In other words, if a paradigm shift happens, there must be a scientific revolution caused by a crisis.

Continuity and Discontinuity in Quasi-metaphors

Behind Kuhn's structure is his philosophical stance, that there have been periods of normal science punctuated by revolution, which alters the historical perspective of the community that experiences it (xi). If so, what maintains the coherence of scientific research? Many scholars debate with Kuhn on this point. Kuhn himself notices that some concepts, like elements, space, and time, run through the history; even when the meanings are changed, the verbal formula are kept. Nevertheless, he dismisses these concepts quickly. He focuses on discontinuity, so those are "the sort of textbook ingredient that is often not invented or discovered at all" (142). About continuity, Wittgenstein says that "the strength of the thread does not reside in the fact that some one fibre runs through its whole length, but in the overlapping of many fibres," which could be Kuhn's defense. Yet why not look carefully at those fibers that do run through, especially in a history believed to have many gaps?

In studying the history of ideas, Margaret Osler adopts the approach in which "thinkers appropriate ideas from the traditions within which they work and use them in their own contexts to solve the particular problems that concern them." Through appropriation, the change in a previously established idea, theory, technique, or practice can be explored, especially as it enters a new historical, or geographical location (6-7). Similar to ideas, quasi-metaphors also can be studied in their different historical contexts, as previous chapters demonstrate. Phlogiston is one of the most famous cases in the history of science, as B. Cohen marks:

> It is evident that Lavoisier's Chemical Revolution passes all the tests for a revolution in science. It has been recognized as a revolution by all historians and scientists, just as it was seen to be a revolution in its own time. Additionally, the whole science of chemistry and its language have followed the lines set forth in the Chemical Revolution. The Chemical Revolution is thus a paradigm example of a revolution in science (236).

Discussed in Chapter 2, the upheaval of the revolution is the formation of modern chemistry, and at the same time, phlogiston theory, including the term, was overthrown. Lavoisier believed that what he did was a revolution. In this picture, the history of chemistry is divided into pre-Lavoisier and post-Lavoisier. The pre-Lavoisier chemistry, "had certainly failed to produce the required structure of scientific thought" (203). The history of eighteen-century chemistry is described as "waiting for Lavoisier to arrive" (Bensaude -Vincent and Stengers 47).

Nevertheless, a few historical facts should not be forgotten. First, fifty years prior to Lavoisier's revolution, Stahl revolutionized alchemy, including establishing the phlogiston quasi-metaphor. His theory then was the best to explain chemical changes. The research set off by his theory was productive. Under its guidance, pneumatic chemistry was well developed, and the nature of corrosion was well understood. Most importantly, it provided the two most important pieces for the revolution. One is oxygen, the very element that could react with inflammable materials to cause combustion, was discovered. When the sensitivity of the analytical balance became good enough to measure change of mass during combustion accurately, it was found that burned metals gained weight. This gain in weight, the other piece, posed a critical problem for phlogiston theory. Together with oxygen, they were produced by Stahl's theory. By the limited knowledge of that time, however, the assumption had to be so. In other words, the assumption was right in its historical context. Although in retrospective, Stahl's epistemic assumption that combustibility is a material— that is, phlogiston--is wrong, it is a necessary cognitive step to understand the natural world. With what was known then, Stahl's theory is the route that had to be taken in order to get what is known now, even though at our current epistemic stance, Stahl's theory looks like a wasteful detour.

The discontinuity of the revolution is discussed as the overthrow of phlogiston theory. From a different perspective, the material first discovered in the phlogiston theory system and called dephlogisticated air by Priestley, and then oxygen by Lavoisier in chemical system has never been abandoned. The material itself is the connection between phlogiston theory and chemistry, the past and the present. In this case, the phlogiston quasi-metaphor could not be adjusted in the fashion of gene, because the former came with an epistemic assumption proved to be incorrect, and the latter, correct. The complication with that episode in the history of science is that it is not about the wrong assumption; rather, scholars like Chang ask, instead of taking Lavoisier's oxygen, why not keep the word phlogiston and use it for something else, say energy[43] (*The Persistence* 421). Perhaps, other than Lord Kevin himself, Crosbie Smith who wrote *The Science of Energy: A Cultural History of Energy Physics in Victorian Britain* is one of the best scholars to answer it. For the purpose of narrating history rather than contemplating

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[43] Energy is another fundamental quasi-metaphor coined by William Thomson (Lord Kelvin) (1824-1907).

where history should have gone, a quasi-metaphorical answer concluded in Chapter 2 is that Lavoisier's establishment of another even more fundamental quasi-metaphor, (modern) chemistry, which provided an epistemic category for the name together with the material oxygen. Moreover, during this revolutionary history, one of the most fundamental quasi-metaphors, element, had been modified a few times, including Lavoisier's work, without any breakdown or crisis, and it was eventually developed into Russian chemist Dmitri Mendeleyev's (1834-1907) Periodic Table of Elements, on which oxygen is the eighth element. (Strathern 236-239). Hence, even with a prototypical revolution in science, such as the chemical revolution, overemphasizing discontinuity leads to a rather arbitrary view of the history of science, for other continuities are ignored.

Fundamental Revolutions and Cumulative Revolutions

While tracking down paradigm shifts, Kuhn notices that "there can be small revolutions as well as large ones," and the small ones are "some revolutions affect only the members of a professional subspecialty, and that for such groups even the discovery of a new and unexpected phenomenon may be revolutionary" (*The Structure* 49). Such description is slippery without a given time span, say weeks or centuries, to measure the effect. For instance, in a memoire, Lieutenant General Leslie Groves (1896-1970) wrote "[w]hen I entered the Manhattan project in 1942, atomic energy was in the kindergarten stage. Only a few people understood the Einstein theory. With the explosion of the bomb, tens of thousands understood the theory, if not the way Einstein derived it" (39).

With respect to quasi-metaphors, on the other hand, the fundamental quasi-metaphors usually last more than a half century. The youngest one is molecular biology whose lifespan is more a half century; and the gene, a century. Genome should be a good candidate, but it is an answer we have to wait for at least a half century. The oldest ones like time, universe, and element have run thousands of years. A group of fundamental quasi-metaphors discussed in this dissertation are the disciplinary ones: physics, chemistry, and molecular genetics. They share a similarity in the epistemic assumptions: methodological commitments. As discussed, to physics, it was mathematics; to chemistry, empirical physics; and to molecular biology, physical chemistry and biochemistry. Dear observed that the new ambition of the seventeenth century, exemplified by Descartes and Bacon was "to forge ahead with professedly novel intellectual programmes." The novelty was often "justified approaches to nature by talk of 'method' instead of talk about classical precedent" (169). This tradition has run through those disciplines.

