The Molecular Biological Bandwagon in Cancer Research: Where Social Worlds Meet*

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This paper analyzes the development of a scientific bandwagon in cancer research using a social worlds perspective and qualitative methods. It shows that a "standardized" package of oncogene theory and recombinant DNA technologies served as a highly transportable interface among many different laboratories and lines of research. That is, the package promoted intersections among different social worlds which, in turn, facilitated the rapid development of oncogene research and the larger molecular biological cancer research bandwagon. The paper proposes the bandwagon as one process by which conceptual shifts in science occur and shows that the process of such change is inseparable from both the local and broad scale organization of work and technical infrastructures.

A scientific bandwagon exists when large numbers of people, laboratories, and organizations commit their resources to one approach to a problem. A package of theory and technology is a clearly defined set of conventions for action that helps reduce reliance on discretion and trial-and-error procedures. This paper analyzes the development of such a bandwagon and package around a molecular biological approach in the study of cancer in the United States.

The data, collected through 1986, come from formal and informal interviews, observation in academic and private industrial cancer research laboratories, participant observation in tumor biology courses, and various documents. I examined journal articles, books, biological materials and instruments catalogues, laboratory manuals, and information on organizations sponsoring cancer research. I attended colloquia, public lectures, and a cancer research conference. Respondents included cancer research scientists, technicians, students, administrators of cancer research institutes and funding organizations, and management of a commercial biotechnology company.

My main premise is that science is work and that scientific information is constructed through negotiations among actors working in organizational contexts. Changing conventionalized and embedded work organizations involves a lot of convincing and persuading, buying and adopting, teaching and learning. Conceptual change in science, in turn, is based in individual and collective changes in the way scientists organize their work. The task of sociology of science is to shed light on these activities, contexts, and processes through which scientific knowledge is constructed and changed.[1]

The development and maintenance of this particular bandwagon was facilitated by a

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1. For a short list of the social studies of science and technology literature in this vein, see Busch (1982), Callon (1985), Cambrosio and Keating (1988), Clarke (1987), Collins (1985), Fujimura (1987), Fujimura, et al. (1987), Gerson (1983b), Knorr-Cetina (1981), Latour (1987), Latour and Woolgar (1979), Law (1986), Lynch (1985), Pickering (1984),

package consisting of a theoretical model for explaining cancer, the oncogene theory, and recombinant DNA technologies for testing the theory. Oncogene proponents hold that cancer is caused by normal cellular genes called "proto-oncogenes" that somehow turn into cancer genes, loosely called "oncogenes." I focus on the construction and marketing of the theory-methods package by oncogene researchers and the buying and importing of the package by their colleagues, members of other lines of research, funding agencies, and suppliers. Early oncogene researchers used this package to push their lines of research and generated a bandwagon that redefined the work organization in many cancer research laboratories. The collective commitments of resources made by early bandwagon joiners set the conditions for gaining new adherents (scientists, laboratories, and organizations) through a "snowball effect." By 1984, the bandwagon was sustained by its own momentum and researchers climbed on primarily because it *was* a bandwagon.

Intersecting Social Worlds in Cancer Research

Scientific problem-solving and fact-making are collective enterprises organized generally along different lines of research, research traditions, and disciplines (Gerson, 1983a). When individuals and organizations commit their resources to a line of research, they are committing themselves to a particular set of problems and oftentimes methods.

Changes in commitment are expensive and require new investments of resources. Moreover, these changes in the conventions of work, including scientific work, occur only through the cooperation of people from diverse social worlds (cf. Becker, 1982; Shibutani, 1955; Strauss et al., 1985). Why would scientists and organizations with existing resource investments in different lines of research be willing to commit their resources to this particular new approach? And how do these members from different social worlds come to practice a common approach to studying cancer?

We can begin to understand these changes by borrowing Everett Hughes's (1971) view of the workplace as "where [diverse] peoples meet." Work gets done in these places only through the conflict, struggle, and negotiations over a set of conventions to guide action and interaction at this meeting of worlds. Here, cancer research is the workplace, the arena where different social worlds meet, and the negotiations are about how one should approach solving the problem of cancer (see Shibutani, 1955; and Strauss, 1978b on arenas). Beginning in the late 1970s in the United States, participants from many different lines of work came to agreement on how best to study cancer. The molecular biological approach has gained an increasing proportion of cancer research commitments although basic, clinical, and epidemiological research with no ties to molecular biological methods continue. That is, in the struggle to define their common object, cancer, molecular biologists and tumor virologists have won acceptance of their definition of the situation by other researchers, sponsors, suppliers, and diverse participants in the cancer research arena. Cancer has become defined for many people as a disease of the DNA, to be studied through oncogene theory and recombinant DNA technologies. The story of this bandwagon's development then is also the story of how molecular biologists managed to impose their definition of the situation on much of the larger world of cancer research.

In the battle over whose "fact" is more "factual," Callon (1985) and Latour (1987) argue that actors enroll allies much as military leaders enlist armies and weapons. A major strategy used by scientists in fact-making is to translate others' interests into their own interests. More generally, translation is the mechanism by which certain entities gain control over the way society and nature are organized, by which "a few obtain the right to express and to represent

Shapin (1982), Star (forthcoming), Star and Gerson (1986). For literature on bandwagons or crusades in science, see Panem (1984), Rip and Nederhoff (1986), and Studer and Chubin (1980).

the many silent actors of the social and natural worlds they have mobilized" (Callon, 1985:224).

Packages in Bandwagon Development: From Custom Tailoring to One-Size-Fits-All

Scientists' commitments are organized into three major interdependent sets of activities that define their work: problem-solving, career-building, and line-of-research-building. Problem-solving in basic science is rarely standardized and requires enormous amounts of work to sort through all the combinations of variables. Results are never assured. Constructing "doable" problems, or successful research projects, is an uncertain process (Fujimura, 1987). For any project to succeed, scientists must negotiate tasks ranging from convincing funding agencies of the project's worth to making or buying necessary supplies to experimental manipulations of DNA. For example, to carry out an experiment, scientists must pull together diverse elements including funds, laboratory space and infrastructure, staff, skills, technologies, research materials, and audiences for the experimental results. This has been called articulation work (Bendifallah and Scacchi, 1987; Fujimura, 1987; Gasser, 1984; Star, 1985; Strauss, 1988; Strauss, et al., 1985). Uncertainty and ambiguity reign at every turn in research paths and require constant surveillance, discretionary decision-making, regular reorganization of activities, and more (Fujimura, 1986a; Gasser, 1984; Zeldenrust, 1985; Star, 1985).

At the same time, scientists are constrained by the requirements of career-building. Since the end of the nineteenth century, industrialization has changed the organization of scientific work in American universities into a rationalized system of production of new knowledge and technologies for "market" consumption (Gerson, 1983b). Basic scientists became "professionals" who were located in research universities. In the university context, scientists are now judged by the amount and quality of their publications and by their students. University molecular biologists, for example, build careers primarily by publishing papers based on the results of laboratory experiments. Even molecular biologists located in private biotechnology companies still build careers through publications (Fujimura, 1986b). They require funds to build their labs and conduct experiments. Further, time scales for experimental results have shrunk in recent years. No scientist today can spend five years to produce results and publications and still expect to win research grants. A tumor virologist with whom I spoke said:

> Researchers have to convince the funding sources that their studies will produce progress, results, within a political time span. . . . If you tell them not to expect progress in five years, they won't fund you. . . . The whole structure of science is pushing for quick results . . .

