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In praise of carbon: Genre and the 1996 Nobel lectures in chemistry by Christian Fredrick Casper

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IN PRAISE OF CARBON

GENRE AND THE 1996 NOBEL LECTURES IN CHEMISTRY

by

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Dedication

For my parents

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Abstract

Using genre theory along with the hierarchy of scientific statement types of Latour and Woolgar (1979) and the scientific *stases* and scientific *topoi* of Prelli (1989), this thesis analyzes the three Nobel lectures in chemistry from 1996 to discern the characteristics of ceremonial discourse in science and its relation to scientific ethos. Throughout, the Nobel lectures are analyzed in reference to the original research reports published in the scientific literature. The unique social context of the Nobel lectures results in a distinct genre with textual characteristics that include statements with little moderation or hedging, arguments for the long-term significance of the work, recognition of colleagues and coworkers, and, significantly, discussion of the nature of science itself and of proper conceptual approaches and procedures.

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Chapter 1: Introduction and Literature Review

To an eminent scientist, a slumber-interrupting phone call is a most welcome surprise in the pre-dawn hours in early October. When a scientist is informed by the Nobel committee that she has been selected to receive a Nobel Prize (for a winner living in North America, this usually happens before sunrise), she becomes a member of the most elite scientific fraternity. Awarded each year for achievements in each of the fields of chemistry, economics, physics, physiology or medicine, literature, and peace, the prize can be split at most only three ways in each of the science categories, elevating a select few into its pantheon and leaving behind a great many more scientists with equally impressive or, arguably, more impressive achievements. Zuckerman (1977) reported in her sociological study of Nobel laureates in the United States that the U.S. had one Nobel laureate for every 6800 self-defined scientists and for every 13 members of the elite National Academy of Sciences (p. 10). Eminent scientists who are considered worthy of the prize but are denied it are so plentiful that they have a collective descriptor: They are said to occupy the “forty-first chair,” drawing a parallel with those “immortals” left out of the forty seats in the French Academy (p. 42).

Upon their arrival in Stockholm in early December to receive the prize (except in the case of the peace prize, which is awarded in Oslo), the new Nobelists, as they are commonly called, are treated to a whirlwind week of activities, including a royal banquet and the ceremony itself, which is held always on December 10, the anniversary of Alfred Nobel’s death. It is during this ceremony that each laureate receives the prize from the hands of the king of Sweden. The laureates are also invited to deliver a lecture about their work, which provides the topic for this thesis. These lectures represent a relatively unexplored area in the rhetoric of science: ceremonial discourse in science. How do scientists construct ceremonial discourse about their research vis-à-vis the original reports in scientific journals?

The focus of this study is the 1996 Nobel lectures in chemistry, delivered by Robert Curl of Rice University in Houston, Texas; Harold Kroto of the University of Sussex in the United Kingdom; and Richard Smalley, also of Rice University. (Print editions of the lectures [Curl, 2003; Kroto, 2003; Smalley, 2003] are available for download at the Nobel Foundation website, http://nobelprize.org, as of early 2005.) These scientists were honored for their discovery of a previously unobserved form, or allotrope, of carbon, called fullerene. This allotrope was so named because it takes the form of a sphere with its surface made up of adjoined hexagons and pentagons—in the case of the 60-carbon-atom form, matching exactly the pattern on a soccer ball. The pattern closely resembles that on a geodesic dome, which was popularized by the American architect R. Buckminster Fuller. This discovery gave birth to entirely new branches of organic and inorganic chemistry and nanotechnology. A brief overview of this discovery and related work is given in Chapter 2. The remainder of this chapter introduces rhetoric and the concepts used in this thesis.

Rhetoric

Rhetoric is a broad field generally encompassing the study of both oral and written discourse. Aristotle (trans. 1991) conceived three basic divisions of rhetoric: forensic, deliberative, and epideictic. Forensic rhetoric deals with events in the past, deliberative deals with future events, and epideictic is usually defined as ceremonial rhetoric whose purpose is to praise or blame. Epideictic rhetoric is frequently characterized by amplification; the object of praise is made to sound greater than it was, and the object of blame is made to sound even viler. Classical texts on rhetoric were primarily concerned with political discourse in the proto-democracy of ancient Athens, but the tools of rhetorical analysis have since been applied to virtually all forms of discourse, both literary and non-literary. Various scholars have applied all three of Aristotle's classes of rhetoric, to varying degrees, to studies of scientific discourse.

Scientific rhetoric. Original scientific reports in the primary literature are clearly forensic. They defend the results of an experiment and the interpretation of those results. Review articles are primarily deliberative. By examining the current literature, they seek to define a field and to direct future research (Myers, 1991). The place of epideictic discourse in science is harder to define, but Gross (1990) claims that a scientific paper exhibits characteristics of all three divisions, including "epideictic because it is a celebration of appropriate methods" (p. 11). Certainly it could be argued that review articles also celebrate achievement in the area that they cover. Sullivan (1991) isolates five functions of epideictic rhetoric—education, legitimation, demonstration, celebration, and criticism—and claims that each is realized in scientific writing. The purpose of this thesis, however, is to explore a corpus of scientific texts—Nobel lectures—that were created specifically for ceremonial purposes and therefore offer an opportunity to examine truly epideictic scientific discourse.

Epideictic rhetoric of science, if considered in some depth, would seem to be an oxymoron. Sullivan (1993) describes forensic, deliberative, and epideictic rhetoric in terms of the appeals to which they are most related. Logos (logic), a central characteristic of most scientific discourse, is related to forensic and deliberative rhetoric. Epideictic rhetoric, because it doesn't try to win an argument, but only solidify shared values, relies very little on logos and much more on pathos (the emotional response of the audience) and, especially, ethos (the reputation of the speaker). Merton (1973) describes the ethos of science as a set of norms and values "held to be binding on the man of science": universalism, communality, disinterestedness, organized skepticism, originality, and humility (p. 269). These mores reflect the value that scientific results reveal the objective physical nature of the universe and are not to be judged on the basis of the scientist's gender, race, or national origin, nor are they the privileged property of the scientist. In this view, science is a process whereby facts about the universe are revealed and must not appear to be affected by human bias. In effect, ironically, the scientist must construct an ethos that denies that she is making an appeal to ethos in the first place.

One distinguishing characteristic of rhetoric is its focus on the directed nature of discourse, the idea that texts are not simply composed in a vacuum but are directed to and received and interpreted by a reader. This focus has led particularly to studies in the rhetoric of science that examine the epistemology of scientific research. Gross (1990), in one of the first book-length studies of the rhetoric of science, claims that scientific knowledge is not to be equated directly to physical reality but is rather the body of explanations accepted by the scientific community, described by Kuhn (1996) as a paradigm. Prelli (1989), in another major book-length discussion, focuses more on the nuts and bolts of scientific writing, and his work provides the foundation of much of this thesis. He outlines in detail how scientists find "in each [particular] case … the available means of persuasion" (Aristotle, trans. 1991, pp. 36–37) and use them to advance their ideas.

This thesis analyzes the arguments made by Curl, Kroto, and Smalley in their Nobel lectures and the arguments in their research papers published in scientific journals. The purpose of this analysis is to elucidate the distinguishing characteristics of the Nobel lectures.

The particular concepts from Prelli that are used in this work are *stasis* theory and common *topoi* in scientific arguments. These are discussed in detail in Chapter 4. Briefly, *stasis* theory traces an argument through “points of stoppage” at questions of fact, interpretation, judgment, and future action. The common *topoi* are recurrent themes used in argument. In the rhetoric of science, these include appeals to the competence of the researcher or to the explanatory or predictive power of a theory or experimental result. An analysis of the Nobel lectures using Latour and Woolgar’s (1979) classification of scientific statements is found in Chapter 3. Latour and Woolgar’s system classifies scientific statements into five numbered types to indicate the level of confidence that a scientist has, or feels is prudent to assign, to the statement.

The common thread between *stasis* theory and Latour and Woolgar’s statement types is that they are most useful when used diachronically, to show the stages though which an argument proceeds with the passage of time. This leads us to a nearly ubiquitous concept in rhetoric: *kairos*.

Kairos. The concept of timing in rhetoric—the structuring of a discourse in response to zeitgeist—is called *kairos*. To be understood correctly, *kairos* must be distinguished from *chronos*. The latter was the Greek term for quantitative, or measurable, time, whereas *kairos* represented qualitative time, “a critical occasion for decision or action” (Miller, 1992, p. 117). In other words, “*kairoi* are important exigences punctuating *chronos*” (p. 117). As an example, Miller uses the pioneering work on DNA by Oswald Avery and by James Watson and Francis Crick to highlight the way differences in the thinking of the scientific community at the times of their discoveries affected how the scientists communicated their results to their colleagues.

Today, DNA is widely known to be the substance that carries the genetic code in humans and all other living things, with the exception of a few RNA-based viruses. However, this was still the subject of intense debate through the first half of the twentieth century. At that time, most biologists believed that proteins carried the genetic code. DNA was known, but its function was a mystery. Because DNA is made up of only four different sugar–phosphate–base building blocks, whereas proteins consist of 20 different amino acids, DNA was thought to be too simple to carry information as complicated as the genetic code. Avery and his colleagues conducted an elegant series of experiments (Avery, MacLeod, & McCarty, 1944) that showed that DNA, but not protein, extracted from dead bacterial cells can alter the genetic makeup of live bacterial cells. Rhetorically, however, there were still problems. Because opinion in the scientific community at the time was slanted so heavily toward proteins as the genetic material, Avery and his colleagues had to tread very carefully and explain their methods in great detail. Halloran (1984) writes that "[t]he character that speaks to us from Avery's paper is that of a cautious skeptic who is forced somewhat unwillingly to certain conclusions" (p. 77). Consequently, their paper lacked the rhetorical punch that the brash Watson and Crick used just nine years later when they announced their solution to the structure of the DNA molecule in a letter to the journal *Nature* (Watson & Crick, 1953) at the end of a furious race with several better-known researchers.

Significantly, Avery, whose work was very arguably at least as important as that of Watson and Crick, never received a Nobel Prize. The significance of his work was not fully recognized until after his death (Zuckerman, 1977, p. 48), and Nobel Prizes are not awarded posthumously. Watson and Crick, on the other hand, received the Nobel Prize in physiology or medicine in 1962 (along with Maurice Wilkins) and went on to scientific superstardom.

One purpose of this thesis is to examine *kairotic* elements in Nobel lectures. How does the story change in the glow of the Nobel ceremony compared to when the glassware is still dirty? A discussion rooted in genre theory should also be illuminating on this point. What characteristics of these discourses change with the genre shift from the research article to the Nobel lecture?

Genre

The research article and the Nobel lecture can be safely said to represent different genres. Traditionally, genres have been defined by literary scholars in terms of textual features: Poetry, for example, has textual characteristics that are different from those of drama and from those of a novel. Further, a sonnet is different textually from an epic poem. This system has been popular in literary criticism because literary criticism, especially in the twentieth century, has often been focused primarily on the writer and textual form. Other discourse, residing in the domain of rhetoric (as opposed to poetic), focuses on the reader: It aims to effect an action by the reader. Miller (1984) defines rhetorical genres as "typified rhetorical actions based in recurrent situations" (p. 159). Any textual similarities between members of the same genre are simply the consequence of the similar social actions that they aim to effect. For example, letters of complaint to the manufacturer of a defective product constitute a genre because they have a similar intended outcome: refunds or replacements. Most scientific writing is suasory, not literary (Prelli, 1989): the goal is to convince others of the validity of one's research. Therefore, we need to apply here the rhetorical, not literary, concept of genre.

Miller (1994) suggested that cultures could be classified by their rhetorical genre sets (p. 70). Graduate students, for example, produce lab notes, lab reports, group meeting presentations, seminar slides, prelim proposals, and dissertations. Faculty produce grant applications, examinations, seminar slides, peer reviews, and student progress reports, among other pieces. Bazerman (1994) describes systems of genres: genres that are begotten by other genres, such as resumes and letters of application that follow from job ads. Similarly, highly regarded research reports often generate popularized or "accommodated" articles about science (Fahnestock, 1986) and sometimes Nobel lectures. So, genre theory can have broad implications in social studies of science.

