Crafting Science: Standardized Packages, Boundary Objects, and "Translation"  

Joan H. Fujimura

Post-Kuhnian sociology of science argues that nature is not directing the construction of scientific knowledge. Post-Mertonian sociology of science has focused on controversies in science and has taught us that consensus is a rarity rather than the norm. Instead, scientific work is heterogeneous in both method and substance. Many different kinds of worlds are involved in constructing scientific knowledge in numerous and diverse ways. The question then is, how are scientific knowledge and technology constructed without nature and consensus as frames? That is, how do these different worlds with different methodological and substantive concerns succeed in cooperating to produce new knowledge?

This paper focuses on two concepts which are useful for analyzing how collective action is managed across social worlds to achieve enough agreement at various times to get work done and to produce relatively (and temporarily) stable "facts." These two concepts were developed from two sets of studies where multiple social worlds intersected and managed to work relatively successfully together. The important point is that both concepts attempt to keep in the foreground the heterogeneous concerns of the different worlds involved.

One concept is what I have called "standardized packages" (Fujimura 1986, 1988). It consists of a scientific theory and a standardized set of technologies which is adopted by many members of multiple social worlds to construct a new and at least temporarily stable definition of cancer as well as a thriving line of cancer research. Another concept is Star and Griesemer's [1989] "boundary objects," examples of which facilitated the coordination of efforts of members of several different social worlds in building the Museum of Vertebrate Zoology at the University of California, Berkeley.

I begin with a brief analysis of the difference between the boundary objects concept and the network building concept of Latour (1987) and his colleagues. Although Star and Griesemer developed their concept in response to the network model, their aim is slightly different. While Latour is concerned more with fact stabilization, Star and Griesemer focus on collective work across worlds with different viewpoints and agendas. The differences in focus are important. The value of Star and Griesemer's work is precisely their focus, since the coordination and management of work across multiple and divergent actors, social worlds, meanings, and uses in producing science is often invisible in both scientific and social studies of science texts. However, because boundary objects are more easily reconstructed in different local situations to suit local needs, they are equally disadvantageous for establishing the kind of "stabilization" of allies behind "facts" which Latour discusses.

I argue that "standardized packages" is a concept which handles both collective work across divergent social worlds and fact stabilization. A package differs from boundary objects in that it is used by researchers to define a conceptual and technical work space which is less abstract, less ill-structured, less ambiguous, and less amorphous. It is a grey box which combines several boundary objects [in this case, genes, cancer, and cancer genes in proto-oncogene theory] with standardized methods [in this case, recombinant DNA technologies, probes, sequence information] in ways which further restrict and define each. Such codeliftion and corestriction narrows the range of possible actions and practices but does not entirely define them. Thus, using a package allows for a greater degree of fact

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I would like to thank Richard Burian, Adele Clarke, James Griesemer, Michael Lynch, Andrew Pickering, Leigh Star, and Anselm Strauss for their comments on an earlier version of this paper.

1. Stability as used here is constructed by social actors and is not assumed to represent reality.

2. As a caveat, I do not assume that social worlds, e.g., disciplines, are stable entities in nature or society. I agree with Keating et al. (in press), for example, that disciplinary boundaries are also constructed and therefore can be destabilized. What molecular biology is, for instance, has changed from its birth through its "molecularization" of other realms of biological research and biological institutions. The University of California, Berkeley, has recently organized its many biological subdisciplines into two general "divisions," molecular and cell biology and "integrated biology," in part because of the general molecularization of biology. Keating et al.'s (1991) view of disciplines as "dynamic, shifting stakes and not as purely static institutions" is similar to the definition of Strauss and colleagues (Buchler and Strauss 1961; Strauss 1978; Strauss et al. 1964) of social worlds as "negotiated orders." Indeed, social worlds is defined as "activities and practices."
stabilization than using boundary objects. Simultaneously, how-
ever, standardized packages are also similar to boundary objects in that they facilitate interactions and cooperative work between scientific worlds and increase their opportunities for being transferred into, and enrolling members of, other worlds. They serve as interfaces between multiple social worlds which facilitates the flow of resources (concepts, skills, materials, techniques, instruments) among multiple lines of work (Futamura 1988). An interface is the means by which interaction or communication is effected at the places where social worlds intersect (Hughes 1971) or by which multiple interactions occur. I present an example of a standardized package which facilitated the crafting of similarities or homologies between laboratories and continuous between interactions and laboratories. The combination of this well-crafted oncogene theory and standardized molecular genetic technologies created a formidable package for further translations to produce a new and highly privileged genetic representation of cancer.

Multiple Translations versus Machiavellian Actors: Collective Work versus Fact Stabilization

Laboratory studies have provided us with understandings of the bri-
colage, tinkering, discourse, tacit knowledge, and situated actions that build local understandings and agreements (Cambrosio and Kearing 1988; Collins 1985; Knorr-Cetina 1981; Latour and Woolgar 1979; Lynch 1985; Pinch 1986). Although histories of science have attended to the details of cross-situational studies of the construction of knowledge, sociologies of science have only recently begun to examine the collective construction of knowledge by different laboratories and especially by members of different social worlds through negotiation, aligning articulation, simplification, and tri-

In this last category, Callon's (1986), Latour's (1987), and Law's (1986) joint work proposes a compelling actor-network approach where actors' "interests" are translated in order to enroll them. However, especially Latour's presentation of this approach has been criticized as too Machiavellian a view in which scientific entrepre-
neurs-generalists go about waging war to conquer and discipline new allies. The disagreement may be based on problems with termino-
logy, the availability of information, and Latour's story-telling perspective. For example, in The Pasteurization of France (1988), Latour tells the story of Pasteur's attempt to spread his theory of the "microbe. While he also demonstrates that other actors enrolled Pasteur's "microbe in their efforts, Latour's focus is primarily on translations which facilitated Pasteur's network building.

In a recent paper Star and Griesemer (1989) shift the focus of Latour's model to the multiple translations present in scientific work. They use an "ecological" approach framed in terms of under-
standing science as collective action from the viewpoints of all the actors and worlds involved. This overviews of any one actor. The ecological approach is based on views which prevailed at the University of Chicago during the first half of the twentieth century and became embedded in the pragmatic perspective in philosophy and the symbolic interactionist school in sociology. It has only recently been used to study science. The ecological approach focuses on the multiple translation efforts through which scientific knowledge is constructed by standing in several positions in order to present multiple perspectives. All actors are simultaneously attempting to interest others in their concerns and objectives. The final (or temporary) outcomes of these efforts are constructed through the processes of negotiation, articulation, translation, triangulation, debating, and sometimes even coercion through "administrative persuasion" by members of different social worlds as actors attempt to install their "definitions of the situation" (Thomas and Znaniecki 1918, Hughes 1971) as the different worlds intersect.

Despite their effort to demonstrate multiple translations, how-
\[5. This view was expressed by two of the three speakers at a special symposium on Latour's Science in Action held at the 1988 meeting of the Society for the Social Studies of Science in Amsterdam. See Amsterdam 1990. See Kondo 1990 for a nice critique of Foucault's use of "powerful, worldlike terms for decentring the whole situation.

6. See Callon (1986, 1987) and Latour (1988) for efforts to take the position of nonhuman actors in the network. See also chapter 10 of this book for a critique of this effort and Callon and Latour's (chap. 12) response to the critique.

7. For more discussion and examples of the ecological or social-worlds approach to the study of science, see Clarke 1990, in press; Clarke and Futamura, in press; Futamura 1987, 1988, Fuuuras et al. 1987; Gerson 1983, Star 1988a, 1989, and Willber 1983.

8. While they are not laboratory studies per se, Lawn 1988 and Sismondo 1987 also belong to this category of studies of local practice.

stabilization than using boundary objects. Simultaneously, however, standardized packages are also similar to boundary objects in that they facilitate interactions and cooperative work between scientific worlds and increase their opportunities for being transferred into, and enrolling members of, other worlds. They serve as interfaces between multiple social worlds which facilitates the flow of resources (concepts, skills, materials, techniques, instrumentation) among multiple lines of work (Fujimura 1988). An interface is the means by which interaction or communication is effected at the places "where peoples meet" (Hughes 1971) or different social worlds intersect, it is a matrix within which multiple intersections occur. I present an example of a standardized package which facilitated the crafting of similarities or homologies between laboratories and continuities between inscriptions and laboratories. The combination of this well-crafted oncogene theory and standardized molecular genetic technologies created a formidably package for further translations to produce a new and highly privileged genetic representation of cancer.

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In this last category, Callon's (1986), Latour's (1987), and Law's (1986) joint work proposes a compelling actor-network approach where actors' "interests" are "translated" in order to enroll them. However, especially Latour's presentation of this approach has been criticized as too Machiavellian a view in which scientific entrepreneurs go about working to conquer and discipline new allies. The disagreement may be based on problems with terminology, the availability of information, and Latour's story-saling perspective. For example, in The Pasteurization of France (1988), Latour tells the story of Pasteur's attempt to spread his theory of the microbe. While he also demonstrates that other actors enrolled Pasteur's microbe in their efforts, Latour's focus is primarily on translations which facilitated Pasteur's network building.

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ever, Star and Griesemer's (1989, 490) case study is still hampered by the same difficulties faced by Latour. That is, their approach is also constrained by the availability of information and its associated story-telling perspective. Their story is based primarily on archival records, papers, and letters of Anne Grinnell and Annis Alexander, who respectively directed and organized and funded the building of the Museum of Vertebrate Zoology at the University of California, Berkeley. Whose story gets told depends on whose life is recorded in more detail. Thus, their story is framed more in terms of the organizational and management work done by the two main characters in building the museum.