As empirical sciences, chemistry and molecular biology share another similarity in their epistemic assumptions--instrumental innovations. "The advent of precise weighing was a central aspect of Lavoisier's chemical revolution" (Burns 142). The essential instruments to molecular biology were listed in Weaver's proposal for molecular biology in Chapter 4 Part II.

As theoretical science, on the other hand, physics emphasizes problem solving rather than experiments. Highly influenced by his training in physics, Kuhn takes research in normal science as puzzle-solving. "Though its outcome can be anticipated, often in detail so great that what remains to be known is itself uninteresting, the way to achieve that outcome remains very much in doubt" (14). It is a typical process in theoretical science, but it is not typical of how the research in empirical sciences works, as presented by the cases. As Rheinberger brings out, the complexity of empirical science is that the collective action of experimental systems "points to unforeseen directions opened up within the experimental process" (Experimental S249). Unlike puzzling solving, there is no way to predict what will result from the confluence and dispersion of experimental systems and technologies. Or, the puzzling is an open one with changing outcomes as the pieces are added in.

Small revolutions in quasi-metaphorical perspective are sub-quasi-metaphors; or, quasi-metaphors derive from the fundamental ones. The history of the quasi-metaphor gene, as in Chapter 4 part I, is similar to that of phlogiston, except at the end, it was discovered that the gene did exist. This history appears linear as if a march toward the discovery, but such a mode can hardly be put into Kuhn's normal science, because the development of the quasi-metaphor had relied on different disciplines at different stages. Even so, the process is an impressive confluence of what Kuhn calls small revolutions. Every step that got closer to identifying the chemical nature of the gene is a revolution; or, the specification and modification of the epistemic assumption. Hence, knowledge with respect to the gene as well as heredity increases at every step. Regarding the discovery of the double helix, Bird notes that "[a] discovery that many regard as the most important of the century simply does not fit Kuhn's description of scientific development—it originated in no crisis and required little or no revision of existing paradigms even though it brought into existence major new fields of research" (60). Quasi-metaphorically, the discovery confirmed a profound and long-existing epistemic assumption, which since then turned into a fundamentally epistemic access, so the discovery is a significant one, the same as the overthrow of the phlogiston theory. In other words, it was not a great upheaval, but only one more step forward closer to the chemical nature of the gene, admittedly a crucial one. The continuity is the continuous transformation from the assumptions to the access.

Post double helix research on the scientific concept gene displays another distinctive pattern. The sub-quasi-metaphors, such as gene action, coding, and genome, deriving from the gene at first develop in a dispersive way. Each repeats the confluent pattern of the quasi-metaphor gene, and the process of each similarly includes steps of conformation of epistemic assumption, for instance, the process from the coding metaphor to codon to establish genetic code. When the Human Genome Project was completed, looking back from that point, the Human Genome Project is the confluent point of the sub-quasi-metaphors. Kuhn insists that cumulativity only happens to normal science; during a scientific revolution, cumulativity breaks down, and the cumulativity will not start again until the revolution is over. The cumulative patterns are much more complicated in the case studies: they can be confluent as in the research on the quasi-metaphor gene, or dispersive as in post double helix research, or from confluence to dispersion as the research from the quasi-metaphor gene to the scientific concept gene; or from dispersion then confluence as the research of post double helix to the Human Genome Project, depending on the particular historical point and perspective.

Metaphorically, what is a scientific revolution? It is the transformation of epistemic assumption to epistemic access or the rejection of an epistemic assumption, and through both human knowledge grows. Scientists constantly project what they want to know as quasi-metaphors and engage with research by the guidance of quasi-metaphors. Successful research constantly transfers their assumptions to access; unsuccessful research suggests the knowledge projection is inadequate. Quasi-metaphors, therefore, are the growth rings of human knowledge that record every step in the history of epistemology.

CHAPTER SIX

On Precision and Vagueness:
Quasi-Metaphors and Reference

> [T]he most important and most fruitful concepts are those to which it is impossible to attach a well-defined meaning.
>
> (Kramers 1)

A few years ago, I attended an international conference on natural products in chemistry. During a break, I saw that three chemists from China, Japan and Germany were discussing their research. It seemed English did not work well for their communication, so the group moved to a white board. By drawing chemical structures and reactions, the discussion continued. Chemists like other scientists believe that scientific language expresses precisely what they do. They neither think that scientific language could be vague nor agree that terms like the gene are metaphorical. Such a view is rather positivist, and contemporary thinkers, such as Kuhn, Burke, Foucault, Boyd, and Derrida, oppose it. To them, language is ultimately metaphorical, and its precision is just a delusion. The previous case studies of phlogiston and the gene quasi-metaphors, however, suggest a view different from scientists and the thinkers. On one hand, it is undeniable that the terms like gene and genome as quasi-metaphors cannot be defined precisely, at least for a period in their history; on the other hand, terms like codon, intro, and exon have always had clear and stable meanings. Vagueness and precision thus seem to comfortably co-exist in scientific language. Both scientists and thinkers are not wrong about scientific language. Nevertheless, each side emphasizes one particular aspect such that it loses the sight of the other aspect.

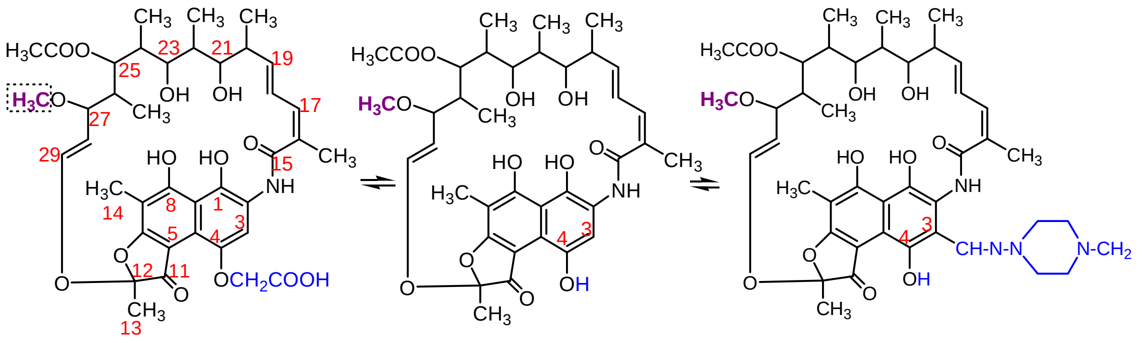
This chapter first discusses what precision means to working scientists. In the process of identifying a piece of DNA sequence as a gene, precision means that the nomenclature system can represent chemical structures. Moreover, experimental narrative can function as an instructor, and it thus has a role as a technique. Kripke's causal theory of reference explains the former; and Putnam's, later. The vagueness of scientific language, however, lies in the quasi-metaphors that cannot be defined. Four approaches to the vagueness—referential indeterminacy, reference potential, nondefinitional reference fixing, and reference as the unknown—are compared. Scientific research is a dynamic process mediated by language: the precision of scientific language plays a role as a technique in research, while the vagueness of quasi-metaphor maintains the coherence of research, allows the productivity, and eventually builds the complexity of world view.