Accommodating Science

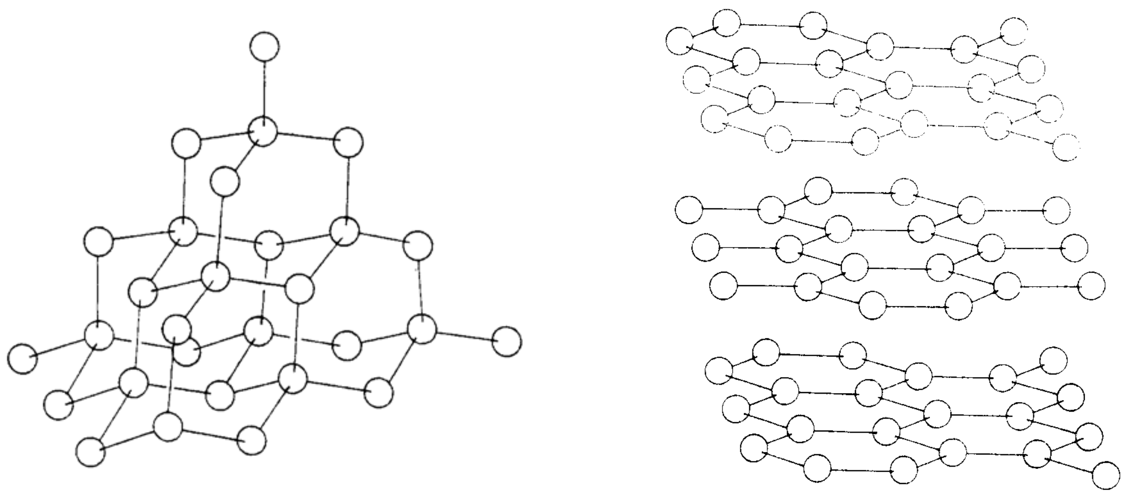
Fahnestock (1986) shows that when scientific knowledge moves from the technical literature to the popular press, it also undergoes a genre shift and transformation from forensic rhetoric to epideictic rhetoric. The knowledge no longer is considered in need of defense and validation, so hedging is removed and more radical claims are made.

It is instructive to consider Nobel lectures in this light. Nobel lectures are similar to "accommodated" accounts in the popular press but aren't exactly the same: They retain much of the technical sophistication found in the original reports, but they have a more conversational style and are clearly epideictic, meant to celebrate knowledge rather than to validate claims. Additionally, Nobel lectures are written by the researchers years after the discovery for a fairly broad but scientifically literate audience, whereas accommodated accounts of science are usually written by professional journalists immediately after the announcement of the discovery and are intended primarily for laypeople. (An exception is scientific journalism aimed at professional audiences in publications such as *Science* and *Chemical & Engineering News*.) Because the intended audiences and the social contexts of Nobel lectures and journalistic accounts of science are different, they cannot be considered the same genre. This analysis of Nobel lectures uncovers characteristics that are uniquely their own—emphasis on the long-term significance of the work rather than defense of experimental results, along with discussion of the nature of science itself and the scientific method—establishing their place as a distinct genre of epideictic scientific rhetoric.

Chapter 2: A Brief Overview of Fullerene Science

Nobel Prizes are rarely awarded for extremely recent discoveries. Zuckerman (1977) found that for all science laureates, the average time between a discovery and its recognition by the Nobel committee is 13.5 years (p. 218). By the time a scientist receives the Nobel Prize, the work has usually aged significantly, so Nobel lectures often cover the entire history of a maturing discipline. In order to understand these Nobel lectures, therefore, it is essential to have some understanding of the field. This chapter gives a short overview of fullerene science before 1996, when Curl, Kroto, and Smalley made the trip to Stockholm, concentrating on topics covered in their Nobel lectures. This discussion is drawn largely from two books about the discovery of the fullerenes (Aldersey-Williams, 1995; Baggott, 1994) and from Kroto and Smalley's own accounts in their Nobel lectures and in two short reminiscences (Kroto, 1992; Smalley, 1991). The interested reader is referred to them for further details.

The first ten days of September 1985 were a remarkable time in the history of chemistry. In a flurry of activity, Robert Curl, Harold Kroto, Richard Smalley, and their coworkers (referred to hereafter as the Rice–Sussex team), working in Smalley's laboratory at Rice University in Houston, discovered a third unique form, or allotrope, of carbon. Prior to this, two allotropes of carbon had been known since ancient times: diamond and graphite. Pure diamond and pure graphite both consist only of carbon atoms (other than chemically and physically insignificant heteroatoms "capping" the edges), but the arrangement of the bonding is different (see Figure 1). In diamond, each carbon atom is bonded to four other carbon atoms arranged in a tetrahedron around it, forming a three-dimensional array. In graphite, each carbon atom is bonded to only three other carbon atoms, which are arranged around it in a flat triangle. This results in graphite resembling sheets of chicken wire; the carbon atoms and their bonds form an extended two-dimensional array of hexagons.

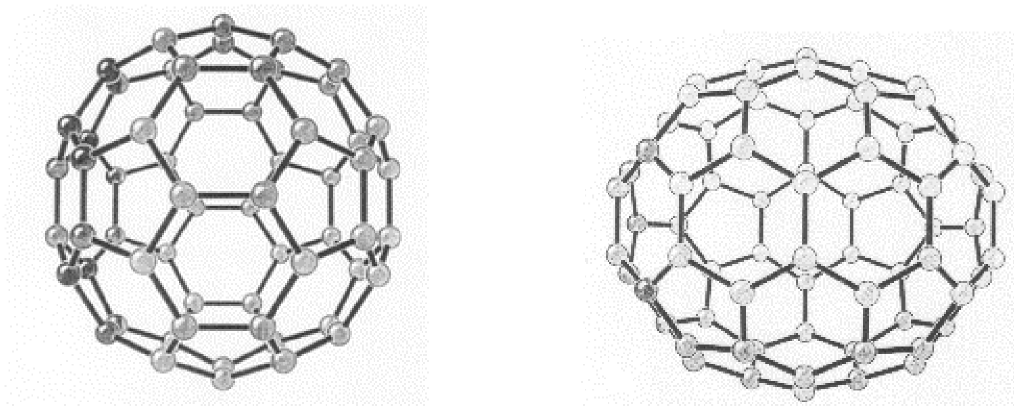


Diamond Graphite

Figure 1. The structures of diamond and graphite. The balls represent carbon atoms and the sticks represent bonds. In both of these materials, the structures extend indefinitely in all three dimensions. (Reprinted with permission from Morrison & Boyd, 1992, p. 446 [diamond] and p. 512 [graphite]. Copyright 1992 Prentice Hall.)

These different bonding geometries result in the vastly different properties of these two substances: Diamond is the hardest substance found in nature, whereas graphite is slippery and has found application as a lubricant because the sheets of carbon atoms that make up graphite are bound only very loosely to each other, so they slide easily over each other. In practice, most samples of elemental carbon are an amorphous material consisting primarily of graphitic carbon with a small amount of diamond-like carbon.

The discovery of the fullerenes was serendipitous. In fact, the experiments began as an exploration of a problem primarily of interest to astronomers rather than chemists. Kroto was interested in solving a long-standing mystery in astronomy: the source of features called the diffuse interstellar bands that appear in microwave emission spectra acquired from deep space using radio telescopes. He believed that the source of these features may be long-chain molecules such as cyanopolyynes, which consist of a rod-like chain of carbon capped at one end with hydrogen and at the other with nitrogen. Kroto came to Houston to work with Curl and Smalley to try to make these molecules in conditions simulating the environment in red giant stars, where the celestial material was believed to originate. He wanted then to measure spectra from them to see if they matched the diffuse interstellar bands. To perform these experiments, they placed a graphite disk in a laser-vaporization supersonic cluster beam apparatus developed by Smalley. Upon vaporizing graphite from the disk with high-power laser pulses, they found in their data, to their surprise, an indication of what appeared to be a cluster consisting of 60 carbon atoms. After furiously debating, building models, and consulting the literature, they theorized that the new 60-carbon structure had the form of a sphere comprising 20 hexagons and 12 pentagons, known to mathematicians as a truncated icosahedron and familiar to us as the form of a soccer ball (see Figure 2). The structure reminded Kroto of the geodesic dome at the 1967 World's Fair in Montreal, so the group decided to name the new C_{60} molecule buckminsterfullerene—"buckyball" in popular parlance—after the architect R. Buckminster Fuller, who popularized the geodesic dome. Also formed in the experiments was a 70-carbon cluster for which they proposed a more elongated structure, like a rugby ball, along with clusters of other even numbers of carbon atoms. This family of clusters is referred to collectively as the fullerenes.



C_{60} (Buckminsterfullerene) C_{70}

Figure 2. The structures of C_{60} (buckminsterfullerene) and C_{70}. As in Figure 1, the balls represent carbon atoms and the sticks represent bonds. Unlike diamond and graphite, which are extended solids, the fullerenes are discrete molecules. (Reprinted with permission from Diederich & Whetten, 1992, p. 120. Copyright 1992 American Chemical Society.)

The significance of the discovery lies in the importance and pervasiveness of carbon in the field of chemistry. The chemistry of carbon compounds is so extensive and so important that an entire branch of chemistry—organic chemistry—was created to encompass their study.[1] That another previously unobserved form of an element so ubiquitous would be discovered late in the twentieth century is truly remarkable. Moreover, even though other

[1] This classification system, though, is imperfect. The forms of elemental carbon discussed in this chapter as well as some types of carbon compounds, such as carbonates, often fit more comfortably into the realm of inorganic chemistry.

scientists had speculated that this third form of carbon might exist and had even tried without success to make it in the laboratory using tedious "wet chemistry" methods, the Rice–Sussex team discovered that carbon can form these clusters on its own under certain conditions.

The road to the Nobel Prize, however, wasn't smooth; universal acceptance of the spherical structure of fullerene didn't follow right away. Particularly, some in the scientific community felt that the Rice–Sussex team had been seduced by the beauty of their proposed spherical structure and were promoting it on that basis. The major obstacle was that at first nobody could find a way to make a large enough quantity of fullerene to be analyzed with other techniques in order to confirm the structure. Smalley's cluster beam apparatus produced only a vanishingly small quantity, which was then consumed in the mass spectrometer that was used as the detection device. The two most reliable techniques for determining the structure of a molecule are x-ray crystallography and nuclear magnetic resonance (NMR) spectroscopy. In x-ray crystallography, x-rays are passed through a crystal of the material to be analyzed and the diffraction pattern (the way the x-rays are scattered by the crystal) is analyzed mathematically to determine the positions of the atoms in the crystal. In NMR, the sample is held in a strong magnetic field while its interaction with radio waves is studied, which gives information on the immediate surroundings of particular atoms in the molecule and can be used to give detailed information on its structure. (The same physical principle is used in medicine in magnetic resonance imaging [MRI].) Although the amount of material that is needed for these techniques is small, it is still much more than the Rice–Sussex team was able to produce in the original experiments. Consequently, they had to use less direct methods to produce evidence for the spherical structure.

Almost immediately they thought to try to encapsulate another atom inside the fullerene cage, and they published the results of an experiment in which they created a 60-carbon cluster with a single lanthanum atom. If the cluster were not spherical or if the lanthanum atom were bound somewhere on the outside of the cage, it would be expected that clusters would also be produced that contain more than one lanthanum atom. In this case, however, only clusters with one lanthanum atom were detected, indicating that the fullerenes have just one unique binding site for lanthanum: inside the cage.