Star and Griesemer's focus on the building of the museum also distinguishes their work from Latour's. While Latour focuses on Pasteur's strategies and negotiations among social worlds to stabilize his theory of the microbe into "fact," Star and Griesemer are concerned with the problem of how members of different social worlds manage to successfully cooperate, in this case, to build the museum and to construct scientific representations despite their different viewpoints and agendas. Cooperation, they argue, is necessary to create common understandings, to ensure reliability across domains, and to gather information which retails integrity across time, space, and local contingencies. But it is not presupposed or consensual. The strength of Star and Griesemer's paper lies in its focus on the viewpoints and concerns of all the participants, as far as possible, involved in building the museum.

The various actors and their interests in Star and Griesemer's study included university administrators who were attempting to make the University of California, Berkeley, into a legitimate, national-class university, amateur collectors who wanted to collect and conserve California's flora and fauna, professional trappers who wanted skins and furs to earn money, farmers who served as occasional field-workers, Annie Alexander, who was interested in conservation and educational philanthropy, and Joseph Grinnell, who wanted to demonstrate his theory that changing environments are the driving forces behind natural selection, organismal adaptation, and the evolution of species.

Star and Griesemer's contribution to the problem of how members of different social worlds interact is a new concept, boundary objects. They argue that boundary objects facilitate the multiple transactions needed if we assume that nature is not directing the show to engineer agreements among multiple social worlds (Star and Griesemer 1989, 393).

Boundary objects both inhabit several intersecting worlds... and satisfy the informational requirements of each of them. Boundary objects are objects which are both plastic enough to adapt to local needs and constraints of the several parties employing them, yet robust enough to maintain a common identity across sites. They are weakly structured in common use, and become strongly structured in individual-site use. They have different meanings in different social worlds but their structure is common enough to more than one world to make them recognizable, a means of translation.

Star and Griesemer propose that boundary objects, along with standardization of methods, were the means by which Joseph Grinnell and Annie Alexander managed the tension between heterogeneity and cooperation in their efforts to build the Museum of Vertebrate Zoology. The specific boundary objects included the museum itself as a repository, ideal-type concepts like species and diagrams, coincident boundaries like the outline of the state of California, and standardized forms like the forms Grinnell developed for trappers and amateur collectors to fill out when they obtained an animal.

These boundary objects emerged through the processes of work when the work of multiple groups coincided. They were not engineered by one individual or group. Rather, Star and Griesemer's story tells us how Grinnell managed these objects in such a way as to create the means for accomplishing the construction of his museum and theory. Grinnell first reconstructed California as his "laboratory in the field." California, the boundary object, was of interest to several of the participating groups. He then used this laboratory to transform himself into a preserver and conservor of California to gain support in the form of work and funding from Annie Alexander and other conservationists. Using the collected specimens and standardized information, he was able to construct unique ecological theories of evolution. Grinnell thus was able to coordinate the work of several different social worlds using several boundary objects for which each group had a different meaning and partial jurisdiction.

Moreover, the process of management became embedded in Grinnell's theoretical constructions. "Grinnell's managerial decisions about the best way to translate the interests of all these disparate worlds shaped [not only] the character of the institution he built but also the content of his scientific claims" (Star and Griesemer 1989, 392). Griesemer (1990, 1991) argues elsewhere that the museum was Grinnell's method and data base for demonstrating his...
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theory. It was the total museum with its ecological information that he considered important for substantiating and instantiating his theory. However, Griesemer argues that Grinnell concentrated more on specifying a standardized methodology and failed to articulate his broad theoretical views with the "methodological nitty-gritty."

Rather than focusing on promoting and teaching his theory, he only promoted and taught his standard methods of collecting for building his method and data representation. Grinnell concentrated his efforts on standardizing his methods in order to get the precise ecological information he needed, along with the specimens from traps, farmers, and amateur collectors. The result was the disappearance of his theoretical aims from contemporary biological theory even as his careful methodology lives on. It required Griesemer's careful study of the organization of the museum's layout of specimens and Grinnell's papers in the early 1980s to reconstruct and promote the theory embedded in the museum's organization.

It should be clear by now that Star and Griesemer discuss the collective work involved in the construction of museums, claims, or theories, while Latour discusses the "hardening" of claims or theories into "facts." Grinnell used and constructed boundary objects like the "standardized methods and forms to construct his theory qua museum. His museum, in turn, also serves as a boundary object used by downstream users from different social worlds for divergent purposes [e.g., by different scientists with diverse theories, conservation groups, the university administration]. It is in the effort to harden his theory into fact that Grinnell failed, and this is where the very ambiguity of boundary objects which support joint organization of work across social worlds leads to the transformation of the claim it supports. Since meanings are not embedded in boundary objects, divergent uses, interpretations, and reconstructions are likely. Thus, for example, Grinnell's theory was lost, while his museum and his standardized methods continue to provide materials and methods for contemporary researchers. Multiple interpretations and uses are not necessarily a bad thing, especially for peaceful coexistence and theoretical and social change, but they are problematic for theoretical entrepreneurs, unsuccessful or successful, like Grinnell and Pasteur.

I argue, then, that although boundary objects promote collective action and coherence of information from different sites because they are more easily reconstructed (re-represented) in different local situations to fit local needs, they are equally disadvantageous for establishing the kind of "stabilization" of allies behind "facts" which Latour (1987) discusses. That is, while boundary objects can promote translation for the purpose of winning allies, they can also allow others to resist translation and to construct other facts. They have a wider margin of negotiation. Latour [1987, 208-9] discusses this issue in terms of the quandary of fact builders. "They have to enrol so many others so that they participate in the continuing construction of the fact ... but they also have to control each of these people so that they pass the claim along without transforming it either into some other claim or into someone else's claim. ... Each of the potential helping hands, instead of being 'conductor' may act in multifarious ways behaving as 'multi-conductor.' They have no interest whatsoever in the claim, shunt it towards some unrelated topic, turn it into an artefact, transform it into something else, drop it altogether, attribute it to some other author, pass it along as it is, confirm it, and so on."

Latour focuses on translation efforts to stabilize facts, while Star and Griesemer's concept of clastic boundary objects promotes our understanding of translation efforts in the management of collective work across social worlds. The strength of the concept of boundary object lies in its attention to multiple and divergent actors, social worlds, meanings, and uses. Star and Griesemer argue that boundary objects are often ill-structured, that is, inconsistent, ambiguous, and even "illogical." Yet they serve to accomplish the work to be done as defined by the actors involved. Since the local viewpoints (interests, requirements, desires, languages, methods) of different groups are usually not identical, rigid or strongly structured entities are less likely to be able to absorb divergent instances and still maintain internal coherence or robustness.

There are both difficulties and interesting new questions in Star and Griesemer's work. What is the meaning of "getting the work done?" Whose work? Which work? For example, Grinnell succeeded in getting one of his jobs done: he built a museum, still a going concern. However, the museum (a boundary object) was and is used by many actors constructing their theories of speciation, evolution, and other things, while Grinnell's own theory disappeared. How did Grinnell's and Alexander's museum building affect the work of 8. Star and Griesemer's discussion differs slightly from Pickering's example of constructing coherence. Pickering (1990) focuses on the practice of theory construction and representation in his study of coherence formation in Morpurgo's research. His concept is more similar to my concept of problem path (that is, the simultaneous construction of problem and solution in an ongoing process) (Fujimura in preparation).
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farmers and trappers? How do abstract concepts like species differ from standard forms? What are the differences between standardized forms and standardized methods? Despite the difficulties, however, the concept is valuable for its emphasis on the coordination and management of work across worlds.

Standardized Packages, Collective Work Across Worlds, and Fact Stabilization

I now want to focus on a more specific concept, standardized packages, which facilitates both collective work by members of different social worlds and fact stabilization (Fujimura 1986, 1988). A package differs from a boundary object in that it defines a conceptual and technical work space which is less abstract, more structured, less ambiguous, and more concrete. It is a gray box which combines several boundary objects (gene, cancer, oncogene or cancer gene) with standardized methods (in this case, recombinant DNA technologies) in ways which further restrict and define each object. Such codification and corestriction narrow the range of possible actions and practices, but also do not entirely define them. These properties of a package allow for a greater degree of "fact (and skill) stabilization" and less of the undermining which concerns Latour [1987, 208, 1990]. Simultaneously, however, a standardized package is also similar to a boundary object in that it facilitates interactions and cooperative work between social worlds and increases its opportunities for being transferred into, and enrolling, other worlds; it serves therefore as an interface between multiple social worlds.

This combination of narrowed "work space" or range of possible practices and cross-world bridge properties is what builds bandwagons. I developed the concept of standardized packages in my earlier work to understand why and how the molecular biological bandwagon in cancer research developed (Fujimura 1986, 1988, in preparation). In my case the package consisted of a scientific theory and a standardized set of technologies which succeeded in enrolling many members of multiple social worlds in constructing a new and at least temporarily stable definition of cancer.

The molecular biology cancer research bandwagon represents a major reorganization of commitments in cancer research and a major change in the organization of work for scientists and organizations. My question was how members of so many different social worlds came to agree to participate in or support molecular genetic studies of cancer, and especially studies framed in terms of a single theory of cancer. The cancer research arena previously had a host of different definitions of cancer that were developed and used by multiple lines of basic research and medical practice. Why would scientists and organizations with already-existing resource investments in different lines of research reorganize their commitments to pursue a new approach to understanding cancer? How did they choose to commit their resources to the particular new approach? How do members of different social worlds come to practice a common approach to studying cancer? I proposed that the "translation" and "interestlessness" of members of multiple social worlds was facilitated by a standardized package of theory and methods, specifically the proto-oncogene theory and recombinant DNA and other molecular genetic technologies, which could be used to get work done by these many worlds, for example, researchers in many different laborato ries could use it to construct and solve "doable" problems. I argued that this theory and set of methods together were used to reorganize the work yet maintain stability, integrity, and continuity in several social worlds: in laboratories in many different biological subdisciplines and medical specialties, science funding agencies (National Cancer Institute, American Cancer Society), the U.S. Congress, in cancer research institutes, in university departments and administrations, and in biological supply organizations.

