Cutting Edges Cut Both Ways

JANE MAIENSCHEIN

Department of Philosophy
Arizona State University
Tempe, Arizona 85287
U.S.A.

ABSTRACT: Emphasis on "cutting edge" science is common today. This paper shows that the concept, which selects some science at any given time as epistemically preferable and therefore "better," actually gained acceptance by the turn of this century in biology and began immediately to have consequences for what biological research was done. The result, that some research is cut out while other work is privileged, can have pernicious results. Some of what is designated as not cutting edge may, in a different – and equally defensible epistemological framework, prove just as "good" as the officially cutting edge research. Cutting edges cut both ways, and those who study science should begin exploring the implications of that fact.

KEY WORDS: Cutting edge, epistemological/epistemic, experimentation, embryology, cytology, Morgan, Harrison.

The "Cutting Edge." This news headline tells us that what follows is the very best, incorporating the most current research. Usually applied to technological or scientific advances, the designation clearly sets off the described item as at the vanguard, in the forefront, leading the way. It is exciting and promises products. If you want to back a winner, the message urges, back this.

The idea of "cutting edge" actually incorporates two types of edges: cutting edges and edges to be cut. To do the cutting, it is necessary to have an effective cutting tool, presumably to cut through human ignorance. Yet just a tool alone, sharp though its edge may be, is not effective if wielded without purpose or direction. Therefore, a second type of edge comes into play: the edge to be cut, which should presumably be a particularly promising or sectile[1] edge. The ambiguity offered by capturing these two types of edge with the same term can be used to favor either the tool or the target. Both deserve special attention and, presumably, more resources according to the cutting edge view of science. The assumption is that the sharp edge of the tool applied to the sectile edge will cut away ignorance, make discoveries, and produce useful results, in effect piling up the accumulating facts and discoveries behind it as it goes. The growing body of knowledge produced demonstrates progress, according to this view, and justifies the selection of a particular area of scientific inquiry over others.

Any set of standards obviously makes distinctions such that some things fall on the acceptable side and others do not. Most such sets of standards probably emerge gradually and in part unintentionally. Standards gain widespread acceptance and then take on the status of being somehow "right" even though

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initially there could have been equally acceptable alternatives. They are not arbitrary, nor are they socially constructed in any simple and direct sense. Neither do they straightforwardly follow the internal logic of the scientific ideas. Rather, the selection results from conventions and from changing epistemological standards. A complex of logical, rational, social, cultural, and institutional factors causes the acceptance as well as the emergence of those standards.

The actual term "cutting edge" seems to have gained widespread use only after the 1950s, but the idea behind the term has been operative for at least a century in biology and is tied to definitions of what should count as good science. Undoubtedly, the demands of the federal funding system in recent decades have reinforced and extended the practice of pointing to one area or another as the cutting edge at any given time. Competition to achieve the designation cutting edge (or its functional equivalent) has also increased because of funding pressures and other award structures. Yet changes within science at the turn of this century brought a tendency for biological researchers to represent their own work as particularly progressive and worth doing. The move followed similar moves in physics and chemistry, for example. Around 1900 the biological sciences articulated a set of epistemic standards (for what would count as legitimate, established knowledge) which selected some lines of research as especially promising. Once the norms were established, they were applied in sometimes unexpected ways with sometimes undesirable results. Even some of the work contributing to the establishment of the norms itself was selected against by the same norms.

This paper discusses the establishment of an experimental approach as the proper cutting tool for biology. Then it examines the move to accept selected specialized lines of research as especially productive and hence as at an edge, deserving support. This occurred first among embryologists and cytologists around 1900 and was extended and reinforced later. Such an epistemological standard for biology as a whole has further implications, which are also explored. This analysis suggests that continued endorsement of the standard, and persistent enthusiasm for identifying and differentially supporting cutting edge research at the expense of other work has costs as well as advantages. It is therefore worth considering carefully how this set of standards has arisen and what effects it has had. This paper begins that exploration.

THE EXPERIMENTAL APPROACH AS CUTTING EDGE

With the Scientific Revolution of the 17th century, science was clearly regarded as a good thing. All science was good. Would-be natural philosophers should go out to experience and collect and explain some aspect of nature. Together the community would produce useful knowledge, in Baconian terms, or would achieve understanding, in Cartesian terms, about all of nature. Science was better than non-science for the advocates, but in theory no one part of science was intrinsically better than any other.

By the 19th century, when the historicists had their day, the most popular views recognized that science evolves and, indeed, improves. Condorcet, for example, pointed to the prospects for progress through history, and Comte and others stressed progress specifically through the history of science. While insisting on his own originality, Whewell perhaps typified the 19th century position with his insistence on the progress of science through the process of employing induction. Science, he assumed, proceeds by process. This view fit nicely with the historicist bent of Goethe, Marx, Darwin, and other leading 19th century thinkers about science.

Within such contexts, the concept of working with only one selected tool at a narrowly defined and limited edge did not make sense. Science as process for these men involved bringing together a variety of advances, discoveries, and new theories all under a larger accepted framework. The idea was to understand all of nature and the evolving interpretations of it, even though some pieces might come earlier than others because of a particular inductive breakthrough. The emphasis remained on the whole body of scientific knowledge rather than focused on any selected edge. The view of the time would suggest, for example, that observers cannot fully understand the whole phenomenon of something like an active volcano by looking just at the advancing edge. Instead, it is necessary to recognize the whole active system which may soon produce new edges. Similarly, science should look at more than just the selected advancing edge, even when it might seem for the moment, and from one perspective, that one approach is preferable.

The work in biology to which contemporaries pointed as especially important reveals this emphasis. Darwin's theory of the origin of species by the means of natural selection offers an obvious example: all species are included, and all nature fits in with the theory. Though some might look back and see Darwin's theory as providing something like a cutting edge tool at the time, since evolution studies gained immediate popularity, the idea of a selected, preferred edge to be cut would not have made sense to Darwin. He certainly would have been horrified to think that his success suggested that others who wanted to be in the elite in science should rush to address just the same questions or examples he did. Instead, researchers should be studying, for example, the wide variety of different organisms or details of physics on which his own study drew, or they should be pursuing other, related problems. Darwin himself offered an "umbrella theory," which could serve as a cover under which many other facts and interpretations would fall. He knew that some ways of doing science were "better" than others, but he did not demand adherence to a single epistemic norm or expect researchers to cluster at defined edges. Instead, he seems to have remained a pluralist, encouraging a variety of approaches and questions to cooperate within the vast realm of legitimate science.[2]

While various biologists by the first half of the nineteenth century felt that they could distinguish good from bad science, the move to thinking in terms of sharp edges came later. It was materialistic reductionists such as Wilhelm Roux who brought biological science closer to thinking in terms of edges to do the cutting away of ignorance as well as in terms of selected edges to be cut. Roux was outspoken and demanding, insisting that there could be no question but that there was a single right way to do science. He felt, of course, that he knew what it was and most people did not. His view privileged some scientific research over others and made it easier to identify which work was more important: namely, that which conformed to his standards and adopted his methods and approach.

