A New Career

Research on pesticides at Patuxent began soon after the beginning of the modern pesticide era and the introduction of DDT in 1943. Our first results of experiments with DDT bear publication dates of 1946.

[Hall, 1987]

Her first words caught me by surprise.

"Now you have a new career," she said, pausing briefly, perhaps to gauge my reaction.

I don't know whether confusion or even shock registered on my face; either one might have shown because until I heard those words I had never doubted that the new job would be a continuation and expansion of the career I had been working on for more than a decade.

The speaker was Dr. Lucille Stickel, Director of the U.S. Government's Patuxent Wildlife Research Center. The year was 1977 and I had just arrived for my first day of work as a scientist on the Patuxent staff. The purpose of this meeting was to welcome me on board.

Dr. Stickel, then in her sixties, was already a legend in some circles. She had done seminal research that helped lead to the banning of DDT and had later taken over as the Director at Patuxent when the Center was approaching the peak of its fame and fortunes. She was revered by my colleague from graduate school who had helped me to get the job; he liked that she had firm control of almost everything going on at the Center and carried out her responsibilities with unfailing wisdom. I was later to learn that most of the scientists at Patuxent also held her in high esteem. To my puzzlement, a few others did not share this liking. Some who were able to find positions in academia had moved on, and others were alert to opportunities.

Our brief meeting concluded with Dr. Stickel telling me that I would be working on one of Patuxent's most important assignments, which was conducting research on the effects of environmental contaminants on wildlife. This work was of urgent importance and was held in high regard by nearly everyone. She knew that all my past research was on reptiles and amphibians, but doubted that I would be able to continue working on these animals. The reason, she said, was they were of relatively little importance to our parent agency, the Fish and Wildlife Service. A packet of materials—mainly scientific papers produced by Patuxent scientists—was being prepared for me and she advised me to spend the next two or three weeks studying them. When I was ready, we would have another meeting and discuss what research I would be doing. For the moment I would be assigned to the Wetland Ecology Project.

What I had to that point viewed as my scientific career, and was instantly transformed into my 'old' career by Dr. Stickel's pronouncement, was not particularly substantial. I had obtained my doctorate from a highly regarded program at the University of Kansas, studying under one of the icons in herpetological ecology. Impatient however, I wanted to complete my education and get on with my life. Before completing my doctoral dissertation, I had accepted a job teaching biology at a small state college in Pennsylvania—surely not what most of my colleagues would have regarded as a desirable situation, but one that seemed adequate for my short-term plans.

Teaching was the college's only mission, and I was to learn that people there looked upon the desire to conduct research with suspicion and even alarm; what if someone were to expect my colleagues to do research also? My intention upon accepting that job had been to finish my dissertation quickly, to remain on the lookout for opportunities, and to move on to a position in a research university. Things did not work out as planned however. The dissertation was completed, but the job market tightened, family matters intervened, and nine years later I was still in a job that was to have been a one- or two-year interlude.

The Pennsylvania system of state colleges was unusual in that it was part of the state Department of Public Instruction; units of the system had begun as teachers colleges and were only beginning to expand into the arts and sciences. The personnel practices of the state colleges tracked those in the public schools, and one normally got tenure after three years, rather than the seven or more years typical in most other colleges and universities. Candidates were notified early, and I found myself at age 27 to be a tenured Associate Professor; I had qualified for lifetime employment at the college. Like most of my colleagues, I enjoyed the security offered by a near-guarantee of career-long commitment. On the other hand, I could not savor the prospect of spending the next 35 years teaching the same or similar courses in the same classrooms. I wanted more, and understood that I had almost no chance of getting a better position unless I could continue to build my research credentials.

I had finished my doctoral dissertation in the first year in Pennsylvania and spent my spare time in the next two years publishing the core parts of it and two or three papers resulting from other work done as a graduate student. I then began studies on salamander populations, which seemed to be the best local opportunity for field research. These activities were motivated by scientific curiosity but to an even greater extent by my desire to remain productive in order to keep alive the possibility that I might someday get a job bringing me more into the scientific mainstream.

The Stickels. Lucille and Bill Stickel are shown here in a photo probably made in the early seventies. Despite her grandmotherly appearance, Lucille was a crack scientist and at the same time a tough-minded leader and kind person. It was said that her Washington Office superiors had difficulty dealing with her because they were used to supervising rough-cut field biologists who they kept under control with a combination of bluff and bluster.