Indeed, the growth of the oncogene bandwagon in cancer research was due to this capacity for maintaining the integrity of the interests of the enrolled worlds while simultaneously providing them with new tools for doing their work. For scientists in other lines of research, the theory-method package provided a theory and procedures for constructing new doable problems and the introduction of new, "sexy," recombinant DNA techniques, to augment or replace their old, well-known routines. At the same time, the oncogene theory did not challenge the theories to which the researchers had made previous commitments. Indeed, the new research provided them with ways of triangulating, of providing new evidence using new methods to support their earlier ideas. For funding organizations, it provided a means of
farmers and trappers. How do abstract concepts like species differ from standard forms? What are the differences between standardized forms and standardized methods? Despite the difficulties, however, the concept is valuable for its emphasis on the coordination and management of work across worlds.

**Standardized Packages, Collective Work Across Worlds, and Fact Stabilization**

I now want to focus on a more specific concept, standardized packages, which facilitates both collective work by members of different social worlds and fact stabilization (Fujimura 1986, 1988). A package differs from a boundary object in that it defines a conceptual and technical work space which is less abstract, more structured, less ambiguous, and more concrete. It is a gray box which combines several boundary objects (gene, cancer, oncogene or cancer gene) with standardized methods (in this case, recombinant DNA technologies) in ways which further restrict and define each object. Such codefinition and corestriction narrow the range of possible actions and practices, but also do not entirely define them. These properties of a package allow for a greater degree of “fact (and skill) stabilization” and less of the undermining which concerns Latour [1987, 208, 1990]. Simultaneously, however, a standardized package is also similar to a boundary object in that it facilitates interactions and cooperative work between social worlds and increases its opportunities for being transferred into, and enrolling, other worlds; it serves therefore as an interface between multiple social worlds.

This combination of narrowed “work space” or range of possible practices and cross-world bridge properties is what builds bandwagons. I developed the concept of standardized packages in my earlier work to understand why and how the molecular biological bandwagon in cancer research developed [Fujimura 1986, 1988, in preparation. a]. 9

In my case the package consisted of a scientific theory and a standardized set of technologies which succeeded in enrolling many members of multiple social worlds in constructing a new and at least temporarily stable definition of cancer.

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justifying past investments whose legitimacy and productivity had been questioned, a tool for organizing and marketing their new funding agenda, and new hope for solving and possibly curing the problem of this virulent dread disease to present to Congress. The National Cancer Institute [NCI], for example, used this new research to lobby Congress for increased appropriations. For Congress, it provided its members with new hope to present to their constituents. For private industry, it provided a new line of products to produce and market in the then slow biotechnology business (Johnson 1984). For university administrators, it provided a means and justification for reorganizing "old-fashioned" cancer research institutes into what seemed to be more fashionable, "hot" molecular biology institutes. In other words, the package gave many different worlds ways of continuing their lines of work while simultaneously introducing novelty. For downstream users, the package constituted conventionalized ways of carrying out tasks [or standard operating procedures] which 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 [concepts, skills, materials, techniques, instruments] among multiple lines of work. People in one line of research could rapidly and relatively easily adopt resources from another line of research and come to practice work in common. As such, it also 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. It is the mechanism by which multiple interactions occur.

My argument was that the proto-oncogene theory was constructed as an abstract notion, a hypothesis, using a new unit of analysis to study and conceptualize cancer. This abstraction was general and specific enough to allow researchers in many extant lines of research to interpret the theory to fit their separate concerns all under the rubric of oncogene research. Further, the theory relied on recombinant DNA and other molecular biology technologies which by the early 1980s were standardized and conventionalized enough to be portable from molecular biology laboratories to other biological laboratories. This combination of the abstract, general oncogene theory and the specific, standardized technologies converted the novel idea into a routine. That is, the combination allowed other researchers with ongoing enterprises to locally concretize the abstraction in different practices to construct new problems, and routinization allowed the new idea to move to new sites and be inserted into existing routines with manageable reorganization.

Note that I do not regard the theory/method package as constituting a necessary connection. The coupling of the oncogene theory and recombinant DNA with other molecular biology technologies is constructed and not born in nature. The theory might in the future continue to exist as an entity separate from these techniques or coupled to another set of techniques. For example, the provirus theory, which many tumor virologists consider the precursor to the present proto-oncogene theory, was coupled with traditional virological techniques e.g., Duesberg 1963, 1985). Similarly, the technologies are coupled with quite different theories in other lines of biological research. I will discuss this issue further in the conclusion. In the next section I discuss the construction of the oncogene theory and its advantages for enrolling others in many different lines of research with a single definition or representation of cancer as one entity. This construction and its success at enrolling others are

10. Interview with Vincenzo de Vita, former director of NCI.

11. Interviews with respondents at the University of California, Berkeley, the University of California, San Francisco, and with a former member of the Memorial Sloan-Kettering in New York. See also Boley 1987 and Mone 1989 on Sloan-Ketterings more general shift in research from immunological to molecular biology approaches to understanding cancer.
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12. While they were new and "hot," recombinant DNA and other molecular genetic techniques for manipulating DNA in eukaryotic organisms (including humans) were also, by 1980, standardized and therefore highly transportable. That is, despite popular views of its state of the art status, the protocols or requisite tasks and procedures were conventionalized and routinized in cookbook recipes and in ready-made materials and instruments. Standardized procedures reduce the amount of tacit knowledge, discretionary decision making, or trial-and-error procedures needed to solve problems. That is, what is done to what material for what reason or purpose and with what outcomes are all built into the black box of transportable technologies. By the early 1980s, molecular biologists had transformed state of the art tools into routine tools and made it possible for researchers in other biological specialties to be able to move these tools into their labs and for new researchers to relatively easily gain access to the tools. However, as I argue elsewhere (Pfurtscheller 1986, 1987), articulation is never entirely eliminated even for black boxes. If we look more closely at reconstruct DNA techniques, as Barnham and Lynch have done, we see that tacit knowledge has not disappeared from DNA manipulations. Even relatively mundane techniques like plasmid prep (a basic prep technique in recombinant DNA technology) involve much tacit or local knowledge, uncertainty, and dispute. Nevertheless, novices can pick up basic plasmid prep techniques on their own from manuals and short visits with experts and without lengthy apprenticeships in other laboratories. The difference is one of degree.
based on the use of component parts which can be called boundary objects. Concepts such as "gene," "cancer," and "cancer genes" incorporated in the oncogene theory allowed members of many social worlds to adopt and adapt it while simultaneously maintaining the integrity of their local projects. However, unlike boundary objects, I argue that the package fundamentally changed local practices in enrollement scientific laboratories in ways which extend and solidify "[harden]" molecular genetic representations of cancer.

Crafting the Oncogene Theory

In this section, I will focus more on how and why the oncogene theory was so successful at translating the interests of so many actors. I do not assume that the theory so closely mapped nature, so closely mapped the way that genes actually cause cancer in nature, that researchers, funding agency administrators, Congresspeople, and private entrepreneurs were convinced of its validity. Instead the plausibility and success of the oncogene theory are due to a great deal of work and the use of several key concepts and techniques which can reconcile multiple conflicting viewpoints which, in turn, allow many different groups or social worlds to cooperate in using the theory and techniques.

Scientific knowledge about cancer is constructed at the intersection of many different social worlds. There is no one world which owns the problem or the solutions. The problem of cancer is distributed among different worlds, each with its own agenda, concerns, responsibilities, and ways of working.

Clinicians frame their problems in terms of individual cases, individual patients, and standard operating procedures: how do we best treat the person given present knowledge? Medical researchers (in the fields of radiology, epidemiology, oncology, endocrinology, neurology, and pathology) work with both patients and theoretical abstractions which they construct using many cases distributed through time and space. How many patients respond to this treatment in which way? What can we say about initiation and progression of the disease when examining a number of patients over time? Basic researchers (in the fields of genetics, virology, cell biology, organismal biology, molecular biology, immunology, and neuroscience) work with theoretical abstractions and material models. How can we duplicate the cancer process in mice or cultured cells in order to use it as a tool for studying the disease? What are the origins of cancer? Among medical and basic researchers, the questions can be broken down further. What is the role of the endocrine system in causing, promoting, or retarding the initiation or growth of the disease? What is the role of chemicals, of radiation, of viruses? What are the molecular mechanisms for the initiation and progression of the disease at the levels of gene and cell? Epidemiologists track the diseases as they appear in their different manifestations (breast, liver, colon, lung, brain, cervix, prostate) across families, racial and ethnic groups, countries, parts of countries, etc. On the other end of the scale pathologists examine cells in culture taken from tumor tissues. The point here is that participants in many different worlds work with cancer. While pathologists and physicians often interact, participants in different cancer research worlds tend to go on with their research with only cursory acknowledgment of events and research outside their narrow lines of research. There has been a proliferation of theories, methods of study, and treatments for the diseases, yielding successful treatments for a few of the leukemias, but so far no genuinely successful treatment or cure for solid tumors. Just looking at the library shelves of books, at the scores of journals and articles on cancer, can be daunting. While we have long assumed that there is some central "thing" linking these multiple representations (definitions, theories, methods, treatments), numerous attempts to "find" this elusive common denominator have failed (Shumkin 1977). Nevertheless, every so often there is a call for integration of the various lines of work. Usually these calls are ignored, not out of malice, but because of momentum and existing commitments to projects and because of difficulties in integrating the different approaches. These different worlds are working with different units of analysis, different representations of data, different scales of time and space, and different audiences.