Precision in Scientific Practice

In dealing with the meaning of the gene, two scholars take a pragmatic approach. Fogle considers that the identification of a gene is a "methodological process," in which a gene is recognized as an individual entity with specific structure, expression and function properties. To Rheinberger, the gene is one of the "epistemic objects" as "material entities or process" that embody what is not known yet. Both have a positive attitude toward the vague meaning of the gene. Yet to Fogle, the gene concept represents a collection of flexible characteristics; to Rheinberger, the gene is an epistemic object of inquiry constituted by "physical structure, chemical reactions, [and] biological functions." In doing so, Fogle has to reject the Mendelian conception of the gene, which could not have any characteristics, because such conception was developed before the discovery that the gene was DNA--that is, when the gene was a quasi-metaphor. Rheinberger's difficulty is to explain, if the gene as an epistemic object that presents itself "in a characteristic, irreducible vagueness," then in practice, how scientists can do something or know something about it. In other words, how to explain what Gaston Bachelard calls the “scientific reality” to which all scientists always cling. Hence, Rheinberger has another concept, *technical object*, which parallels to *epistemic object*. By technical object, he means “experimental condition” or “method;” the function of a technical object is to articulate and entrench a epistemic object (*Toward a History* 29, 31). Rheinberger concludes that “the [experiment] systems, then may be governed by *différance*,” because such systems must be capable of being differentially reproduced in order to serve and behave as machines for generating the future (*Toward a History* 28, 29, 31, 234).

Language is not a major concern of Rheinberger’s studies, but it is of mine. My project shares the same departure point, the vagueness, though to him, it is that of epistemic objects, and to me, that of quasi-metaphors. From my perspective, the reason why scientists never question the precision of their language is exactly because their language can provide accurate differentiation at every level of practice. Here is an example from my own lab work that demonstrates how language functions in scientific research. In the late 1950s, scientists discovered that the fermentation of bacterium *Amycolatopsis mediterranei* produced a chemical named rifamycin B. After a few steps of chemical reactions, rifamycin B could be made into a medicine of highly oral activity in the treatment of tuberculosis, leprosy, and AIDS-associated mycobacterial infections. It has two generic names: rifampin (US) and rifampicin (elsewhere) (Figure 6-1).

The structural difference between Rifamycin B and Rifampin, or between a chemical and a clinically active drug, is merely the functional groups at C-3 and C-4--(the parts in blue) (Arora and Main 178). Calling sugar $C_6H_{12}O_6$ and salt NaCl may not seem a great clarification, but the references of the formulae are considerably more precise than those of *sugar* and *salt.* For much laboratory work, further precision about the type (isomer) of $C_6H_{12}O_6$ is needed and also



Rifamycin B

Rifamycin SV

3-(4-methyl piperazinyl)-iminomethyl rifamycin SV
Rifampin/Rifampicin

Figure 6-1: Rifamycin B and Rifampin

available in the nomenclature. Without such a nomenclature system, scientists would not be able to conduct the research and also communicate the results.

By the early 1980s, the general opinion was that tuberculosis was effectively controlled. In early 1990s, however, the World Health Organization declared tuberculosis “a global emergency” due to the spread of multi-drug resistant tuberculosis that resists rifampin and isoniazid, the two standard and most potent anti-tuberculosis drugs (*Tuberculosis*, World health Organization). One of the approaches to search for new active drugs is to modify the chemical structures of the old ones. While chemical modifications on rifamycins did not introduce any promising drugs, the development of molecular biology made it possible to alter the structure of rifamycins through gene manipulation. A piece of 120 Kb (Kilobase) DNA that accounted for most, if not all, of the genes necessary for rifamycin biosynthesis in *Amycolatopsis mediterranei* was sequenced and then compared with the sequences of known genes in genetic database. The result is shown below in Figure 6-2. Each number or letter represents an Open Reading Frame (ORF[44]) that has homologous sequence with a known gene, and the function of the known gene

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[44] Discussion about this concept is on page 203.

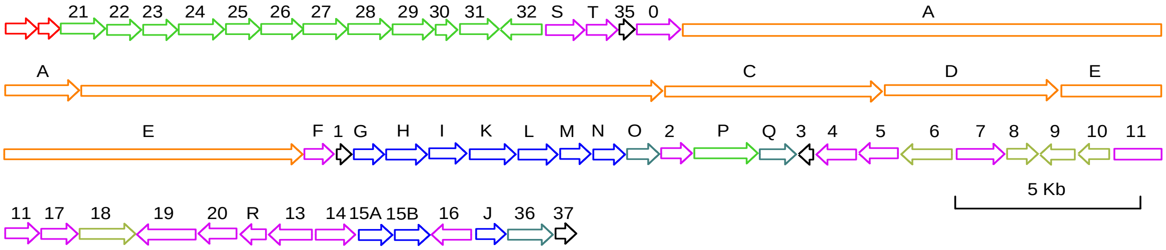

Figure 6-2*:* Rifamycin Biosynthesis Gene Cluster

is considered as the putative function of the ORF. For instance, ORF 14 is 1.4 kb in length and homologous with a gene known to code for methyltransferase, an enzyme catalyzing the transportation of methyl groups. So, ORF14 most likely is the one that brings the methyl group (-CH_3) to the functional group at C-27--(the parts in purple in Fig. 6-1). It is almost impossible to get rid of that methyl group by chemical synthesis, but it might be possible to do so by gene manipulation, namely, by inactivating ORF 14, if it is really responsible for that.

ORF14 was cloned to a specific *E. coli* strain where its genetic code was messed up so that the reading of its sequence was no longer homologous with the known gene coding methyltransferase. It then was transferred back to *Amycolatopsis mediterranei*--that is, the wild type, which accordingly became an *Amycolatopsis mediterranei* mutant with ORF14 knocked out. The fermentation of the mutant produced a chemical that had 14 units of molecular weight[45] less molecular weight. Structural elucidation by Nuclear Magnetic Resonance (NMR) proved that the methyl group at C-27 was lost in the new fermentation product. At this point, it could be concluded that ORF14 had something to do with the methyl group at C-27.

Although knocking ORF14 out did apparently block the synthesis of the methyl group, it was still unknown if ORF14 was the sole gene or one of the genes responsible for the synthesis.