The salamanders I studied had some inherent interest in that the local species represented different stages in an evolutionary progression from the almost wholly aquatic to the fully terrestrial, and these differences were reflected in their population dynamics. I used this range of variation and its consequences as a way of tying together my few published studies on these animals when I gave my interview presentation at Patuxent. My selection of research topics had been mostly haphazard, but the aquatic to terrestrial transition give me a chance to draw inferences from the several studies taken together.

My research in those nine years broke little new ground and attracted scant attention. It did, however, have two important effects. It helped me to get the job at Patuxent, and it led me to a sense of professional identity as a population biologist. Thus the pronouncement that I had a new career was not only surprising, but seemed somehow incongruous. How could the achievements that helped me build my credentials and get the job be so quickly dismissed as parts of my ‘old’ career?

While the novelty of one’s first day in a new job is likely to produce images that one can long recollect, Dr. Stickel’s statement about my new career resonates far more than can be attributed to its transgressing my expectations. Tension between old careers and new, and the different understandings of science they convey, has been a near constant, arising in a variety of situations and organizations. That day in the Director’s Office at Patuxent was pivotal in my scientific career. As I was to learn in the next thirty years, all science is on one side or another of that pivot point. In government science especially, it is essential for us to understand where we are on the continuum that straddles both sides of that point.

Box 1.1 The Patuxent Wildlife Research Center

My expectations upon arriving at Patuxent were of an isolated research station where a small group of us would be quietly working away on our individual projects. Imagine my surprise therefore, when on my first visit I discovered a full parking lot capable of holding perhaps 75 cars; there were more parking lots and I later learned that nearly 300 people came through the gate each workday morning.

Established in 1939 as the nation's first National Wildlife Refuge dedicated to research, the Patuxent Research Refuge as it was then called, covered nearly 3,000 acres in what had been marginal farm land in the Patuxent River valley, not far from Washington, DC. See Perry (2001) for a comprehensive early history of the Center. At the time of its founding scientists believed that one of the greatest threats to wildlife was agriculture, and one of the best hopes for wildlife conservation was in developing farming practices that were compatible with sustaining wildlife populations. In the early days of Patuxent, farming resumed and experiments were begun on enlightened agriculture that might benefit wildlife. Different practices, including construction of ponds and wetlands, planting of cover crops and wildlife food plants, maintaining hedgerows, and employing different methods of tillage sought to demonstrate how scientific farming could promote desirable wild animals. The Second World War intervened, and few definitive scientific results came from the farm wildlife program. Fortuitously, however, it served as the foundation of what were to become two of Patuxent's three major scientific programs. Studies on farming methods led easily to research on effects of agricultural pesticides, and experiments on habitat management led to important research on migratory bird conservation. The third important program—research on endangered species propagation—was located at Patuxent because the abundant land provided a buffer for this sensitive work. However, it too had an agricultural connection, if only an indirect one; its founders believed that the adjacent Beltsville Agricultural Research Center—a facility of the U.S. Department of Agriculture—would be a ready source of expertise in animal husbandry useful in managing captive wildlife breeding programs.

In the beginning, Patuxent was merely a “research refuge”—a place to do research; actual studies were supervised from headquarters offices in Washington. By the time I arrived, the facility had grown to 4,700 acres, scientific programs were expanding, and most of the research conducted there had come under the supervision of the Center Director. We at Patuxent could feel with some confidence that our organization had few peers. We were world-class, if only in the relatively small world of wildlife research.

A New Kind of Science

Art is me, science is we.

[Claude Bernard (1813-1878)]

What kind of science would I be doing at Patuxent, and how would it differ from the kinds of studies I did in my 'old' career? The work I had done on salamanders was almost certainly basic science, and it appeared to me that the new work I would be doing would be much better described as applied science—maybe I would be trying to figure out how findings in basic science applied to real-world problems. On the surface this seemed the most likely way in which my old and new careers were to differ from one another. However, I was soon to learn that far from applying the discoveries of others to practical problems, much of the work done in this new field was truly groundbreaking. And if calling the work we did at Patuxent 'applied science' didn't seem quite right, then what was it?