Occasionally, however, a line of research, an approach, or a theory gains immense fame across the different worlds. Oncogene research is one such example. How does one theory gain so many adherents? I suggest several answers elsewhere (Fujimura 1986, 1988). Here I want to focus on the role of boundary objects and standardized tools in facilitating the translation of "oncogenes" from world to world to produce a robust theory.

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Positions

Using boundary objects to re-pose cancer and maintain plurality

Because of the multiple ownership of, and collective work on, the problem of cancer, members of different social worlds have to successfully cooperate to construct scientific representations despite having different viewpoints on cancer. Yet as Star [1988, 9] argues, compromise between multiple and sometimes conflicting understandings and ways of dealing with phenomena while maintaining the integrity of each viewpoint is difficult to achieve. Compromise usually tends to work against pluralism, where each viewpoint maintains its own integrity. How can two entities (or objects or nodes) with two different irreconcilable epistemologies cooperate?

There are several complex answers to the question of how cooperation is accomplished. I will present just two scenarios as my partial answers. The first is a brief example of a relationship between three groups of cancer researchers which allowed two of them to keep their work going through negotiation, nagging, and mutual support, or what they call "politics." This example is about hands-on work, about how researchers manage to gain the materials they need to do their research. The second scenario portrays a set of negotiations among more abstract entities. I will demonstrate that in the crafting of the oncogene theory, researchers translate the concerns of other lines of work. Boundary objects are critical elements in both of the scenarios.

Scene 1: From operating room waste to research material to research funding

In the first example, cells and cancer are concepts with different meanings in different situations. "Norma Oakdale," a cell biologist, studies the complexity of normal epithelial cells as they become or do not become cancerous. One of her primary goals is to improve early detection and successful treatment of human breast cancer. Oakdale's work is based on the assumption that each individual's cancer is different than any other. By growing cells of each patient's tumor in culture, she can then test different treatments (e.g., various chemotherapies, prepared antibodies, hormones, etc.) in vitro and then determine the best treatment for each patient and tumor before administering it in vivo. Oakdale and her colleagues built an institute to conduct this research and test these ideas. They located it next to a hospital in Oakland, California, where surgery on both normal and cancerous breast tissue was performed. She chose a hospital which was located near a residential community in order to make it easier for the institute to obtain breast fluid secretions from women on a regular, routine basis. Here I want to point to the appearance of breast tissue cells and cancer in several different worlds interacting with the institute's work.

Human epithelial cells (cells that line the membranous lining tissues of the organism) are very difficult to grow in culture. "I grow [normal breast cells] in tissue culture in the lab. I get a very small sample from you, for example, I will amplify it to a very large number. And I can then do biochemistry on them. Now that took me thirty years. I said it in one sentence, but it took me thirty years to learn how to grow those cells in culture." The few researchers who are successful at doing all this know each of other. They constitute a very small club. In part to provide a resource for her research and in part to support the institute, Oakdale grows normal and cancerous epithelial cells in culture. These cells are needed by other biological researchers for experimental purposes and were supported for a long time by National Institute of Health (NIH) grants in part because they were also necessary for experiments carried out in much more visible research. In the late 1970s, across San Francisco Bay in a microbiological laboratory at the University of California, San Francisco, Medical Center, Michael Bishop and Harold Varmus were doing research which they decided required human epithelial cells grown in culture. Instead of taking the time to grow the cells themselves, they chose to "buy" them from Oakdale's laboratory. Bishop and Varmus's laboratory did not have the time and patience, but it did have the influence to support the work done elsewhere.

It's a lot of politics, just simple politics and people interaction [to get funded by NIH]. Now, see, I've been funded to do something very fundamental and basic, and perhaps not very exciting, because Bishop and Varmus like to call me up on the phone and say, 'I need cells of such-

16. Surgeons refer to normal tissue removed as reduction mammoplasty.
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and such. Where can I get them?" Or, "Do you have them?" Or, "Will you make them for me?" And they like to have a resource like that. So my kind of work is in their interest to see that it's done. So they'll suggest it. At some time, they need to take DNA from human cells. We've already done it on a small scale with them.

To Oakdale and her colleagues, both cancerous and normal breast tissue cells are the objects of study. Cancer cells are "cancer" in their work. I do not suggest that these researchers believe that cancer cells are equivalent to cancer in the organism. I do suggest that their representations of "cancer" are constructed from their work on cancer cells and their experiences as cancer victims. To Bishop and Varmus, human cells are primarily sources of the valuable DNA (normal and transforming) with which they work and are sometimes used as testing grounds for the transformative (cancer-causing) properties of their DNA. Since human epithelial cells are so difficult to grow, Oakdale's cells have been transformed into sources of funds through Bishop and Varmus.

Growing human epithelial cells also requires interactions with doctors in order to obtain the human source tissues. Getting the "fresh" human tissue needed to grow epithelial cells is a difficult task for a number of reasons. Human tissue means different things to physicians and pathologists. Normal breast tissue is material to be routinely discarded in a bucket of formalin, which kills the cells. It is waste. Breast tissue diagnosed as cancerous is viewed as material for further analysis by pathologists, and otherwise as waste and as the disease cut away from the patient. Tissue is also sometimes regarded as a legal threat in the hands of others. For all these reasons, surgeons, operating room staff, and pathologists need to be persuaded to provide the tissue. For example, besides having "ego" difficulties, physicians and pathologists fear that researchers will find something they have missed and will subject them to malpractice suits.

The major one is that the clinicians won't cooperate. . . Ego, primarily. [The pathologist] take a little piece [of tissue], and then they throw the rest away. When a breast is taken off, for example, a pathologist will take samples from various places—little tiny samples—and then he throws the rest away. Even though I'm sitting here dying to have it. And other research scientists as well. And if you went to the woman who gave up her breast, and you asked for it, she'd say, "Gee, I don't need it anymore! If you can use it and it can be of some use to the world, have it with my blessing!" I've never had a patient turn me down. But I've had doctor after doctor after doctor. They're afraid. They're afraid I will find something that they missed. And they'll be sued. It's money. Basically money. And ego. They do not want you to suggest in any way that they missed something.

In order to overcome the legal concerns, Oakdale argued that her research on the cells could lead to better treatments and early detection of cancer, which would increase the cancer survival rate. Yet even if malpractice threats are defused, retraining surgeons and operating room staff out of old habits is another problem to be overcome. "Operating rooms and teams in operating rooms develop habits. Over the last thirty years, what they've always done with tissue is take it out, throw it in a bucket of formalin, a fixative. And once they've done that, it's finished as far as research is concerned. So that the difference in getting an operating room team to have an empty bucket or a bucket full of formalin is retraining, and that's hard to do. . . Formalin . . . inactivates—it denatures the protein. So that's a real hard problem—retraining."

Finally, while this might seem to Oakdale to be a bad habit, to physicians and surgeons it seems to be a good habit. They are not researchers. To them residual tissue is all so much waste. Retraining them to take the researcher's point of view is something that Oakdale and her colleagues, despite great effort, have not yet managed on a permanent basis. It still remains a daily task. For example, besides having "ego" difficulties, physicians and pathologists fear that researchers will find something they have missed and will subject them to malpractice suits.

Clinicians do not understand research, and how repetitive it is. So they'll say, "Well, I gave you one of those tumors two years ago, what did you do with that?" They don't understand that you need to look at them over and over and over again. So they give it to me! Once, they think that's all they need to do. So you have to tell them every single day. I get the OR [operating room] report here, I look at it, I know what surgeons or pathologists are doing in their operating room, and I have to call them. Every single time. I've been doing that for eight years. And if I miss one, if I'm busy and I miss one, and they dump it down the drain, they say, "Oh I didn't realize you were still collecting tumors!" . . . it's frustrating, I'll tell you. I have to have a staff that does nothing but collect specimens. I'm a mindless person, goes and gets them, processes them. I have a liquid nitrogen tank. I can freeze and store and reconstitute cells in liquid nitrogen. So I have a liquid nitrogen bank here that is a unique resource in all the world.

Thus, tissue usually thrown into buckets of formalin becomes material for research in Oakdale's institute and in Bishop and Varmus's experimental research on oncogenes. At the same time, through Bishop and Varmus's support, tissues also turn into money.
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and such. Where can I get them?" Or, "Do you have them?" Or, "Will you make them for me?" And they like to have a resource like that. So my kind of work is in their interest to see that it's done. So they'll suggest it. At some time, they need to take DNA from human cells. We've already done it on a small scale with them.

To Oakdale and her colleagues, both cancerous and normal breast tissue cells are the objects of study. Cancer cells are "cancer" in their work. I do not suggest that these researchers believe that cancer cells are equivalent to cancer in the organism. I do suggest that their representations of "cancer" are constructed from their work on cancer cells and not on cancer as experienced by organisms. To Bishop and Varmus, human cells are primarily sources of the valuable DNA (normal and transforming) with which they work and are sometimes used as testing grounds for the transformative (cancer causing) properties of their DNA. Since human epithelial cells are so difficult to grow, Oakdale's cells have been transformed into sources of funds through Bishop and Varmus.

