UNIVERSITY OF CALIFORNIA, SAN DIEGO

On Shifting Ground: Earthquakes, Retrofit and Engineering Culture in California

A dissertation submitted in partial satisfaction of the
requirements for the degree Doctor of Philosophy
in Sociology (Science Studies)

by

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The dissertation of Benjamin Sims is approved, and it is acceptable in quality and form for publication on microfilm:

Chair

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For Kathy

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ABSTRACT OF THE DISSERTATION

On Shifting Ground:
Earthquakes, Retrofit and Engineering Culture in California

by

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Doctor of Philosophy in Sociology (Science Studies)
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This thesis focuses on changes in seismic design and retrofit methods at the California Department of Transportation (Caltrans) from the 1970s through the 1990s, situating them in relation to the wider social world of earthquake engineering in California. In particular, it examines the connection between retrofit technology and definitions of seismic risk, the relationship between formal codes and design practice, the incorporation of the results of academic research into design practice, Caltrans' use of peer review panels, and the power of the engineering profession in relation to the news media and the state.

This material is used as the basis for an argument that scientific and technical work depends on a division of labor between work settings and people with expertise in different areas, but at the same time requires coordinated activity across these divisions. This coordination is facilitated largely by face-to-face interactions and collaborative work efforts that make the skills and knowledge gained in particular work settings relevant to larger arenas of technical practice. During the 1990s, Caltrans engineers faced a period of particularly rapid change in design practice. In such situations, personal interactions take on a particularly prominent role in coordinating the design process because codes and other formal modes of regulation are slow to adapt.

The thesis concludes by examining the implications of the increasing prevalence of retrofit and renovation projects in civil engineering. Unlike the design of new

structures, in which technology seems relatively flexible, such projects demonstrate the degree to which existing infrastructure may constrain our future technological choices.

Chapter 1

Introduction

If one had to pick just two defining motifs that run through life in California, one might do well to choose these: earthquakes and freeways. Nowhere else in the world are freeways a more integral part of culture than in California. There are more freeways, bigger freeways, better freeways in California than anywhere else, and fewer alternatives to taking the freeway. Though seismologists warn that earthquakes can happen all over the United States, and there is surely an enduring fascination with such disasters in our popular culture, there is nowhere else in the country where earthquakes have entered into political discourse, professional work, and public consciousness the way they have in California. When earthquakes and freeways come together violently, as they did in the 1989 Loma Prieta (San Francisco Bay Area) earthquake, the result is a major cultural event.

1.1 Caltrans and Loma Prieta

As a sociologist interested more in the dynamics of scientific and technical work than in the broad interpretation of culture, I have channeled my desire to understand the significance of both freeways and earthquakes into a study of the practice of earthquake engineering, and specifically of one institution where these two entities routinely coincide: the California Department of Transportation (Caltrans) — in particular, its structural design and earthquake engineering units, where engineers analyze and design the bridges, overpasses, ramps, and viaducts (elevated structures that carry freeways over city streets) that tie the freeway system together. Not surprisingly, Caltrans is known as the most sophisticated state transportation department in the nation where earthquake engineering standards are concerned.

For the past 30 years, seismic safety has been one of the central challenges of engineering practice at Caltrans. This thesis focuses on one watershed event in this time span, the Loma Prieta earthquake. When it struck on October 17, 1989, this tremor caused serious damage to two Caltrans-designed structures, knocking down a segment of the deck of the San Francisco-Oakland Bay Bridge and causing the collapse of a long section of the double-deck Cypress viaduct in Oakland, which killed 41 people. These disasters created a political controversy that carried on intensely for a number of weeks, then rapidly faded. The investigation into Caltrans actions was delegated to a panel composed largely of engineers.

The political controversy and subsequent investigation had two major implications for Caltrans. First, the earthquake provided justification for funding a massive program to "retrofit" thousands of older bridges to current seismic standards. Caltrans engineers had been working on such a program since the 1970s, when a large earthquake near Los Angeles damaged some freeway structures and demonstrated a number of deficiencies in existing design practices. This led to a period of rapid development in earthquake engineering at Caltrans that continued until the Loma Prieta quake. There had been little funding for retrofit work before Loma Prieta, so the process had gone rather slowly. Now Caltrans engineers were given abundant funding, but with the demand that all retrofit work be done very quickly. The 1994 Northridge earthquake in the Los Angeles area added to this sense of urgency. The result was a huge amount of design work, and Caltrans engineers were forced to make fundamental changes in the way the design process was managed in order to keep up.

The panel also recommended that Caltrans institute procedures for "peer review" of its design standards and of specific projects by outside experts, including both practicing engineers and university professors. A period of intense interaction between Caltrans engineers and academic researchers followed, in which Caltrans funded a great deal of research and included researchers in the design process as participants in peer review panels. This interaction generated an influx of new knowledge and new analytical methods into the design process at a time when designers were already faced with a huge amount of retrofit work. The rapid introduction of new design approaches meant that they could not be integrated into design codes quickly enough, so Caltrans engineers had to fall back on more informal ways of regulating the design process.

Caltrans structural engineers have different levels of training and experience, and different positions within the organization. The largest number work in several "design sections" in the Division of Structures and are responsible for most routine design work. Most of these engineers have bachelor's degrees, but a few have Ph.D.s and tend to play a larger role in the application of complicated analytical tools to design. There have been a number of more specialized units over the years. The most prominent of these during the 1990s was the Office of Earthquake Engineering. Now called the Division of Earthquake Engineering and Design Support, this group employs a number of engineers who either have training in earthquake engineering or complex structural analysis — often at the Ph.D. level — or extensive practical experience. The engineers in this office are the focus of the portion of this dissertation that deals with Caltrans design practice.

The time period I focus on here was one of rapid change in the social context of civil engineering in California, particularly at Caltrans. This turbulent era exposes important aspects of civil engineering practice and its relationship to society at large that might be less visible under other circumstances. In relation to design, it brings out the tension that often exists between formal rules of practice, such as codes, and the less formalized knowledge and skills that are necessary to carry out any particular design task. At a professional level, it focuses attention on the relationship between researchers and practicing engineers, and on the differences between academic knowledge and the working knowledge of practitioners. Finally, it raises questions about the dependence of the engineering profession on the State and about the political uses of engineering expertise.

1.2 The structure of the thesis

This work describes and analyzes the social world of earthquake engineering, particularly as it exists within the state of California and in relation to the design of freeway bridges. The concept of a "social world," as elaborated by Anselm Strauss and others, is straightforward: it refers to a group of people who participate in a common set of activities — for example, playing bluegrass music or doing research on cancer.[1] Though the concept is a simple and very flexible one, its real significance is that it stands for a characteristic way of analyzing social life that pays close attention to the fine differentiation of culture across interactive settings, such as workplaces and professional organizations. It looks at how social worlds, and smaller segments within these worlds, become differentiated, how these local cultures are organized, and how people communicate and coordinate activities between groups despite cultural differences.

In this thesis, I take a similar approach, analyzing a series of work settings and situations in which different segments of the engineering profession come together. The common thread is an argument about how scientific and technical work depends on a division of labor between work settings and people with expertise in different areas, but at the same time requires coordinated activity across these divisions. Even in this digital age, this coordination is facilitated largely by face-to-face interaction and collaborative work, perhaps necessarily so.

This theme surfaces differently in each chapter. Chapter 2 explains how the participation of outside experts influenced the way earthquake risks were defined at Caltrans. Chapter 3 looks at how engineers maintain professional power in their interactions with government and the news media. Chapter 4 examines the workings of peer review panels for civil engineering projects, showing how engineers manage the tensions that arise between the academic, private, and government sectors of the profession in a forum that brings them into close personal contact. Chapter 5 shows how engineers within Caltrans deploy both formal design codes and informal standards of practice to manage change in engineering practice. Chapter 6 examines the chain of interactions across social boundaries that makes it possible to bring laboratory test results to bear on design practice.

As this brief overview suggests, another central focus of the thesis is on the dynamics of social and technical change: how engineers initiate and manage rapid changes in knowledge, practice, and codes; what happens when the structure of a social world changes fundamentally over a short time period; and how past engineering decisions limit our present technological choices. Accordingly, the chapters are organized to provide a rough chronology of events. This introductory chapter discusses the more specific themes of the thesis in the context of an argument about the nature of civil engineering and civil infrastructure and their relationship to State power. Here and in the conclusion, the aim is to relate what the individual chapters say about design practice to larger concerns about the nature of technology itself.

1.3 Social studies of technology

This thesis is intended to contribute to the growing body of literature on the sociology of technology. Philosophers, social scientists, and historians from various Marxist and post-Marxist schools of thought have long studied the connections between technology and society from a general cultural perspective.[2] More traditional historians of technology focused on the internal dynamics of technology, but since the 1960s a “contextualist” approach has become dominant that emphasizes the close interactions between internal technical factors and social context in shaping technological change.[3] One subset of this contextualist work has focused on the construction of technological systems. Most notably, Thomas Hughes argues that system builders — like Thomas Edison and others who built the earliest electrical grids — must pay close attention to both the social and technical elements of their systems if the systems are to work successfully.[4]

This strand in the history of technology had a strong influence on a group of sociologists who were trying to apply to technology the insights of the “social constructivist” approach to the study of science. Sociological studies in this tradition indicate that scientific ideas are the product of social processes or, more accurately, that science should be conceived of not only as a set of ideas but also as a set of practices dependent on the skills and cognitive abilities of scientists and other scientific workers, the technological infrastructure and social organization of laboratory work, and a society that provides the physical, cultural, and economic resources to support research. Those who have used these insights as a basis for studying technology have similarly argued that technological artifacts and knowledge about them are the product of social processes and are tied to skills, existing infrastructure, and the organization of technical work. Going beyond the contextualist argument from the history of technology, they have argued not only that technical and social elements interact to produce technological change, but that the meanings we attribute to technology, and ultimately even our assessments about whether a technology works or not, are socially shaped. These meanings, in turn, shape the future material development of technology.[5]

Sociological studies of technology that take a constructivist view can be roughly divided into those that try to generalize about the origins and evolution of technology through the study of particular technological artifacts, and those that try to generalize about the nature and historical development of technological (mainly engineering) practice itself. These two types are unified by the general goal of showing that engineering is a "heterogeneous" activity: that it is not simply about the application of engineering technique to well-defined technical tasks, but rather depends on the manipulation of social and technical resources to define and solve problems in new and creative ways.[6]

Artifact-focused studies seek to further explicate the heterogeneous nature of engineering work by showing how engineers assemble stable networks of resources, both within the technical work setting and in the larger social world, in order to complete technological projects. Donald MacKenzie, for example, traces the intricately connected political, institutional, and technical developments that shaped the history of nuclear missile guidance technology.[7] Bruno Latour examines a radical light-rail system that was never realized, in which small cars would be programmed to carry passengers directly to their destinations without intermediate stops. He argues that the system failed in part because project engineers and managers focused too narrowly on the technical details of coupling and uncoupling cars, and didn't allow themselves to be influenced by the interests of elected officials or potential users until it was too late to save the project.[8] John Law and Michel Callon try to account for the failure of a British military aircraft project by showing how project backers sought to line up a global network of political, economic, and technical resources in order to create a local network in which the work of design could proceed without interference. As the project continued, though, the separation between these networks could not be maintained. The result was that political conflicts and design problems reinforced one another, leading to the cancellation of the project.[9]

Practice-centered studies of technology draw on many of the same analytical resources as artifact-centered studies, but they make the individual technological artifact or project a secondary concern. Instead, they focus on the general features of engineering practice across projects, though often within a particular technical area, such as aerospace or mechanical engineering, or within a particular organization. An example of this approach from the contextualist tradition in the history of technology is the work of engineer and historian Walter Vincenti. He assembles a series of case studies on the history of design standards, the interaction between design and production, experimental research and testing, and the development of theoretical tools in aeronautical engineering. Based on these studies, he concludes that engineering develops its own body of knowledge (both conceptual and practical) and is not simply the application of scientific knowledge to practical problems.[10] The work of Louis Bucciarelli, an engineer trained in anthropological methods, is more directly concerned with the design process. Based on an ethnographic study of three design firms, Bucciarelli argues that engineers understand and work with technology through individually- or collectively-constructed "object worlds." The design process is enabled and constrained by the contents of these worlds, which include such things as the object being designed and its components, mathematical models, theoretical concepts, codes and standards, and design tools including computers, software, programming languages, graphical conventions and reference books.[11] Sociologist Kathryn Henderson studied a number of engineering organizations, focusing on one in particular that designs and produces turbines. She found that design practice depends on a widely-held set of visual conventions among engineers, and particularly on the frequent use of informal sketches as an aid to individual thinking and as a means of communication between engineers. She examines the implications of computer-aided design and manufacturing (CAD/CAM) systems for this visual culture, concluding that excessive reliance on rigid, computerized representations of design objects can undermine essential informal communications among engineers and between engineers and production workers.[12]

The present thesis is, for the most part, an example of practice-centered work in the social study of technology. Although the historical development of particular types of technological objects is an occasional part of the story, it focuses to a much greater extent on the institutional and organizational conditions of technical practice and on the development of knowledge, techniques, and standards of practice in engineering. It tries to situate engineering practice within an organizational context much more than Vincenti's work, but it takes a broader historical and institutional approach than do Bucciarelli and Henderson. Because of this, it is able to address questions about the local origins of broad changes in engineering practice that these other works have not sought to discuss in a systematic way.

1.4 The nature of civil engineering

Definitions of civil engineering

Earthquake engineering can be described as a specialized area in the broader field of civil engineering, even though parts of it are closely related to other fields like mechanical engineering. Consequently, this thesis is, at the most general level, an effort to contribute to a sociological understanding of civil engineering. When civil engineers try to define the scope of their profession, they often resort to ostensive definitions — lists of the things that civil engineers work on. For example, civil engineers are described as engaged in the analysis, design and construction of "fixed works for irrigation, drainage, waterpower, water supply, flood control, inland waterways, harbors, municipal improvements, railroads, highways, tunnels, airports and airways, purification of water, sewerage, refuse disposal, foundations, grading, framed and homogeneous structures, buildings, or bridges."[13] A more general description has it that civil engineers are responsible for "massive infrastructure," which is a particularly apt term in light of considerations discussed below.[14]

The canonical view reflected in these descriptions is a relatively recent invention. Although one can look back through recorded history and find many examples of building projects that are similar to those civil engineers would now be responsible for, such projects did not become the province of a single, coherent professional group until well into the 19th century. The term “engineer” came into common use around the 15th century, when it was used to describe the designers and fabricators of weaponry and fortifications.[15] Engineering retained its military associations until the rapid expansion of the civilian profession in the 19th century.[16] It was during this period that the term “civil engineer” gained currency and was used to refer to any engineer not in military service.[17] Civil engineering took on its modern connotation, referring to the design of large public works, as the outcome of intra-professional struggles in the latter part of the 19th century.

The American Society of Civil Engineers (ASCE), the oldest of the U.S. engineering societies, became a national organization beginning in 1867 and, in keeping with contemporary terminology, “claimed to represent all American engineers not in military service.”[18] From the beginning, the Society sought to represent an elite within engineering, restricting membership to those “in charge of engineering work” and progressively raising its requirements for full membership by increasing the number of years of experience required.[19] In part in reaction to this policy, engineers working in more industrially-focused areas chose to create their own associations, and by the 1880s the American Institute of Mining Engineers and the American Society of Mechanical Engineers were viable competitors to the ASCE. Nonetheless, at least through the end of the 19th century, “spokesmen for the ASCE maintained that the difference between civil and other engineers was not that between coordinate branches of engineering, but rather between professionals and nonprofessionals.”[20] In general, civil engineers tended to emphasize both public service and the advancement of a more technical form of engineering knowledge, and were oriented toward professional autonomy. Mining and mechanical engineers were more concerned with practical knowledge, more business-oriented and consequently less interested in professional autonomy.[21] The civil engineers confronted a rising tide, however, as the other branches of the profession themselves became more exclusionary and technical, and as general use of the term “civil engineer” shifted toward an association with particular technologies, namely massive public infrastructure.

The engineering of massive infrastructure

While it is difficult to draw any hard-and-fast distinctions between civil engineering and other branches of the profession, there are significant features of the engineering profession that seem to come out particularly strongly in civil engineering work. Civil engineering is centrally concerned with the design and maintenance of infrastructure. Infrastructural technologies are those that are necessary for carrying out a range of other social or technical activities. They generally appears to us as transparent means to other ends, and as a result tend to be less noticed and less analyzed than some other technologies. Of course, it is part of the very nature of technology that we use it as a means to carry out further activity. Infrastructure is not unusual in this respect, yet it can be distinguished from other technologies in a loose way in terms of the *range* of activities it enables.[22] Freeways and mass transit systems, for example, provide for a basic mobility that becomes engrained in the ways we organize our lives. When an earthquake shuts down the Bay Bridge in San Francisco, thousands must figure out, perhaps for the first time in their lives, how to get to work on public transit; if transit facilities were shut down as well, many would hardly be able to make it to work at all. If phone lines go out, it becomes a challenge to coordinate even the most basic social activities. When water lines are being repaired, we go through our daily activities, finding ourselves constantly and unexpectedly reminded of the small but essential ways the instant availability of clean water has worked its way into habits and routines.

Civil engineering is certainly not unique in dealing with infrastructure. The engineers and technicians who design the vast data-transmission networks that make the internet and other communications technologies work, for example, would not be considered civil engineers, though the object of their efforts is clearly a form of infrastructure. What distinguishes civil engineering in many cases is the sheer bulk of the infrastructure it is concerned with. A power grid or telephone system can have vast, even global, scope. However, at a local level, parts of the network can be almost inconsequentially small and vastly stretched out. In contrast, buildings, bridges, highways, dams and the like physically dominate the landscape and dwarf the human form in every location where they appear. This kind of infrastructure is also sometimes tied together into large networks, like road systems, but its distinguishing characteristic is that it is *locally* massive.

Local engineering

This may seem to be a trivial point, a distinction in degree rather than in kind, but in fact it has fairly deep implications for the nature of civil engineering practice. Because they are so locally massive, civil engineering projects often engage the natural, technological and social landscape in a much more direct and significant way than other infrastructural efforts.[23] Civil engineering projects are almost always rooted in the earth or joined to other structures that cannot easily be modified. Civil engineers must find ways to ensure that these junctions are harmonious. The flourishing specialty of geotechnical engineering, for example, focuses entirely on the connection between structures and the rock and soils beneath them. To borrow a technical term, one of the key characteristics of civil engineering practice is that each design project, no matter how routine, has to take into account local “boundary conditions” to an extent that is uncommon in other fields.

Of course, all engineers have to deal with boundary conditions in some sense. The designer of office furniture may go to great lengths to understand the characteristics of the human body that a chair must conform to. Similarly, an aeronautical engineer must take interactions between the skin of a plane and the air into account, and the designer of a telephone must ensure that it can be plugged into and successfully interact with the existing telephone system. But these boundary conditions are all global in character, based either on empirically observed regularities in nature or on the standardization of existing systems. Civil engineers are unusual in the extent to which they have to deal with boundary conditions that are specific to a particular location. The enormous dams of the American west, for example, have undoubtedly caused tremendous damage to the landscapes around them, but at the same time each dam emerges out of the surrounding rock in an oddly organic way. Similarly, a subway tunnel must fit in with the particular layout of underground utilities and take into account the geological conditions along its path. An office chair or a 747 does not need to fit into a specific landscape in this way, though it may need to fit into certain generic landscapes, like carpeted floors or runways.[24] Structures that civil engineers design are almost always site-specific in some way.

Civil engineering projects also engage the social landscape particularly directly. They typically are intended to serve the needs of society at large, or embody a vision of how society might be improved. Because of this, civil engineers devote a good deal of effort to trying to understand social conditions, at least insofar as they are related to the technical problems at hand. Freeway designers, for example, do studies of commuting patterns and driver behavior, develop computer models of traffic flow, and try to anticipate population trends. When they are built, civil engineering projects can transform the social fabric in dramatic ways that are not necessarily considered by designers. In many cities, freeway construction has involved condemning large numbers of residences, causing fundamental changes in the character of urban neighborhoods while encouraging the rapid growth of suburbs. The political controversies that sometimes erupt around civil engineering projects are one indicator of their tremendous impact on local ways of life.

Local problems and global standards

One consequence of the site-specific nature of most civil engineering projects is that only a limited degree of standardization is possible between products. With most engineered products, like microwaves, automobiles, or airplanes, many identical (or nominally identical) copies are manufactured based on a single design. In civil engineering, each product is typically designed individually and produced only once, even if some general features and parts are standard. Yet civil engineers, like other engineers, usually want their products to measure up to certain universal standards, whether for aesthetic consistency, efficiency, or in the interests of safety. Because each structure they produce is necessarily unique in some way, civil engineers must find ways to standardize *design practice* if they are to maintain consistency between products. True, design must follow certain standards in any engineering field, but this issue takes on particular prominence in civil engineering.

There is always a certain degree of tension between universal standards and the application of engineering expertise to particular projects. Advances in engineering practice frequently originate in the interaction between the two. In civil engineering, universal standards are usually embodied in design codes. Codes are formalized documents that specify basic functional requirements, standard details and dimensions, and methods for performing design calculations, and often provide charts and tables to simplify these calculations. They can be enormous, tediously detailed documents, but even the most detailed code is necessarily an abstraction. Codes can never provide a complete set of rules that capture the design process in all its complexity, nor are they intended to. To figure out how they apply to a given design task, engineers must always interpret codes in light of their own knowledge, skill and experience. After years of use, designers learn to depend on them as an integral part of the creative process.[25]

Codes are usually not completely static, however. Over time, the small innovations that engineers come up with in the course of applying the code to particular projects feed back into newer versions, which then push design practice in new directions. New theoretical developments and experimental results work their way in as well. But codes are generally conservative documents that tend to change only slowly. They must satisfy a range of users, so they tend to rely on tried-and-true methods that everyone agrees on and is used to, and are slow to incorporate cutting-edge technical developments, particularly when those developments are a radical departure from past practice. The introduction of new material is further complicated by the complexity of codes and the interdependence of their provisions, which means that changes made in one section may have implications throughout the document. As long as the unformalized aspects of engineering practice evolve slowly, they remain in a kind of equilibrium with code provisions, each supporting the other through long cycles of change. But problems arise when there is a radical shift in the basis of engineering practice, for example when an existing engineering theory is discredited in favor of a new one. Even if engineers are able to quickly put the new approach into practice, codes can be slow to catch up. How, under these circumstances, are the practices of designers to be regulated and made consistent?

Caltrans engineers faced just such a situation in the 1990s as radically new methods were introduced into their design practice by outside peer reviewers, particularly university engineering researchers. Caltrans engineers responded to the increasing irrelevance of codes to design practice by falling back on other modes of professional regulation that are usually overshadowed by formal codes. In particular, they responded by increasing reliance on face-to-face interaction throughout the organization. Peer reviewers interacted intensively with the designers on particular projects. Different groups of designers held more meetings with each other. Retrofit design projects were coordinated by a small group of engineers who met with each design team personally to ensure that they were following consistent procedures. Finally, certain people inside and outside the organization became centralized sources of information and advice that most designers attended to. The suggestion here is not that these more personalized ways of regulating practice are unique to situations of rapid change, but rather that they represent an important aspect of normal engineering practice that comes out more strongly under such circumstances.

As time went by, more and more of the new techniques were incorporated into informal papers and memos that circulated among designers, and computer programs that automated elements of the new methods. These informal documents are gradually being put together into new codes. Examination of this process illuminates some of the strengths and weaknesses of both formal and informal mechanisms for regulating design practice. Because they are formal and have sanctions attached to them, codes are much better than other mechanisms at making sure every designer follows certain minimum standards. The danger, however, is that certain engineers may apply them without great insight, following just the minimum standards instead of doing the best work possible. More informal, personalized approaches may not reach every designer. Yet those who are influenced may gain a deeper and more flexible understanding of design principles through more regular interactions both with experts in a particular area and with their immediate colleagues.

Since the 1970s, the civil engineering profession has sought to capitalize on some of the strengths of more personal modes of regulation by promoting “peer review” of professional practice. This most often takes the form of project peer review, where a group of outside engineers is brought in to assess the work of the project design team. At Caltrans, such peer review panels are often intensively involved in overseeing the design process, meeting monthly or sometimes even weekly throughout the course of a project. The idea is that the experienced engineers on the peer review panels will bring their accumulated wisdom to bear on each project, ensuring that the designers don’t just follow minimum standards, but produce the best design possible given the constraints at hand. Instead of working to an impersonal set of rules, the designer must work to satisfy his or her most respected colleagues.

While such interactions strengthen the sense of community in the profession, and may help protect it from outside oversight, they are something of a departure from the usual social interactions between engineers, and as a result create tension between designers and reviewers. Of particular concern is the possibility that reviewers might try to take over the design process from those they are reviewing, encroaching on their professional autonomy and making them look incompetent in front of colleagues or clients. To address this concern, peer reviewers are often selected for their social skills and ability to maintain a disinterested attitude, and try to manage their actions so as to appear particularly objective and sensitive to the concerns of the designers. These tensions are part of a broader, but usually latent, conflict between individual engineers’ responsibility to the profession and their responsibility to clients and organizations in which they work. Peer review provides a forum in which this conflict can be addressed through personal interaction.

Engineering, perhaps more than any other profession, has been associated with the modernist impulse to transform the world according to principles of reason, ignoring tradition and historical precedent wherever they conflict with this goal.[26] It is often stereotyped as a calculative, impersonal profession, characteristics that are embodied in its reliance on formal codes and procedures. These associations tend to obscure the fact that engineering practice is, in fact, highly dependent on personal interactions and teamwork, mainly because engineers face more complex problems and employ more broadly distributed problem-solving approaches than many other professionals. If codes are more prevalent in engineering, it is in part because of the need to manage this kind of complexity. Codes, in any case, are not simply the products of rational calculation; new methods are gradually incorporated into practice and checked against experience and precedent. The rise of peer review suggests that the engineering profession is actually becoming less dependent on codes and moving toward more personalized, collegial forms of oversight. To gloss over these points is to fundamentally misunderstand the nature of engineering, technology, and even modernity itself.

Civil engineering and State power

While most of this dissertation focuses on the technical setting of engineering practice, it is also important that the group of engineers at the center of the story are employees of, or consultants or advisors to, a government agency. In addition, many of the changes in practice described were made in response to intense political scrutiny following an earthquake that damaged State-owned structures.[27] To put engineering practice in its proper context, in this case, we have to ask why the State should be concerned at all with civil engineering.

Part of the answer has to do with the way civil engineering projects fit into and yet dramatically transform local landscapes. Historically, as Chandra Mukerji has argued, this has made both civil and military engineering projects important tools in establishing State control over territory. Before the 17th century, State power in Europe extended mainly from widely scattered centers whose influence declined with distance. But as the century progressed, and wars were fought, the modern concept of the State as a territorial entity that exercises control within well-defined national boundaries began to take hold. In France, this ideal was realized through the transformation of the landscape by enormous engineering projects, including military fortifications, canals, roads, and bridges. These projects enabled France to mobilize fields and forests for economic and military purposes while serving as “grand and clear” markers of national boundaries, reminding the citizenry of the power of the State throughout its territory, and transforming the landscape so it appeared to be distinctively and naturally French.[28]

For similar reasons, States remain concerned with these sorts of infrastructural projects, which are now largely the province of civil engineers. In the U.S., the Interstate Highway System is a good example. The system was conceptualized largely at the federal level with both military and economic concerns in mind. Military planners sought to emulate the German Autobahn system, arguing that a network of high-speed roads would be crucial to the defense of U.S. territory in a coming land-based war, and later that it would be necessary for the evacuation of cities in the event of a nuclear attack.[29] Others saw a new freeway system as a key to future economic growth, as existing roads were increasingly unable to handle the rapid expansion of commercial and private vehicle traffic resulting from the post-World War II economic expansion.[30]

The system was loaded with national symbolic significance from the beginning. Supporters played on its patriotic implications as a marker of American resourcefulness and progressivism, and on its support of American ideals of individualism and mobility.[31] Highway engineers adopted the aesthetic stance of the "parkway" movement of the 1930s and 40s, which saw roads as a way of enhancing people's experience of the natural landscape and sought to create a harmonious relationship between road alignments and geographical features.[32] Interstate highways at least ideally gave all Americans equal and democratic access to the land within national borders. Because they offered limited access and bypassed existing roads, the Interstates laid the ground for the development of generic roadside "strips," replete with chain stores and restaurants, that are now one of the most characteristic features of the American landscape. As Phil Patton notes, highways have become "as close as anything we have to a central national space" — though presumably in a cultural rather than a purely geographical sense.[33]

The Interstate Highway System also illustrates some of the weaknesses and contradictions of State power in the contemporary U.S. context. Although the program was initiated at the federal level, its actual implementation escaped centralized control. Numerous special interests had to be satisfied — farmers, urban businessmen, truck drivers, for example. Classic conflicts between state and federal power emerged, particularly in light of the fact that the federal government is not, strictly speaking, constitutionally authorized to build roads. As a result, the Interstate program emerged as a system for federally funding highway construction, leaving the actual design and choice of routes to the states, within certain broad federal guidelines.[34] As the new freeways began to penetrate cities and displace neighborhoods in the 1960s, local discontent emerged, and some local governments successfully mobilized against federal and state highway plans.[35]

As this example illustrates, civil engineering projects in the contemporary U.S. context have been seen as means for marking and controlling territory, as indicators of State power in local settings, and as symbolic expressions of American identity and ideals. But engineering is not a straightforward vehicle for State power in this setting as it was under the absolute monarchy of Louis XIV. When civil engineering projects are seen as symbols of State power, more often than not it is in the context of local political resistance to state and federal plans. Still, people seem to view these projects as a legitimate and necessary functions of the State, and failures of civil infrastructure are often taken to be clear, nonpolitical indicators of bad government. Finally, because engineering is regulated largely at the state level, and many large projects are carried out by states rather than the federal government, the relationship between civil engineering and the State in the U.S. context is sometimes more accurately characterized as a relationship between civil engineering and the states.

The State also makes use of civil engineers in their capacity as expert advisors, in much the same way as it makes use of scientific advisors. Although it funds scientific research, the government grants considerable autonomy to the scientific community, allowing its members to regulate the quality of research and the allocation of funding, within certain boundaries, through processes of peer review. In return, scientists provide the government not only with research products that have military and economic implications, but with expert advice for making policy. Their status as independent experts is used by the government to lend legitimacy to its decisions, and because of this scientists are sometimes able to have significant impact on policy choices.[36] But scientists' autonomy in this area is limited because the government controls the arenas in which their advice is given.[37]

Many engineers, particularly civil engineers, are likewise dependent on government funding or government employment, both as designers and as academic researchers.

They are granted considerable autonomy to regulate their own professional practices and, in the academic context, to allocate research funds and evaluate research results. They provide the government with concrete products by designing and doing research on infrastructure with military and economic implications. Increasingly, civil engineers are being called upon to serve on advisory panels which review design and construction practices for government agencies. Like their scientific counterparts, the engineers on these panels have a certain amount of power to effect change, but the government is ultimately in control of the arenas in which they participate. As a result, their participation as advisors can lend legitimacy to government decisions even if they do not completely agree with those decisions.

1.5 Earthquake engineering

Development of a professional field

The first efforts to systematically understand earthquake damage to buildings and other structures took place in Japan, beginning in the late 19th century. By the 1920s, Japanese engineers had worked out a basic principle of many later codes, suggesting that buildings be designed to stand up to a horizontal force equal to some percentage of their weight.[38] Interest in earthquake effects picked up in the U.S., specifically in California, after the 1906 San Francisco earthquake, through the creation of the Seismological Society of America.[39] Among engineers, significant interest began to develop in the U.S. after the devastating 1923 earthquake in Tokyo, which was followed by a smaller quake in Santa Barbara in 1925.[40] A group of California civil engineers invited Kyoji Suyehiro, a top earthquake researcher from the University of Tokyo, to present several lectures on seismic design. Subsequently, these lectures and other works by Japanese researchers were published and widely circulated among civil engineers in the state.[41] Japanese-inspired approaches were gradually incorporated into California design regulations, particularly following the 1933 Long Beach earthquake, which damaged a number of public schools and prompted state legislation specifying seismic requirements for schools and other buildings.[42]

Some of the early experimental research in earthquake engineering in California was done by practicing engineers working independently in garages and basements.[43] During the 1930s, researchers were also beginning to become established at academic institutions. Theoretical and experimental work on the dynamic behavior of structures was pushed forward by groups at Stanford University and the California Institute of Technology (Caltech). Researchers at Caltech pioneered the spectral analysis of earthquake records, which were just becoming available as seismographic instruments became more common. This type of analysis enabled engineers to determine how much a building with a certain fundamental period of vibration would move in response to an earthquake, and is still widely used. Because of the ready availability of fast computers, the necessary spectral calculations are now routine, but at the time researchers had to rely on laborious mechanical methods.[44] The results of this research informed design codes in California beginning in the 1940s.[45]

The institutional basis for the current professional specialty of earthquake engineering was laid down in the late 1940s and 1950s. During the 1930s, California engineers had formed the Structural Engineers Association of California to address engineering concerns particular to the state. Beginning in the 1950s, this organization developed and published influential seismic design criteria that formed the basis of many subsequent codes.[46] In 1949, a small group of California engineers formed the Earthquake Engineering Research Institute (EERI), initially to try to raise money for the installation of seismographic instruments so that more earthquake records could be obtained, and to obtain research funding in general.[47] The Institute began with 12 members, and at first expanded slowly and by invitation only.[48] In 1956, it organized the first World Conference on Earthquake Engineering. The conference drew about 140 participants from earthquake-prone countries around the world, and 40 papers were presented. In 1960, a Second World Conference was held in Japan, and the Japanese initiated the International Association for Earthquake Engineering. This group became an umbrella society encompassing many national earthquake engineering societies, including the EERI, and sponsored future World Conferences on Earthquake Engineering.[49] By the 1990s, each of these conferences was attracting well over 1000 papers. The EERI adopted an open membership policy in the 1970s, and its membership now exceeds 2,000. Although the Institute has continued to fund research, it now functions as something of a professional society for both practicing engineers and academic researchers in earthquake engineering and produces numerous publications and reports, including the journal *Earthquake Spectra.*[50]

The rise of State interest in earthquakes

During the 1960s, the federal government began to take an interest in earthquake-related matters. Partly, this had to do with the federal government's increased funding of scientific research and greater reliance on expert advice in the post-World War II era. Engineering found a place in Washington in 1964 through the creation of a National Academy of Engineering to complement the existing National Academy of Sciences.[51] Cold War fears and the development of nuclear technologies were also important. In the early 1960s, the Department of Defense began to heavily fund seismological research in order to develop methods for detecting nuclear blasts. It also funded research to investigate the potential structural effects of U.S. nuclear testing in Nevada on buildings in Las Vegas.[52] At the same time, the Nuclear Regulatory Commission started supporting research in engineering, geology, and seismology in order to develop better methods of assessing the earthquake risk to nuclear power plants.[53] Finally, the Defense Department, and later the National Academy of Sciences, sponsored a great deal of research on the effects of natural and technological disasters on urban environments, as analogs to a nuclear attack. Through these various efforts, disaster research was established as a legitimate concern of the federal government.[54]

These developments coincided with a huge 1964 earthquake in Alaska. Along with a smaller earthquake in the San Fernando Valley near Los Angeles in 1971, this was a watershed event that sparked increased government attention to earthquakes during the 1970s, culminating in the passage of legislation creating the National Earthquake Hazards Reduction Program (NEHRP) in 1977.[55] This program distributed responsibilities among several federal agencies. The Federal Emergency Management Agency coordinated the program and managed earthquake preparedness and local mitigation efforts, such as implementing stricter building codes and retrofitting existing structures. The U.S. Geological Survey supported research in seismology, while the National Science Foundation (NSF) and the National Institute of Standards and Technology (NIST) took the lead in funding engineering research and the development of new code approaches.[56]

The original NEHRP legislation put more emphasis on seismological research than on other elements of the program, specifically focusing on prospects for predicting earthquakes. At the time, many seismologists felt confident that a 5- or 10-year research program could lead to accurate predictions, and this possibility seems to have motivated many members of Congress to support the legislation.[57] The prominence of prediction was enhanced by the lead role taken by presidential science advisor Frank Press, a geologist, in shaping discussion of the earthquake problem in government circles during the 1960s and 70s. Earthquake engineering advocates did not have this prominence, but were able to realize elements of their agenda in the legislation.[58] Funding through NSF and NIST was the basis for the expansion of earthquake engineering programs at universities in California during the 1970s and 1980s, particularly at U.C. Berkeley, and at universities around the country, most notably the State University of New York at Buffalo, where the NSF-funded National Center for Earthquake Engineering was established.[59]

Meanwhile, in California, a number of policy documents and legislative initiatives appeared in the wake of the Alaska earthquake. State Senator Alfred E. Alquist took an interest in seismic issues as a result of some of these reports, and took the lead in setting up a joint legislative committee to examine earthquake hazards in 1970. In the wake of the 1971 San Fernando Valley earthquake, Governor Ronald Reagan's administration established the Governor's Earthquake Council. The joint legislative committee wrapped up its business in 1974, producing an influential report which recommended, among other things, the creation of the California Seismic Safety Commission to continue the work of the committee and the Governor's Earthquake Council. The Seismic Safety Commission continues to play a major role in promoting legislation on earthquake safety and planning issues in the state.[60]

Earthquakes are seen primarily as a threat to existing civil infrastructure. No wonder, then, that the politics of the earthquake threat have been driven by many of the same factors that shape large-scale civil engineering projects. Government funding of research and mitigation efforts stemmed initially from military concerns, and later was justified in economic terms and in terms of saving lives, but with military motives never far out of the picture — just as in the case of the Interstate system. Highways and other large engineering projects are central to State control of territory for both military and civilian purposes, and therefore must be protected from disaster.