That I was confused is not surprising. Endless arguments inevitably erupt concerning the natures of basic and applied science and whatever general or particular activities should be included under these different classifications. The relationship between science and technology is likewise murky and almost certain to generate debate.

The conventional wisdom—what many of us learned in school—is this. Basic science establishes principles and deep understanding. Application of these principles—stepping them down to a more practical level—gives rise to applied science, which uses this knowledge in devising new approaches to problem-solving. Both basic and applied science can be turned into useful things by engineering, and the application of these useful products to real-world needs results in a group of tools that collectively known as technology. The practice of medicine is similar to engineering in that it applies the products of science to practical ends, using the findings of an applied branch of science to address health-related problems. Tidy as they sounded, however, these distinctions were difficult to apply to our pesticide studies and to many other kinds of science.

Not only were we scientists apt to be vague about the distinctions, but authorities likewise had questions about how science should be classified. But the almost universal acceptance in the 1970s of the old model (basic science leads to applied science leads to useful products) has in more recent decades given way to new thinking. Both a committee of the U.S. House of Representatives (1998) and Bowler and Morus (2005) pointed out that the standard hierarchical relationship was no longer widely accepted and was difficult to support. Instead the relationships among these fields were complex and difficult to categorize. For example, contrary to conventional wisdom it could be argued that technology may be the mother of science, as when advances in building microscopes, spacecraft, microprocessors, and lasers led to entirely new horizons in established fields of inquiry.

Gibbons et al. (1994) actually made the relationship even murkier by arguing that successful applications in the scientific realm often precede basic scientific understanding. In these instances, some successful practice developed by trial and error could lead to research examining why it is successful, ultimately resulting in new understanding of underlying principles. This seemed obvious to me. People were successfully making cheese for centuries before anyone grasped how and why it worked or even knew of the existence of bacteria. Similar tacit knowledge exists in almost every field of endeavor, in many instances bereft of the scientific understanding of why things work.

At the risk of generating yet another argument and for the moment relying on what will later prove to be an outmoded terminology, let it be said that if one insists on sticking to the old classification, at its core the science done by government agencies is neither 'pure science' or 'basic science'. At the most superficial level, it seeks to apply the principles and methodologies of science to real problems rather than to seek understanding for its own sake. We conduct science that people want or think they need. Government science exists to provide the nation and its people with useful information, and the emphasis on usefulness is seldom lost.

Informal job titles such as "wildlife biologist", "groundwater hydrologist", and "coastal geomorphologist"—the kinds largely filling the ranks of government scientists—all convey the message that practitioners are not studying basic or "pure" biology, hydrology, or geology, but rather are applying their scientific skills to things that are of interest to government and society.

Even if, as frequently is the case, one finds a government scientist studying apparently esoteric things normally included in the realm of basic science such as fundamental particles, astrophysics, or the nature of acoustic waves, these investigations are almost always undertaken with the goal of supporting the development of something useful. In this broader picture, the 'applied science' category breaks down when one attempts to bundle together both the kinds of more basic science needed to inform the more

Box 1.2 My Old Career

Seeking a way to summarize what I have called my first career, I recently went back and looked at a list of my scientific publications. I hoped that some insights into the culture of academic science might be obtained by examining what I had done as an academic scientist and trying to reconstruct why I studied what I did.

I found 11 papers bearing publication dates between 1968 and

1978. All of them resulted from the career I left behind when I joined Patuxent. Two additional publications, completed long after I had left the college, were based on work begun there. The earlier papers range from two-pagers to the largest; at 38 pages it was the primary product of my doctoral dissertation. Eight papers were related in some way to work done in graduate school, and the remaining three resulted from studies done at Mansfield State College.

The core of my graduate school research at the University of Kansas centered on studies of a species of lizard, the Great Plains skink (*Eumeces obsoletus*). My decision to undertake those projects was based on two primary facts. My graduate advisor, Dr. Henry Fitch, had conducted studies of many wildlife species in northeastern Kansas, and he felt this one deserved additional study. Also, and more importantly, it was felt among some scientists at the time that comparative studies of lizard species offered an extraordinary opportunity to learn more about vertebrate population dynamics in general. Lizard species differed over a range of 'life history strategies.' These strategies formed a dichotomy in which patterns of growth, reproduction, and allocation of resources in some species favored rapid production of offspring, while in others they favored slower reproduction and greater protection of individuals. The skinks I studied tended more toward the protection strategy, and I believed that what one learned about them could add to the growing body of knowledge on the divergent strategies for survival employed by animal species.