Organizations and institutions also have careers. University departments, universities, research institutes, and biotechnology and pharmaceutical companies are betting resources on molecular biological cancer research with the goal of ensuring or increasing productivity, maintaining their own existence, and increasing their power and credibility (Latour and Woolgar, 1979).

Problem-solving under highly uncertain conditions and career-building in the context of rationalized knowledge production seem diametrically opposed. Under these conditions, scientists and organizations make commitments to particular lines of research. They aim to construct doable problems which will produce novel information and marketable products within short time frames.

The careers of research traditions can vary in duration, growth or decline, amount and degree of participant support, and kinds of activities and concerns. Scientists, supplying and sponsoring organizations, and academic and commercial research enterprises are committed to the continuance and growth of a line of research because the careers of these lines of research are tightly tied to individual and organizational careers.

In this context, one way to attract adherents to one's approach to a problem and to build up a line of research is to provide a way of organizing work that facilitates the construction of doable problems for scientists, research institutes, and commercial laboratories. Latour (1987:109) argues similarly that

> the first and easiest way to find people who will immediately believe the statement, invest in the project, or buy the prototype, is to *tailor the object* in such a way that it caters [to] these people's *explicit interests* . . . "[I]nterests" are what lie *in between* actors and their goals, thus creating a tension that will make actors select only what, in their own eyes, helps them reach these goals amongst many possibilities [first emphasis added].

In the case studied here, one approach succeeded in quickly "translating the interests" of many members of different social worlds. However, diverse worlds usually have diverse problems and concerns. Tailoring the "object" for each world is a very expensive strategy, requiring much negotiation and many resources. The question then is how the molecular biological approach to understanding cancer succeeded in winning the commitments (or "translating the interests") of members of these different worlds.

The following case study of the molecular biological cancer research bandwagon tells the story of the efforts of certain scientists who constructed a "package" and then convinced members of diverse social worlds that they could use it to construct doable problems.

The Molecular Biological Bandwagon in Cancer Research

Since 1978 the National Cancer Institute has awarded an increasing amount of its basic research funds (versus clinical and educational funds) to molecular biological studies of cancer. Before 1983 the National Institutes of Health (NIH) had no category for oncogenes (or cancer genes) in its computer databank of funded projects.[2] In 1983, the NIH instituted an oncogene category and listed the number of sponsored projects at 54 and the number of dollars disbursed to oncogene projects at $5.5 million. By 1987 the NIH was distributing $103.2 million to 648 oncogene projects. NIH support for projects on genetic manipulation in cancer research grew from $16.3 million in 1977 to $194.4 million in 1987.

Molecular biological cancer research articles increasingly crowded the pages of general science journals like *Science* and *Nature* as well as journals specializing in biochemistry, molecular biology, and cancer research. By 1984 the journal *Science* wrote that "the evidence implicating oncogenes as causes of human cancers, although still circumstantial, has been accumulating rapidly during the past few years" (Marx, 1984:2). In 1984 even popular weekly magazines like *Newsweek* carried articles on oncogenes (Clark, 1983; Clark and Witherspoon, 1984; Clark, et al., 1984).

New investigators chose to study oncogenes, and even established investigators shifted their research agendas to include oncogene or oncogene-related problems. Cancer research institutes changed their agendas by hiring molecular biologically trained researchers and establishing the proper facilities. The Memorial Sloan-Kettering Cancer Center, the country's oldest and largest research and hospital complex devoted exclusively to cancer, recently shifted from immunology to molecular biology. A molecular biologist, Paul Marks, was appointed to head the organization, and he replaced the old leadership with molecular biologists. A respondent outside the organization commented to me on Marks's agenda: "So now

2. Reported NIH funding data is from the Computer Retrieval of Information on Scientific Projects (CRISP) system of references to United States Public Health Service grants and contracts and to NIH intramural projects and retrieved through their keywords. I am grateful to F.M. Biggs and Seu-Lain Chen of the Division of Research Grants and N. Sue Meadows of the Office of Grants Inquiries for their assistance in this data collection.

you have Memorial Sloan-Kettering in lockstep going toward molecular biology." Marks himself states:

> You have the feeling now that this research is making inroads toward the control and cure of the disease. . . . Most of the answers to cancer lie down on the level of the genes, in our understanding of how cells differentiate and divide. . . . You have to work with people who've been trained to think like that (quoted in Boffey, 1987:27).

How did this bandwagon develop, and why did it develop at the intersection of molecular biology and cancer research? I present the story of the bandwagon's development in seven broad stages, beginning with a description of the state of cancer research before the 1970s. Later stages include the development of recombinant DNA technologies, the standardization of recombinant DNA technologies, oncogene theory development, marketing the package of oncogene theory and recombinant DNA technologies, buying the package, and, finally, bandwagon maintenance through the snowball effect.

The State of Cancer Research Before the 1970s

Cancer is the name given to over a hundred different diseases, all of which have one property in common: uncontrolled cell growth. Scientists have studied cancer for a century from many different perspectives and using many different technologies (Bud, 1978; Cairns, 1978; Fujimura, 1986b; Rather, 1978; Studer and Chubin, 1980), yet except for some success at treating a few leukemias, there are no cures or reliable treatments for solid tumors, which form the large percentage of human cancers. Still the hundreds of millions of dollars spent on cancer research by the National Institutes of Health (including the NCI), the American Cancer Society, and other private foundations make it an attractive area of research. The search for the elusive "magic bullet" goes on.

Prior to the development of recombinant DNA technologies in the mid-1970s, cancer research was populated by endocrinologists, immunologists, classical geneticists, biochemists, chemotherapists, and medical researchers of all kinds. Molecular biologists played a limited role. Many researchers pursued the causes of cancer at the cellular and whole organism level. These lines of research included studies of the roles of chemicals, hormones, and radiation in cell transformation. Classical geneticists studied the role of genes in cancer using inbred mice strains and tumor transplantation experiments. Tumor virologists examined the roles of various types of animal viruses and viral oncogenes in cell transformation, often using established cell lines (for a review of these approaches, see Shimkin, 1977). However, until the mid-1970s, researchers had no technologies for testing theories of cancer at the molecular level.

The Development of Recombinant DNA Technologies

Recombinant DNA technology provided a method for manipulating eukaryotic cell DNA. Molecular biologists had previously focussed their research on prokaryotes (simple organisms like bacteria, viruses, and algae whose cells have no defined nuclei). The new methods permitted research on higher organisms whose DNA are enclosed in structurally discrete nuclei (eukaryotes). Since the ultimate goal of this research is to find a cure for human cancer and since humans are mammals with complex eukaryotic DNA, no molecular biological research on human cancer was possible until the mid-1970s.