The politics of earthquake risk also embodies a characteristic tension between state and federal control. As sociologist Robert Stallings notes, advocates of the NEHRP legislation had to make a major effort to convince members of Congress that earthquakes were a national problem, not just limited to California, in order to pull enough votes together. In addition, the NEHRP's emphasis on developing strategies for mitigating earthquake risk can be seen as a federal effort to place the financial responsibility for disasters more at the state level, since the states are ultimately responsible for building codes, retrofit programs, and the like. Many states would prefer to rely on federal disaster aid after the fact than make massive investments of state funds in advance.[61] California's more active approach to addressing earthquake hazards has been something of an exception to this rule.

Californians expect, almost as a matter of faith, that the state will protect vital infrastructure from earthquakes. This faith generally leads to a certain complacency about the earthquake threat among the general public. The other side of the coin, however, is that when earthquakes do cause damage to civil infrastructure, especially with loss of life, people initially respond with incomprehension and outrage. The reaction of politicians, the news media, and the public to damage caused by the Loma Prieta earthquake followed this model. Initial assessments assumed that some sort of blatant misconduct occurred, involving either shoddy engineering practices, construction mistakes, or lack of concern about structures known to be in imminent danger of collapse. Much of the initial scrutiny, therefore, fell on Caltrans engineers, with politicians vowing to get to the bottom of the problem. Once Caltrans engineers were able to gain access to the media, however, a different story emerged of an agency strapped for funds with little support forthcoming from the governor or legislature for necessary seismic upgrades.

From this point onward, the media reported the issue as a standard political controversy, and substantive questions about how Caltrans ought to conduct its design and retrofit efforts were left for the engineering profession to resolve.

The politics of earthquake engineering issues seems to be dominated by technical experts to an even greater extent than general civil engineering issues. Most of the political activity surrounding civil engineering projects has to do with issues of funding or siting rather than technical feasibility. Code requirements that deal with gravity loads or simple structural details are usually based on many generations of experience, and often may be understood by people without specialized training in engineering. Earthquake engineering requirements, by contrast, are based on extremely complex analytical methods and experimental research that requires a great deal of expert interpretation to be understood by people without technical backgrounds. Earthquake engineering as a field is also presently in a state of constant flux. Many theories on how to design earthquake-resistant structures are relatively new, and there is still no overwhelming consensus about what approach is best either within the academic community and between researchers and practicing engineers.

This lack of consensus might make earthquake engineering issues vulnerable to "deconstruction" in political arenas, as has happened with many environmental issues.[62] In general, though, earthquake safety has not been a particularly salient political issue, even in California, and political actors seem to prefer to let the earthquake engineering community deal with technical problems internally.[63] Accordingly, the issues raised by the Loma Prieta earthquake were turned over by the political establishment to a board of inquiry composed mainly of engineers. In the end, political discussion focused mainly on the issue of funding. In many cases, defining a problem as technical and delegating its solution to a professional forum is a way for politicians to isolate themselves from debates that could potentially be politically explosive. A group of technical experts can usually be relied upon to reach consensus where a more political process might generate further controversy.

Research, design practice, and earthquake risk

The funding provided by NEHRP spurred a boom in earthquake engineering research and the development of a number of new approaches. Early research on the dynamics of building shaking had assumed elastic behavior, as if a building vibrated like a tuning fork in response to an earthquake (though much more slowly, and with much more complex motion). But a structure can actually absorb a great deal more earthquake energy than these models suggest if it is able to deform beyond an elastic state without suffering too much damage. The ability to do this, called ductility, became a new focus of research beginning in the 1960s. Steel is a naturally ductile material, but some engineers had doubts about concrete. This led the Portland Cement Association to sponsor fundamental work on the use of steel reinforcement to make concrete more ductile.[64] This was the first in a long line of research on the ductility of concrete, involving a great deal of laboratory testing, that is only now being incorporated into codes in a comprehensive way. Improvements in computer technology seem to have been the impetus behind a number of other developments in the field, including improved elastic models; finite element analysis, where a structure is modeled as an aggregate of many tiny material elements; and time-history analysis, which enables engineers to calculate the movement of a structure in tiny time increments without having to assume elastic behavior.[65]

There is a big jump to be made, however, between coming up with new methods in an academic setting and applying them in practice. In recent years, the increased funding of academic researchers by agencies like the NSF may have contributed to this problem by allowing them to build careers without having to respond directly to the needs of practicing engineers. More fundamentally, the very general analytical approaches developed through research need to be brought to bear on specific structures. It is the rare theory that can be used in structural design without making many assumptions and analytical simplifications, just because real structures have a lot of complex characteristics that are difficult to describe analytically.

One of the key characteristics of engineering assessments of risk is that what engineers know about the risk posed by a particular object is difficult to separate from their definitions of what the object is and how it works in other respects. In the Caltrans case, a simple freeway overpass started out as a relatively straightforward structural system; its vulnerability to earthquakes was characterized in terms of the ability to resist a small lateral force. Learning from the 1971 San Fernando Valley earthquake, engineers focused their seismic concerns on very specific structural components: first, the expansion joints between segments of bridge deck, and then on the steel reinforcement in columns. They brought various analytical approaches to bear on the design of these specific elements, and conducted tests of the resulting designs. New computer tools became available that enabled designers to model overall seismic response using the latest analytical methods. Response spectrum approaches were introduced that enabled engineers to think in a much more sophisticated way about the relationship between a structure and the land on which it stood, specifically noting the characteristics of the soil and the depth to bedrock. New seismic maps were developed that characterized earthquake risk geographically in relation to known faults. Many of these new techniques were based on the research community's rapidly-evolving understanding of seismic risks. Caltrans also funded some research at UCLA on the performance of restraining devices for expansion joints, the results of which were incorporated into design codes.

By the 1990s, engineers' understanding of the risks posed to Caltrans bridges by earthquakes was extremely complex, incorporating very specific insights into the behavior of specific structural elements as well as general knowledge of how earthquakes affect structures as a whole, and even some formal principles of risk assessment. As definitions of risk evolved, so too did designers' knowledge of their bridges. A Caltrans engineer looking at a bridge today, even a simple overpass, most likely sees a far more complicated object than his or her predecessors did in the 1930s. And, just as importantly, engineers' knowledge of the landscape and how their structures interface with it has never dropped out of the picture — instead, it has redoubled in complexity and become more central than ever before.

One of the key problems faced by earthquake engineering is the relative infrequency with which its hypotheses are tested in the field.[66] If a certain design method produced buildings that could not stand up to gravity, engineers would soon know about it. But they may have to wait for many years to see a structure subjected to the seismic forces it was designed for. For this reason, “chasing earthquakes” — trying to visit earthquake locations in the immediate aftermath in order to observe structural damage — has become a major professional activity. Adding to the difficulty, every earthquake features a unique set of ground movements, and may produce effects never seen before. Furthermore, it is often hard to tell precisely what caused a particular structure to collapse once it is reduced to rubble.

Because of these uncertainties, laboratory testing has become a central activity in earthquake engineering. In the controlled environment of a laboratory, engineers can simulate earthquakes and document precisely how structures respond over time, a luxury which is not available to the engineer observing a collapsed structure in the field. Structures can be simplified to provide clearer results. Test models can have instruments built into them that measure their response in exacting detail, providing vast amounts of data for analysis.

There are a number of testing methods available to researchers, ranging from those using a “shake table” that can play back the recorded motion of an actual earthquake to tests in which a structural element is pushed back and forth by a hydraulic arm in a very simplified, slowed down representation of earthquake motion. Interestingly, many researchers prefer the simpler tests, not only because they are easier to conduct and to document, but because they represent the motion of an earthquake in a very generalized way, rather than using a record of a particular quake. This makes them more readily applicable to a range of actual earthquakes. These very generic results are also useful because they can easily be used to calibrate computer models. Researchers then rely on the computer models as tools for determining how a particular structure outside the laboratory might respond to a particular earthquake.

But the knowledge generated by research is rarely imported into the design context simply through the transfer of computer programs or test reports. Instead, a complex chain of personal interactions seems to be necessary. The relationship between researchers and designers is only one element of this chain. First, researchers rely on well-trained teams of laboratory technicians to bring knowledge of construction techniques into the laboratory so the test models will accurately represent structures as they are built outside the laboratory. Second, in this laboratory, researchers relied on graduate students to integrate the knowledge of these technicians with research considerations in order to carry out a successful testing program. Finally, the professors who run the laboratory serve as mediators between the laboratory setting (through their supervision of graduate student research) and the world of design. They travel to design organizations like Caltrans in order to communicate research results and provide advice in integrating these results into design practice. This personal interaction seems to be the most effective way to bring research results to bear on design problems, because it allows a flexible accommodation to be reached between designers and researchers, instead of leaving designers to struggle with unwieldy academic products that are perhaps not immediately relevant to their needs.

1.6 Meeting face to face

The social world of earthquake engineering has many internal divisions. It includes academic researchers as well as practicing engineers, engineers working in the public sector and the private sector, engineers working in different firms and different research laboratories, and engineers specializing in narrower sub-fields like soil-structure interaction or structural risk assessment. And there are participants who are not engineers at all, like some managers at Caltrans and the technicians who work in research laboratories. There are often real cultural differences between all of these groups. Even though most engineers have similar social backgrounds and share an overall professional culture, those who work in one setting or on one sort of project often have relatively little experience with the technology, the theoretical tools, the nomenclature, and the work practices that are common elsewhere. If these different groups are to work together successfully, they must find ways to translate concepts and coordinate activities between them.

The necessity of this sort of coordination is a theme that runs throughout the thesis. Scientific and engineering research and the design and construction of technological artifacts are extremely complex tasks. They require the participation of a wide range of actors possessing very specific, highly developed sets of skills and bodies of expert knowledge. But if laboratories are to produce new truths about nature or technology, and if designers are to produce functioning objects, the activities of these disparate categories of actors must be coordinated somehow. Fortunately, they need not share a common view of the world in order to work together. Instead, the various specific settings in which technical work is performed can be linked at their boundaries. This can be accomplished by the circulation of "boundary objects" that cut across work settings, such as computer models, design standards, research reports or even very broad representational conventions that everyone understands.[67] These devices seldom suffice, however. Work settings are most significantly linked through the activities of particular individuals — such as the the engineering laboratory technicians, graduate students, and professors discussed above — who have roles in, and knowledge of, two or more work settings. Through long chains of intermediaries like these, widely separated work settings and even distinct social worlds can be linked even if they have very little in common, enabling knowledge and technologies to be effectively transferred between them. Personal interactions are the glue that holds these chains together.

If engineering work were simply a process of rationally adapting technical means to well-defined ends, the transfer of information in formalized forms would be sufficient for the successful coordination of engineering activities. Anthony Giddens, along with other social theorists of modernity, argues that social life is increasingly being "disembedded" by impersonal tools like these, which enable the coordination of work across vast expanses of space and time by eliminating the traditional need for face-to-face interaction in localized settings.[68] He does note, though, that there are some exceptions to this trend toward the disembedding of social relations, which he calls "reembedding" mechanisms. Peer review in engineering is a good example of what Giddens has in mind with his use of this term.[69]

The study presented here suggests that either the idea of disembedding as the defining feature of modernity is incorrect, or the engineering profession has never been as clearly modern as we might like to think.[70] In fact, engineering still depends to a great extent on face-to-face social interactions and teamwork, and wherever communication successfully occurs across spatial or cultural divisions, there is usually a physical movement of people across those divisions. The transfer of computer models and research reports alone do not seem to be sufficient to allow Caltrans engineers to adapt research results to design problems. Instead, we find that researchers frequently pay visits to Caltrans to explain their ideas and research results, that Caltrans engineers travel to universities to observe laboratory tests, and that certain engineers at Caltrans have the training to be to understand academic ideas and research results and translate them into computer programs and documents that are useful to practicing designers. In such a thoroughly "embedded" profession, it makes little sense to talk about reembedding social relations. The formal institution of peer review is just an explicit articulation of this general reliance on face-to-face interaction and the circulation of people between social settings. Maybe the professions are anomalies, exceptions to an overall trend that will soon overwhelm even them. But studies like this raise questions that ought to be addressed by theorists of modernity.

Notes

[1] The concept is drawn from symbolic-interactionist sociology, and is described concisely in Strauss 1978. Examples of how a social worlds perspective has been applied to the study of science and technology include Kling and Gerson 1978, Gerson 1983, Star and Griesemer 1989 and Fujimura 1992. A useful overview of the approach is given in Garrety 1997. See also Becker 1982.

[2] Here I am thinking of works like Ellul 1964, Marcuse 1968, Habermas 1970 and Braverman 1974.

[3] See Staudenmaier 1985 for a history of these various approaches.

[4] Hughes 1983.

[5] See, for example, Pinch and Bijker 1987, Bijker 1995 and MacKenzie 1989.

[6] The term "heterogeneous engineering" is John Law's. See Law 1987.

[7] MacKenzie 1990.

[8] Latour 1996.

[9] Law and Callon 1992.

[10] Vincenti 1990, especially pages 200-240.

[11] Bucciarelli 1994, 62-63.

[12] Henderson 1999, especially pages 25-57, 135-183.

[13] Professional Engineers Act, California Business and Professions Code Sections 6700-6799, Chapter 7: Professional Engineers, Section 6731, "Civil Engineering Defined." Available (April 11, 2000) on the web site of the California Board for Professional Engineers and Land Surveyors at http://www.dca.ca.gov/pels/e_exam.htm.

[14] This term is used on the website of Carnegie Mellon University, "Civil and Environmental Engineering: Department Overview: General Information," http://www.ce.cmu.edu/Overview/ (October 23, 1999).

[15] Straub 1964, 118.

[16] Here I focus largely on the U.S. context. Alder 1997 examines the relationship between French military engineers and the state during the French revolution, while Mukerji 1997 describes an early stage in the application of military engineering methods to civilian projects — in this case, 17th-century French formal gardens. Porter 1995, 114-147, describes the 19th-century rise of the French Corps des Ponts et Chausées, which pioneered many of the methods of modern civil engineering.

[17] Merritt 1969, 5.

[18] Layton 1971, 29.

[19] Ibid., 29-30.

[20] Ibid., 30.

[21] The civil engineering attitude is described in Merritt 1969, 6; Layton 1971, 25-46, describes the conflicting agendas of the different engineering societies.

[22] A definition of infrastructure that includes the characteristics "transparency" and "reach or scope" can be found in Bowker and Star 1999, 35 (originally from Star and Ruhleder 1996). Their definition is clearly meant as an ideal type; here I put more emphasis on the continuity between infrastructure and technology in general.

[23] On the use of engineering representations to visualize the relationship between a structure and the surrounding landscape, specifically in the context of a Caltrans design project, see Suchman, forthcoming.

[24] One can imagine a designer who makes customized chairs to fit individual bodies, but such a person would probably be categorized as an artisan rather than an engineer. 747s are able to land on runways all over the world because civil engineers have already taken local conditions into account in the design of the runways.

[25] See Bucciarelli 1994, 131-132 and Star 1995, 97-98.

[26] See Alder 1997, 15, where this point is elaborated in connection with the French revolution.

[27] "State" is capitalized here to refer to a generic national political entity, distinct from an individual U.S. state, which is left in lowercase.

[28] Mukerji 1997, 1-38.

[29] Moon 1994, 5.

[30] Rose 1990, 29-30.

[31] Patton 1986, 89.

[32] Ibid., 127-141; Kemp 1986.

[33] Patton 1986, 2; on the democratic nature of roads and the desire to see the land, 19, 232-234; on roadside strips and franchises, 155-225.

[34] Patton 1986, 91; Moon 1994, 16.

[35] Rose 1990, 101-117; Patton 1986, 102, refers to controversy over urban Interstates as the "highway Vietnam."

[36] See Jasanoff 1990, 229-250, for a review of the political use of expert advice.

[37] This point, along with a more general argument about the tension between scientific autonomy and dependence on the State, is made in Mukerji 1989; see especially pages 190-203.

[38] Scott and Olson 1993, xv.

[39] Ibid., xiv.

[40] Ibid., xv, xvi.

[41] Degenkolb 1994, 29; Scott and Olson 1993, xvi.

[42] Scott and Olson 1993, xvii.

[43] Ibid., xx.

[44] Ibid., xix-xx; Housner 1997, 23-36.

[45] Scott and Olson 1993, xx.

[46] Ibid., xviii, xxi.

[47] Ibid., xxi; Housner 1997, 131-133.

[48] Housner 1997, 136.

[49] Ibid., 134-136.

[50] See Scott and Olson 1993, xxi.

[51] Stallings 1995, 180.

[52] Ibid.; Scott and Olson 1993, xxv.

[53] Scott and Olson 1993, xxvi.

[54] Stallings 1995, 180-181.

[55] The rise in interest was remarkable: sociologist Robert Stallings notes, for example, that Congress produced only 12 documents on earthquakes during the 1960s, an increase from previous years, but a small number compared to 99 produced during the 1970s and 119 produced in the 1980s. Ibid., 176.

[56] Ibid., 78-79.

[57] This optimism was in part the result of a rapidly settling consensus around the theory of plate tectonics in geology at this time. See Stallings 1995, 50; the history of plate tectonics is detailed in Stewart 1990 and LeGrand 1988.

[58] For discussion of the conflicting perspectives of geologists/seismologists and engineers on this issue and its prominence in congressional hearings and legislation, see Scott and Olson 1993, xxii-xxv and Stallings 1995, 50-51, 74-76.

[59] George Housner notes that California engineers were extremely chagrined by the location of this center far from seismically-active areas (Housner 1997, 146-147).

[60] Scott and Olson 1993, xxii-xxiv, xxvi, 97.

[61] Stallings 1995, 79-81.

[62] See Jasanoff 1990, 13, 37-38, and the case studies on pages 84-207.

[63] On the low political salience of earthquake issues in general, see Stallings 1995, 2-5. On the relative lack of political attention to the earthquake threat compared to other environmental hazards, see Geschwind, forthcoming.

[64] Blume *et al.* 1961.

[65] For a description of the various analytical methods now available, including those discussed here, see Priestley *et al.* 1996, 226-249.

[66] The idea that every new structure is a hypothesis waiting to be disconfirmed by structural failure is discussed in Petroski 1985, 40-52.

[67] Star and Griesemer 1989.

[68] Giddens 1990, 21-36, 88-92.

[69] Ibid., 79-88.

[70] Bruno Latour claims, *contra* Giddens, that we have never been modern at all, that the only distinguishing thing about the "moderns" is that they deny the premodern blurring of the human and the nonhuman while blurring the line more then ever in practice — see Latour 1993. From this perspective, Giddens should give roughly equal weight to disembedding and reembedding as characterstics of modern society, and pay more attention to their mutual dependence. I suspect that modernity is characterized as much by the proliferation of new modes of embedding social relations as by the disembedding Giddens focuses on.

Chapter 2

Constructing Risk at Caltrans

2.1 Introduction

The culture of risk at Caltrans has changed dramatically over the past 70 years, at least as it concerns seismic issues. From the 1930s until 1971, no earthquake caused any significant damage to transportation structures in California. There was only a vague sense of the earthquake threat at Caltrans, and it played little part in design practice. In 1971, the San Fernando earthquake near Los Angeles caused catastrophic damage to a freeway interchange under construction, much to the surprise of Caltrans engineers. This led to an intensive effort to understand and compensate for earthquake effects on bridges that has continued, with some ups and downs, to the present day. The 1989 Loma Prieta earthquake in the San Francisco Bay area drew a great deal of public attention to Caltrans, since it was the first earthquake to cause a significant number of deaths on freeway structures. Caltrans engineers, however, felt that the Loma Prieta quake told them nothing new. This chapter focuses on the construction of risk at Caltrans, and tries to explain why it was that these two earthquakes had such different impacts.

This chapter looks at how risk is defined in a technical context, but it is not about "risk assessment" or "risk management" as those terms are used by professional risk analysts. Instead, it shows that the activities of analyzing and designing structures, deciding how to allocate resources, and other normal practices in an engineering organization can in themselves generate sophisticated definitions of risk, before risk analysts or the general public arrive on the scene. In order to explain how this occurs, I make use of Stephen Hilgartner's concept of "risk networks," which are sociotechnical networks that contain all the elements we consider when we attach risks to particular objects. I argue that these networks are best understood as descriptions of the way particular "risk communities" conceptualize the risks they work to resolve.

At Caltrans, the risk network relating to seismic hazards changed dramatically during the 1970s and 80s. One important cause of this change was the expansion of the risk community to include researchers and outside peer reviewers. Changing a risk community in this way almost always has a significant impact on the shape of that community's risk network. Another important cause was the increasing incorporation of earthquakes themselves into the risk network. The first earthquake Caltrans engineers experienced in the 1970s had an impact by shocking them into a sudden realization that the measures they had taken to make structures earthquake-resistant were inadequate. As time went by, however, organizational routines were established for evaluating damage and smoothly incorporating the knowledge gained from each subsequent quake into design practice. These routines "domesticated" what had previously been seen simply as disastrous events.

The sociology of risk and risk objects

Attempts to describe the social dimensions of risk have, until recently, been heavily influenced by the psychological literature on risk perception. One strand of this literature attempts to explain why different types of risk cause different psychological reactions in members of the general public, as measured by survey responses.[1] Another strand focuses on cognitive biases that influence the ways both the public and experts estimate risk.[2]

Mary Douglas and Aaron Wildavsky's influential *Risk and Culture* added a sociological and anthropological slant to the analysis of risk perception. Rather than examining individual risk perceptions as a psychological phenomenon, they attempt to explain group variations in risk perception according to social structure, focusing particularly on environmental pollution. They argue that social groups which lack internal differentiation, but which emphasize the boundary between themselves and the rest of the world, tend to be most concerned about pollution. This is exemplified by close-knit, egalitarian religious communities like the Amish, who try to prevent corruption of their way of life by isolating themselves from mainstream culture. Douglas and Wildavsky claim that this type of social structure is also common among radical environmental groups, and explains their extreme concern about pollution.[3] This argument is problematic because it suggests that cultural ideas about risk are shaped solely by social structure. Like the constructionist social problems literature, it neglects to examine the practices of the professional communities in which risks come to take on objective properties, and it also fails to consider how members of the public, including activist groups, actually find and analyze the information which shapes their beliefs.[4]

In addition to studies of risk perception, the sociological literature on risk has another important side that focuses on how organizations manage high-risk technologies, describing the organizational factors that can contribute to technological accidents. The best-known work in this genre is Charles Perrow's *Normal Accidents*.[5] Perrow focuses on the characteristics of technological systems, including their human components, which make them prone to accidents. The riskiest systems, he argues, are those that combine a high degree of complexity with tight coupling between components, meaning that the failure of one component cannot easily be contained, but will tend to disrupt the entire system.[6] Others have looked at this issue from a slightly different perspective, examining the characteristics of "high reliability organizations" which are able to manage complex, tightly-coupled systems without accidents.[7]

Risk objects, risk communities and risk networks

Sociologists who study social problems have examined how risk is defined and discussed in public discourse. Much of this work follows what is called the "social constructionist" approach in the field. Instead of trying to describe problems themselves as objective social conditions, this approach looks at how different groups mobilize to make claims and define social problems in public arenas.[8] As a methodological point, most constructionist social problems authors avoid examining how problems are defined in scientific or professional contexts before they become public issues.[9] The portion of this literature that focuses on risk, however, has been influenced by the sociological literature discussed above. As a result, some of this work has made the interaction between scientific/professional and public definitions of risk a central focus of analysis.[10]

Examining public settings alone leads some social problems researchers to objectify certain elements of the risks they study. In particular, concepts drawn from professional settings are sometimes used unproblematically, since these settings are not themselves examined sociologically. For example, crime statistics or medical ideas about the health risks of chemical exposure might be treated as objective facts even as public debates about how to deal with these problems are analyzed as social constructions.[11] As a corrective to this trend, sociologist Stephen Hilgartner has suggested that we ought not to think of risk as a characteristic which people simply attach to objects which are already well-defined. Instead, we should understand that an attribution of risk usually involves the active construction of an object deemed to pose a threat, which he calls a "risk object."[12] For example, as Joseph Gusfield has shown, drunk driving never really emerged as distinct risk to the general population until it was embodied in a particular risk object, the "killer drunk."[13] By describing risk as embodied in particular objects, this approach shows us how definitions of risk are constructed not only through activities explicitly labeled as risk assessment, but through a wide range of professional practices that seek to define other aspects of these objects. Medical research on the prevalence of alcoholism, for example, entered into public discourse, suggesting new ways of defining and mitigating the risks of drunk driving.[14]

Drawing on the constructivist science and technology studies literature, Hilgartner argues that risk objects are defined in relation to broader socio-technical systems which he calls "risk networks." A risk object is *emplaced* within a network when actors identify it as being potentially harmful. Once a risk object is emplaced, certain actors — engineers, for example — may then put a great deal of effort into *displacing* it from the system, either by showing that it does not actually pose a risk, or by putting mechanisms into place to prevent it from causing problems.[15] These measures may shift the risk to other parts of the network, resulting in new risk objects and new efforts to displace them.

There are several other important characteristics of risk objects and risk networks that are not discussed by Hilgartner. First, the activities of emplacing and displacing risks are not usually completely independent from one another. Which elements of a system are singled out as risk objects often depends upon the technological solutions that are available at a given time. In some cases, the relationship between the process may approximate James March and Johan Olsen's "garbage can" model of organizational decisionmaking, which notes that solutions often float around an organization as people actively seek and promote problems they can be applied to.[16] Similarly, advocates of a new technology may actively seek to promote risk objects which that technology could be used to displace. This is a common advertising tactic. For example, manufacturers of luxury automobiles have played up the dangers of getting lost or stranded as a way of promoting Global Positioning System devices in their cars. Without the availability of that technology, the idea that not knowing one's exact latitude and longitude while driving could be seen as an unnecessary risk would seem absurd. In other cases, it is not a specific solution but rather a new type of technology — computers are the best recent example — which suggests new problems by presenting a range of possible solutions. Then again, established technologies can just as easily limit the problem agenda by ruling out certain risks for consideration because no solutions are available.

It is also useful to distinguish between "risk networks" and "risk communities." Risk networks can contain many different sorts of elements that are relevant to the definition of risk objects, including both people and natural and technological objects. For example, sociologist Robert Stallings describes the things that the disaster policy community has considered in its definitions of earthquake risk. These include water systems, power lines, transportation facilities and other "lifeline" installations; earthquake-caused fires; the impact of media coverage and non-expert earthquake predictions on earthquake preparedness; the characteristics of vulnerable social groups such as children, the disabled, minorities, and non-English speakers; and the latest geological maps, demographic data, and engineering methods.[17] Though this risk network includes people, most of those who are considered are not actually allowed to actively participate in the definition of seismic risk as a policy issue. At the same time, many people who do have an active role in defining the risk are not considered to be significant elements in the network itself. Further, different groups may construct different risk networks around similar sets of objects, particularly if the groups don't interact very much.

As a result, it seems useful to specify that a risk network is always attached to a particular "risk community," and that it reflects that community's collective views about what aspects of the world are important in the definition of risk. These communities are composed of groups of people who interact in the definition of a particular set of risks, and who share, at least to some extent, a common set of cultural assumptions and a common language for talking about risk.[18] A risk network is the complete set of natural, technological, human, conceptual, moral, etc. resources invoked by these groups in the course of their risk definition activities.[19] Risk communities are often, although certainly not always, coincident with professional or organizational boundaries. Changes in their membership and scope can have a significant effect on the definition of risk objects and on the composition of the risk network. Since many decisions about risk are made within those restricted boundaries without ever becoming public issues, or are defined for public consumption within these arenas, change in who is allowed to participate can be a matter of some political significance.

Risk and engineering

This chapter examines the history of seismic risk definitions among engineers at the California Department of Transportation (Caltrans) from the early 1970s through the 1990s. Although the focus here is on risk, these engineers, most of them designers, could not themselves be considered experts in this area. There is a field of structural reliability analysis, which develops tools for analyzing the probability of failure of a given structure under a specified load, but this is a specialty field most practicing engineers are not trained in.[20] But even if they do not usually speak in the language of risk analysis, practicing professionals in organizations like Caltrans play a central role in the definition of risk because they are responsible for the emplacement and displacement of a great variety of risk objects.[21] During this time period, it was Caltrans engineers who played the largest role in defining the risk posed to bridges by earthquakes in California. They decided what elements of bridges posed the greatest risk under earthquake forces, what measures should be taken to mitigate those risks, in what order those measures should be implemented, and (within certain externally imposed financial restraints) how quickly work should proceed.

2.2 Earthquake risk at Caltrans before 1971

The risk of earthquake damage to bridges had long been a concern of Caltrans engineers and their predecessors at the California State Highway Department. However, prior to the 1960s, very little useful research had been done on how earthquakes cause damage to structures. In addition, bridges in the United States had suffered only very trivial damage in earthquakes up until the 1964 Alaska earthquake, and bridges in California never experienced serious damage until the 1971 San Fernando quake near Los Angeles.[22] With little engineering knowledge to go on and no experience of the effect of a substantial earthquake on their bridges, Caltrans engineers did not have the ability to define this risk in very precise terms. The apparent lack of great public concern about earthquake safety gave them little reason to try to develop this ability.

Up until the 1960s, the state-of-the art in seismic design was simply to check and see if a structure would be able to stand up to a horizontal force equal to some percentage of its own weight. Caltrans adopted this approach beginning with the design of the San Francisco-Oakland Bay Bridge in 1933. Its earliest formal codes left the exact percentage to the discretion of the designer, by the mid-1940s codes specified two to six percent. The exact percentage was based on the type of footing a bridge was supported on — either a spread concrete footing or piles driven into the ground — and the “bearing capacity” of the underlying soil.[23] Although these measures were not as stringent as the most advanced building codes of the time, they were far more sophisticated than most bridge design codes, which generally had no explicit seismic requirements.[24] Engineers did not have to pay a great deal of attention to seismic resistance, however, since most bridges that were designed to support the weight of traffic would have already met the code’s seismic requirement.[25]

These very basic provisions remained in place unchanged until 1965. By this

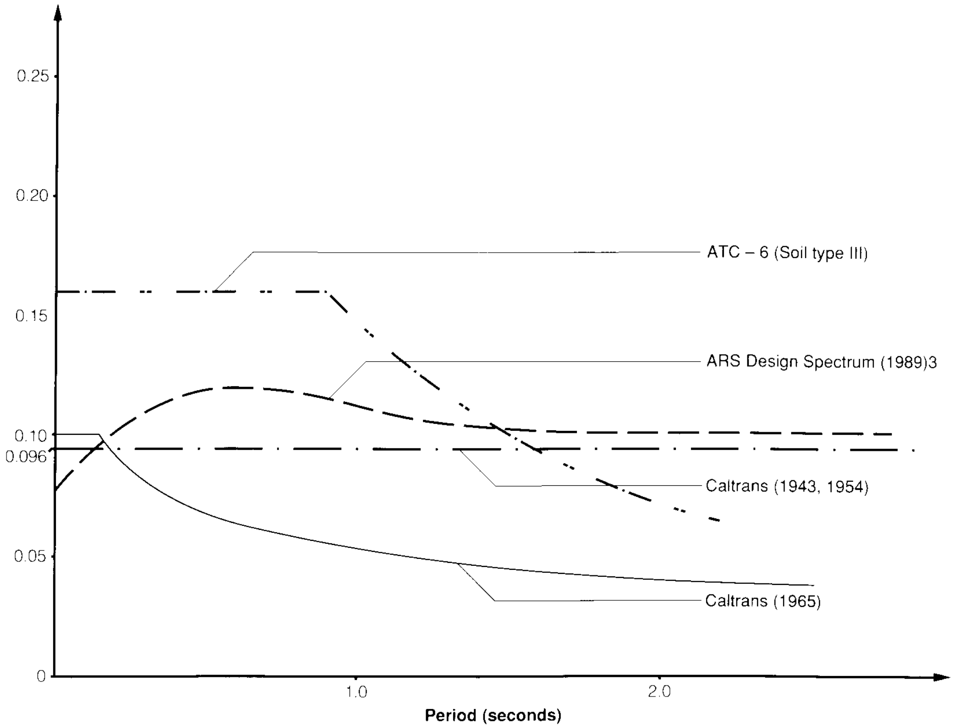


Figure 2.1: Comparison of Caltrans design spectra as they would be applied to bridges with multi-column bents on soft soil, 1943-1989. Note the sharp drop in required strength for most structures between 1954 and 1965. Source: Governor's Board of Inquiry 1990, 125.

point, research into the effects of seismic vibrations on structures suggested that taller, more slender structures — those with natural periods of vibration of one second or more — would be less strongly affected by earthquakes. As a consequence, the formula for calculating the horizontal force a bridge must be able to withstand was modified to take its period of vibration into account.[26] This refinement dramatically lowered the seismic force requirement for most bridges (Figure 2.1). At the same time, the availability of more sophisticated design methods and computerized analysis tools was inspiring Caltrans engineers to push the limits of design practice a little further, in particular to use fewer and more slender columns. When examining bridges for retrofit, Caltrans engineers later found that these structures were much more fragile than those built in the 1940s and 50s.[27] At the time, however, the belief was that Caltrans was using the best knowledge available to design more cost-effective and aesthetically pleasing bridges.[28]

Through the 1960s, Caltrans engineers — along with most other practicing civil

Figure 2.2: Collapse of bridge at I-5/I-210 interchange, 1971 San Fernando earthquake, showing fallen columns (foreground) and segments of bridge deck. Source: Steinbrugge Collection, Earthquake Engineering Research Center, University of California, Berkeley.

engineers — recognized earthquake damage to bridges as a risk object in some abstract sense, but its outlines were vague and its effect on design practice almost nonexistent. This was all to change, dramatically, when a big quake finally did arrive in 1971.

2.3 1971: The San Fernando earthquake

On February 9, 1971, at 6 a.m., a strong earthquake — magnitude 6.6 — shook the San Fernando Valley, then a rapidly-developing suburban area of Los Angeles, causing extensive damage and loss of life. At the time, Caltrans was in the process of building two major freeway interchanges in the area, and large portions of several finished (but not yet in service) structures collapsed in very dramatic fashion (Figures 2.2-2.4).[29] Caltrans engineers were surprised at the extent of the damage, which they realized could have killed people if the structures had been open to traffic. They immediately launched an effort to determine what went wrong and how it could be fixed. As a result of this effort, they were able to begin breaking down the very vague threat to bridges from earthquakes into some quite specific and carefully delineated risk objects.

Figure 2.3: Close-up of column from Figure 2.2, showing that failure was caused by column reinforcement pulling out of the footing. Source: Godden Collection, Earthquake Engineering Research Center, University of California, Berkeley.

Initial reactions

Their personal observations of the damage and knowledge of the research literature almost immediately led Caltrans engineers to focus on two key areas. First, they determined that segments of the bridge decks had separated at the expansion joints. Bridge decks are usually made up of a number of different segments with gaps between them to allow for expansion or contraction of the concrete due to temperature changes or aging. At these joints, one segment of the deck is built with a protruding shelf, or "seat," and the other segment with an overhang which rests on this seat. Caltrans engineers determined that earthquake movements had separated some of the joints, causing some sections to slip off their seats, contributing to the collapse of the columns supporting them. In the worst case, some Caltrans structures contained "drop in spans" held up only at the joints, which would simply fall to the ground if they came unseated. The second problem engineers observed was that the concrete columns themselves disintegrated under stress too easily, in a way that suggested they were designed with insufficient steel reinforcement (Figure 2.4).[30]

The generalized risk of earthquake damage to structures had now been broken down into two distinct risk objects: expansion joints and poorly-designed columns.

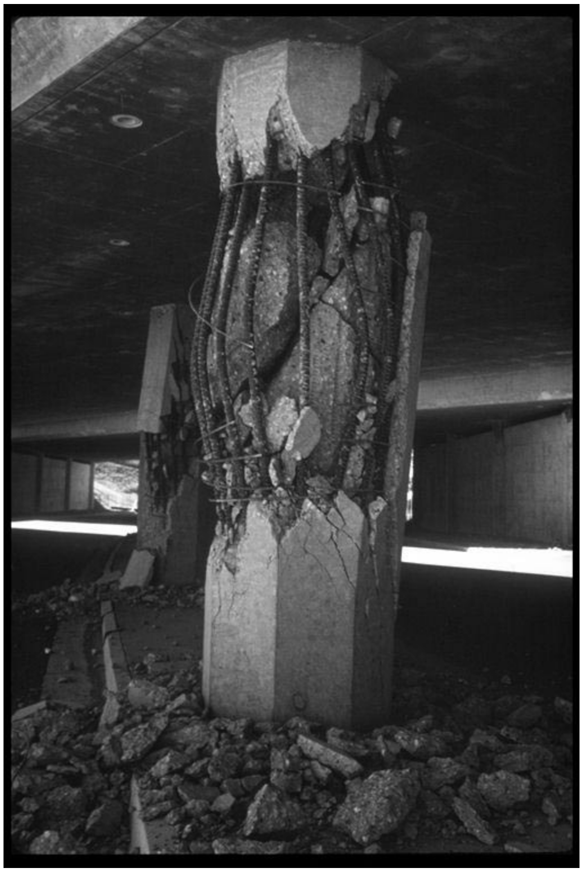


Figure 2.4: Column damage to another structure at the I-5/I-210 interchange, 1971 San Fernando earthquake, caused by rupture of horizontal reinforcement hoops. Source: Steinbrugge Collection, Earthquake Engineering Research Center, University of California, Berkeley.