Growing human epithelial cells also requires interactions with doctors in order to obtain the human source tissues. Getting the "fresh" human tissue needed to grow epithelial cells is a difficult task for a number of reasons. Human tissue means different things to physicians and pathologists. Normal breast tissue is material to be routinely discarded in a bucket of formalin, which kills the cells. It is waste. Breast tissue diagnosed as cancerous is viewed as material for further analysis by pathologists, and otherwise as waste and as the disease cut away from the patient. Tissue is also sometimes regarded as a legal threat in the hands of others. For all these reasons, surgeons, operating room staffs, and pathologists need to be persuaded to provide the tissue.

For example, besides having "ego" difficulties, physicians and pathologists fear that researchers will find something they have missed and will subject them to malpractice suits.

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research hub for Oakdale’s institute. These transformations [or translations] require careful, patient, and time-consuming management on the part of Oakdale and her colleagues.

To summarize, “cells” and “cancer” are sometimes different things to doctors, operating on the cancer cell biologists, and oncogene researchers. Yet they are similar enough to allow Oakdale and her colleagues to translate others’ concerns in order to satisfy their research requirements. If they do for Oakdale to do her research, she needed to coordinate her efforts with the work styles and interests of these different groups. Since she did not have the power to demand obedience, she had to persuade, cajole, beg, and cooperate to get them to act in her interest to preserve and give her living breast tissue cells. We see then that surgeons, patients, oncologists and operating-room staff in hospitals, and women in the community and invisible participants in the construction of the oncogene theory through work like Oakdale’s and the supply of research materials.

Scene 9: Crafting the oncogene theory using boundary objects

For the second scenario, I present the broader-scale crafting of the oncogene theory with an emphasis on the use of boundary objects in the processes of translation, triangulation, and re-representation. The point I want to make here is that the oncogene theory uses a package of boundary objects and specific, standardized tools which make it possible for different worlds to cooperate in constructing a robust theory. Boundary objects in this case include concepts like genes, cancer, cancer genes, viral genes, cells, tumors, development, and evolution, which are quite plastic terms and often have different meanings for the various groups. The theory also relies on data bases of sequences which are standardized tools. Data bases allow different lines of research to share information on gene and protein sequences. These sequences allow different lines of research on evolution, cancer, and normal growth and development to interact in ways that had not previously been possible.

Oncogene theorists, including the aforementioned J. Michael Bishop and Harold T. Varmus, working in the late 1970s in a micro-

biology laboratory at the University of California, San Francisco, Medical Center, drew on boundary objects and standardized tools to construct a theory which mapped onto the intellectual problems of many different scientific social worlds. For some translations (of cancerogenesis, etc.), they recrafted existing lines of research using a new unit of analysis. For other translations (of developmental biology—normal growth and development), they constructed equivalences between previously inequivalent units of analysis. For yet other translations (such as of viral oncology), they constructed continuities through time and space while introducing novelty into the scheme.

By using the concepts of genes, cancer, cancer genes, viral genes, cells, tumors, development, and evolution and standardized tools, especially data bases of sequence information, oncogene theorists succeeded in constructing working relationships with biologists in evolutionary biology and population genetics, medical genetics, tumor virology, molecular biology, cell biology, developmental biology, and carcinogenesis. The concepts were used quite loosely to allow for both variability among viruses and specificity within work sites, while the tools were used very specifically. It is this combination that allowed researchers in several fields of biology to draw on each other’s work to support and extend their own lines of research and to harden their theory into fact. I will sketch a few of these interactions, which in turn show how important this combination of ambiguous concepts and specific, standardized tools were to the development of a stable oncogene theory and to the development of the handbag in oncogene research.

Between tumor virology and evolutionary biology: Proto-oncogenes

During the 1960s and early 1970s, tumor virologists extended their research on viral oncogenes to develop the concept of normal cellular genes as causes of human cancers by borrowing and using the concept of gene conservation from evolutionary biology. Tumor virologists reported that they had found specific “cancer” genes in the viruses which transformed cultured cells and caused tumors in laboratory animals. This experimental work was done using traditional virology and molecular biology methods to investigate RNA tumor viruses. 19

19. Since the time of the original interview, the National Cancer Institute has made some effort to assist accrual of specimens through legislation to protect human subjects in experimental research and through establishing regional collection networks. However, recent legal suits for property rights over commercial products constructed from tissue taken from patients have further complicated the acquisition of research materials.

20. RNA virus tumors are retroviruses, which have genes constructed of RNA sequences rather than DNA. They replicate by producing a strand of DNA sequences
research lab in Oakdale's institute. These transformations (or translations) require careful, patient, and time-consuming management on the part of Oakdale and her colleagues.

To summarize, "cells" and "cancer" are sometimes different things to doctors, operating on breast cancer cell biologists, and oncogene researchers. Yet they are similar enough to allow Oakdale and her colleagues to translate others' concerns in order to satisfy their research requirements. So for Oakdale to do her research, she needed to coordinate her efforts with the work styles and interests of these different groups. Since she did not have the power to demand obedience, she had to persuade, cajole, badger, educate, and reciprocate with others to get them to act in her interest to preserve and give her living breast tissue cells. We see then that surgeons, patients, oncologists, and operating room staff in hospitals, and women in fact and invisible participants in the construction of the oncogene theory through work like Oakdale's and the supply of research materials. 19

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20. RNA tumor viruses are retroviruses which have genes constructed of RNA sequences rather than DNA. They replicate by producing a strand of DNA sequences
explored other viruses, they reported discoveries of more viral onco-
genases. These viral onco- genes, however, caused cancer only in vitro
and in laboratory animals. No naturally occurring tumors in animal
and human populations were credited to viral oncogenes.21

In 1971, Michael Bishop, Harold T. Varmus, and their colleagues
at the University of California, San Francisco, announced that they
had found a normal cellular gene sequence in various normal cells
of several avian species which were very similar in structure to the
chicken viral oncogene, called src [Stehelin et al. 1976]. Two years
later, after constructing a probe for their viral oncogene, they reported
that they had also discovered DNA sequences related to the src viral
oncogene in the DNA of normal cells in many different vertebrate
species from fish to primates, including humans [Spector et al. 1978].22
Bishop and Varmus and their collaborators suggested that the
viral gene causing cancer in animals was transduced from the
ancestral cellular genes by the virus, that is, the virus took part of the
cellular gene and made it part of its own genetic structure. Based on
their research and that of others, Bishop and Varmus speculated that
some qualitative alteration (through point mutation, amplification,
and chromosomal translocation) of this normal cellular gene may play
an important role as a cause of human cancer.23 Before this theory,
human cancer research and viral oncogene research had been en-
through the activities of an enzyme called reverse transcriptase. See Studer and Chu-
21. However, researchers did report suspected links between some human cancers
and viruses. See especially Gallo 1986.
22. Molecular biologists claim that since a gene is constructed of a specific se-
quence of nucleotide bases along a continuous strand of DNA, simply locating a
particular gene of interest is akin to searching for the proverbial needle in a haystack.
The human genome, divided into twenty-three paired DNA molecules, for example,
is very long and complex. It contains three billion nucleotide base pairs (constituting
perhaps fifty to one hundred thousand genes). The genome of a frog is even longer.
Even the viral genome is long, for example, the DNA of the SV40 monkey tumor virus
consists of 2,525 nucleotide base pairs. Molecular biologists argue that con-
structing DNA probes is one way of locating homologous genes. A probe is a synthe-
thesically constructed strand of DNA, called an oligonucleotide. In 1975 probes were
relatively difficult to construct. In 1990 most probes were constructed by automated
DNA synthesizers. The procedure is routine. See below for more discussion of probes.
23. The protooncogene theory in 1990 included the concept of anti-oncogenes
(tumor suppressor genes) introduced by Robert Weinberg [see below]. Inactivation of
these anti-oncogenes is another proposed mechanism by which normal genes can be-
come cancer-causing genes. In addition, by 1990 a total of nearly thirty possible proto-
oncogenes had been reported in the literature. I discuss the early origins of the theory
tirously orthogonal to each other, despite decades of efforts to link vi-
ruses to human cancer.24

These speculations were based in part on an earlier theory, the
oncogene hypothesis of Huhner and Todaro [1959] and on accumu-
lated research reports about the structures and mechanisms of viral
oncogenesis. The difference between the earlier theory and the Bishop
and Varmus theory was Bishop's and Varmus's conjecture that the
genome was originally part of the cell's normal genome rather than a
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tionary history.

Bishop and Varmus's proposal that the gene which caused normal
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endowment was based on arguments about "evolutionary logic." Since
the gene was found in fish, which are "evolutionarily quite an-
cient, the gene must have been conserved through half a billion
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ment of normal cellular genes homologous to a viral oncogene in
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The first couple of years after the discovery were difficult. Our find-
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hard for us to come to grips with the idea that a gene carried by a
chicken virus that caused cancer was also in human beings. It didn't
make sense. Why would we have cancer genes as part of our evolu-
tionary history? [Interview 7:79]

On the other hand, Bishop argues that their proposal was also
"evolutionarily logical."

Our first evidence that human beings had this gene, although it evo-
uotionary looked just fine, there are a lot of biologists who don't
really accept the evolutionary logic . . . So until the gene was isolated
from humans and shown to be the same as what we'd started with,
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tirely orthogonal to each other, despite decades of efforts to link viruses to human cancer.24 These speculations were based in part on an earlier theory, the oncogene hypothesis of Hoelzer and Todaro [1969] and on accumulated research reports about the structures and mechanisms of viral oncogenesis. The difference between the earlier theory and the Bishop and Varmus theory was Bishop's and Varmus's conjecture that the gene was originally part of the cell's normal genome rather than a viral gene implanted by viruses sometime in the organism's evolutionary history.