In 1973, molecular biologists artificially recombined the DNA of two different species, a prokaryote and a eukaryote (Morrow et al., 1974). According to Wright (1986:315),

> the impact of this new capacity to move pieces of DNA between species at will was immediately understood by molecular biologists in the dual scientific and practical terms that characterized the perceptions of its inventors. On the one hand, they saw the ability to do controlled genetic engineer-

> ing experiments as a powerful means to open up new lines of inquiry into the structure and function of DNA. . . . On the other hand, industrial applications were also anticipated.

Recombination by itself, however, did not allow scientists to do controlled experiments at the molecular level. Molecular biologists needed other techniques, including the cloning, sequencing, mapping, and expressing of genes, before they could easily manipulate DNA to do controlled genetic engineering experiments. By 1977, molecular biologists had developed cloning techniques that allowed them to insert eukaryotic DNA into and among bacterial DNA. They could then grow the bacteria to produce more copies of the isolated foreign piece of DNA in order to analyze it. By 1977 they had also developed faster and more efficient sequencing techniques, which they used to delineate the location of genes on the genome (the complete genetic message of an individual organism) and later to map out the structure of entire genomes, or at least of smaller genomes. Finally, molecular biologists also developed new methods to make the recombined eukaryotic genes, especially mammalian genes, express themselves in bacterial systems. Gene expression is the transformation of the genetic code into the proteins which make the cell function. Gene transfer was one of these new methods. While some molecular biologists used cloning and sequencing techniques to study the DNA structure and the location of genes on the DNA, others used gene transfer techniques—introducing foreign DNA sequences into living cells—to study the functions of genes and gene fragments. However, the techniques were still not standardized enough for researchers other than recombinant DNA methodologists to use efficiently in pursuing other biological problems.

Standardization of Recombinant DNA Technologies

The bandwagon was further developed with the standardization of recombinant DNA technologies. Even if scientists want to incorporate new technologies into their work, they may not be able to do so for lack of funding, available skills, time, or other needed resources. Scientists working in existing laboratories and institutions have made major commitments of resources to particular problems and approaches to those problems. Altering current problem paths requires making changes in ongoing organizations of work: acquiring new skills and knowledge, hiring new staff, and buying new instruments and supplies. Yet, scientists in both university and commercial laboratories tend to work within relatively stable resource allotments that can rise or fall in small increments. They can gain additional resources only through the expenditure of other resources (time and effort taken away from experimental work to write grants or put on "roadshows"). Thus, preferred changes are relatively inexpensive. Investing in standardized technologies is, in fact, an economical way around resource constraints, if entry costs are low enough.

Standardized technologies are tacit knowledge made explicit and routine via simplification and the deletion of the contexts in which the technologies were developed (Kling and Scacchi, 1982; Latour and Woolgar, 1979; Star, 1983). They are conventions for action that are carried with little or no change from one context to another. Standardized technologies take the form of pre-fabricated biological materials (reagents, probes), procedural manuals (often called "cookbooks") spelling out "recipes" for action, industrial standards, computing protocols, and instruments that automate many procedures (Fujimura, 1987; Kling and Gerson, 1977, 1978).

Because they are explicit and routine procedures, standardized technologies are highly transportable. They reduce the amount of tacit knowledge, discretionary decision-making, or trial-and-error work needed to solve problems. What is done to which material for what reason or purpose and with what outcome are all built into the "black box" of transportable technologies. Thus, they are easier to learn and cost less time and effort in retraining and monitoring laboratory staff. To illustrate, before the development of standardized electrophor-

esis procedures and equipment, a molecular biologist would need "golden hands" to separate DNA lengths of different molecular weights using the centrifuge. Most researchers found the centrifuge too clumsy a tool for such delicate work. Thus, few researchers performed experiments requiring separation techniques until the development and marketing of electrophoresis procedures and equipment. With the development of standardization, experiments requiring the technique flourished.

Finally, these technologies have to be financially affordable in many laboratories for a bandwagon to roll. Standardized technologies allow for rationalized production and distribution of the materials, techniques, and instruments. This reduces the costs of consumption, while economies of scale in production firms reduces the unit cost of the goods. The net result is reduced entry costs.

By saving in training efforts, time, trial-and-error procedures, and/or material costs, standardized technologies reduce the costs of importing them into uninitiated laboratories and the costs of doing research in each laboratory. Reduced costs increase the number of experiments that can be done in a given laboratory with a given budget.

By 1980, recombinant DNA technologies had become conventionalized ways of organizing work (cf. Becker, 1982).[3] Conventionalized tools are relatively stabilized ways of doing work that reduce the uncertainties of daily bench work and the number of "moments" of discretion. Conventions effectively designate some steps in the work processes as taken-for-granted or unproblematic, so that scientists can concentrate on the problematic (Gerson and Star, 1988; Latour and Woolgar, 1979).

First, a few researchers had refined methods for recombining and manipulating DNA into a set of standardized technologies that other molecular biologists and even non-molecular biologists could also use. The developers of recombinant DNA technologies had codified the technologies in three forms: standardized sequences of standardized tasks, standardized materials, and standardized instruments. By 1982, "cookbook" manuals containing technical "recipes" were available. Cold Spring Harbor Laboratory—a leading institution in molecular biology directed by James Watson, who delineated the structure of DNA in 1953—had compiled and published its first edition of *Molecular Cloning: A Laboratory Manual* (Maniatis, et al., 1982) and, by 1984, many other manuals and textbooks were available.

Walter Gilbert spoke of the relative ease of learning the new Maxam-Gilbert DNA sequencing method as he accepted a Nobel Prize in 1977:

> To find out how easy and accurate DNA sequencing was, I asked a student, Gregor Sutcliffe, to sequence the ampicillin resistance gene . . . of Escherichia coli. . . . All he knew about the protein was an approximate molecular weight, and that a certain restriction cut on the [pBR322] plasmid inactivated that gene. He had no previous experience with DNA sequencing when he set out to work out the structure of DNA for his gene. After 7 months he had worked out about 1000 bases of double-stranded DNA, sequencing one strand and then sequencing the other for confirmation. . . . The DNA sequencing was correct. Sutcliffe then became very enthusiastic and sequenced the rest of plasmid pBR322 during the next 6 months, to finish his thesis (quoted in Cherfas, 1982:124).

Second, materials used in recombinant DNA technologies (restriction enzymes, oncogenes, DNA probes, herring sperm DNA, reverse transcriptase, cell lines, antibiotics, many kinds of chemical reagents, agarose and polyacrylamide gels) are easily available due to the commitments of many organizations. Non-profit organizations like the American Type Culture Collection (ATCC), located in Rockville, Maryland, collect and maintain recombinant DNA research material samples from laboratories for distribution on demand. Among other

3. The "logical" sequences of steps in recombinant DNA research discussed above are logical only insofar as a number of contemporary biologists collectively agree to represent nature in this fashion. When the organization of their work changes (for whatever reasons), their representations of nature and their "logical" experimental steps will most likely also change (Gerson and Star, 1988).

materials, researchers can order plasmids and DNA sequence probes (including cloned human DNA) and pay only maintenance and shipping charges.

Scientists can also mail order from private companies high quality biological materials used in recombinant DNA experiments. These materials range from standard to customized products. For example, instead of purifying or constructing their own materials, scientists can purchase restriction enzymes, modifying enzymes, and vectors required for DNA cloning from New England Biolabs (NEB) and Bethesda Research Laboratories (BRL).