To further bolster their case, they tried an experiment to "shrink-wrap" fullerene cages around atoms of a variety of elements. The sizes of atoms of most elements are known with reasonable accuracy. If the fullerenes were in fact closed cages, the size of the smallest cage that could encapsulate each different element could be predicted rather easily. They created fullerenes with atoms of different elements encapsulated within the cage and subjected them to experiments in which the fullerenes were bombarded with photons to break carbon atoms off of the cluster and thereby gradually shrink the cluster. They found that they could break pairs of carbon atoms away from the clusters bit by bit until, finally, the clusters broke apart completely. For each different encapsulated element, they found that the cluster disintegrated at the point at which the carbon cage was calculated to be too small to stretch over the atom that was contained within. The accuracy of their predictions therefore provided strong evidence for the validity of their hypothesis that the fullerenes were closed cages of carbon atoms.

The controversy over the fullerene hypothesis did not cease, however, until 1990, when Wolfgang Krätschmer of the Max Planck Institute for Nuclear Physics in Heidelberg, Germany, and Donald Huffman of the University of Arizona in Tucson reported the isolation of milligram quantities of fullerene by vaporizing graphite in an electric arc. They also reported data from x-ray diffraction and another common analytical technique, infrared spectroscopy, that confirmed the spherical structure. Kroto, whose group had independently found its own macroscale synthesis but failed to beat Krätschmer and Huffman to a publication, followed quickly with NMR data also in support of the structure. Now that fullerenes could be made in quantities that could be manipulated easily, the field exploded. Other researchers added chemical substituents to the outside of the fullerene sphere and linked fullerenes together like a pearl necklace or in a pendant fashion like a charm bracelet. Other researchers created metal-doped fullerene materials that were found to be superconducting. Related carbon species called carbon nanotubes were discovered as well, and it was discovered that their electronic properties were very sensitively dependent upon their size and structure—a very unusual property in solid-state physics.

Fullerene science is still very much in its infancy, with no major commercial applications as of this writing. However, fullerenes and carbon nanotubes are showing great promise for possible use in such applications as advanced carbon-based materials for quantum computing, in electronic devices such as transistors and diodes, and in pharmaceutical applications such as targeting antibiotics to bacteria and attacking cancer cells. Carbon nanotubes can encapsulate metal atoms, like peas in a pod, to create molecular-scale wires, and they appear to exhibit extraordinary strength and flexibility, properties that could be used to create super-strong ropes and cables. Currently the major obstacles to these applications are their cost and the inability to produce specific nanotube structures with sufficient purity. Kroto and Smalley today are leaders in the science of fullerenes and carbon nanotubes, but Curl has moved on to other interests in developing laser-based techniques to follow chemical reactions on the molecular level.

Chapter 3: Introduction to the Texts Using Latour and Woolgar's Statement Types

One of the goals of this thesis is to follow the transformation of scientific facts over time and between genres. In order to do so, we need a classification system for these facts. Latour and Woolgar, in their field investigation of a neuroendocrinology laboratory (1979), describe the maturation of scientific facts over time as the evolution of scientific statements through a set of statement types, each of which represents a different level of implied acceptance by the research community. The movement of a fact from one statement type to another can occur only when *kairotic* conditions warrant. The creation of facts requires "the successful persuasion of readers, but the readers are only fully convinced when all sources of persuasion seem to have disappeared …. There is then an essential congruence between a 'fact' and the successful operation of various processes of literary inscription" (p. 76).

Latour and Woolgar classify statements in science into five general types, depending on the implied level of certainty (p. 77):

Type 1: Conjectures or speculations: "It *may also signify* that not everything seen, said and reasoned about opiates may be applicable for the endorphins" [emphasis added] (p. 79). These statements are commonly found at the ends of papers and in everyday discussions in the lab. They admit or indicate that evidence for the claim is weak or nonexistent.

Type 2: Claims that are not yet widely accepted and are stated with modalities: "There is a large body of evidence to support the concept of a control of the pituitary by the brain" (p. 78).

Type 3: Accepted knowledge that is stated with modalities: “The structure of GH.RH was *reported to be* X” (p. 78). These statements are more commonly found in review articles than in textbooks.

Type 4: Accepted knowledge that is stated without modalities: “The structure of GH.RH *is* X” (p. 78). These statements are more commonly found in textbooks than in everyday discussion in the lab.

Type 5: Taken-for-granted facts that are usually not made explicit except when speaking to newcomers or outsiders. An example of a *type 5* statement might be an explanation of what DNA is.

According to Latour and Woolgar, “[t]he aim of the game [is] to create as many statements as possible of *type 4* in the face of a variety of pressures to submerge assertions in modalities such that they [become] artifacts” (p. 81). They claim that modalities in scientific statements indicate less certainty about the validity of the claim. Therefore, successful scientific theories are those that eventually appear as statements of *type 4* well in the future and when stated by other scientists. Latour put it a different way when he described the process of fact-making as, in essence, increasing the cost to your opponents in terms of isolating themselves from the rest of the scientific community by contradicting or challenging your statements (Latour, 1987, pp. 56–59).

Fahnestock (1986) used this system to compare claims made in scientific research papers written by the researchers themselves and in popularized accounts of the same work written by journalists. She found that between original scientific research reports and popularized accounts of science, statements of *types 2* and *3* are often transformed to statements of *type 4* because this serves the purpose of amplifying the achievement (which is consistent with the view that popular accounts of science are epideictic). *Type 1* statements, though, are common in the popular scientific press for the same reason: amplification. Speculations about the consequences of the research increase its perceived significance.

This chapter contains a similar analysis of the fullerene Nobel lectures although the transformations may not be analogous in every way. Nobel lectures are sometimes similar to popularizations superficially but are produced under different circumstances and by different rhetors with usually different goals. Although both seek to celebrate science, popularizations seek to inform a lay audience, often by appealing to wonder or to applications stemming from scientific discoveries. (This is discussed further in Chapter 4.) In contrast, Nobel lectures are delivered to more knowledgeable audiences and are meant to inform but also to solidify shared values of the scientific community, as will be shown in this work. The purpose of this research is to show at the textual level how Nobel lectures are used to achieve these goals, focusing especially on the effect of *kairos,* "the principle of timing or opportunity in rhetoric" (Miller, 1992, p. 115) that was explained in Chapter 1. Between the original paper in *Nature* (Kroto, Heath, O'Brien, Curl, & Smalley, 1985) and the Nobel lectures were eleven years and a large amount of research from Curl, Kroto, and Smalley and their coworkers and from others who followed them into fullerene science. With changing times came changing exigences to address. This chapter particularly examines the evolution of the claims regarding the fullerenes' structure. Not surprisingly, as will be demonstrated, the papers and the Nobel lectures contain a complex intermingling of Latour and Woolgar's statement types, reflecting the many different kinds of claims they needed to make.

Fahnestock contends that Latour and Woolgar's system, if used carelessly, can introduce specious rigor into analyses like this. For example, Watson and Crick's coy teasing

("*We wish to suggest* a structure for the salt of deoxyribose nucleic acid (D.N.A.)" and "*It has not escaped our notice* that the specific pairing we have postulated immediately *suggests* a possible copying mechanism for the genetic material" [emphasis added] [Watson & Crick, 1953]) could be mistaken for actual hedging. I agree with this, but the system does provide a useful framework as long as statements are considered carefully in context, as Latour and Woolgar themselves point out (p. 80). This is discussed further later in this chapter.

The Evolution of the Evidence

The prenatal period of the fullerene hypothesis began in 1966 in the British magazine *New Scientist.* Writing under the pseudonym Daedalus, the British chemist David Jones (who was then a regular contributor to *New Scientist* and later to *Nature*) speculated that graphite sheets could be curled up and closed to form large, hollow, spherical molecules. He imagined that these molecules would form a "vague fifth state of matter" because they "could hardly evaporate, but would interact so weakly at their few points of contact as not to be solid or even liquid" (Jones, 1966, p. 245). Attempting to reconcile the low densities of gases and the much greater densities of liquids and solids, he wrote in the third person that

> this week Daedalus *has been contemplating* ways of bridging this gap and has conceived the hollow molecule, a closed spherical shell of a sheet-polymer like graphite, whose molecules are flat sheets of benzene-hexagons. He *proposes* to modify the high-temperature graphite process by introducing suitable impurities into the sheets to warp them (rather like 'doping' semiconductor crystals to cause discontinuities), reasoning that the curvature thus produced will be transmitted

throughout the sheet to its growing edges so that it will ultimately close on itself. [emphases added] (p. 245)

Primordial fullerene science thus formed at Latour and Woolgar's furthest extreme: a purely speculative *type 1* statement. In the meantime, Eiji Osawa, Orville Chapman, and others would tinker with the idea of a spherical C_{60} molecule (Aldersey-Williams, 1995; Baggott, 1994), but it would be 19 years before Curl, Kroto, and Smalley, ignorant of previous speculations in this area, would push fullerene science to the next level: detecting material in the laboratory.

The Original Fullerene Report

If Jones's article marked the conception of fullerene science, then a 1985 letter to *Nature* (Kroto et al., 1985) from Curl, Kroto, and Smalley and their coworkers was its birth. Over the years, others besides Jones had speculated about spheroid carbon clusters, but never was it the focus of intense research by multiple groups, and the Rice–Sussex team were the first researchers to claim to have observed the clusters experimentally (Aldersey-Williams, 1995; Baggott, 1994).

The Rice–Sussex team's original paper, written hurriedly during that first week of September in 1985 (Kroto et al., 1985), opens with a barrage of *type 4* statements, that is, accepted knowledge that is stated without modalities. The abstract states, "During experiments aimed at understanding the mechanisms by which long-chain carbon molecules are formed in interstellar space and circumstellar shells, graphite has been vaporized by laser irradiation, producing a remarkably stable cluster consisting of 60 carbon atoms." They were satisfied that the cluster was real, not an artifact of the ionization process, so nothing here appeared likely to be disputed; the proposal of the spheroid structure comes later. Their first rhetorical move was to establish what appeared to be the least controversial part of their discovery.[1] Having accomplished this, they could then move into more dangerous territory. A more modulated summary of their conclusions that the molecule takes the form of a truncated icosahedron follows: “Concerning the question of what kind of 60-carbon atom structure *might* give rise to a superstable species, *we suggest* a truncated icosahedron, a polygon with 60 vertices and 32 faces, 12 of which are pentagonal and 20 hexagonal. This object is commonly encountered as the football shown in Fig. 1” [emphasis added] (p.162).

They then launch directly into a description of the experimental methods, explaining the operation of the cluster beam apparatus and giving some details on the laser used to vaporize the samples (“the second harmonic of Q-switched Nd:YAG producing pulse energies of ~30 mJ”). It is fairly unusual to put experimental methods in a position of such prominence (Berkenkotter & Huckin, 1995), but in this case putting this information here was highly appropriate and necessary: the cluster beam apparatus was hardly standard laboratory equipment, and their conclusions rested heavily on their audience’s acceptance of the reliability of the data that come from it.

The third paragraph of the paper briefly describes previous work, followed by the fourth paragraph describing the mass spectra and elaborating on the experimental procedure. These are unrelentingly *type 4*. The results, as they say, speak for themselves. Only when they begin to discuss the interpretation of the data do modalities creep in. The opening sentence of the fifth paragraph states, “Our rationalization of these results is that in the laser vaporization, fragments are torn from the surface as pieces of the planar graphite fused six-

[1] In fact, even this would come under heavy scrutiny (Aldersey-Williams, 1995, pp. 98–108; Baggott, 1994, pp. 100–104). See also Chapter 4.

membered ring structure" (p. 162). So begins a series of *type 2* statements about the soccer-ball structure of the molecule, a requirement because a claim to have discovered a new allotrope of such a common and important element was quite radical. As Fahnestock (1986) points out, new claims have to be hedged, so Kroto et al. tread lightly: "[A] search was made for some other plausible structure which would satisfy all sp^2 valences. Only a spheroidal structure *appears likely* to satisfy this criterion" [emphasis added] (p. 162). Having stated their case for the spherical structure of the fullerene molecules, they can now discuss the significance of this proposed structure. The seventh paragraph begins, "*Assuming that our somewhat speculative structure is correct,* there are a number of important ramifications arising from the existence of such a species" [emphasis added] (p.162), particularly in astronomy, their original interest. The speculative nature of these claims is indicated by their structure as *type 1* statements. They claim that fullerenes "may be widely distributed in the Universe" (p.162), composing part of carbon-rich shells of gas surrounding stars or existing in interstellar dust, possibly acting as catalysts for chemical reactions in deep space. "Even more speculatively, C_{60} or a derivative might be the carrier of the diffuse interstellar lines" (p. 163).