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[45] A methyl group (-CH_3) is weighted 12 + 3 = 15, (C = 13, H = 3). When substituted by -H, the difference of molecular weight is 14.

The next experimental method was *in vitro*, intended to observe the function of the protein expressed by ORF14, presumably a methyltransferase. The idea was to get ORF14 expressed and then isolated outside both *Amycolatopsis mediterranei* and *E. coli* as a pure enzyme. Then, the enzyme was incubated with only the chemical providing the methyl group and the mutant product with the methyl group missing. If ORF14 was the single gene responsible for the synthesis, the mutant product should get the methyl group back at the same position, as the product of the wild type. In practice, ORF14 was cloned into another specific *E.coli* strain that could express proteins. The product from the *E. coli* was isolated and purified (Figure 6-3). The tiny stones were the crystals of expressed ORF14 enzyme. Under proper conditions, incubating expressed ORF14 enzyme with the chemical produced by the mutant with inactivated ORF14 yielded a chemical exactly the same as that from the wild type. ORF14 thus was proved to be a gene named “methyltransferase gene” that coded an enzyme capable of transferring a methyl group. (Unfortunately, the new chemical from this gene manipulation was not active against tuberculosis. It was a successful project, because it proved the function of an ORF not only by the change of phenotype—the structure of a metabolite yielded by the bacterium, but also by

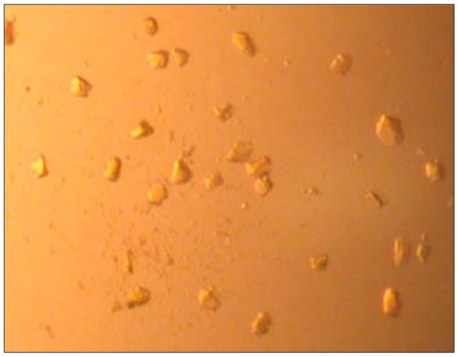


Figure 6-3: Crystals of expressed ORF14, presumably a methyltransferase.

genotype—the mutation induced on the chromosome that caused the change. Nevertheless, it had no immediate use, for the de-methylated rifamycin had no enhanced activity as a drug. Such result is common in research.)

In this process, the scientific reality has two aspects: the difference of fermentation products between the wild type and mutant of *Amycolatopsis mediterranei*, and the function of methyltransferase as the cause of the difference. The products as chemicals could be isolated and purified, and their structural difference could be elucidated by instrumental spectra such as NMR and Mass. The gene is neither abstract nor vague to the scientists. With ORF14, they can read the 1.4 kb DNA sequence and see it by electrophoresis gel (Figure 6-4). When ORF14 is expressed as an enzyme *in vitro*, it is a material as in Figure 6-3. Fogle has a rather firm grasp of the identification of a gene as a methodological process, in which scientists try to determine what

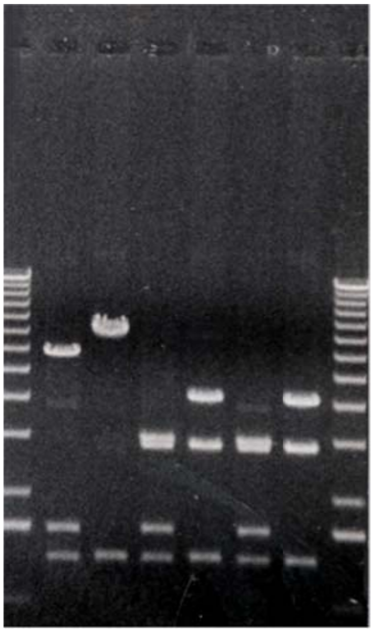


Figure 6-4: Electrophoresis of DNA in gel. (The most left and right lanes are the "DNA ladders" that indicating the size of DNA)

a piece of DNA sequence can do. His view is contemporary and practical, which fits the case of ORF 14 as methyltransferase. To him, the incompatibility of Mendelian gene concept and molecular gene concept is precisely the incompatibility of their research methodologies: the former is search for the gene, and the later, gene function. To Rheinberger, the experimental system as "instruments, inscription devices, model organisms, and floating theorems" is a "technical object," through which the gene as an "epistemic object" is "articulated and entrenched." Yet the material objects, methyltransferase gene and alike, are absent in his theory. To me, these material objects lie between the gene concept and the experimental system. As in the case of ORF14-methyltransferase gene, what the techniques and instruments apply to is these material objects; and through them, quasi-metaphors like gene function are articulated--though in this particular case, only one of the gene functions. There are regulatory genes, transporter genes, as well as genes with unknown function in the gene cluster. Hence, technical objects in my perception should be the material objects like methyltransferase gene and methyltransferase enzyme. Each technical object has a unique name and unique known function; there is nothing vague about them. The vagueness of quasi-metaphor *gene function* is regarding how to accommodate all the diverse functions; or, how unite those functions in a united definition.

In the process of research, the language functions to represents accurately the nuances that the project addresses at every level. The nomenclature system makes it possible to distinguish the structural difference in Figure 6-1. Moreover, the same chemical is represented by different ways at different levels yet the precision is maintained. In Figure 6-5, the structures of each DNA base are important to biochemists, like Chargaff and Sanger, who wanted to understand their chemical behaviors and how to separate them. At sequence level, as in ORF 14,

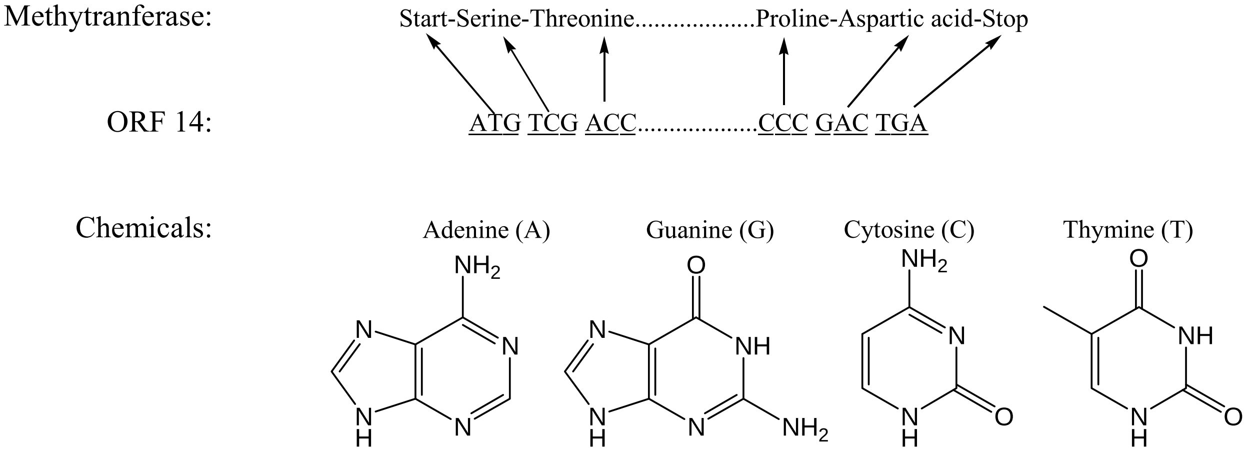


Figure 6-5: Linguistic characteristics at different level

the importance becomes the order of the chemicals, so they are presented in the simplest way. When translated, then the focus shifts to the order of amino acids that the sequence codes. How a chemical is represented indicates the epistemic categories within which it functions differently.