Roux's standards were, at root, epistemological. Though he insisted on a materialistic metaphysic, he assumed it and did not even consider the possibility that nature might be anything but a Newtonian sort of matter in motion, on which change is effected by forces. The goal of science, according to Roux, is to discover the causal explanations of phenomena by uncovering the operative natural laws. Since this can only rarely be done directly, through passive observation, the search requires interventionist experimentation. Experiments will produce facts and allow selection of the best hypotheses, those that "explain the most facts and permit of the successful prediction of new facts" (Roux 1895, pp. 115, 126, 124). Experiments allow the researcher in effect to see "inside" the processes, and only experimentation can yield certainty and (eventually) truth.

Roux's own studies focused on embryology. He wanted to explain the formative processes which allow an apparently unformed egg to become a fully differentiated and functioning adult – and an adult of the right sort at that. For Roux, not everyone had to work on embryology, however. In his introductory essay to the first volume of his journal *Wilhelm Roux's Archiv für Entwickelungsmechanik der Organismen*, he explained that other branches of biology which are properly analytical and experimental could be good science and eligible for publication. These branches were the progressive, the leading sciences. Insofar as studies contributed to achieving causal explanations, they were admissible. Merely descriptive papers were not. It happened that experimental embryology was a particularly productive area at the time and would provide a special focus in the journal. Experimental embryology was a particularly sectile edge, he would have agreed, but only for the moment.

What emerges from Roux's manifesto is the insistence that science must be materialistic, reductionistic, and experimental to be good science. Some work will simply be better than other work. Some will advance science and achieve progress, while another line of research will not. The implication was that the good, progressive science would be accepted in his, the leading journal, while other work would not. This good science would, in later terms, do the cutting through ignorance.

This is not to say that Roux was unique or, indeed, even original in this view. In fact, he was following a path laid in France a few decades earlier which, in turn, echoed even earlier convictions in Germany. In the 1840s, the reductionist physiologists had articulated the view that physiology should eliminate all vitalism and embrace materialism, reductionism, and an experimental approach (Cranefield 1957). They sought, in effect, to make physiology an extension of the physical sciences. Their program did not immediately "take" for the life sciences more generally, however, nor did they explicitly argue that it should.

When the leading French physiologist Claude Bernard took up the call for experimental medicine founded on a strong experimental physiology, he also endorsed the idea that some research is better than others. Specifically, experimental work is properly scientific and non-experimental work is not. What Bernard meant by experimentation was the whole experimental context involving observation, comparison, and control within a theoretical framework seeking causal explanation. All the aspects, including passive observation, are part of the "experimental reasoning" insofar as they are used to experimental purpose. For Bernard,

> The true scientist is one whose work includes both experimental theory and experimental practice. (1) He notes a fact; (2) *a propos* of this fact, an idea is born in his mind; (3) in the light of this idea, he reasons, devises an experiment, imagines and brings to pass its material conditions; (4) from this experiment, new phenomena result which must be observed, and so on and so forth. The mind of a scientist is always placed, as it were, between two observations: one which serves as starting point for reasoning, and the other which serves as conclusion (Bernard 1865, p. 247; Holmes 1974).

The informed observation which forms the conclusion is then useful for formulating further ideas and theories. Science remains a process such that "these theories and ideas are by no means immutable truth, one must always be ready to abandon them, to alter them or to exchange them as soon as they cease to represent the truth. In a word, we must alter theory to adapt it to nature, but not nature to adapt it to theory" (Bernard 1865, p. 39).

Zoology, Bernard suggested, with its concern about taxonomy and description of phenomena, is not properly experimental (Bernard 1865, pp. 105–106). Some of the work of naturalists and that of astronomers, for example, had similar limitations and could not be fully and properly experimental. Bernard clearly implied that these areas of research must remain inferior to the foundational work on physiology, in particular, which has all the characteristics of the very best science. But insisting that there is a right way to do science, Bernard was using his experimental standard to devalue any research which could not conform.

His compatriot Henri Lacaze-Duthiers agreed with Bernard that "science can and must be experimental" (Lacaze-Duthiers 1872, p. 135). But he absolutely disagreed with Bernard's suggestion that zoology cannot conform to the highest experimental standards. It can, and it should. He insisted that though zoology, like all science, began historically with a descriptive approach, it had moved beyond that and had embraced experimentation as well. Lacaze-Duthiers also recognized some science as better than other science, but according to the degree of conformity to the general experimental epistemic standard and not influenced by the similarity to any one foundational field such as physiology. While largely agreeing with Bernard, he set up a much broader arena in which the cutting approach should operate. What should be cut out is only that science which fails to adopt what are obviously the best modern experimental standards. What is included at the edge to be cut will yield additional scientific knowledge and, as Bernard had said, move us toward the truth. The ability to succeed, then, rests on the researcher's choice of approach and not on whether he has chosen an "inferior" field. In this, Lacaze-Duthiers remained close to Roux, and together they gave a clear message to the would-be biologist: adopt an experimental approach if you want to do science and cut away at ignorance.

Yet, though they insisted that some research really is better than other work, their experimental-approach-as-cutting-tool was not very sharp; it did not cut out much. Nor was its range clearly defined. Aside from Bernard's view that physiology was more foundational, with the biomedical sciences resting on it, there was no clear recognition that some areas of inquiry or lines of research were, at a given time and place, more promising, better, or more "at the edge" than others.

The next generations of biologists began to refine that view and to identify selected areas deserving attention. The best science would work at those edges. Experimentation was given as the most useful cutting approach for biological science, but what began to matter more was what one did with the sharp tool of experimentation: experimentation to what productive end, to answer what questions, to discover what facts, to make what progress? Attention shifted from the edge doing the cutting to the area to be cut, and to the products of the cutting.