Information for using recombinant DNA technologies is also readily available. DNA, RNA, and protein maps and sequences have been published in journal articles and books. Even more helpful are the centralized, systematic databases holding DNA and RNA sequence information on many organisms, including humans.

Finally, instruments automating standardized procedures are well-developed in recombinant DNA technologies, including DNA synthesizers and sequenators, centrifuges, and electrophoresis systems. A "gene machine," or automated DNA synthesizer, exemplifies the prodigious efforts that molecular biologists, commercial entrepreneurs, and venture capitalists have invested in recombinant DNA technology. A DNA synthesizer manufactures customized synthetic oligonucleotides, or short segments of DNA (or RNA). Synthetic DNA is useful to both applied genetic engineering and to basic research scientists. For example, an article in *High Technology* describes a common task in molecular biological laboratories which can be accomplished by the DNA synthesizer:

> Perhaps the most common use of DNA synthesis today is to make "probes" that help locate a natural gene of scientific or commercial interest—such as that for human growth hormone, insulin, or interferon—among thousands of different genes in the DNA of a cell. . . . [f]inding a particular gene is akin to searching for the proverbial needle in a haystack. Fortunately, the ability to make pieces of [complementary] synthetic DNA has greatly simplified the search (Tucker, 1984:52).

The first automated DNA synthesizer was introduced in 1981. By 1984 seven more reliable machines were on the market and operating in biotechnology companies. A 1986 article reported: "[t]he chemistry has been refined to such a degree that the synthesis of oligonucleotides up to 50 bases in length has become *routine* and oligonucleotides longer than 100 bases have been synthesized" (Smith, 1986:G63).

In 1986, automated DNA sequenators took the facility and efficiency described by Gilbert one step further. The operator need only know what solvents and reagents to put into the instruments. The sequenators thus reduce training requirements even more. Sets of tasks that once were considered thesis problems are now routinely performed by machines.

Thus, even researchers who were not at Harvard University, Massachusetts Institute of Technology, Stanford University, or Cold Spring Harbor Laboratory, the institutions where the "state-of-the-art" technologies were being developed, were able to easily acquire the materials and tools needed for recombinant DNA research. Standardization has also allowed for rationalized production and distribution of the materials, techniques, and instruments used in recombinant DNA research. Thus, the cost of the research has been reduced to affordable levels.

The Oncogene Theory: One-Size-Fits-All

The development and standardization of recombinant DNA and other molecular biological technologies, however, were not solely responsible for the molecular biological cancer research bandwagon. Theories of cancer framed around DNA were also part of the package. Researchers used such theories to guide their use of recombinant DNA technologies and to interpret the results of such applications. Oncogene theory is an important example as it played a ideological role in the initial bandwagon formation.

While some molecular biologists tinkered at improving recombinant DNA technologies,

others further explored the structure of DNA, and still others applied the techniques to long-standing questions in almost all biological disciplines (Kumar, 1984:ix). Molecular biologists regarded recombinant DNA technologies and eukaryotic cell genes as the keys to the previously locked doors in normal differentiation and development, cell proliferation, cancer, and even evolution. A group of molecular biologists and a group of tumor virologists took the opportunity to apply the techniques via the oncogene theory to build new lines of research on human cancer.

In the late 1970s, a few molecular biologists and tumor virologists were engaged in two separate lines of research on the molecular mechanisms of cancer causation. The tumor virologists claimed they had found a class of genes in the normal cell that can be triggered to transform the normal cell into a cancer cell (Bishop, 1982; Fujimura, 1986b; Weinberg, 1983). In 1983, Weinberg, a molecular biologist, and his associates claimed their oncogenes to be in the same class as those found by tumor virologists:

> Two independent lines of work, each pursuing cellular oncogenes, have converged over the last several years. Initially, the two research areas confronted problems that were ostensibly unconnected. The first focused on the mechanisms by which a variety of animal retroviruses were able to transform infected cells and induce tumors in their own host species. The other, using procedures of gene transfer, investigated the molecular mechanisms responsible for tumors of nonviral origin, such as those human tumors traceable to chemical causes. We now realize that common molecular determinants may be responsible for tumors of both classes. These determinants, the cellular oncogenes, constitute a functionally heterogeneous group of genes, members of which may cooperate with one another in order to achieve the transformation of cells (Land et al., 1984:391; emphasis added).

Although their data came from two different areas of research, both groups used recombinant DNA technologies to try to prove their claims. They posited several types of triggering mechanisms. There are many little debates among oncogene researchers about specifics, but the general oncogene theory has become the most popular theory of cancer causation in the 1980s.

These tumor virologists and molecular biologists framed the oncogene theory in a way that they claimed encompassed and unified many other areas of cancer research. They claimed that further investigation using the oncogene framework would produce explanations at the molecular level for problems previously pursued in classical genetic, chemical, radiation, hormonal, and viral lines of research on cancer. They also proposed connections between their theory and other work in the molecular biology of normal growth and differentiation. At the time (and at present), the oncogene theory was the only coherent theory for activities at the molecular level in oncogenesis. The claims were quite grandiose. For example, Robert Weinberg (1983:134), one of the first researchers who claimed to have found cellular oncogenes in human tumor cell lines, said in 1983 that the oncogene theory accounted for findings in many lines of cancer research:

> What is most heartening is that the confluence of evidence from a number of lines of research is beginning to make sense of a disease that only five years ago seemed incomprehensible. The recent findings at the level of the gene are consistent with earlier insights into carcinogenesis based on epidemiological data and on laboratory studies of transformation.

The claim of oncogene theorists to have found one set of causal elements that unified all pathways fell on welcoming ears. *Newsweek* (Clark & Witherspoon, 1984:67) ends one article on oncogenes with the following acclamation:

> Such discoveries shed important light on the fundamental processes of cancer as well as the growth and development of all forms of life. In the future, they will surely lead to better forms of diagnosis and treatment. The presence in cells of abnormal amounts of proteins caused by gene amplifications, for example, could lead to sensitive new tests for certain kinds of cancer. As for treatment,

> scientists envision the development of drugs designed to specifically inhibit oncogenes. These would be far better than anticancer drugs that indiscriminately kill normal cells along with cancerous ones. "We would," says Frank Rauscher of the American Cancer Society, "be using a rifle rather than a shotgun."

The unifying theory appealed to scientists because of its elegance, a term they use to describe a theory that can precisely and simply explain many disparate observations.

Oncogene theory proponents claimed to have developed a "one-size-fits-all," molecular explanation for many different types (classifications) and causes (causal explanations) of cancer. The ultimate claim was that their research might lead to a common cure, a "magic bullet" for cancer.

Marketing the Package

Claiming that one's theory unifies many lines of research, however, does not mean that others will agree with the claim and rush to pursue experiments based on the theory. In the fifth stage, tumor virologists and molecular biologists jockeyed for position and finally joined forces to construct and promote the package of the oncogene theory and recombinant DNA technologies. They marketed the package as a tool by which other researchers could transmute their work organizations and construct doable problems. Incentives for "old-fashioned" cancer researchers included a chance to use "hot," new recombinant DNA technologies. Molecular biologists were offered a chance to attack the ***human*** cancer problem through the oncogene theory. These incentives contributed to the development of the bandwagon.