Moreover, the language also has a technical role. For instance, in research, a routine operation is to isolate DNA from bacteria. A standardized protocol is established by Sambrook and Russell from Cold Spring Harbor Laboratory, part of which is below (1.32):

METHOD

Preparation of Cells

1. Inoculate 2 ml of rich medium (LB, YT, or Terrific Broth) containing the appropriate antibiotic with a single colony of transformed bacteria. Incubate the culture overnight at 37°C with vigorous shaking.

   To ensure that the culture is adequately aerated:

   - The volume of the culture tube should be at least four times greater than the volume of the bacterial culture.
   - The tube should be loosely capped.

   - The culture should be incubated with vigorous agitation.

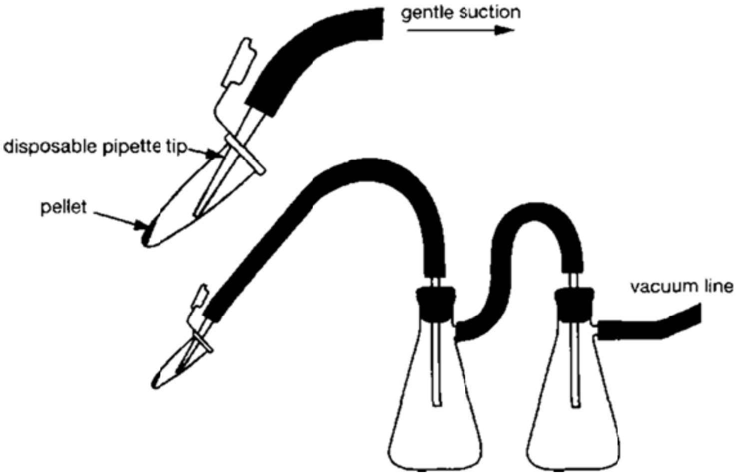
2. Pour 1.5 ml of the culture into a microfuge tube. Centrifuge at maximum speed for 30 seconds at 4°C in a microfuge. Store the unused portion of the original culture at 4°C.

3. When contrifugation is complete, remove the medium by aspiration, leaving the bacterial pellet as dry as possible.

> This step can be conveniently accomplished with a disposable pipette tip or Pasteur pipette attached to a vacuum line and a side arm flask (please see Figure Vacuum Traps). Use a gentle vacuum and touch the tip to the surface of the liquid. Keep the tip as far away from the bacterial pellet as possible as the fluid is withdrawn from the tube. This minimizes the risk that the pellet will be sucked into the side arm flask. Alternatively, remove the supernatant using a pipette or Pasteur pipette and bulb. Use the pipette tip to vacuum the walls of the tube to remove any adherent droplets of fluid.
>
> The penalty for failing to remove all traces of medium from the bacterial pellet is a preparation of plasmid DNA that is resistant to cleavage by restriction enzymes. This is because cell-wall components in the medium inhibit the action of many restriction enzymes. This problem can be avoided by resuspending the bacterial pellet in ice-cold STE (0.25 x volume of the original bacterial culture) and centrifuging again.

4. Resuspend the bacterial pellet in 100 of ice-cold Alkaline lysis solution I by vigorous vortexing.

> Make sure that the bacterial pellet is completely dispersed in Alkaline lysis solution I. Vortexing two microfuge tubes simultaneously with their bases touching increases the rate and efficiency with which the bacterial pellets are resuspended.
>
> The original protocol (Birnboim and Doly 1979) called for the use of lysozyme at this point to assist in dissolution of the bacterial cell walls. This step can be safely omitted when dealing with bacterial cultures of less than 10 ml in volume.



Vacuum traps

Bench work at large relies on this kind of protocol, which provides not only accurate recipes for all kinds of media and buffers, like LB, YT, or Terrific Broth, but also precise instruction on how to achieve required conditions or obtain target materials. In the narrative of the protocol, every term has its clear and stable referent. If the referent is not specific, such as "a microfuge," without description of brand or scale, then the operational outcome is secured by the operational conditions, such as "maximum speed for 30 seconds at 4°C." When the referent is not well-known, like the vacuum traps, the protocol illustrates the image to void confusion or misunderstanding. These protocols are a part of the interaction among scientists, instruments, and experimental materials. If they are not as accurate and precise as the instruments can be, the instruments would be useless to scientists. The protocols are as important as the instruments, and scientists have no reason to doubt the precision of the language as long as they can obtain expected outcomes according to the instructions.

Precision and Reference

The language in the case of methyltransferase exhibits the positivist view of scientific language as "impersonal, mechanical, third person, deanthropomorphized explanation of reality… a science (logos) seems to liberate us from metaphor" (Edie 162). As discussed in Chapter 2, Bacon is the precursor to positivist science as well as plain style. Here is how he describes an experiment:

> Try an experiment with burning-glasses in which (as I recall) the following happens: if a burning-glass is placed (for example) at a distance of a span[46] from a combustible object, it does not burn or consume as much as if it is placed at a distance of (for example) a half-span, and is slowly and by degrees withdrawn to

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[46] A span equals to nine inches.

> the distance of a span. The cone and the focus of the rays are the same, but the actual motion intensifies the effects of the heat (*The New Organon* 123).

He narrates experiments in present tense as what happens rather than what happened, in the form of instructions on what to do in order to obtain the claimed results. His writing appears to reveal, instead of telling, the facts of natural world, such that the agency between the reader and the natural world disappears.