Bishop and Varmus's proposal that the gene which caused normal cells to become cancer cells was part of the cell's normal genetic endowment was based on arguments about "evolutionary logic." Since the gene was found in fish, which are evolutionarily quite ancient, the gene must have been conserved through half a billion years of evolution. Their critics simultaneously based their criticisms on the theory's "evolutionary illogic." Why would a cancer gene be conserved through evolution? At the time, the announcement of normal cellular genes homologous to a viral oncogene in humans was greeted with some skepticism.

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to whether we had really found the same gene in all humans.
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happy that the gene was in chickens and even mice, but it wasn't sup-
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"Evolutionary logic" is used here to argue for and against their
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to support their theory, while the location of gene sequences similar to
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Before discussing developmental biology, I want to point out that
Bishop and Varmus are here attempting to establish a two-way rela-
tionship with evolutionary biology. They are not simply drawing on
evolutionary arguments. They are also attempting to inject their
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Transduction by retroviruses is the only tangible means by which vir-
tein genes have been mobilized and transferred from one animal to
another without the intervention of an experimentalist. How does this
transduction occur? What might its details tell us of the mecha-
nisms of recombination in vertebrate organisms? What does it reflect
of the potential plasticity of the eukaryotic genome? Can it transcribe
and genetic loci other than viral oncogenes? Has it figured in the
course of evolution? How large is its role in natural as opposed to
experimental carcinogenesis? These are ominous questions, yet the
means to answer most of them appear to be at hand. (Bishop 1983,
347–48, emphasis added) 25

Links to developmental biology

Normal growth and development are research problems which form
the basis of developmental biology. This has been, and remains,

25. Evolutionary biology, and especially evolutionary genetics, is so entangled in
debates that oncology researchers may succeed in this effort to propose a role
for oncogenes in evolutionary biology. The units of selection debates so closely
influenced by philosophers of science as to make one wonder about the
units, levels, and processes by which selection and evolution occur. See, for example,
Lloyd 1984 and Brandon 1990 for an overview and analysis of the units of selection
debates.

26. Other suggestions of oncogenes as a source of genetic variation and as an in-
dication of the course of evolution were made by Tramjn (1971, 1980) and by Walter
Gilbert's research group (Schwartz et al. 1983).respectively.

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that their "normal" proto-oncogene had something to do with
cell division. Later, as researchers in molecular biology and bio-
chemistry of normal growth and development began proposing the
existence of growth factor genes based on research on growth fac-
tor proteins, Bishop and Varmus began to tie their work on onco-
genesis both theoretically and concretely to concurrent studies on
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The logic of evolution would not permit the survival of solely
random genes. Powerful selective forces must have been at work to assure the
conservation of proto-oncogenes throughout the diversification of
metazoa. Thus, we know nothing of why these genes have been
conserved, only that they are expressed in a variety of tissues and at
various points during growth and development, that they are likely to
represent a diverse set of biochemical functions, and that they may
have all originated from one or a very few founder genes. Perhaps the prototype
these genes encode are components of an integrating net-
work that controls the growth of individual cells during the course of
differentiation. We are badly in need of genetic tools to approach these
issues, tools that may be forthcoming from the discovery of proto-
oncogenes in Drosophila and nematodes. (Bishop 1983, 347–45, em-
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And it took us a while to convince people that these genes might
have a different purpose in the normal body. And then finally that
perhaps they had a different purpose in the normal body, but if some-
thing went wrong with them, they would become cancer genes as they
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The logic of evolution would not permit the survival of solely autonomic genes. Powerful selective forces must have been at work to assure the conservation of proto-oncogenes throughout the diversification of metazoa metazoa. Yet we know nothing of why these genes have been conserved, only that they are expressed in a variety of tissues and at various points during growth and development, that they are likely to represent a diverse set of biochemical functions, and that they may have all originated from one or a very few founder genes. Perhaps the process these genes encode are components of an interrogating network that controls the growth of individual cells during the course of differentiation. We are badly in need of genetic tools to approach these issues, tools that may be forthcoming from the discovery of proto-oncogenes in Drosophila and nematodes. (Bishop 1983, 347–48; emphasis added)

And it took us a while to convince people that [these genes] might have a different purpose in the normal body. And then finally that perhaps they had a different purpose in the normal body, but if something went wrong with them, they would become cancer genes as they were in the virus. [Interview 7:8]

Bishop expanded the number of research problems in his laboratory from one viral oncogene to studies of several viral oncogenes and their related proto-oncogenes, and included questions regard-
ing the normal functions of the proto-oncogenes in developmental biology.

My laboratory doesn't much resemble what it was ten years ago... [How has it changed and why?] The work's evolved in response to progress in the field. You get one problem solved, and you move on to something new that presents itself. A number of people in my laboratory are explicitly interested in normal growth and development. They're here because we believe that the cellular genes we study are probably involved in normal growth and development. And I wasn't studying cellular genes involved in normal growth and development fifteen years ago... There is a conceptual and probably mechanistic connection between cancer and development. But I'm not a developmental biologist, and I haven't read seriously in the field. There are people in my laboratory who will probably become developmental biologists as they fashion their own careers. (Interview 7:19)

The links between viral and cellular oncogenes and developmental biology were concretized during his collaboration with a Drosophila genetics laboratory through a shared student. He has a major collaboration with another member of the biochemistry faculty here, a Drosophila geneticist, because we use genetic analysis in Drosophila to try to see what the genes we study do in development. And I'm not a geneticist, and he's not a student of oncogenes, so that's a necessary collaboration. We have joint students between us, several now. (Interview 7:20-21)

By now, retroviruses and viral oncogenes are linked to the course of evolution, Drosophila genetics, and normal growth and development in developmental biology through proto-oncogenes. Here again proto-oncogenes are the boundary object which facilitates the translation of one group's interests into the interests of other groups and links laboratories in different lines of research into a single network.

Mutual translation: molecular biological oncogenes and virus virological oncogenes. I discussed how tumor virologists used oncogenes to translate their own interests into the interests of others. Here I present an example of mutual translation between viral oncogene researchers and a group of molecular biologists attempting to link their work to viral oncogenes. In 1978, soon after the Bishop and Varmus announcements, a few molecular biology laboratories began to study cancer using recombinant DNA technologies, especially gene transfer techniques, and soon reported that they had found cancer genes similar to Bishop and Varmus's proto-oncogenes. In one experiment researchers in Weinberg's laboratory at the Whitehead Institute at the Massachusetts Institute of Technology first exposed "normal" mouse cells to DNA from mouse cells that had been transformed by chemical carcinogens. The outcome, as reported by the researchers, was the transformation of the "normal" cells into cancer cells. They [Weinberg 1983, 137A] concluded from the experimental outcomes that "the information for being a tumor cell [was] transferred from one [mammalian] cell to another by DNA molecules." These and other research groups attempted other more sophisticated experiments where they used human tumor DNA to transform normal cells in culture. Using recombinant DNA technologies to devise a new molecular cloning approach, these researchers reported that they had finally isolated an oncogene which was the transforming factor, independent of any epigenetic (or environmental) factors. More significantly, this single gene was mutated at a single point. Weinberg claimed that a single point mutation had caused the normal gene to become a cancer-causing gene.

The successful isolation of transforming DNA in three laboratories by three different methods directly associated transforming activity with discrete segments of DNA. No longer was it necessary to speak vaguely of "transforming principles." Each process of molecular cloning had yielded a single DNA segment carrying a single gene with a definable structure. These cloned genes had potent biological activity. The transforming activity previously attributed to the tumor-cell DNA as a whole could now be assigned to a single gene. It was an oncogene: a cancer gene [Weinberg 1983, 130].


28. These "normal" cells, called NIH 3T3 cells, are somewhat ambiguous cells. They are not truly normal, since they have been passaged to many times in the laboratory. That is, the original cells taken from normal mouse tissue in the early 1960s have by now adapted to the artificial conditions of cell cultures (plates of agar filled with nutrients to feed them and antibiotics to prevent them from being infected with bacterial and are no longer entirely normal. They are referred to as "immortalized cells."

29. Weinberg's claims have since been toned down. Current views are that at least two events, and perhaps up to eight events, are necessary to transform "truly normal" cells into cancer cells. See Fujinura (preparation of for more details.)
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Weinberg (1983, 134) argued that his transfected oncogenes were of a class with the oncogenes reported by tumor virologists Bishop and Varmus.

A second question concerns the relation of these oncogenes to those which have been isolated from the cellular genome by retroviruses and used to form chimeric viral-host genomes. The most well-known of these genes is the avian sarcoma virus src gene, the paradigm of a class of more than a dozen genes. Of these two classes of oncogenes, those from spontaneous tumors and those affiliated with retroviruses, overlap with one another or do they represent mutually exclusive sets? Although the answer to this is not yet at hand, it will be forthcoming, since many of the sequence probes required to address this question are already in hand.

Weinberg (1983, 135) argued that while "the study of the molecular biology of cancer has until recently been the domain of tumor virologists," it now was also the domain of molecular biologists. In 1983, he and his associates (Land et al. 1983, 391) claimed to have confirmed this equivalence between these sets of oncogenes.

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 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 can be switched on or off one another in order to achieve the transformation of cells. (emphasis added)

Bishop (1982, 92) supported Weinberg's arguments.

Weinberg and Cooper have evidently found a way of transferring active cancer genes from one cell to another. They have evidence that different cancer genes are active in different types of tumors, and so it seems likely that this approach should appreciably expand the repertoire of cancer genes available for study. None of the cancer genes uncovered to date by Weinberg and Cooper is identical with any known oncogene. Yet it is clearly possible that there is only one large family of cellular oncogenes. If that is so, the study of retroviruses and the procedures developed by Weinberg and Cooper should eventually begin to draw common samples from that single pool.

