The Limits of the Rhetorical Analysis of Science

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> When you come to a fork in the road, take it.
> — Yogi Berra

In the wake of the publication of Alan Gross's *The Rhetoric of Science*, a debate ensued concerning the role of rhetoric in the analysis of science (Gross, 1990). It concerned Gross's claim that science was rhetoric all the way down. The debate culminated in *Rhetorical Hermeneutics,* a book collection edited by Gross and William Keith (Gross and Keith, 1997). Featured was a lengthy essay by Dilip Gaonkar, an argument deeply skeptical of the possibility of a rhetoric of science in any shape or form. Gaonkar averred that rhetoric was, simply, not equal to the task of analyzing an enterprise so complex. Rhetoric was, Gaonkar asserted, not an analytic, but a productive art. Forced to do duty analytically, it revealed only how impoverished its machinery was. Rhetoric of science may be dismissed, however, only if we accept Gaonkar's view of rhetoric. Rhetorical analysis is more than the mobilization of an analytical apparatus; it is also an orientation toward the persuasiveness of concepts that the philosopher W.D. Gallie calls "essentially contested." To Gallie, what counts as science will never be settled: arguments concerning its nature and scope will never cease. For Gallie, this is to say that

> a certain piece of evidence or argument, put forward by one side in an apparently endless dispute, can be recognized to have a definite logical force, even by those it fails to win over, or convert to the side in question; and that when this is the case, the conversion of a hitherto wavering opponent of the side in question can be seen to be *justifiable* (Gallie, 1968, 185).

Gallie's is an argument about what science is, not what it produces: its facts and theories.

In this paper, I will agree with Gallie that, once they are effectively beyond argument, these facts and theories are not essentially contested; to view them so is a category mistake. While a great deal of science is properly an agonistic field, open to rhetorical analysis, it is not open to debate that there is a periodic table, or that DNA has the structure it most certainly has. In other words, I wish to contend that the rhetoricity of science is time sensitive. There was a time when the earth was on good scientific grounds thought not to be old enough for evolution to have occurred. Radioactivity had not been taken into account; it had not yet been discovered. The goal of science is to place its claims beyond argument. I think it is incontestable that, on occasion, it succeeds, as with evolutionary theory. When it does, rhetoric can have no purchase, since no legitimate forum for debate exists as a target for such analysis.

In this paper, I will look at three contrasting cases, the discovery of the reproductive process of DNA, a case in which an existing agonistic field eventually disappears; Robert Hooke's *Micrographia,* in which science and rhetoric exist side by side, the former making substantial progress toward facts and theories that are beyond argument, the latter barred forever from reaching that goal. Hooke's deliberative contention is that science, far from being subversive to religion, reveals the details of God's handiwork. In the third case, global warming, science policy is at issue. While the disappearance of uncertainty signals the termination of the appropriateness of rhetorical analysis for the science, the political issues that remain are very much a target.

The Structure of DNA and the Progress of Science

It does not seem possible to argue against the theory that bacteria and viruses cause many diseases; it does not seem possible to argue against the periodicity of the elements or against the conversion of mass and energy embodied in Einstein's famous equations. It does not seem possible to argue against some forms of scientific progress: that astronomy gets closer to the truth as we move from Ptolemy to Copernicus to Kepler to Newton; that chemistry progresses as we move from Lavoisier to Pauling; that continental drift progresses from Wegener's unlikely geophysics to the reality of sea-floor spreading. The discovery of the structure of DNA and, following that, the discovery of the means by which it reproduces, show us just how science progresses and how its achievements become more and more robust, less and less subject to argumentative challenge until, at some point, this sense of certainty is so considerable that it is beyond challenge altogether. In other words, there is a firm consensus. But it is vital to point out that this consensus is achieved not by negotiation but by interactions among scientists, their equipment, and the material world, interactions whose results are persuasive only because they are replicable. While science is without doubt a social phenomenon, and while scientific practices are without question socially constructed, it does not follow that the products of these practices are socially constructed as well. Only a global and implausible skepticism would deny the reality of electrons. If they can be used as tools—and they can be—then they are as real as hammers and nails. On these matters, there is, as philosopher Bernard Williams says, a convergence that is, importantly, uncoerced (Williams, 1985, 171). Moreover, there is no variation in this consensus from culture to culture, from ethnic group to ethnic group. There is no Chinese science, there is no Jewish science, and there never will be.

When they first proposed the structure of DNA, Watson and Crick were appropriately cautious: "The previously published X-ray data on deoxyribose nucleic acid are insufficient for a rigorous test of our structure. So far as we can tell, it is roughly compatible with the experimental data, but it must be regarded as unproved until it has been checked against more exact results" (Watson and Crick, 1953a, 737). However, throwing caution aside, they speculated that "the specific base pairing we have postulated immediately suggests a possible copying mechanism for the genetic material." In a subsequent paper, they are specific about the nature of this copying mechanism, later known as semi-conservative replication:

> We imagine that prior to duplication the hydrogen bonds are broken, and the two chains unwind and separate. Each chain then acts as a template for the formation onto itself of a new companion chain, so that eventually we shall have *two* pairs of chains, where we had only one before. Moreover, the sequence of base pairs will have been duplicated exactly (Watson and Crick, 1953b, 966; emphasis theirs).

This was not the only scheme of duplication in contention. Gunther Stent hypothesized a conservative mechanism, one in which a new strand of RNA would develop within the physical confines of the DNA double helix. When fully formed, this strand would separate and combine with another of RNA. Together, these two strands would form a template that would stamp out other DNA double helices. Max Delbrück suggested another scheme, dispersion, in which new double helices would form when existing helical chains broke and then rejoined at "growth points." In the chains thus produced, new and existing DNA would alternate. Delbrück also made an important contribution by suggesting a test that would differentiate decisively among the three hypotheses: if the parent strains could be labeled in some way, the daughter strains would exhibit a clear differential distribution.

Four years after this suggestion was made, it was realized in an experimental program, famously referred to as "the most beautiful experiment in biology" (Holmes, 2001). After rejecting various alternative labelings, two young scientists, Matthew Meselson and Franklin Stahl, incorporated a heavy isotope of nitrogen, N^{15}, into the DNA of the bacterium *Escherichia coli*. After fourteen generations, the *E. coli* with its heavier DNA was transferred to a medium in which *E. coli* incorporated standard nitrogen, N^{14}, with its slightly lower atomic weight. The hope was that the distribution of labels and, therefore, of weights, in the contrasting generations of daughter molecules would decisively differentiate among the three hypotheses. Only in the case of Watson-Crick's hypothesis would Meselson and Stahl obtain daughter generations of purely hybrid (N^{14}/N^{15}) DNA molecules, followed by daughter generations consisting of an equal mix of hybrid and N^{14} DNA molecules. In contrast, Stent's hypothesis predicted the presence of N^{15} DNA throughout succeeding generations; Delbrück's, predicted neither hybrids, nor pure N^{15}, nor pure N^{14} DNA in succeeding generations.