The Follows of the early Royal Society adopted the Baconian form. One of them was Robert Boyle (1627-1691). His "experimental essays" exemplify a "naked way of writing," an effort to eschew "florid style." The essay is made up of piecemeal reporting of experimental trials, explicitly contrasted to the natural philosophical system. Steven Shapin and Simon Schaffer identify the essay as a "literary technology," which functions to provide "virtual witness" by "the production in a *reader*'s mind of such an image of an experimental scene as obviates the necessity for either direct witness or replication" (60, emphasis original). Boyle's investigation with the air-pump was not intended to build a theory about the possibility of void space, but rather to establish the springiness of the air. To historians like Butterfield who focus on theoretical sciences, "Boyle made very few important discoveries or strategic changes in science" (139). Ironically, Butterfield's summary of Boyle''s merits, that "[h]e kept careful register of observations himself and insist on the importance of recording experiments, of confirming them by unwearied repetition, and of distrusting a great of amount of what purported to be the published record of experiments," is what every empirical scientist is trained to perform in the lab today (140). Boyle attempted to reshape chemical discourse through his own writing and his criticism of the chemical texts then (Golinski 384). Similar to Lavoisier but much earlier, he pointed out the ambiguity of those texts in his *The Sceptical Chymist*:

> I find that even Eminent Writers, …do so abuse the termes they employ, that as they will now and then give divers things, one name; so they will oftentimes give one thing, many Names; and some of them (perhaps) such as do much more properly signifie some Distinct Body of another kind ; nay even in Technical Words or Termes of Art, they refrain not from this Confounding Liberty; but will, as I have Observ'd, call the same Substance, sometimes the Sulphur, and Sometimes the Mercury of a Body (113).

Boyle was acutely aware that, as material practice, an empirical science like chemistry should have a language whose terms have stable and unique reference.

Since John Locke (1632 - 1704), the empiricist view on precision is that natural kind terms are associated with stable and explicit definitions of their extensions or referents. Among theories regarding the reference of natural kind terms, one is the Simple Nominal Essence theory, proposing "a natural kind term is associated with a (usually complex) open sentence whose constituent expressions are (a) predicates that can be applied on the basis of observation and (b) the connectives of propositional logic; the natural kind to which the term refers is just the set of things that satisfy the open sentence." For instance, the open sentence associated with gold is that "gold is metal, and shiny, and yellow, and ductile, and so on." The other is the Simple Real Essence theory, suggesting "that a natural kind term is associated with a sample of some substance, and that the term refers to the set of things that share the same inner constitution as the sample." Take gold as an example again: gold is the kind of such things as the same inner constitution as the sovereign in Locke's pocket (Standord and Kitcher 99).

The empiricist view fits the Simple Real Essence theory, which is the simplest version of a causal theory of realism. On Saul Kripke's account, the reference of natural kind terms appeals to internal constitutions alone. For instance, if a substance has an atomic number 79 (that of gold) and a green color, the conclusion is that not all gold is yellow. On the contrary, if a substance looks like gold yet has different atomic number, the conclusion is that the substance is not gold but seems like gold (123 – 125). Hence the atomic number is decisive and is determinable, though in practice determining the atomic number of a (presumably pure) sample is not a simple matter. Hilary Putnam emphasizes that the "ostensive character" of reference of natural kind terms will be the local samples to which the term is applicable, and anything else in the world that bears a particular "sameness relation" to them. For instance, ostensive definition of water has the following empirical presupposition: the body of liquid bears a certain sameness relation to most of the stuff that a linguistic community calls 'water' (225). Chemical nomenclature system is the realization of Kripke's causal theory. The name of a chemical corresponds to its structure, which determines its chemical properties. For example, H_2O determines that the pure material constituted by this molecule is colorless, transparent, tasteless, with a boiling temperature of 100^0C and freezing temperature of 4^0C. Moreover, structural differences between chemicals can be indicated by the name, as the chemical name of Rifampicin is 3-(4-methyl piperazinyl)-iminomethyl rifamycin SV, referring to the functional group at C-3 that Rifamycin SV does not have, while rifamycin is a family name of a group of chemical that have a macrocyclic lactam structure in common. Putman's ostensive definition is especially important to apparatus like the vacuum traps that have to be assembled by whoever does the experiment. The language with which scientists work does have a causal relation between the terms and their reference.

Vagueness in Scientific Language

Nevertheless, the precision of scientific language is called into question by the realization that some important concepts cannot even have a stable and clear definition. The gene is one of the famous cases. Mass is another one: as fundamental as it is, physicists still cannot achieve an agreement on its definition. A contemporary physicist, Wendy Padgett, remarks that "mass is a mess." She argues that "[t]he attempt to distinguish between inertial mass and gravitational mass is meaningless in the present state of confusion about the definition of these terms," such as gravitational force and contact force (181). Max Jamer's examination of mass concept ranges from inertial mass, to relativistic mass, to velocity-independent mass, as well as the trichotomy of mass as inertial, active gravitational, and passive gravitational mass. He concludes that, in spite of all the strenuous efforts of physicists and philosophers, the notion of mass is still shrouded in mystery (167).

Similar, Kitcher points out, are terms like *magnet*, *temperature*, *acid*, *compound*, *species, planet*, *electrical attraction*, *molecular*, and *homology*, together with *elements*, *space*, and *time* on Kuhn's list; cases like *mass* and *the gene* are not rare. The vagueness of their terms is first of all their vague reference. So far, the approaches are Field's referential indeterminacy, Kitcher's reference potential, Boyd's nondefinitional reference fixing, and reference as the unknown to quasi-metaphor.

Hartry Field argues that, when Newtonian mechanics is replaced by the special theory of relativity, *mass* "as used before relativity theory was discovered had no determinate denotation." Hence, terms like *mass* are "referentially indeterminate," namely, "there is no fact of the matter as to what they denote (if they are singular terms) or as to what their extension is (if they are general terms)" (462). John Earman argues against referential indeterminacy, because there is "strong evidence" supporting the claim that "Newtonian term 'mass' has the same denotation as the special relativistic term 'proper mass'" (535). Earman's rebuttal is about what exactly *mass* refers to, rather than the concept referential indeterminacy itself. Even though Newtonian mass has the same denotation as the special relativistic proper mass, the reference of *mass* is still open. The strength of reference indeterminacy as an approach is that it can approach a term through its developmental process, from a vague idea to a mature term with reference established. If applied to the gene, one can argue that before the discovery of the double helix, the gene had no determinate denotation; and after that, it had. The problem with an open-ended case like mass is that applying referential indeterminacy becomes rather difficult, because scientists do not agree that relativity theory is the answer, unlike what Earman argues. As *Mass* remains referentially indeterminate, what happens from Newton to Einstein? And what is the relationship between the two mass concepts?

Like Field, Kitcher also perceives that the problem with reference of the terms is how they change as theory changes, although Kitcher's approach is built around reference potential. In general, as far as Kitcher is concerned, conceptual change is change in reference potential, defined as:

> The reference potential of a term for a speaker is the set of events which a speaker is disposed to admit as initiating events for tokens[47] of that term. A linguistic community, with respect to a term, is a set of individuals disposed to admit the same initiating events for tokens of the term. An event is the initiating event for a token if the hypothesis that the speaker referred to the entity singled out in that event provides the best explanation for her saying what she did. Explanations are judged by their ability to provide a picture of the speaker's intentions which fits with her environment and history and with the general constraint of the Principle of Humanity. Three kinds of intentions are prominent: the intention to conform to the usage of others, the intention to refer to natural kinds, and the intention to refer to what can be specified (*Gene* 346-347).