In the eighth paragraph, they turn to terrestrial ramifications and are able to speculate—again, in *type 1* statements—about possible fullerene derivatives and their potential applications, such as $C_{60}F_{60}$ as a potential super-lubricant, using the unique topology of the molecule to develop "new branches of organic and inorganic chemistry" and investigating prebiotic chemistry. In keeping with our analogy of the *Nature* letter as the birth of a field, the letter ends with a cheeky discussion on naming the new molecule in which the authors feign disturbance about the name buckminsterfullerene and invite the scientific community to settle on a name by consensus. Clearly they expect big things from this discovery.

The Evolution of the Evidence, Cont.

Subsequent papers from the Rice–Sussex team follow the general pattern laid out in the 1985 *Nature* letter. It would be outside the scope of this thesis to comment on each paper in the detail given to the *Nature* letter, but a summary can be given: Experimental procedures and results are for the most part unmodulated, while the discussion and conclusion sections are hedged. Statements about the proposed spheroid structure are supported by arguments about the special stability of C_{60} and various pieces of experimental evidence as they become available. Through the first few years, they prudently keep the statements about the structure of fullerenes very much in the lower region of Latour and Woolgar's system, which is demonstrated later. While they themselves were convinced from the beginning that the spheroid hypothesis was correct, as scientists they had to maintain an ethos of disinterestedness and organized skepticism (Merton, 1973) in order to be considered to be upholding the values of the scientific community, so they restrain themselves.

The Rice–Sussex team's statements about the spheroidal structure of the fullerenes evolved gradually. In 1985, they were already convinced of the reality of the spheroidal model, but they used prudently restrained verbiage, phrasing the statements as *type 2*. In a paper that was submitted to the *Journal of the American Chemical Society* (Heath et al., 1985) very shortly after the first *Nature* letter, they discuss the early work of Smalley's student Jim Heath in which he incorporated lanthanum within the carbon shells of fullerenes. Although the title, "Lanthanum Complexes of Spheroidal Carbon Shells," is unflinchingly direct, betraying their confidence in their interpretation, their verbiage in the paper is less so. Referring to the *Nature* letter about buckminsterfullerene, they say that previously they "suggested that the stability of this new species arises from its unique ability to close into a highly aromatic, spheroidal shell, a truncated icosahedron where all carbons occupy equivalent sites at the juncture of (two) six-membered and (one) five-membered rings" (p. 7779). Heath placed in the cluster beam apparatus a graphite disk impregnated with lanthanum. In addition to the familiar fullerene peaks, the resulting mass spectrum showed peaks at masses that were the sum of those of a fullerene and a single lanthanum atom. In no case could a peak be assigned to a cluster bound with more than one lanthanum, implying that a single lanthanum atom was bound within the fullerene cage. The paper puts it this way: "In all cases the complexes *are thought to* have the single La [lanthanum] atom either wholly or partially surrounded by an aromatic shell of carbons arranged in networks of five- and six-membered rings" [emphasis added] (p. 7779). Discussing the experimental results, they point out that "[e]xperiments such as those in Figure 1 reveal that C_nLa complexes are also highly stable—particularly $C_{60}La$—just as would be expected if these C_n species surround the metal atom much as an egg (shell) surrounds its yoke" (p. 7780). Nowhere except in the title—where, again, they reveal their own strong feelings on the matter—do they state as a given that the fullerenes are closed spheroids, a nod to the still-controversial nature of that interpretation.

Early the next year, they published a paper (Zhang et al., 1986) reporting the resistance to chemical attack of large carbon clusters with even numbers of carbon atoms, while odd-numbered clusters and those below 32 carbon atoms were consumed, further bolstering their claim. But they were still building their case for the spheroidal structure, as evidenced by the remarkable chemical stability of the molecule:

> The C_{60} cluster is shown in these experiments to be quite inert to chemical attack by small molecules such as NO, SO_2, and O_2, which are known to be extremely active free radical scavengers. This is fully in accord with expectations for a closed, edgeless carbon shell with a highly aromatic electronic structure. The postulated soccerball structure of this C_{60} molecule, Buckminsterfullerene, *is therefore supported by* these observations of its low reactivity. [emphasis added] (p. 527)

This was clearly still a *type 2* statement, as it was the next year when Kroto (1987) published alone a paper on rules that he devised to find which spheroidal carbon clusters should have enhanced stability: "This is not an obvious magic number sequence and it would be very surprising if an alternative model that does not involve closed cages were able to arrive at the same set" (p. 531). In 1988, two papers they published describing photofragmentation experiments on empty fullerenes and endohedral complexes with metal ions like lanthanum contained these statements: "Although the spheroidal shell model for the large clusters of carbon is as yet unproven, it remains the only model yet advanced which is fully consistent with all known results" (O'Brien, Heath, Curl, & Smalley, 1988, p. 229) and "The observation that C_2 rather than M^+ loss from $C_{60}M^+$ suggests that the metal ion is sterically bound, since a bond between K^+ or Cs^+ and carbon should be weaker than a carbon–carbon bond. These results are then strong evidence that $C_{60}M^+$ clusters are composed of closed carbon cages with the metal ion trapped inside" (Weiss, Elkind, O'Brien, Curl, & Smalley, 1988, p. 4465).

Here a question arises: When is the transition made from *type 2* to *type 3*? The Rice–Sussex team was clearly convinced before 1988 of the validity of the spheroidal model for fullerenes, and the statement from the Weiss paper, taken alone, could easily be classified as *type 3*. The answer, however, requires putting the statement in context. Earlier in the paper, in the first and second paragraphs, we have this: "[The exceptionally high photophysical stability of $C_{60}La^+$] led to the suggestion that the La atom had been trapped inside the closed shell of the (putatively) icosahedral carbon cage, [reference to Kroto et al., 1985] C_{60}. *To test this rather controversial* [three references] *hypothesis*, we have performed extensive photophysical and chemical measurements in the magnetic trap of an FT-ICR mass spectrometer" [emphasis added] (p. 4464). Herein lies the major weakness of Latour and Woolgar's system. Taken out of context, the statement quoted above from p. 4465 gives no indication of the controversy still surrounding the cage structure. It may appear to be a *type 3* statement, but it is actually *type 2*: a statement about something that is not yet widely accepted, illustrating the importance of context or, in other words, that the classification of a statement depends on the other statements around it.

Finally, in 1990, Krätschmer and Huffman (Krätschmer, Lamb, Fostiropoulos, & Huffman, 1990) reported their isolation and structural characterization of C_{60}. The first sentence of the abstract was momentous: "A new form of pure, solid carbon has been synthesized consisting of a somewhat disordered hexagonal close packing of soccer-ball-shaped C_{60} molecules" (p. 354). This is almost certainly the first *type 4* sentence in the literature concerning the structure of C_{60}, and it marks the end of the initial phase of fullerene science, the work for which Smalley, Curl, and Kroto were awarded the Nobel Prize six years later. Interestingly, none of the three discuss in their Nobel lectures much of their own work past the Krätschmer–Huffman paper. Probably this is because the Nobel Prizes were awarded "for their discovery of fullerenes" (the official citation), not for their subsequent work. As Kroto put it in his Nobel lecture, "So in September 1990[,] some 20 years after the molecule [C_{60}] was conceived by Osawa and five years after we had discovered that it could self-assemble, Krätschmer *et al.* had extracted it and Fullerene Science was well and truly on its way" (p. 71).

The Nobel Lectures

By the time Curl, Kroto, and Smalley were awarded the Nobel Prize in 1996, nanotechnology in general and fullerene science in particular were thriving areas of research in the physics and chemistry communities. As discussed in Chapter 2, fullerenes and carbon nanotubes were being investigated for their unique electronic and mechanical properties. The setting for the Nobel lectures [Curl, 2003; Kroto, 2003; Smalley, 2003], therefore, is significantly different from that for the research reports discussed above.

The Nobel lectures: Smalley. Smalley's lecture places us in a different world than the original research reports did. As described above, fullerene science had advanced to the point where much of the speculation in that original letter was turning to accepted fact. Krätschmer and Huffman's macroscale synthesis of fullerenes in 1990 had led to a remarkable period of discovery and had driven the final nail into the coffin for the arguments against the Rice–Sussex team's characterization of fullerene as a closed spheroid, significantly altering the kairotic situation relative to the 1980s, when the fullerene structure was still in doubt—at least, outside of Houston and Sussex.

In response, the *type 2* statements in the *Nature* letter about the structure of the fullerene molecule have been replaced with this: “[T]he discovery that garnered the Nobel Prize was the realization that carbon makes the truncated icosahedral molecule, and larger geodesic cages, all by itself. Carbon has wired within it, as part of its birthright ever since the beginning of the universe, the genius for spontaneously assembling into fullerenes” (Smalley, 2003, p. 90). The thrust of the statement isn’t about the discovery of fullerene, nor of its structure, but of its ability to self-assemble. The structure is a given, hardly worth mentioning; for Smalley, it has now made the leap all the way to *type 5*, validated by the ultimate external confirmation, the Nobel Prize.

Smalley moves into describing some developments coming from other labs, particularly in the area of nanotubes. Few experimental details are given and no modalities are needed. Now isn’t the time for Smalley to get into the nuts and bolts of experimental “wet chemistry.” For Smalley, these species don’t exist as Latour and Woolgar’s “inscriptions”—squiggles in a mass spectrum or an NMR spectrum—they’re real molecules, existing independently of any experiments in the lab. These are bold, confident *type 4* statements. Referring to a figure depicting a carbon nanotube, he says,

> We are beginning to understand that what causes this tube to be the most favorite of all tubes is wired within the instruction set what it means to be a carbon atom. The propensity for bonding that causes C_{60} to be the end point of 30–40% of all the reactive kinetics, leads as well to this (10,10) tube. This detour on the road that otherwise leads to spheroidal fullerenes is taken if you somehow (with cobalt or nickel atoms) frustrate the ability of the open edge to curve in and close. The metal atoms prevent by local annealing the addition of the seventh, eighth, ninth pentagons,

> and insure by judicious choice of temperature and reaction rate that the growing tublet can anneal to its most energetically favored form. (p. 92)

Again, as in the original research reports, once the molecule has been discussed, the subject can move to possible future developments. We see Smalley's first foray into *type 1* speculation with a discussion of a metal-derivatized tube that he describes as the model for future technology in which objects such as enzymes, membranes, or surfaces can be attached to opposite ends of carbon nanotubes, which would enable them to "communicate" across the metallically conducting tube:

> Imagine what the impact could be. Essentially, every technology you have ever heard of where electrons move from here to there has the potential to be revolutionized by the availability of molecular wires made of carbon. Organic chemists will start building devices. Molecular electronics could become reality. (p. 94)

This is still a pretty far-fetched statement in 1996, but Smalley finds it appropriate for his Nobel lecture, phrased appropriately as a *type 1* statement.

Importantly, besides describing the cluster beam apparatus in some detail, Smalley spends very little time, compared to Kroto and Curl, discussing experiments. His lecture exemplifies Latour and Woolgar's evolution of scientific facts from modulation to unspoken acceptance. For Smalley, the experiments are important (shown by his attention to the cluster beam apparatus), but they are a means to an end. Descriptions of experiments and experimental evidence are like the hedging and modulation found in statements of *types 2* and *3*. The goal is to get to a point where they can be omitted in favor of discussing facts and features of nature.