These differences in atomic weight are minute; at the time, they could not be detected even with the most delicate of balances, but only by means of an ultracentrifuge, a machine that spins at an incredible 44,770 revolutions per minute. This is the equivalent of as much as 289,000 times the force of gravity. During centrifugation the DNA is concentrated in the solution and forms a band whose position depends on its weight. As a consequence of this density-gradient centrifugation, after a day or two DNA molecules suspended in a solution of a heavy cousin of sodium chloride, cesium chloride (CsCl), are separated according to their weight. Meselson and Stahl visualized the result for further analysis by use of an automatic camera whose result is seen in Figure 1.

In the first ten photographs in the sequence (generations 0 to 4.1), we see the stages of bacterial growth from generation to generation. The one dark band in the top photograph represents pure N^{15} DNA. In the second photograph, another, lighter density gradient (hybrid N^{14}/N^{15} DNA) appears to the left of the first (N^{15} DNA). In the third, the initial density gradient begins to fade; in the fourth, the first daughter generation, it completely disappears, and the hybrid alone remains. As we move toward the second daughter generation—photographs five through eight—a new, lighter density gradient appears to the left and grows in intensity, indicating the presence of hybrid N^{14}/N^{15} and N^{14} DNA alone, confirming Watson and Crick's hypothesis. In photographs nine and ten, this has become the dominant density gradient. The graphs in Figure 1 depict the same sequence in a different visual modality, where the peaks represent the concentration of DNA in the bands.

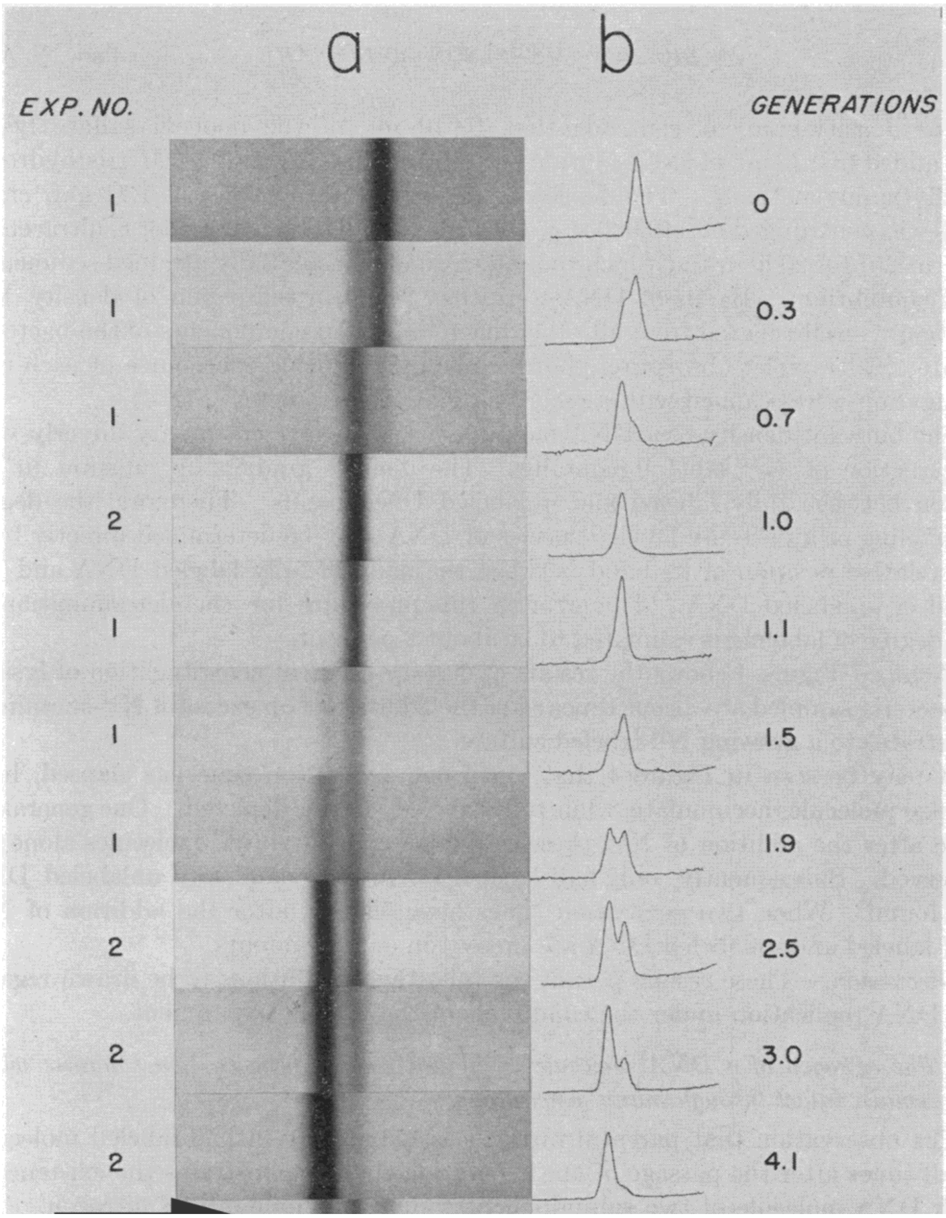

Figure 1. Photographs of DNA bands formed after centrifugation and graphs of band density (Meselson and Stahl, 1958, 312). On the left side of this figure (a), we see DNA bands showing density gradients for the various "generations" listed in the right-hand column. Density increases as we move from left to right. On the right side (b), we see microdensometer tracings of these DNA bands. The height above the base line of these tracings corresponds to the concentration of DNA.

Meselson and Stahl's experiment was designed to provide evidence for decisively selecting one among three hypotheses in contention: Watson-Crick's, Stent's, or Delbrück's, each predicting a particular experimental result. The result obtained supported Watson-Crick's hypothesis, and undermined the hypotheses of Delbrück and Stent. The eliminative induction by which they argue can be modeled as a deductive process:

If hypothesis A is the case, x will be the result.

If hypothesis B is the case, y will be the result.

If hypothesis C is the case, z will be the result.

The result is x.

Therefore, A is the case.

The apodeictic quality of this conclusion, however, is an illusion. There is no guarantee that the correct hypothesis is among those being tested, that another hypothesis, also compatible with the experimental evidence, is really the correct choice. Secondly, there is always the possibility that equipment whose proper functioning has been taken for granted has, in fact, malfunctioned; perhaps the ultracentrifuge was improperly calibrated. Thirdly, it may be the case that one of the procedures by which Meselson and Stahl obtained or manipulated their DNA introduced a variable of which they were not aware, one that compromised their result. Nevertheless, a general consensus concerning the means of DNA reproduction was soon achieved.