Around 1900, then, the idea of a productive leading sectile edge sharpened. Individuals and groups began to argue persuasively not just that they were conforming to a general approach which worked best, but further that they were pursuing a particularly successful line of work which was better than others – for the moment at least – in producing facts, making progress toward larger interpetations, and pointing in productive directions. Particularly in the United States, the focus shifted from theoretical explanations and "truth" and toward the pragmatic production of solid, reliable knowledge in the form of definitive facts and their interpretations. Buoyed by the opportunities afforded by increased funding and hiring opportunities, cutting at the edge gained importance. As a result, some work was cut in – into the center of attention. But cutting edges cut both ways, and other work was cut out. Some of what fell away from the edge was also important. Thus both the establishment of the norms and the wider implications for biology are important.

EMBRYOLOGY AND CYTOLOGY

Looking more closely at biology around 1900 reveals what was happening, and how and why embryology and cytology emerged as central. It demonstrates how the evolving epistemic commitments created and legitimized the special position of those areas of research. This section therefore focuses on these changes within biology after 1900 and the way that these fields subsequently slipped away from the edge. This is primarily a story about ideas and assumptions within biology, but those are obviously influenced by a wide range of factors which may originate outside of scientific research. Thus, the emergence and establishment of epistemic norms and the judgment of which areas of biology have best conformed to them is strongly directed by the social, political, economic, technical, and other factors which make up part of science.

Actually, "around 1900" is shorthand for the period of roughly 1880–1915. By 1880 several critical factors had converged to produce a professional interest in something that soon came to be called "biology" (Caron 1988). The acceptance of evolution of species provided a starting point, which led to an emphasis on development of the individual as a key to development of the species. In addition, a focus on cells as the basic unit of life reinforced an increasingly analytical and reductionistic framework for biological work. New techniques (such as improved microscopes and microtomes for serial sections), new questions (such as about the earliest stages of cell division), and new resources (in the form of new research organisms, new research organizations, and new money for medically-related life sciences) all supported research and attracted researchers, and all contributed to the conviction that biology was becoming exciting and successful. That, in turn, reinforced the pragmatist convictions that biology *should* be exciting and successful. Success was increasingly defined in terms of products and progress, especially progress in answering increasingly narrowly formulated questions and the production of generally recognized definitive facts.

This is not to say who or what caused what. The point here is that a confluence of changes in the late 19th century had produced the conviction that biology should be analytical and experimental rather than merely descriptive, as we have seen. In addition, it was increasingly felt that biology should be progressive in the sense of producing positive results even when that meant asking much smaller questions and generating very specific and limited answers. No longer was the emphasis on amassing large collections of observed data alone, nor on the creation of highly suggestive general theories that did not yet have practical utility in generating knowledge. In short, the turn of the century brought a revised set of epistemological norms for biology.

At the same time, the research emphasis changed. Evolution, as we know, was quickly accepted as a foundation for study of life after the publication of Darwin's *Origin.* This assumption suggested that it should be possible to learn about current existing organisms by studying their adaptive, evolutionary past, with the obvious problem that we have no very good way to get at that past and that such study must remain indirect. One could compare body parts and look for homologies, for example, but for learning more about what makes systems function as they do, peering at dead parts would not help much. Consequently, a host of researchers took up diverse lines of research to explore other lines of attack on difficult problems. Some worked independently, some in museums, others in anatomy programs in medical schools, a few in morphology programs in either medical schools or universities, a number in special zoological institutes, and a few worked in physiological institutes (Coleman 1977; Nyhart 1986, 1987).

These researchers disagreed on many issues, and historians have only recently begun to explore the wide range of views and approaches which appeared in the third quarter of the 19th century. What is important is that some – but not all – of these researchers emerged as victorious in the struggle for scientific existence. Some succeeded in attracting more research resources, followers, students, and sufficiently supportive public relations that they appeared to emerge at a forefront of biological research – and by successfully establishing the conventions, they remained there. By the 1880s, a few morphologists and a few physiologists had turned to the study of embryology in the name of understanding basic organic structure and function more generally. The individual embryo, they decided, would yield answers to more than just questions about evolution – or even just about embryonic development. They assumed that if they could observe the structure and function being put into place, they would better understand the adult organism as well. In addition, the focus on the individual fit nicely with the interests of the emerging German medical elite.

Technical advances in microscopy helped, for studying the details of embryonic development depends on being able to see, in effect, inside the egg. And this requires technical intervention. Consequently, historians have asked which came first: the technical advances or the egg, so to speak. It is *not* the case that it was simply the development of the apochromatic microscope, or improved microtomes for serial sections, or better stains that caused the move to developmental questions. Rather, researchers who already wanted to study individual development could make progress and could answer previously intractable questions which they already found interesting because of the existence of the improved techniques and the capacity for further improvements. The rushes of technical changes and new discoveries reinforced each other. They made study of development more visible and more apparently exciting than less active areas of research (Gilbert 1991). In the early twentieth century environment in which more researchers were competing for increasingly larger pools of resources, this ability to promise results and publications could convince institutions to put their resources where the results were. In effect, the universities and their researchers were engaged in a struggle for authority, even though many of the researchers themselves did not recognize that fact (Sapp 1987). In the struggle, the embryologists were winning.

These researchers also emphasized the central role of cells in development. The cell theory, or a theory about cellular units of life, had first emerged with Schleiden and Schwann in the 1830s, of course. But because their particular materialistic account of the nature of cell development remained problematic, it was not until after Rudolph Virchow and other well established researchers insisted that all cells come only from other cells, and thus that life – and not some mere blob of inanimate material – gives rise to life, that the cell theory become widely accepted (Coleman 1977). A host of studies of the cell then began to appear by the 1880s, and by 1990 biologists repeatedly insisted that the cell theory had joined evolution as the second great foundation for biology. Oscar and Richard Hertwig, Theodor Boveri, Edward Laurens Mark, and others led the way into study of the cell on its own terms.

Again, it became easier to carry out quite sophisticated maneuvers on the cell because of technical advances. Many of the same techniques that made it possible to study the egg (which is, it was recognized by then, just a special kind of cell) also made it possible to extend observation inside the cell. Killing, preserving, preparing, slicing, staining, and otherwise bringing the innermost cellular secrets into focus became a favorite research pastime. Again, this work was exciting. It seemed to be producing results, in the form of a plethora of facts, and promised more and more of the same. It was therefore productive. True, the work became increasingly specialized and required technical training. That was fine; that supported the suggestion that the work was important and created an aura of expertise and a bit of mystery. Any gentleman could wander about the English countryside peering at fossils in the early 19th century and, by mid-century, even gentlewomen could explore the seashore. But only the select trained few could carry out this advanced specialized study of cells and development, and those few could certify that they were making progress according to their own practical standards.