I had wanted to continue field research after I got the job in Pennsylvania, but there were no lizards to study anywhere close to the college. I looked around for subjects, and settled on salamanders. Several species could be found nearby and, of interest to me, different species displayed a broad range of adaptations either to more aquatic or more terrestrial life. Coincidentally, these different sets of adaptations also influenced life history strategies, with more aquatic species living more dangerous lives and producing large numbers of offspring, while more terrestrial species tended to live longer and reproduce more slowly. These animals were quite different from lizards, and approaches to studying them were far different from the research I had done in graduate school. Nevertheless, the underlying principles of interest to me were very similar.

So the research I undertook as a graduate student and the studies begun independently after I had completed graduate school had a strong conceptual relationship. Both examined abstract concepts and were undertaken without any regard for possible practical application. In fact, the results of studies done by many scientists on life history strategies have resulted in knowledge useful in conservation biology; for example, they may bear importantly on such things as developing effective conservation actions to protect animals as divergent as butterflies and sea turtles. Had I been aware of possible practical applications, this knowledge might or might not have affected what I did and how I proceeded to do it. The question is moot, however; I required no further motivation than the belief that my studies might improve understanding of some fascinating relationships.

applied kinds of science. Gibbons et al. (1994) took an important step toward understanding this problem by noting that all the kinds of studies mentioned above are conducted "within the context of intended application."

The term "regulatory science," was used and described by Jasanoff (2005). In a broad sense, all government science fits in this category because on the whole it ultimately is meant to inform actions by governmental bodies. In the narrower sense, however, few studies or projects undertaken by government scientists are initiated to directly result in specific policies or actions, and hence fit poorly with obviously regulatory science in the literal sense. The connection may be indirect as for example, when fundamental studies of the behavior of chemicals are undertaken in order to generate basic knowledge that ultimately may help to inform those establishing regulations for the use of products.

One dimension of the apparent schism between basic and applied science has to do with identification of end-users. Different end-users tend to have their own ideas about what is basic and what is applied. When we were working as Fish and Wildlife Service scientists at Patuxent, it was not uncommon for field managers—the nominal end-users and beneficiaries of our science who would be applying it to real problems—to complain that we Patuxent scientists did nothing they found to be useful; their interpretation of this problem was that we were overly involved in 'basic science.' In fact, although highly applied to issues that may have ultimately been of great interest to these critics, the intended immediate users of our science often were other research scientists. These other scientists might validate our work, extend or expand it, or pass it on to others who would find a way to make it a useful product for field managers, policy-makers, or others.

A later section will examine in greater detail the question of who were the clients, customers, and intended beneficiaries of our science. For now, it is of interest to consider the nature of the products coming from our work and how they were used. In many instances, the end-users of information produced by government scientists were decision-makers—members of Congress, Executive Branch officials, interest groups, and others who used the knowledge and insights gained to inform development of policies. When used in this way, information was the primary product, and the involvement of technology—in the sense of making some new tool or capability available to the masses—did not readily apply. In fact, probably only a tiny minority of government scientific activities resulted directly in usable technologies, although the findings of studies might ultimately become useful to engineers and others developing technological approaches to problem-solving. For example, although having no direct practical use to managers, a government study relating the size of particles and surface roughness found on the ocean floor to the generation of waves might be useful to other scientists developing storm surge models, and ultimately to engineers developing seawalls or levies to protect coastal developments.

The close, complex and often poorly defined relationship between science and technology could lead to confusion regarding the role of various government-sponsored scientific activities. Was an employee who used state-of-the art sonar technology to produce a detailed map of the ocean floor engaged in science? Or was he or she merely using an advanced tool to develop a technologically based product? The answer depended on the context in which the work was done.