Consensus, even general consensus, is, of course, not truth. Once, there was a general consensus that the earth did not rotate on its axis; it endured for many centuries. In the *Almagest,* for example, Ptolemy dismisses the earth's rotation as "ridiculous." He finds unbelievable the idea that the heavier earth moves while the lighter air remains still. Moreover, if the earth rotates from west to east on its axis—at a speed of about 1000 miles an hour—it would mean that clouds could never be seen to move toward the east. But that is not the whole of Ptolemy's story. He admits that "there is nothing in the celestial phenomena which would count against that hypothesis [of rotation]," conceding thereby that the question of the earth's motion is, in fact, a matter of contention, that it is not beyond argument (Ptolemy, 1984, 44–45).

The general consensus concerning the structure of DNA was consensus of another kind than that achieved by sheer argument, by negotiation, or by indoctrination. When first announced, it is true, the structure was only a model that was both consistent with the facts and mechanically feasible. At first, then, "the sense that a molecule of this structure exists at all, the sense of its reality, is an effect only of words, numbers, and pictures judiciously used with persuasive effect" (Gross, 1990, 54). But this state of affairs was temporary. Molecular biology progressed, and as it did, as the successful encounters between scientists and the world mounted, the robustness of the original model increased to a point at which its reality was no longer open to challenge. In helping to reach this goal, Meselson and Stahl's experiment accomplished two tasks: first, it chose decisively among competing models of reproduction; second, in so doing, it increased our confidence that Watson and Crick's structure was itself correct. Meselson and Stahl's was the first considerable step in the direction of a robustness currently so overwhelming that the reality of the molecule's structure has become permanently beyond argument.

Hooke's *Micrographia:* Science and Rhetoric Side by Side

There exist two explorations of Hooke's rhetoric, articles by John T. Harwood and Jordynn Jack. In "Rhetoric and Graphics in *Micrographia,*" Harwood sees the book's many images as central. These create meaning in interaction with their accompanying verbal descriptions. He sees these interactive complexes as a form of the classical figure, *enargia,* a term he interprets correctly as vivid description. *Enargia,* which surfaces first in the anonymous *Ad Herennium,* re-appears in Cicero's *De Oratorio,* and in Quintilian's *Institutes*. An example from *Ad Herennium* illustrates the central characteristics of the figure, its emphasis on drama and emotional impact. The subject is the assassination of Tiberius Gracchus:

> When Gracchus begins a prayer to the gods, these creatures in a rush attack him, coming together from all quarters, and a man in the crowd shouts: "Fly, Tiberius, fly! Don't you see? Look behind you, I say!" Then the fickle mob, stricken with sudden fear, take to flight. But this fellow, frothing crime from his mouth, breathing forth cruelty from the depth of his lungs, swings his arm, and, while Gracchus wonders what it means, but still does not move from the place where he stood, strikes him on the temple. Gracchus does not impair his inborn manliness by a single cry, but falls without uttering a sound. The assassin, bespattered with the pitiable blood of the bravest of heroes, looks about him as if he had done a most admirable deed, gaily extends his

murderous hand to his followers as they congratulate him and betakes himself to the temple of Jupiter (Page, *ed.,* 1956 IV.lv.68).

This passage may be compared with one from *Micrographia* describing the louse depicted in Figure 2.

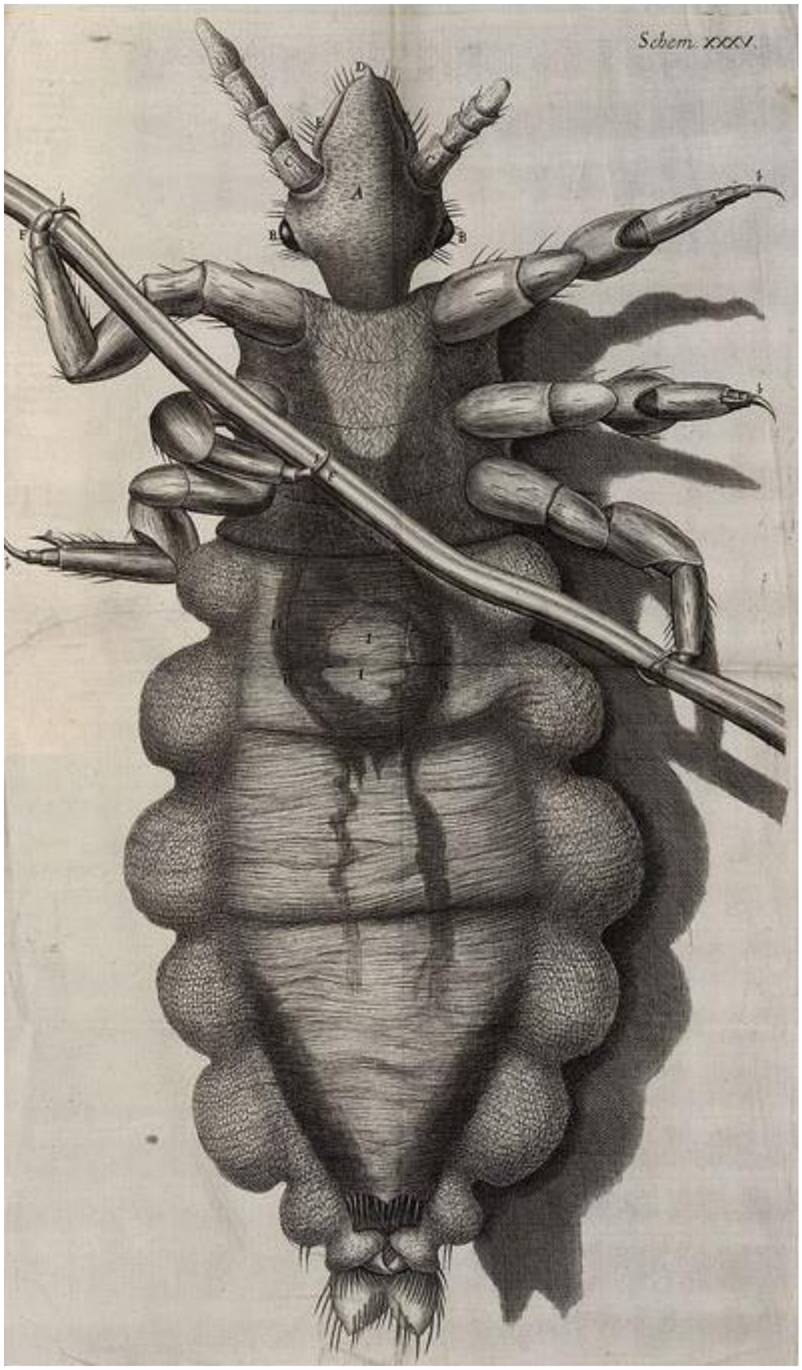

Figure 2. Robert Hooke's louse. (*Micrographia,* 1961, fold-out, end-papers).