Of course, this was not all that was going on. Many people were pursuing many different lines of research, using different approaches to ask different sorts of questions. It is not only in retrospect that cytology and embryology seem especially interesting. Riding on the coattails of medicine and its promise, there was a greater enthusiasm, more resources, and an influx of younger researchers and students to such work. The experimental study of cells and development was moving very close to what might be legitimately considered as a particularly promising line of research deserving differentially higher support, in other words on a cutting edge. Yet the expressed commitment to working preferentially in select fields came only shortly later, aided by a group of young Americans who subscribed to a revised set of epistemological norms.

A REVISED EPISTEMOLOGY

It is not that these Americans were the best possible scientists in any larger objective sense, nor that they adopted the perfect view of what science should be like – if there were any such thing. Indeed, their view had some unwelcome consequences. Yet the fact remains that some science is regarded as better than other science at any given time and place, and the Americans did succeed in persuading others that their new, tighter epistemological standard was the best for the time being at least. To return to the volcano example: it is as if the researchers had become overwhelmed with trying to understand the whole and decided to pay close attention to what the advancing edge is doing. At some level this is understandable, but it misses much of what would be helpful in the long run for understanding even the action at the edge.

These men were not all entirely conscious of what they were doing, nor were their contributions and its implications entirely intentional. Rather, a group of men together introduced a definition for what good biological research should be like. In the American context, it took on a more pragmatic flavor than the European leaders had adopted. Not all Americans endorsed this standard, certainly, but the advocates had the institutional support to make their view known and to make it appear as the successful view. After all, the advocates were recognized as successful scientists. To some extent, then, the cutting edge is self-defining and reinforcing: research that is successful must be at the cutting edge since it has been successful; therefore it deserves additional support since it must be at the cutting edge. The definers then apply the identification of what lies at the cutting edge to judge journal articles, grant proposals, and other resources. Around 1900, what was at the progressive edge of good biological science was work in development and heredity, especially through a careful study of cells.

Ironically, it is precisely the set of epistemic norms that these embryologists and cytologists adopted that caused their work to be replaced in the following decade by other areas within biology. Almost by definition, it is difficult to remain on a sharp cutting edge for long. And the cuttable edge must also change, as it softens or crumbles and becomes less well defined, or it toughens and becomes intractable. Other lines of research, with their techniques, ideas, and approaches, move into the vanguard; the older work slides back – sometimes very quickly and at other times after decades of success. Perhaps it can regain its position at the edge, but only by either a change in epistemic standards or acknowledged progressive innovation.

The American story begins with Charles Otis Whitman, who did research in Germany and at the Naples Zoological Station early in his career and thereby became familiar with what the Europeans considered central issues and techniques. He then directed the Marine Biological Laboratory (MBL) and began the first biological journals in the United States. Whitman considered problems of development as central to biology, and he thought of himself as leading the way to solid biological practice in the United States. Led by Whitman, the Americans labelled their marine laboratory "biological," began such journals of the *Biological Bulletin*, and named their departments in universities "biology." In part this was inspired by T.H. Huxley and his call for a "biology" as the Americans sought actually to carry out Huxley's ideal and to combine it with the best of German techniques (Huxley 1876).

In contrast, the German researchers, who were regarded as the leaders in studying life did not call themselves "biologists." Instead they generally held allegiance to some specialty or identified themselves with medicine or with morphology or physiology. Many of the younger Germans (in contrast to Roux) did have a revised epistemology which closely paralleled that of the Americans. Yet they were much more theoretical in outlook and saw the importance of embracing a wider subject matter and approach for their research (Maienschein 1991; Harwood 1993).

To define something new that was identifiable, professional, and distinct from what had been labelled as natural history was to call for something forward-looking and different, which could establish standards for what counts as making progress in terms of producing solid and reliable results. Pragmatic overtones suggested that looking for practical results and concrete products, asking sufficiently well-defined questions and seeking definitive and incontrovertible facts, would constitute good and progressive work in biology. These biologists, and the students they trained, began to see what we can legitimately think of as sectile edges. And they began increasingly to look for the tools to cut at those edges.

Central to the story are four friends who had been graduate students at Johns Hopkins: Edmund Beecher Wilson, Thomas Hunt Morgan, Edwin Grant Conklin, Ross Granville Harrison (Maienschein 1991b). Others agreed and developed parallel views, but these four were widely recognized by 1910 as among the most prominent biologists. Each had achieved a high-status professional position and had established a research program recognized as successful. Further, these four worked together to help Whitman establish the MBL the way they wanted it. They started the *Journal of Experimental Zoology* (after rejecting the word "biology" since they did not wish to include botany and were aware that they would be criticized if they did not). They remained friends and served in rotation as presidents of leading biological societies, on national boards, and as advisors in many capacities. By any measure, these four were leaders. They each held powerful positions that made it possible for them to put their ideas into wide use and to institutionalize their choices. They served themselves as, or were well connected with, advisors for the Rockefeller Foundation, the National Research Council, and other influential funding groups.

They succeeded in getting their ideas accepted as standard epistemic views because of these connections, because their ideas conformed to (indeed grew out of) mainstream pragmatism, and also because they became extremely good at carrying out the sort of work their standards demanded. They also succeeded partly because they were in the right place at the right time in an academic environment such that their new epistemology was recognized and rewarded as appropriately productive, within a society that valued productivity and results generally. Aside from the fact that their approach "worked" in some senses, it is important that they convinced others that they were up to something new and important.

Again, they did not all or always do this intentionally or even consciously. Rather, they followed Whitman's lead and pursued the lines of research with the approach suggested by their Hopkins advisor William Keith Brooks. All four had worked with Brooks at Hopkins, and all had carried out embryological research on invertebrates under his guidance. Wilson finished first, receiving his Ph.D. in 1881. After an additional year at Hopkins as Brooks's assistant, he decided to travel to Europe to learn more about cells. He visited England, then stopped by Haeckel's lab and worked briefly with the Hertwigs there on cytology, then moved on to the Stazione Zoologica at Naples. Though the Stazione's director, Anton Dohrn, tried to entice Wilson into staying in Naples, he returned to the United States. When the opportunity came in 1885 to move to the forward-looking Bryn Mawr College, where he could carry out research as well as teaching and where he could have advanced students as well as beginners, he accepted willingly. Only when a position became available six years later at the newly reorganized Columbia University did Wilson leave Bryn Mawr.

When Wilson left, Bryn Mawr's president Martha Carey Thomas hired Morgan, who was fortunate that Wilson's move opened the Bryn Mawr job just as he completed his degree. Though not primarily focused on cells, Morgan's work certainly centered on the same sorts of developmental questions that dominated both Brooks's and Wilson's work. After nearly fourteen years Morgan left Bryn Mawr only because Wilson hired him at Columbia.