This tendency to deemphasize experimental methods is not limited to Nobel lectures. Berkenkotter and Huckin (1995) report that between 1944 and 1989, the percentage of journals in their study that downplayed the methods section, by printing it in miniprint, for example, or by eliminating it altogether, increased from 0% to 45% (pp. 37–38). This is, they claim, part of a general trend toward a greater emphasis on “news value” in scientific papers, wherein papers, and especially short papers with rapid turnaround, commonly called communications or letters, are gradually being given more informative titles, abstracts that focus on results, powerful visual aids, and prominent statements of findings and claims, deemphasizing raw data and experimental methods. Partly this is because as science advances, it becomes more and more difficult for any one scientist to become deeply knowledgeable outside of her particular niche, so many scientists increasingly rely on journal editors to make decisions about what constitutes sound science rather than examine experimental minutiae themselves.

Could it be said that the Nobel Prize confers sufficient validation to render experimental details unnecessary? Not necessarily. The Nobel Prize is not without controversy, exemplified—to cite the most recent example as of this writing—by the dispute over the awarding of the 2003 medicine prize to Paul Lauterbur and Peter Mansfield for their work in magnetic resonance imaging (MRI), omitting Raymond Damadian, whom many credit with inventing MRI in the first place (Henry, 2003). Laureates must gauge for themselves the prevailing opinion surrounding their work and particularly the prevailing opinion surrounding their receiving the Nobel Prize for it, which certainly affects the exigences that they will need to address. Smalley seems not to have seen a need to belabor the point.

The Nobel lectures: Kroto. Kroto presents some similarities with and some interesting contrasts to Smalley. True to scientific custom, both begin with a thorough literature review, placing their work in a broader context. But where Smalley's lecture places the audience solidly in 1996 and speaks primarily of scientific concepts in a vacuum, essentially—in the absence of many experimental details—and uses primarily strong *type 4* statements, Kroto, by contrast, takes the reader back to 1985 and before. He presents the fullerene story as an odyssey through many detours and obstacles before arriving serendipitously at the discovery of the fullerenes. After the brief literature review on fullerenes, he writes a section that he titles "Prologue: Symmetry, The Key to the Theory of Everything," followed by the story of his scientific journey through investigations of double-bonded carbon–sulfur and carbon–phosphorus species and long-chain carbon compounds found in deep space, described in two sections that he entitles "Phase I: Carbon, Still Crazy After All These Years" (p. 49) and "Phase II: A Tale of Cold Black Giant Clouds and Warm Red Giant Stars" (Kroto, 2003, p. 52), an extended introduction establishing the context for the fullerene discovery.

Finally, about a third of the way into the lecture, Kroto comes to his collaboration with Curl and Smalley in "Phase III: Ten Days in September 1985" (p. 54), in which he describes how his interest in the diffuse interstellar bands brought him to Houston to work with Smalley and Curl on the cluster beam apparatus. Using the story of those first ten days in September 1985, Kroto outlines how they used paper models to arrive at the conclusion that the C_{60} molecule was spheroid, ushering in "Phase IV: Little Fullerenes, Giant Fullerenes, Red Solutions and One-line Solutions, or—How to Play Football Without Knowing the Rules." This transition marks the historical transition from speculative *type 1* statements about the soccer-ball structure to *type 2*, in which they're confident in the structure, but they have yet to convince the scientific community at large. This section describes their working out of other fullerene structures, such as the rugby-ball structure of C_{70} and their photofragmentation experiments, their synthesis of fullerene derivatives, and their attempt, thwarted by Krätschmer and Huffman, to be the first to make macroscopic quantities of fullerenes.

Kroto concludes his lecture with “Phase V: C_{60} Buckminsterfullerene, Not Just a Pretty Molecule” (p. 71), in which he briefly describes some subsequent work by other groups in the field, and an epilogue, “The Cosmic and Microcosmic Charisma of the Soccerball” (p. 75), reflecting on the elegance, symmetry, and “charisma” of the soccer-ball-shaped fullerenes, a summation of a theme throughout his talk, discussed further in Chapter 4. These two sections consume just a few pages in the printed text, far fewer than the preceding sections. Kroto is concerned primarily with how fullerene science got to where it is, not as much with where it's going.

Kroto's lecture is replete with statements of *type 4*. Indeed, it's a little difficult to find statements of any other type. Like Smalley, Kroto was confident in his conclusions from the beginning; the goal was to convince everyone else of them: “I remember thinking that the molecule was so beautiful that it just had to be right—and anyway even if it were not, everybody would surely love it, which they did—eventually!” (p. 59). Kroto certainly fulfilled Latour and Woolgar's dictum that scientific statements strive to lose their modalities. Kroto puts more emphasis on experiments than Smalley does, but Kroto's lecture isn't about false starts and dead ends. It's about his ascent to the scientific ultra-elite, a journey he takes us on with wit and humor.

Highlighting the importance of careful consideration of context when making this kind of analysis, statements that take the appearance of *type 2* can be found embedded within sentences recalling thought processes in which the Rice–Sussex team engaged in the past: “I started to play around with a model of C_{60} by adding atoms and before I had made much headway it suddenly struck me that *perhaps* closure could not occur again until C_{70}” [emphasis added] (p. 61). “It also struck me that this species *should be* a sort of superatom cluster analogue of the carbon atom with effective tetravalency suggesting that an elegant tetrahedral $C_{28}H_4$ derivative, [reference to a figure], *might actually be* a more stable molecule [reference]” [emphasis added] (pp. 62–63). “The idea was that as the He pressure increased[,] C_{60} formation *might be* initiated and this *would be* accompanied by the creation of round carbon particles” [emphasis added] (p. 65).

Leaving behind Kroto’s whimsy, we turn, for a striking contrast, to the careful skepticism of Robert Curl.

The Nobel lectures: Curl. With respect strictly to Latour and Woolgar’s statement types, Curl generally follows the pattern exemplified by Smalley and Kroto. The Nobel lecture is not a place for hedging about the results of their experiments. Curl even declines to explain many of the early experiments, instead referring the reader to existing books and monographs describing the details of the initial discovery in 1985.

However, although Curl is as solid as Kroto and Smalley in his confidence in his results, his lecture is unique. Filling his role as the group’s cautious skeptic (Aldersey-Williams, 1995, describes the “Bob Curl Test”: the gauntlet that data from their group had to pass through before an interpretation would be accepted [p. 69]), he works his lecture as a series of conjectures, usually with a description of experiments supporting those conjectures, for example, “Conjecture – The Fullerene Hypothesis,” “Experiment – Reactivity and Photofragmentation,” “Conjecture – The Existence of Endohedral Complexes,” “Experiment – Endohedral Metallofullerenes and ‘Shrink Wrapping,’” “Conjecture – C_{70} has D_{5h} Symmetry,” and so on, establishing the structure that distinguished him from Kroto and Smalley by placing more emphasis on processes and methods and their line of thought through their research in the late 1980s.

The heart of Curl’s lecture is found in one line relatively near the beginning of his talk: “Our claim has always been that the situation is much closer to proof than to conjecture” (p. 16). Without stating it directly, Curl has captured the essence of Latour and Woolgar’s system. He claims that their results deserve to be spoken of in *type 4* statements rather than *type 1*. Indeed, his lecture is rife with *type 4* statements, always backed up with experimental evidence and rebuttals of possible counterclaims, for example,

> Thus, we believed that the very prominent C_{60} peak … could only be explained by a single isomer of C_{60} remarkably impervious to chemical attack. A readily imaginable alternative explanation would be in terms of a C_{60} isomer that is much easier to photoionize than its neighbors by the 6.4 eV ArF ionization laser employed. However, this explanation ignores the clear increase of the prominence of the C_{60} signal when more time is allowed for chemical reaction. Thus an explanation for the prominence of C_{60} based on its easier photoionization does not take into account the obvious reduced chemical reactivity of C_{60} compared with its neighbors. (p. 17)

Curl backs up his discussion of endohedral fullerenes with strong experimental evidence: “Thus under laser fluences capable of destroying the less stable carbon clusters, one and only one lanthanum stuck. This is a strong indication that the lanthanum atom is inside the cage" (p. 24). For endohedral complexes of potassium cation, K^+, "[t]he even carbon loss breaks off for $C_{60}K^+$ at $C_{44}K^+$ and for $C_{60}Cs^+$ at $C_{48}Cs^+$[,] which agrees well with predictions from the van der Waals radii. We can conceive of no explanation for these observations other than that we were observing fragmentation of endohedral fullerene complexes" (p. 25). Curl believes strongly in the scientific method and for him, in contrast to Smalley, the story isn't complete without describing in some detail exactly what the evidence was for their claims.

Like Kroto, where Curl appears to possibly be using a lower-level statement, he is often actually setting up for an explanation: "If the activation barriers to fragmentation follow the energetics, one *would expect that* the special stability of C_{60} would be apparent in the fragmentation pattern" [emphasis added] (p. 21), which, according to Curl, turned out to be the case.

Discussion

Like the popular accounts of science examined by Fahnestock (1986), the Nobel lectures show a transformation of hedged statements of *type 2* to bolder statements of *types 3* and *4*, with some *type 1* speculations sprinkled in, representing a polarization of statements. However, one important difference should be noted: Popular accounts of science are published relatively concomitantly with the original reports that they discuss, whereas Nobel lectures come long after the experimental work. The three Nobel lectures examined in this study don't lack statements of *type 2*; they simply embed them in higher-level statements, looking back at a time when their conclusions were less certain rather than at the current state of the art. With regard to the types of statements made about the interpretations of the data leading to the fullerene structure, whereas these are lower, hedged statements in the original articles, they are transformed boldly to *type 4* in the Nobel lectures. These contrasts, however, are superficial; with regard to the Latour–Woolgar classification system, the research papers and the lectures are more alike than they seem. Adjusting for the change in status of their results as time went on, in both genres claims that are uncertain *at the time of the publication* are phrased as *type 2* statements, whereas those that are widely accepted are found in higher level statements. How a claim is phrased depends directly on the timing—on *kairos*. In 1985, the idea that a third form of carbon could have evaded discovery for millennia was nearly unthinkable except for the few scientists prior to then who speculated about it. Until the publication of the Krätschmer–Huffman paper in 1990, the list of claims that were uncertain certainly had to include this new spherical form of carbon. By 1996, the controversy had effectively disappeared, and Curl, Kroto, and Smalley could be bolder in their statements about the fullerene structure, which by then had become a taken-for-granted fact and was treated as the *type 5* statement that it deserved to be. The subject of their speculations turned from the reality of the spheroid structure to its implications.

Despite the specious rigor that Latour and Woolgar's system imposes due to the lack of regard for context, it is still useful as a framework for discussion as long as this limitation is heeded. We saw in this chapter that the difference between statements of *type 2* and *type 3* in particular can be unclear without knowledge of the extent of any controversy surrounding the topic at hand. However, this does not negate the usefulness of this system, and it does provide a convenient way of tracing the evolution of statements over time. The next chapter examines the Nobel lectures in light of Prelli's (1989) discussions of scientific *topoi* and *stasis* theory in order to shed more light on the way the Rice–Sussex team's rhetoric evolved.

Chapter 4: A Prellian Analysis

Like other rhetors, scientists must make their arguments reasonable to their audience. As Prelli (1989) put it, “to be judged reasonable and persuasive in any specific situation, scientific discourse must be perceived as identifying, modifying, or solving problems that bear on a specific scientific community’s maintenance and expansion of their comprehension of natural order” (p. 122). Different rhetors addressing different communities at different points in time invent different exigences to address. This chapter uses Prelli’s adaptation of *stasis* theory for scientific discourse to analyze the issues that Curl, Kroto, and Smalley chose to address in their Nobel lectures. Along with *stasis* theory, I use Prelli’s set of common *topoi* in science to explore how these scientists invented lines of argument to support those issues or exigences. Throughout, the Nobel lectures are compared to the popularized, or “accommodated,” articles about science examined by Fahnestock in her classic study (1986).