Under the microscope, Hooke notices

> that it is a creature of very odd shape; it has a head like that expressed in the 35^{th} scheme, marked with A, which seems almost conical, but is a little flatted on the upper and undersides, at the biggest part of which, on either side behind the head (as it were, being the place where other creatures' ears stand) are placed its two black shining goggle eyes BB, looking backwards, and fenced around with several small cilia or hairs that encompass it, so that it seems this creature has no very good foresight. It does not seem to have any eyelids, and therefore perhaps its eyes were so placed that it might the better cleanse them with its fore-legs; and perhaps this may be the reason why they so much avoid and run from the light behind them, for being made to live in the shady and dark recesses of the hair, and hence probably their eyes having a great aperture, the open and clear light, especially that of the sun, must needs very much offend them (Hooke, 1961, 211).

Both passages have in common that Cicero's criterion for *enargia* that "these things, which you have not seen with your eyes, you can see with your mind" (Cicero, quoted in Quintilian 2001, 57). In *Ad Herennium*, you are a by-stander to a scene from the past; in Hooke, you and he together are peering through the same microscope. But there the parallelism ends. *Ad Herennium* is deliberately engaging, dramatic, selective, its unwavering goal a vividness that evokes pity, terror, and indignation. Not so Hooke, who does not merely describe; he over-describes; he is not simply undramatic; he is anti-dramatic. In short, Harwood has made a category mistake. *Enargia* does not belong in a work of science. The job of scientific prose is exhaustively to describe the anatomy of the louse, to see this creature under a particular description: the louse as a living machine. Hooke's prose is designed to be transparent: readers are meant to see through it verbiage directly to the object of study. Prose that called attention to itself would only distract from this purpose.

There is another significant difference. *Ad Herennium* is dramatizing the behavior of Gracchus and his assassins; Hooke is not describing *a* louse, but *the* louse, not an individual, but a species.

At this point, a rhetorician might object: Haven't I shown that Hooke's prose is in its own way just as rhetorical as Cicero's? And isn't its transparency just an illusion this prose is designed to foster? My claim is the target of these objections only because I have so far ignored the most important “words” in Hooke’s passage: “A” and “B.” These majuscule letters are deictic: they point to the object of study. Reading Hooke’s description in conjunction with his engraving we are not merely reading; we are engaging in a virtual practice. Images are central to *Micrographia* because they participate in a system that re-creates the practice exemplified by each of Hooke’s observations. It is not, as Steven Shapin and Simon Schaffer famously assert, that we are virtual witnesses; rather, we are virtual participants in a practice Hooke performs and advocates (Shapin and Schaffer, 1985, 60–61).

What is a practice? It is not an example of knowing what, but of knowing how and of doing accordingly. Knowing what is never sufficient for a practice. Also, a practice is not like a habit, routinely reiterated; rather, it is a succession of actions oriented toward a consciously held goal, actions shaped and re-shaped by experience, and superintended always by intelligence. Mountaineering is a practice:

> A mountaineer walking over ice-covered rocks in a high wind in the dark does not move his limbs by blind habit; he thinks what he is doing, he is ready for emergencies, he economizes in effort, he makes tests and experiments; in short he walks with some degree of skill and judgment. If he makes a mistake, he is inclined not to repeat it, and if he finds a new trick effective he is inclined to continue to use it and to improve upon it. He is concomitantly walking and teaching himself to walk in conditions of this sort. It is of the essence of habitual practices that one performance is a replica of its predecessors. It is of the essence of intelligent practices that one performance is modified by its predecessors. The agent is still learning (Ryle, 1949, 42).

Hooke’s practice of microscopy is exactly parallel:

> Of these kind of objects there is much more difficulty to discover the true shape than of those visible to the naked eye, the same object seeming quite differing in one position to the light from what it really is and may be discovered in another. And therefore I never begin to make any draft [drawing] before by many examinations in several lights and several positions to those lights, I have discovered the true form. For it is exceeding difficult in some objects to distinguish between a prominence and a depression, between a shadow and a

black stain, or a reflection and a whiteness in the color (Hooke, 1961, xxviii).

This is intelligent seeing. It is only such a practice, as philosopher Ian Hacking says, that “creates the ability to distinguish between visible artifacts of the preparation and the instrument, and the real structure that is seen with the microscope” (Hacking, 1983, 191).

Although Hooke’s engravings are an important precursor of the images that appear in today’s scientific journals, they fail the test of complete transparency. As science progresses, so does its communication. Hooke tells us that the engravings are largely accurate representations of his drawings (Hooke, 1961, xviii). But scientific illustrations should do more than that; they should reveal not only what Hooke sees but what he can legitimately infer about the structures the microscope reveals. Scientific images should be about both sight and insight. But Hooke’s engravers employ tricks of shading that create the illusion of three-dimensionality. This illusion is, from a scientific point of view, a distraction. Compare Figure 3, a recent depiction of the louse’s left antenna with its depiction in Figure 2. In Figure 3, all the parts are labelled, and no visual distractions are evident. Comparing Figures 2 and 3, we see the result of several centuries of evolution in scientific communication. Figure 3 places some aspects of louse anatomy, at last, beyond argument.

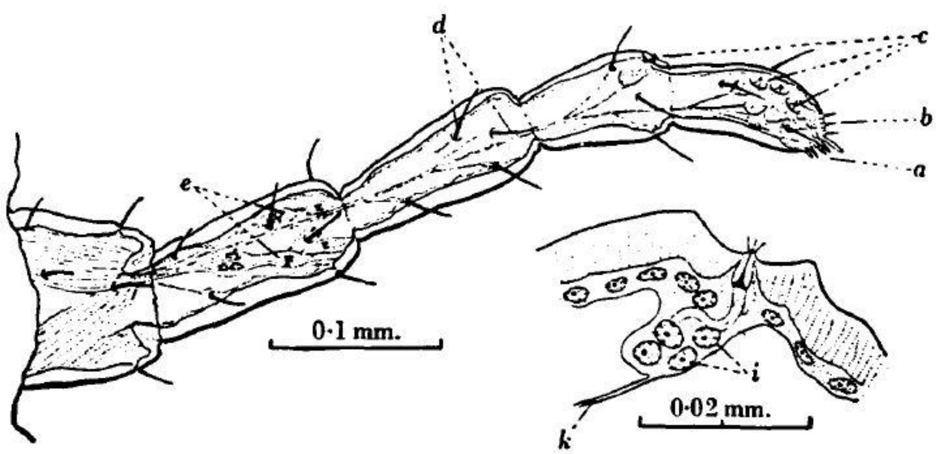

Fig. 1. Dorsal view of left antenna of *Pediculus humanus corporis* as seen in the light microscope; (a, b) peg organs on antennal tip with presumed olfactory function; (c) tuft organs with presumed hygroreceptor function; (d) tactile hairs; (e) scolopidial organs. Inset shows tuft organ at higher magnification. (i) Sensory cells; (k) axons (from Wigglesworth, 1941, with kind permission of the author).