Throughout the 1970s and 80s, Caltrans engineers tried to come up with ways of displacing the risk posed by these elements from their structural systems, taking several different approaches. First, they worked to develop new ways of calculating the seismic force a structure should be able to withstand. Second, they introduced specific changes in design details, particularly in the area of steel reinforcing bar ("rebar") layout and connection methods. Finally, they created techniques for retrofitting existing structures. These efforts proceeded concurrently, with each new development feeding back into the construction of the risk objects, subtly changing them and leading to new methods of displacing risk.

Early code changes

Oris Degenkolb, an engineer in charge of one of Caltrans' design sections, took the main responsibility for developing new seismic design approaches in the period following the San Fernando earthquake. He was assisted in this by Jim Gates, a designer who worked under him at the time and later became head of Caltrans' Office of Earthquake Engineering. Gates recalls taking on the primary responsibility for documenting and implementing the changes.[31] Knowing that there were serious problems, but faced with the pressure to keep building new bridges, the two engineers, "for lack of anything better,"[32] issued an interim instruction to designers a month after the earthquake, directing them to multiply the design seismic forces in the existing code by 2 for structures on normal concrete footings, and 2.5 for those on piles.[33] This was a noteworthy development, because the 1964 code changes had dropped any reference to footing type. The new force multiplication factors were an acknowledgement that the interaction between a structure and the ground it rested on might be more significant than the existing code recognized. However, this measure was not seen as a satisfactory way of addressing the more specific problems with joints and columns.

Columns

Based on the effects of the earthquake, Caltrans engineers concluded they were not properly reinforcing bridge columns. Specifically, they identified the problem as a lack of sufficient ductility. Ductility is the ability of a material to deform beyond the level where it springs back elastically without losing strength. Steel is a very ductile material, as can easily be seen by bending and straightening a paper clip; concrete is not. In the 1960s, engineers had just begun to understand that reinforced concrete structures needed to have a reserve of ductility if they were to withstand earthquake forces.[34] To make them more ductile, concrete columns are usually designed with vertical and horizontal steel reinforcement bars, which form a cage running the length of the column a few inches inside its outer circumference. The vertical reinforcement consists of long bars, while the horizontal reinforcement is often arranged in hoops. Research published by the Portland Cement Association in 1961 suggested that the key to ductile design was to employ more transverse reinforcing hoops.[35] This is not just because of the ductility of the steel, but because concrete itself is able to withstand more stress without disintegrating when it is tightly confined.[36]

Before the 1971 earthquake, standard Caltrans practice for transverse reinforcement was to use hoops of half-inch diameter rebar spaced every 12 to 18 inches.[37] New regulations, issued within a few months of the earthquake, required that designers employ a continuous spiral of reinforcing steel for the entire height of the column, rather than individual hoops. This provided greater continuity of reinforcement, since hoops have unconnected ends that are simply hooked into the vertical reinforcement and held in place by the surrounding concrete. These hooks may come undone under stress, a problem which is avoided by using a spiral. Spirals were to be fabricated from three-quarter-inch diameter rebar with 3 1/2 inches between turns of the spiral.[38] This corresponds to a roughly five- to eight-fold increase in the total amount of horizontal reinforcement in columns.

Caltrans engineers also took steps to improve the continuity of vertical reinforcement in columns. Typically, individual reinforcing bars do not run the entire height of a column. Rather, a given element is made up of several pieces which overlap at their ends and are held together by the surrounding concrete. These overlaps are known as "lap splices," and they can pull apart under sufficient stress. The new rules required designers to avoid lap splices wherever possible, and, if they were necessary, to avoid placing them near the base or the top of the column, areas which experience the most stress in an earthquake. The rules also specified that reinforcement should be continuous from the column into the footing, to avoid problems with rebar pulling out of the foundations, which may cause columns to topple over, as shown in Figure 2.3.[39]

These changes were strictly a matter of what engineers call "design details." In other words, they did not involve any fundamental alteration in design methodology, but simply stated that a certain amount of rebar should be arranged in a certain standardized way. It was understood that these measures would significantly improve ductility, but this was an "empirical" determination not based on any specific method for quantifying ductility.[40] However, this certainly does not mean that the changes were trivial or without sound basis. There is a general sense among engineers that good detailing practices are at least as important to earthquake-resistant design as methods for calculating specific structural demands and capacities.

The new reinforcement specifications can be read as an elaboration of the risk posed by insufficiently reinforced columns. This risk object was now more precisely defined in terms of the lack of ductility of existing columns, and this was further attributed to a lack of continuous, closely spaced transverse reinforcement to provide confinement, columns with lap splices in high-stress areas, and columns that weren't well attached to their footings. Such columns were judged to be risky, but the solution was simply to change the way columns would be designed in the future. Columns that had already been built according to the old methods were not yet considered enough of a threat to warrant fixing. This view of the risk posed by columns and what should be done about it was a direct consequence of the approach that was already being developed for dealing with the separation of expansion joints.

Expansion joints

Also within a few months of the earthquake, Caltrans designers came up with ways of preventing the separation of expansion joints. The main approach was to use "hinge restrainers" consisting of a number of steel cables or rods that stretched across the expansion joint, tying adjacent segments of the bridge to each other (Figure 2.5).

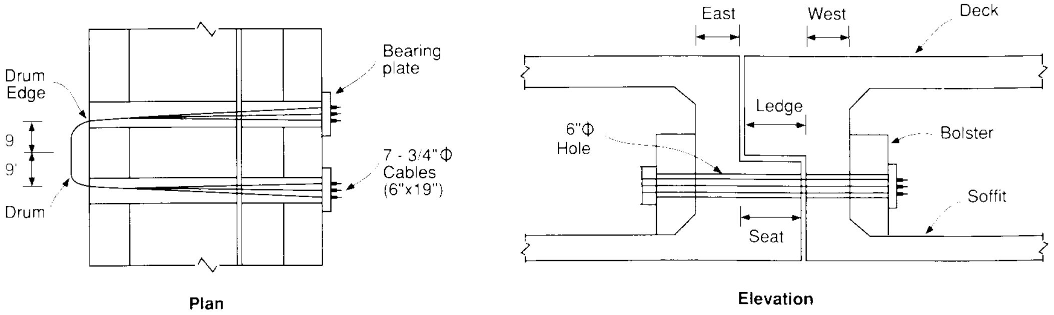


Figure 2.5: A standard version of the Caltrans expansion joint restrainer for concrete bridges. The restrainer is meant to keep the ledge from sliding off the seat. Source: Governor's Board of Inquiry 1990, 131.

Unlike the new column reinforcement criteria, which could only be implemented in new structures, this approach could be applied to both new and existing structures, and was developed with this dual use in mind from the beginning.[41] Choices about the number, size, and arrangement of the cables or bars were made, at least initially, through simple hand calculations. Laboratory testing was done to determine the strengths of the cables and bars under cyclic loading, but Caltrans did not have the facilities to test complete restrainer systems.[42]

The development of hinge restrainer technology raised, for the first time, the issue of seismic retrofitting: going back and correcting design deficiencies in structures already standing. In 1973, Caltrans began a program to install hinge restrainers on 1,261 bridges throughout the state.[43] This program proceeded very slowly, and was only completed in 1987.[44] But why choose to retrofit? And why limit retrofitting, at least initially, to the installation of hinge restrainers? The answer to the first question is fairly straightforward: Caltrans engineers seemed to feel a basic professional obligation to do something about structures that were known to be flawed. Indeed, there is no evidence that the desirability of retrofitting was ever debated. The second question is a little more complex.

In 1978, Oris Degenkolb wrote an article for the internal Caltrans newsletter *Bridge Notes* which described the thinking behind the decision to focus on hinge restrainers. The most basic reason was just that hinge restrainers were a relatively simple technology that could easily be applied to existing structures. Their performance characteristics were thought to be well-understood; Degenkolb's article described the methods for designing hinge restrainers and their expected performance characteristics in great detail, with little evidence of uncertainty. By contrast, he mentioned techniques for retrofitting columns by providing additional exterior confinement, but only as unproven possibilities and with the proviso that further testing would be necessary to determine their effectiveness. At this time, Caltrans just did not have the means to calculate the probable ductility of a given column, with or without retrofitting. The performance of hinge restrainers, by contrast, was not supposed to depend on ductility in any significant way.[45]

Another reason Degenkolb gave for focusing on expansion joints was that retrofitting these would also make the columns less vulnerable:

> When hinges are not restrained, segments of a bridge can act independently and forces in the columns can be significantly greater than if hinge movements are limited. Thus, retrofitting hinges with restrainers can significantly reduce the probability of column failures.[46]

Still, Degenkolb and other engineers at Caltrans did not believe that hinge retrofits would completely solve the problems with columns. Their decision to focus on hinge retrofits to the exclusion of column retrofits was also influenced by an emerging philosophy of seismic design:

> It is not practical to design bridges that will economically serve our normal transportation needs but not be damaged to some extent if subjected to severe seismic shaking. The aim is to make structures seismically resistant to the extent that they may sustain damage but not collapse completely. It is also desirable that they be capable of carrying at least a minimum amount of emergency traffic even though they may be damaged.[47]

Retrofitting hinges, engineers felt, would at least provide a certain measure of protection against collapse. This was backed up by a common belief that bridges probably would not have been damaged to the point of collapse in the San Fernando earthquake if their joints had been kept together.[48] Installing joint restrainers seemed consistent with the overall goal of preventing collapse but not necessarily all damage to a bridge.[49]

Cost-effectiveness

In what would later become a continuing theme in the Caltrans retrofit program, concerns about cost-effectiveness and the efficient use of resources also played a key role here. Near the end of his article, Degenkolb again describes the reasoning that led to the focus on hinge restrainers, but in language which emphasizes the economic side of the decisionmaking process:

> Since the restraining of the superstructure at hinges and bearings was judged to be a more serious problem, and providing that restraint alleviated the seriousness of the column deficiency, more can be obtained for the money by retrofitting the hinges and bearings first.[50]

Indeed, Caltrans spent only $54 million on the hinge retrofit program from 1973 to its completion in 1987, a very small proportion of its total budget during that period.[51] In general, decisions about retrofit were made under the assumption of limited resources. Caltrans engineers made little effort to get more funding for retrofitting. Instead, they tried to work efficiently within the existing budgetary framework. Times were particularly lean during the mid-1970s because the Caltrans budget — which is funded largely through gasoline taxes — shrunk dramatically as a result of the oil crisis and the consequent reduction in gasoline sales. Caltrans was forced to lay off many employees, including a number of engineers in the seismic unit.[52] This played a role in the slow pace of the expansion joint retrofit program.

Columns and joints as risk objects

In the course of developing new seismic design criteria and retrofit technologies, and deciding how to deploy them, Caltrans engineers sought to displace risk from structural systems in more than one way. Most obviously, in the case of hinge restrainers, specific retrofit methods were developed that were thought to render hazardous expansion joints safe. Columns were another story. Expansion joint retrofit was supposed to make columns less vulnerable, but only under certain limiting assumptions: that damage was acceptable as long as there was no collapse, that hinge restrainers would likely prevent complete collapse, and that cost-effectiveness was of paramount importance. Although the risk from poorly-confined, nonductile columns was eliminated in new structures through improved design methods, the risk posed by existing nonductile columns was never firmly emplaced or displaced. Some suggested that columns did not pose a significant risk so long as expansion joints were tied together. But Caltrans engineers continued to discuss column retrofit technologies even after the hinge retrofit program was well underway, an indication that there was still some concern. There was no clear consensus about whether columns designed before 1971 posed a significant threat or whether anything could be done about it. The elaboration of this risk seems to have been put off as engineers focused instead on expansion joints, a risk object that could be understood and controlled relatively easily based on existing technical knowledge.

2.4 Elaboration of the risk network into the 1980s

Location, geology, and seismic forces

When Caltrans modified its design codes by increasing seismic force requirements and taking footing types into account, it was an acknowledgment that previous methods for calculating design forces were flawed. The increases in required strength were somewhat arbitrary, however, a fact which did not sit well with some engineers. As Jim Gates recalls, "we wanted something more rational than what we were currently doing."[53] So he and other engineers sought to develop a new method for calculating the force a bridge at a given location would be likely to experience in an earthquake that explicitly took into account the seismic potential of nearby faults and the soil conditions at the site.[54]

The first step Caltrans took was to commission a map of known faults in the state from the California Division of Mines and Geology (DMG). Based on a determination of the "maximum credible earthquake" that could be expected from each fault, the map indicated, via contour lines, the maximum earthquake acceleration that could be expected at bedrock for every point in the state.[55] The map was digitized so that Caltrans engineers could easily and precisely calculate the acceleration at a given site.[56] Getting the DMG to produce such a map was an undertaking in itself, because many geologists consider the "maximum credible earthquake" approach to be outmoded. Instead, they prefer more refined “probabilistic” methods of evaluating earthquake risk, which describe the likelihood that an earthquake of a given magnitude will occur in a given time period on a particular fault.[57] At Caltrans, however, engineers “wanted something that was very conservative, that could never be challenged.”[58] The geologist who ended up working on the report, according to Jim Gates, “understood what we wanted and he was more than willing to do it, and he did it. And as a result of that, he got a lot of flack.”[59] The DMG was clearly not interested in being widely associated with this sort of map; the final version they published, in 1992, was still labeled “Prepared for Internal Use by Caltrans.”[60] Faced with continued resistance from the DMG, Caltrans eventually hired the author of the 1992 map as staff seismologist.

Obtaining a useful seismic map was just the first step in developing a comprehensive new approach for calculating earthquake forces based on *response spectrum analysis*. This approach gives designers a straightforward method for determining the acceleration that a given structure would experience during an earthquake, based on bedrock acceleration, soil conditions at the site, and the natural vibrational period of the structure. The complex calculations that would ordinarily go into this analysis are replaced by a series of graphs called Acceleration Response Spectra, or ARS curves. The designer chooses a particular curve corresponding to the soil depth and bedrock acceleration at a particular site, finds the period of the structure on the x axis of the graph, and then reads off the seismic acceleration on the bridge from the y axis (Figure 2.6). This acceleration is then multiplied by the mass of the structure to come up with a seismic force for design. The resulting force is divided by a factor, Z, which varies according to structural type, taking the positive effects of ductility into account (Figure 2.7).

These new methods for calculating design forces significantly redefined the risk network associated with earthquake damage to bridges. Although new structural characteristics were being taken into account, the more significant development was that attention was now focused, to a much greater extent than ever before, on the hazards associated with location. The earthquake threat to bridges was now located as part of a system containing geographical and geological as well as structural elements. While hinges and columns were still significant risk objects, the danger they posed could now

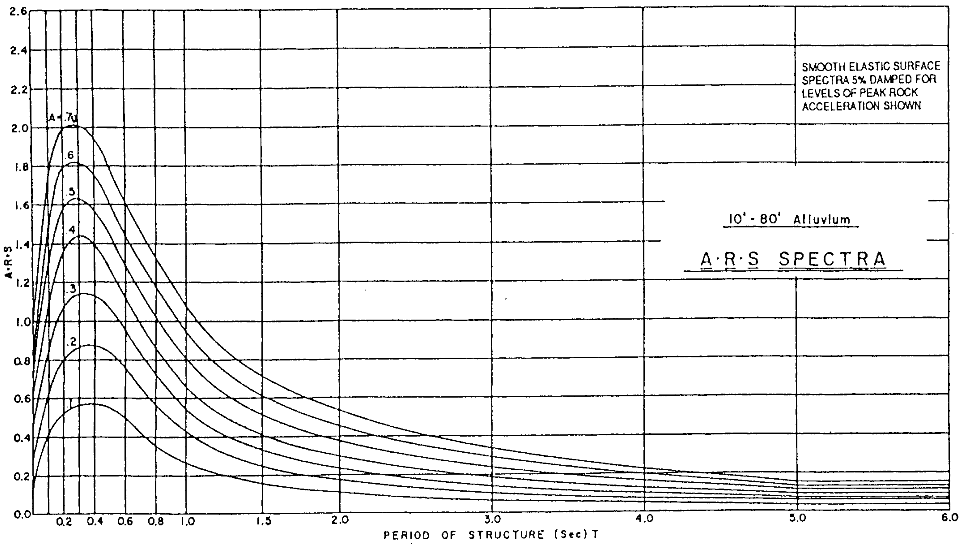


Figure 2.6: Example of Caltrans Acceleration Response Spectra for 10 to 80 foot soil depth. Each curve corresponds to a different bedrock acceleration between 0.1 and 0.7g, and enables the designer to calculate the acceleration a structure with a given natural period must be able to withstand. Source: Caltrans Bridge Design Specifications, May 1988, Figure 3.21.4.3B, p. 30A. Used by permission.

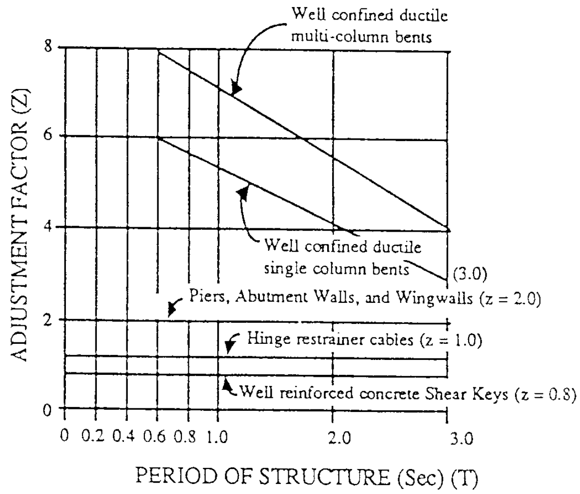


Figure 2.7: Caltrans Z factors for various types of structures. Source: Caltrans Bridge Design Specifications, May 1988, Figure 3.21.1.2, p. 28. Used by permission.

be further refined based on the seismic characteristics of the place where they stood. This expanded the scope of the risk network to include ancient features of the earth's crust as well as elements of modern freeway structures.

Listening to earthquakes

Perhaps the most dramatic and influential change to the risk network at Caltrans during this time period was the inclusion of earthquakes themselves. Before 1971, seismic design at Caltrans was based largely on simplified formulas for calculating the earthquake force a structure should be able to withstand. Even as these formulas became more sophisticated, changes were driven mainly by developments in structural theory. Particular seismic events were generally not mentioned as playing a role in earthquake-resistant design. The San Fernando earthquake was the first to be taken up by engineers as an integral part of their thinking about risk. It is described as having provided Caltrans engineers with information about specific flaws in their design practices — namely, unrestrained expansion joints and poorly reinforced columns — that they had never understood before.

This earthquake presented itself as such an independent agent of change in part because Caltrans engineers had no place for earthquake damage in the risk networks they had developed. Its effects were not anticipated, and there were no procedures or customs in place which told engineers how to react to it. This changed throughout the 1970s and 80s. In 1971, several design engineers had traveled to the site of the freeway damage to observe and photograph it and try to determine causes for the structural failures. Building on this experience, the Division of Structures put together a formal Post-Earthquake Investigation Team (PEQIT).[61] Caltrans was not alone in this; during this period earthquake investigation began to flourish as a tool in academic research, partly through the efforts of professional organizations like the Earthquake Engineering Research Institute (EERI).[62]

By the late 1980s, procedures for Caltrans investigations had become quite well-established, and a PEQIT manual was distributed.[63] This manual explained that the PEQIT team was to consist of a group of volunteers prepared to leave home at a moment's notice to travel to an area where an earthquake had occurred. A senior seismic specialist was designated as the PEQIT coordinator. The state Department of Water Resources and a Caltech/USGS group, both of which maintain networks of seismic devices, were to notify a Caltrans operator in the event of a significant earthquake. The operator would then call the PEQIT coordinator within half an hour if a quake of magnitude 5.5 or greater had occurred, or the next day if the magnitude was 4 or greater.[64] The coordinator then contacted the rest of the team, if they hadn't already heard through the media and come into the office on their own, and made travel arrangements for the group.[65]

Each document describing PEQIT procedures since the late 1980s contains more specific and detailed advice on how to conduct an investigation. In a 1991 article, engineers Ray Zelinksi and Earl Seaberg describe the learning process which has facilitated this change:

> While the mission of the team remains the same as when the team was born, the ability to function efficiently has improved considerably. The efficiency has been improved through an evaluation process which is conducted following each event. All team members attend the evaluation session and analyze the latest excursion in terms of spontaneity, travel means, factual notes, equipment aides, communications, etc. Through this process, response to seismic events has been streamlined, and a collection of investigating equipment has been accumulated.[66]

PEQIT documentation also reveals an accumulation of knowledge about how to collect information on earthquake damage. Zelinksi and Seaberg, for example, advise team members that

> determining movement can be made in many ways. Using features at joints in the superstructure such as sleeved pipe connections of barrier rails or scribes placed across joints on concrete barriers, evidence of disturbed material within the joint itself, and offsets between joined members are some ways of determining movement. Gaps between soil and structure can be observed at the base of columns and at abutments. Fill settlement can be determined by looking for previous ground lines on faces of abutments, wingwalls, or columns. Cracking of soil or pavement within the vicinity of footings can indicate ground settlement or foundation movement.[67]

The 1998 PEQIT manual, in addition to written advice, provides team members with photographs of damage from the Loma Prieta, Northridge, and Kobe, Japan earthquakes, along with photographs of laboratory tests, describing what kind of damage each photograph indicates.

By establishing the PEQIT, Caltrans has been able to incorporate experiences with earthquakes into changes in design procedures in an increasingly direct and routine way. The 1971 earthquake had an impact on design, but mainly because it was so unexpected. The significance of the new approach is that it establishes a permanent organizational mechanism for expanding the risk network to encompass damage from future earthquakes. This has led to earthquakes being considered less as shocking and unpredictable freaks of nature which literally "shake up" design practice, and more as anticipated events that engineers can learn from in a systematic way.

From the 1970s to the 1980s

By the end of the 1970s, the culture of risk at Caltrans was becoming increasingly complex. The generalized risk of earthquake damage to bridges was coming to be embodied in two very specific risk objects: unrestrained expansion joints and columns with insufficient confining reinforcement and poor detailing. At the same time, the risk network was being expanded to include geological factors and the damage observed in particular earthquakes. But although both the risk objects and the networks they were connected to were being elaborated, the composition of the risk community itself did not change significantly. The only exception was the inclusion of the Division of Mines and Geology in the process of defining the risks posed by faults. But most decisions still were made within a relatively small circle of engineers at Caltrans, and these people had most of the power to define, displace, redefine, and complicate the risk objects associated with the seismic threat to the freeway system. While political and economic conditions played a role, they did so rather indirectly; nobody from outside Caltrans tried to change the way risk was defined within the agency, and nobody from within tried to take their concerns into a larger arena. Seismic concerns largely existed within the established social arena where Caltrans engineers were used to working.

In the late 1980s, the risk culture at Caltrans began to shift in a slightly different direction as column retrofit was put back on the agenda and became a focus of organizational activity. This coincided with two other developments. First, because of the scope of the column retrofit program, engineers began to focus much more on developing procedures for ranking bridges for retrofit in terms of their relative vulnerability to earthquake damage. This work brought formal risk assessment methods into the risk network for the first time. Second, the scope of the risk community expanded as Caltrans began to fund laboratory research on retrofit techniques. All of these developments actually began *before* the Loma Prieta earthquake hit in 1989, a fact which is often neglected in retrospective accounts. The earthquake and subsequent public attention aimed at Caltrans accelerated these existing trends, however. The column retrofit program was greatly expanded, and began to move at a much faster pace. Methods for prioritizing bridges for retrofit took on added importance and complexity. Finally, the risk community grew much larger as Caltrans increased its funding of research and did more and more design work under the supervision of peer review groups which included practicing engineers from outside Caltrans as well as university professors. This expansion of the risk community resulted in important changes in the scope and structure of the risk network, as well as in the definitions of particular risk objects.

2.5 Column retrofit and beyond

A second look at columns

In 1985, Jim Roberts was appointed head of bridge design within the Division of Structures. He was returning to the division after a 13-year absence spent working elsewhere in the organization.[68] In this role and in his later role as head of the Caltrans Engineering Service center, which subsumed the Division of Structures in the mid-1990s, Roberts was the senior engineer in charge of structural design at Caltrans through the end of the time period covered here. Roberts is a stout figure with a sharp military haircut and a gravelly voice that carries authority well, even though he is surprisingly soft-spoken most of the time. He would be the primary spokesperson for and defender of Caltrans engineers in numerous press conferences and hearings following the Loma Prieta earthquake in 1989.

Upon his appointment, Roberts expressed concern that people were getting a bit complacent, “resting on their laurels” instead trying to live up to the division’s longtime reputation for cutting edge engineering work.[69] At this time, the expansion joint retrofit program was nearing completion, but column retrofit was still considered a relatively low priority. Roberts may have seen this as part of the complacency he referred to. In any case, early in 1986 he put together a small working group consisting of seismic experts Ray Zelinski and Jim Gates and an engineer from bridge maintenance, and asked them to report back on the feasibility of a column retrofit program.[70]

Jim Gates and Ray Zelinski were the two most prominent seismic experts at Caltrans during 1980s and 1990s. Both are long-time employees of Caltrans, more than making up in experience what they lack in graduate degrees. Gates played a central role in the various incarnations of the Caltrans seismic engineering group since the 1970s, and was head of the Office of Earthquake Engineering until his retirement in 1997. Ray Zelinski was also prominent in many of the seismic engineering groups, and during the 1990s became head of the Seismic Technology group, which was in charge of developing retrofit methods. Gates has a more quiet, diplomatic character, while Zelinski tends to be more enthusiastic and blunt, but both are extremely knowledgeable about the practice of design and have a healthy skepticism about excessive academic abstraction.

The group headed by Gates and Zelinski came up with some basic retrofit strategies and a preliminary estimate of the number of bridges that would need retrofit and the total cost. The $4 million per year that was still available for retrofit projects was mostly devoted to finishing the expansion joint retrofits, however, and the remainder would not be enough to make much progress on this new program. Still, Roberts recalls feeling at the time that “we’ve got to hack away at it regardless, what ever funding we can get, we’ve got to get started.”[71]

By early 1987, still without much funding, Gates and Zelinski began preliminary design work on some column retrofits.[72] The break they needed was provided later that year by the Whittier earthquake which struck near Los Angeles. Although this quake was relatively mild, with a magnitude of 5.9, it struck during commuting hours and was thought to have come very close to taking down a bridge carrying Interstate 605 over Interstate 5. An analysis by U.C. San Diego professor Nigel Priestley indicated that only the joint restrainers prevented it from completely collapsing. This was widely taken as tangible evidence that joint restrainers alone might not provide sufficient protection from collapse, even in a relatively moderate quake.[73] Roberts wrote a letter to upper management using the Whittier quake as evidence that the column retrofit program needed to be accelerated. He convinced them to increase retrofit funding to $16 million per year. However, due to bureaucratic problems, this additional money did not actually reach the Structures Division prior to the Loma Prieta quake in 1989. Still, since the expansion joint retrofit program was nearly finished, they were able to make some progress and had several completed retrofit designs "on the shelf" when that earthquake hit.[74] After that event, funding for retrofitting was suddenly abundant.

The re-emergence of columns as a risk object coincided with the arrival of Jim Roberts as head of the Division of Structures. Although much of the work of defining this risk object was done by others, Roberts' concerns about complacency served as a catalyst for their activities. As this example indicates, the expansion of a risk community by even one member can have a great effect on the definition of risk, particularly if that person has very specific ideas and the authority to see that they are accepted and implemented.

Retrofit technology

In the course of their analysis of retrofit possibilities, Jim Gates and Ray Zelinski looked at a number of different technologies. Some of these, such as wrapping columns in tensioned wire and fitting them with steel shells, had been mentioned by Degenkolb in 1978. At the time, retrofit techniques using high-strength composite materials were also being developed. All of these methods had also been described in Federal Highway Administration publications prepared by the Applied Technology Council, a non-profit engineering research group.[75] However, Zelinski recalls, "we pretty much just jumped on the steel jackets — there wasn't much information out on composite type jackets at that point, and we're very familiar and comfortable with using steel."[76] Steel jackets had the additional virtue that they could easily be applied to rectangular columns, which would be more difficult with wire or composite wraps. The idea was to fit columns with

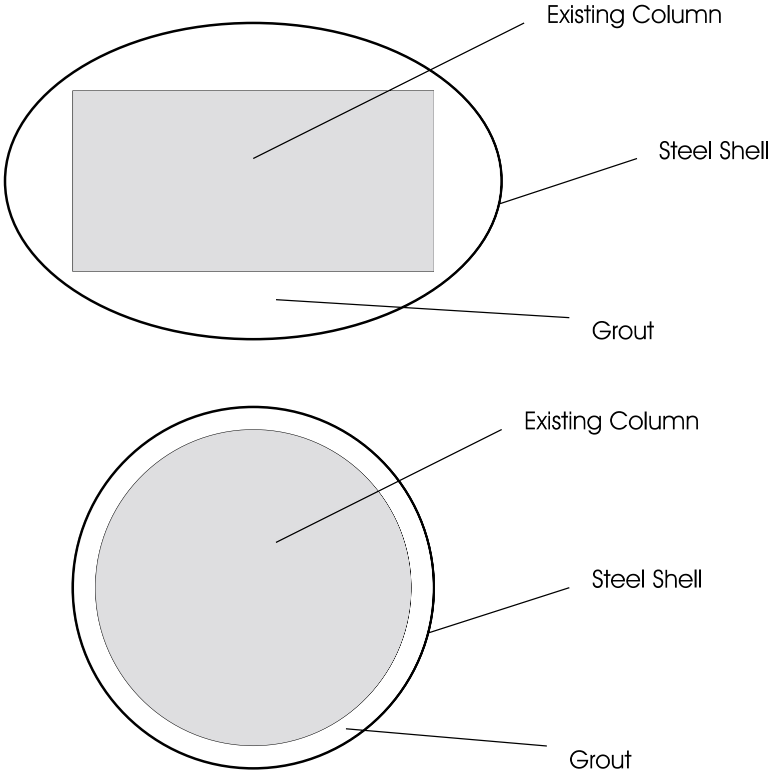


Figure 2.8: Cross-sections through steel shell retrofits as applied to round and rectangular columns (not to scale). Diagram by the author.

round or elliptical jackets that were somewhat larger than the column diameter, or in the case of rectangular columns, just cleared the corners. Then grout could be pumped into the remaining space, bonding the shell to the column and ensuring that they worked together as a single structural unit (Figures 2.8 - 2.10).

The appeal of steel jackets was enhanced by another set of circumstances. In 1986, the Structures Lab at U.C. San Diego had just opened. Eager to build its reputation, the university managed to hire Nigel Priestley, an internationally-known reinforced concrete expert from New Zealand with a strong experimental background. Caltrans engineers were already familiar with Priestley's work from trips they had made to New Zealand.[77] As soon as he arrived in San Diego, Caltrans and the laboratory faculty began informal talks about starting up a testing program for column retrofit methods. They were particularly interested in working with Priestley because of research he had done in New Zealand testing steel-encased concrete bridge piles.[78] In 1987, Caltrans ob-

Figure 2.9: Steel shell prior to installation, intersection of State Route 52 and Genesee Avenue, San Diego. Note irregular shape to fit around hexagonal flared columns, seen in background. Photograph by the author.

Figure 2.10: Steel shell in position around column, ready for welding and grouting. Photograph by the author.

tained funding from the Federal Highway Administration to begin this research, which was then accelerated and expanded as more money became available for retrofit following the Whittier earthquake in October 1987.[79]

Steel jackets emerged as the most viable method for retrofitting columns out of a complex set of circumstances. The familiarity of steel and the relative ease with which steel shells could be applied to rectangular columns were significant factors, but it is not clear that this method would have become so dominant if the work of Gates and Zelinski had not coincided so neatly with the arrival of Priestley in California, or if Priestley had not happened to have been involved in a project on steel-jacketed piles while in New Zealand. This highlights the importance of chance and timing in the emplacement and displacement of risk objects. Again, the "garbage can" model of organizational decisionmaking would appear to be relevant.

Retrofit design

At least initially, steel jackets appear to have been seen by Caltrans engineers as a single, generic technology for column retrofit, much as hinge restrainers had been for expansion joint retrofit. But almost immediately this new phase of the retrofit program took a distinct turn. For one thing, column retrofit seemed to require retrofit of the column footing in most cases. Also, in order to keep retrofit as economical as possible, Gates and Zelinski had early on come up with the idea that it would not be necessary to retrofit every single column on a given bridge — just the minimum number needed to bring it up to current seismic-safety standards.[80] This required designers to take a more systematic view of the structures they were retrofitting, as Zelinksi and another engineer explained in a 1991 article:

> The analysis of the bridges in the current State Highway Retrofit Program consists of a total seismic evaluation. Whereas the original program concentrated on superstructure continuity, this program concentrates on total structural behavior. Seismic forces are tracked to all joints/connections, through columns and abutments, and into surrounding soils.[81]

Indeed, many retrofits were quite complex, and so employed more project-specific retrofit methods in place of or in addition to steel jackets on columns. Many of the more complex projects were done under the supervision of peer review panels, which came up with some innovative approaches. For example, the Santa Monica freeway viaduct in Los Angeles was retrofitted by encasing the bottoms of the columns and adding concrete "link beams" between the columns.[82] The double-deck San Francisco viaducts required massive joint reinforcement that could not be provided by steel jackets, so peer reviewers and designers together came up with a method of stiffening the structure by running "edge beams" along the roadways between the joints.[83] Even simpler retrofits required designers to use dynamic analysis computer programs to calculate total structural response of the retrofitted structure to an earthquake.[84]

The risk network at the end of the 1980s

This new complexity marked a significant change in the way the seismic risk to bridges was conceptualized. After the 1971 San Fernando quake, this risk shifted from being embodied by very general lateral forces to being attached to very specific parts of bridges, namely the expansion joints and columns. The expansion joint retrofit program focused almost exclusively on keeping the joints together, without specifically addressing the impact of this on the rest of the structure. As the column retrofit program evolved, however, it ended up not focusing on columns alone. Instead, attention was paid to many specific parts of the bridges, including expansion joints, footings, abutments, beams and roadways. With the help of more sophisticated computer analysis tools, these many specific elements were considered as part of a total structural system, and the ultimate criteria for a successful retrofit had to do with the performance of this system as a whole. Individual parts were even allowed to fail so long as this would not cause overall structural collapse. The risk object had, in a way, come full circle: early methods of assessing seismic risk were based on general calculations of the ability of a structure as a whole to stand up to certain lateral forces, but without consideration of any specific design details. After 1971, attention focused on details almost to the exclusion of systematic performance. Finally, by the early 1990s, the primary risk object was once again the structure as a whole, but with consideration of the effects of numerous design details on this overall performance.

2.6 Prioritization

Identifying and fixing the problems that might put a particular bridge at high risk from earthquakes was certainly an involved process in itself, but the fact that each bridge was part of a retrofit *program* which involved thousands of other bridges created an additional level of complexity. During the hinge restrainer program, hundreds of bridges had to be retrofitted with very little funding. Funding for the column retrofit program was plentiful after the 1989 Loma Prieta earthquake, but this program involved thousands of bridges and there was a great deal of pressure to move quickly to fix the most vulnerable of them. In both cases, decisions had to be made about which bridges were the highest priority for retrofitting. Since retrofit analysis and design are time-consuming, a full analysis of every bridge at the outset would be difficult, so prioritization decisions have to be made on somewhat limited information. Various formal mechanisms were developed for prioritizing bridges for retrofit, beginning with the expansion joint retrofit program in the 1970s. The complexity and scope of these methods expanded greatly as the column retrofit program got into full swing.