To summarize, a few molecular biologists constructed an equivalence between their cancer genes and the proto-oncogenes of tumor virologists. They argued that their cancer genes were in the same class of cancer genes reported by tumor virologists. This representation expanded the category of proto-oncogenes to include genes which had been transformed by chemicals reported to be carcinogens, in volumes of previous studies on cells, on whole organisms, and especially on humans. The work in Weinberg's laboratory links carcinogenesis studies, human cancer, and oncogenes. This simultaneously provided a new link between Bishop and Varmus's oncogene and carcinogenesis studies. As sets of researchers embraced one another's work, the concept of a normal gene causing cancer becomes more stable.

Re-representing cancer

By 1983 the new unified proto-oncogene theory of cancer had been adopted into and used as the basis of research in investigation in programs in several new and established lines of biological and biomedical research. The oncogene theorists constructed cancer genes which they claimed mapped onto the intellectual problems of many different scientific social worlds. They claimed that their cancer genes accounted for findings in many other lines of cancer research and represented a unified pathway to cancer in humans and other higher organisms. If one looks closely at these alliances, however, one sees that the mapping is quite heterogeneous. Links were constructed between evolution, developmental biology, and molecular biology as well as between established lines of biomedical research on cancer. Those various links were patched together to present a coherent re-representation of cancer in molecular genetic terms.

For example, Weinberg (1983, 134) speculated broadly that the proto-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."

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neticists may have detected the effects of cancer 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, editors, Furmanski, Hager, and Rich (1985, xx), also called for further links to be made between oncogene research on causation and clinical problems in cancer research: "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." Cancer genes, however, do not in and of themselves mechanistically connect together the multiple viewpoints (approaches, theories, methods) mentioned above. Rather the oncogene theory is a new representation of cancer, this time in terms of normal cellular genes, the proto-oncogenes. The multitude of representations of chemical carcinogenesis, radiation carcinogenesis, tumor progression, metastasis, and so forth are re-represented using a new unit of analysis. They are locally re-represented in laboratories, research protocols, and transforming cells in culture, and formally re-represented in a new theory. While this new theory provides a metaphoric tying together of the "nodes of the system," the work is done by many heterogeneous actors. Some of these re-representations were facilitated by standardized tools such as probes and sequence data bases, which eventually became part of the standardized package of proto-oncogene theory and molecular genetic technology.

Using Standardized Tools to Maintain Continuity by Standardizing the World Inside and outside the Laboratory

Oncogene researchers went beyond speculation by reconstructing their laboratory work to pursue some of the proposed problems, as the above example of Bishop's laboratory's work on normal growth and development shows. At the same time, researchers in other lines of research took the opportunity to reconstruct work in their laboratories to pursue some of the proposed problems. This recon- construction introduced novelty into their laboratory's work while simultaneously maintaining continuity with previous and other ongoing research. That is, Bishop's student was still working with oncogenes, but now in the context of a different problem: normal growth and development.

In another example, a senior biophysicist whose laboratory studied the effects of radiation on carcinogenesis (on transforming cells in culture) similarly expanded his laboratory's research by incorporating oncogene research to explore new levels of analysis. 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 study two problems: first, whether radiation played a role in the 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. In this example, radiation stayed constant, while the experimental process and problem context changed from manipulating cells to manipulating genes.

Reconstructing laboratories can, however, lead to deconstructing theory. In order to shape these subsequent reconstructions and re-representations, oncogene theorists attempted to standardize the world. Standardizing the world outside one's laboratory is one way to maintain continuity in scientific constructions. The oncogene researchers' tools for standardizing the world include probes, data bases, and sequences.

Probes are constructed strands of DNA, called oligonucleotides, which researchers use to locate homologous gene sequences in larger strands of DNA. In their efforts to allay the skepticism met by their new theory and to win converts to it, Bishop and Varmus distributed their probes for proto-oncogenes to other laboratories and to suppliers, thus specifically facilitating replication of their results as well as further oncogene research in other laboratories by providing standardized tools: "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." [Interview 19:3] Any researcher can call or write the ATCC to order the probes at the cost of maintenance and shipping. These probes are more than physical materials. They are constructed categories which embody the specific work organizations of the laboratories in...
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30. See n. 22 for more details on probes.
which they had been constructed. Exporting probes is one attempt to standardize the world outside. With Bishop and Varmus's probe, researchers are more likely to find what Bishop and Varmus found than if they constructed probes of their own.

Data bases allow different lines of research on evolution, cancer, and normal growth and development to intersect in ways that had not previously been possible.

Data bases are the computerized version of publications of sequence information. Before more efficient retrieval software was constructed for accessing the computerized data bases, scientific journals and books published information related to particular topics. For example, some scientists served as "curators" for book "repositories" by pulling together and publishing in one document all of the published sequences on a specific research topic. A search-and-retrieval procedure. Computerized data bases and new search-and-retrieval software increase the speed of work. For example, by searching through the data base, Michael Waterfield, a technical expert on peptide mapping and amino acid sequences, constructed ties between the epidermal growth factor (EGF) receptor protein and the erbB viral oncoogene's protein product and between platelet-derived growth factor (PDGF) and the protein product of the sis oncogene of simian sarcoma viruses. These earlier publications and the new computerized data bases are repositories of information which is coded in standardized forms in order that it can be used by many different scientific worlds.

Centralized, systematic data bases hold DNA, RNA, and protein amino acid sequence information—organized and annotated (for example, by selected host organisms and by taxonomies of organisms)—on many organisms, including humans. The major data bases are located at the Los Alamos National Laboratory in the United States and at the European Molecular Biology Laboratory (EMBL) in Heidelberg, Germany. The American data base, called Genetic Sequence Data Bank or GenBank, is funded by several NIH agencies (including the National Cancer Institute), the National Science Foundation, the Department of Energy, and the Department of Defense. GenBank and EMBL share the job of collecting sequence information and then pool their information. By 1987 GenBank contained 1.3 million base pairs of total DNA sequence information and 1.9 million base pairs of human DNA sequence information, and it has since rapidly expanded. Information in both data bases is organized in standardized, computer-readable form (Office of Technology Assessment Report 1988). Access to the data is through distribution of magnetic tapes and floppy disks, direct computer-to-computer and computer-to-terminal transfer over telephone lines, and computational resources—which provide access to both sequence-data and sequence-analysis programs for the nation's academic molecular biologists (Friedland and Kedes 1985, 1172).

The sequence data bases and sequence analysis programs offer the potential for a general and more efficient methodology for accessing information needed for experiments or for interpreting experiments. Some of the kinds of analyses scientists can perform using the data base system include translation and location of potential protein-coding regions, inter- and intrasequence homology searches, and location of restriction enzyme sites. That is, for example, researchers put their DNA, RNA, or amino acid sequence information into the computer in order to seek homologies—other DNA, RNA, or amino acid sequences which are homologous to theirs. Homologies are similar sequences which are hypothesized to have a common ancestor at some point in their evolutionary history (see Fujimura 1991b for a discussion of homologies). An oncogene researcher describes the speed and efficiency with which two previously unrelated areas of research (arthritis and growth factors) were "found to be related" through the use of computers and the sequence data bases. (Note that the epidermal growth factor (EGF) receptor protein had earlier been reported to be identical to the erbB oncogene's protein product.)

31. Walter Gilbert, a molecular biologist at Harvard University, argues that these data bases and software also change the quality of work. Indeed, he argues that they are creating a paradigm shift in biology from an experimentially based discipline to a theoretically based discipline.
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32. For literature on sequence data bases, see Friedland and Kedes 1985, and Smith 1986.

33. See Friedland and Kedes 1985, 1972–73, for concise descriptions of these functions.

34. By streamlining the procedures and knowledge requirements for identifying sequence homologies, the computerized sequence data bases allow scientists to pass some of these tasks on to other lab members. In an academic oncogene laboratory the director had hired an undergraduate student to handle much of the computerized data base work. The students did not have to know about the relevant journals, authors, and articles in the research base area in order to search for sequence homologues using the computer.

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In fact, nobody has to read any more. . . . (At least nobody has to read pages of sequen... data in search of specific information), because the computer's changed the face of that aspect of science. . . . The way this is usually done is to take your experiment and plug it into the computer and ask the computer to search a gene bank, a sequence bank, for relationships. So just yesterday, for example, a fellow visiting here . . . described some experiments in which he was looking at the receptor for low-density lipoproteins. This is a receptor which is required to clear the blood of cholesterol. People who lack this receptor develop atherosclerotic and myocardial infections at an early age. (The visitor) and his colleague . . . some years ago defined the receptor. They recently purified and cloned and sequenced the gene, that is, sequenced a copy of the messenger RNA of the gene. When they plugged their sequence into the computer, they got back information that the receptor was very similar to a protein that serves as a precursor for the growth factor we've been talking about, EGF [epidermal growth factor]. So there we're dealing not with identity but with similarity. We have the information that two genes that seem ostensibly unrelated are, in fact, closely related members of a gene family. (Inter...view 12:10–11)

In order for the data bases to be constructed and to be useful, information is standardized. The sequence data bases contain information in terms of the biochemical sequences of DNA, RNA, and amino acids. The sequences are used to represent genes and proteins in terms of a linear description of deoxyribonucleic acids (DNA), ribonucleic acid molecules (RNA), and amino acid molecules of proteins. If we just limit our concerns to the terms of these realistic representations, the complex properties of each molecule, of each set of molecules that constitute genes and proteins, and of gene and protein interactions with other parts of its environments (cellular, organismal, extracellular) are eliminated from this data base. The sequence information for different types of phenomena is expressed in the same chemical language. This language standardizes the form of the representation of the phenomena. This standardization or common language is what allows for collaborative work across both laboratories and worlds. It is also what allows for claims of triangulation of different lines of research on a particular phenomenon. Homologies, for instance, are coincident representations. This coincidence, however, is based on interdependence rather than independence. Phenomena are first represented using

one language standard, and then similarities within the language system are constructed or found.