Figure 3. Dorsal view of a louse left antenna. Labelled c in Figure 1 (Steinbrecht, 1994, 260).

What it is to be a virtual participant in this interaction between words and images? Some analytical apparatus borrowed from Alan

Gross and Joseph Harmon's *Science from Sight to Insight* will allow me to address the question (Gross and Harmon, 2014). Gross and Harmon make use of five principles of Gestalt theory. Figure and ground allow us to differentiate the louse from its shadow; good continuation allows us to experience the human hair to which the louse clings as a single entity; contrast allows us to differentiate the human hair from the louse that clings to it; enclosure allows us to experience individually each of the jointed parts of the louse's body; *Prägnatz* allows us to experience these parts as segments of a whole.

Gross and Harmon also employ scanning and matching theory. It is scanning and matching that allows us to move meaningfully from text to image and back, allows us to see under a certain description. For example, we see the part of the image that AA designates as "a head … which seems almost conical, but is a little flatted on the upper and underside" (Hooke, 1961, 211). Finally, Gross and Harmon employ Peircian semiotics. Semiotics allows us to track the transformations Hooke's louse undergoes. The louse is iconic: Hooke depicts the creature with all the accuracy of which he is capable. Hooke's image is also indexical: he shows us, not only how the louse looks, but how its various components work together to create a way of life. Hooke's image is finally symbolic: one louse represents all lice. It is symbolic in another sense; it stands for the scope and power of microscopy.

In *Micrographia's* "Preface," Hooke invites his readers to practice microscopy, for "gentlemen of our nation, whose leisure makes them fit to undertake, and the plenty of their fortunes to accomplish, extraordinary things in this way" (Hooke, 1961, xix). Hooke is saying that the practice of microscopy can be learned. When it is, it will unequivocally demonstrate the truth of his claims: not only does the louse look exactly as he depicts it; it functions exactly as he describes it. It is a mechanism, a living machine. Microscopy moves us decisively "towards the increase of the operative and mechanical knowledge to which the age seems so much inclined, because we may perhaps be enabled to discern all the secret workings of nature, almost in the same manner as we do those that are the productions of art, and are managed by wheels, and engines, and springs that were devised by human wit" (Hooke, 1961, viii).

We have so far have treated *Micrographia* as a gallery of images accompanied by their descriptions, theory-free except for the implication that living things are, at bottom, living machines. The full title of the book seems to support this view: *Micrographia, or Some Physiological Descriptions of Minute Bodies Made by*

Magnifying Glasses with Observations and Inquiries Thereupon. So, apparently, does the organization that moves from the man-made to the natural and in nature from plants to insects. Within categories, moreover, there is no principle of order, a lack of organization that reinforces the idea that Hooke is neither a proto-botanist nor a proto-entomologist; he simply chose what was ready at hand to illustrate the power and range of magnifying lenses in describing the otherwise invisible characteristics of the micro-world. Observations 9, 10, and 68, however, force us to seriously modify this interpretation. All three sections analyze the nature of light, the theoretical subject of *Micrographia*. This contention should not surprise. Better microscopes are contingent, not only on improvements in lens manufacture, but on advances in our understanding of light, both as it passes through a lens, and more generally. *Micrographia* is not only about observations, but about observation.

In Figure 4, the behavior of light is analyzed theoretically. For Hooke, light is a rapid vibratory pulse proceeding outward from the illuminating object, much as ripples expand outward when a stone is dropped in water. When this pulse hits the cornea, a lens, it refracts: the rays that hit the retina at D register blue, while those that hit the retina at F register as red. For Hooke blue and red are the only two colors; all others are derived from these. We have here an example of geometry employed in the service of theory.

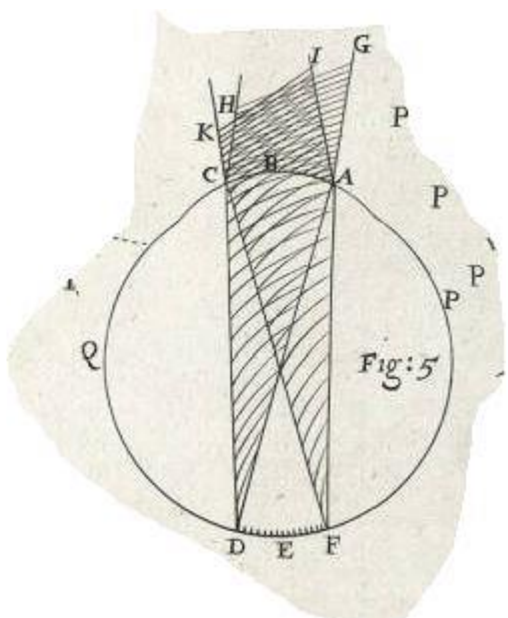

Figure 4. Light rays hit the retina. (Hooke, *Micrographia*, 1961, between pages 60–61).

The corneal lens can distort: the myopic suffer from this problem. Since the atmosphere is also a lens, it can also distort, an effect that can undermine the accuracy of observations. To make his case, Hooke experiments, using the apparatus depicted in Figure 5. To drive out the air in the glass bubble, he vaporizes water. Then he seals the now airless bubble. When he peers through the peep-hole at A, he sees the whole object C, from F to G (below I but missing in the diagram). When Hooke unseals the bubble, letting in the air, he can see only a segment HI. From this experiment, Hooke draws the following conclusion: since it is "more likely that there is a continual increase of rarity in the parts of the air the further they are removed from the surface of the earth, it will hence necessarily follow that … the ray of light passing obliquely through the air also, which is of very different density, will be continually and infinitely inflected or bended from a straight or direct motion" (1961, 228). In setting up the experiment, Hooke has created a model of the atmosphere, a stroke of experimental ingenuity: he has "consider[ed] the atmosphere as a transparent shell encompassing an opacious globe [the earth]" (Hooke, 1961, 230).