Early approaches

Bridge retrofit prioritization formulas developed by Caltrans and others tend to focus on three general areas: the structural characteristics of the bridges, the seismic characteristics of the sites where they stand, and their social or economic importance.[85] The earliest Caltrans prioritization approach, for the expansion joint retrofit program, took all of these into account, but not in a completely systematic way. Engineers first reviewed bridge structural elements and site seismicity, selecting for the initial pool all "questionable structures" in "high seismic areas." These bridges were then analyzed in detail, producing an unranked group of several hundred bridges needing retrofit. Structures within this group were each assigned a certain number of points according to various characteristics: up to 40 points depending on the likely seismic acceleration at the site, 20 for replacement cost, 8 for detour length, 22 for average daily traffic, 4 for status as a defense and/or emergency route, and 6 for any other facilities which the bridge crossed over.[86] The bridges were then prioritized according to the number of points they had received. This system alone was not seen as sufficient to ensure an accurate ranking, however. Oris Degenkolb noted that "the prioritizing numbers obtained did not always reflect the true relative importance of some structures," cautioning that "the results from any prioritizing system should be subject to adjustment by good judgment."[87]

There were no further developments in retrofit prioritization until Jim Roberts initiated the review of the column retrofit program in the mid-1980s. At this point, an engineer from the SASA unit, Brian Maroney, was given responsibility for putting together a new prioritization mechanism. Since the 1970s, many new developments had occurred in the areas of risk analysis and structural reliability theory. In addition, a Federal Highway Administration-funded report by the Applied Technology Council giving retrofit guidelines for highway bridges had been published in 1983. This report suggested that prioritization methods should explicitly rate bridges according to structural vulnerability, site seismicity, and importance.[88] A 1988 article by Maroney about his new prioritization system reflected these developments. He refers specifically to the risk analysis literature, but mainly to demonstrate how his approach differs from a full-scale risk analysis:

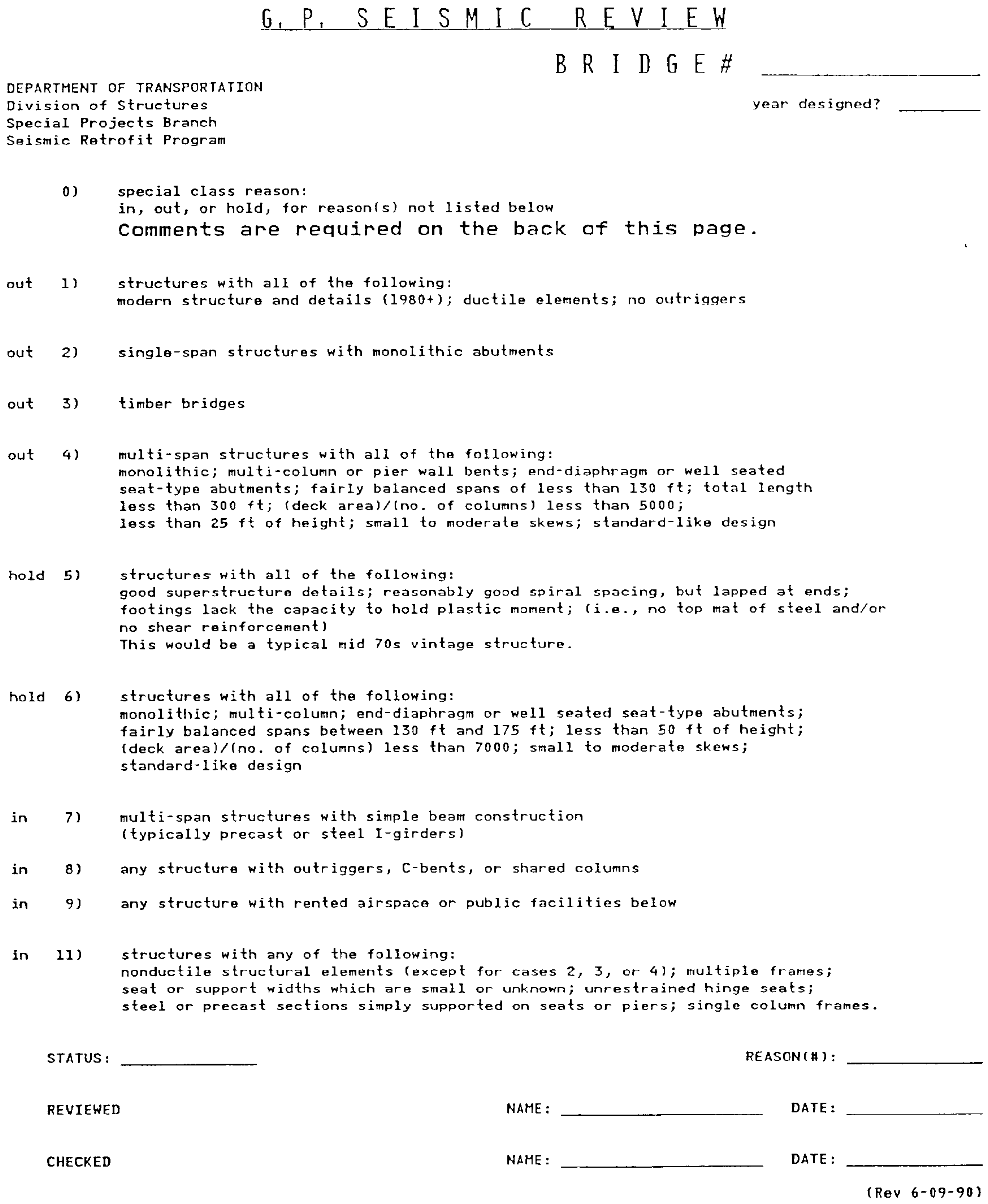
> A conventional risk analysis produces a probability of failure or survival. This probability is derived from a relationship between the load and resistance sides of a design equation. Not only is an approximate value for the absolute risk determined, but relative risks can be obtained by comparing determined risks of a number of structures. Such analyses generally require vast collections of data to define statistical distributions for all or at least the most important elements of some form of analysis, design and/or decision equations. The acquisition of this information can be costly if obtainable at all. . . . To avoid such a large investment in resources and to obtain results which could be applied quickly as part of the Phase II Retrofit Program [i.e., column retrofit], an alternative was recognized. What can be called a level one risk analysis procedure was used. The difference between a conventional and level one risk analysis is that in a level one analysis judgements take the place of data supported statistical distributions.[89]

The new ranking algorithm also included, for the first time, structural factors as well as seismic and importance factors. Instead of assigning different numbers of points for each characteristic, it assigned them each a value between 0 and 1, and then used a weighting system to reflect their relative importance. The weights were assigned to various factors as follows: likely ground acceleration, 18%; adequacy of confinement, 18%; single columns (or not), 18%; length (which affects structural performance), 16%; average daily traffic, 12%; route type (i.e., state highway, interstate highway, street), 8%; length of detour, 5%; and skew (another structural risk factor), 5%.[90] Although the technique was somewhat more sophisticated, this method still added the values associated with each characteristic to reach the final ranking number, much like the old expansion joint prioritization approach.

After the Loma Prieta earthquake, the new retrofit program took off very quickly, with a sense of urgency and, soon, a great deal of funding. The prioritization mechanism began to take on additional complexities. The weights in Maroney's first scheme had been arrived at through informal consultation among a small group of seismic experts at Caltrans, including Jim Gates.[91] After the earthquake, a more formal, documented approach to gathering expert opinion was adopted. A group of twenty-one engineers with seismic expertise was selected, which included Jim Gates, Ray Zelinksi, and Jim Roberts. This group was sent a written survey and asked to assess the importance of possible prioritization criteria by assigning each a number between 0 and 10.[92] The averaged results of this survey formed the basis for a new weighting system.[93]

Implementing prioritization

Although the prioritization algorithm was now well-developed, implementing it was a major undertaking. Before the bridges could be ranked, the data required by the algorithm somehow had to be acquired and entered into a database. Caltrans, like all state transportation agencies, has a maintenance database which includes structural, economic, and traffic information about its bridges. Some, but not all, of this data is required to be kept under federal law.[94] This provided a great deal of the basic information for prioritization. Caltrans engineers also began the laborious process of individually reviewing bridge "general plans," which provide basic structural information, for over ten thousand state-owned bridges. In order to cope with this enormous workload, SASA called on engineers from throughout the Structures Division as well as retirees to review each bridge for certain structural characteristics (Figure 2.11). The purpose of this re-



G. P. SEISMIC REVIEW

BRIDGE# ______________

DEPARTMENT OF TRANSPORTATION
Division of Structures
Special Projects Branch
Seismic Retrofit Program

year designed? ________

0) special class reason:
in, out, or hold, for reason(s) not listed below
Comments are required on the back of this page.

out 1) structures with all of the following:
modern structure and details (1980+); ductile elements; no outriggers

out 2) single-span structures with monolithic abutments

out 3) timber bridges

out 4) multi-span structures with all of the following:
monolithic; multi-column or pier wall bents; end-diaphragm or well seated seat-type abutments; fairly balanced spans of less than 130 ft; total length less than 300 ft; (deck area)/(no. of columns) less than 5000; less than 25 ft of height; small to moderate skews; standard-like design

hold 5) structures with all of the following:
good superstructure details; reasonably good spiral spacing, but lapped at ends; footings lack the capacity to hold plastic moment; (i.e., no top mat of steel and/or no shear reinforcement)
This would be a typical mid 70s vintage structure.

hold 6) structures with all of the following:
monolithic; multi-column; end-diaphragm or well seated seat-type abutments; fairly balanced spans between 130 ft and 175 ft; less than 50 ft of height; (deck area)/(no. of columns) less than 7000; small to moderate skews; standard-like design

in 7) multi-span structures with simple beam construction
(typically precast or steel I-girders)

in 8) any structure with outriggers, C-bents, or shared columns

in 9) any structure with rented airspace or public facilities below

in 11) structures with any of the following:
nonductile structural elements (except for cases 2, 3, or 4); multiple frames; seat or support widths which are small or unknown; unrestrained hinge seats; steel or precast sections simply supported on seats or piers; single column frames.

STATUS: ______________ REASON(#): ______________

REVIEWED NAME: ______________ DATE: ______________

CHECKED NAME: ______________ DATE: ______________

(Rev 6-09-90)

Figure 2.11: General plan review form, used by Caltrans engineers in the first level of screening to determine which bridges required retrofit. Source: Brian Maroney and Jim Gates, "Seismic Risk Identification and Prioritization In the Caltrans Seismic Retrofit Program, Update." SN December 1990, 7-21; Figure at 17. Used by permission.

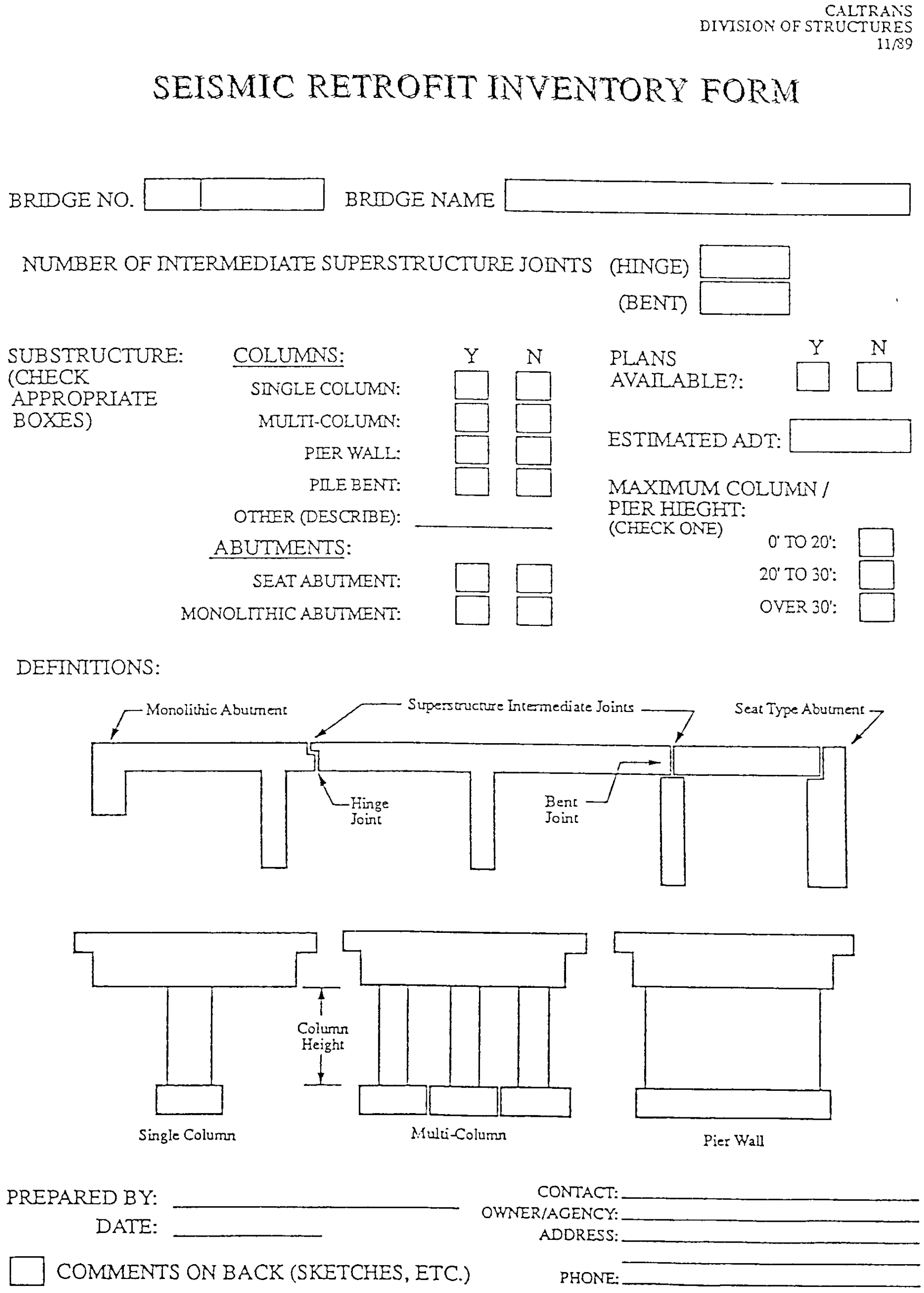
view was simply to exclude those bridges from the list which did not require retrofitting. Caltrans had also been assigned the task of overseeing retrofit on over ten thousand locally-owned bridges — those maintained by cities, towns or counties within California. This created a new set of problems, since general plans were not available for these bridges. Bridge inventory forms were sent to these agencies so they could review their own bridges (Figure 2.12).[95]

Based on the information from the general plan review and the inventory forms from local agencies, a large number of bridges were screened out of the program. Then engineers in SASA faced another daunting task: performing a full plan review on the 7,302 state and 5,138 local bridges remaining. Again, they called on engineers from throughout the Structures Division and on retirees. According to one observer, "projects were delivered fast and furious — stacking up everywhere."[96] Another recalls that people would take stacks of plans home with them at night.[97] It was only once this review was completed that the full prioritization algorithm could be applied to rank the bridges.

A new approach

But the process was not yet over. The rankings produced by the weighting procedure did not have a very wide range, which resulted in many bridges being given the same prioritization numbers. Also, some results were not intuitively plausible. For example, the Fort Sutter Viaduct in Sacramento — which runs directly outside the windows of the Division of Structures offices — was ranked near the top of the list, even though Sacramento is in a very low seismic zone.[98] At this point, another engineer, Ann Gilbert, joined Maroney in supervising the development of the prioritization algorithm. She was recently out of graduate school with specific training in structural reliability theory, and realized that much of the problem with the original weighting system was simply that it added all the values.[99] For example, since seismic potential only accounted for 12% of the final value, a bridge in an area with zero seismic potential could still be high on the list if it had other vulnerable features or carried a lot of traffic.

There is a basic principle in engineering failure analysis that, if several independent events must occur together for a failure to happen, the probability of failure



CALTRANS
DIVISION OF STRUCTURES
11/89

SEISMIC RETROFIT INVENTORY FORM

BRIDGE NO. ☐ BRIDGE NAME ☐

NUMBER OF INTERMEDIATE SUPERSTRUCTURE JOINTS (HINGE) ☐
(BENT) ☐

SUBSTRUCTURE: (CHECK APPROPRIATE BOXES)

COLUMNS:	Y	N
SINGLE COLUMN:	☐	☐
MULTI-COLUMN:	☐	☐
PIER WALL:	☐	☐
PILE BENT:	☐	☐
OTHER (DESCRIBE): ____		
ABUTMENTS:		
SEAT ABUTMENT:	☐	☐
MONOLITHIC ABUTMENT:	☐	☐

PLANS AVAILABLE?: Y ☐ N ☐

ESTIMATED ADT: ☐

MAXIMUM COLUMN / PIER HIEGHT:
(CHECK ONE)
0' TO 20': ☐
20' TO 30': ☐
OVER 30': ☐

DEFINITIONS:

PREPARED BY: ____
DATE: ____
CONTACT: ____
OWNER/AGENCY: ____
ADDRESS: ____
PHONE: ____

☐ COMMENTS ON BACK (SKETCHES, ETC.)

Figure 2.12: Bridge inventory form sent to local agencies during the initial retrofit screening process. Source: Brian Maroney and Jim Gates, "Seismic Risk Identification and Prioritization In the Caltrans Seismic Retrofit Program, Update." SN December 1990, 7-21; Figure at 19. Used by permission.

can be obtained by multiplying the probabilities of each individual event. That way, if the probability of one of the events is zero, the engineer will correctly conclude that the probability of failure is also zero.[100] If the events do not need to occur together to result in failure, it is appropriate to add their probabilities to reach the final value. Many prioritization systems follow simplified procedures because they are not meant to precisely calculate probabilities of failure, but to roughly group structures according to the relative risk they face. Since such an approach no longer seemed adequate to the task at hand, Caltrans engineers put together a more sophisticated algorithm that added or multiplied probabilities as appropriate. The ranking criteria were grouped into three classes: Vulnerability (V) for structural characteristics, Hazard (H) for seismic potential, and Impact (I) for social/economic significance. The criteria within each of these categories would be added together, since they all could independently contribute to failure, but the coefficients for each of the categories would be multiplied together to reach a final ranking. This would ensure that a structure with a low score in any single category would have a low rank on the list, meaning that a bridge in a low seismic zone would be low in the ranking regardless of its structural characteristics, and that a little-used bridge would be lower on the list regardless of its structural or seismic risk.

Caltrans engineers still wanted to maintain some kind of relative weighting of the three categories, however, since they were not considered exactly equal in importance. The categories were weighted as follows: Vulnerability, 27%; Hazard, 33%; and Impact, 40%. The value in each category would be multiplied by a weight before all three were multiplied together to reach a final value. The weights for each category, as well as the weights for each individual factor which made up the categories, were determined through yet another survey of Caltrans seismic experts.

By this point, Caltrans had established an external Seismic Advisory Board as a result of political events surrounding the Loma Prieta earthquake. Around the same time, Caltrans engineers noticed they had made a basic mathematical blunder in their weighting scheme. Because of the associative principle, the weights assigned to each category would have no effect on the overall ranking when the values were multiplied. The formula

$$\text{Risk} = (.27 \times V) \times (.33 \times H) \times (.40 \times I)$$

is mathematically equivalent to

$$\text{Risk} = (.27 \times .33 \times .40) \times (V \times H \times I)$$

Whatever the weights were, then, they would simply change all of the final values by the same proportion, leaving the relative ranking exactly the same.[101] Some compromise was required that would give the weights some meaning without doing away with the advantages of multiplying the three main factors. The advisory board intervened here, proposing that the algorithm be changed to[102]

$$\text{Risk} = [V \times H \times I] \times [(.27 \times V) + (.33 \times H) + (.40 \times I)]$$

Discussions continued with board members, who wanted to include a probabilistic measure of fault activity in the calculation in addition to the "Maximum Credible Earthquake" acceleration that had been used as the main indicator of seismic risk. After further debate, a factor A was added which represented earthquake probability, and the algorithm took on its final form:[103]

$$\text{Risk} = [A \times H] \times [(.60 \times I) + (.40 \times V)]$$

Although the retrofit program was already underway, the bridges were reprioritized according to this new formula. To some at Caltrans, this seemed like an example of the academic desire for exactness getting in the way of basic organizational common sense. According to Jim Gates, the new system

> didn't change things too much. It was a kind of more of a minor refinement I think, in just the way we put the numbers together. But that was a real agony, in fact if I had to do over again I wouldn't reprioritize, because it was just, the way things work in our system, once you get things going down the pipeline, the inertia's so strong it just screws everything up. But we did it. And that was not easy to change. But that's one of the things that a lot of people don't seem to understand . . . the only reason that we prioritize it is to decide which projects to do first. . . . There's a commitment on the part of everybody that we will go through the whole list and look at each bridge. So the only thing the prioritization does is get the ones on the top of the list into the system quickly. Once it gets into the system, then it's worked on [and] . . . it doesn't come out the other end until it's done.[104]

2.7 Expanding the risk community

Prior to 1984, the definition and mitigation of seismic risks at Caltrans was generally an in-house affair. Although outside information from observations of earthquake damage and from knowledge of the research literature did play a role, it did so only through the interpretative efforts of Caltrans engineers. Laboratory testing was restricted to the relatively limited capabilities of the Caltrans laboratory. Data about cable strength for restrainer design were obtained from this facility, for example, but full scale tests of the restrainer units were never done. During the 1970s, Caltrans did fund some research — apparently analytical, rather than experimental — at U.C. Berkeley, some of which played a role in the hinge retrofit program. Other research during this time period was either done in-house or was mostly analytical. Caltrans also participated in a study of bridge columns by the National Bureau of Standards in 1983, which was supposed to test column retrofit measures, but apparently was not completed.[105] It does not appear that any of this research had a very large impact on Caltrans definitions of seismic risk, since it is rarely mentioned in documents from the period.

Researchers

The first substantial research that Caltrans funded on a specific seismic issue was initiated in 1984 with UCLA professor Lawrence Selna. He performed a series of tests on full-scale models of joint restrainer units. This research indicated that the joint restrainers tended to fail by pulling through the concrete in which they were anchored, rather than by stretching in a ductile way, and suggested other problems with the design of the restrainers. Unlike previous research, this led to immediate changes in design practice and in the section of Caltrans code dealing with restrainers.[106] By the time this research was completed in 1987,[107] Caltrans had already initiated contacts with U.C. San Diego to test steel-jacket retrofitting of bridge columns, as described previously. After the Whittier earthquake in 1987, Caltrans expanded this research and initiated other research projects at San Diego. This was ongoing when the Loma Prieta earthquake hit in 1989.

Following that earthquake, the California state legislature dramatically increased the Caltrans budget for retrofit. A significant portion of this funding, roughly \$10 million, was spent on research in the two years following the earthquake.[108] Since then, spending on research has been maintained at \$5 million per year, although not without some difficulty as the memory of the earthquake fades.[109] Caltrans solicited research proposals and by December of 1989 had over 50 to choose from.[110] This list was quickly pared down to only the crucial projects, and by the end of 1991, 16 contracts had been executed and 8 were in negotiation. The projects that were selected reveal a distinct geographical bias toward California and the west. The vast majority of these were with researchers at University of California campuses in Berkeley, San Diego, Davis, and Irvine. Some projects went to the private University of Southern California and to private firms and research institutes based in the state. Proposals from Northwestern University, the University of Michigan and the University of Texas were rejected — although research was later funded at Texas A&M University. The only out-of-state institution to receive substantial funding was the University of Nevada at Reno, close to the California border and within a 2-hour drive of Sacramento.[111]

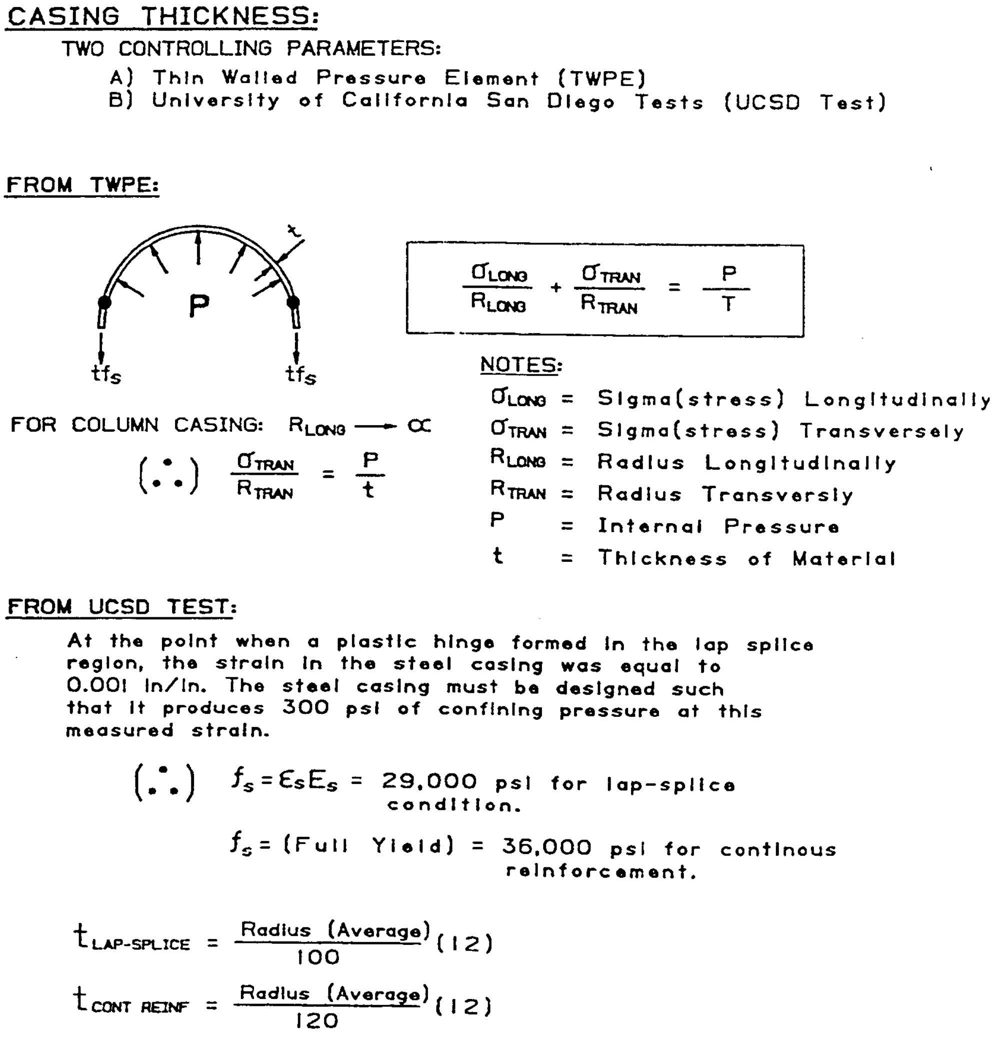
None of this was accidental. According to Jim Gates, research was spread out over several different campuses to avoid the appearance that Berkeley or San Diego had special access to Caltrans resources. Caltrans engineers also wanted to build up a community of researchers within the state who could address their research needs. This reasoning was also behind the decision to fund very little research outside the state or very far from California's borders.[112] There were undoubtedly political considerations involved as well. As the increased level of research continued, Caltrans established an internal committee to establish research priorities, as well as an external committee composed of faculty from California universities, most of whom were actively involved in Caltrans-funded research.

Outside reviewers

The research committee was only one of numerous outside advisory panels that oversaw work at Caltrans following the Loma Prieta earthquake. This was partly because of the recommendations of the Governor's Board of Inquiry, and partly because Caltrans management believed that some degree of outside oversight would help insulate them from criticism.[113] There were peer review panels for many specific projects, and a Seismic Advisory Board was established to give advice on overall seismic policies at Caltrans. All of these panels, with the exception of the previously mentioned research committee, were composed of practicing engineers as well as academic researchers, although the academics tended to play a more prominent role. By funding mostly researchers within the state, and then bringing these same people in to serve on advisory panels, Caltrans created a well-defined group of advisors with close ties to Caltrans engineers. This made it possible for these outsiders to be integrated into the Caltrans risk community with relatively little conflict.

The risk community and the risk network

This dramatic expansion of the risk community led to many changes in the risk network at Caltrans. With the inclusion of academic researchers, the risk network came to include laboratories and all of the testing and data collection systems, analysis tools, and technical personnel associated with them.[114] Test results were increasingly incorporated into design practices and codes, as in the case of steel shells (Figure 2.13) and hinge restrainers. And because researchers were involved in the design process through peer review, they often proposed testing to resolve difficult design choices on specific projects.[115] This changed the risk network by making it possible to empirically show that unfamiliar retrofit technologies and design approaches worked, where these methods might previously have been rejected because of uncertainty about their effectiveness. Researchers proposed many of these new design methods themselves. For example, U.C. San Diego professors Priestley and Seible introduced a new method called “displacement ductility analysis” to Caltrans engineers.[116] This new approach made it possible to eliminate the “Z” factors that had previously been used to take ductility into account and calculate the ductility of each column directly. This represented a significant change in the way the risk posed by columns was understood. The members of the Seismic Advisory Board also played an important role in many decisions about risk, as in their contribution to the development of the prioritization algorithm, described earlier. In fact, there were few



CASING THICKNESS:

TWO CONTROLLING PARAMETERS:

A) Thin Walled Pressure Element (TWPE)
B) University of California San Diego Tests (UCSD Test)

FROM TWPE:

$$\boxed{\frac{\sigma_{LONG}}{R_{LONG}} + \frac{\sigma_{TRAN}}{R_{TRAN}} = \frac{P}{T}}$$

FOR COLUMN CASING: $R_{LONG} \longrightarrow \propto$

$$(\therefore) \quad \frac{\sigma_{TRAN}}{R_{TRAN}} = \frac{P}{t}$$

NOTES:

σ_{LONG} = Sigma(stress) Longitudinally
σ_{TRAN} = Sigma(stress) Transversely
R_{LONG} = Radius Longitudinally
R_{TRAN} = Radius Transversly
P = Internal Pressure
t = Thickness of Material

FROM UCSD TEST:

At the point when a plastic hinge formed in the lap splice region, the strain in the steel casing was equal to 0.001 in/in. The steel casing must be designed such that it produces 300 psi of confining pressure at this measured strain.

$(\therefore)$ $f_s = \varepsilon_s E_s$ = 29,000 psi for lap-splice condition.

f_s = (Full Yield) = 36,000 psi for continous reinforcement.

$$t_{LAP\text{-}SPLICE} = \frac{\text{Radius (Average)}}{100}(12)$$

$$t_{CONT\ REINF} = \frac{\text{Radius (Average)}}{120}(12)$$

Figure 2.13: Steel jacket thickness calculations from Caltrans code. One of the approved methods is based directly on test results from U.C. San Diego. Source: Caltrans Memo to Designers 20-4, Attachment B, August 1996, Figure B1, p. 4. Used by permission.

aspects of the risk network that these new members of the risk community did not affect in some significant way. These impacts are described in greater detail in later chapters.

2.8 Conclusion

When a Caltrans engineer looked over design drawings for a bridge in 1970, seismic risks did not stand out. During design, these risks were taken care of through relatively simple calculations, a minor part of the total task which rarely governed structural choices. The procedure must have seemed rather abstract: part of the state-of-the-art, a symbol of Caltrans' technical sophistication, probably not associated in anyone's mind with nightmare images of crushed concrete and twisted steel.

The San Fernando earthquake was so significant because it presented Caltrans engineers with piles of rubble in place of structural theories. The earthquake "told" them not only that their structural calculations had significantly underestimated the extent of the risk, but that these theoretically-informed calculations did little good if nothing was holding the expansion joints together, if columns weren't ductile, if reinforcing bar could pull out of footings. So these details were analyzed and improved, new methods for calculating structural forces were introduced, and a more sophisticated accounting was made of what hazard could be expected at a given location. First joints, then columns were retrofitted. Increasingly sophisticated methods for weighing one risk against another and prioritizing bridges for retrofit were developed. New actors appeared on the scene. Different design details were tested in laboratories. New approaches to understanding ductility were introduced from academia. By the end of this story, an engineer looking at design drawings for a bridge saw a whole tapestry of risk laid out in front of them, dozens of small details that could bring down a structure if not attended to. Complex seismic calculations ran throughout the design process, often becoming the primary issue on a designer's mind. From a subjective standpoint, this is what it means for a risk network to grow, as risk objects multiply and methods of displacing risk flourish all around them.

Nature can sometimes confront human beings in frightening and incomprehensible ways. Natural disasters and accidents, particularly those that are unprecedented or surprising, may be so far removed from existing social categories that they initially seem to be beyond comprehension. Under such circumstances, human beings usually work very hard to come up with new categories, to socially construct the world in a way that makes an event meaningful. People are usually successful at this, so long as the existing social fabric remains intact. Failure to construct meaning can lead to intense anxiety and alienation and an inability to take action that can lead to further catastrophe.

The classic example of an extended loss of meaning of this sort is the experience of the survivors of the Buffalo Creek flood, described in Kai Erikson's *Everything in its Path*.[117] Faced with the nearly complete destruction of their community, and with normal social routines destroyed by relocation efforts, many survivors were unable to make any sense what had happened for many months afterwards. Another example can be found in Karl Weick's re-analysis of Norman Maclean's account of the death of 13 "smoke jumpers" during the rapid "blow up" of a Montana forest fire. [118] Weick argues that the disintegration of social roles within the group of firefighters in the face of a rapidly-evolving, terrifying situation made it impossible for them to collectively construct a coherent interpretation of events. As a result, they were unable to respond to the unexpected conditions in a meaningful way.

In the San Fernando earthquake, Caltrans engineers were confronted with a set of events that did not immediately make sense. The extent of damage was completely unexpected, and furthermore Caltrans engineers had never been faced with the task of drawing lessons from actual earthquake damage. In order to make the event meaningful, they had to put a great deal of effort into developing new engineering categories to explain the damage, and new organizational routines for incorporating this knowledge into design practice. They had to work to construct an elaborate risk network containing many different specific risk objects where only a rudimentary network had previously existed. Through these efforts, earthquakes were "domesticated" at Caltrans, transformed from wild outsiders that could only cause destruction to integral parts of the risk network which engineers could rely on to tell them about the weaknesses of their bridges.

Why does the 1989 Loma Prieta earthquake not play a similar role in this narrative, which encompasses events surrounding that earthquake? In fact, it did not have a significant transformative effect on the risk network at Caltrans, at least not directly. Caltrans engineers do not say that it told them anything fundamentally new about their bridges, and, unlike the San Fernando earthquake, it is not noted as having had a decisive impact on design practice. While the public response to the earthquake was focused around the collapse of the double-deck Cypress viaduct in Oakland, many at Caltrans felt that this did not teach them anything new, because the deficiencies of structures from this era were already well-known — there just had not been enough funding to retrofit every bridge that needed it.[119]

The changes that were introduced into the risk network after Loma Prieta were largely tied to the expansion of the risk community in response to public concerns rather than to the earthquake itself. New techniques for calculating seismic forces and ductility, for example, came from contacts with academic researchers, not out of observations of earthquake damage. These methods were used within the academic community even before the earthquake. So although the Loma Prieta earthquake did have a significant effect on the risk network, the quake itself did not play a significant role in the definition of new risk objects.

The low profile of the Loma Prieta earthquake in the risk network at Caltrans is an indicator of the extent to which earthquakes have been domesticated within the organization. Most of the damage caused by earthquakes since San Fernando has been integrated into existing categories of risk with little difficulty. Each new earthquake has tended to confirm the soundness of the existing risk network rather than transforming it. Earthquake effects are also anticipated through laboratory testing of structural components under simulated seismic conditions. Large earthquakes have come to be expected events, rather than anomalies, in the thinking of Caltrans engineers.

In addition, standard organizational routines have been developed for responding to and learning from earthquakes. The most significant development in this respect is the establishment of an organized Post-Earthquake Investigation Team to observe earthquake damage and draw conclusions from it. Even where earthquakes produce some anomalous effects, such as the unexpected damage to flared columns observed in the 1994 Northridge earthquake, pathways are already in place for noting the damage, deciding if a potential problem exists, executing research contracts, and integrating the results of testing into design codes.

Of course, it is always possible that future earthquakes may overwhelm existing routines and expectations. Saying that earthquakes have been domesticated within existing risk networks at Caltrans simply means that members of the risk community think and act under the assumption that they now know how to handle earthquakes. But most Caltrans engineers will admit to the possibility that their current understanding of earthquakes may be challenged someday — perhaps when the “Big One” finally arrives. Meanwhile, they are cautiously optimistic.

Figures 2.6, 2.7, and 2.11-2.13 are used with the permission of Caltrans. Figures 2.2-2.4 are from the EQIIS image database of the Earthquake Engineering Research Center, University of California, Berkeley; the center permits them to be freely reproduced. Figures 2.1 and 2.5 are from the report of the Governor's Board of Inquiry on the 1989 Loma Prieta Earthquake, which specifies that excerpts may be reprinted.

Notes

[1] Good reviews of this literature are Covello 1983; Clarke 1988; and Gould *et al.* 1988, 45-59.

[2] A very influential work in this area is Kahneman *et al.* 1982.

[3] Douglas and Wildavsky 1982.

[4] A similar criticism is made in Shrader-Frechette 1991, 38.

[5] Perrow 1984.

[6] Many elements of Perrow's arguments are anticipated in the lesser-known Turner and Pidgeon 1997, originally published, with Turner as sole author, in 1978.

[7] See the review of this literature in Sagan 1993, 14-28.

[8] See, for example, Blumer 1971, Spector and Kitsuse 1973, Spector and Kitsuse 1977, and Hilgartner and Bosk 1988. Gusfield 1981 is a central work, and defines its subject matter as “public problems” rather than “social problems”.

[9] The construction of problems within professional communities is sometimes integrated into constructionist social problems literature in a roundabout way, by treating the research community as a public arena, as in Hilgartner and Bosk 1988, 59. However,

works that do this usually focus on statements made by researchers in published articles or in public forums, rather than communications and practices internal to the research community. For example, see Gusfield 1981, 83-108.

[10] See Clarke and Short 1993, Clarke 1988. Hannigan 1995 examines the construction of environmental risk, examining the connections between scientific practice and public claims-making activities; see in particular pages 76-91 and 100-103. A similar effort is made in Yearley 1991. Short 1984, examining this connection from a different perspective, argues that risk perception is linked to public trust in risk management institutions. Stallings 1995 examines the construction of earthquake risk as a public problem, but does not look very closely at scientific and professional practice.

[11] See Woolgar and Pawluch 1985.

[12] Hilgartner 1992.

[13] Hilgartner 1992, 43; Gusfield 1981, 51-82.

[14] Gusfield 1981, 55-60.

[15] Hilgartner 1992, 48-50.

[16] March and Olsen 1976, 24-37.

[17] Stallings 1995, 116-132.

[18] Compare to Constant's term "communities of practice," which refers to groups of technological practitioners (Constant 1980).

[19] Risk networks almost always refer to objects and relationships that are believed to be real, but here I make no attempt to assess the extent of this correspondence.

[20] See, for example, Ang and Tang 1984 and Thoft-Christensen and Baker 1982. Modern methods for analyzing probabilities of structural failure have their origin in the nuclear power industry; Meehan 1984, while focusing on geological issues, describes some of the history of risk assessment methods in nuclear engineering.

[21] The best and most detailed recent work on how engineers construct risk in organizational settings is Vaughan 1996, which examines the organizational culture of NASA in connection with the Challenger accident.

[22] Oris Degenkolb, "Retrofitting Techniques for Highway Bridges," BN August 1978, GBI CT070 Attachment 5, 1.