Sequence information, then, is just one kind of re-representation of earlier theories of the gene and proteins which in turn are kinds of representations. For instance, Burian and Fogel [1990] agree that there is a qualitative difference between the traditional definition of gene, even as late as 1965, and what molecular genetics now considers to be a gene (cf. Kitcher 1981). I argue that gene and protein sequence information are markers for complex phenomena and that the homologous relationships constructed through comparing sequence information on line may be more a construction of coincident markers than of homologous phenomena. The robustness of the oncogene theory, then, is based on coincident representations or markers which in turn are based on a standardized language or form of representation.

Thus, concepts, probes, and data bases of sequences are the result of "homologies" between laboratories as well as between representations of phenomena. These collective constructions are then used to reconstruct laboratory work organizations as well as experimentally produced representations. Both kinds of homologies are part of creating and maintaining continuities across lines of research and through time.

Continuity and the National Cancer Institute

NCI administrators joined in the effort to promote the oncogene theory for several reasons. Their sponsors were Congress and the public it represented, including other scientists. The oncogene theory provided them with both the justification for past research investments in the Virus Cancer Program (VCP) and with a product to present to Congress.

In the 1960s the National Cancer Institute focused on the role of viruses in cancer etiology through a special, well-funded Virus Cancer Program. Many virologists and molecular biologists were funded through NCI through this program, both before and after the National Cancer Act of 1971, to study what are now called DNA tumor viruses and retroviruses (or RNA tumor viruses). Both the act and

35. See Fujiwara 1996 for more on the constructed complexities and simplicities of DNA and proteins.
36. This is similar to the processes in naming (nomenclature) and classifying medical diseases, biological flora and fauna, and races.
37. This paper discusses one reason for NCP's promotion of oncogene research. See Fujiwara 1998 for further reasons.
38. I present more detailed versions of this history below. See also Chubb and Stuber 1978; DeVita 1984; Requ 1977; Streicker 1972; and Stuber and Chubb 1980.
In fact, nobody has to read any more. [At least nobody has to read pages of sequence data in search of specific information], because the computer’s changed the face of that aspect of science. . . . [The way this is usually done is to take your data and plug it into the computer and ask the computer to search a gene bank, a sequence bank, for relationships. So just yesterday, for example, a fellow visiting here . . . described some experiments in which he was looking at the receptor for low-density lipoproteins. This is a receptor which is required to clear the blood of cholesterol. People who lack this receptor develop atherosclerotic and myocardial infections at an early age. [The visitor] and his colleague . . . some years ago defined the receptor. They recently purified and cloned and sequenced the gene, that is, is sequenced a copy of the messenger RNA of the gene. When they plugged their sequence into the computer, they got back information that the receptor was very similar to a protein that serves as a precursor for the growth factor we’ve been talking about, EGF [epidermal growth factor]. So there we’re dealing not with identity but with similarity. We have the information that two genes that seem ostensibly unrelated are, in fact, closely related members of a gene family. [Interview 12:10–11]

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its viral research component were controversial and much-maligned efforts. Controversy raged over both the contractual basis for dispensing research funds and the huge sums of money concentrated on the virus cancer program, that is, on what was considered by many at that time to be a high-risk bet that viruses caused human cancer.

After twenty years of research, no viruses had been linked to human cancer, and the program had been thoroughly maligned by its critics. As the following statements demonstrate, the proposed role of proto-oncogenes in causing human cancer was in the early 1980s used to justify past investments in viral oncology.

The study of viruses far removed from human concerns has brought to light powerful tools for the study of human disease. Tumor virology has survived its failure to find abundant viral agents of human cancer. The issue now is whether viruses cause human tumors (as perhaps they may, on occasion) but rather how much we can learn from tumor virology about the mechanisms by which human tumors arise.

[Michael Bishop (1982,92), tumor virologist]

Given the still prevalent unfair public misconception that the NCI Taucon Virus Program was a failure, and the new strong possibility (fact?) that most if not all of viral oncogenes have their human counterparts, the time is more than ripe for NCI to point out how well the public purse has, in fact, been used. [James D. Watson, molecular biologist]

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 true fund of information, the dividend of which defies the imagination. [Vincent T. DeVita, Jr. (1984, 5), former director, National Cancer Institute]

Both oncogene researchers and cancer research administrators argued then, that the "new" oncogene research would be based on the "extraordinarily powerful new knowledge" produced by past investments. The viral cancer genes constructed from the investments of the NCI in the Viral Cancer Program during the 1960s and 1970s have in the 1980s become human cancer genes through the oncogene theory and recombinant DNA technologies. Viral cancer genes with no previous connection to human cancer have now become human cancer genes. In their view, the NCI's and James Watson's earlier choices and predictions have been proven fruitful and justified, while Bishop's theory gains credibility from DeVita's and Watson's translations. Here, then, is mutual translation for mutual benefit.40

Discussion and Conclusion

In a recent interview, an oncogene researcher balked at my use of the term "oncogene theory." He argued that oncogenes are a fact, not a theory. I have used the concepts of standardized packages, boundary objects, and translation to show how different social worlds interacted through time and space to collectively craft this fact. Each world is changed in some manner, yet each also maintains its uniqueness and integrity in the construction and adoption of the standardized package of proto-oncogene theory and recombinant DNA technologies. The package provided both dynamic opportunistic for divergent meanings and uses as well as stability. Using recombinant DNA technologies and selected boundary objects, Bishop and Varman constructed multiple translations between oncogene research, on the one hand, and evolutionary biology, developmental biology, cell biology, carcinogenesis research, and more, on the other hand. They are not simply drawing on arguments from these lines of research. They are also installing their theories, inscriptions and materials into these ongoing lines of research. A combination of ambiguous concepts and standardized tools are used to construct technologies between laboratories as well as between representations of phenomena.41 These collective constructions packaged together are

39. This statement was quoted by DeVita in his 1984 essay. Watson, as Nobel laureate (1956), has used his influence to push for the institutional growth of molecular biology. More recently he has been a prime mover and shaker behind the Human Genome Initiative, the three-billion-dollar effort to map and later sequence the entire human genome.

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used to reconstruct laboratory work organizations as well as experimentally produced representations. Both kinds of homologies are part of creating and maintaining continuities within and across lines of research and across time and space. Hybrid lines of research are also constructed through this process of intersection. For example, Bishop's student who worked on the problem of oncogene activities in development is a hybrid product of two formerly separate lines of research. However, the original lines of research also continue.

The package of concepts and standardized tools is useful for understanding both the stability and the dynamism of the oncogene theory. Less structured concepts, such as cancer, cells, genes, and cancer genes, and standardized tools, such as probes, the language of sequence information, and sequence data bases, were used to craft the oncogene theory. These objects provide a way of talking about a theory which appears to be both simple and complex, both static and dynamic. Together they help to explain how the theory can be continuous across time and space through different social worlds. The newly crafted oncogene theory was then used in conjunction with newly standardized recombinant DNA and other molecular genetic technologies as a package to enroll other researchers, biological supply companies, the National Cancer Institute, the American Cancer Society, members of Congress, and the Nobel Prize Committee.

My point is that packages of ambiguous concepts and standardized tools, of theory and methods, are powerful tools for insuring fact stabilization. Whether concepts or standardized tools alone can achieve fact stabilisation is an empirical question. The two examples discussed in this paper suggest otherwise.

In contrast to Grinnell's focus on standard methods of collecting and on building the museum and his relative neglect of his ecological theory of evolution, oncogene theorists immediately began to promote and teach their theory to new audiences. They also used molecular genetic technologies to instantiate and substantiate their theory. The combination is what I called the standardized package. This combined theory-methods package, the triangulation of efforts by several lines of research, and a great deal of work constitute the new vision of cancer which has become part of the canon. I do not regard the theory-methods package as constituting a necessary connection. The coupling of the oncogene theory and recombinant DNA and other molecular biology technologies is constructed, and not born in nature. The theory may in the future continue to exist as an entity separate from these techniques or coupled to another set of techniques. Similarly, the technologies are coupled with quite different theories in other lines of biological research.

I am interested in standardized packages and other such crafted tools because I would argue that they can be used by scientists to define their areas of expertise and power. It is through the use of standardized packages that scientists constrain work practices and define, describe, and contain representations of nature and reality. The same tool that constrains representations of nature can simultaneously be a flexible dynamic construction with different faces in other research and clinical and applied worlds. A standardized package is used as a dynamic interface to translate interests between social worlds. This is true for the social as well as the natural sciences. Examining the construction, maintenance, and augmentation of these packages will help us to understand not only how we came to have the representations we now hold sacred but also that there are other possible representations, other ways of knowing and practicing.

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Bijker, W. E., T. P. Hughes and T. J. Pinch, eds. 1987. The Social Construction of Technological Systems: New Directions in the Sociology and History of Technology. Cambridge: MIT Press. Specific examples are now being made to add more fluid categories, but bureaucrats are finding that a difficult task precisely because of the static property of forms. Thus, this boundary object both enables some action and disables others.

Joan H. Fujimura
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etic technologies are the methods. See Fujimura 1985b. While standardized forms are static, they still act by constraining other actions. Censor forms, for example, force people to fit themselves into one of several social or ethnic categories. The only choice left for local people is that of death. But social forms are now being made to add more fluid categories, but bureaucrats are finding that a dif-
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P O S I T I O N S


J O A N H. F U J I M U R A