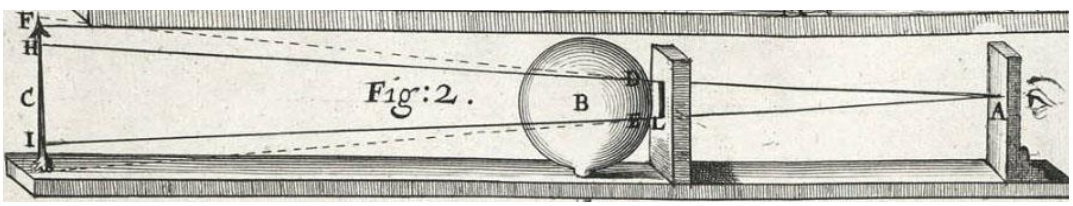

Figure 5. An experiment showing that the atmosphere is a distorting lens. (Hooke, *Micrographia,* between pages 220 and 221).

Hooke's observations can be seriously off-base. He makes three mistakes in his description of the louse: he gets its feeding mechanism wrong, identifies a non-existent liver or pancreas, and sees an imaginary filling of veins and arteries as the creature feeds (Jervis, 2013, 2557–58). Hooke's theories are also subject to error. Light is not a pulse or a ray; it consists of photons, massless particles. It has always been the case—it is in fact still the case—that much of science, though ever so carefully argued into place, has been over time shown to be in error. "Overwhelmingly," notes physicist and philosopher Arthur Fine, "the results of the conscientious pursuit of scientific inquiry are failures: failed theories, failed hypotheses, failed conjectures, inaccurate measurements, incorrect estimations of parameters, fallacious causal inferences, and so forth". What requires explaining, he avers, is why “the very same methods” occasionally produce “a pattern of successes” (Fine, 1986, 119).

That they do on occasion succeed, however, is beyond question; equally beyond question now are the nature of light and louse anatomy. Observation, experiment, and theory have over time led to conclusions that placed them beyond argument. Sometimes, as with the discovery of X-rays, they were immediately so. But hardly ever is acceptance immediate, a fact that accounts for the frequent long delay in Nobel awards. In his mordant and malicious *Postmodern Pooh,* Frederick Crews satirizes rhetorical and literary critics who presume to analyze science:

> You may have believed, for instance, that the DNA molecule possesses a certain eternal structure that was definitively revealed by Watson and Crick. But as science-as-literature specialist Alan G. Gross has shown, “the sense that a molecule of this structure exists at all, the sense of its reality, is an effect only of words, numbers, and pictures judiciously used with persuasive effect (Crews, 2001, 171).

If Gross is asserting that the structure of DNA is not now beyond question, the barb is well deserved. But it is not true that Watson and Crick “definitively revealed” anything whatever in their initial paper. Certainly *they* did not assert that their conclusion was beyond argument. But if it was not beyond argument in 1953 when the structure was announced, it was beyond argument in 1962 when their Nobel Prize was awarded.

We now turn to those aspects of *Micrographia* that can legitimately be called rhetorical, aspects that can never be beyond argument. To do so, we must first question Jordynn Jack’s view that Hooke provides us with a “pedagogy of sight,” that he “provided amateur microscopists with a guidebook they could use to help them interpret what they saw” (Jack, 1985, 195). While it is true that Hooke opens the door to the wide dissemination of the microscopic skills he practices, in fact, in the two decades following the publication of *Micrographia,* no such network emerged. The only microscope studies that appeared in the *Transactions* were by produced Anton van Leeuwenhoek and Edward Tyson, both distinguished scientists and members of the Royal Society. This is a period that includes a time when Hooke was the editor of the journal (or rather its substitute when the journal temporarily ceased publication).

This failure to teach should come as no surprise. Until the end of the 19th century, microscopes were unreliable; it was an open question whether they were tools or toys. Indeed, in 1800, the great anatomist, Xavier Bichat, forbade microscopes in his laboratory. Ian Hacking tells us that "even up to the 1860s there were serious debates as to whether globules seen through a microscope were artifacts of the instrument or genuine elements of living material. (They were artifacts)" (Hacking, 1983, 194). Moreover, since most living material is transparent, it was not until after the 1860s that anything much was seen, as it was not until then that aniline dyes were available. For the ladies and gentlemen of the 17th and 18th centuries, microscopes were toys accompanied by boxes of ready-made slides. Without them, they would have seen nothing whatever. Even today, the idea is laughable that someone untrained can place a drop of pond water on a slide and see little creatures squirming about (Hacking, 1983, 192).

There is second reason for the absence of a network of collaborating microscopists. *Micrographia* is not a manual at all; it is a different genre altogether. A proper contrast is with Agricola's *De Re Metallica,* a true manual that explains the mining, smelting, and working of metals so expertly that it was translated into German and Italian, and reigned supreme for 180 years as *the* guide to the mining and processing of metals (Agricola, 1912). *De Re Metallica* is a detailed description of a complex practice, accompanied by equally detailed explanatory woodcuts. Here is an example of Agricola's meticulous prose along with Figure 6, its accompanying illustration:

> The metal which is dug out in a pure or crude state, to which class belong native silver, silver glance, and gray silver, is placed on a stone by the mine foreman and flattened out by pounding with heavy square hammers, These masses, when they have thus been flattened out like plates, are placed either on the stump of a tree, and cut into pieces by pounding an iron chisel into them with a hammer, or else they are cut with an iron tool similar to a pair of shears. One blade of these shears is three feet long, and is firmly fixed to a stump, and the other blade which cuts the metal is six feet long (Agricola, 1912, 269).

Figure 6. Metal processing (Agricola, *De Re Metallica,* 1912, 269).

Even this example does not fully exemplify the difficulty of communicating a complex practice. Manuals are never sufficient for transfer. Toys 'R Us is a firm in the process of transferring its accounting department from America to India. No manual was sufficient; instead, a young woman from India shadowed an accountant, recording every keystroke, taking screen shots of her computer, accompanied by detailed notes of every aspect of her behavior:

> "She just pulled up a chair in front of my computer," said the accountant, 49, who had worked for the company for than 15 years. "She shadowed me everywhere, even to the ladies' room."

Another young lady from India made a digital recording of another accountant's work day, transmitted to India, where others mimicked the accountant's tasks (Preston, 2015, A1).

Micrographia is two things at once: a work of science and a work of deliberative rhetoric, a genre designed to exhort its readers to support the aims of the newly-founded Royal Society because those aims strengthen our conviction that the world is God's intricate handiwork. In *Micrographia,* character persuades: Hooke is its modest hero, an imagined Hooke careful to stress his limits as a scientist but equally careful to point out that those limits need not undermine the value of his endeavors:

> I here present to the world my imperfect endeavors, which though they shall prove no other way considerable, yet I hope they may be in some measure useful to the main design of a reformation in philosophy [that is, science], if it be only by showing that there is not so much required towards it any strength of imagination or exactness of method or depth of contemplation (though the addition of these, where they can be had must needs produce a much more perfect composure) as a sincere hand and a faithful eye to examine and to record the things themselves as they appear (Hooke, 1961, viii).