[23] Governor's Board of Inquiry 1990, 122.

[24] Ibid., 125.

[25] Ibid., 123.

[26] Ibid., 122-123.

[27] Interview, Jim Gates, August 28, 1997.

[28] Phil Patton notes that, starting in the 1960s, many freeway designers adopted an aesthetic that emphasized horizontal flow and tried to minimize the number of columns "in order to make the structure seem to float." Patton 1986, 135

[29] Degenkolb, "Retrofitting Techniques for Highway Bridges," BN August 1978, GBI CT070 Attachment 5, 1.

[30] Degenkolb, "Retrofitting Techniques for Highway Bridges," BN August 1978, GBI CT070 Attachment 5, 1.

[31] Interview, Jim Gates, August 28, 1997.

[32] Ibid.

[33] Caltrans, "Bridge Details Resulting from Experience Gained from Los Angeles Earthquake," GBI CT006, D-1. In the version of this document provided by Caltrans to the Governor's Board of Inquiry, the page describing these multiplication factors is dated November 2, 1971, but most of the basic material in the package is dated March and Jim Gates recalls that these regulations were originally put out at that time.

[34] Governor's Board of Inquiry 1990, 119.

[35] Blume *et al.* 1961.

[36] See Priestley *et al.* 1996, 267-270.

[37] Jim Roberts, "Recent Advances in Seismic Design and Retrofit of California Bridges," SN December 1990, 2.

[38] Caltrans, "Bridge Details Resulting from Experience Gained from Los Angeles Earthquake," GBI CT006, C-1.

[39] Ibid., C-3.

[40] Ray Zelinski, "Northridge Earthquake Influence on Bridge Design Code," November 9, 1995, 4. Photocopy given to me by the author.

[41] Caltrans, "Bridge Details Resulting from Experience Gained from Los Angeles Earthquake," GBI CT006, H-1. This page consists of a design drawing which is labeled "Hinge Restrainer — Existing Structures" and is dated April 6, 1971.

[42] Governor's Board of Inquiry 1990, 131; Degenkolb, "Retrofitting Techniques for Highway Bridges," BN August 1978, GBI CT070 Attachment 5, 3.

[43] The year the program started is provided in Roberts' testimony, GBI CT070, 5; the number of bridges retrofitted is given in Governor's Board of Inquiry 1990, 132.

[44] Governor's Board of Inquiry 1990, 132.

[45] Some concerns about the ductility of cables and rods under tension are mentioned in Degenkolb's article, but he concludes that restrainers should be designed to function within the elastic range because of the uncertainty associated with ductility. Degenkolb, "Retrofitting Techniques for Highway Bridges," BN August 1978, GBI CT070 Attachment 5, 4.

[46] Degenkolb, "Retrofitting Techniques for Highway Bridges," BN August 1978, GBI CT070 Attachment 5, 2. Jim Roberts later expanded upon this explanation, arguing that a deck that was tied together would transfer seismic force into the bridge abutments rather than the columns. Roberts testimony, GBI CT 070, 5. See also Governor's Board of Inquiry 1990, 130-131.

[47] Degenkolb, "Retrofitting Techniques for Highway Bridges," BN August 1978, GBI CT070 Attachment 5, 2.

[48] Memorandum, James H. Gates, "Subject: Summary of the Single Column Retrofit Program," December 11, 1989, GBI CT029.

[49] See Governor's Board of Inquiry 1990, 132.

[50] Degenkolb, "Retrofitting Techniques for Highway Bridges," BN August 1978, GBI CT070 Attachment 5, 7.

[51] Governor's Board of Inquiry 1990, 132.

[52] Memorandum, James H. Gates, "Summary of Earthquake Engineering at Caltrans," February 22, 1990, GBI CT059.

[53] Interview, Jim Gates, August 28, 1997.

[54] At this time, a non-profit structural engineering group known as the Applied Technology Council (ATC) was also working along the same lines; this work was published as U.S. Federal Highway Administration 1983. See Governor's Board of Inquiry 1990, 124.

[55] Greensfelder 1974.

[56] Interview, Jim Roberts, August 25, 1997.

[57] See more extensive discussion of this method in Chapter 4.

[58] Interview, Jim Gates, August 28, 1997.

[59] Ibid.

[60] Mualchin and Jones 1992.

[61] Ray Zelinski and Earl Seaberg, "Post Earthquake Investigation Team," SN December 1991, 3.

[62] See Scott and Olson 1993, xxii.

[63] Memorandum, James H. Gates, "Subject: Summary of Earthquake Engineering at Caltrans," February 22, 1990, and attached excerpt from PEQIT Manual, "Division of Structures Post Earthquake Investigation Team," April 20, 1988, GBI CT059.

[64] Ray Zelinski and Earl Seaberg, "Post Earthquake Investigation Team," SN December 1991, 3; Mark Yashinsky, "Post Earthquake Investigation Team (PEQIT) Manual," January 1998, 2. Given to me by author.

[65] Interview, Ray Zelinksi, August 26, 1997.

[66] Ray Zelinski and Earl Seaberg, "Post Earthquake Investigation Team," SN December 1991, 3. This collection of equipment is quite extensive and varied. To give a sense of what is involved, here is a sample of some of the items listed in the equipment inventory in the 1998 PEQIT manual (Mark Yashinsky, "Post Earthquake Investigation Team (PEQIT) Manual," January 1998, 10):

1 100' tape
2 12" x 12" magnetic Caltrans auto door signs. (This item comes with a reminder: "remember not to attempt driving with magnetic signs as they will blow off.")
1 8 1/2 x 11 notebook w/protector
1 compass
1 first aid kit
1 General Services credit card for autos
4 sample bags
1 hacksaw and extra blades
1 map of California
2 safety glasses
2 safety vests
1 Standard Specifications and Plans
2 pair of "Easy Talk" walkie talkies
3 California Airport Ground Transportation Directories
1 camera w/flash and 2 extra lenses
3 hard hats

[67] Ray Zelinski and Earl Seaberg, "Post Earthquake Investigation Team," SN December 1991, 4.

[68] Interview, Jim Roberts, August 25, 1997.

[69] These concerns are expressed in a 1985 article by Jim Roberts which is excerpted in Roberts, "Message from the Division Chief," SN December 1991, 1-2.

[70] Ray Zelinski and Anthony T. Dubovik II, "Seismic Retrofit of Highway Bridge Structures," SN June 1991, 9; Interview, Zelinski, August 25, 1997; Interview, Jim Roberts, August 25, 1997.

[71] Interview, Jim Roberts, August 25, 1997.

[72] Interview, Ray Zelinski, August 25, 1997.

[73] Governor's Board of Inquiry 1990, 133.

[74] Interview, Jim Roberts, August 25, 1997.

[75] U.S. Federal Highway Administration 1983.

[76] Interview, Ray Zelinski, August 25, 1997.

[77] Interview, Jim Gates, August 28, 1997.

[78] Interview, Jim Roberts, August 25, 1997; see Park *et al.* 1983 for the results of this research.

[79] Governor's Board of Inquiry 1990, 133; M. J. Nigel Priestley, F. Seible and Y. H. Chai, "Seismic Retrofitting of Bridge Columns," 1988, GBI CT035.

[80] Interview, Ray Zelinski, August 25, 1997.

[81] Ray Zelinski and Anthony T. Dubovik II, "Seismic Retrofit of Highway Bridge Structures," SN June 1991, 10.

[82] See Chapter 5.

[83] See Chapter 4.

[84] Ray Zelinski and Anthony T. Dubovik II, "Seismic Retrofit of Highway Bridge Structures," SN June 1991, 11.

[85] See Janise Sundstrom and Brian Maroney, "Sensitivity Study of Bridge Seismic Risk Algorithms Used in the U.S.A.," SN July 1992, 3-10.

[86] Ray Zelinski, "California Department of Transportation Bridge Earthquake Retrofitting Program," 1985, GBI PAS005.

[87] Oris Degenkolb, "Retrofitting Techniques for Highway Bridges," BN August 1978, GBI CT070 Attachment 5, 2.

[88] U.S. Federal Highway Administration 1983, 8-11; mentioned in interview, Ann Sardo, August 26, 1998.

[89] Brian Maroney, "Prioritization in the Phase II Retrofit Program," SN September 1988, 6-7. The difference between a level 1 risk analysis and probabilistic level 2 and level 3 risk analyses is described in detail in Thoft-Christensen and Baker 1982, 10.

[90] Brian Maroney, “Prioritization in the Phase II Retrofit Program,” SN September 1988, 15.

[91] Interview, Jim Gates, August 28, 1997.

[92] Memorandum, Brian Maroney, “Re: Seismic Retrofit Program,” March 3, 1990.

[93] Brian Maroney and James Gates, “Seismic Risk Indentification and Prioritization in the Caltrans Seismic Retrofit Program, Update,” SN December 1990, 13.

[94] Brian Maroney and James Gates, “Seismic Risk Indentification and Prioritization in the Caltrans Seismic Retrofit Program, Update,” SN December 1990, 8; Interview, Ann Sardo, August 26, 1998.

[95] Cynthia MacLeay, “Report to the Seismic Design Advisory Board: Caltrans Screening Process for Seismic Retrofit,” SN March 1992, 9-10.

[96] Cynthia MacLeay, “Report to the Seismic Design Advisory Board: Caltrans Screening Process for Seismic Retrofit,” SN March 1992, 11.

[97] Interview, Ann Sardo, August 26, 1998.

[98] Janise Sundstrom and Brian Maroney, “Sensitivity Study of Bridge Seismic Risk Algorithms Used in the U.S.A.,” SN July 1992, 4; Inteview, Ann Sardo, August 26, 1998.

[99] Interview, Ann Sardo, August 26, 1998; Sardo’s maiden name was Gilbert.

[100] This is a basic principle of fault tree analysis. See, for example, Modarres 1993, 148-149 and Lewis 1996, 380.

[101] This example is taken from Janise Sundstrom and Brian Maroney, “Sensitivity Study of Bridge Seismic Risk Algorithms Used in the U.S.A.,” SN July 1992, 5.

[102] Ibid.

[103] Seismic Advisory Board 1994, 38.

[104] Interview, Jim Gates, August 28, 1997.

[105] Memorandum, “Subject: Seismic Research Since 1971,” GBI CT070 Attachment 2; on National Bureau of Standards tests see Governor’s Board of Inquiry 1990, 133.

[106] Mark Yashinsky, “Caltran’s Bridge Restrainer Retrofit Program,” WS June 1993, 29; Governor’s Board of Inquiry 1990, 132; Caltrans Memo to Designers 20-3, May 1994, page 34, states: “Earthquake restrainer devices should fail in a ductile rather than brittle manner when subjected to ultimate loading. Therefore, the brackets, connections, and anchorages should be at least 25 percent stronger than the cables or rods.”

[107] Selna and Malvar 1987.

[108] Eric Thorkildsen, “Overview of Caltrans’ Bridge Seismic Research Program,” SN July

1992, 11.

[109] Interview, Jim Roberts, August 25, 1997.

[110] Memorandum, D. E. Kirkland to Jim Roberts, "Subject: Proposed Seismic Related Research Projects," December 6, 1989, EF 55.

[111] Memorandum to Jim Roberts, "Subject: Status/Dollar Amount of Seismic Related Research Projects (Primarily SB36)," December 2, 1991, EF 55; Texas A&M research funding is noted in Memorandum, Tim Leahy, "University of Calif 'Research' Contracts," April 14, 1997, given to me by Leahy, August 27, 1997.

[112] Interview, Jim Gates, August 28, 1997.

[113] Interview, Jim Roberts, August 25, 1997; Interview, Thomas Post, August 26, 1997.

[114] See Chapter 6.

[115] See Chapter 4.

[116] See Chapter 5.

[117] Erikson 1976.

[118] Weick 1993, Maclean 1992.

[119] For example, Jim Roberts wrote of the Cypress collapse that "It was an anomaly, a detail not used to that extent on any subsequent structure. Additionally, the structure was designed in 1951-53 in accordance with the 1949 code which required only 6% seismic force. There was very little new knowledge learned from this structure." Jim Roberts, "Message from the Division Chief," SN March 1990, 2.

Chapter 3

Going Public: Engineering, Media and the State

3.1 Introduction

On October 17th 1989, a strong magnitude 7.1 earthquake hit northern California, centered about 60 miles south of San Francisco near the city of Santa Cruz.[1] This tremor was called the Loma Prieta earthquake after a peak in the Santa Cruz mountains near the epicenter.[2] Although smaller communities to the south were much more strongly affected by the quake, the most striking damage was to the San Francisco-Oakland Bay Bridge and to the Cypress viaduct, a double-decked segment of Interstate 880 in Oakland. 41 of the 62 deaths attributed to the earthquake occurred when the top deck of the Cypress structure fell onto the bottom deck (Figure 3.1).[3] The Bay Bridge was closed to traffic after one segment of its upper deck slid off its seat, tilting down to rest on the roadway below (Figure 3.2). Since these were Caltrans structures, the department became the center of controversies that took shape in the news media, in a number of hearings before legislative bodies, and finally through a board of inquiry convened by the governor. Here, I focus on the interactions between technical experts and government officials in two of these arenas, the media and the board of inquiry, since these seemed to have played the most decisive role in defining the problem at hand and generating solutions to it.

The way events played out in these two arenas reveals the sources but also some of the contradictions of professional power. The media almost automatically grant a certain degree of authority to professional interpretations of problems with technical components. In part, this is because professionals, as credentialed experts, fit the mold of good news sources. Another important reason is that professional groups often have an opportunity to develop coherent interpretations of particular problems long before they become public, and so can speak authoritatively right away. In the wake of the Loma Prieta earthquake, Caltrans engineers exploited these facts in order to get reporters to accept their interpretation of events as legitimate instead of seeking scandal at Caltrans.

After the Loma Prieta earthquake, the political establishment in California turned over much of the authority to investigate the causes of damage to the Cypress and Bay Bridge structures to a board of inquiry composed entirely of technical experts, most of them engineers. Such expert advisory panels are appealing to government officials because they can turn what might be a messy public debate into a dialog between professional peers. The civil engineering profession in the state gained a certain amount of autonomy through this arrangement, using the panel to establish professional oversight of Caltrans design practices and to resolve other intra-professional issues. However, as is often the case when experts give advice to government, politicians were able to use the credibility of these engineers for their own purposes while giving them little real power outside of a limited professional domain.

3.2 Media

The news as a public arena

The first part of this chapter looks at explanations for damage to freeway structures as they were reported in the news media, specifically California newspapers. It focuses primarily on the collapse of the Cypress viaduct because this is where much of the media attention focused. However, it is not primarily an analysis of the activities of reporters, but focuses rather on the claims and counter-claims made by engineers and public officials with the media as their forum. Of course the news media, like any other

Figure 3.1: Part of the collapsed portion of the Cypress Viaduct in Oakland following the Loma Prieta Earthquake. Source: Loma Prieta Collection, Earthquake Engineering Research Center, University of California, Berkeley.

Figure 3.2: Collapsed deck segment of the San Francisco-Oakland Bay Bridge after the Loma Prieta earthquake. Source: Loma Prieta Collection, Earthquake Engineering Research Center, University of California, Berkeley.

public arena, has a strong influence on the way claims are made, problems are constructed, and blame is allocated within it. Journalists follow routines for constructing stories that tend to “frame” issues in characteristic ways.[4] But the ability of the news media to sustain a particular interpretation of events is also heavily constrained by other journalistic conventions.

In particular, reporters become dependent on sources, the organizations and individuals that provide the facts and quotations that are the basis for a story. Journalistic norms of objectivity are now widely taken to mean that reporters should avoid explicit interpretation of events, instead using only those interpretations that are supplied by sources.[5] When reporting on controversies, especially those with a scientific or technical basis, reporters tend to avoid analysis of the content of the problem, instead following a “polarized” style in which the views of opposing experts are contrasted to one another with little further interpretation.[6] In addition, reporters often come to identify with their sources, accepting aspects of their view of the world, both because they need to maintain good relationships with the sources and because their status in the eyes of other journalists is partially tied to the status of the sources they have access to.[7]

Although reporters and news organizations do exercise considerable control over the selection and promotion of problems, they often function more as “gatekeepers” who, through their selection of sources, determine which actors are able to gain access to the news arena. They tend to overwhelmingly select government officials and, secondarily, credentialed experts since they are regarded as the most authoritative sources.[8] The relatively small group of actors who are allowed into the arena have substantial freedom to make claims, push agendas, and react to statements made by others, even if their actions are limited by the way the arena is structured. In these respects, however, the news media is not very different from other public arenas — political institutions, for example — in which problems are defined and constructed. It is an arena in which sources can legitimately be said to interact with each other, not just with reporters.

Physical causes

In the days following the October 17th earthquake, the media reported a number of different stories about why the Cypress viaduct collapsed. These accounts generally fell into two distinct categories: those focusing on the immediate physical causes of the collapse, and those focusing on human agency.[9] The physical causes of the collapse were given significant space in news stories following the earthquake, but the issue was settled quickly and soon disappeared from media accounts.

Initial reporting on the earthquake focused almost entirely on its impact on people, on rescue efforts and reports of damage and injury. However, discussions about the causes of the collapse began to appear on October 19th, the second day of press coverage. Reporters relied almost entirely on sources in the engineering profession, both practicing engineers and university researchers, in their articles about the physical reasons for the collapse. A significant exception in some of the initial stories was State Assemblyman Richard Katz, the chair of the Assembly's transportation committee. Katz gave a confident analysis of the problem, locating the cause in the columns. The *San Francisco Chronicle* summarized Katz's analysis: "the difficulty arises from the fact that in an earthquake, stresses can pull the left-hand columns further to the left and the right-hand columns further to the right, causing the freeway to drop cleanly through the middle."[10] While Katz' explanation would be considered a bit naïve by most structural engineers, it was not entirely inconsistent with their initial explanations. On the same day, the *Los Angeles Times* quoted a practicing engineer and a well-respected U.C. Berkeley professor, both of whom blamed the collapse on insufficient reinforcement of the columns.[11]

This story of insufficient column reinforcement no doubt made sense to these engineers because they were aware that Caltrans had been planning to retrofit bridges for just this reason. But attention almost immediately turned to a somewhat different explanation, first articulated in the press by Nigel Priestley of U.C. San Diego. Priestley suggested that the real weakness was the lack of continuous reinforcement between upper and lower segments of the columns on the elevated structure. Although he conceded that the columns were "under-designed by today's standards" he claimed that the joints between column segments were “the real weak link.”[12] On October 26th, several academic earthquake experts gave a press conference at U.C. Berkeley essentially confirming this analysis. Professor Jack Moehle cautioned that “the failure of the (Cypress viaduct) was a failure of the whole system (of the viaduct). It’s a very complicated structure,” but explained that “there was no reinforcing steel to hold the upper columns in place under heavy loading, which means those columns could break loose and drop. When the column breaks free, it slides out and the deck comes down” (Figure 3.3).[13] Although there was some discussion at the press conference of the possibility that the structure resonated at the same frequency as the earthquake shaking, making it more vulnerable to collapse, in newspaper reports the joints were clearly emphasized as the main cause.[14] In the next day’s *Chronicle*, Jim Roberts of Caltrans is quoted as agreeing with this assessment.[15]

After reporting this remark from Roberts, the newspapers printed very little further about the physical cause of the collapse until it was discussed again by the Governor’s Board of Inquiry. The idea that it was the joints that failed was still mentioned occasionally, but no new authorities were cited. The media treated this issue as having been definitively decided — or at least, as lacking in news value — after the engineering community appeared to have reached consensus on the issue.

Blame

In contrast to physical causes, there was a much greater variety of stories about human responsibility for the Cypress collapse, and these stories were given much more prominence for a longer period of time following the earthquake. “Winning” media accounts — those seen as having the greatest news value by journalists and editors — tend to focus on individuals and their motivations, rather than abstract physical and social forces.[16] Because of this, the news media tends to frame situations of natural, technological, or social disorder — all of which certainly come into play after an earthquake — as “moral disorder” stories. The classic form these stories take is “exposés” that “reveal instances of legal or moral transgression, particularly by public officials and other prestigious individuals who, by reason or virtue of their power and prestige, are

Figure 3.3: This damaged column from a section of the Cypress Viaduct that did not completely collapse illustrates the failure of the joint at the level of the lower roadway, which engineers agreed was the main cause of the disaster. Source: Loma Prieta Collection, Earthquake Engineering Research Center, University of California, Berkeley.

not expected to misbehave."[17]

Engineers both inside and outside of Caltrans felt that reporters started out looking for a "smoking gun" within Caltrans.[18] Journalism scholar Conrad Smith conducted a survey of news sources cited in reporting on the Loma Prieta earthquake. A U.C. Berkeley engineering professor commented:

> Coverage by local newspapers in the Bay Area was *very* sensational. I was interviewed many times by these reporters about the earthquake and in most cases I felt the most important item in their mind was to find out if there is a 'Watergate' case here. Rather than trying to obtain technical facts from me . . . the local newspapers were mostly interested in discovering a scandal of a grand scale, maybe for a Pulitzer.[19]

Another engineer reported that "the young, well-meaning reporters consistently begged me to 'really tell 'em who is at fault,' there must have been someone who knew the Cypress was going to collapse, come on, tell us who it was."[20] An engineer who had been involved with the original construction of the Cypress viaduct found that "the reporters who called me after the collapse were all convinced that there was one engineer sitting on a porch somewhere drinking a martini, and that he was fully and completely to blame for the collapse."[21]

Although such lurid stories never actually appeared in news accounts, probably because reporters could not find any sources who would make such claims, some stories did appear that suggested, in a more subtle but somewhat inconclusive way, that Caltrans engineers might be to blame for the collapse. One track this took was speculation about whether mistakes had been made in the design and construction of the Cypress structure. Reporters were evidently asking questions on this subject, because Jim Roberts, who had been involved with original construction, had explain that the Cypress was built "exactly in accordance with their plans and specifications" and said he was sorry for those who lost loved ones, "but do I feel guilt? I don't, because there was nothing we did that was out of line with what was required."[22] A few days later, a former Caltrans engineer was quoted as saying the Cypress had not been built according to prevailing design standards as specified in a 1952 manual of the Concrete Reinforcing Steel Institute. Roberts responded that the freeway had been built according to the standards of the American Association of State Highway Officials, and if it were flawed, "a whole lot of people who are paid to review the construction, including the federal government, which comes back and does an audit inspection, didn't do their jobs."[23] This line of speculation quickly disappeared, again perhaps because there were not enough authoritative sources willing to sustain it.

Reporters still seemed to be looking for a scandal within Caltrans, however, which was reflected in coverage of an apparent dispute within Caltrans about whether the technology to retrofit the Cypress existed or not. Assemblyman Katz had stated, when interviewed two days after the quake, that Caltrans knew about problems with the Cypress but had not been able to develop the technology to fix it.[24] A number of engineering sources suggested the opposite, that the technology existed but that money was not available. Caltrans Chief Engineer William Schaefer seemed to support Katz's contention, stating that existing retrofit methods, such as steel jacketing, had not been tried on multiple-column structures like the Cypress: "We really don't have the expertise to know what to do to fix these . . . we don't have the technical knowledge, nor does it exist anywhere in the world," noting that research was underway at U.C. San Diego to develop the technology.[25]

Shortly thereafter, an anonymous engineer described as a "high ranking seismic expert" at Caltrans spoke to the *Los Angeles Times* and contradicted Schaefer, claiming that the technology to retrofit structures like the Cypress had existed for nearly 20 years, but had not been used because of budget limitations. Schaefer responded that it was not known whether steel jackets would work on multiple-column structures, but "if we understood how to put those same steel jackets on the multiple-column structures, that probably would have worked." However, "we don't know how to do that yet." [26] Finally, Jim Roberts spoke to the press and acknowledged that the technology did exist, but that any retrofitting would have to be temporary and not "pretty" since more research was required on how to implement the technology.[27]

It is not entirely clear what the news media saw as being at stake here, but the suggestion initially appeared to be that Caltrans might somehow be responsible for the collapse had retrofit technology been available but not used, but could not be blamed if the technology did not exist. The alleged dispute seems to have been largely a semantic one, however, and it is not clear whether the engineers quoted actually disagreed with one another or were just presenting their opinions differently. In the end, as sources clarified their remarks, it became apparent that there was no simple answer to the question of whether the technology existed, and this moral disorder story ceased to be of interest to the media.

Politicians vs. engineers

Attempts by the media to find an engineering scandal at Caltrans were probably fed, in large part, by the remarks made by many politicians after the earthquake. Initial reports about possible human or political responsibility for the Cypress collapse revealed a striking disjunction between the expectations of politicians and engineers about the survivability of freeway structures, particular older ones like the Cypress. Political figures, like most of the general public, appear to have been extremely and perhaps unrealistically confident in the ability of the transportation system to survive a major earthquake intact. When Governor George Deukmejian, on a trip to Germany, was woken in the middle of the night by reporters and asked about the Cypress collapse, his response was “I was always under the impression that they were built to withstand that kind of quake.”[28] The following day, he made similar comments, which were printed alongside an incorrect statement by Assemblyman Katz that all freeways in the state were supposed to survive an earthquake of magnitude eight or greater.[29] Two days later the Governor’s chief of staff made an even stronger statement that seemed to place the responsibility for the disaster squarely on Caltrans. He claimed that “the governor was assured by Caltrans that the freeways would withstand an earthquake of Tuesday’s magnitude” and that the governor would have ordered the Cypress viaduct closed if he had known there was a risk. He added, “there was no indication that something like this would occur. Something obviously went very, very wrong and we’re determined to find out what it is, so we can prevent it from happening again.”[30]

In sharp contrast to these views, engineers interviewed by the newspapers expressed little surprise about the collapse. Unlike the politicians, they appeared to be familiar with the history of seismic issues at Caltrans. An engineer associated with the

California Seismic Safety Commission explained that the Cypress structure consisted of "nonductile concrete, a well-known type of hazardous construction,"[31] and Vitelmo Bertero of U.C. Berkeley added that "this is a problem typical in many bridges . . . they were designed many years ago." A civil engineer based in Berkeley described how Caltrans had been trying to address seismic problems with existing bridges since the 1971 San Fernando earthquake.[32] In contrast to this lack of confidence, Caltrans seismic expert Ray Zelinski told the *Los Angeles Times* "we didn't think a total collapse was a real possibility," although not everyone at Caltrans had been so confident.[33]

After these comments were printed, politicians like Deukmejian and Katz, perhaps pushed by the media, seemed to realize that their assumptions about seismic safety had been wrong, and that there were known problems with freeway structures in the state. Deukmejian's remarks took on a defensive tone as he backtracked from his claim that Caltrans had assured him all freeways in the state were safe from large earthquakes. Instead, he made the negative claim that "at no time have they ever said 'Governor, there's a possibility that these bridges or double-decked freeways might collapse in an earthquake.' Never at any time had I been given that kind of information."[34] Meanwhile, Katz took a different approach that was to set the tone for later debate, asking why it was that the retrofit program hadn't been completed after 18 years if Caltrans knew there was a problem.[35]

As the engineering community was able to effectively neutralize the rather inflammatory rhetoric of the politicians, questions about engineering misconduct seem to have been resolved decisively in Caltrans' favor in news accounts within a few weeks of the quake. The extent of the media's conversion was evident when San Francisco Supervisor Bill Maher held a news conference in November suggesting that he would "like to see Caltrans consider prosecution for the people who designed the freeway that murdered people." The *San Francisco Chronicle* noted that "no other politician has called for prosecution of the freeway's designers" and caught Maher on the defensive as he insisted his remarks "were not a political ploy" designed to bolster his long-time campaign to have the Embarcadero freeway in San Francisco — a double-deck structure like the Cypress — demolished.[36]

Politics-as-usual

As efforts to find a scandal at Caltrans were frustrated, media accounts turned their attention to another "moral disorder" story about political conflict between the governor and other state officials. Rather than looking for wrongdoing at Caltrans, reporters simply began to tell a polarized, two-sided story in which opposing interests were given roughly equal space to make their case. The dispute was mainly between Governor Deukmejian and Caltrans, and centered on the question of who was responsible for the lack of funding and slow pace of seismic retrofit. This theme was summarized concisely by a highway contractor quoted in the *San Francisco Chronicle*: "It's the classic problem — Caltrans wants more money for highway maintenance and you can't get the politicians off the dime."[37]

The first volley in the debate came from Professional Engineers in California Government (PECG), a group representing many Caltrans engineers. After Governor Deukmejian's early suggestion that Caltrans was to blame for the collapse, the group said in what the *Los Angeles Times* termed "a scathing news release" that if Deukmejian wanted someone to blame, "he needn't look any further than his own bathroom mirror. Year after year, Caltrans requested funds to hire the staff needed to do its work. Instead, the governor imposed hiring freezes and budget cutbacks. . . . No wonder that now, 18 years after the Sylmar [i.e., San Fernando] quake, a . . . project to strengthen these bridges is only one-third complete." In the same article, the afore-mentioned anonymous Caltrans engineer stated that retrofitting had not occurred because of budget and priority considerations, that "there is only so much money" and "you just get caught in trying to spread the money where it is best used."[38] The following day, former governor Jerry Brown, who had been responsible for dramatic cuts in Caltrans' budget in the mid-1970s, appeared on CNN and said of highway maintenance, "I didn't make it enough [of a priority], and I don't think George Deukmejian learned from what I didn't do. The money must be invested. That means taxes; that means bonds."[39]

Deukmejian responded angrily to these remarks: "No one has ever said to me there hasn't been enough money to carry out repair (and) maintenance work for public safety. Listen, the safety of the people comes first. I mean, that comes before relieving traffic congestion. And certainly if there's anyone in Caltrans — or anybody else in the state — who would think otherwise, they don't understand what our policy is." He added that "all of the priorities that Caltrans recognized (as) needed to be accomplished have been accomplished. To my knowledge, any request that we have received [for funding] . . . for work relating to protection against seismic activity . . . has all been approved and authorized. Nothing has ever been turned down or denied."[40]

Caltrans' side of the story was presented in more detail in a carefully researched October 26th article in the *Los Angeles Times*. The *Times* spoke to a number of current and former Caltrans engineers who reported that their anxiety about getting the retrofit work done was countered by a grim financial situation and the need to address other priorities. Former head of the Division of Structures Oris Degenkolb said he had urged faster work because "you knew an earthquake was coming" and "there was going to be a catastrophe coming out of it and something ought to be done about it before we get caught with our pants down." One engineer said Caltrans engineers felt lucky to get the money they were getting for retrofit: "We were all champing at the bit and wanting to get it done" but "the money was the main drawback on why we weren't moving faster." Another said "the attitude was there was only so much money and that's it." Some unnamed "top Caltrans and Administration officials" were said to believe that "every aspect of highway construction and maintenance has been affected by a shortage of gasoline tax funds that began with the Arab oil embargoes of the 1970s." The California State Finance Director disagreed, stating that "we put a lot of money into highways — there has been a lot of rehabilitation, a lot of widening, a lot of safety work. So it just doesn't make sense to say there is a chill in the air that has scared people away (from requesting funds for earthquake safety)."[41]

Under financial pressure, according to Caltrans officials, it was a legitimate choice to make trade-offs between seismic retrofit and other needs. Caltrans Director Robert K. Best acknowledged that retrofit "was not the highest priority in the department for the expenditure of whatever funds became available. It's just as simple as that." Chief Engineer William Schaefer said "I could not in good conscience say we would focus every dime on earthquake safety programs to the exclusion of every other program." An unnamed “senior transportation official in a previous Administration” explained that a trade-off was often made between seismic safety and fixing so-called “blood alleys,” or sections of freeway where many accidents occur, for example by regrading or adding median barriers. “The debate always was whether you worked on a freeway such as this or fixed the blood alleys. The blood alleys always won out over questions of structural integrity. It was a case of some obscure engineering report that says there may or may not be a problem someday versus a freeway where people are getting killed now.”[42] This reasoning was amplified in remarks by Jack Moehle of U.C. Berkeley, who stated in a press conference that Caltrans shouldn’t be “greatly castigated” because it is necessary to balance the risk against the cost of retrofitting. “There are limits to what one can do,” he said, “to bring all buildings and bridges up to code would (financially) ruin society.”[43]

By this point, Governor Deukmejian and his aides were no longer making many public statements on the funding issue, instead focusing their remarks on the board of inquiry that was being convened to look into the causes of the damage. But by December, other political figures seemed to be shifting their statements in Caltrans’ favor. Assemblyman Katz, while critical of Caltrans, seemed to find their explanation plausible. In an article that chronicled Caltrans’ financial woes in considerable detail, the *Los Angeles Times* reported that Katz “believes that the engineers fell victim to an attitude typical of bureaucracies.” Katz explained, “they’re too willing to accept the answer of ‘we can do it tomorrow if we can’t do it today.’ And if you’ve gone through Caltrans under eight years of Jerry Brown and gotten your brains beaten in pretty bad, and then Deukmejian’s early signals were that it’s not going to be much different in terms of more staff for engineering or more money for roads, my guess is that after eight, 10, 12, years of that, you decide ‘What’s the point?’ ”[44] State Senator Quentin Kopp, head of the Senate Transportation Committee, said Caltrans “became the whipping boy” because “they were an easy target.”[45]

By June 1990, when the Board of Inquiry report came out, the conflict between Caltrans officials and the Governor seemed largely to have died out. Instead of seeking to put blame on one another, both Caltrans Director Robert Best and Governor Deukmejian were looking forward, trying to convince the public to vote for a ballot proposition

(Proposition 111) that would increase the gasoline tax and provide the funding necessary for a full-scale retrofit program.[46]

Exploiting journalistic conventions

Many engineers felt that Governor Deukmejian's remarks after the earthquake provided added motivation for the media to seek out a scandal at Caltrans. According to Jim Roberts,

> It was unfair because we had a governor who was in Germany when he got the news and he said I was led to believe these bridges were all safe. I have no idea where he got that feeling or that impression, because we had just put a little video together showing what we were going to do, and we were just embarked on the program. . . . Obviously some staff has fed him that information . . . he essentially . . . said we're going to find the guilty parties, he used words like that. Of course . . . you look at his background, he's a district attorney and law enforcement's his whole life . . . before he was a governor, and I think that he was totally misinformed.

To add insult to injury, when the governor arrived in Oakland from Germany the day after the earthquake, Roberts and Caltrans Director Robert Best met him at the airport: "we were there to explain . . . some of the facts to him, and our director couldn't even get close to him, there were so many press and politicians around." Before they had a chance to speak to him, he held a press conference and repeated the same "misinformation."[47]

Under pressure from the media and with little prospect for help from higher political levels, Caltrans administrators and engineers decided that they would have to win the media over in order to avoid becoming political scapegoats. The most intensive scrutiny came from the *Los Angeles Times*, which wrote to Robert Best two days after the earthquake, asking that Caltrans provide copies of "all reports, notes, memos, correspondence, supporting documents and any computer-stored data gathered by your department after the Whittier earthquake of October 1, 1987 concerning the seismic safety of all single- and multiple- column freeway structures in California" under the provisions of the California Public Records Act.[48] Legal counsel for Caltrans responded, noting the difficulty in making copies of all these documents with time in short supply and inviting *Times* reporters to go through Caltrans files in person in Sacramento.[49]

Jim Gates, who supervised *Los Angeles Times* reporters' access to files, recalls that the process "was kind of scary" and that reporters initially misinterpreted some documents to suggest an ongoing dispute between him and Nigel Priestley over some aspect of research at U.C. San Diego. But the process also put Caltrans engineers in close contact with reporters from the *Times*, which gave them an opportunity. According to Gates, they were able to get their views across to a key Sacramento-based reporter: "we finally sat down with her and explained to her what was going on, and they finally understood. . . . Once we turned the L.A. Times around, everything was okay."[50] The general strategy was to be as open and responsive to media inquiries as possible, as Jim Roberts explains:

> I mean I did press conferences for two weeks every day, [for an] hour, an hour and a half, after that earthquake, and it took us well over a month to turn the *San Francisco Chronicle*, the *San Jose Mercury News*, and the *Los Angeles Times* around. And once they got turned around and they understood the whole thing was budget-driven, they became fairly supportive of us . . . we basically educated the press, we gave them a status report every day, and any question they asked we either answered or got an answer [to].[51]

At the same time, management was trying to limit the number of conflicting statements from Caltrans engineers that appeared in news stories, such as the remarks of the anonymous seismic expert disputing Chief Engineer Schaefer's claim that the technology did not exist to retrofit structures like the Cypress. Schaefer sent a memo to all engineers telling them that "the buck stops here" and that if they have problems with statements made by him or Jim Roberts, they should bring them to the attention of the Caltrans Ombudsman rather than the media.[52] On November 7, Robert Best sent out a memo stating that "Caltrans employees are to be complimented for their effective efforts in providing information to the public during this difficult period" but ordering that all further media contacts on earthquake-related issues should be directed through the public affairs office.[53] At some point between October and December, Caltrans had also initiated a contract with consulting firms Cygna and ICF/Kaiser to develop a "public awareness program" on seismic issues. The firms provided a number of suggestions not only on how Caltrans ought to deal with the public and the media, but also on how they could respond to questions raised by the Board of Inquiry.[54]

Caltrans was able to exploit journalistic conventions and routines to their advantage in this case. The media tends to focus not just on officials and experts, but on those officials and experts that they are familiar with and have easy access to.[55] Initially, this worked against Caltrans because most of the engineers and managers who were associated with bridge engineering were not familiar to reporters, who relied instead on readily-available official sources like Governor Deukmejian and Assemblyman Katz for explanations. But following a policy of maximum openness to reporters, particularly in the weeks immediately following the earthquake, ensured that they would become familiar with Caltrans experts. Once this familiarity was established, Caltrans employees had the credentials and official status to serve as authoritative sources for reporters. Finally, because reporters generally seek to balance the opinions of opposing experts in a technical controversy without evaluating the merits of each side's arguments, Caltrans engineers were given space to make their arguments without intense scrutiny of their substantive actions.