Reference potential and quasi-metaphor share the element of reference uncertainty. "[T]he intention to refer to natural kinds, and the intention to refer to what can be specified" is another way of elaborating the conception of a quasi-metaphor, as to project the known existence in an epistemic category to a virtual existence in an epistemic or presupposed category. By

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[47] Kitcher's conception of token is that "the idea is that the production of the expression-token is the terminal event in a sequence of events which would be described in detail by the correct (and complete) explanation of that terminal event. This sequence links the expression-token produced to an entity singled out in the first event of the sequence, and that entity is the referent of the token" (525).

emphasizing the intension of the speaker, however, Kitcher puts the communicative function of a term over its putative causality. Further, in this statement:

> Discoveries and acceptance of new hypotheses can enlarge the reference potential by suggesting new kinds of causal interaction or new descriptions through which the reference of tokens of the term can be fixed (*Genes* 346).

By putting discoveries and acceptance of new hypotheses as equal contributors to extend reference potential, Kitcher hints that acceptance of new hypotheses, as a social agreement, is not solely based on discoveries. In other words, a scientific community chooses whether to accept results from research. In doing so, a particular reference potential becomes the choice among potential references by scientific community. In Kitcher's application of reference potential to the gene, his contextualizes the gene concept in genetics. To him, the early *gene* referred to a set of chromosomal segments each of which plays a functional role in the determination of phenotypic trait (350). Molecular biology brings the possibility of many different concepts of the gene, generated by different decisions about the phenotypic level. These concepts may be useful for different areas of research. *Gene* thus has a highly heterogeneous reference potential. The study on quasi-metaphor gene, on the other hand, only emphasizes its epistemic assumption, or speculative causality. The gene and genetics are tightly related, but as its history shows, the gene has not always been about genetics. Molecular biology, as discussed, was about gene functions.

Kitcher's case study of phlogiston concludes that the reference potential of terms such as *phlogiston* and *dephlogisticated air* are heterogeneous. *Phlogiston* was used on record by Priestley and Kirwan to name both oxygen and hydrogen (*Theories* 534). On the other hand, Priestly and Cavendish used *dephlogisticated air*, supposedly referring to the air without phlogiston, for oxygen. Even worse, the modes of reference are connected by a faulty theoretical hypothesis, that “there is a substance which is emitted in combustion and which is normally present in the air. The result of removing this substance from the air is a gas which can also be produced by heating the red calx of mercury.” Lavoisier made a conceptual advance by revising reference potentials so as to avoid presupposing a false hypothesis (*The Advancement* 102- 103). As a quasi-metaphor, phlogiston was overthrown because its epistemic assumption, combustibility is a material that phlogiston stood for, was wrong. Lavoisier’s proposal of naming dephlogisticated air as oxygen was accepted together with his proposal to conduct chemical research through the methodology of empirical physics, newly invented instruments, and especially his nomenclature system to name chemicals according to their structures, or, the causal reference of chemical names in Kripke’s sense.

Boyd’s approach is different from Field’s and Kitcher’s. His concern regarding the significance of the terms is their “programmatic features” as “theory-constitutive metaphorical expressions” (495). He discerns the paradoxical situation in which the terms referring to “presumed kinds of natural phenomena” are introduced in order to conduct research to understand the very phenomena. It is usually long before the point when the essence or causality of the reference is specified, which is a crucial condition for positivists to define a term. Yet the terms should have some tentative or preliminary indication of the properties of the presumed kinds, which maintaining programmatic open-endedness. Metaphors serving such a purpose are “a nondefinitional mode of reference fixing which is especially well suited to the introduction of terms referring to kinds whose real essences consists of complex relational properties, rather than features of internal constitution” (483). As discussed in Chapter Two, Boyd comes to the very tricky point in scientific research where one has to name something by its essence before it is discovered.

Quasi-metaphor theory departs exactly at this tricky point, although the proposal of a quasi-metaphor is to frame the unknown through what is known. Hence, the causality of the reference regarding a quasi-metaphor is not important; rather, the projected known existence and its epistemic category are crucial, for they are the matrix of the quasi-metaphor, and they suggest the tentative or preliminary indication of the properties of the presumed kinds that Boyd wrestles to identify for his scientific metaphors. Quasi-metaphors are not nondefinitional, because they are proposed with epistemic assumptions, though like metaphors, they are open-ended. Quasi-metaphors are not nonreferential, either. The reference of a quasi-metaphor is the unknown, or more accurately, the unknown defined by known. When the unknown is recognized, it immediate signifies its affinity to the known, and it thus obtains the possibility to become known. Different from Boyd's scientific metaphors, a quasi-metaphor can refer to either speculatively real essence or internal constitution. For instance, the proposal of *gene* as hereditary material could only be done when the taxonomy of the natural world and experiments on breeding and hybridization brought understanding to establish the basics of heredity. *Gene* as material reality is essence, for such assumption endows the gene general properties as any other chemicals existing; thus the gene is detectable and traceable by the means that detects and traces chemicals. The discovery of the double helix is thus recognized as the decisive even to the gene, for it is the last solid piece of evidence that the gene is a chemical named DNA. *Gene* as constituent of genotype is internal constitution, suggesting that where genotype is, where the gene is—to put in Morgan's words, that "the gene is a material unit, it is a piece of a chromosome; if it is a fictitious unit, it must be referred to a definite location in a chromosome" (315). The establishment of the gene as DNA is not a process in which the gene is fixed to be DNA, as Boyd describes; nor is that in which DNA was waiting to be found. It is a dynamic process in which the epistemic assumption of the gene was progressively narrowed down and chemical properties of DNA were understood better so that the two could be put together.

Precision, Vagueness, Productivity and Complexity

Scientists engage with nature through quasi-metaphors such that they acquire the intelligibility of the word. The characteristic of quasi-metaphors is vagueness; whereas the intelligibility must be precise. By either transforming or rejecting epistemic assumptions, the unceasing production of scientific knowledge keeps refining and extending the intelligibility, hence constantly shakes the precision. Such is a dynamic system of complexity, so is its language.

The linguistic precision has two levels, the technical and the categorical. The language of technicality is relatively stable, because it has causal reference, such as names of chemicals in the nomenclature system, as well as description of experimental conditions. Rheinberger puts experimental conditions as technical objects, the opposite of epistemic objects, because they are the stable and precise part. So is empirical language, and particularly the language, because without the stability and precision of the language, to communicate experimental techniques is not possible. In other words, delocalizing a technique developed by a specific lab through the protocol, like the case of DNA isolation protocol by the Cold Spring Harbor Laboratory, becomes impossible.