It is no accident that in this passage Hooke resorts to two devices of style, twin verbal turns that underline his devotion to careful manipulation and visual fidelity, the two pillars on which microscopy must rest if it is to accurately track the contours of God's creation, the natural world.

The organization of *Micrographia* is equally a means of persuasion. The book begins with human artifacts such as razors and pins. Despite their apparent perfection, these are shown to be imperfect under the microscope. These are contrasted with plants and insects, whose structures reveal God the Designer:

> Nature does not only work mechanically, but by such excellent and most stupendous contrivances that it were impossible for all the reason in the world to find out any contrivance to do the same thing that should have more convenient properties. And can any be so sottish as to think all those things the products of chance? (Hooke, 1961, 171–72)

This revelation of God's handiwork generates awe, a persuasive emotion triggered by the vastness of His enterprise and by its complexity, a vastness and complexity only the microscope reveals. This exemplification of His power overwhelms us, forcing us to realize that science is itself a kind of worship (Keltner and Haidt, 2003; Piff *et al.*, 2015). Hooke finds these living mechanisms not only intricate, but beautiful, an aesthetic appeal that is equally persuasive: "And indeed, you can hardly look at the scales of any fish, but you may discover abundance of curiosity and beautifying;

and not only in these fishes, but in the shells and crusts or armor of most sorts of marine animals so invested" (Hooke, 1961, 163). This feeling applies not to the quality of the image, as in fine art, but to the plant or animal being depicted. It is a beauty God has created.

Hooke's mechanical philosophy is a heuristic; the assertion that it is probative is rhetorical. The view that God is a master craftsman cannot be sustained in the face of numerous defects in human anatomy. The existence for example of the vermiform appendix and of wisdom teeth are examples. Neither can the reduction of life to its mechanical expression be sustained. Philosopher Thomas Nagel explains:

> The inescapable fact that has to be accommodated in any complete conception of the universe is that the appearance of living organisms has eventually given rise to consciousness, desire, action, and the formation of both beliefs and intentions on the basis of reasons. . . . This is not just anthropocentric triumphalism. The entire animal kingdom, the endless generations of insects and spiders in their enormous, extravagant populations, all pose this same question about the order of nature (Nagel, 2012, 32).

It seems unarguable that such characteristics cannot the consequence of the operation of "springs" and" wheels." But if this is so, there must be "a cosmic predisposition to the formation of life, consciousness, and the value that is inseparable from them" (Nagel, 2012, 123). This conclusion does not negate naturalism, but it does show materialism can only be part of any explanation of who we and our fellow creatures are. The mechanical philosophy nonetheless remains a powerful heuristic in biology, one that, as Figure 3 clearly shows, has maintained its strength to this day.

Micrographia is one of a triumvirate of publications the Royal Society generated, each with deliberative intent. In its fifth year, the Society produced, not only *Micrographia,* but the first issue of *Philosophical Transactions*. While the content of the *Transactions* was not of course deliberative, the act of publication was an affirmation that science mattered, both in Restoration England and in the learned world at large. The Society's seventh year saw its third deliberative publication, Thomas Sprat's *History of the Royal Society*. While Hooke focused on the activities of the Society, the pursuit of knowledge of the natural world, Sprat trained his attention on the social and political context that made this pursuit desirable. This three-pronged rhetorical assault was successful: It established the Society as the robust rival of the soon to be established French *Académie des Sciences*.

Global Warming: The Rhetoric of Science Policy

Weather is about whether it is going to rain tomorrow; climate is about the average temperature a thousand years ago and a hundred years from now. Climate is a tougher nut to crack. Accurate records may go back a couple of hundred years, but only in some places, hardly everywhere. Beyond that period, proxies like tree rings must suffice. All of this data, however, no matter how accurate, is meaningless by itself. However accurate, this data takes on meaning only when inserted in a computer model. P.N. Edwards puts it well in *The Vast Machine:* climate science is "models almost all the way down. In this very important sense, comprehensive model building is a central practice of global knowledge infrastructures" (Edwards, 2013, 421). But model builders must perform a "difficult balancing act" (Edwards, 2013, 175). If they want their model to have better resolution, they must sacrifice complexity; if they want more complexity, they must sacrifice resolution. Given the tsunami of data that must be fed into any model, it is no wonder that "tiny errors that occur with each rounding can accumulate to a problematic degree" (Edwards, 2013, 175). Moreover, even if global warming happens to be real, it is not necessarily anthropogenic. To determine whether there is a human fingerprint amid the noise of other factors, you must

> calculate the amount of "noise" produced by natural variability in the control runs [of the model]. If your fingerprint signal remains after subtracting this noise, you've found a candidate for a *unique* anthropogenic effect, one that could not be caused by any known combination of natural events. Then you check the observational data. If you see the same fingerprint there, you've found some evidence of anthropogenic change (Edwards, 2013, 335).

These difficulties are reflected in the history of climate science. In 1970, an article in *Science* stated that "Our estimates of CO_2 production by natural causes, such as volcanic exhalations and organic decay are very inaccurate; hence the ratio of these natural effects to anthropogenic effects remains to be established" (Landsberg, 1970, 1268). Four years later, in another article uncertainty still prevailed: "To know whether the observed temperature change at the earth's surface was the result of changes in the greenhouse effect . . . is probably beyond our present capability of measurement" (Bryson, 1974, 756). In 1988, *Science* still reported serious measurement problems (Kerr, 1988a; 1988b). In 1990, Philip Abelson, then *Science* Deputy Editor, expressed continuing doubts. By 1995, however, with the first report of The Intergovernmental Panel on Climate Change, the tide had turned. By that time, “the balance of evidence suggest[ed] a discernable human influence on global climate” (IGPCC, 1995, 22). By the 2001 report, there was “new and stronger evidence” for a human contribution (IGPCC, 2001, 5). By the 2007 report, anthropocentric global warming could be asserted with “*very high confidence;”* it was the consensus of 90% of the scientists (IGPCC, 2007, 37). By the 2014 report, there was high confidence that “about half of the anthropogenic cumulative CO_2 emissions between 1750 and 2010 occurred in the last 40 years” (IGPCC, 2010, 7). For a certainty high enough to make action advisable, the last report was crucial. In 2007, *American Scientist* called for action. But *Scientific American* published its call for action as late as 2009. While today models of anthropogenic global warming converge on a consensus permanently beyond argument, in 2007, legitimate contention was still possible; anthropogenic global warming was still not beyond argument. The “contrarian” Steve McIntyre is a case in point. Rhetorical analysis was still appropriate.