A consequence of this was that Caltrans engineers' definitions of and ways of dealing with earthquake risk were almost automatically given a certain degree of legitimacy in media accounts. Caltrans was able, as a result, to dictate the grounds of debate to a very significant degree: there was relatively little discussion, and less as time went on, about whether Caltrans seismic design and retrofit policies were the correct way to deal with seismic safety problems. Even their most stringent critics generally focused on whether the program had been carried out fast enough. Other engineering experts, particularly those from universities, generally backed up Caltrans engineers' interpretations of events, and their explanations were presented as authoritative in news stories.

As suggested in the previous chapter, professional communities often are able to have a decisive influence on how certain issues are shaped as problems in public arenas, because even though they lack political power, they have often had nearly exclusive control over a problem before it reaches public consciousness, and so have had an opportunity to define the problem and possible solutions to it. Their definitions often form the basis for public debate, and discussion of possible solutions often centers around those that are readily available from the professional community. In this case, no alternatives to the existing retrofit program were ever discussed in detail. Those alternatives that were brought up, such as getting rid of double-deck structures, were quickly dismissed by the expert community and dropped from media accounts. Other possibilities, such as demolishing and rebuilding older structures, never came up. In the end, even though they were subjected to some outside oversight, Caltrans engineers got the money to carry out the retrofit program they had always wanted to do, essentially on their own terms.

3.3 The Governor's Board of Inquiry

Two days after the earthquake, on October 19th, Governor Deukmejian announced that he would create an independent panel to investigate the Cypress viaduct and Bay Bridge collapses.[56] Even as the war of words between Deukmejian and Caltrans raged on in the media, Deukmejian and his aides sought to establish a panel that would be seen as disinterested and objective.[57] To this end, they turned much of the process of assembling a panel and conducting hearings over to representatives of the engineering profession. This gave the profession a great deal of power to shape public interpretations of the events surrounding the earthquake, and to shape governmental responses to them. It also provided engineers with an officially-sanctioned forum for advancing professional agendas. As might be expected, the issues raised in this arena, as well as the manner in which they were discussed, differed significantly from the content and style of media reports.

Constituting a professional body

The governor and his staff played a crucial role in setting the ground rules which the engineering profession would have to follow in order to be given authority over the issues raised by the earthquake. Initially, the main criteria seemed to be stature and technical competence in the field of earthquake engineering. An indicator of this was that, despite the evident political tension between the governor and Caltrans engineers, the governor turned to them for advice about who would be qualified to lead the inquiry. From a list of six candidates provided by the transportation department, Deukmejian selected Ian Buckle, a respected researcher and deputy director of the National Center for Earthquake Engineering Research at the State University of New York at Buffalo.[58]

The governor was apparently unaware that Buckle had already written an op-ed piece for the *Los Angeles Times* that defended the pace and priorities of Caltrans' retrofit program, which read in part:

> Considering the potential for disaster, one could ask why something wasn't done about these bridges. In fact, Caltrans engineers were well aware of the problem and had been actively pioneering various solutions for retrofitting bridges to bring them up to post-'71 codes. No other agency, state or federal, is as far advanced as Caltrans concerning this technology. But when it comes to bridge retrofit, where do you start? There are more than 13,000 bridges in the state system. It is simply not possible to upgrade all of them simultaneously. . . . The difficult bridges, you leave for later. Structures like the Bay Bridge and the Nimitz [i.e., Cypress] double-deck are in this class. Their monumental size, plus their structural type, precluded the use of conventional methods of retrofitting. . . . Now is not the time for knocking Caltrans. Rather, it's time to push for more funds, both state and federal, to develop and implement strategies for the retrofitting of the remaining bridges in the Caltrans program."[59]

When asked about this article, the governor and his staff initially defended their choice of Buckle. Deukmejian's press secretary stated "we believe he is going to be objective in his evaluation" and "we are certain he is going to be critical where he needs to be critical."[60] But the governor was more cautions the next day, saying of Buckle's piece "I will talk to him about that" and "if he has already made some preconceived decision, then he obviously would not be the appropriate person to head up that team." Buckle defended himself, indicating that "if I find fault (with Caltrans) I would have no problem in bringing it out in public." At the same time, however, he probably sealed his fate by revealing to the reporter that he was familiar with Caltrans because he had served as a consultant in the design of a bridge on State Highway 101. He hastened to add that "one cannot work on the seismic design of bridges without working closely with the people at Caltrans, because they do more of it than anyone else."[61]

Deukmejian apparently was not satisfied with this explanation. The next day, he rescinded his appointment of Buckle, expressing concern that he might already have come to a conclusion in Caltrans' favor. A few days later, he selected Caltech engineering professor emeritus George Housner as the new head of the inquiry panel.[62] Housner was a pioneer researcher in the field of earthquake engineering and, ironically, a significantly more prestigious figure than Buckle in some ways. But from the governor's perspective, his most important qualification for the job may have been that he had no connections to Caltrans. He was also from California, which many engineers in the state regarded as an important qualification.[63]

Housner immediately began working with the governor's staff to select the remaining members of the inquiry panel. The emphasis on technical competence that led to Buckle's appointment was now accompanied by a demand that prospective members should, like Housner, "indicate that they had no preconceived opinions and that they had no current contractual or other ties with Caltrans that might be perceived as a conflict of interest."[64] Coupled with another new requirement that most of the panel members should be from California, this made the selection of board members a somewhat difficult process.[65] Despite the difficulties, Housner managed to assemble a technically-qualified and respected 11-person panel (including himself), of which 10 members were from California.[66] The panel consisted of four structural engineers and one geotechnical engineer from California universities; one structural engineer and one architect in private practice; a geologist from the U.S. Geological Survey; and two engineers representing the Federal Highway Administration and the National Transportation Safety Board. The last two were *ex officio* members who participated in the hearings as representatives of their agencies, but were not involved in the preparation of the final report.[67]

Deukmejian formally established the Governor's Board of Inquiry on the 1989 Loma Prieta Earthquake by an executive order issued on November 6th.[68] The board held seven hearings around the state from November 1989 through May 1990. Like the panel itself, these hearings were unmistakably dominated by the engineering profession. 53 of the 69 persons who testified at the hearings were engineers, and engineers were responsible for 84% of the lines of testimony recorded in the detailed meeting minutes.[69] The rest of those called to testify included six geologists or seismologists, a number of government officials, and a photographer who had conducted an aerial survey of the collapsed Cypress freeway. Unlike in many public hearings, there were no lawyers or elected officials controlling the proceedings. Most of the questioning and answering was between professionals. There was little public participation, either, though the hearings were open to the public. Minutes indicate that only between 15 and 30 spectators were present on any given day, with the exception of the final meeting where Deukmejian formally accepted the board's report, which drew a large crowd. Audience questions and comments were solicited near the end of most of the hearings, but only four questions were asked.[70] The proceedings also were not given much attention in the media; newspapers noted the first meeting and reported extensively on the conclusions of the board, but there was virtually no reporting on the content of the hearings themselves.

Creating a professional setting

In contrast to media accounts about the damage caused by the earthquake, which were (at least initially) full of efforts to pin responsibility on one party or another — "moral disorder" stories — the board of inquiry hearings are striking in their non-confrontational tone and almost complete lack of any kind of rhetoric of blame or even overt criticism. These aspects of the hearings were the result of a deliberate effort by the governor, the board, and those testifying before it (particularly Caltrans engineers) to make the hearings a forum for collegial interaction between technical experts rather than an adversarial contest. Deukmejian made it very clear to Housner at the outset that he wanted a "fact-finding" rather than "fault-finding" report from the board.[71] Housner tried his best to set this tone in the hearings, first by selecting board members "who were not going to get in there and rock the boat." (And, in the end, "nobody did.")[72] He did take the effort to invite two of the original designers of the Cypress structure to dinner to make sure they had no doubts about the design or construction that they were reluctant to discuss publicly, but they had nothing critical to say and were never asked to testify at the hearings.[73]

Though neither the board nor Caltrans wanted a confrontation, neither side was sure it could trust the other at first. Housner later recalled: "I had expected more people to come in and try bluffing. In 1971, following the San Fernando earthquake, I testified at a hearing of the Assembly Committee on Transportation on the collapse of the freeway structures. I felt that the Caltrans people who testified were not very open in their remarks."[74] But his doubts quickly dissipated: "We got a lot of information from Caltrans. They were very cooperative. Of course we knew the people — Jim Roberts, and Jim Gates . . . they provided a lot of information in testimony, reports and papers."[75] Based on their experiences with the governor and the media, Roberts and Gates at first worried that the board might be similarly critical of Caltrans. But when the hearings started, they found themselves in a much more comfortable position: "we were talking to technical people who understood what we were talking about . . . these were understanding people who understood the issues." [76] Just as they had with the media, Roberts and Gates stuck to a strategy of answering all questions straightforwardly, and "overwhelmed" the board with information and documentation.[77] Still, they worried about one or two board members who seemed to ask more "probing kind of questions."[78] Roberts reports that their doubts were largely dispelled when one of these board members approached him at a breakfast buffet at the hotel where both were staying during one of the hearings and told him that it was becoming obvious that Caltrans was "shackled" by "lack of the money to do what you needed to do."[79] There was clearly other back-channel communication going on between the board and Caltrans; at one point the hearing minutes reported that "G. Housner stated that several members of the Board would want to go to Caltrans to discuss more technical matters than suitable for a public meeting."[80]

With engineers and a few other professionals on both ends of the interaction, the hearings immediately took on the very technical tone of a conversation between expert peers. Much of the testimony was simply aimed at providing the board with the facts of the case: the size of the earthquake, ground motions and soil conditions where damage occurred, and the structural details and exact nature of the damage to the Cypress structure and the Bay Bridge. The other major portion of testimony focused on the technical substance of engineering practice. Caltrans engineers and outside structural engineering experts were called on to provide engineering analyses of the precise nature of the structural failures and their root causes. The panel asked detailed questions about

Caltrans seismic design procedures and performance requirements, and about retrofit design and prioritization. Researchers from U.C. Berkeley were called to testify about tests they had done on the remaining portions of the Cypress structure, and professors Nigel Priestley from U.C. San Diego and Lawrence Selna from UCLA were called to discuss the results of their research for Caltrans. Other engineering witnesses talked about the history of design codes and how Caltrans codes compared to those elsewhere.[81] Though some of these discussions could clearly being construed as efforts to assess the competence of Caltrans engineers, the dominant impression carried away by the major participants was of a professional dialog, the sort of discussion engineers might have at a conference or a design meeting, not anything resembling courtroom drama.

The board's report, *Competing Against Time*, generally followed this pattern.[82] Of its 12 chapters, 5 were factual reports about the seismology of the earthquake and its effect on Bay Area transportation networks in general and on the Cypress structure, the Bay Bridge, and the other San Francisco freeway viaducts. Three dealt with the history of seismic design codes and the Caltrans retrofit program, and the ongoing retrofit of the viaducts. In addition, there were two short chapters dealing with the activities and composition of the board, and two chapters containing lists of findings and recommendations.

Although many of the 52 findings were straightforwardly informational — e.g., "the duration of the strong phase of ground shaking generated by the Loma Prieta earthquake was unusually short for an earthquake of Magnitude 7.1"[83] — an equal portion took the form of judgments about Caltrans engineering practices. In general, however, the tone of these findings was not particularly critical. They were, perhaps, an attempt to navigate between the governor's request that the report be "fact-finding" rather than "fault-finding," and his executive order to the board, which did ask it to evaluate engineering practices relating to the damaged structures.[84] In keeping with these demands, the findings all related to Caltrans as a whole, and did not single out any particular individuals. Some findings, while apparently critical of Caltrans, were phrased in very neutral ways, for example: "Caltrans does not have a management-directed seismic safety performance goal that must be met by all its structures." There is no direct indication that Caltrans engineers might have been negligent in not having such a standard, and in fact the further text of the finding actually stops short of explicitly saying Caltrans *should* adopt such a goal — though this is strongly implied. This finding, like many others, is written in very impersonal language, further softening the impact of the language, as in: "the 17-year period to implement the modest-cost cable restrainer program after the 1971 San Fernando Earthquake suggests that seismic safety was not as pressing as other issues."[85]

Perhaps the most overtly critical finding was one stating that "most Caltrans concrete structures are of an age that they have nonductile detailing. Therefore, it should have been assumed by Caltrans that these were all at varying degrees of risk of failure in an earthquake." Again, however, the text of the finding does not make any suggestion about what Caltrans should have done with this knowledge.[86] Other findings directed blame away from Caltrans engineers. One stated that "historically, the fiscal environment at Caltrans has inhibited giving the level of attention to seismic problems they require."[87] Another praised Caltrans as having "the reputation of being the best transportation agency among the States and a leader in bridge design," citing its leading role in developing national seismic design codes for bridges.[88]

In contrast to media reports, which looked mainly for individual moral failures, the board sought to make a detailed assessment of Caltrans design processes and standards. This could have made the board more dangerous to Caltrans than the media, which in the end avoided in-depth analysis of Caltrans engineering practices in favor of broader political analyses. But by focusing on fairly abstract physical and organizational forces, making an effort to phrase its findings in very neutral terms, and balancing its criticisms with praise of Caltrans engineers, the board produced a final document that successfully dampened the controversy surrounding the collapse of freeway structures in the earthquake, rather than amplifying it. No wonder Jim Roberts' final assessment was that "the board of inquiry process saved Caltrans' neck as far as I'm concerned."[89]

Professional agendas

In addition to engaging with the substance of engineering practice, the hearings were turned into a forum for discussing issues of professional organization and power. As is often the case with peer review panels (see Chapter 4), different segments of the profession brought conflicting agendas to the hearings, or brought them to the attention of the board through letters and personal communications, and the board took on a mediating role. The most controversial of these professional issues was whether the earthquake indicated there was a need to raise the licensing standards for civil engineers, and bridge engineers in particular. The argument turned on a quirk in the way civil engineers and structural engineers are licensed. After gaining a certain number of years of experience after college — in the range of two to four years, depending on the state — an engineer who wants to continue in the civil area takes an exam to become a licensed civil engineer (CE). To become a licensed structural engineer (SE), one must practice as a licensed CE for a certain number of years and then pass the more-rigorous SE exam.[90] The catch is that the licensing rules are written in such a way as to effectively prevent anyone from taking the exam who has not had experience specifically in the area of *building* engineering, which is also the focus of the exam. Any civil engineer can legally design buildings, though an SE must be in charge of the design of hospitals and schools, and local governments may require an SE license for certain kinds of work.[91] But it is very difficult for a civil engineer who exclusively designs bridges, or tunnels, or anything other than buildings, to become a licensed structural engineer, no matter how much experience or expertise they may have.

This distinction is all the stranger since structural engineering is not specifically associated with buildings in engineering theory or education. All civil engineering students are trained in principles of structural engineering, and anyone designing a civil structure, whether building, bridge, or something else, draws on the same body of structural theory. Yet only building engineers can be specifically certified in the area of structural engineering. Though the original purpose of this system may have been to subject building engineers to a higher degree of scrutiny because they design complex, inhabited structures, it also gives building engineers access to a special status that is unavailable to other civil engineers.

Not surprisingly, this discrepancy causes some tensions between SEs and CEs, which entered into the inquiry process with the testimony of Albert Blaylock, president of the California Board of Registration for Professional Engineers and Land Surveyors, which is responsible for the licensing of engineers in the state. Blaylock reported to the Board of Inquiry on several proposals made by the Structural Engineers Technical Advisory Committee of the Board of Registration (SETAC). What really drew the ire of bridge engineers was this proposal: "there should be a separate license for bridge designers, beyond the level of the normal civil engineering registration."[92] Two civil engineering trade groups, the California Council of Civil Engineers and Land Surveyors and Professional Engineers in California Government, took strong exception to this idea in a letter to the Board of Inquiry and in testimony before the Board of Registration. This testimony, by Art McDaniel, a prominent bridge engineer, was also forwarded in written form to the Board of Inquiry.[93]

McDaniel, and probably many of his colleagues, saw the proposal to require additional licensing for bridge engineers as part of a larger pattern of disrespect and condescension toward bridge engineers on the part of structural engineers following the earthquake. He was particularly annoyed that these proposals had come out of meetings of a structural engineering group, which had made no effort to include any bridge engineers in the proceedings.[94] He also cited a number of offending statements by SETAC and Blaylock, including discussions held at one SETAC meeting on "deficiencies" in current bridge design practice; minutes from another meeting that referred to "changes that could be made to assure the public that people designing bridges are appropriately knowledgeable and technically up-to-date"; statements by SETAC members suggesting that civil engineers might lack "sufficient and adequate experience" and that Caltrans engineers, in particular, might not have "a reasonable level of competence"; and a remark attributed to Blaylock in his Board if Inquiry testimony: "There were some people in Caltrans who apparently had little respect for earthquake design. I think it should be pointed out that they didn't have quite the respect that the structural engineering profession in California had relative to buildings."[95]

To McDaniel, the proposed bridge engineering license, in the context of these remarks, was nothing more than a grab for professional power on the part of building engineers who believed the SE license should give them some special authority over all civil engineering matters:

> SETAC is on record as planning to recommend to you a separate license for bridge engineers. What form they have in mind is not clear. Mr. Blaylock has stated, and I quote, 'we would like to bring the engineers who design bridges into the fold of structural engineering.' I don't know what Al means by 'the fold of structural engineering.' Does he mean into the fold of building engineering? I suspect so. In any event, let's all be aware that building engineering represents a very small segment of the very large world of structural engineering.[96]

Bridge engineering, McDaniel argued, requires a broad set of skills that a building engineer would not necessarily possess simply by virtue of having an SE license. Still, bridge engineers are, "by the very nature of our work, structural engineers . . . without the California SE license."[97] But, he added,

> realize that a title or examination has little bearing on competence or what kind of work an engineer should practice in. A registered SE by virtue of his license can design the most critical buildings, including hi-rise buildings. By virtue of experience, only a relative few SEs should be designing such buildings. Experience, not examinations or titles, is the great qualifier.[98]

There were some more specific reasons given by McDaniel and his colleagues for rejecting the idea of a separate license for bridge engineers. The president of the Council of Civil Engineers and Land Surveyors argued that bridge design standards were improving steadily under the existing system, and that the public would not be served by implementing a new license because it would reduce competition in the area of bridge design.[99] Their strongest argument, though, was a plea for professional unity, as McDaniel wrote:

> If we create a 'Bridge Structural Engineer,' then maybe that will lead to a 'Hi-Rise Structural Engineer,' a 'Dam Structural Engineer,' a 'Tunnel Structural Engineer,' a 'Cable-Suspended Bridge Engineer' and who knows what else — the array of possibilities is only limited by our imagination and self-interest. In the process of proliferating practice acts and title acts, we can fragment the profession to death. No other profession, such as law or medicine, would ever consider crippling itself by such practices. We can easily see the dissension and confusion already created by the Structural Engineer title authority.[100]

Still, it seems likely that the idea of a bridge engineering license, if advanced in a different context, could have been seen as an opportunity to significantly enhance the prestige of the specialty. In fact, not all bridge engineers strongly objected to it: Caltrans engineering representatives Roberts and Gates testified to the Board of Inquiry that they would not necessarily be opposed to the creation of a new license.[101] But the board was apparently convinced by the arguments of McDaniel and the Council of Civil Engineers and Land Surveyors, dismissing the SETAC recommendation with the caution that "the public would be ill-served by creating a proliferation of specialized structural engineering licenses."[102]

Though the proposal for a new bridge engineering license generated the most heated rhetoric, another, more innovative reform in the regulation of engineering practice took on much greater prominence and became one of the most significant results of the inquiry. This was the idea of engineering peer review. In 1990, peer review was just beginning to catch on in a major way within the civil engineering profession, as described in detail in Chapter 4. Generally, engineering peer review takes the form of either project peer review, in which outside engineers reviewed the plans for a particular structure, or organizational peer review, in which an outside panel evaluates engineering processes and standards within an entire firm or agency.[103] The board called representatives of a number of structural engineering and seismic safety organizations to testify, many of whom strongly urged that Caltrans be made to use peer review on its projects; none of them argued against it. Among those in favor of peer review were L. Thomas Tobin, the head of the California Seismic Safety Commission, a state group set up to promote seismic safety policy; Albert Blaylock of the state Board of Registration; and a representative of the Structural Engineers Association of California. In some cases, the board seems to have called people to testify specifically because of their prior experience with review panels, including the Engineering Criteria Review Board of the San Francisco Bay Conservation and Development Commission, which regulates building around the bay, and the California Division of Safety of Dams, which has one of the longest-standing independent review policies in state government. Caltrans had also already made limited use of peer review on one elevated freeway structure being built in the San

Francisco area, and one of the reviewers, Nigel Priestley of U.C. San Diego, was asked to talk about how it worked.[104]

Those recommending peer review were mainly from groups dominated by structural engineers, and this again made some bridge engineers feel they were being treated in a condescending manner by the structural engineering community. Art McDaniel, in his written testimony sent to the Board of Inquiry, did not express outright opposition to the proposal for independent review, but noted that building design was as much in need of outside oversight as bridge design and questioned why structural engineers were focusing on bridge engineering rather than taking care of problems in their own area of expertise.[105] Professional Engineers in California Government was less sanguine. In a position paper dated March 9, 1990, they accused structural engineers of "slander[ing] the outstanding efforts and expertise of the public and private sector teams which plan, design and build California's freeway and highway structures," and went on to ask:

> Does a need exist to have 'independent consultants' review Caltrans' bridge design criteria and the design of major structures? The <u>criteria</u> are based on <u>standards</u> which are developed through the joint efforts of the public and private sectors. No one has suggested that Caltrans bridges were not designed in conformance with those standards. It is highly questionable if the Board should formally involve itself in the internal procedures of an agency unless it has evidence that the agency is violating laws or regulations.[106]

But by March 2, three months into the hearings, the board had already released its preliminary report, which strongly recommended that Caltrans engage peer review panels both to review its design practices generally and to oversee specific major design projects. Caltrans apparently knew in advance what the recommendations would be, and announced during the hearing on March 1 that they would be setting up peer review panels. By the end of March, a peer review panel was up and running to supervise the retrofit of freeway viaducts in San Francisco.[107] After the board put out its final report in June, Caltrans appointed a permanent Seismic Advisory Board to oversee its seismic design practices, headed by Housner and consisting largely of former members of the Board of Inquiry.

In contrast to the engineering employees represented by PECG, Caltrans management, including engineering managers, enthusiastically embraced the concept of peer review as a way of insulating the organization from future criticism. Jim Roberts felt that peer review would be helpful "because of the attention that we were getting as a result of the Cypress Viaduct damage . . . we needed . . . a security blanket, like building people have building officials that review their plans . . . you're less vulnerable to criticism because you've got all these experts."[108] Though now required to make use of peer review by an executive order of the governor, Caltrans chooses the panel members and has considerable discretion in the implementation of their recommendations.[109]

Recommendations of the board

The Board of Inquiry ended up making eight general recommendations consisting of 19 specific points. One of their main recommendations, of course, was that Caltrans and other transportation agencies in the state — for example, local commuter rail districts and port authorities — should make use of outside review panels. Other recurring themes included the need to comprehensively analyze all transportation structures for earthquake safety using the latest methods, and the need for Caltrans and other agencies to intiate "vigorous program[s] of professional development in earthquake engineering disciplines." They also heavily emphasized the importance of research, suggesting that Caltrans "fund a continuing program of basic and problem-focused research on earthquake engineering issues," and that the governor should should create and fund, at a statewide level, "a vigorous, comprehensive program of research to improve the capability in engineering and the physical and social sciences necessary to mitigate earthquake hazards and to implement the technology transfer and professional development necessary to hasten practical use of research results." Finally, the board indicated that the governor should set policies based on its recommendations and direct the Seismic Safety Commission to report to him and the legislature on the implementation of these policies by state agencies.[110] Not suprisingly, given that the board was composed of highly-trained professionals, the recommendations seemed to take a somewhat technocratic approach, focusing on the development and transfer of technical knowledge and the application of engineering expertise to the earthquake problem.

3.4 Experts and the State

Since the 1970s, government at both the federal and state level has increasingly turned to panels of expert advisors to resolve complex political issues that have a technical component, most notably in the area of environmental protection.[111] Scientists and professionals have generally been only too happy to contribute their time and knowledge to such efforts. It has become more and more natural for government to turn to expert panels for oversight of agency programs and procedures and for inquiries into potentially controversial accidents and disasters.

The success of this model for the relationship between experts and the state suggests that both sides feel they have something to gain from it. And indeed, advisory panels do seem provide opportunities for professions (including the scientific profession) to expand the scope of their power, while at the same time providing government with a way to resolve technical/political problems without divisive public debate. The Governor's Board of Inquiry on the 1989 Loma Prieta Earthquake, convened in the wake of a disaster with serious political fallout, and composed entirely of technical experts, principally from the engineering profession, fits the model well.[112]

Expanding professional jurisdiction

The Governor's Board of Inquiry, like other advisory panels, granted a profession control over judgments about the competence or culpability of its members, even though those judgments were related to actions which had a significant impact on public well-being. Being put in such a position is appealing to a profession for two reasons. First, it allows the profession to circumvent possibly contentious public hearings which might call its credibility into question by exposing and possibly exaggerating the misconduct of some of its members. Instead, dialog can take place in more collegial terms, and any changes that may be necessary to prevent future embarrassment to the profession can, more often than not, be worked out through consensus and without a great deal of public scrutiny.[113] The Board of Inquiry, for example, addressed deficiencies in Caltrans seismic practices not with punitive measures, but by putting the agency under more direct supervision from the rest of the profession through the use of peer review panels. By muting its criticisms, the board was able to gain the cooperation of Caltrans engineers and produce a very thorough, extensively documented report that was largely factual, reinforcing the technical expertise and disinterested image of the engineering profession.

Second, because advisory panels provide an officially-sanctioned forum for discussing professional issues and making policy recommendations, they can provide platforms for professions to assert new jurisdictional claims.[114] The Board of Inquiry, for example, used its authority to further promote the use of engineering advisory panels in state government, giving the profession as a whole greater control over decisions about public infrastructure. It also sought greatly expanded state funding for civil engineering research, specifically in the area of earthquake engineering, arguing that this was necessary to promote the overall state goal of earthquake safety. The turn toward expert advisory panels at all levels of government similarly represents a progressive extension of the professional jurisdiction of scientists and other professionals to include what had traditionally been considered policy decisions.[115]

But it is not as though advisory panels always provide a forum for the celebration of professional unity. In fact, along with providing the profession as a whole with a platform to advance its agendas, they may give particular segments within a profession an opportunity to bring up their own potentially divisive agendas.[116] The harmony of the Board of Inquiry was briefly threatened when structural engineering groups began to question the competence of bridge engineers and suggest that new licensing requirements should be imposed. The board brushed off their suggestions with an argument against further fragmentation of the profession. It is not hard to imagine that this sort of issue might have caused significant divisions if the board had been composed somewhat differently, which might have made the inquiry process more contentious.

In some ways, however, the board itself promoted the agenda of a particular segment of the engineering profession. Of the nine active members of the board (excluding the *ex officio* members), seven were engineers; one was a practicing structural engineer while the other six were current or former university researchers in various areas of earthquake engineering. Perhaps because of this research background, the board's report clearly emphasized the development of basic knowledge and the sharing of expertise between the research community and the design community as solutions to the problems they observed. Furthermore, new resources were to be directed particularly in the exact professional specialty, earthquake engineering, in which most of the board worked. These are arguably worthwhile responses to the problems at hand, but it is worth thinking about what recommendations might have come out of a board composed entirely of practicing bridge engineers.

The board encouraged peer review while dismissing licensing perhaps in part because peer review seems to promote greater integration within the profession, while new licenses might further fragment it. But engineering peer review panels are generally dominated by university researchers and principals in structural engineering firms, who are viewed as top experts in the field. Such panels help unify the profession in part by placing one elite group of engineers in a position to pass judgment upon their lesser-known colleagues. The groups being reviewed generally do not object because the process gives them access to the extra credibility carried by these outside experts. In the federal regulatory context, expert advisory panels may play a similar role, keeping agency scientists in line with the views of the research community while giving them access to a new reservoir of credibility. To the extent that researchers may be more competent and disinterested than agency scientists, this may be a good thing, but researchers may bring their own agendas to bear in the process.

Capturing the voice of expertise

Politicians and government officials often turn policy decisions over to experts for the same reason that professionals agree to take on these decisions: to preserve their credibility by removing controversial issues from the arena of open public debate. This allows them to avoid making choices between alternative courses of action that each may unacceptable to certain segments of the population.[117] Appointing an expert Board of Inquiry to investigate damage to transportation structures in the earthquake helped Governor Deukmejian avoid choosing between two equally unpalatable alternatives: condemning Caltrans, a government agency, which could backfire by shifting the blame back to him as the chief executive; or defending Caltrans, which might create the impression he was trying to cover up wrongdoing in an agency under his administration.

Government officials know that professional advisory panels can be counted on to resolve problems by negotiating to consensus, both because of the norms of professional interaction and because professions generally have their own stake in limiting public controversy over the conduct of their members.[118] Issues can legitimately be removed from public debate in this way to the extent that the actions of technical experts are presumed to be constrained by objective reality. This makes their decisions, at least in theory, accountable to some publicly accessible standard.[119] But the esoteric technical nature of the discussions held among experts makes it difficult for the public to participate even if the proceedings are open.[120]

Even as they delegate certain kinds of decisions to expert advisors, political leaders have generally managed to prevent them from gaining a great deal of independent political power. The government controls the arenas in which expert advice is given, and these arenas are usually at least partially hidden from public view. Particularly where advice is given only verbally, this allows the government to co-opt the "voice of science" for its own purposes, selecting which bits of advice to use and which to keep silent about.[121] The privacy of the environments in which technical advice is given is troubling because it encourages expert advisors to orient their actions toward their professional colleagues rather than toward a wider public discourse.[122]

Even where advice is given in written form, as in the report of the Board of Inquiry, officials can usually rely on it not being widely attended to by the general public. When the board's report came out, for example, stories in the *San Francisco Chronicle* and *Los Angeles Times* focused almost entirely on the findings of the report, particularly as they related to possible Caltrans responsibility for earthquake damage; they largely ignored the board's policy recommendations, such as the call for increased research funding and the use of peer review.[123] The governor used his acceptance of the final report as a platform to push voters to approve a ballot proposition to fund a comprehensive seismic retrofit program, not to push for research funding and peer review.[124] In his executive order implementing the recommendations of the board, Deukmejian followed the board's advice to fund a retrofit program and require peer review for state-owned structures, but only directed Caltrans to fund earthquake engineering research, rather than calling for the additional state-funded research program on earthquake hazards suggested by the board. The executive order also did not mention the comprehensive seismic instrumentation program that the board had recommended.[125] So, while he implemented most of the board's core recommendations, the governor ignored some of their advice and played up other elements of the report to advance his own agenda.

The main thing experts are given in return for advising the State is not, then, any great political power, but rather the promise of professional autonomy — the power to control their own affairs.[126] At the broadest level, this is given in the form of State-funded, peer-controlled organizations like the National Science Foundation or the National Institutes of Health, or by granting quasi-legal authority to professional bodies, particularly in the areas of licensure and professional discipline. But advisory proceedings themselves can be used as arenas for professional reform and the consolidation of professional power. The Board of Inquiry proceedings, for example, were used to secure a long-term advisory relationship between the civil engineering profession and the state in the form of peer review of Caltrans engineering practices. Within this more restricted professional arena, representatives of the larger civil engineering community were given a great deal of influence over the agency's operations. This enhanced the integration and autonomy of the profession across organizational boundaries, but did not do much to promote the power of the profession to shape state policy at a higher level. Thus, the state was able to draw on the advice of engineers while successfully keeping them in their professional place.

3.5 The contradictions of professional power

One of the main sources of professional power to shape public debate and political action is the simple fact that professions have "ownership" of certain crucial problems before they become public.[127] This was particularly clear in the media accounts described in the first part of this chapter. They showed that there is an extended community of earthquake-engineering experts in California, spanning across universities, private companies, and government agencies. These experts share a common body of knowledge: a common set of tools for analyzing and understanding engineering problems, but also a common familiarity with the history and the institutional contexts of earthquake engineering efforts. Besides agreeing on the immediate physical causes of structural failures in the Loma Prieta earthquake, they told a common story to the media about the known deficiencies of existing structures and the lack of political will or funding to support earthquake-safety measures.

When reporters sought to piece together a “moral disorder” story at Caltrans, nearly every engineer they talked to told this other story instead. Under prevailing journalistic conventions, journalists have to find authoritative sources to sustain a story. But the authoritative sources in the engineering profession had already come to a consensus about the nature of the problem and the preferred solution, which was to put more resources into retrofitting. News stories usually report what the experts say without too much analysis, giving the appearance of objective analysis by balancing opposing expert views against one another. But when the relevant profession has reached such an overwhelming consensus about a problem even before it becomes a public issue, which is often the case, reporters are faced with a “take it or leave it” choice — either accept the expert view as it is or question the very basis of professional authority.[128] They usually do not choose the latter. Politicians and the general public were faced with a similar situation, and generally made the same choice. Therefore, earthquake engineering specialists were able to play a decisive role in narrowing the range of alternatives that ever saw public debate, essentially leaving a choice between speeding up the existing retrofit program or doing nothing.

It would seem that professions ought to be able to gain even more power to shape public and political opinion when they are given formal authority to settle certain issues through an advisory panel or inquiry board. But this is not always so. In fact, once the earthquake engineering community began to focus its energies on the Board of Inquiry, it seemed to have less impact in the broader public arena. The professional nature of the hearings meant that they often took place in technical language that would be difficult for the general public to follow, and ensured that the media would see them as being of limited interest to the average reader. The board’s activities became publicly visible again at the end of the process, but at this point the governor got involved again and was able to play a major role in interpreting the results of the inquiry for the media and the public. The profession did gain some power, but mainly over its own internal affairs. In this case, being asked to provide formal advice to the government seemed, to a significant degree, to divert the engineering profession from the public arena where it had actually been quite effective in shaping opinion. Advisory panels in general may have this effect, giving experts greater control over particular professional domains while further distancing them from broader public discourse.

The figures in this chapter are from the EQIIS image database of the Earthquake Engineering Research Center, University of California, Berkeley; the center permits them to be freely reproduced.

Notes

[1] Governor's Board of Inquiry 1990, 19.

[2] Ibid., 89.

[3] Ibid., 27.

[4] On media frames, see Tuchman 1978 and Gitlin 1980; Gamson and Modigliani 1989 uses the similar concept of "media packages."

[5] Sigal 1986, 16.

[6] Sigal 1986, 15-16; Nelkin 1995, 60.

[7] Gans 1979, 144; Tuchman 1978, 69.

[8] Sigal 1986, 20; Gans 1979, 145.

[9] See Stallings 1990, 84.

[10] Michael Taylor and Bill Wallace, "Design Blamed in Collapse of I-880; Reinforcing Plan Was Planned," *San Francisco Chronicle*, October 19, 1989, A2 (NEXIS).

[11] Frank Clifford, "Experts Say Danger To Structures Was Known," *Los Angeles Times*, October 19, 1989, A8 (NEXIS).

[12] Greg Johnson, "UCSD Engineer Blames Cracked Joints for Nimitz Freeway Collapse," *Los Angeles Times*, San Diego County Edition, October 20, 1989, B4 (NEXIS).

[13] Michael Taylor, "UC Engineers Discuss I-880, Bridge Failures," *San Francisco Chronicle*, October 26, 1989, A10 (NEXIS).

[14] Thomas H. Maugh II, "Joints Blamed in Nimitz Collapse," *Los Angeles Times*, October 26, 1989, A1 (NEXIS).

[15] Michael Taylor and Bill Wallace, "Probe of I-880 Failure Narrows to 3 Theories About Pillars," *San Francisco Chronicle*, October 27, 1989, A12 (NEXIS).

[16] Stallings 1990, 90; Gans 1979, 8.

[17] Gans 1979, 56.

[18] Interview, Jim Roberts, August 25, 1997; Interview, Jim Gates, August 28, 1997.

[19] Quoted in Smith 1992, 131.

[20] Ibid., 132.

[21] Ibid., 132.

[22] Maura Dolan and Sheryl Stolberg, "Bay Area Quake: Fallen Monument to Civic Pride," *Los Angeles Times*, October 22, 1989, A3 (NEXIS).

[23] John Hurst and Daniel M. Weintraub, "Official Saw No Need To Bolster Bridges in '85," *Los Angeles Times*, October 25, 1989, A1 (NEXIS).

[24] Michael Taylor and Bill Wallace, "Design Blamed in Collapse of I-880; Reinforcing Plan Was Planned," *San Francisco Chronicle*, October 19, 1989, A2 (NEXIS).

[25] Frank Clifford and William Trombley, "Double-Deck Road Safety Stirs Debate," *Los Angeles Times*, October 19, 1989, A1 (NEXIS).

[26] John Hurst and Daniel M. Weintraub, "Freeway's Fall Possibly Avoidable, Expert Says," *Los Angeles Times*, October 21, 1989, A1 (NEXIS).

[27] John Hurst and Richard C. Paddock, "Caltrans Was Warned on Vulnerable Design," *Los Angeles Times*, October 28, 1989, A1 (NEXIS).

[28] David Dietz, "'Please God, Let That Freeway Hold,'" *San Francisco Chronicle*, October 18, 1989, A4 (NEXIS).