When Morgan had taken leave to visit Europe in 1894–1895, President Thomas again looked to Hopkins and hired Harrison. Harrison too had studied development and had looked at early cellular changes. Unlike Wilson and Morgan, Harrison had decided to pursue a German medical degree as well as his Ph.D. from Hopkins. To satisfy both objectives, he concentrated on the embryonic development of nerve cells and specifically the generation of nerve fibers. This neuroembryological problem lay at the focus of several German research programs as well, and served as the focus of considerable debate in the emerging neurosciences.

In contrast, during graduate school Conklin married a woman who had taught, as Conklin had, as a missionary at the not-yet-so-historically black Rust University in Mississippi. This kept him from roaming as freely as the others; so he did not go off to Europe or to the seashore in Jamaica or Bermuda or other places with Brooks as the others did. Instead, Conklin spent nearly every summer of his professional life in Woods Hole, Massachusetts – first at the U.S. Fish Commission facility and then the MBL.

In 1891, Conklin was working on his dissertation project, studying early cell divisions of the slipper snail *Crepidula fornicata.* Wilson crossed the street from the MBL to see what Conklin was doing and to compare notes. This sparked a change in the research of each as well as a change at the MBL more generally. This also contributed to the move toward the new epistemology.

Wilson was studying details of the early stages of cell division in the annelid worm *Nereis* and he had heard that Conklin was carrying out similar research on *Crepidula.* He thought that they might find parallels, or an interesting lack of parallels, in the embryonic stages of the two and that this might help them determine how closely related the two organisms were phylogenetically. Even more interesting, it might also help them discover patterns of cell division which would prove fundamental for all organisms – or at least all organisms of a certain type. In other words, the time-honored morphological technique of comparison might give them more than each individual study could produce alone.

What they found was striking. Indeed, the two found promising similarities in their two organisms and significant differences as well, and Wilson at least realized the potential of such work more generally. Back at the MBL, he told Whitman of Conklin's work. Whitman immediately invited Conklin over to talk. Whitman was inspired by Wilson and Conklin and by their shared results to make cell lineage studies a central core of work at the MBL and at the University of Chicago where, after 1892, he was head of the Zoology Department within the biology program. He drew other researchers in and used their comparisons of similar studies across different organisms to forge a coherent research community. New students quickly learned that the first question they must answer in graduate school was "which organism," since the basic questions and techniques and lines of research were already set. They would each employ the most advanced of techniques for preparing, staining, sectioning, and observing cells to gain facts about cells and development. They could then use these facts to begin answering larger questions and enter discussion with other researchers. The work incorporated experimental manipulations and also conformed to the general standards for an experimental approach.

Within this context, Wilson and Conklin each reshaped his own work to make the studies as parallel as possible to facilitate comparisons. They compared their techniques, stains, and other details. They saw the value of working on a variety of closely related organisms to make comparisons more productive, and they sought experimental manipulations to produce useful comparative data. Whitman had succeeded in creating an environment in which cell lineage study seemed at least for the moment to be at the productive edge of biological research. It attracted wide attention and interest and created the impression of promising productive results and of allowing progress of sorting through a plethora of preexisting theoretical speculation about how development occurs. The message was clear: if a new researcher wanted to be at the forefront in biology, he or she should adopt this research program (Maienschein 1986).

This success was possible in large part because of the nature of the MBL and its close connection to the University of Chicago, Columbia University, and other leading universities. The MBL was a comfortable place where biologists could take their families and settle in for a delightful summer of work (Pauly 1988). They could exchange ideas and learn the latest technical advances from the other leading researchers who gathered there by the 1890s. They shared these new discoveries informally every day and also more formally through the evening lectures which Whitman organized every Friday throughout the summers of the 1890s. By choosing to address some questions and not others, and to use some approaches and methods and not others, they began to create a perception, at least, that there was a central core in biology and that they were at it (Rainger et al. 1988). Because Whitman and the MBL controlled the major American biology journals, and because many of the MBL Board members were well-connected socially and institutionally, they gave the wider impression that their way was the best and even right way to do science.

This sense of community further reinforced the development of a changing set of epistemic norms. For, as Whitman had begun to urge, biological science needed to seek solid answers and to refine its questions. It was important, he suggested, to define biology (Maienschein 1986). With a group of researchers coming to the same place, working intensely together, and bringing their own graduate students into their field of excitement as well, they produced the sense that they were getting at the best questions – and in the best way. Even if they did not adopt Whitman's program, they contributed to it by their presence. The idea prevailed that progress in obtaining products was possible and that this group knew how to achieve such progress and how to recognize it when they did. The individual researchers began to voice their convictions.

Wilson described the commitments which the community was coming self-consciously to share in 1896 in his classic text *The Cell.* As he put it, the goal for science is to achieve "positive knowledge" through careful observation and experimentation which yield definitive facts. These facts then combine, through induction, to produce well-founded empirical generalizations, which in turn provide the ground for new hypotheses. Thus, the researcher is not left with simple individual facts alone and should not reject the value of carefully formulated guiding hypotheses for the production of new facts and legitimate interpretations. Rather, in Wilson's words,

> it should be clearly understood that when we attempt to approach these deeper problems [of embryology and cytology] we are compelled to advance beyond the solid ground of fact into a region of more or less doubtful and shifting hypothesis, where the point of view continually changes as we proceed. It would, however, be an error to conclude that modern hypotheses of inheritance and development are baseless speculations that attempt a merely formal solution to the problem, like those of the seventeenth and eighteenth centuries. They are a product of the inductive methods, a direct outcome of accurately determined fact, and they lend to the study of embryology a point and precision that it would largely lack if limited to a strictly objective description of phenomena (Wilson 1896, p. 296).

Based on cell lineage and cell study more generally and against a background of mutual support and enthusiasm for the new progressive biology, then, such pronouncements about the nature of good biological science emerged. Good science should strive for definite facts and, through inductive generalizations, for the best currently available interpretations of those facts. These should, however, change with time and with the discovery of new facts. Positive knowledge and certainty lie in the production of facts and in the procedure through which progress toward better interpretations can be achieved based on these facts, according to Wilson.