Oncogene theory proponents enrolled allies behind their package not only by claiming to have accounted for findings in many other lines of cancer research, but also by framing and posing new doable problems on oncogenes for other researchers to investigate. That is, they posed questions which: (1) scientists could experimentally investigate using recombinant DNA and other molecular biological technologies; (2) laboratories were already organized and equipped with resources to handle, or could relatively easily import the requisite resources; and (3) satisfied significant audiences.

The proposed problems were both specific and general. Researchers could immediately begin experimentation on specific problems, while thinking of possible ways to translate more general problems into specific experiments.

J. Michael Bishop's (1983:345-48) article on "Cellular Oncogenes and Retroviruses" in the 1983 *Annual Review of Biochemistry* is an excellent example of the rallying cries used to market the package. The article proposed problems that mapped onto established laboratory organizations and available technical skills. Bishop first summarized work in several other lines of cancer research and then presented proposals for research that linked oncogenes with these other lines of work in cancer research and in biology generally, including experimental carcinogenesis, evolutionary biology, normal growth and differentiation, medical genetics, and epidemiology. Bishop (1982:91) states:

> Medical geneticists may have detected the effects of cancer genes years ago, when they first identified families whose members inherit a predisposition to some particular form of cancer. Now, it appears, tumor virologists may have come on cancer genes directly in the form of cellular oncogenes.

In a volume entitled *RNA Tumor Viruses, Oncogenes, Human Cancer and AIDS: On the Frontiers of Understanding*, the editors Furmanski, Hager, and Rich (1985:xx) proclaimed that

> we must turn these same tools of molecular biology and tumor virology, so valuable in dissecting and analyzing the causes of cancer, to the task of understanding other equally critical aspects of the cancer problem: progression, heterogeneity, and the metastatic process. These are absolutely crucial to our solving the clinical difficulties of cancer: detection, diagnosis and effective treatment.

Oncogene theorists also used other strategies to gain allies. For example, in 1984 they established the first of annual national meetings devoted entirely to oncogene research. At a more hands-on level, they distributed their probes for oncogenes to other laboratories and to suppliers, thus facilitating the spread of oncogene research by providing standardized tools. One oncogene researcher told me:

> We've had so many requests for our probes for [two cellular oncogenes] that we had one technician working full-time on making and sending them out. So we finally turned over the stocks to the American Type Culture Collection.

These probes were more than physical materials. They were "black boxes," designed with reference to specific hypotheses about their involvement in cancer causation. Any researcher can call or write to ATCC to order the probes at the cost of maintenance and shipping. In other words, oncogene researchers made the tools for testing and exploring their theory available and accessible to a host of other researchers.

Oncogene theory proponents taught and talked about their work to students and researchers in other biological disciplines. A respondent described the positive response of cell biology conference participants to an oncogene promotion talk. Most of the conference participants, uninitiated in the complexities of oncogene research, were awed by the promotion and unable to evaluate the difficulties in the data. Proponents also spoke about their work in the popular media. On October 6, 1987 the *New York Times* published an article headlined "Young Science of Cancer Genes Begins to Yield Practical Applications."

Efforts to build lines of research and to gain allies for particular perspectives, theories, and research are common. The issue here is not that tumor virologists and molecular biologists attempted to gain allies but how they did it and why they succeeded. I have discussed their strategies for gaining allies here. Next I discuss why they succeeded.

"Buying" the Package: Many Prizes in the Box

The decisions of individual researchers, funding agencies, research institutes, and private companies to pursue oncogene research were based on their goals: to construct doable problems, build careers, produce marketable products, and build successful "going concerns" (Hughes, 1971).

Researchers do not readily alter successful research programs just to pursue new opportunities. As I argued elsewhere (Fujimura, 1986a), they redirect, shift, or add to their problem paths when opportunities outweigh costs or when they cannot work around contingencies that block further progress. New recruits decided to import the package and change the course of their research because: (1) the oncogene theory offered the chance to pursue research on human cancer; (2) the package of oncogene theory and recombinant DNA technologies provided a pathway to exploring new, uncharted territory ("sexy" problems); (3) researchers could incorporate the new, "hot" standardized recombinant DNA technologies into their laboratories at relatively economical start-up costs; (4) work in some laboratories had led to "dead-ends" or "roadblocks," while work in some cancer research institutes had been criticized as "old-fashioned." The package of oncogene theory and recombinant DNA technologies fit both organizationally and intellectually with the requirements, goals, and conditions of the work of researchers in several different lines of work.

Incentives for Tumor Virologists. Tumor virologists decided to shift to or augment their research with oncogenes questions in order to work more closely with human cancer etiology. This concern grew in prominence during the 1970s because tumor virology research had come under fire for its lack of payoff in human cancer terms.

Using traditional virological methods to investigate RNA tumor viruses, tumor virologists had found specific genes in the viruses that transformed cultured cells and caused tumors in

laboratory animals. These viral oncogenes, however, caused cancer only in vitro and in laboratory animals. No naturally-occurring tumors in animal and human populations were credited to viral oncogenes although researchers have argued that they have confirmed suspected links between some human cancers and retroviruses (see especially Gallo, 1986).

Most of the tumor virology research was funded by the National Cancer Institute's Viral Cancer Program, which was established on the premise that many human cancers were virally induced. The VCP, initiated as a contract program in 1964, was thereafter heavily funded. In 1971 the National Cancer Act continued to fund allocations to virus cancer research despite heavy criticism from biological and biomedical scientists (Chubin and Studer, 1978; DeVita, 1984; Rettig, 1977; and Studer and Chubin, 1980). After further protests and controversy, the ad hoc Zinder committee was constituted to review the VCP. In 1974 the committee, headed by Norton Zinder of Rockefeller University, submitted an extremely critical report to the National Cancer Advisory Board (NCAB), which oversees the work of the entire NCI (Rettig, 1977). In 1980, as a further consequence of this and other in-house controversies, NCI leaders decided to break up the VCP and integrate the pieces into other NCI programs. This overhaul had cumulative negative effects on viral cancer research funds.

In response to criticism and to NCI cutbacks in support, many tumor virologists decided to shift their research toward cellular oncogenes. The oncogene theory and recombinant DNA technologies provided the means for tumor virologists to construct doable problems on a group of genes in normal cells in all eukaryotes (including human) that were suspected of causing cancer. The possible impact of this research on human cancer satisfied sponsor demands and thus increased its marketability.

Incentives for Molecular Biologists. As I noted earlier, molecular biologists were not participants in cancer research until very recently. Molecular biologists used recombinant DNA technologies and the oncogene theory as a means to insert themselves into cancer research, especially into work on the human cancer problem. Weinberg (1982:135; emphasis added), an important player in this field, spelled this out clearly: "The study of the molecular biology of cancer has *until recently* been the domain of tumor virologists."

Incentives for the National Cancer Institute. NCI administrators "bought" the package for reasons similar to those of the tumor virologists. Their sponsors were Congress and the publics they represented, including other scientists. The oncogene theory and recombinant DNA technologies provided them with both the justification for past research investments in the Viral Cancer Program, then under heavy criticism, and with a product to market to Congress.