To purists, the essence of science was application of the scientific method. In this methodology which uses inductive logic to extend knowledge from the particular to the general, some aspect of the workings of nature or of man was described in a hypothesis or model. This model needed to provide for prediction and for testing and potential falsification. If the predictions could not be shown to be false, the result was taken as tentative evidence that the relationship posited in the hypothesis was a valid—or at least useful one. Thus, the use of technology to map the ocean floor was in the strict sense not science. It might be part of the scientific process, however. For example it could lead to development of a hypothesis about the relationship between underwater topography and fish aggregations, or the relationship of sea floor morphology to the presence of oil and gas deposits—both scientific questions of conceivable interest to the nation. Much of science had to do with developing the information and insights required to generate hypotheses.

On the other hand, if mapping of the ocean floor were to be undertaken solely to identify grounding hazards for naval vessels and used only for this purpose, the activity would be clearly the application of technology rather than the practice of science.

Struggling with the problems of distinguishing kinds of research in attempting to create a national science policy, the U.S. House of Representatives Committee on Science (1998) added its own terminology.

> Understanding-driven research makes up an important, but limited, segment of the federal government's overall research portfolio. Much of the research funded by the federal government could more accurately be called "targeted basic research." This term describes research that is largely basic in nature but is done with a sense that some downstream use may exist—but is not done in direct pursuit of a specific application. This targeted basic research occurs in the mission-oriented national laboratories and federal agencies, and is also pursued by many of the scientists funded by individual federal grants.

The Committee went on to explain that the distinctions are not absolute and tend to break down in specific instances.

So what kind of science and technology did we do at Patuxent? Much of what did was clearly applied science, applying the principles and procedures developed by others to the particular problems with which we were faced. But we also did some basic science. Many of our studies involved trying things and developing fundamental kinds of understanding that could be applied across the range of future studies conducted by us and by others. And probably most of our research investigations applied only indirectly to the problems they addressed and were meaningful only to other scientists. Surely we were involved in the development and application of technologies, particularly in the area of analytical chemistry where new methods were needed and, once developed, were used routinely in generating data for research studies. So our science seemed to span the entire spectrum from basic, to applied, to technology, and a typical line of investigation might conceivably involve all of these. The more we wrestled with the problem, the more it became clear that the terms we had learned in school were inadequate to describe most of what we were doing.

Only decades later would it become clear that others were seeking better ways to describe the kinds of science intended to benefit society.

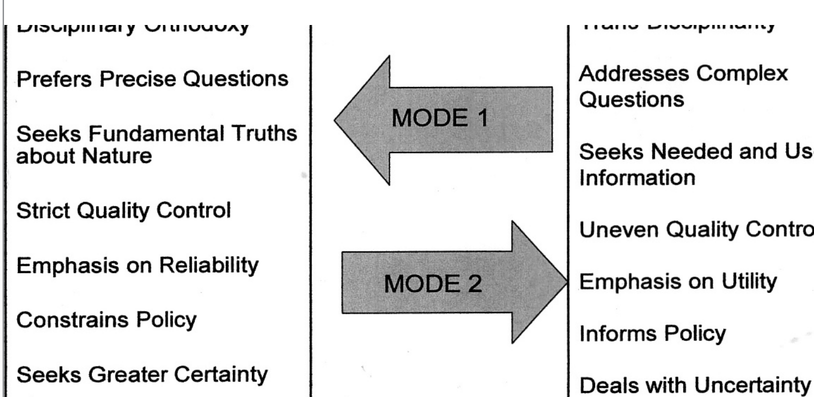
To improve understanding and avoid the overused and much abused basic versus applied terminology and in effect to leapfrog many of the debates, Gibbons et al. (1994) recognized new categories that they called Mode 1 and Mode 2 science. In this classification, Mode 1 science was conducted to generate knowledge for its own sake, while Mode 2 science was conducted within the context of societal needs. These new terms conveyed differences much more fundamental than the ones they replaced, and similar distinctions have been noted and given different names by other authors. For example, Latour (1999) distinguished 'research,' which he saw as a quest for useful answers with 'Science,' which he characterized as a branch of knowledge, in his view unfairly staking claims on sole possession of the truth. In her use of 'regulatory science,' Jasanoff (2005) in some ways paralleled the Mode 1 versus Mode 2 distinction. Likewise, Holling (1998) identified distinctions between 'two cultures of ecology' that share some of the contrasting qualities of the two modes of science.