The next level is categorical precision. Namely, the intelligibility comes with organization of epistemic categories, and reference is not only about the name of the natural kind, but also its categorical position. In regard to revolutions as changes of world view, Kuhn describes:

> [P]aradigm changes do cause scientists to see the world of their research-engagement differently. In so far as their only recourse to that world is through what they see and do, we may want to say that after a revolution scientists are responding to a different world.
>
> It is as elementary prototypes for these transformations of the scientist's world that the familiar demonstrations of a switch in visual gestalt prove so suggestive. What were ducks in the scientist's world before the revolution are rabbits afterwards. The man who first saw the exterior of the box from above later sees its interior from below (111).

In order to illustrate "a switch in visual gestalt," he employs a metaphor that the revolution changes what are ducks into rabbits. Kuhn is obviously not aware of the game to name things, for if he had known, he should have provided the visual thing (or things) that he refers as *duck* and *rabbit*. There are two possible interpretations of his metaphor, as Figure 6-6:

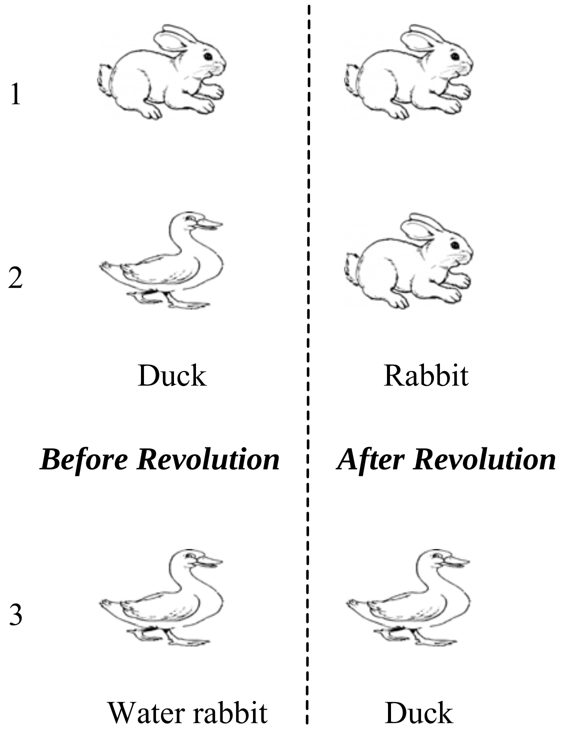

Figure 6-6: Change of views by scientific revolutions.

First, a referent called a rabbit now is named *duck* (erroneously) before the revolution but changed to *rabbit* after it; second, a referent called a duck is transferred into a referent called a rabbit by the revolution (magic). In this passage, it is impossible to determine which interpretation Kuhn means, thanks to the vagueness of metaphor, but by contextualizing the metaphor in Kuhn's case studies, the meaning might be clarified. Take his favorite case, the chemical revolution. Lavoisier renamed Priestley's dephlogisticated air as oxygen, which is not the second situation, for the gas as the referent was not changed. So Kuhn means the first, in which what the referent is does not matter, and the issue is rabbits and ducks switch names--*duck* is no better *rabbit,* and *vice versa.* Accordingly, the acceptance of the name oxygen is only because of Lavoisier's successful political campaign.

By the quasi-metaphorical analysis in Chapter Three, the change of world view should be the third interpretation: a referent that we call duck is named water rabbit before the revolution but changed to duck after it. Or more precisely, Lavoisier insists that the category to which water rabbits (*dephlogisticated air*) belong does not exist, so the referent (the species of gas) named water rabbit (*dephlogisticated air*) should be named as duck (*oxygen*). In this sense, the chemical revolution can also be understood as a revolution in epistemic categories: the referent--the species of gas that support combustion, respiration and can make acids has never been changed; to rename it from *dephlogisticated air* to *oxygen* is to move it from the phlogistic category of alchemy to the elementary category of modern chemistry. The technical and categorical precision produces experimental data, precise and accurate, which articulate objects[48] like methyltransferase, which in turn, become material constitution of the gene/gene function; or

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[48] I define Rheinberger's technical objects as methodological commitment, or experimental conditions, and materials to which the technology and instruments apply as technical objects.

objects like oxygen whose old name only serves as a reminder of false epistemic assumption.

The vagueness of quasi-metaphors is able to accommodate their productivity. In the case of the gene, the productivity has two patterns. One is the confluence in which the developments of different disciplines substantiate the material reality of the gene. In the process, connections among terms are also established: the gene, chromatin, chromosome, enzyme, protein, and DNA. Their relation, such as the gene is DNA, protein is the expression production of the gene, and the gene is located on the chromosome, and so on, is epistemic access--the knowledge of heredity and of nature. Another is dispersion in which quasi-metaphors derived from a concept develops simultaneously. More terms, such as gene action, gene function, genome, and so on, are produced; they in turn, produce more. For instance, under gene action, there are code, codon, translation, transcription, expression, intro, exon, and so on. This is a dynamic system in which the precision effectively and efficiently produces data that constantly modifies or transfer or disproves epistemic assumptions. Quasi-metaphors hence keep evolving such that their meanings are changing and meanwhile they obtain historical complexity. Those whose assumptions are confirmed produced more quasi-metaphors, each of which starts their own evolution. Quasi-metaphors and the quasi-metaphors of quasi-metaphor form network complexity. In so doing, the language, as well as knowledge, turns into a complex system.

Those quasi-metaphors that run through the history of civilization, on one hand, indicate how much has changed in the picture called reality, and on the other hand, suggest the unknown remains vast. Burtt praises that “Newton was the man who took vague terms like force and mass and gave them a precise meaning as quantitative continua,… he gave new meanings to the old terms space, time, and motion, which had hitherto been unimportant but were now becoming the fundamental categories of men’s thinking” (19-20). The same tribute can be said about Albert

Einstein and Stephen Hawking. There is no more beautiful and more elegant example to illustrate the evolution of a quasi-metaphor than Hawking's theoretical models in *A Brief History of Time*, as the history of human attempts to explain the universe. It cannot be more obvious that knowledge is not about the world but about "us," who claim the knowledge.

Those who conceived the tortoise universe, or no boundary universe, or string universe, met their world through their quasi-metaphors of their epistemic assumption and access. We meet our world through the same quasi-metaphors, but with our epistemic assumption and access, as well as our quasi-metaphors of our epistemic assumption and access, like genome. Upon quasi-metaphors, we build a world that we know.

Figure 6-7: Some of the theoretical models attempt to explain the universe (Hawking 229)

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