Efforts to use oncogenes to justify past investments in viral oncology are evident in the following statement by Vincent T. DeVita, Jr. (1984:5), director of NCI:

> We have often been asked if the NCP [National Cancer Program] has been a success. While I acknowledge a bias, my answer is an unqualified "yes." The success of the Virus Cancer Program which prompted this essay is a good example. Since its inception, this Program has cost almost $1 billion. If asked what I would pay now for the information generated by that Program, I would say that the extraordinarily powerful new knowledge available to us as a result of this investment would make the entire budget allocated to the NCP since the passage of the Cancer Act worthwhile. There may well be practical applications of this work in the prevention, diagnosis, and treatment of cancer that constitute a significant paradigm change. The work in viral oncology has indeed yielded a trust fund of information, the dividend of which defies the imagination.

Indeed, DeVita used the oncogene theory to justify the entire National Cancer Program. Similar justificatory statements were made by leading oncogene theorists, such as J. Michael Bishop (1982:92), and leading molecular biologists, such as James D. Watson (DeVita, 1984:1).

DeVita told me he also used oncogenes to sell their general future program of molecular genetic research on cancer to Congress:

> Molecular genetics is a term nobody in Congress understands. Oncogenes they know. How do they know? I tell them. I can explain oncogenes to them much better than I can explain molecular genetics. When I point my finger at a Congressman, I say, "Mr. So-and-So, you and I both have genes in us, which we believe are the genes that are responsible for causing cancer." It gets their attention. They say, "My God! What do you mean I have genes in me . . . ?" I have to explain it to them. If I tried to explain molecular genetics, they'd fall asleep on me.

The statement that we all have genes that can be triggered to cause cancer can engender great fear in congressional members and their constituents. This fear, however, was laid to rest by the claim that a unifying pathway to all cancers may exist and that there may be ways to intervene in this pathway.

The National Cancer Institute bought completely the oncogene theory and molecular genetic approaches to cancer in general. DeVita summarized for me NCI's investments in molecular genetic cancer research for 1984:

> [In 1984] we had $198 million in molecular genetics. . . . [That figure] includes oncogenes, but it also includes people who are walking up and down the genome, tripping on oncogenes but looking for something else. And they're going to find the regulatory elements that control the oncogenes, [which is] really the major step. Oncogenes have told us something very important, but now what you want to find out is what regulates these genes so that you can use this information to turn them on and off.

In addition, in 1981, while NCI leaders were in the process of reorganizing research at the Frederick Cancer Research Facility, they decided to similarly shift the facility's emphasis. "We put three or four crackerjack oncogene scientists up there, and they're up there cranking out the data and having a fun old time," said one administrator.

NCI appointed a viral carcinogenecist who had worked with George Todaro, one of the originators of an early version of the oncogene theory, to the position of associate director of the entire NCI. He oversaw the Frederick Cancer Research Facility and specifically kept track of oncogene research progress (Shapley, 1983:5). Finally, NCI committed funds to a supercomputer to facilitate oncogene and other molecular biological cancer research.

The NCI focus on oncogenes and other molecular biological cancer research studies meant cutbacks in other basic research lines, as a 1983 article (Shapley, 1983:5) in the journal *Nature* indicates:

> If NCI allows spending on oncogene research to expand naturally, does this mean less prominence for important traditional fields such as chemotherapy? DeVita says that some other work must obviously go, given the fact that NCI is unlikely to receive any budget increases in the next few years. He notes that chemotherapy has been cut by about 30 percent in the past six years on scientific grounds: "some things we didn't need to do any more," he says. And, as explained by Alan Rabson, director of NCI's division of cancer biology and diagnosis, "if you understand oncogenes you may learn where to go in chemotherapy. It may open up whole new areas of chemotherapy."

Although chemotherapy was the only area mentioned in the article, other areas of research were also neglected just by the fact that Congress did not increase their budget while oncogene research expanded.

Incentives for Students, New Investigators, Established Researchers. The growing commitments of tumor virologists, molecular biologists, and the NCI to oncogene and related molecular biological cancer research became, in turn, further incentives for students, new investigators, and even researchers established in other lines of work to frame their theses and research problems in these terms.

Besides interesting intellectual questions and the problem of curing cancer, new researchers had to attend to career development contingencies in making problem choices. The immediate foreground was filled with the exigencies of their daily work lives: researching and

writing Ph.D. theses, establishing and maintaining laboratories and staff, publishing and gaining tenure, writing grant proposals, attracting and training students. Constructing doable problems that produce results that someone will publish is a practical and pressing concern. Thus, desirable "cancer research" becomes "doable research."

Students and beginning researchers gained major advantages for establishing their careers and laboratories by choosing to investigate problems under the rubric of oncogene research. By 1982 these advantages included clearly articulated experiments, research funds, high credibility, short-term projects, increased job opportunities, and the promised generation of downstream doable problems.

By 1982, the package of oncogene theory and molecular biological technologies provided clear problem and experiment structures. At the end of 1983, a researcher in an oncogene laboratory explained his work to me as a set of logical steps:

> Everyone knows that it would be worthwhile to sequence this gene. It's obvious what should come next. There are logical steps in this work. 1. Identify the protein [involved in transforming normal cells into cancer cells]. 2. Clone the gene associated with the protein. 3. Sequence the gene. You can always find money to sequence. We get enough grant money to sequence what we want to. It may not solve the cancer problem, but it will give some information.

By 1982 oncogene research was a "hot" area, consisting of several lines of research. NCI had made substantial funding allocations to oncogene research. Here a graduate student who had been studying cellular immunology and shifted his thesis problem to investigate an oncogene talks about the growth of commitment to the new line of work:

> I studied cellular immunology and am now doing c-myc [cellular myc oncogene] research. My professor said, "There's the funding, go for it." So I did. I wrote a grant to fund this oncogene research project. When you write a grant, it also forces you to get into it, to think up innovative ways of approaching the problem. Why not go for it? It makes sense to use the funding that's there to do your work. If it turns out to be significant work, then good. If not, you can always change later.

Oncogene research uses skills in molecular biology, including recombinant DNA technologies. These technical skills were, by then, standardized and available to new researchers at affordable investment costs.

Moreover, new researchers raised the credibility of their work by constructing problems using molecular biological technologies. Latour and Woolgar (1979:242) note that

> credibility is a part of the wider phenomenon of credit, which refers to money, authority, confidence and, also marginally, to reward. . . . [For example,] the mass spectrometer is . . . an actual piece of furniture which incorporates the majority of an earlier body of scientific activity [in physics]. The cost of disputing the generated results of this inscription device [is] enormous.

Very few biologists would be willing to attack findings based on results from a mass spectrometer, often used in molecular biological experiments, because physics ranks high among scientific hierarchies of credibility (Becker, 1967; Gerson and Star, 1988). Although power is negotiated, the relative power of the negotiating parties matters (Strauss, 1978a). Since many molecular biological methods came from physics and chemistry, oncogene researchers increase their negotiating power and credibility by enrolling recombinant DNA and other molecular biological technologies. That is, they are adding the appeal, legitimacy, and credibility of molecular biology, physics, and chemistry to the construction of their "facts." For these reasons, oncogene research problems were extremely good bets in the early 1980s for gaining marketable problems, skills, support, and products.