Ironically, the cell lineage work which has pushed the MBL researchers to articulate views about how science should work was itself relatively shortlived even at the MBL. Once the community of biologists and recognizably progressive research was forged, it led in new directions. Those who accepted the new epistemology had to work at continually sharpening their tools and seeking more promising edges on which to work as either the tools or areas of research seemed too dull. Wilson continued his work on cytology and cells in development. By the 1920s when the third and final edition of *The Cell* appeared, however, it had become a brilliant synthetic product of the past more than a leading text as the first edition had been. It was rapidly replaced by a host of other diverse works embracing a variety of biochemical and more molecular approaches (Maienschein 1991a). Wilson's study of the chromosomal role in development gained the major attention, even though this remained only a small part of his research. His own cytological edge had slipped backward, and other work moved forward to the edge, to be cut by the sharp tools of experimental analysis and to be acknowledged as important by the community.

Other new directions led toward such research as experimental study of somewhat later developmental stages than those which the cell lineagists had pursued, or toward genetics. Morgan, for example, took up study of the frog's egg. Clearly inspired by the work that Roux, Eduard Pflüger, Gustav Born, and others were doing at Breslau on the frog, Morgan decided to summarize that work and to extend it with a textbook on the subject (Morgan 1897). He began the project at Bryn Mawr, where he was also influenced by his experimentally-oriented colleague Jacques Loeb, and he continued it with a trip to Europe. He reproduced Roux's experiments, for example, obtained different results and showed how they led him to different conclusions. He also dismissed Roux's exaggerated claims for his own importance.

In the process, he recognized that it was difficult to learn all he would like to know about developmental processes and that there might be other ways to get information. Perhaps the study of regeneration could unlock some secrets, he decided, since it provides a case of natural accidental experimentation in which the developmental processes might be more manageable and productive of results. This might, in effect, provide a way to cut through the confusion of variables. He joined dozens of others exploring regeneration as a promising avenue to understanding development since this appeared to be the most productive – for the time being. Morgan began a series of over fifty publications on regeneration and related abnormal developmental phenomena, about which he eventually concluded that he had not learned as much as he had hoped. He felt that regeneration and development generally must result from factors internal to the organism and responsive to the organism as a whole. He offered an hypothesis in terms of differential "pressure relations of the cells," not yet proven but still potentially useful, based on the best available facts and hence productive (in the sense of possessing the capacity to yield solid results).

The fundamental goal of science for Morgan was to achieve positive knowledge in the form of definite "everybody-must-agree-with-them facts." Yet like Wilson, he accepted that along the way, hypotheses could guide the way toward producing more facts and pointing to mechanical and causal explanations of those facts. As Morgan put it,

> So long as an hypothesis is of a sort that it is within the range of observations and experimental test, it may be of service, even if it prove erroneous: for our advance through the tangled thread of phenomena is not only assisted by the advances in the right direction, but all possibilities must be tested before we can be certain that we have discovered the whole truth (Morgan 1901), p. 296).

Scientific hypotheses have value, he continued, but only if they can be tested against facts by direct observation or experiments, can lead to "advance," and can eliminate other possibilities.

As Morgan articulated his views of how science should work, he agreed that it should be experimental and analytical, but should never allow the interpretations to outstrip the demonstrable facts. Unobservable theoretical entities were absolutely forbidden. He ridiculed August Weismann, for example, for his web-spinning theories about unobservable ids and idants and determinants as the carriers of inherited information. He criticized Roux for repeatedly extending too far beyond his data. He questioned research on the cell nucleus, work which suggested that chromosomes and other hypothetical nuclear parts might play a special role in carrying heredity from parent to offspring. Between 1905 and 1910, this meant that he disagreed with his colleague Wilson and with some of his own former students, including Nettie Stevens (whose work on chromosomes and chromosomal heredity proved extremely important), and began to talk about such non-facts as "Mendelian factors" (Brush 1978). Instead, he continued his work on regeneration and related problems, trying to get at what causes the organism to develop in the right way even when it has to respond to confounding external influences. He almost missed his chance to be at what was soon regarded as the new cutting edge of genetics.

Some historians and especially biologists have shaken their heads over this period of Morgan's work and asked such things as "why was he so stubborn" or "why did he take so long to accept chromosomes and genes as the basis for heredity?" They assume that he should have instantly embraced the connection of genetics, chromosomes, and Mendelism since we now know it to be obviously true. Why was Morgan so slow to see this truth, they wonder. The answer, of course, lies in his epistemology. To conform to his own standards of good science, he required something more than a bold hypothesis; he needed evidence in the form of solid, incontrovertible facts. He would have been quite inconsistent to have accepted the rather indirect and circumstantial evidence offered about the importance of chromosomes.

Yet the situation changed with the establishment of new facts, and Morgan then needed to make no apologies for his change. Indeed, as he cautioned in his *Heredity and Sex* in 1913, "I beg to remind the reader and possible critic that the writer holds all conclusions in science relative, and subject to change, for change in science does not mean so much that what has gone before was wrong as the discovery of a better strategic position than the one last held" (Morgan 1913, p. iv). Facts and observations should be definitive, but interpretations can – and should – change. He accepted the fact that heredity produced certain patterns of development, even while he remained skeptical about the metaphysically-based genes or the theoretical role attributed to the chromosomes themselves.

The reason that his little white-eyed fruit fly could change things so apparently suddenly therefore lies in these epistemic norms. With the discovery came facts upon which he could build. It was no longer necessary to build speculative castles in what Morgan saw as the hazy theoretical sky. He could concentrate on the solid, down-to-earth matters of fact – on productive and definitive facts. There was a white-eyed male fly, it had offspring which followed certain statistical patterns, and they just kept doing it reliably and predictably (Morgan 1910). The facts were consistent with an interpretation of Mendelian-like heredity as being carried in units, and Morgan was willing to consider such a view even though he did not at first enthusiastically endorse it. He remained careful about interpretations, following his students' enthusiastic theories only hesitantly and sometimes reluctantly. He maintained that heredity could, in the long run, only really be understood in terms of its effect on development, which is what we can observe and document. For the time being, though, *Drosophila* genetics seemed most promising and likely the most sectile edge to which to apply the experimental cutting edge.

Harrison followed a different path but ended up in fundamentally the same place epistemologically. Unlike his compatriots at Hopkins, Harrison did not continue to study marine invertebrates or cell lineage for very long. In part because his wife's beloved violin suffered in the damp air in Woods Hole, he did not go to the MBL very often to do research, though he remained a loyal Trustee for many years. Instead, following the Breslau bunch led by Roux and Gustav Born, Harrison adopted frogs. In particular, he took up the popular questions: how do nerve fibers make their proper functional connections in the developing frog embryo? Frogs provided an excellent organism for study, since they seemed to be big enough and readily available, similar enough to humans to provide useful information and yet simple enough to yield less complex results.