Pielke (2007) lamented persistence of the basic versus applied science distinction, arguing that the arrangement erroneously implied that what others regarded as "pure science" almost ncecssarily led to useful products or outcomes. This he convincingly connected to the tendency for so-called basic science to become a tool of political interests rather than a reliable informer of policies.

In breaking through all the clutter that afflicted the old classification, Gibbons et al. (1994) and more recent authors took the next step beyond the observations of others that basic science, applied science, and technology had fuzzy boundaries and were intertwined. Mode 2 science, Gibbons and colleagues asserted, superseded and made meaningless the earlier distinctions because Mode 2 used all three kinds of science simultaneously, collectively, and often seamlessly to generate useful products or endpoints—just like we did at Patuxent. In this process, knowledge was created not because of any desire for more

Box 1.3 Contrasting Mode 1 and Mode 2 Science

These contrasting qualities are based on the writings of Gibbons et al. (1994) and Nowotny et al. (2001). The authors also point out that most Mode 1 science is conducted in universities, whereas Mode 2 science may be conducted in government laboratories, some university departments, and elsewhere. Applying metaphors used by these authors and others, the venue for Mode 1 science might be called 'the ivory tower,' while the venue for Mode 2 science is 'the trading zone.'



fundamental understanding (as in Mode 1 science), but because it was necessary to solve a problem, build a product, or support an intended outcome. Arguably all of government science meets one criterion for Mode 2 science in that its origin and justification lie in the belief that it will ultimately result in some useful and needed outcome.

The ideas generated by these authors will be considered further and applied to specific situations in later sections. A real temptation persists in many of us to erroneously equate the Mode 1 versus Mode 2 dichotomy as synonyms of the basic versus applied distinction, doing little more than providing new names for the old understandings. However, much very basic science qualifies as Mode 2 science because of the context in which it is conducted. For example, research on the physics of fundamental particles would fit squarely into Mode 2 if conducted as part of an effort to produce controlled nuclear fusion.

Adding credence to the utility of distinguishing Mode 1 and Mode 2 science, the U.S. House of Representatives Science Committee (1998) stated:

> Government agencies such as the National Aeronautics and Space Administration (NASA) and the National Institutes of Health (NIH), and cabinet level departments—Defense and Energy, for instance—employ science in pursuit of their missions.

and later:

> Research within federal government agencies and departments ranges from purely basic, knowledge-driven research, to targeted basic research, applied research and, in some cases, even product development.

When these statements are taken together, they seem to indicate that all recognized varieties of research are typically 'applied' to agency missions, and this fact transgresses the conventional understandings of the old terminology used to distinguish basic and applied science.

The work I did as a research scientist at Patuxent differed in significant ways from research conducted in graduate school or as a college professor, although it was many years before I grasped the complete dimensions of the difference.

Box 1.4 Contextualization

As asserted by Gibbons et al. (1994) and Nowotny et al. (2001) it is the context in which science is practiced that distinguishes its two major categories, namely science conducted primarily to advance knowledge of the physical world, and that conducted to address some societal need. Science of this second kind was said to be *contextualized*, and the authors further distinguished subcategories based on whether contextualization is strong, moderate, or weak. Our work on endangered Florida manatees provides an example of Interior Department research that was strongly contextualized. Our scientists on that project communicated extensively with federal and state agencies, and with interested citizens, seeking to provide them with better survey methods, better methods of assessing population trends, and models capable of predicting the future of trajectory of populations. All the products of this research supported the need of managers and others to develop appropriate regulatory frameworks, and the managers often collaborated with research scientists in identifying needs and approaches. On the other extreme, scientists studying astrogeology sought to understand processes responsible for the formation of the features of planets and other heavenly bodies. This weakly contextualized research was undertaken not to address any known societal needs—like helping to ensure the survival of manatees—but rather because understanding the forces that shape extraterrestrial bodies might someday be of value to space travelers, or perhaps because it could lead to further useful understanding of earthbound geology. Most Interior Department science fell somewhere in the wide continuum between these extremes; studies tended to address known present or anticipated needs, but varied in the degree to which they were shaped by specific needs. In some instances, scientists produced information or tools that could be used directly in advancing societal goals, whereas in many others, products of research contributed only incrementally to bodies of knowledge that might only indirectly lead to useful outcomes.