Scientific Stases

Prior to constructing an argument, a rhetor must determine the issues at hand. Classical rhetoricians described a hierarchy of three to four “points of stoppage,” or *stases,* at which questions arise. In any argument, questions of fact must be settled first: “What is it?” or “What happened?” Once the bare facts are established, the argument can turn to the definition or the cause of the act: “How should the act be named?” or “What caused it?” Many arguments stop here, but they also can continue into questions of judgment: “Is it good or bad?” and questions of procedure: “What should be done about it?”

Consider an example of an argument that moves through all four *stases*: A defendant is on trial for causing an automobile accident in which a pedestrian was hit and injured. If it is established that the defendant was the driver of the car that hit the pedestrian (a question of fact: the first *stasis*), the defense can move to arguing that the incident was an accident and not the result of negligence on the part of the driver (the definition of the act: the second *stasis*). If the jury is convinced that the driver was negligent, they may judge the negligent act a felony (a question of judgment: the third *stasis*). The jury can issue a conviction that requires a prison term for the driver (a question of procedure: the fourth *stasis*).

Again, not all pieces of discourse address all four of the *stases*; the particular *stases* that are addressed depend on the event or exigence and the needs of the audience. Prelli (1989) put it this way for discussions in science:

> Scientists—not nature—choose which problems to work on and how to formulate those problems …. Scientists must choose the issues they will address, and they need to show their peers that issues addressed are logically significant given the present state of scientific knowledge. Put differently, they must establish for themselves and others the relevance of their problems and the proposed solutions whenever that relevance is not self-evident. (p. 144)

Prelli refined the classical *stases* to clarify them for use in analyzing scientific discourse. He described four superior *stases* for scientific arguments: evidential, interpretive, evaluative, and methodological. Discourse takes place in the evidential *stasis* when the issue at hand concerns matters of "what exists or does not exist in the natural world" (p. 147). This can be taken as similar to the first *stasis* described above: "What is it or what happened?" When this is settled and the issue moves to the interpretation of theories, the discussion takes place in the interpretive *stasis*. In this case, the data may be accepted but there is disagreement about what theories can be used to account for the phenomenon. This is related to the second *stasis* from classical rhetoric: "What caused it or what is the definition of the act?" Once the findings and their interpretation are established, discussion can focus on their significance, which takes place in the evaluative *stasis* and is equivalent to the third classical *stasis*: "Is it good or bad?" Finally, in the methodological *stasis*, discussion can take place on future action, which obviously is equivalent to the fourth classical *stasis*: "What should be done about it?" Again, when agreement is reached in one *stasis,* the argument can proceed in the next. The following statement, devised for illustration purposes, signifies a shift from the first (evidential) *stasis* to the second (interpretive) *stasis*: "Although this group of spiders has mandibles of a slightly different structure, it likely does not constitute a new species, as their ranges overlap and my evidence has shown that they are able to reproduce together. Therefore, I have classified these as two subspecies."

Prelli took *stasis* theory one step further. His superior *stases* can each be refined by four subordinate *stases*—conjectural, definitional, qualitative, and translative—depending on how the question is approached. For example, when the evidential superior *stasis* is intersected by each of Prelli's subordinate *stases*, the following questions arise: "Is there scientific evidence for claim *x*?" (conjectural), "What does the evidence mean?" (definitional), "Which empirical judgments are warranted by available evidence?" (qualitative), and "Which evidence more reliably grounds claims about what does and does not exist?" (translative) (p. 146).

Fahnestock (1986) and Fahnestock and Secor (1988) use *stasis* theory to analyze scientific journal articles along with, respectively, popularized accounts of science and pieces of literary criticism. Using the four classical *stases* described above, they find that journal articles normally operate in the first two *stases* ("What is it?" and "What caused it?") and that popularizations operate in the latter two *stases* ("Is it good or bad?" and "What should be done about it?") and should be considered epideictic discourse. As we have seen, these *stases* can be translated into Prelli's scientific *stases*. Fahnestock and Secor claim that the *stases* can be used as probes for the rhetor's sense of audience (p. 430). One might expect that in most cases, Nobel lectures would also operate in the latter *stases* because except on those occasions when controversy arises, as it inevitably does from time to time, the question of what was done and if it was sound and significant would not be expected to be in question by the time a researcher receives the level of recognition that results in a Nobel Prize. In the case of the 1996 chemistry prize, this seems to have been the case, as demonstrated below.

Scientific Topoi

Once the rhetor has decided what the issues are, she constructs an argument to address these exigences, usually by appealing to recurrent themes or topics, known as *topoi*. Prelli (1989) outlines three classes of field-invariant general *topoi* in science: problem-solution *topoi* (p. 186), evaluative *topoi* (p. 199), and exemplary *topoi* (p. 205). If Nobel lectures address different exigences in different *stases*, then it would be expected that they also would appeal to different *topoi*.

Problem-solution *topoi* are "[l]ines of thought that relate to solving problems … [constituting] ways of talking that treat claims positively or negatively on the basis of whether the claims help to solve scientific problems in scientifically reasonable ways" (p. 186). They show that experimental results or field observations are valid and useful for solving scientific problems. Just a few of the more important examples of problem-solution *topoi* are experimental competence, observational competence, experimental replication, corroboration, explanatory power, and predictive power.

Evaluative *topoi,* according to Prelli, are the *topoi* that "suggest values that can be (and are) applied in choosing among theories or data or methods" (p. 199). Prelli assigns to this category Kuhn's (1996) values of scientific communities: accuracy, internal consistency, external consistency, scope, simplicity, elegance, and fruitfulness.

Exemplary *topoi* include examples, analogies, and metaphors. They "suggest that some ordering rule transcends the particular example, illustration, analogy, or metaphor used." Further, they "conceptualize phenomena in ways that guide investigations," they "help scientists probe and conceptualize relationships among phenomena," and they "prescribe metaphysical commitments about what phenomena really are" (Prelli, 1989, p. 206). Exemplars are frequently used in science education to link concepts to specific historic experiments or experiments that a student might perform in the teaching laboratory.

Fahnestock (1986) argues that whereas primary research reports are aimed at a scientific audience and are meant to validate experimental data, popularized accounts of science written by journalists for the general public are meant to celebrate science and demonstrate the significance of scientific discoveries. They do this by appealing to "wonder" or to "application," corresponding to the deontological and teleological arguments in ethical rhetoric (p. 279). In a deontological argument, the rhetor attempts to link the subject to something having value for the audience, either positive or negative. In a teleological argument, the rhetor shows the value of the subject by demonstrating its benefits. Fahnestock uses the example of the space shuttle, wherein a deontological argument praising it would focus on the work of the engineers in constructing such a magnificent vehicle or of astronauts on the shuttle performing experiments in zero-gravity that push the frontiers of science. This we can associate with Prelli's evaluative *topos* of elegance: An experiment that reveals a little bit of the elegance and beauty of the universe is very appealing. A teleological argument praising the space shuttle might focus on potential terrestrial applications of technology resulting from the space program, which is obviously synonymous with the evaluative *topos* of fruitfulness. Both of these arguments, as we will see, are found in the original reports from the Rice–Sussex team as well as in their Nobel lectures.

Because all arguments involve both deciding what the issues are and discovering lines of argument, it is appropriate and necessary to use *stasis* theory and Prelli's scientific *topoi* concomitantly in this analysis.

The Original Reports

As described in Chapter 3, fullerene science was born with the publication of a letter to the journal *Nature* in 1985 (Kroto et al., 1985). The Rice–Sussex team had detected unusual clusters of even numbers of carbon atoms, with C_{60} and C_{70} clusters particularly abundant relative to the others. Although the cluster beam apparatus that they used was a unique home-built instrument, the detection device that produced the experimental data was familiar to all chemists: a mass spectrometer. After including the necessary description of the instrument, they could assume that most of their audience could understand the data as it was presented in the paper and interpret the results competently. They believed that mass spectra provided ample evidence that they had created even-numbered clusters of carbon atoms with those with 60 and 70 atoms standing out. The first sentence of the paper makes no nod to any controversy about this interpretation: "During experiments aimed at understanding the mechanisms by which long-chain carbon molecules are formed in interstellar space and circumstellar shells, graphite has been vaporized by laser irradiation, producing a remarkably stable cluster consisting of 60 carbon atoms" (p. 162). The next sentence introduces the point of contention: "Concerning the question of what kind of 60-carbon atom structure might give rise to a superstable species, we suggest a truncated icosahedron, a polygon with 60 vertices and 32 faces, 12 of which are pentagonal and 20 hexagonal" (p. 162). Kroto et al. are operating here in the evidential *stasis*, but they have skipped the conjectural subordinate *stasis* ("Is there scientific evidence for claim *x*?" Clearly there is, from the mass spectrum in Figure 3 of the paper) to settle onto the definitional subordinate *stasis* ("What does the evidence mean?"). This is what the paper sets out to argue. Their theoretical model for this new cluster is the soccer-ball-shaped truncated icosahedron, but this is not the only possibility. Perhaps the cluster is a ring of 60 carbon atoms. Perhaps it's a bowl-shaped or layered structure. These possibilities, and others, are conceivable, but it's the goal of the Rice–Sussex team to show that their model is the most reasonable, a fight they will carry on for the next five years until the Krätschmer–Huffman paper (Krätschmer et al., 1990) vindicats them.

Over the years of building the case for the spheroid structure, they don't deviate much from this formula of introducing experimental evidence and "safe" interpretations before presenting potentially more controversial interpretations. This makes sense, as they must establish the existence of spherical C_{60} clusters—the first (evidential) *stasis*—before they can move on to discussing their ramifications to science in the evaluative *stasis*. Although in the 1985 *Nature* letter they do acknowledge potential applications and make mention of the possibility of fullerenes as the source of the diffuse interstellar bands, it is never the central point of any of their papers. The most urgent need prior to the Krätschmer–Huffman paper is just to show that their spheroid model is valid:

> Thus C_{60} and other even neutral clusters of similar size which we detect appear to be *survivors* of the process taking place with C_{60} being the most inert. The fact that the negative ion distribution produced by ionization in front of the flight tube is so similar to the positive ion distribution produced by laser photoionization of the beam a meter downstream is convincing evidence that the distributions shown in figs. 1a and 1b reflect the neutral cluster distribution and are *not* an artifact of the ionization process. This neutral distribution, in turn, is strong evidence supporting the spheroidal carbon shell model for even carbon clusters in this size range in general and the truncated icosahedron (soccer ball) model for C_{60} in particular. [emphases appear in the original] (Liu et al., 1986, p. 217)

Highlighting the fact that not all of the *stases* can be applied in every argument, the Rice–Sussex team's journal articles did not delve much into the second (interpretive) *stasis*, in which questions of classification take place. There was no need to. There is one argument that could have arisen that would have taken place in the interpretive *stasis*: whether the spheroid carbon structures constituted a new allotrope of carbon or a special case of graphite. Diamond and graphite are easily distinguished by their obviously different macroscopic properties as well as by their properties on the molecular level (see also Chapter 2). At the microscopic level, diamond has a three-dimensional structure extending indefinitely in all directions with each carbon atom bound to four other carbon atoms in a tetrahedral geometry, like a pyramid with four sides, each of which is an equilateral triangle. (In more technical terms, this description indicates that the four valence atomic orbitals of the carbon atoms are all sp^3-hybridized.) By contrast, graphite exists as, essentially, two-dimensional sheets with chicken-wire-like hexagonal rings extending indefinitely in just two directions and having two well-defined flat sides, like a sheet of paper. Each carbon atom is bound to just three other carbon atoms in a trigonal planar arrangement, a flat triangle. (In this case, the carbon atoms have three sp^2-hybridized valence atomic orbitals arranged in a trigonal planar geometry around the atom while a dumbbell-shaped p-orbital bisected perpendicularly by the plane of the hybridized valence orbitals remains.) As in Jones's fantasy (1966), fullerenes can be envisioned as graphite sheets curled around and stitched together into spheres, with some pentagonal rings incorporated into the structure in order to facilitate the curving. Indeed, fullerenes resemble graphite in that each atom is bound to just three other carbon atoms in a slightly distorted trigonal planar geometry (the distortion is due to the curvature of the molecule), and three of the valence atomic orbitals in fullerenes are sp^2 hybridized with a p-orbital remaining. However, this never seems to have been a point of contention; fullerene, if it was as the Rice–Sussex team said it was, was readily accepted as a new allotrope because it exists as discrete molecules, whereas graphite is an extended solid, and so this debate in the interpretive *stasis* did not figure prominently. In any event, taxonomic classification seems not to be as important for chemists as for biologists, for example, because for chemists classification usually isn't intended to reflect something as significant as, say, evolutionary relationships, as it often is in biology.