[29] Katz did, however, go on to mention that more recently constructed structures are less vulnerable than older ones. Michael Taylor and Bill Wallace, "Design Blamed in Collapse of I-880; Reinforcing Plan Was Planned," *San Francisco Chronicle*, October 19, 1989, A2 (NEXIS).

[30] John Hurst and Daniel M. Weintraub, "Freeway's Fall Possibly Avoidable, Expert Says," *Los Angeles Times*, October 21, 1989, A1 (NEXIS).

[31] Frank Clifford, "Experts Say Danger To Structures Was Known," *Los Angeles Times*,

October 19, 1989, A8 (NEXIS).

[32] David Dietz, "Bay Bridge To Be Closed At Least Several Weeks," *San Francisco Chornicle*, October 19, 1989, A2 (NEXIS).

[33] David G. Savage and Ronald B. Taylor, "The Bay Area Quake: What Next?" *Los Angeles Times*, October 20, 1989, Q12 (NEXIS).

[34] George Skelton and Daniel M. Weintraub, "OKd All Funds for Safe Roads, Governor Says," *Los Angeles Times*, October 23, 1989, A1 (NEXIS).

[35] Virginia Ellis and Frank Clifford, "2 L.A. Bridges To Be Strengthened," *Los Angeles Times*, October 21, 1989, A24 (NEXIS).

[36] Clarence Johnson, "Freeway Designers Blasted; Maher Red-Tags Embarcadero," *San Francisco Chronicle*, November 10, 1989, A2 (NEXIS).

[37] Bill Wallace and Michael Taylor, "State Was Slow To Reinforce The I-880 Pillars, Experts Say," *San Francisco Chronicle*, October 20, 1989, A (NEXIS).

[38] John Hurst and Daniel M. Weintraub, "Freeway's Fall Possibly Avoidable, Expert Says," *Los Angeles Times*, October 21, 1989, A1 (NEXIS).

[39] Pete Carey, "Columns Unsafe for Lack of Funds," *San Jose Mercury News*, October 22, 1989, GBI files.

[40] George Skelton and Daniel M. Weintraub, "OKd All Funds for Safe Roads, Governor Says," *Los Angeles Times*, October 23, 1989, A1 (NEXIS).

[41] Paul Jacobs and Douglas P. Shuit, "Bay Area Quake: Freeway Seismic Safety Not 'Highest Priority,'" *Los Angeles Times*, October 26, 1989, A3 (NEXIS).

[42] Ibid.

[43] Thomas H. Maugh II, "Joints Blamed in Nimitz Collapse," *Los Angeles Times*, October 26, 1989, A1 (NEXIS). This is an argument that is repeated frequently within the earthquake engineering community and is the basis of research into retrofit prioritization methods.

[44] Daniel M. Weintraub and Virginia Ellis, "Column One: Caltrans' Hard Jolt of Reality," *Los Angeles Times*, December 3, 1989, A1 (NEXIS).

[45] Greg Lucas, "Caltrans — A Troubled Agency On The Spot," *San Francisco Chronicle*, January 15, 1990, A1 (NEXIS).

[46] Tracie L. Thompson, "Caltrans Backs Prop 111 for Seismic Retrofitting," *San Francisco Chronicle*, April 20, 1990, A2 (NEXIS); Steve Massey, "Quake Report Has Bad News for Caltrans," *San Francisco Chronicle*, June 1, 1990, A1 (NEXIS).

[47] Interview, Jim Roberts, August 25, 1997.

[48] Letter, Craig Turner to Robert K. Best, October 19, 1989, EF 19.

[49] Letter, Joseph A. Montoya to Craig Turner, October 20, 1989, EF 19.

[50] Interview, Jim Gates, August 28, 1997. Gates may be referring to Virginia Ellis, the only female Sacramento-based correspondent to write articles on the earthquake for the *Los Angeles Times*.

[51] Interview, Jim Roberts, August 25, 1997.

[52] Memorandum, W. E. Schaefer to All Engineering Employees, October 27, 1989, EF 9.

[53] Memorandum, Robert K. Best to Deputy Directors, District Directors, Division Chiefs, "Subject: Press Contacts Regarding Earthquake Recovery Program," November 7, 1989, EF 9.

[54] A collection of viewgraphs by Dick Stuart, President, Cygna Group, Ron Polivka, Vice President, and Polly Quick, Vice President, ICF/Kaiser, "Recommendations for Caltrans' Public Awareness Program, Presented to: Jim Roberts/Jim Drago, Caltrans," December 15, 1989, EF 55.

[55] See Gans 1979, 9-13 and Molotch and Lester 1974, 107.

[56] Bill Wallace and Michael Taylor, "State Was Slow to Reinforce the I-880 Pillars, Experts Say," *San Francisco Chronicle*, October 20, 1989, A (NEXIS); Daniel M. Weintraub, "Bay Area Quake: Inspectors Had Rated Nimitz Sound," *Los Angeles Times*, October 20, 1989, A5 (NEXIS).

[57] See Chapter 4 for an extended discussion of the association between objectivity and disinterestedness in the context of engineering peer review.

[58] George Skelton and Daniel M. Weintraub, "OKd All Funds for Safe Roads, Governor Says," *Los Angeles Times*, October 23, 1989, A1 (NEXIS).

[59] Ian G. Buckle, "No Easy Solutions on Spans, Bridges," *Los Angeles Times*, October 19, 1989, B7 (NEXIS).

[60] Daniel M. Weintraub, "Bay Area Quake: Inspectors Had Rated Nimitz Sound," *Los Angeles Times*, October 20, 1989, A5 (NEXIS).

[61] George Skelton and Daniel M. Weintraub, "OKd All Funds for Safe Roads, Governor Says," *Los Angeles Times*, October 23, 1989, A1 (NEXIS).

[62] Michael Taylor and Bill Wallace, "Probe of I-880 Failure Narrows to 3 Theories About Pillars," *San Francisco Chronicle*, October 27, 1989, A12 (NEXIS).

[63] John Hurst and Daniel M. Weintraub, "Bay Area Quake: Strengthening Nimitz May Have Led To Its Fall," *Los Angeles Times*, October 27, 1989, A3 (NEXIS).

[64] Quotation from Governor's Board of Inquiry 1990, 13-16.

[65] George Housner described the process in an interview, April 16, 1998; see also Housner 1997, 175-176.

[66] Governor's Board of Inquiry 1990, 16.

[67] Housner 1997, 175.

[68] Reproduced in Governor's Board of Inquiry 1990, 14-15.

[69] Hearing minutes, GBI files. The minutes are more like detailed technical notes, providing the names of speakers and summaries of their questions and answers. In a later oral history, Housner put the number of people who testified at 72, and the board's report says 70 people testified, but only 69 are listed by name in the report. See Housner 1997, 176 and Governor's Board of Inquiry 1990, 17, 229-234.

[70] Hearing minutes, GBI files.

[71] Housner 1997, 174.

[72] Ibid., 176.

[73] Ibid., 177.

[74] Ibid., 174.

[75] Ibid., 176.

[76] Interview, Jim Roberts, August 25, 1997.

[77] Interview, Jim Gates, August 28, 1997.

[78] Ibid.

[79] Interview, Jim Roberts, August 25, 1997.

[80] Hearing minutes, December 13-14, 1989, page 11.

[81] Various hearing minutes, GBI files.

[82] Governor's Board of Inquiry 1990.

[83] Governor's Board of Inquiry 1990, Finding 3, 45.

[84] Executive Order D-83-89, reproduced in Governor's Board of Inquiry 1990, 14-15.

[85] Quotes from Governor's Board of Inquiry 1990, Finding 13, 51.

[86] Ibid., Finding 18, 53-54.

[87] Ibid., Finding 8, 48.

[88] Ibid., Finding 9, 48.

[89] Interview, Jim Roberts, August 25, 1997.

[90] See the Professional Engineers Act, California Business and Professions Code Sections 6700-6799, Chapter 7: Professional Engineers, especially sections 6731, 6751, 6752; see also the Rules of the Board for Professional Engineers and Land Surveyors, California Code of Regulations Sections 400-474.5, Title 16, Chapter 5, especially sections 404 (d)-(f), 426.10-426.14, 427.30. Both available (April 11, 2000) on the web site of the California Board for Professional Engineers and Land Surveyors at http://www.dca.ca.gov/pels/e_exam.htm.

[91] See California State Board for Professional Engineers and Land Surveyors, "Plain Language Pamphlet of the Professional Engineers Act and the Board Rules," available (April 11, 2000) on the board's web page at http://www.dca.ca.gov/pels/e_plppe.htm.

[92] See Agenda, Structural Engineers Technical Advisory Council (SETAC), February 23, 1990, GBI VAR076.

[93] Letter, Timothy G. Psomas, President, California Council of Civil Engineers and Land Surveyors to Dr. George W. Housner, April 10, 1990; Art McDaniel, testimony before the California Board of Registration for Professional Engineers and Land Surveyros, March 9, 1990. Both GBI VAR076. Note that McDaniel is the president of McDaniel Engineering, one of the lead consultants in the design of the San Diego-Coronado Bay Bridge retrofit described in Chapter 7.

[94] McDaniel testimony before the Board of Registration, 1.

[95] Ibid., 5.

[96] Ibid., 6-7.

[97] Ibid., 6.

[98] Ibid., 11.

[99] Letter, Timothy G. Psomas to George Housner, April 10, 1990, GBI VAR076.

[100] McDaniel testimony before the Board of Registration, 12.

[101] Hearing minutes, March 1-2, 1990, 6, GBI files.

[102] Finding 49, Governor's Board of Inquiry 1990, 71.

[103] See ASCE 1990, 95.

[104] Hearing minutes, various locations, GBI files.

[105] McDaniel testimony before the Board of Registration, 8-10.

[106] "PECG Position Paper Regarding SETAC Recommendations to the Board of Registration for Professional Engineers and Land Surveyors," GBI VAR076.

[107] "Peer Review Panel Summary Report for Evaluation and Strengthening of San Francisco Double Deck Freeway Viaducts," December 31, 1992, EF 28.

[108] Interview, Jim Roberts, August 25, 1997.

[109] See California Executive Order D-86-90, GBI files.

[110] Governor's Board of Inquiry 1990, 9-11.

[111] See Jasanoff 1990, 229-250, for a review of the political use of expert advice.

[112] Carl-Henry Geschwind notes that earthquake hazards have never been subject to the kind of adversarial politics that characterizes many environmental issues in any case. See Geschwind, forthcoming.

[113] For discussion of the negative effects of adversarial proceedings on scientific credibility and the benefits of peer review in this respect, see Jasanoff 1990, especially 234, 246-247.

[114] On the importance of public and legal arenas for the settlement of jurisdictional claims, see Abbott 1988, 59-69, 157-167. On the importance of state sanction to professional domination more generally, see Berlant 1975 and Larson 1984, 34-35.

[115] See Jasanoff 1985, 24

[116] On the internal segmentation of professions and the competition between segments, see Bucher and Strauss 1961. On the relevance of the internal social structure of professions to the success of jurisdictional claims, see Abbott 1988, 79-85.

[117] See Larson 1984, 63-64.

[118] On the significance of advisory panels as negotiating arenas, see Jasanoff 1990, 234-236.

[119] This way of justifying turning political decisions over to technical experts is what Yaron Ezrahi identifies as the centralized variant of liberal-democratic instrumentalism. In contrast to the decentralized variant, which presumes that common reference to objective reality provides a rational basis for public debate, this version justifies removing the public from the process on the grounds that the experts, by virtue of being constrained by objective reality, are in effect representing the public interest. See Ezrahi 1990, 19-66.

[120] See Larson 1984, 64.

[121] Mukerji 1989, 190-203.

[122] Habermas 1970, 78-80; on the increasing "privatization" of science in the American political and cultural context — its increasing "disengagement from its earlier role in directly upholding public values," see Ezrahi 1990, 239-290, quotation at 248.

[123] Steve Massey, "Quake Report Has Bad News For Caltrans," *San Francisco Chronicle*, June 1, 1990, A1 (NEXIS); Daniel M. Weintraub, "Nimitz Collapse Forseeable, Report

Says," *Los Angeles Times*, June 1, 1990, A1 (NEXIS).

[124] Steve Massey, "Quake Report Has Bad News For Caltrans," *San Francisco Chronicle*, June 1, 1990, A1 (NEXIS).

[125] California Executive Order D-86-90, GBI files.

[126] See Mukerji 1989, 197.

[127] See Gusfield 1981, 10-15 and Abbott 1988, 59-85.

[128] This is similar to the "take it or leave it choice" the public faces when confronted with a "technological frame" — engineers and other technological insiders are able to make decisions about what form the fluorescent light bulb will take, for example, but the consumer practically has only two choices: buy in to the technology as it exists, or don't use it at all. See Bijker 1995, 284-285.

Chapter 4

Civil Engineers: Peer Review and Professionalism

4.1 Introduction

Peer review has long been a routine feature of scientific practice, where it has mainly been used by journals to determine what articles to publish, and by funding agencies to decide which research proposals to support. In the 1980s, federal regulatory agencies turned to peer review as a way of validating their decisions. As Sheila Jasanoff notes, however, peer review in this setting was quite different from journal or grants peer review. These forms of peer review are fairly impersonal — the review focuses on written documents, and the process often entails soliciting reports from referees by mail; it is up to the editor or program manager to sort out conflicting judgments. In regulatory peer review, panels of reviewers typically meet repeatedly in person, and these meetings are essentially a process of negotiation and consensus-building between different elements of the relevant scientific community, including both researchers and agency experts.[1] In the process of being transplanted into a science policy context, peer review was transformed to such an extent that it hardly appears to be the same institution.

Peer review in engineering has several characteristics which make it similar to regulatory peer review. It too is typically done through in-person meetings of panels of reviewers which operate on the basis of consensus. However, it also has some features which distinguish it from regulatory peer review. Instead of reviewing policy, most peer review panels are engaged to review the practices of a particular firm or the design approach of a particular engineering project. The aim of this chapter is to describe what is distinctive about engineering peer review and to see what it can tell us about the culture of engineering. Specifically, it focuses on discussions of peer review within the American Society of Civil Engineers (ASCE) and on the implementation of peer review at Caltrans. These two sources diverge a bit because peer review at Caltrans, a public agency, has some features that are found in regulatory peer review and not in engineering peer review as described by the ASCE.

Because peer review forces engineers to interact with one another in new ways, it generates certain tensions. Specifically, civil engineers recognize that peer review might be seen as calling a designer's competence into question or undermining their authority to make final design decisions. As a result, a great deal of emphasis is placed on the social skills and tact of peer reviewers. Mechanisms are also provided to ensure that peer reviewers have no conflicts of interest, and in general reviewers try to project an image of disinterestedness in their dealings with designers. These concerns are related to a larger conflict in the engineering profession between two distinct views of what constitutes professional autonomy. One ideal holds that engineers should be free to compete with one another for work on the open market. The other holds that engineers should be able to maintain a strong professional identity that frees them from the influence of particular clients or organizations. Peer review generates tensions because it forces designers to respond to the needs of their employers and the concerns of representatives of the profession at the same time. Since it takes place in face-to-face meetings, however, it also provides a forum in which to negotiate and work out some of these tensions.

Boundary work

Jasanoff argues that a key feature of regulatory peer review is its dependence on "boundary work," in which scientists involved in peer review try to maintain a clear demarcation between science and policy, claiming that their decisions are based only upon the former even if they do mark changes in policy.[2] This move seems to be crucial to maintaining the effectiveness and legitimacy of peer review. These same concerns come up in engineering peer review, particularly at Caltrans. However, there is another boundary that is also significant in engineering peer review, and that is the boundary between design work itself, which is supposed to be left to the project designers, and the review of this work, which is the legitimate role of the peer review panel. Although this boundary has significance mostly within the engineering profession, it generates the same kind of boundary work seen in policy settings.

Codes, community, and trust

Codes and other formal standards of practice are ubiquitous in the world of design engineering.[3] Such standards were an important element of professionalization in engineering, beginning in the 19th century.[4] One reason for this may have been that rules of practice are very useful tools for legitimating technologies, and by extension, engineering practices, in the public eye.[5] But Andrew Abbott makes the point that excessive routinization of practice can leave a profession vulnerable by making it possible for other groups to take over elements of its work.[6] Along similar lines, Theodore Porter has argued that the effort to replace "personal judgment" by "quantitative rules" in 19th-century French engineering was actually "a response to conditions of distrust attending the absence of a secure and autonomous community."[7] The shift toward formal standards of design practice may be linked to a general dissipation of close-knit professional communities within engineering.

Social theorists Anthony Giddens and Niklas Luhmann have noted that social life in the late modern period seems increasingly to depend upon both experts and laypersons putting trust in abstract systems rather than in familiar persons.[8] In assessing the probable competence of an engineer, for example, both laypersons and fellow professionals now often rely on credentials and the understanding that the engineer in question is following design codes. In less complex societies, such trust might be based much more on direct acquaintance with the practitioner and knowledge of his or her personal qualities. This shift is an aspect of what Giddens calls "disembedding," the restructuring of social life "across indefinite spans of time-space."[9] But even in the areas of society where abstract systems are most pervasive, Giddens and Luhmann argue that personal trust is still important.[10] One way it is commonly sustained is through particular social settings which "reembed" action in localized, personal interactions.[11]

In engineering, peer review is one such reembedding mechanism. It provides a means of regulating engineering practice that does not depend upon codes or other formal rules. It puts an emphasis on collegial interaction and on personal knowledge and expertise, rather than strict adherence to code, as an indicator of professional competence. As a result, discussions about peer review in engineering often focus on the personal qualities that a reviewer should possess, rather than on formal qualifications and rules of conduct. Because of these characteristics, peer review has the potential to strengthen the professional community in engineering, making it more integrated and enhancing its autonomy by demonstrating that good engineering practice cannot be reduced to a set of written regulations.

The ASCE and peer review

Peer review has been a topic of discussion within the ASCE since at least 1978, when the first of a number of articles on the subject appeared in the ASCE journal *Civil Engineering*.[12] In 1984, the ASCE sponsored a workshop to develop an "agenda for improving quality in planning, design and construction."[13] This workshop eventually led to the publication of a manual, *Quality in the Constructed Project*, which was published as a draft in 1988 and in a final version in 1990.[14] Elements of this manual were also incorporated into a joint publication of the ASCE and the American Consulting Engineers Council (ACEC), *Project Peer Review Guidelines*.[15]

The ASCE manual distinguishes between *organizational peer review* and *project peer review*. Organizational peer review is a kind of general audit of the "policies, procedures and practices" of a particular engineering firm, usually carried out at the request of the firm.[16] Project peer review focuses more narrowly on assessing the quality of the design of a particular structure. The ASCE and ACEC recommend that project peer review be done on projects involving issues of public health and safety or national defense, or where the design is unique or the design and construction schedule is unusually short.[17] Because peer review efforts at Caltrans include no clear-cut example of organizational peer review, I focus here primarily on project peer review, which seems to be a more common topic of discussion within the ASCE as well.

The ASCE recommendation for peer review is not without historical precedent. One effort that is mentioned in the ASCE manual is the organizational peer review program run by the Association of Soil and Foundation Engineers (ASFE), which started in 1978. This is a service provided on a voluntary basis to firms interested in improving their business and technical practices. It was started specifically in response to increasing liability problems faced by geotechnical engineering firms, which had reached the point that most firms could no longer afford professional liability insurance. The preliminary edition of the ASCE manual notes that six years after the initiation of the program, "the geotechnical engineering profession had achieved the lowest rate of liability claims among design disciplines" — a good argument for peer review as a quality-enhancing measure.[18] The manual cites similar organizational peer review programs run by the ACEC and by the Chicago chapter of the American Institute of Architects.[19]

The origins of project peer review can be traced back to a number of different sources. Organizations like the Army Corps of Engineers and the Nuclear Regulatory Commission have long maintained advisory boards that perform some peer review functions on specific projects.[20] The Urban Mass Transportation Administration has used peer review panels on certain projects it funds since 1979.[21] Going even further back, one author cites German "proof engineers" as a precedent for project peer review. Proof engineers are government-licensed engineering consultants who are paid a portion of the main consultant's fee to check and certify plans and construction procedures; this practice goes back as far as 1934. [22] Whatever precedents are cited, however, it seems likely that their significance took shape in light of the trend toward "quality"-oriented management and the increasing use of advisory panels by federal regulatory agencies in the 1980s.[23]

Peer review at Caltrans

At Caltrans, the extensive use of peer review began in the aftermath of the Loma Prieta earthquake, based on the recommendations of the Governor's Board of Inquiry — although Caltrans had tried peer review, in a fairly limited way, on one project in the year before that earthquake.[24] Peer review is now used in three distinct ways at Caltrans. First, there are numerous project peer review panels set up to oversee work on particular structures. Each of these panels has four or five members drawn from a larger pool of around 20 reviewers. Second, Caltrans maintains an eight-member permanent Seismic Advisory Board (SAB) to advise it on issues of "policy," although it usually sticks to fairly technical issues. The SAB plays a similar role to advisory panels at regulatory agencies, and in some ways engages in what the ASCE would call organizational peer review. Both the SAB and the project peer review panels are composed of practicing engineers as well as academic researchers.[25] Finally, Caltrans maintains a Seismic Research Advisory Committee to supervise their research program and to oversee the distribution of research funds between institutions. This committee, which is composed entirely of academic researchers, functions in much the same way as a grants peer review panel at NSF or NIH, reviewing research proposals and deciding which merit agency funding.[26] In this chapter, I focus largely on the first two types of peer review, since review of research proposals is not as unique to the engineering context.

4.2 The boundaries of peer review

As Jasanoff discovered in the case of federal agency peer review, the acceptability of a peer review panel's conclusions depends greatly upon its ability to maintain a sharp distinction between technical and policy concerns, and to position its work strictly on the technical side of the boundary, making it less vulnerable to criticism by non-experts.[27] This is not to say that particular issues naturally fall on one side or the other of this boundary, especially in the regulatory arena. The participants in peer review have to actively work to define the issues they are dealing with in technical terms if they want to maintain their credibility.

In engineering peer review, there are two boundaries that are particularly problematic. The first is the distinction between technical and policy issues, and is important mainly in public agency settings. The second seems to be more specific to the engineering context. This has to do with the demarcation between the legitimate *review* function of a panel and the actual work of design. For peer reviewers to cross this line is seen as an infringement on the professional autonomy of the design engineer. Of course, this boundary is not entirely self-evident either, and peer reviewers must work to ensure that their contributions are not seen as efforts to step into the role of the designer.

Setting ground rules

While these boundary-setting problems raise the most interesting issues, there are a number of much more mundane parameters that have to be established to get a peer review process up and running. The most significant of these has to do with the intensity of involvement of the peer review panel in the decisionmaking process. In practice, this often works out in terms of the number and length of panel meetings. Within the ASCE, there seems to be very little consensus on this, and the official position of the Society is that a number of different formats can work, depending upon the desired goals. The Society's guide *Quality in the Constructed Project* suggests that peer review might properly include just a single meeting to go over either final project plans or initial concepts, or a series of meetings throughout the life of the project.[28]

At Caltrans, the timing and extent of peer review panel activities has evolved over time. The only peer review effort initiated by Caltrans before the Loma Prieta earthquake involved just a single, one-day meeting and apparently did not result in a formal report by the panel.[29] A peer review panel convened rather hastily after the earthquake to review the design of a new elevated freeway structure in Los Angeles met more than once and produced a formal report, but the report is dated only three months after the initial meeting.[30] These were early exceptions, however. All of the peer review panels convened after the recommendations of the Governor's Board of Inquiry were given much broader scope, with regular meetings continuing throughout the design phase of a project, which might take many months. In many cases, peer review panels were charged with reviewing retrofit plans for groups of similar projects over an even longer time span, often several years. For example, single peer review panels covered San Francisco double-deck freeway structures and all of the state toll bridges. These later reviews commonly involved a great deal of back-and-forth, personal interaction between panel members, Caltrans engineers, and consulting engineers.

San Francisco viaducts peer review

The issues that peer review has raised at Caltrans show up particularly clearly in the activities of the first post-Board of Inquiry peer review panel, which looked at retrofit plans for the double-deck freeway structures in San Francisco. This panel was, at the time, referred to simply as the "Caltrans Peer Review Panel" or the "Seismic Safety Peer Review Panel," but is now known more specifically as the "San Francisco viaducts peer review panel." (The term "viaduct" is used by highway engineers to refer to a long structure which carries a freeway over city streets.) This group was first convened in March 1990 at the suggestion of the Board of Inquiry and finally produced a report more than two years later, in December 1992.[31] In the interim, the panel met many times — during some stages of the process, on a weekly basis.[32] The panel was made up of six regular members, all engineers from well-known San Francisco Bay Area consulting firms, and four "technical advisors" from the University of California: Professors Stephen Mahin and Jack Moehle from U.C. Berkeley, and Nigel Priestley and Frieder Seible from U.C. San Diego.[33]

By the time the panel was convened, design work on the six retrofits in question was already well under way. The original plan was ambitious: to complete design work within two months of the earthquake, by December 1989, and to complete all construction by December 1990.[34] It soon became apparent that this would not be possible, and Caltrans switched to a two-stage plan in which the consultants would come up with temporary retrofit plans to be implemented quickly, and then complete permanent retrofit designs by January 1990.[35]

By March 1990, when the peer review panel first met, temporary retrofit plans were complete, and design was well underway on the permanent retrofits. However, by

June the panel had concluded that the temporary retrofit schemes would not provide significant protection in the event of a future quake, and that the existing plans for a permanent solution would not meet Caltrans performance criteria.[36] Retrofit plans had called for the steel jacketing of the bridge columns, which would increase their capacity to resist earthquake forces, but panel members found that most of the consultants had not adequately analyzed the large stresses that could occur within the joints between the columns and the beams supporting the roadway, and had therefore failed to provide enough strength in this area. Furthermore, they found that steel jackets alone would not be sufficient to retrofit these joints.[37] This threw the retrofit process into turmoil. Caltrans decided to abandon plans for temporary retrofit and to start over with the permanent retrofit plans, and to keep the freeways closed in the mean time, a decision that "was not accepted kindly by the local press."[38] At this point, the peer review process became much more politically sensitive, and the panel also became more involved in the design process. These developments would increasingly challenge the prescribed boundaries of peer review.

Reviewing and designing

In a number of articles in *Civil Engineering*, the comments of engineers about their experiences with peer review panels suggest that one of the key tensions in project peer review is between the project designers and the reviewers. In part, this tension probably results from the fact that practicing engineers might easily find themselves in either role on a particular project. As a result, even strong advocates of peer review emphasize the need for panel members to exercise restraint and avoid the temptation to usurp the role of the designer — the "that's not the way I would have done it" syndrome, as one designer put it.[39] For example, an engineer describes a situation in which "The 'slight modifications' [a peer reviewer] had mentioned suddenly became an 'if you had hired me' situation," creating a situation in which he "ended up being embarrassed in front of my client."[40] One of the reasons peer review generates tension is because it subjects design engineers to a degree of professional scrutiny that has not been common in recent engineering practice, where designers are often directly responsible to clients or to the managers in the organizations where they work. This may create the impression that the designers subject to this scrutiny are somehow less than competent; indeed, the ASCE's *Quality in the Constructed Project* takes pains to point out that peer review is not "an indication that the owner, design professional, or constructor is incompetent or suspect in any way."[41]

These comments suggest that the boundary between simple review and actual participation in the design process is an extremely important and sensitive one for engineers, however difficult it might be to draw a formal demarcation between the two activities. The original duty outline for Caltrans' San Francisco viaducts peer review panel seemed to reflect this understanding, using the language that the panel should "review" various plans and calculations and "identify" potential problems.[42] In practice, however, the panel went well beyond this duty outline, participating in the design process in a way that expanded the boundaries of "review" far beyond what ASCE guidelines would suggest. The circumstances under which this happened and the people who were allowed to push this boundary provide important clues about the nature of the demarcation between review and design.

Once the peer review panel had rejected the initial retrofit strategies proposed by the consultants, Caltrans and the consultants were faced with a problem: they would essentially have to start designing again from scratch, and they were under considerable pressure to come up with a good retrofit plan quickly. In fact, in mid-July, several days before the panel formally notified Caltrans that the existing plans were unacceptable, one of the academic "technical advisors" on the panel, Nigel Priestley of U.C. San Diego, had proposed an alternative approach in which new beams would be added parallel to the edges of the roadway, linking adjacent columns to one another. Since the major joints were located at the level of the roadway, these new beams would take some of the stress off of the joints and stiffen the structure in the longitudinal direction. The new beams would be precast and attached to the existing structure by reinforcing bar extending through the joints.[43] At some point, one or more of the professors from U.C. Berkeley proposed an alternative version of this in which the new beams would be built as an integral part of the existing structure.[44] These "edge beam" concepts became the new focus of the retrofit effort.

In its July letter informing Caltrans of the inadequacy of the existing retrofit schemes, the peer review panel suggested that Caltrans should take the initiative and "develop workable concepts of typical retrofit solutions in-house," in consultation with the academic members of the panel, and then take this standardized approach to the consultants for implementation, after getting the approval of the panel.[45] Caltrans responded with a slightly different arrangement. Perhaps not wanting to take too much control away from the consultants, they organized a working group including not only the four technical advisors and Caltrans engineers, but also personnel from each of the consulting firms.[46] This group had five weekly meetings through August and early September.[47] Meeting minutes suggest that there was a very active interchange of ideas between the academics and the consultants. The consultants introduced a number of different ideas for dealing with the longitudinal stiffness problem which do not appear to have been straightforward adaptations of the edge beam concept. The group managed to reach consensus on many issues, in particular that the existing columns and joints would have to be completely replaced, whatever other retrofit measures were taken, but no agreement was reached about which approach should be used to improve longitudinal stiffness.[48] Later, though, after consulting with the entire peer review panel, Caltrans chose the professors' edge beam approach over the others developed by the consultants.[49]

After this decision was made, the professors from Berkeley and San Diego were given contracts to carry out laboratory testing on their differing versions of the edge beam concept. The consultants worked with them to design the test models.[50] The central role that the academics played, or at least saw themselves as playing, in this process is underscored by an incident reported in a rather exasperated memo from Ray Zelinski — the Caltrans engineer who was managing the retrofits — to his boss, Jim Roberts. The two Berkeley researchers had misgivings about a proposed joint configuration they were supposed to test, which had been designed by one of the consultants. Their concerns were made known to the consulting firm, which revised the design. When the revised plans were sent out to the panel members, and no complaints were received, Zelinski took it that the issue had been resolved and told the researchers to go ahead with the test, even though they still had misgivings, since delaying the test would further delay the beginning of construction on the projects. Nevertheless, the Berkeley professors unilaterally suspended construction of the test specimen, delaying the test, because they still did not feel the design was correct.[51]

In this case, then, peer reviewers were clearly seen as playing an important role in the design process. Instead of deferring to the designers, they put out design ideas which, it seems, had more of a determining influence on the final plans than those proposed by the consultants. Furthermore, this active role seems to have been taken by mutual consent, at least between the panel and Caltrans. While the Berkeley professors later crossed a line which provoked some irritation, most of the design activities of the reviewers drew little complaint. This may indicate that the line between design and review, while drawn very sharply by some individual engineers, has not become firmly institutionalized in peer review practices, either as a matter of custom or through the efforts of professional associations like ASCE. There is still a lot of room for participants in a peer review to determine, on a case-by-case basis, what the limits of panel intervention in the design process should be.

Even if there is an emerging consensus about this demarcation in the engineering profession, there are a couple of reasons why it might not have as strong an influence in this particular case. First, the public agency setting may have made it possible to rely more on peer reviewers from academia, following the model of regulatory agency peer review. In addition, peer review, in this case, was not independently sought out by the “client” — Caltrans — or the consultants, but was required by the Governor of California at the recommendation of the Board of Inquiry. In contrast to a typical engineering peer review situation, this peer review panel had a mandate from an outside political authority even though it was appointed by Caltrans. This meant that the panel had an unusual degree of independent power over the peer review process. All of the panel members, particular the academics, as employees of the state, may have felt they had a responsibility to take a more active role in shaping the design practices of both the agency and the consultants, and in changing them to reflect newer, higher standards of seismic design as a matter of public safety. My conversations with various engineers who have served on these panels suggest that this sort of public-service ethos does play an important role in their thinking.

Second, ASCE engineers largely seemed to assume that a peer review panel would be made up of their fellow consulting engineers. Many of the calls for avoiding the "that's not the way I would have done it" syndrome seem to be based on the idea that peer reviewers are going to be professional colleagues and potential competitors of the designers. This means that there is a certain presumed equality of status between reviewers and designers, so that the reviewers have to give due deference to the professional judgement of the designers. Also, because peer review is meant to be a collegial process, reviewers have to be very careful to avoid appearing competitive with the designers.

The Caltrans peer review panel, by contrast, had a contingent of university professors who were not really seen as full-fledged panel members, but rather as "technical advisors," even though they seemed to share in many of the review activities of the panel as a whole. It is significant that the rest of the panel members suggested that the technical advisors, rather than they themselves, take on a more active role in the design process. This probably had something to do with the higher status of academics, which gives their suggestions added credibility. Also, input from academic researchers may be less threatening because they aren't potential competitors for design work, and because they are generally perceived to be more disinterested and objective than practicing professionals. All of these factors give them more leeway to intervene in the design process without creating a great deal of tension.

Between the political and the technical

At Caltrans, the distinction between technical and policy activities in peer review is generally held to correspond to the respective roles of the peer review panels appointed to oversee particular projects and the permanent Seismic Advisory Board. This distinction originates in the recommendation of the Governor's Board of Inquiry that Caltrans appoint an outside advisory board to "advise Caltrans on seismic safety policies, standards, and technical practices."[52] Peer review panels, by contrast, are supposed to stick to more narrowly technical issues. For example, Jim Roberts, the head of the Division of Structures, clarified that

> the technical matters of retrofit analysis and design [should] be left solely as the responsibility of the Peer Review Panel. Likewise, The [sic] Seismic Advisory Board should have the sole responsibility in commenting on Caltrans policy regarding seismic issues.[53]

Perhaps because of this need to distinguish the roles of these two types of panels, nobody with an interest in the SAB seems to have any problem with designating its work as policy-oriented, even when it appears to be quite technical in nature. This is an interesting contrast to regulatory peer review, where Jasanoff observed the opposite tendency. But this difference is not as significant as it initially appears to be. Many of the "policy" recommendations the SAB makes are in fact framed in very technical terms, even where a more political idiom might be applicable.

For example, Caltrans has consistently relied on estimates of the "Maximum Credible Earthquake" (MCE) to be expected in a particular location as part of its procedure for estimating the seismic risk to a given structure. The MCE is a basically time-independent determination of the maximum possible magnitude of earthquake that could be produced by a given fault, without considering how active the fault is. Following this approach, any major fault would be considered an earthquake risk, even if it was judged not likely to produce an earthquake in the next several thousand years. In one of its first recommendations to Caltrans, the SAB urged them to use a more probabilistic approach which considered the maximum earthquake likely to occur during the actual life of the structure, arguing that "There is a need for a realistic time-frame in assessing seismic hazard."[54] The suggestion that the Caltrans method is not realistic accords with a general feeling of seismologists, particularly those in academia, that the MCE approach is out of date and unsophisticated.[55]

But this recommendation is as much about determining acceptable levels of risk as it is about the relative technical merits of the two approaches. As Jasanoff notes, among policy analysts "decisions about whether or not to accept a certain level of risk are generally regarded as involving both personal and social values, and hence as inappropriate for delegation to experts."[56] Nevertheless, the SAB, like many scientific advisory panels, has been able to step into this area without appearing to usurp legitimate political authority. It has done this by putting the choice between alternate methods of risk assessment in purely technical terms. There is still a certain amount of boundary work going on here, but it takes the form of ensuring that recommendations that are identified as policy are grounded in the language of engineering expertise, rather than denying that policy decisions are being made.

These boundary concerns also emerge, in a slightly different way, in the more focused and "technical" peer review of particular projects, such as the retrofit of the San Francisco viaducts. When the peer review panel for these projects was appointed, Caltrans had already determined which structures should be retrofitted and which should be demolished. On one particular structure, the Central Viaduct, panel members began to feel that they were being forced to choose between various unsatisfactory retrofit schemes, when it really made more sense to tear the structure down. Two of the professors on the panel noted being "somewhat frustrated that the Peer Review Panel is asked to review designs for technical feasibility, but is not asked to comment on suitability of the chosen strategy with respect to alternative strategies."[57] If this frustration seems to have been largely a matter of wanting to have more leeway to pick the best possible technical alternative, a subsequent letter from non-academic panel members to Jim Roberts suggested that broader political and professional issues were at stake:

> In the process of our deliberation the Panel has become deeply concerned about its charge and role in the retrofit process. We understand that Caltrans envisions our role as that of a technical review panel to assess the technical merit of the proposed retrofits. The Panel is not privy to all of the socio-economic and political aspects of this task. Nevertheless, the Panel is aware of the controversies surrounding the retrofit program. We are extremely aware that the public and our own profession see us as representing the engineering community and identify decisions reached by Caltrans as being representative of the Panel's views. This is not the case. For example, in the case of the Central Freeway it is obvious to the Panel that the economic and technical benefits of the planned retrofit are marginal, or less, than complete replacement. This may well be the case also for parts of the other freeway sections.
>
> The Panel is aware that consideration of other than technical and economical aspects of retrofitting may influence decisions reached by Caltrans. Our quandry is that by being unable to explain publicly the limits established for the Panel we are professionally put into a position of appearing to endorse decisions reached by Caltrans, a position that is becoming increasingly

unacceptable to the Panel.[58]

It appears that this issue was satisfactorily resolved, since there is no further record of its discussion, and Caltrans did ultimately retrofit the Central Viaduct (although it chose not to retrofit certain other viaducts for cost reasons).[59] Still, this letter illustrates the dynamics of boundary work between technical and policy decisions in a particularly vivid way. In their letter, the panel is careful to draw a very clear distinction between its defined role of "assessing technical merit" and a variety of other sorts of assessments that might be made, particularly those relating to "socio-economic" and "political" factors. Like the Seismic Advisory Board, this group is concerned about the distinction between technical and policy review, but they come at this issue from a different direction. For the SAB, the problem was to make their decisions, which were considered to have a "policy" component, more defensible by basing them primarily on technical criteria. This peer review panel, by contrast, was charged with addressing only narrowly-defined technical issues, but they seem to have been worried that their technical decisions would be interpreted as an endorsement of non-technical policy. This was an unacceptable outcome in itself, but the potentially more significant problem was that it could weaken their credibility as technical experts. Note, however, that the panel does not suggest that it would be inappropriate for them to comment on these matters if given the opportunity. Advisory panels generally have little problem with addressing politically relevant issues so long as they can do so in technical terms. The problem in this case was that the crossover between the technical and the political was out of the control of the panel members, which could have made them vulnerable to outsiders' attempts to redefine their role.