In Germany and at Hopkins, Harrison experimented with different ways to find out what goes on with the nerve fiber as it develops invisibly inside the frog. He tried a series of transplantation experiments, in which he grafted together pieces from two different frog species which were pigmented differently. He then watched as the cells intermingled from the two parts, assuming that the migration patterns reflected the way cells would move in normal development. But it was his work with tissue culture that caused Harrison to worry quite self-consciously about epistemology and to endorse the concept of edges that can better cut or better be cut themselves.

In 1907, Harrison had a great success. He attacked the much debated question of whether nerve fibers grow independently by protoplasmic outgrowth or whether they follow preexisting bridges and are pulled out toward their final ends in some special way. With the advice of a bacteriologist at Yale (where he had just moved after a number of years at Hopkins as a student and then faculty member in the medical school), he placed bits of tissues which normally give rise to nerve fibers into some frog lymph on a microscope slide. The tissue grew, and some nerve fibers crept slowly out from their original tissue and into the surrounding lymph. Harrison concluded with great excitement that he had just *proven* that nerve fibers normally grow by a process of protoplasmic outgrowth (Harrison 1907).

Yet the opponents did not roll over or even play dead. They remained unconvinced and offered a host of reasons why Harrison's experiment was so artificial that it was not useful at all for understanding normal development. Some just scoffed, while others counterattacked with alternative theories and explanations. Harrison then realized that he had to make the case differently if he was to persuade others that his work was better. Accordingly, he began a series of publications to demonstrate absolutely that his interpretation was well founded and that accepting it was the best thing to do scientifically. In the process, he articulated for himself and worked to persuade others about what he meant by good science (Harrison 1910; Maienschein 1983). It must be analytical, experimental, must control as many variables as possible, and it must produce solid and unquestionable positive results. Knowledge, he agreed with Morgan, Wilson, and Conklin, results from the proper application of experimental procedures and the production of universally accepted facts. For the next three years, Harrison carried out additional studies of nerve fiber development and worked on how best to present his results so as to make them convincing to others and to show how they contributed to the process of making progress with other questions considered important.

His success by 1910 in making his point, establishing the legitimacy of his program, and in suggesting other lines of developmental research to follow caused many students to go to Yale to work with Harrison on what they saw as his "gold mine" in transplantation and explantation studies and nearly won him a Nobel Prize. After 1910, Harrison's research program clearly lay at a cutting edge for biology, as cell lineage had and as genetics would. It generated products and made progress, cutting away confusion and theoretical disagreement, and it achieved external visibility and acceptance. In the process of seeking acceptance from his critics, Harrison had adopted a definition of good science and particularly of what should count as important science.

Harrison did not, however, choose to develop the practical applications of tissue culture himself. Nor did he change direction when it seemed that Hans Spemann's search for the "organizer" (the tissue or substance which organizes the developing embryo) might prove more productive. Harrison found Spemann's ideas too speculative, and he preferred to continue his own researches in experimental embryology even though this decision kept him from remaining on the new cutting edges. Good science, Harrison agreed with Wilson and Morgan, produces positive facts and moves from these only cautiously toward theoretical interpretation, which may hold only for the moment. Harrison accepted that some work was more productive than others, that there can be a cutting edge, but his own cautiousness kept him from trying to move with it along ever-changing boundaries.

Thus, all four of these men did early work at one or another cutting edge for biology, using sharp experimental tools to address tractable problems. All four endorsed – more or less explicitly and more or less intentionally – a view about how good science should be done that implied the existence of a proper approach, or cutting tool, that would designate some work as good and other work as not. All four accepted that some research programs were more sectile and hence more productive – at least for the moment. And all four saw their work replaced by other research which either employed sharper tools or yielded more successfully to the cutting.

There are several ironies in this case. First, it is ironic that these scientists who were adopting an epistemology emphasizing the products of science rather than theory were, in fact, emphasizing epistemic considerations themselves. Because of the nature of the emerging cutting edge at the time, which was defined in terms of experimental analysis of certain sorts, this meant that their own epistemological considerations were not at the cutting edge within science, even while they were effectively defining it! It is also ironic that accepting this set of epistemic standards meant that three of these men were soon dealt out of the scientific leadership because they worked on areas that seemed less productive or less worthy of being cut. Each continued to pursue an active research program, but their own standards of good scientific work dictated that other areas would move to the forefront. Even while they maintained their political and social leadership positions, to the extent that they pursued the same research paths, the spotlight moved past them to other more exciting edges of research.

Morgan reacted somewhat differently. He realized that the facts were taking him away from the immediate study of development and toward heredity. His research programs to study development, especially his commitment to studying regeneration, had produced many detailed facts but relatively little that explained the way developmental processes normally work. Here was a new program for studying heredity that did promise results. For someone committed to pursuing the production of results and to making progress, Morgan had to set aside his embryological ambitions and turn to study heredity instead – at least temporarily. This separation of what had been inextricably related sets of questions, this setting apart and controlling one of the complexes of factors guiding development could produce progressive results. With a reluctant sigh alongside the claims of success, Morgan took up the study of chromosomes. Partly because Morgan had achieved such an established recognition within biology, his work in genetics quickly received external acceptance as useful for producing new and thus attention-grabbing results or facts. The program was adopted by others who defined "facts" differently, and genetics thereby became a new cutting edge for biology. The tool of genetics experimentation provided a sharp tool, and problems of heredity seemed to yield to it.

CONCLUSIONS AND IMPLICATIONS

What we see, then, is a process by around 1900 through which biology was shaped so that it was thought of as having a cutting edge. First biology – at least temporarily and at least for the leading group of researchers – achieved a definition as *biology*. At the same time, it embraced a set of highly pragmatic and conventional epistemic norms. These stressed the importance of making progress: the accumulation of positive knowledge in the form of facts and of exploration of suggestive working hypotheses that could guide that production.

As biology succeeded in this production of knowledge, its advocates became articulate and self-conscious spokesmen for their vision of what good biology should be.

Thus, biology appeared to have a sharp edge for cutting around 1900, with study of developmental and cell biology as particularly yielding to that edge. Yet the very commitment to what counted as good biology, and of what would count as productive, progressive, and visibly successful meant that the most sectile edge soon changed. New promising areas emerged, with shifts of focus away from development and from cells. In large part, this occurred because the old tools of cytology and cell lineage (which relied on preserved and hence non-living organisms) and of embryology (which relied on study of changing morphological patterns) failed to continue generating sufficient numbers of valuable facts. Other techniques and approaches could produce results faster or more easily – or could produce more publicly visible or socially more exciting results. Study of the biochemistry of cells, the physiology of cellular interactions, or the hereditary background of development followed new lines of research in these areas. In addition, more universities and research centers entered the competition for prestige and resources, so that the situation began to parallel late 19th century Germany, where it was advantageous to specialize and work productively and visibly on a newly defined front. Thus, though the basic experimental cutting tool remains primary, new supplementary tools have arisen, and different areas of research have appeared more productive at different times.