The first of McIntyre’s interventions concerns a “Millennial Temperature Reconstruction Intercomparison and Evaluation,” an article that eventually appeared in the well-respected specialty journal, *Climate of the Past* (Juckes *et al.*, 2007; McIntyre, 2006). In a comment during open peer review, McIntyre finds the paper scientifically and morally defective, establishing its academic credibility on false pretenses. The parallel syntax, the use of quotation marks to convey irony, and the metaphor from bank fraud hint at the rhetorical feast that awaits anyone who examines online peer review documents.

> Juckes *et al*. have already withdrawn a false allegation that we had failed to archive our source code and, after the above admission, should also have withdrawn these further false allegations concerning supposed “errors.” In making these allegations, Juckes *et al*. also perpetuated prior “academic check-kiting” by Wahl and Ammann (Juckes *et al*., 2007).

Of the paper itself, McIntyre is dismissive: “Given the already crowded controversy in this field, I see little purpose in reviving an issue in peer-reviewed literature that is not actually in controversy and which has negligible impact on any result” (McIntyre, 2006, s701–s702).

Unlike McIntyre, the corresponding author, Martin Juckes, maintains civility in his replies to criticisms. In his relatively brief responses to individual comments, he adopts an even tone that sends a message: McIntyre's accusations have not changed his convictions or disturbed his equanimity. At one point, however, Juckes does become exasperated. McIntyre ends his final commentary with a reference to his blog: "There has been extensive discussion of various aspects of Juckes *et al.* at www.climateaudit.org—see http://www.climateaudit.org/?cat=36." Juckes responds dismissively, "Extensive and ill-informed," a juxtaposition that conveys deep-seated contempt and anger (McIntyre, 2006, s702; Juckes *et al.,* 2007).

This is not to say that McIntyre's criticisms are without foundation. The question is not whether he is annoyed or angry, but whether his anger and annoyance are justified. Despite their polemical edge, his remarks contribute to the paper's considerable improvement from its first to its final draft. Juckes acknowledges and agrees to correct at least some of the errors McIntyre alleges, as in the case of their initial geographical proxy locations. Furthermore, he makes some changes based on McIntyre's criticism of proxy selection.

Beginning in 2005, McIntyre initiated a campaign to force the prestigious Goddard Institute for Space Studies to share its data so that an audit could be performed. Goddard resisted at first but a cataract of negative publicity generated by McIntyre's blog forced them to comply in 2007. Errors were found. In this case, McIntyre's

> blog provided an unprecedented forum for any interested party to signal audit-worthy issues, and Climate Audit and other blogs uncovered further errors made by GISS in an early release of October 2008 data. GISS thanked McIntyre publically for these contributions. In response to the calls of Climate Audit and other blogs for greater transparency many climate centers have begun mounting data and even some climate models on public web servers (Edwards, 2013, 424).

Here we have an interesting instance of the rhetorical *stasis* of jurisdiction. Having failed to persuade through professional channels, McIntyre creates a new forum professionals cannot control, a blog, an instance of the persuasive power the internet can unleash.

Even as late as 2014—even as late as yesterday—it was still possible legitimately to debate national and international policy, given the reality of anthropocentric global warming. John Christy is an example of a scientist who legitimately deviates from the consensus that a crisis is at hand. Christy is a respected climate scientist. His co-authored 2005 paper on the problems of climate measurement, published in *The Bulletin of the American Meteorological Association,* the most prestigious journal in climate science, was cited a more than respectable 83 times (Thorne *et al.*, 2005). A 2011 review article on climate measurement, itself cited 58 times cites *31* articles authored or co-authored by Christy on the subject (Thorne *et al.,* 2011). In 2007, Christy acknowledges that global warming is real and that there is a definite human footprint; in 2009 he deplores the politicization of the issue, whether it comes from those he feels push the IPCC in the direction of advocacy or those whose skepticism is merely ideological (Revkin, 2007; 2009a; 2009b; Rudolf, 2011). While in 2014 he concurs that anthropogenic global warming is real, he disapproves of drastic measures designed to curb carbon emissions, believing that the disruptions they cause cannot justify their implementation:

> Dr. Christy argues that reining in carbon emissions is both futile and unnecessary, and that money is better spent adapting to what he says will be moderately higher temperatures. Among other initiatives, he said, the authorities could limit development in coastal and hurricane-prone areas, expand flood plains, make manufactured housing more resistant to tornadoes and high winds, and make farms in arid regions less dependent on imported water — or move production to rainier places (Wines, 2014; see also Revkin, 2005).

While this view can be contested—while it is in fact contested most vigorously—it is also the view of David Archer, a professor of geophysical sciences at the University of Chicago:

> A century of global warming in the United States will probably involve uncomfortable summers, maybe some drought, and colorful headlines about hurricanes. Holland is an example of a prosperous country that has managed to build and maintain dikes, to keep the sea out of their low-lying landscape. Sea level rise in Bangladesh is less likely to be defended against, because of the length of the coastline, and the lack of economic resources to throw at the problem (Archer, 2009, 53).

While the fact of anthropogenic global warming is no longer in question, the public policies that follow from that fact remain a target for rhetorical analysis; nations must be persuaded that action on global warming trumps other priorities. Moreover, scientists are involved in these debates, not only as scientists, but as advocates. Rhetorical analysis is always appropriate in both of these cases.

Conclusion: The Humanities

Even when we leave the sciences behind, we do not leave behind matters that are permanently beyond argument. Sharing this status are John Austin's discovery of speech acts and Paul Grice's, of conversation implicatures. Also beyond argument is David Hume's case against induction:

> From a body of like color and consistency with bread, we expect like nourishment and support. But this surely is a step or progress of the mind which wants to be explained. When a man says, *I have formed, in all past instances, such sensible qualities, conjoined with such secret powers,* and when he says, *similar sensible qualities will always be conjoined with similar secret powers,* he is not guilty of a tautology, nor are these propositions in any respect the same. You say that the one proposition is an inference from the other; but you must confess that the inference is not intuitive, neither is it demonstrative. Of what nature is it then? To say it is experimental is begging the question. For all inferences from experience suppose, as their foundation, that the future will resemble the past and that similar powers will be conjoined with similar sensible qualities. If there be any suspicion that the course of nature may change, and that the past may be no guide for the future, all experience becomes useless and can give rise to no inference or conclusion. It is impossible, therefore, that any arguments from experience can prove this resemblance of the past to the future, since all these arguments are founded on the supposition of that resemblance (Hume, 1955, 51; his emphasis).

None of these instances of claims beyond argument undermines the role rhetoric can play in the analysis of issues of policy generated by scientific claims, however well founded, nor its role in the analysis of scientific claims still in contention, nor its role in analyzing articles and books in which scientific and rhetorical claims exist side by side.

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