In addition, new researchers increased their chances for successful future careers by enrolling molecular biological technologies. Molecular biology has steadily expanded its boundaries during the past two decades. There are very few fields of research and biological disciplines that molecular biologists have not entered. University biology departments, re-

search institutes, commercial biotechnology companies, and pharmaceutical companies all continue to vie for the best molecular biologists in the world (Stokes, 1985; Wright, 1986). Expansion of genetic screening programs for inherited diseases have extended the impact of molecular biology to the public, including private citizens, insurance companies, and governments (Duster, 1981, 1987, in press). If only for the chance to gain highly marketable molecular biological skills, then, oncogene research became a good bet for graduate and postdoctoral students. A postdoctoral student training in an oncogene laboratory specifically stated his view that his research on oncogenes, with one foot in cancer and the other in molecular biology, would definitely get him a job in either the university or in industry.

Cancer research similarly was a well-funded arena, with a $1.2 billion budget in 1984 for the NCI alone ($1.3 billion for 1987). With additional funds from other National Institutes of Health, National Science Foundation, American Cancer Society, and other private foundations, cancer research was a thriving enterprise. At the intersection of the molecular biology and cancer research, oncogene research provided new researchers with important resources for building careers.

Oncogene problems also provide the advantage of results in relatively short time-frames. Most oncogene researchers publish from two to fifteen articles a year depending on their professional status, the size of their laboratories, and the number of collaborations. These short time-frames fuel the oncogene bandwagon and draw attention away from other areas like cell biology. One respondent said: "The reason that oncogene research is surging is that the work is doable. There is a productive methodology. And you get results quickly. . . ." He argued that the fault for this focus on short-term payoff lay with the supporters of research rather than with the researchers themselves.

Another senior investigator concurred that bandwagons develop because of time and task constraints on new investigators:

> How do waves get started and why do they occur? I think the reason for that has to do with the way science is funded. And the way young scientists are rewarded. . . . [A] youngster graduates and . . . gets a job in academia, for instance. As an assistant professor, he's given all the scut work to do in the department. . . . In addition, he must apply for a grant. And to apply for a grant and get a grant . . . nowadays, you have to first show that you are competent to do the [research], in other words, some preliminary data. So he has to start his project on a shoestring. It has to be something he can do quickly, get data fast, and be able to use that data to support a grant application . . . so that he can be advanced and maintain his job. Therefore, he doesn't go to the fundamental problems that are very difficult. . . . So he goes to the bandwagon, and takes one little piece of that and adds to that well-plowed field. That means that his science is more superficial than it should be. And that's bad for the field of science.

Thus, if it takes many more years to construct a doable problem using one approach versus another, most researchers choose the shorter, surer bet.

At this stage of the bandwagon's development, early oncogene researchers had circulated claims that problems framed in terms of their package were fast, highly doable, highly credible, and highly productive. They had lined up sponsors, integrated transportable recombinant DNA technologies, worked their way into the human cancer and normal cell genomes, and connected oncogenes to many other lines of research, including the "hot areas" of evolutionary biology and normal growth and differentiation. They had connected such heterogeneous things as human cancer, a class of genes called oncogenes, recombinant DNA technologies, normal growth and differentiation, and the National Cancer Institute. They had persuaded other researchers and funding agencies that, because of these connections, oncogene research would satisfy their interests and the demands put upon them by their sponsors, colleagues, students, and university administrations. With respect to many worlds within cancer research and molecular biology, then, the oncogene theory held high privilege

(Gerson and Star, 1984). Presented with such a highly privileged theory as an incentive in a race for time, new investigators jumped on the bandwagon.

Finally, new and more established investigators also bought the package because of promised intellectual payoff and new generations of downstream questions. Novel findings from oncogene research were not only useful for fattening publications lists for academic researchers. They were also intellectually "hot." For example, in 1983, *Nature* (Newmark, 1983:470) published an article in its "News and Views" entitled "Oncogenic Intelligence: The Rasmatazz of Cancer Genes." The article was just one of many published between 1978 and 1985 announcing exciting new findings from oncogene research. Researchers provided novel, intellectually exciting information and mined their findings for further experimental questions.

The intellectual excitement was not limited to oncogene research but extended to all molecular biological research. Almost all respondents, independent of their political views about how the new molecular biological technologies should be used, echoed this excitement. A respondent who had been conducting protein biochemical research explained the excitement in terms of a whole new level of analysis opening up to scientists:

> You can ask certain sorts of questions which you can't really answer with just the biochemical methodology. . . . Genetics essentially involves modifying what's already there, rather than simply describing what's going on. It allows you to ask much more specific questions about which components of the system are necessary to do what. Recombinant DNA technology is starting to allow one to ask those sorts of questions in animal cells, tumor cells. . . . Questions which there is as yet no other way of approaching.

Established researchers also found the possibilities for exploring new levels of analysis useful. An example was the senior investigator who had been studying the effect of radiation on transforming cells in culture. After much excitement about the oncogene theories of carcinogenesis, he sent his student to train in recombinant DNA techniques in a nearby laboratory in order to test two hypotheses: first, whether radiation played a role in mutating or transposing one or several proto-oncogenes and, second, whether radiation damage to cells made it easier for the viral oncogene to become integrated into the normal cellular genome. Thus, the graduate student gained the benefits enrolled behind the oncogene theory, and the senior investigator imported new skills and a new line of research into his laboratory. There appear then to be many benefits to be gained from pursuing oncogene research.

Incentives for Private Industry. Despite the uncertain commercial payoff from oncogene research, several large pharmaceutical companies and major research and development (R & D) companies have committed funds, researchers, and laboratories to oncogene research and recombinant DNA technologies (Koenig, 1985). These "betting" pharmaceuticals include Hoffman-La Roche Inc., Smith Kline Beckman Corporation, Merck and Co., and Abbot Laboratories. Investing R & D biotechnology companies include Genentech and Cetus and especially smaller companies aimed specifically at oncogene products including Oncogene Science Inc., Oncogen, and Centocor Oncogene Research Partners.

One respondent regarded these commitments as an effort to "get in on the ground floor." Even if a particular company is not the home of the desired new discovery that leads to a patentable diagnostic or therapeutic product, it will have established the infrastructure for early entry into the race to produce the final commercial product(s). A research director at Hoffman-La Roche stated, "If you're interested in [oncogene] products, you can't afford not to be in the race now" (Koenig, 1985:25).

Commercial biological material suppliers, however, could invest in oncogene materials only with the arrival of standardized technologies. They would not have been able to profit on high-cost, state-of-the-art technologies. Stephen Turner founded Oncor

> to capitalize on the emerging diagnostic market from recent developments in cancer molecular biology. . . . There is no clear-cut link between oncogenes and clinical claims, but I'm gambling that it will happen. It was a greater risk a year ago than today. Look at *Nature*; there are four to five articles per week about oncogenes (quoted in Johnson, 1984:18).