The original reports, because they propose to answer scientific questions (namely, what are the properties of this new form of carbon?) are rife with arguments via the problem–solution *topoi*, which can be used to address exigences in the first (evidential) *stasis* ("Is there scientific evidence for claim *x*?" "What does the evidence mean?"). In the original *Nature* letter (Kroto et al., 1985), the first mention of the spheroidal structure, in the first paragraph, includes an appeal to explanatory power. After declaring the remarkable stability of the C_{60} cluster, they propose the spheroid structure, pointing out that the "molecule which results when a carbon atom is placed at each vertex of this structure has all valences satisfied by two single bonds and one double bond, has many resonance structures, and appears to be aromatic" (p. 162), all of which would be understood by other chemists to be evidence of the chemical stability that would be expected to be imparted by this structure. (In chemistry, aromaticity refers not to odor but to a special delocalization of electrons in a molecule—spreading the electrons between atoms—that gives the molecule added stability.)

After this introduction to the buckminsterfullerene molecule, the very next paragraph describes the cluster beam apparatus. Again, this was a home-built instrument that was not commercially available and, beyond the mass spectrometer as the detection device, was hardly a standard piece of equipment in a chemistry laboratory, so the Rice–Sussex team had to establish that the experimental results produced using this instrument were valid, an appeal to arguably the most important problem–solution *topos*: experimental competence. Failing on this point can wreck an entire argument regardless of the strength of other appeals because it casts doubt on all experimental results. As explained previously, the readout of the data was easily interpretable by most chemists, but scientists well versed in mass spectrometry know that the results are not necessarily as straightforward as they appear and, indeed, others did argue that the C_{60} was not in fact an intrinsically stable molecule but rather that its prominence in the Rice–Sussex team's mass spectra was the result of the particular conditions of the experiment (Baggott, 1994, pp. 100–102). Much of Curl, Kroto, and Smalley's work over the next several years was aimed at fighting this challenge.

Evaluative *topoi* are in evidence in the original reports as well and, by definition, are used to address exigences in the third (evaluative) *stasis* ("Is the claim scientifically significant?"). Even in the original *Nature* letter, the Rice–Sussex team has already called the proposed spherical structure "unusually beautiful," using the *topos* of elegance. Immediately after describing the proposed spherical structure, the Rice–Sussex team outlines "a number of important ramifications arising from the existence of such a species" (p. 162), including possibly being found in circumstellar shells and interstellar dust or possibly as the source of the diffuse interstellar lines. Possible terrestrial uses for buckminsterfullerene or derivatives thereof are speculated to be lubricants, catalysts, or models for prebiotic chemistry. All of this is an appeal to the possible fruitfulness of the research (another of Prelli's evaluative *topoi*) and Fahnestock's (1986) teleological argument (the "application" appeal described above) that she found in the popularized articles in her study.

The selection of the name buckminsterfullerene and the comparison of its structure to that of Buckminster Fuller's geodesic dome and to that of a soccer ball are clever uses of the exemplary *topos* of analogy. By associating the structure of the molecule with these icons, Kroto et al. solidify them in the minds of their audience. Unless it can also be compared to a manmade structure like these, any other proposed structure would be at an inherent disadvantage. In this case, they have used this exemplary *topos* in the evaluative *stasis*: to highlight the significance of their discovery. A molecule that looks like a geodesic dome is inherently more interesting than one with a less symmetric, less beautiful structure. Here they have used the *topos* of analogy to make a deontological argument (the “wonder” appeal) to support the significance of their work.

With the publication of the Krätschmer–Huffman paper in 1990, this stage in the fullerene story was ended. The questions in the first (evidential) *stasis* (“Is there scientific evidence for claim *x*?” “What does the evidence mean?” “Which empirical judgments are warranted by available evidence?” and “Which evidence more reliably grounds claims about what does and does not exist?”) had been answered. The Rice–Sussex team’s appeals had succeeded, with some help from Krätschmer and Huffman. We now examine how different circumstances, Nobel Week in Stockholm, reshaped the story.

The Nobel Lectures

We saw in Chapter 3 how Curl, Kroto, and Smalley adjusted their statements about the spheroidal structure as appropriate for the time and audience. So too do their Nobel lectures [Curl, 2003; Kroto, 2003; Smalley, 2003] reside in different *stases* than the original reports. Fahnestock (1986) found that when accounts from the primary scientific literature are reworked for popular audiences, a genre shift occurs: “The movement of a scientific observation through the *stases*, its ‘rhetorical life,’ is an inevitable consequence of changing the audience for a piece of information and thus the purpose of relating it and thus the genre of the discourse that conveys it” (p. 291). Likewise, the Nobel lectures, delivered several years after the original reports described earlier, have different aims to achieve, and so, not surprisingly, they operate in different *stases.* Whereas in the original reports the scientists carefully laid out experimental evidence for their claim that C_{60} was a new, spheroidal allotrope of carbon, in the Nobel lectures, Smalley and Kroto generally give little attention to experimental procedures. When they do describe experiments, the intention is usually just to include them as part of the story. Consequently, although they do address issues in the first (evidential) and second (interpretive) *stases*, these issues are not nearly as important as issues in the third (evaluative) and fourth (methodological) *stases*. Curl takes a different approach, which is discussed later in this chapter.

This raises the question, though, about the usefulness of *stasis* theory in examining epideictic discourse such as the Nobel lectures. *Stasis* theory, by definition, is the examining of the points of *stasis*, or stoppage, that can occur in an argument. Are there really points of stoppage in epideictic discourse, which is meant to celebrate and solidify shared values—to "preach to the choir," as it were? The answer to this is yes. The aim of much epideictic discourse is to cause the audience to cling more strongly to their shared values. Although it may not instill altogether new values, it still aims to effect a change in the audience, even if one of intensification, and this can rightfully be considered an argument worthy of analysis by *stasis* theory.

The Nobel lectures: Smalley. Early in his lecture, Smalley (2003) rhapsodizes about what he considers to be the inherently more interesting nature of graphite over diamond because the structure of diamond at the atomic level extends indefinitely in all three dimensions, whereas graphite exists as sheets that extend in only two directions and have an upper and a lower surface at which other species can adsorb. As a way of segueing into fullerenes and nanotubes, Smalley says that carbon's "genius of making a chemically stable two-dimensional, one-atom-thick membrane in a three dimensional world … I believe, is going to be very important in the future of chemistry and technology in general" (p. 91). This statement is Smalley's lecture in microcosm, and it rests firmly in the third (evaluative) *stasis*, using the evaluative *topos* of fruitfulness. Smalley need not prove anymore that he found a new allotrope of carbon; he needs to show why the discovery is significant. Presumably the Nobel committee made its decision partly on this basis, but now, in one's Nobel lecture, is a good time to reinforce it in the minds of one's audience.

True, Smalley, like Curl and Kroto, does tell the tale of the experiments in September 1985 that got them to this point, but the purpose is no longer to convince others of the validity of their evidence or of their interpretation. In fact, Smalley uses the tale of the discovery of fullerene at least partly for a different purpose than to provide experimental evidence of their discovery. In telling the story, he dispels the notion that the discovery of the fullerenes was the result of purely basic, unapplied research and shows how well-funded, focused research results in solutions to real-world problems. In doing so, he also takes the opportunity to highlight once again the continuing fruitfulness of their work:

> [I]f fullerenes turn out to lead to the technological wonders that some people (like me) believe are in our future, then perhaps one can argue that any research project could get lucky too, no matter how irrelevant to worldly problems it may currently seem. I have argued this way in the past, and I still believe there is some sense to it—but only a little. In fact, the fullerenes were discovered as a result of decades of research and development of methods to study first atoms, then polyatomic molecules, and ultimately nanometer-scale aggregates. It was well-funded research that at nearly every stage was justified by its perceived significance to real-world technological problems. To a great extent, many of these earlier bets as to the worldly significance of fundamental research actually paid off. (p. 101)

The fullerene story is one of the relatively rare cases in which the object of a discovery couples both great aesthetic appeal with obvious scientific significance. Smalley's lecture is meant to celebrate and emphasize both of these. He does not aim to defend his methods or his results; he aims to celebrate them and their place in the history and the future of science, and so he passes over the evidential and interpretive *stases* to settle primarily in the evaluative and methodological, which is consistent with Fahnestock's findings for popular science.

The Nobel lectures: Kroto. In Chapter 3 we were introduced to Kroto's charming, whimsical style. The printed text of his lecture is over twice as long as Smalley's, and, as noted in Chapter 3, he uses it to spin the entire tale of his and his Sussex colleagues' role in the discovery of the fullerenes. Compared to Smalley, Kroto tells the story in much more detail and with more personal anecdotes, such as hitting Houston's bookstores with Smalley's student Jim Heath and spending late nights at local eateries. Again, he includes experimental details only as far as they will help tell the story. Like Smalley, Kroto is celebrating their work, not defending it.

Whereas Smalley drives home the importance of their work in terms of its fruitfulness, Kroto, more than either of the other two, works in another of Prelli's evaluative *topoi*: elegance. In fact, Kroto uses the very word repeatedly throughout his lecture, and even when he doesn't, it's the point he's often trying to make. As in the popularized articles examined by Fahnestock (1986), he focuses primarily on the significance of the discovery, in this case by promoting the fullerenes' aesthetic appeal. He's clearly smitten, with a propensity to descend into hyperbole:

> The story of C_{60} cannot be recounted without reference to its beauty which results from the incredible symmetry. Another important aspect of the molecule's aura lies in the name buckminsterfullerene and the direct association it has with geodesic domes designed by Buckminster Fuller [using the *topos* of analogy, discussed above]. It invests this elegant molecule with a charisma that has fascinated scientists, delighted lay people and has infected children with a new enthusiasm for science *and in particular it has given chemistry a new lease of* [*sic*] *life.* [emphasis added] (p. 46)

Kroto continues with the theme that the fullerenes' elegance is derived from its symmetry:

> The electron angular momentum wavefunctions are described in terms of spherical harmonics—known by mathematicians long before 1925 when Quantum Mechanics was created and thus these symmetric mathematical functions lend an *elegant beauty* to the abstract description of rotational/orbital motion in atoms and molecules. Thus we discover that symmetry principles underpin the *elegant* quantum mechanical description of atoms and molecules in an abstract picture in which statics and dynamics are paradoxically conflated in a way that often leaves us hovering on the boundary between abstract mathematical understanding and literal physical misunderstanding. *It is the existence of similar abstract aspects of symmetry that has invested the C_{60} molecule with a charismatic quality that few other molecules possess.* [emphasis added] (p. 47)

Although it would be unseemly to use the *topos* of elegance in an evidential sense, that is, to argue for or against the existence of the fullerenes in nature, as doing so would be a gross violation of the strongly held value in science that scientific work should be driven by logic and reason, not by emotion, it is frequently used evaluatively; hence, Prelli's classification of it among the evaluative *topoi*. Note also that although Curl, Kroto, and Smalley all make mention of the inherent beauty of the C_{60} molecule, they take pains also to point out the fruitfulness—the "real results"—of their discovery.