What does this tell us about history, about philosophy, and about science? Historians of biology have typically focused on changing theories and the arguments for them. By downplaying epistemology or the basic assumptions about how science should get done, these historians have missed something central to the practice of science. Similarly, philosophers of science have concentrated on theory change, but few have been sufficiently alert to the role of epistemic commitments in determining what sorts of things will count as evidence or when the accumulated results will be accepted as knowledge. The most important implication may be for science. Most reasons for making the particular choices may not always have been good ones – not good for science generally, that is.

Looking beyond the advent of the cutting edge idea in the early part of this century, we can easily find reinforcing factors. As Robert Kohler and other have shown, the Rockefeller and other foundations selected certain fields of research for heavy support, thus giving the clear message that these were more deserving (Kohler 1991). The National Research Council gave its endorsement to particular research areas and endorsed individual research programs. The National Science Foundation followed this trend as well, suggesting that they would support the best and most productive science research and implying that the American people would benefit the most thereby. Universities and industry have set up centers for some kinds of research and not others. Some have won Nobel and other prizes. Yet perhaps the emphasis on making selections and on identifying particular edges is overemphasized and has misled science at times.

Choices are necessary in a limited resource pool, of course. But our way of choosing and publicizing the choices in bold headlines gives a misleading message, implying that we have a valid selection procedure which chooses the best, the truly cutting edge of science. This endorses the image of science as cutting ideally along, piling up successes in the form of accumulated facts and well-founded interpretations as it progresses. Progress will result inevitably because of the inexorable addition of new knowledge, according to this view.

But this idea does not accord with evidence from history and philosophy of science about how science really works. Scientific knowledge does not always come with the accumulation of new facts or new ideas; it may result from rethinking, reshaping, reworking older ideas and facts. It may result from putting together facts and interpretations from what were widely different fields of research, from interdisciplinary research as it were.

Furthermore, focusing on the currently favored cutting edges (and indeed on the process of identifying cutting edges) emphasizes quickness in science. Funding agencies and institutions with non-scientific constituencies want visible results, as soon as possible. If the scientific community promises results and suggests that cutting edge research will produce results the fastest, it is reasonable for the funders to demand the results right away. Thus, research which takes longer times – and which may, in fact, be more thoughtful – gets cut out. Long-term studies of ecology, evolution, behavior, population changes, or embryology, for example, tend to suffer as they slip from the public's (including the scientific "public's") consciousness. Different research areas suffer more from external factors than others, of course.

A further danger of focus on cutting edge science lies with the lack of control over the identification of the cutting edge. As long as it was the biologists themselves making the selections or directly advising the selectors, the process could at least remain responsive to the scientific community and its correctives. When Lysenko, some senator, or proponents of political correctness gain control of the definitions, for example, the results are less clear, and less clearly conducive to the advancement of science. These "outsiders" may apply what looks superficially like the same criteria for valuable or cutting edge science but which are not really the same.

This does not mean that it never makes sense to identify some scientific work as particularly promising and to concentrate resources there. As the MBL and Rockefeller Foundation recognized, promoting the advantages of cooperation and comparison allowed a concentration of effort that could produce results. As the efforts to discover the structure of DNA show, the excitement of the chase to be first to unlock "the secret of life" had special allure and caused exceptional productivity, admittedly at the expense of other work that could have been done. At other times, as with AIDS research, the potential practical benefits of success are so high that some argue for concentrating efforts to increase the chances of success, even when this means that other research will necessarily be discontinued or not taken up. Identifying some work as especially worthy and concentrating efforts there recognizes that science is a communal and increasingly political activity and may profit from competition. At some times for some purposes, this makes sense, though the values that hold for medical research are clearly not all scientific values.

What does not make sense is to imply that all science should be done this way. By allowing the focus on the cutting edges, revolutions, and science specially selected because of the results of the scientific community gives the message that this is the real thing, the reason for doing science. Students turn away from the sciences when they feel that they are not on the edge and are engaged in what looks, in contrast, like humdrum work (the most "normal" of Kuhn's normal science). Researchers find themselves left behind, cut out, undersupported as the officially-designated cutting edge advances. Cutting edges cut both ways and sometimes painfully so.

There is more to science (and more that is worthwhile) than just the edge, just as there is more to the volcano than the advancing edge. Indeed, we will never understand the edge nor be able to predict its movement without a much wider sense of the overall action. Similarly, there is much more to the Grand Canyon than the rim. While most visitors only look at the edge, snap a few pictures, and believe that they have captured its essence, those in the know realize that the richness lies in the smaller details, the deep recesses, the side trails, and in experiencing and changing one's interpretation of the experience over time.

The present preoccupation with the cutting edge is unfortunate for biology. The dominance which some areas of biology have been allowed to assume because of the acceptance of a concept of a cutting edge is pernicious. It has placed an unjustified emphasis on a few people, a few lines of research, and a few ideas at times when there is really much more going on and when the diversity of approaches and problems is what will make the science most productive in the longer run. It has provided excuses for funding agencies to give to those who have and to ignore those who may very well be more creative or more exciting but who, for whatever reason, have not quite succeeded in convincing others that they have the necessary sharp tools or a sectile enough area to succeed in cutting at the edge. Let us recognize the concept as the social and political tool it is. Let us explore its origins and its effects. Then let us work to articulate better epistemic standards for what should count as good science.

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NOTES

[1] Thanks to Michael White for offering the rather arcane term, "secable," which means "capable of being cut" and suggests giving way to the cutting, and to Chuck Dinsmore for offering the slightly more familiar mineralogical term "sectile," suggesting capability of being cut smoothly. It would be informative to know more about the origins and evolution of the term "cutting edge," but I have found no authoritative illuminating source. It does seem to have popped up fairly widely in discussions of technology by the 1950s.

[2] As with any claim about Darwin, I realize that this one may be controversial. And perhaps, as various scholars have suggested, Darwin preferred one approach himself (though there is some disagreement about what it was). Yet, the way in which Darwin draws on evidence from a wide variety of sources and calls for more work of many sorts, along with his general approach and personality, suggests that he remained quite open in what he was willing to include as good science.

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