Turner also referred to other commitments made to this goal by Oncogen (another small biotechnology supply firm), a collaboration between Genetic Systems and Syntex, and a joint venture between Becton-Dickinson (a large, diversified biological research supply company) and Cold Spring Harbor Laboratory. In Turner's words,

> clearly this is a hot area. My business goal is not unique. . . . In cancer molecular biology, there is a need for standard reagents in highly convenient, quality-controlled assays that researchers can use to detect human and other species of oncogenes, genetic arrangements, gene amplification and gene expression (Johnson, 1984:18).

While Turner aimed his efforts at the long-term goal of supplying clinical researchers with tools for diagnosing tumors in human patients, his immediate clients were basic cancer researchers. They used the products to manipulate genes and tumor tissue in their fundamental research in test tubes, petri dishes, and lab animals. A well-worn photocopy of an advertisement for Oncor products from the journal *Science* was taped to the wall by one graduate student's bench in an academic oncogene research laboratory.

These commitments to oncogene research on the part of private industry refer back to similar commitments of new researchers. These commercial investments provide both job opportunities and more affordable research tools for new investigators jumping on the bandwagon.

The Bandwagon's "Snowball" Effect

By 1984 the molecular biological cancer research bandwagon was a distinct phenomenon. Scientists were acting on the basis of its existence. Researchers referred to the oncogene bandwagon in conversations. More generally, modern biology was molecular biology. The bandwagon had grown to the point where it was sustained by its own momentum. That is, researchers joined the bandwagon primarily because it *was* a bandwagon, its continued growth produced by a "snowball" effect.

Actors from diverse social worlds had committed their resources to molecular biological cancer research. These commitments included: (1) very large increases in funding allocations; (2) designated positions in academic departments, research institutes, and private industrial laboratories; (3) easily accessible training and tools, including knowledge, standardized technologies, materials, and instruments; and (4) a cadre of researchers training in molecular biological skills. That is, an infrastructure of skills, funding allocations, committed researchers and teachers, positions committed to molecular biologists, biological material suppliers, and even whole companies and research institutes committed to oncogene research problems was established by 1984. This infrastructure then constrained and influenced the decisions of new investigators. It served to maintain previous commitments as well as to gain new commitments.

Molecular biological cancer research by 1984 appeared to new investigators to be the research line of choice. Scientists joined the bandwagon in order to build successful careers. For many new researchers, this decision to jump on the bandwagon to construct and solve problems on cancer in molecular biological terms was independent of whether or not the problems would yield cures for cancer. Building individual and collective careers was their foremost concern. While curing cancer would be a welcome reward, it was only one consideration among many for their decisions to jump on the bandwagon.

Discussion

The research on which this paper draws is a study of change in science. It examines how the process of theoretical or conceptual shifts is inseparable from both the local and broad scale organization of work and technical infrastructures of science. In the molecular biological cancer research bandwagon, cancer was re-packaged as a disease of the cell nucleus and specifically of the DNA. Researchers in other lines of work had previously studied cancer as a disease of the cell, the immune or endocrine system, the entire organism, or the interaction between organism and environment. In the late 1970s and early 1980s, molecular biologists and tumor virologists developed a theory-method package and "marketed" it as a new and possibly more productive perspective on cancer. This new package consisted of a molecular biological *theory* of cancer and a *set of technologies* for testing and exploring the theory. They constructed the oncogene theory so that it mapped onto the intellectual problems of many different scientific social worlds. In addition, by the early 1980s, recombinant DNA technologies were standardized and thus highly transportable between social worlds. The combined advantages of the theory and technologies made the new package, and therefore the new definition of cancer, accessible and attractive to many different scientists, laboratories, organizations (including funding agencies), and lines of research.

Accessibility depends on the transportability of methods. Technologies are highly transportable when tasks and procedures are standardized, that is, conventionalized and routinized. Standardized procedures reduce the amount of tacit knowledge, discretionary decision-making, or trial-and-error procedures needed to solve problems. What is done to which material for what reason or purpose and with which outcome are all built into the "black box" of transportable technologies. In the molecular biological cancer research bandwagon, molecular biologists constructed standardized tools for manipulating DNA in higher organisms. They transformed "state-of-the-art" tools into routine tools and made it possible for established researchers in other biological specialties to move them into their own labs and for new researchers to gain access easily to these tools. Another important aspect of accessibility is funding. Since funding agencies had bought the package, getting research grants to incorporate the new tools was relatively easy. This combination of routine tools and available research funds resulted in decreased articulation requirements for those who joined the bandwagon.

My point about the package of theory and methods is that the package itself is another step in the construction of pre-packaged conventions for action. Scientists now manipulate a particular gene through a relatively straightforward series of steps with reference to the role that that gene plays in cancer. Oncogene researchers know not only how to manipulate the gene, but which gene to manipulate and for which purposes. They also have a framework within which to interpret the outcomes of such manipulations. Thus, scientists benefitted, first, from the standardization of recombinant DNA technologies and, secondly, from the "standardization" of the package of oncogene theory and recombinant DNA technologies.

The standardized package of the oncogene theory and transportable recombinant DNA technologies together served as an *interface* among different social worlds. An interface is the means by which interaction or communication is effected at the places "where peoples meet" or different social worlds intersect. A standardized package is an interface that operates simultaneously in many different social worlds as compared to an interface that links two social worlds. It is the mechanism by which multiple intersections occur.

In science, we see many intersections between two or three disciplines or lines of research, for instance, between biology and chemistry (Gerson, 1983a). Molecular biology itself is the product of the intersection of physics, chemistry, and biology. These intersections arose through long periods of detailed, repeated, and long-term negotiations among many participants and in many situations (see, for example, Clarke, 1985; and Gokalp, 1987). In oncogene

research and the molecular biology of cancer, we find an intersection among multiple different lines of work that arose very quickly.

The standardized package facilitated this bandwagon's rapid development because it facilitated interaction among members of different social worlds.[4] Its conventionalized ways of carrying out tasks (or standard operating procedures) allowed people in different lines of work to adopt and incorporate them into their laboratories and ongoing enterprises more easily and quickly. That is, it facilitated the flow of resources among many different lines of work. People in one line of research could rapidly and relatively easily adopt resources from another line of research and come to a common practice.

The success of molecular biologists' and tumor virologists' marketing efforts to many different social worlds was largely due to the following: first, the theory-method package provided procedures for a relatively straightforward construction of doable problems, or what Kuhn (1970) would call "normal science." Second, the experiments involved new, "sexy," recombinant DNA techniques, as compared with old, well-known routines. Third, early oncogene theorists demonstrated that, within their model, their doable problems quickly produced novel information about cancer at the molecular level. As scientists making maps of nature, they attempted to chart previously unexplored territory, that is, cancer at the molecular level of analysis. However, anyone entering uncharted territory will likely find something interesting, some new way of representing nature. Finally, then, this particular representation of cancer won so many allies because the theory-method package fit within the institutional and organizational constraints of scientific work of these multiple different social worlds.

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4. Whether theory-method packages play such significant roles in the development of other bandwagons is an empirical question. Other kinds of packages—for example, problem-data representations, problem-methods, methods-data representations, and other combinations of problems, methods, data representations, and theory—may serve as interfaces in other bandwagons. We also need comparative studies of bandwagons at different organizational scales: within and across lines of research, within and across disciplines, and across countries.

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