Diverging somewhat from Smalley, Kroto takes time to point out that much of his work at the University of Sussex leading up to the fullerene discovery was basic research with little or no anticipated application:

> It was the epitome of basic research and in these times when the mania for applied research is rampant, it is hard to imagine one less likely to gain support. There is little doubt in my mind that, had this project not been initiated, we at Sussex would not have been involved in the discovery of C_{60}. (p. 52)

Kroto does not neglect the problem-solution *topoi*. He delves into using the *topoi* of explanatory and predictive power when he discusses Heath's work incorporating lanthanum into the fullerene cage (p. 61; Heath et al., 1985; discussed also in Chapter 3) and when he discusses how they discovered that, in theory, no cage structure could be constructed with 22 or fewer atoms, which explained why the smallest even-number cluster that exhibited enhanced stability was the 24-atom cluster. "Thus there appeared to be 'semistable' fullerenes (at least in the beams) down to C_{20} that were predictable … on the basis of the cage closure concept. What more 'proof' could one possibly want for the whole fullerene concept? None" (p. 63). Predictive power appears again in Kroto's lecture when, shortly after learning of Krätschmer and Huffman's breakthrough, his group makes enough fullerene to acquire the ^{1}H and ^{13}C nuclear magnetic resonance (NMR) spectra and finds that the spectra of C_{60} and C_{70} correspond precisely to their predictions for a molecule of buckminsterfuller-ene's structure. However, whereas in the original report they use the problem-solution *topoi* to defend their interpretations of their results, here they're used for a different purpose: to celebrate the method that achieved them.

Like Smalley, Kroto does make a point to acknowledge work by others that sprung from the fullerene discovery, showing that he too considers the fruitfulness of the work to be important, but his lecture is primarily about the elegance of the fullerene structure:

> Last but not least we should note that, perhaps, the molecules [*sic*] most delightful property lies in the inherent charisma which arises from its elegantly simple and highly symmetric structure that is quite unlike any other. It is this charisma that has stimulated delight and fascination for chemistry in young and old alike. (p. 75)

The Nobel lectures: Curl. Chapter 3 introduced Curl's carefully skeptical approach to his lecture. More than either Kroto or Smalley, he appears on the surface to remain in the evidential *stasis*, explaining the reasoning and the experimental results that led them to the spheroid structure, as if he's still trying to convince the world that he and Kroto and Smalley were right. Curl even structures his lecture somewhat like an ordinary scientific paper with the familiar introduction–methods–results–discussion (IMRD) structure.

Like Smalley and Kroto, he begins with a short review of other researchers' work that preceded theirs and explains the experimental methods used in their own discovery. He then carefully lays out and explains their results. For a man who has just received science's highest honor, he can sometimes come across on the surface as surprisingly defensive: "Our claim has always been that the situation is much closer to proof than conjecture" (Curl, 2003,

p. 16). "[W]e have never really considered the assignment of the prominent C_{60} peak in the mass spectrum to the buckminsterfullerene structure to be a guess" (p. 17).

Curl, though, is merely exercising the characteristic skepticism that we saw in Chapter 3. Gradually he builds his case, explaining their reasoning with the evidence at hand. "The next few years of our lives were devoted to testing experimentally this fullerene hypothesis and finding that it passed every test" (p. 19). As described in Chapter 3, he builds his story by stating and evaluating a series of conjectures and answering them with experimental evidence: "Conjecture – The Fullerene Hypothesis," "Experiment – Reactivity and Photofragmentation," "Conjecture – The Existence of Endohedral Complexes," "Experiment – Endohedral Metallofullerenes and 'Shrink Wrapping,'" "Conjecture – C_{70} has D_{5h} Symmetry," and so on. He carefully states his warrants to each claim, appealing largely to the problem-solution *topos* of predictive power. If their claim that C_{60} is a closed spheroid molecule is correct, they should be able to predict the results of experiments designed to test this hypothesis: "If the activation barriers to fragmentation follow the energetics, one would expect that the special stability of C_{60} would be apparent in the fragmentation pattern. … Figs. 9, 11, and 12 taken together provide striking evidence that [the] cations being examined are structurally related to each other and to C_{60}^{BF}" (pp. 22–24). (In their notation, the superscript "BF" indicates spherical buckminsterfullerene, as opposed to any other possible isomer.) Other details were given previously in Chapter 3. He concludes with the following:

> Finally, I believe that the conjecture that started it all, namely that truncated icosahedron C_{60} forms spontaneously in condensing carbon, scarcely belongs in the category of conjecture. The three mass spectra in Fig. 5 when coupled with the conditions under which they were obtained demand that the species responsible for

the prominent peak at C_{60} must be singularly different and chemically relatively unreactive. The human mind can conceive of no other isomer of C_{60} that better fits this requirement. (p. 30)

It would be tempting to say that Curl operates primarily in the first (evidential) *stasis*, but this would be a mistake, and for this reason, Curl's lecture demonstrates the transforming power of *kairos*. Although his arguments resemble forensic rhetoric in the evidential *stasis* in many superficial ways, especially as he carefully lays out experimental evidence, as described here and in Chapter 3, the lecture is, in the end, epideictic, taking place not even primarily in the third (evaluative) *stasis* with Kroto and Smalley, but in the fourth (methodological) *stasis*: "What should be done about it?" Curl doesn't need to defend his research now, and he knows it. His Nobel lecture is about celebrating science and the scientific method, presenting an ideal for scientific reasoning. This point is taken up in more detail later in this chapter.

Significantly, in addition to the conjectures and experiments named above, he also discusses two conjectures that turned out to be wrong: "Conjecture – C_{60} Might Be the Carrier of the Diffuse Interstellar Bands" and "Conjecture – Soot is Formed From Spiraling Icosahedral Carbon Shells," discussing how, eventually, evidence was found that rebutted those claims. Negative data can be hard to find in the regular scientific literature, but Curl readily includes it because he aims with this lecture to discuss not only fullerenes in particular but also science in general, which is not as clean and straightforward as the literature would sometimes indicate (see especially Latour & Woolgar, 1979).

In his conclusion Curl again diverges from Smalley and Kroto. He brings his lecture to a close not with a discussion of the fruitfulness of his work nor with a paean to the elegance of truncated icosahedra but with a thoughtful discussion on the philosophy of science. According to Curl,

> conjecture drives both experiment and theory for it is only by forming conjectures (hypotheses) that we can make the direction of our experiments and theories informed. … Conversely, experiment and theory drive conjecture. One makes a startling observation or has a sudden insight and begins to speculate on its significance and implications and to draw possible conclusions (conjecture). (p. 29)

He continues, reflecting on the fact that not all conjectures are equally valid, as borne out by their incorrect conjectures about the possible roles of fullerene in the diffuse interstellar bands and in the formation of soot. "[C]onjectures should be judged by the accumulation of evidence that supports and contradicts them." He criticizes their conjecture about the diffuse interstellar bands with an appeal to Prelli's evaluative *topos* of fruitfulness: "Primarily, this conjecture has the fatal defect that it has stimulated little productive science" (p. 30). He contrasts this with praise for the fullerene discovery:

> On the other hand, the conjecture that a new whole class of carbon cage compounds, the fullerenes, are formed spontaneously in condensing carbon vapor has led to sweeping consequences. At the time, this hypothesis seemed to be the only logical explanation of the observed carbon mass spectra distributions, but it was not self-evident. As we have seen, we tested this conjecture in a variety of experiments which always provided evidence supporting the conjecture. This pattern of repeated confirmation of expected consequences is what is expected for a correct hypothesis. In the long run, the fullerene hypothesis has proved to be spectacularly correct and it has provided the basis for a whole new branch of organic chemistry. (p. 30)

So, Curl has used this *topos* both to praise a portion of his work and to condemn another, but his lecture is not, at the core, about fullerenes. Curl loves the way science is done, and that's the real topic of his lecture. In 1985 he had to defend his methods; in 1996 he didn't. The different *kairotic* situation allowed him to use a lecture ostensibly about fullerene to praise science itself.

Discussion

The Nobel lectures generally follow the pattern described by Fahnestock (1986) for research reports and popularized accounts of science. The original journal articles aim to convince a skeptical audience of the quality of their data and the validity of their interpretations of it, and so they operate in the first one or two *stases* and use the problem-solution *topoi* to demonstrate the validity of their experimental results and their interpretations thereof. Like the popularized accounts analyzed by Fahnestock, the Nobel lectures are aimed at a more general audience than the journal articles are. The journal articles, other than those in the greatly respected and widely read *Nature,* are aimed at scientists practicing in a relatively narrow field; the Nobel lectures are of interest to a much wider audience of scientists and, to a lesser extent, the general public. Although they do describe experimental results, the purpose is not to defend or validate the results themselves but to explain their long-term significance and to celebrate the process of science, primarily in the third (evaluative) *stasis* and the corresponding evaluative *topoi*. However, they also differ in some respects from the popular articles in Fahnestock's study. Both popularized accounts of science for a general audience and Nobel lectures are primarily epideictic, meant to celebrate science, but the ways that they do so are quite different. Fahnestock showed that popularized accounts appeal to a layperson's wonder or to practical applications. The Nobel lectures examined here to some extent do appeal to these, especially the beauty of the fullerene structure, but they primarily appeal to the values of a scientific audience regarding how science ought to be performed.

Chapter 5: Conclusion

These Nobel lectures are somewhat paradoxical. While presenting statements as *type 4* facts, as opposed to the more hedged statements of *types 2* and *3* found in the original research reports, they also are more likely than the research reports to discuss science as it is actually performed, with the stops and starts and pitfalls. The Nobel lectures therefore are a reflection of the paradigm among scientists that although scientific research is messy sometimes, their work produces objective knowledge apart from and unmediated by language or other human constructs, or at least that it must be presented as such. This conception of scientific rhetoric stems from positivist philosophy, especially the logical positivism that reached its zenith in the early twentieth century (Miller, 2004):

> [I]n this view, science and rhetoric are mutually exclusive. Science has to do with observation and logic, the only ways we have of approaching external, absolute reality. Rhetoric has to do with symbols and emotions, the stuff of uncertain, incomplete appearances. (p. 49)

In their Nobel lectures, Curl, Kroto, and Smalley feel less restricted by the expectations that come with publishing in the primary research literature, and their core values on the epistemology of scientific rhetoric shine through.

These differences suggest that Nobel lectures can be defined as a distinct genre having the following characteristics:

- Statements on the upper end of Latour and Woolgar's scale, reflecting little need for moderation or hedging

- Arguments in the third and fourth *stases,* arguing for the value of the research and for future action rather than defending the conclusions
- Recognition of the contributions of coworkers and colleagues to the field
- Discussion on the nature of science itself and on proper procedures and methods in research, often celebrating the scientific method

Although this work owes some of its inspiration to Fahnestock (1986), it does not represent a duplication of it. Whereas she examined the genre shift between research articles aimed at fellow scientists and popular articles on the same topics written by journalists and aimed at a lay audience, I examined the genre shift between research articles aimed at fellow scientists and epideictic lectures delivered by scientists themselves primarily to other scientists. Fahnestock examined "similar subject matter being communicated to dissimilar audiences" (p. 292), but I have examined similar subject matter being communicated to similar audiences but under different circumstances.

Significantly, it could be argued that in some ways Curl, Kroto, and Smalley were addressing the same audience in 1996 in their Nobel lectures as they were in 1985 in the *Nature* letter but that in those eleven years the audience had evolved. Both the Nobel lectures and the journal *Nature* are aimed at an audience of scientists but a more general audience than a more specialized journal, such as the *Journal of the American Chemical Society* or, further, the *Journal of Chemical Physics*. However, the difference is again that property that pervades the study of rhetoric: *kairos*. Although the description of the audience—scientifically literate, not necessarily limited to chemists in particular—may not have changed significantly in eleven years, an important property of that audience had changed: In 1996 they took the existence of the fullerenes for granted. Curl, Kroto, and Smalley responded to this evolution in their audience with an evolution in their rhetoric. Their earlier efforts to lay out and defend their work during the late 1980s gave them their bully pulpit in Stockholm in 1996 to argue for the future significance of their research, to bring honor and recognition to others in the field and in their laboratories, and, especially for Curl, to make their own commentary on the nature of science and the scientific method, given added weight by the luster of the Nobel Prize. In 1985 they were slaves to scientific ethos. In 1996 they helped to shape it.

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