Making boundaries work

In *The Fifth Branch*, Jasanoff argues that peer review panels and advisory boards are useful mainly as forums for negotiating solutions to complex problems which aren't easily categorized as technical or political in nature. She argues that the ability to do this successfully depends on two factors: first, that the panel should not, in practice, be required to respect a rigid boundary between technical and policy issues; and second, that the panel must be able to rhetorically set such a boundary, and define its activities as being on the purely technical side of the divide.[60] Peer review in engineering faces similar demarcation problems, not only along the boundary between technical and policy decisions, but also with respect to the boundary between the technical work of design and the review function of the panel. In both cases, mechanisms are deployed that enable actors to cross these boundaries in practice, while at the same time maintaining the impression that no boundaries have been violated. Seismic Advisory Board members were able to comment on policy issues as technical experts by discussing these issues in largely technical terms. The San Francisco viaducts peer review panel was able to get involved in the design process without violating professional norms that would require them to defer to the project designers by assigning this task to panel members of ambiguous status — academic "technical advisors."

Even if there were some reason to draw very rigid limits on the activities of peer review panels in regulatory agencies or in engineering, this would probably be a difficult task. Perhaps this is not done consistently now simply because peer review is too new an idea in engineering and in policy settings, so people have to make up the rules as they go along, without strong institutional or cultural precedents. But there may be a more fundamental reason. If the primary purpose of peer review in policy or engineering settings is to facilitate negotiation and compromise, its usefulness lies precisely in the fact that it provides an alternative to more rigid, rule-bound ways of regulating practice. It is a setting in which reviewers are supposed to act in their full professional capacity — as autonomous, objective experts — rather than in their organizational or client-service roles. Setting very sharp or firm boundaries on the activities of peer reviewers would likely make them appear less independent and less credible as experts, erasing one of the key benefits of peer review over more rule-based approaches for both the profession and the agency being reviewed.

4.3 Picking a good peer

Although engineering practice is often bound by codes and other formal rules, it is also very collaborative. It therefore both demands and provides ample opportunity for the development of personal familiarity between practitioners. The problem with studying personal commitments in engineering is that the grounds for trusting colleagues are not usually articulated, and in practice it is difficult to disentangle trust in systems from trust in persons. The unique feature of engineering peer review is that it does this disentangling for us. Because peer review is supposed to be a process of negotiation and consensus building, it is in the interest of everyone concerned to maintain an orderly, non-confrontational environment. A standard way to do this would be to draw up a set of rules for people to follow, in effect trusting an abstract system to maintain order. But because part of the purpose of peer review is to avoid such rigid rules, it becomes necessary to trust individual persons to behave in a responsible way.

Both the literature on engineering peer review and Caltrans records consistently emphasize that the success of peer review depends greatly upon the composition of the panel and the personal traits of the individuals chosen to participate. One ASCE engineer gives some examples of factors to consider when looking at prospective panel members:

> In selecting peer members, consider the project scale that each peer member is familiar with; the years their projects are in operation; whether their projects are new or old; the individual member's experience; their area of specialty within their respective disciplines; and the member's personality. It is important to research these facts before assembling a trial group of members. A good rule to follow is 'don't be surprised by a member.' If proper attention is paid to member selection, a high quality peer review will be produced.

Furthermore, there is an art in assembling a panel of complementary individuals: "balancing the members on the review panel is vital if the review is to be successful . . . pay particular attention to outspoken individuals, who you will want to balance with another strong advocate."[61] Although this does not provide an exhaustive or systematic description of all of the factors that have to be considered when selecting panel members, it does give a sense of the importance of this process.

Who is my peer?

In common usage, the term "peer" has no clear and universal definition. It usually refers to people who are equals in some way, but this can be applied very broadly, to all of a person's professional colleagues, or very narrowly, to specify only those colleagues who share a common rank within the profession, however that rank is defined. Neither of these senses of the word seems to apply to the selection of peer reviewers, however. Instead, peer reviewers tend to be drawn from an elite group of practitioners who are clearly not the professional equals of the people being reviewed.[62] As one Caltrans peer reviewer put it, reflecting on his own status, "they pick older, grey-haired guys to be the peer review panelists" because "there's some sense, some truth in grey hair, or some kind of designated expertise" even though "the difference between say a 'peer' and a peer reviewer might be two percent in knowledge." [63] In the engineering context, the ASCE and Caltrans have both struggled to provide a definition of "peer" for the purposes of peer review that takes into account and attempts to justify the higher status of reviewers.

The ASCE's *Quality in the Constructed Project* notes that "a peer is defined as a person or group of persons with the same or higher level of technical or managerial expertise as those who are responsible for the subject of the review."[64] This definition appears to be an attempt to bridge the gap between actual practices in the selection of peer reviewers and the egalitarian ideal. However, the preliminary edition of the manual, distributed for review and comments, also stated that a peer review team would "normally consist of registered professionals with at least 15 years of experience in their design professions," which would tend to make the reviewers all quite a lot more experienced than the average participant on a design team, if not the design supervisor.[65] Interestingly, though, the final version of the manual drops this language, instead suggesting only that the reviewers should be "senior professionals."[66] Perhaps the more rigid definition cut against the egalitarian vision a little too sharply.

Instead of referring to years of service, the manual more typically uses the language of expertise and professional reputation, two factors which aren't directly connected to seniority, but which typically accompany it. For example, it states that "reviewers are recognized for their expertise and contributions of practice, and are active in their respective professional or trade associations" and that "the review team's effectiveness is greatly influenced by the independence, experience, and stature of its members."[67] At Caltrans, similar considerations apply; reviewers are supposed to be "genuine experts" with "recognized reputations" and "established credibility within the engineering profession."[68] Putting the selection criteria this way avoids the impression that seniority itself is a qualification, rather than particular, definable professional abilities.

There is still a recognition that a certain degree of similarity in experience between peer reviewers and project designers is helpful. For example, one ASCE member argued that, in the case of public agency peer review, "selecting members from [other] public agencies increases the likelihood that the reviewed agency might accept their comments more readily since the agencies usually share many common problems."[69] Still, the fact remains that it is usually higher-status, senior engineers who end up on peer review panels. This is not too surprising, since the central purpose of peer review in engineering is to provide some kind of certification, both to the rest of the profession and to the public, that a project has been designed, according to the highest professional standards. Since more experienced practitioners tend to have accumulated more "symbolic capital" in all sorts of forms — professional visibility, access to resources, public recognition, etc. — it is not surprising that they would be seen as better able to serve this purpose. This explanation makes sense in light of the specific criteria in the ASCE manual that reviewers be professionally active and well respected by their colleagues.

The inclusion of academic researchers on peer review panels has been a matter of some controversy at Caltrans. The issue does not appear to have been considered at all by the ASCE: *Quality in the Constructed Project* seems to assume that reviewers will be practicing professionals, which may have something to do with the fact that only one of the 41 contributing authors of the manual is listed as having a university affiliation.[70] When Caltrans put together the San Francisco viaducts peer review panel, they were also a little unsure whether academics could really be peers of practicing engineers, so they made the university professors "technical advisors" rather than normal members of the panel. Why this skepticism about academics? As one professor put it, there is "still some distrust of academics amongst the professional community," based mostly on the feeling that they might not have a "real world" perspective on design problems. He also noted that some of the professors, including himself, were not licensed structural engineers, so "what credibility did I have to say anything about what these guys did?"[71]

This distrust, however, seems to be balanced by the enormous respect which many academics are granted, particularly by younger practitioners. This impression is probably enhanced by the fact that many younger engineers were students at various University of California schools, and either took courses with these prominent professors or read their books as part of their training. Also, academics are generally accorded a great deal of respect, in part because they control the most abstract, generalized and cutting-edge professional knowledge.[72] This is particularly relevant in earthquake engineering, since there are few non-academics who specialize solely in that area. So even though they may be less in touch with the problems that practicing engineers face, academics are appealing as peer reviewers because they do bring a level of prestige to a panel that few practicing engineers can command. Practitioners might still have some lingering doubts about academics, even while holding them in high esteem, but university professors seem to be particularly credible in the eyes of the public, in part because of their perceived independence.[73]

Caltrans seems to have almost immediately recognized the particular usefulness of academics on peer review panels. On the San Francisco viaducts, for example, it was the academics who were in a position to work with the consultants on improved retrofit designs. Also, the academics who serve on peer review panels have a much closer relationship to Caltrans engineers, and in some respects seem to have better knowledge of the procedures and the typical problems that they face than some consultants do. This is because they are usually also involved in research for Caltrans and are often called on as informal advisors as well. Caltrans quickly dropped the practice of designating academics as "technical advisors." The second peer review panel to be appointed, for the Santa Monica Viaduct in Los Angeles, included academics as full members.[74] The 1993 guidelines for panel membership state that "a Panel must consist of a mix of academics who are in the forefront of research and of experts in the practice of engineering."[75] Academics now seem to be the most prominent members of many of these panels, and are often called upon to chair them.[76]

Characteristics of a good reviewer

Deciding on the definition of a "peer" narrows the field down to a select group of professionals who are qualified, both by credentials and by reputation, to serve as peer reviewers. But further selection criteria are needed in order to determine which professionals are likely to be good reviewers and should actually be put in that role. But credentials and general reputation alone don't seem to provide the necessary information to make this sort of judgment. Instead, engineers want to know if a particular colleague can be trusted to follow certain norms of conduct that will result in a successful review. They typically make this judgment based on an assessment of the personal qualities of potential panel members.

One of the commonly mentioned characteristics of a good reviewer is that they have good technical skills and knowledge of the particular area being investigated so they can "render sound advice and recommendations."[77] But simply having this knowledge in some abstract sense is not thought to make a person a good reviewer. The reviewer must also be able to apply this expertise in a careful, responsible way. For example, it is recommended that reviewers be professionals with "sound judgment" who are "thorough" and "meticulous."[78] The sort of expertise that is wanted here is clearly not simply a mental accumulation of facts and procedures. Instead, some kind of normative judgment is being made about how a person thinks and behaves while putting his or her expertise into action.

Another characteristic that is frequently mentioned as desirable in a peer reviewer is good interpersonal skills. This is often discussed in terms of communication ability. The ASCE quality manual, for example, states that peer reviewers should be "good listeners and skillful communicators."[79] Another manual emphasizes that "assessing potential reviewers' interpersonal skills and specifically their ability to effectively communicate with the design organization is a subjective evaluation," deliberately contrasting this with the presumably more objective evaluation of the technical competence of the reviewer.[80] A sense of tact is also seen as an important quality in a reviewer. This is particularly important for avoiding tension between reviewers and designers. For example, when one of the ASCE engineers talked about a situation in which he was embarrassed in front of a client by an overzealous peer reviewer, he concluded that "Here's a case where tact and diplomacy were needed."[81] This language suggests that sometimes such an imposition on the designer is necessary, but emphasizes the potential for misunderstanding and ill-will that this can create, warning reviewers to present themselves in a way that will avoid potential conflict.

It is not too difficult to see why tact and communication skills could be so important in a peer reviewer. In some ways, this simply reflects the overall structure of peer review in engineering and policy settings. Unlike journal or grants peer review, where an editor or program manager is in a position to assess and reconcile reviewer reports which may conflict with one another, engineering peer review and regulatory peer review work only on the basis of consensus. Peer review is supposed to avoid the divisive conflicts that can emerge out of a more representative or adversarial processes. An ability and willingness of reviewers to engage with others — their fellow panelists and others involved in the review — in a careful, respectful way is necessary if the integrity of the process is to be maintained, and if its outcome is going to be successful.

Objectivity and disinterestedness

The most frequently mentioned characteristic of a good peer reviewer is objectivity. Engineering follows the general modern trend, common to the sciences as well as the professions, of identifying objectivity not with faithfulness to an objective reality *per se* but rather with the suppression of individual interests and biases: in a word, disinterestedness.[82] Because disinterestedness is supposed to involve the elimination of personal factors, it is interesting that, in the context of engineering peer review, it is seen primarily as a characteristic an individual may possess. The presence or absence of this characteristic is evaluated both through formal rules and through familiarity with the personality of the reviewer.

One classic element of disinterestedness is freedom from the influence of others. In 17th century England, for example, only gentlemen were considered reliable observers of nature because they were the only group thought to be in a position to act independently of the will of others.[83] In engineering, independence is also considered to be a prerequisite for objectivity, but it is not a quality that any one social group is presumed to possess. Instead, independence is associated mainly with the lack of *conflicts of interest*. This legalistic way of talking about disinterestedness reflects the professional situation of engineers, whose work frequently intersects with business and legal concerns.

The ASCE specifies that peer reviewers should be "peers of the original owner(s), manager(s), author(s), design professional(s) or constructor(s) who are independent of the subject of the review."[84] In practice, this means that

> if the reviewers are from a separate organization, there should be no strong relationships that would interfere, or seem to interfere, with the completeness and impartiality of the review. If the reviewers are from within the organization being reviewed (as might occur in some large organizations that have formal internal peer review programs), the reviewers should be sufficiently remote geographically and administratively so that there is no question of their complete independence.[85]

The idea that there should be no "strong relationships" between reviewers and those being reviewed created a problem for Caltrans, because the earthquake engineering community in California is quite close-knit, and the group of engineers who both have expertise in earthquake engineering and the necessary reputation to serve on a peer review panel is quite small. This is further complicated by the fact that Caltrans is one of the major employers of bridge engineers in the state, not to mention a major supporter of earthquake engineering research at state universities. This has only become more of a problem because of the scale of Caltrans' seismic retrofit program. When Governor Deukmejian appointed the Board of Inquiry into the Loma Prieta earthquake, he specified that nobody on the Board have any past or present ties to Caltrans, but this proved to be quite a challenge to implement.[86] Now, even the former members of that board have strong ties to Caltrans. In fact, nearly every prominent structural engineer, in practice or in academia, has had some dealings with Caltrans. As a result, Caltrans and the Seismic Advisory Board adopted a somewhat less stringent standard for independence:

> A member of a Peer Review Panel may at one time or another be involved in a Caltrans contract as a consultant. This is acceptable so long as there is no conflict of interest. It would severely hamper Caltrans operations if

> such experts were eliminated from consideration because they had done engineering work for Caltrans in the past, or may do work for Caltrans in the future. If a conflict of interest should develop, the member must formally excuse himself/herself from the review process.[87]

In practice, the “conflict of interest” provision is interpreted to mean that panel members should excuse themselves from decisions made about projects their own company is involved with.

It is interesting to note that the “conflict of interest” language in the Caltrans peer review guidelines only addresses engineering consultants, not academics. Almost all of the academics who serve on Caltrans peer review panels have ongoing research projects funded by Caltrans, and some of them have built their professional reputations, and their laboratory facilities, largely on the basis of Caltrans funding. Furthermore, academics who serve on these panels are frequently involved with peer review decisions which directly result in testing contracts for them. For example, academic researchers on the San Francisco viaducts panel came up with and promoted the very designs which they later ended up testing in their laboratories. This practice has not ended with the promotion of academics to full panel membership, either. In the peer review process for the Coronado Bridge in 1997, for example, an academic member of the panel was involved in the decision to perform a test on a model of one of the bridge piles, which was funded by Caltrans and performed in his laboratory.

Publicly, at least, nobody seems to have criticized Caltrans for these practices. In any case, the small community of academic researchers in earthquake engineering and the limited number of testing facilities, coupled with Caltrans’ urgent need for research results, would make it difficult to avoid all appearances of possible conflict. The fact that there is little concern about academics and conflict of interest, while consulting engineers are subjected to a much more rigid standard, suggests that academics can lay claim to a certain presumed disinterestedness simply based on their status as university researchers. There is an art of “impression management” which also plays a role here, however.[88] One professor described, as an example, how his laboratory made an effort to do testing work for all of the companies developing advanced composite materials for bridge retrofit, rather than working for only one or two, in order to avoid being identified with one particular product. He also turned down requests to serve as an analytical consultant for engineering firms competing for one major project because he knew he was likely to be called on as a peer reviewer on that project. Furthermore, he explained, he tries to project an idealistic and objective image while serving on peer review panels by sometimes going out of his way to pursue technical disagreements that he has with Caltrans or their consultants.[89]

This suggests that formal rules about conflict of interest shade gradually into judgments of a person's manner and character. Disinterestedness is, in fact, talked about as a personality trait in itself, not just in terms of freedom from outside influence. For example, one article in *Civil Engineering* suggests that a good reviewer should be "intent on producing a better job, not on putting down the designer, causing conflict, or asserting personal prejudice." In particular, they should be "intent on improving plans and specs," not "intent on implementing their own ideas into a design."[90] Another ASCE engineer takes this concern to an even higher level, suggesting "the thing that a reviewer should remember is that he or she doesn't need to enhance the project or in any way leave a mark on the project that is being reviewed."[91]

Some design engineers clearly would like to push the definition of interest or prejudice quite far, so that a truly unbiased reviewer would seek to have virtually no impact at all on the final form of a project. Interest, in this case, would seem to include even a narrow technical interest in introducing improved techniques to designers. At Caltrans, however, such a narrow definition has not been followed. In fact, peer reviewers, especially academics, are allowed a great deal of leeway to suggest and even take a role in implementing specific technical solutions they have come up with. This more interested role for peer reviewers is made more acceptable by putting academics, who are perceived as generally disinterested, in the boundary-crossing role. In general, it appears that there are no strongly institutionalized criteria for judging disinterestedness beyond the relatively broad rules about conflicts of interest, just as there is no generally accepted definition of the boundary between review and design. The important characteristic of a peer reviewer, it appears, is not strict adherence to any one definition of disinterestedness, but rather a general ability to project a disinterested, competent image and to avoid conflict. Good communication skills and tact are important because they help a reviewer to project this kind of image.

4.4 Conclusion

Engineering peer review, much like regulatory peer review, seems to generate concerns about the proper boundaries of panel activities and about the personal abilities of individual reviewers. What is truly distinctive about engineering peer review, however, is the specific boundaries and characteristics that are seen as important. The most important demarcation that is made in engineering peer review is generally not between technical matters and policy, but rather between the activities of review and design. The sensitivity of this boundary is reflected in the criteria used for assessing the competence of an individual reviewer. In particular, an appearance of disinterestedness on the part of reviewers may help defuse possible tensions generated by the somewhat ambiguous boundary between their work and the work of project designers. At Caltrans, academics have taken on a particularly prominent role in peer review in part because their status as scholarly researchers carries with it a certain presumed impartiality.

The potential tension between reviewers and designers can be traced back to broader features of the professional ideology of engineers. Edwin Layton has argued that the ideology of the engineering profession has historically had two competing strands which reflect the conflict between business and professionalism. The strand which ties engineering more closely to business draws on the classic middle-class values of individualism and faith in the system of private enterprise. The other emphasizes engineering professionalism, and focuses on the esoteric technical knowledge which sets engineers apart from businessmen. The first of these perspectives tends to picture an engineer as a professional free agent who is responsive mainly to the concerns of his clients or the organization in which he or she works. The second focuses on the responsibility of the engineer to the profession, and emphasizes the need for professional autonomy and collegial control over engineering practice.[92]

The conflict between these ideologies largely comes down to their differing conceptions of autonomy. In the business-oriented perspective, autonomy means that the individual engineer is able to sell his or her services to clients on the open market. In the profession-oriented perspective, autonomy means freeing the engineer from dependence upon clients or organizations by engaging him or her in a self-regulating community of colleagues. The two concepts of autonomy are somewhat contradictory. In the business-oriented view, increased professional control is seen primarily as an imposition on the relationship between engineer and client. In the profession-oriented view, the dependence of engineers on clients or bureaucracies is a restriction on professional autonomy which can be addressed by making engineers more responsible to their fellow professionals.

Tensions along the boundary between peer review and design have their roots in this larger conflict of views in engineering. Although they are subject to many guiding rules and codes, engineers in their capacity as designers seek to maintain one kind of autonomy by asserting that they alone are ultimately responsible for the design of a structure and for responding to the needs of the client. In civil engineering, this private enterprise orientation is reinforced by the fact that many designers are actually employed by engineering consulting firms, rather than in diversified corporate bureaucracies. The authors of the ASCE's *Quality in the Constructed Project*, for example, appear to be drawn overwhelmingly from consulting firms.[93] These same design engineers, in their capacity as advocates of professionalism, may see peer review as a way of enhancing the autonomy of engineers by placing some control over the evaluation of their work in the hands of their fellow professionals. Engineers who may be called upon to organize or serve on a peer review panel, many of whom work as designers themselves, are aware of this tension. They seek to resolve it by insisting that peer reviewers tread lightly on the independence of the designers, or at least present themselves in a way that makes such an imposition seem less threatening.

The central feature of engineering peer review is that it provides an institutional arena where these sorts of professional tensions can be worked out through personal interaction and negotiation. It is a new way of linking local engineering practices to the broader life of the profession. Peer review reembeds the conflict between the business orientation and the professional orientation in engineering in small-scale, interactive social settings, and at the same time makes the local practices of designers respond to a larger community of professional colleagues.

Because of its very interactive qualities, peer review has the potential to increase the sense of professional community in engineering. The Caltrans peer review program seems to have had just such an effect, bringing elements of the earthquake engineering community in California closer together and especially strengthening ties between the academic, government, and private sectors of the profession. Although this may have positive consequences for the profession as a whole, not all engineers are affected by it in the same way. Peer review places high-status professionals in a new position to review and pass judgment on the work of their colleagues. At the same time, it promotes social ties within this elite group, particularly where they may serve on many panels together. But it does less to promote social ties between lower-level project designers, while subjecting them to increased scrutiny from above.

Reliance on codes and other formalisms has historically been associated with a weak professional community in engineering. A weak profession can gain public credibility by showing that its practitioners follow rational, publicly-accessible procedures like those set down in codes. The problem with this approach is that it tends to perpetuate the weakness of the professional community by making engineering practice appear routine to outsiders and by allowing individual engineers to orient their work more toward formal standards than directly to the judgments of their colleagues. Peer review, by contrast, seeks to strengthen professional community by requiring engineers to respond directly to representatives of the profession. At the same time, it suggests to the outside world that good engineering practice is something which only trained, experienced professionals are in a position to judge — and which certainly cannot be reduced to a set of rules in a code book.

Notes

[1] Jasanoff 1990, 234-235.

[2] The idea of boundary work is introduced in Gieryn 1983 to refer to efforts by scientists to distinguish science from non-science. In Jasanoff's work, it usually refers specifically to the demarcation between science and policy.

[3] See Shapiro 1997, 290-291.

[4] Ibid., 291; Henderson 1999, chapter 2; Porter 1995, chapter 6.

[5] Wynne 1988, 156.

[6] Abbott 1988, 51.

[7] Porter 1995, xi; see also 114-147.

[8] See Luhmann 1979, 48-58; Giddens 1990, 83-92.

[9] Giddens 1990, 21.

[10] See Luhmann 1979, 45-46.

[11] Giddens 1990, 87.

[12] Gnaedinger 1978.

[13] Fox and Cornell 1985, i.

[14] ASCE 1988, ASCE 1990.

[15] ACEC and ASCE 1990.

[16] ASCE 1990, 95.

[17] ACEC and ASCE 1990, 5.

[18] ASCE 1988, 87.

[19] Ibid., 88-90.

[20] Gnaedinger 1978, 46; Meehan 1984 describes the role of Nuclear Regulatory Commission review panels in several power plant siting controversies.

[21] Dougherty 1984.

[22] Preziosi 1988, 48.

[23] On the rise of regulatory agency peer review, see Jasanoff 1990, 32-36.

[24] Memorandum, James Gates to James E. Roberts, October 30, 1989, "Subject: Chronology of Embarcadero Peer Review 1989," EF 28.

[25] The roles of the SAB and project peer review panels are outlined in a memo, James E. Roberts to James Van Loben Sels, Director of the Department of Transportation, April 29, 1994, attached to a letter from Jeffrey Reid, Undersecretary of the California Business, Transportation and Housing Agency to Ellen Johnck, Executive Director of the Bay Planning Coalition, May 5, 1995, EF 25.

[26] California Department of Transportation, "Report to the Governor on Seismic Safety," August 31, 1990, 14, GBI.

[27] Jasanoff 1990, 236.

[28] ASCE 1990, 98.

[29] Memorandum, J. Gates to J. E. Roberts, October 30, 1989, "Subject: Chronology of Embarcadero Peer Review 1989," EF 28.

[30] Road Information Bulletin, "Special Media Advisory: Caltrans Assembles Blue Ribbon Panel to Review Harbor Freeway," fax (with handwritten annotations) dated October 23, 1998, EF 28; "Independent Seismic Design Review of H.O.V. Viaduct No. 2 — I-110 Harbor Project 15 — Report to Caltrans Office of Structures Design, District 7," December 19, 1989.

[31] "Peer Review Panel Summary Report for Evaluation and Strengthening of San Francisco Double Deck Freeway Viaducts," December 31, 1992, EF 28.

[32] James E. Roberts, "Seismic Retrofitting of the Multi-Level San Francisco Freeway Viaducts," SN, September 1991, 2; see also meeting minutes, EF 28.

[33] Listed on the cover of the panel's report.

[34] Ray Zelinski, "San Francisco Double-Deck Viaduct Retrofits," SN, September 1991, 7; also Zelinski 1994, 36.

[35] James E. Roberts, "Seismic Retrofitting of the Multi-Level San Francisco Freeway Viaducts," SN, September 1991, 3.

[36] Ibid.

[37] Interview, Stephen Mahin, August 20, 1998; Ray Zelinski, "San Francisco Double-Deck Viaduct Retrofits," SN, September 1991, 10; also Zelinski 1994, 37.

[38] James E. Roberts, "Seismic Retrofitting of the Multi-Level San Francisco Freeway Viaducts," SN, September 1991, 3.

[39] Quoted in Preziosi 1988, 47.

[40] Ibid.

[41] ASCE 1990, 96.

[42] "Caltrans Peer Review Panel Duty Outline," attachment to "Caltrans Peer Review Panel, Meeting Notes from Monday, March 19, 1990, 9:30 a.m., Office of Forell/Elsesser Engineers," EF 28.

[43] Memorandum to the Review Panel from M.J. Nigel Priestley, July 14, 1990, "A Reinforced Concrete Frame Concept for Retrofitting Double Decker Freeways," EF 29. The memo mentions that Priestley briefly presented this scheme to the panel during its

meeting on July 12th. See also Zelinski 1994, 37.

[44] James E. Roberts, "Seismic Retrofitting of the Multi-Level San Francisco Freeway Viaducts," SN, September 1991.

[45] Letter, Nicholas Forell to James E. Roberts, July 17, 1990, EF 29.

[46] Letter, James E. Roberts to Nicholas Forell, August 9, 1990, EF 29.

[47] Memorandum from Ray Zelinski to Peer Review Panel, September 13, 1990, "SUBJECT: Miscellaneous Handouts," EF 29.

[48] Frieder Seible, "Comments on Final Retrofit Meeting with Consultants," September 5, 1990, EF 29.

[49] Zelinski 1994, 37-38; see also "Peer Review Panel Summary Report for Evaluation and Strengthening of San Francisco Double Deck Freeway Viaducts," E-3, EF 29.

[50] James E. Roberts, "Seismic Retrofit of the Multi-Level San Francisco Freeway Viaducts," SN, September 1991, 4; Zelinski 1994, 37.

[51] Memorandum, Ray Zelinski to James E. Roberts, December 11, 1991, "Subject: Summary of UCB Edge Beam Prooftest Events," EF 29.

[52] Governor's Board of Inquiry 1990, 81.

[53] Letter, James E. Roberts to Joseph Penzien, December 9, 1995, EF 25.

[54] Memo, Seismic Advisory Board — Caltrans to Robert K. Best, April 3, 1991, EF 25.

[55] Interview, Lalliana Mualchin, Caltrans seismologist, August 28, 1998. See also Yeats *et al.* 1997, 452, which argues that deterministic evaluations like the MCE always make implicit probabilistic assumptions; a footnote on the same page sarcastically refers to the MCE as the "maximum dredible earthquake" and suggests that it would be just short of the "maximum *in*credible earthquake."

[56] Jasanoff 1990, 232.

[57] Letter, M.J. Nigel Priestley and Frieder Seible to Nicholas Forell, March 5, 1991, EF 28.

[58] Letter, Caltrans Peer Review Panel to James E. Roberts, March 8, 1991, EF 28.

[59] Ray Zelinski, "San Francisco Double-Deck Viaduct Retrofits," SN, September 1991, 10.

[60] Jasanoff 1990, 230-236.

[61] Dougherty 1984, 49.

[62] According to Chubin and Hackett 1990, 193, this is true in journal and grants peer

review as well as in engineering.

[63] Interview, Charles Seim, May 28, 1997.

[64] ASCE 1990, 96.

[65] ASCE 1988, 83.

[66] ASCE 1990, 99.

[67] Ibid., 95, 99.

[68] Letter, George Housner to James E. Roberts, July 19, 1993, EF 25.

[69] Dougherty 1984, 49.

[70] ASCE 1990, 155-116.

[71] Interview, Stephen Mahin, August 20, 1998.

[72] On the high status of academics in the professions, see Abbott 1988, 118-119.

[73] For example, professor Frieder Seible of U.C. San Diego was able to play a crucial mediating role in the Chaicano Park murals controversy described in Chapter 7, partly because the public perceived him as being independent from Caltrans.

[74] Seismic Safety Review Panel, "Seismic Safety Review of Santa Monica Viaduct Retrofit — Outline of Assessment and Retrofit Procedures for Typical Bents Selected by Caltrans," November 8, 1991, EF 28.

[75] Letter, George Housner to James E. Roberts, July 19, 1993, EF 25.

[76] An example of an academic who has chaired a number of peer review panels is Frieder Seible of U.C. San Diego. Interview, November 8th, 1996.

[77] Letter, George Housner to James E. Roberts, July 19, 1993, EF 25.

[78] "Sound judgment" is mentioned in ASCE 1990, 99 and ASCE 1988, 83; "thorough" is mentioned in ASCE 1990, 99 and ASCE 1988, 83; and "meticulous" is mentioned in ASCE 1988, 83 and ACEC and ASCE 1990, 10.

[79] ASCE 1990, 99.

[80] ACEC and ASCE 1990, 9-10.

[81] Preziosi 1988, 47.

[82] In the sociology of science, this term is most closely associated with Robert Merton, who notes that it is a basic institutional element of the professions as well as of science (Merton 1973 (1942), 275). Dear 1992 notes the shift from "truth" to "disinterestedness" as the primary criterion for objectivity in the 17th century. Daston 1995 notes that objectivity is typically opposed to the personal.

[83] Shapin 1994, 83-95.

[84] ASCE 1990, 95-96.

[85] ACEC and ASCE 1990, 3; the wording here is a somewhat more pointed version of a passage in ASCE 1990, 96.

[86] Housner 1997, 175-176.

[87] Letter, George Housner to James E. Roberts, July 19, 1993, EF 25.

[88] See Goffman 1959, especially 208-237.

[89] Interview, Frieder Sieble, November 7, 1997.

[90] Gnaedinger 1978, 46.

[91] Quoted in Preziosi 1988, 47.

[92] Layton 1971, 54.

[93] ASCE 1990, 115-116.

Chapter 5

Change and Formalism in Design Practice

5.1 Introduction

By the late 1980s, Caltrans engineers had gotten used to doing things a certain way. Most projects were new bridges, and the design process was guided mainly by codes. Caltrans codes were very stable, changing gradually to accomodate new information, and were considered by many to be the most sophisticated in the country, especially regarding seismic issues.[1] Design work required a certain amount of coordination between engineers within the organization, but very little interaction with outsiders. After the Loma Prieta earthquake in 1989, engineering work suddenly became a lot more complicated. Designers had to work with a proliferation of research data and new design approaches that weren't set down in any code. Instead of a relatively small number of routine projects, they were swamped with retrofit projects that presented unfamiliar complexities. They went to meeting after meeting to coordinate project design, and had to defer to the judgment of highly-trained seismic experts in many of these meetings. Quite often, they had to interact with critical outside peer reviewers, many of them from academic institutions, or with outside design consultants who were unfamiliar with Caltrans procedures.

For many at Caltrans, the period from 1989 into the mid-1990s was a very exciting time, even a "golden era," as one engineer described it:

> It was an era in which there was a need to change the practice, and there were resources available outside, and Caltrans was willing to let it loose and just, you know, ask for data. ... There was a period of time where, you know, engineers were picking up information very quickly ... we were changing every month. ... There were specific tests done for specific projects, and data was coming in at a very fast rate, because the consultant engineers were designing and Caltrans was designing. And the university was trying to keep up as many tests as they could. So for a period of time, new data was coming on all fronts, columns, footings, superstructures, shear issues, you know all kinds of data. And that was very good.[2]

Others were pleased with the increase in collegial interaction within the organization, as one Caltrans employee, probably an engineer, noted in an email message:

> As I sat through our meeting last Friday, I could not help but notice something truly outstanding! Here in one room was the entire hierarchy of Caltrans ... from an Associate R.E. [registered engineer?] to the Chief Engineer ... and all facets of our organization ... The District, Headquarters, the Bridge dept, and legal. We were all together, working hard as a TEAM to solve a difficult problem! This is the BEST example of team spirit I've seen in a long time, and I can tell you I've felt very proud to be a part of Caltrans![3]

This more open and interactive environment had a downside, however. As one Caltrans engineer explained, a particular type of person thrived under these conditions:

> This whole new environment is ... the engineers are required to do a lot more thought and have a better understanding of how that bridge is gonna perform. And that's good for some people because they like that, and they're interested, and they enjoy that, and they feel more comfortable and more empowered. ... I think people that like that ... really enjoyed the fact that they could bring academics in and interact with them. ... So I think that was good. I think we were [sometimes] frustrated because it's a lot of information at once, and a lot of information with no answers to ... when you have choices and decisions to make, it's harder to be decisive and get things done. So it depended on your nature. And some people thrived in the environment, and other people don't like that. They really miss the way it used to be.[4]

5.2 Coordinating the design task

Engineering projects are among the most complex of professional activities. Because of this complexity, design work typically involves an extensive division of labor among many participating engineers. If the design team is to carry out its task successfully, ways must be found to coordinate the activities of its members. In an organization like Caltrans, which attempts to impose some overall regularity on individual design projects, the problem of coordination takes on even larger dimensions. One way engineers solve this problem is through the extensive use of codes and other formalisms which constrain individual practice. Another way they solve it is by making design work an intensely social process. Face-to-face meetings, both formal and informal, are a universal feature of modern engineering practice, and serve as arenas for reintegrating the distributed tasks of design.

The uses of formal representations

Representations play a crucial role in coordinating and standardizing work activities in our technologically complex society. There are a number of different ways in which they serve this purpose. First, they can act as what Bruno Latour calls "immutable mobiles," textual or graphical representations that can be reproduced and moved from place to place while maintaining a relatively fixed form.[5] Codes, which are endlessly reproduced and distributed, are an example of immutable mobiles. The universal availability of a single code within a design community ensures that, whatever idiosyncrasies might creep into the work of individual engineers, at least some portion of their task is based upon a common set of procedures.

Second, representations can serve as "boundary objects" which help coordinate work by providing a common frame of reference for actors with divergent roles.[6] Navigation charts, for example, serve such a role in the piloting of large ships.[7] In engineering, Kathryn Henderson has noted that drawings and sketches often serve as focal points which allow participants in the design process to communicate and organize their work together despite their sometimes divergent interests.[8] A code, although it is much less flexible than a sketch, can serve as a boundary object in a similar way, by providing designers with a common set of procedures to which they can refer while working together.

One reason codes can coordinate practice is that they become an integral part of the cognitive and work practices of individual designers. As Louis Bucciarelli notes, codes "become part of habitual ways of thought and action, a dog-eared page in the code book."[9] Codes, then, are not simply abstractions, but function as tools in the ongoing stream of design activity, at both the individual and group levels.

The limits of formal representations

Although they play an important role in coordinating work in a variety of technological settings, codes and other formal representations have certain limitations in this respect as well. One difficulty is that formal representations cannot capture the complexity of actual work practices. H.M. Collins, for example, has shown that the skills necessary to replicate scientific experiments are not easily transmitted through written sources alone.[10] He argues that rules always require interpretation when applied in particular situations, and therefore can never fully specify a course of action, at least not in the absence of certain common social conventions.[11] Similarly, Susan Leigh Star has argued that formalization always proceeds through a process of "deleting the work" as it is actually performed by individuals and groups under specific, local circumstances.[12]

These arguments should not be taken to imply that formal representations of work practices are simply defective in some way. Indeed, they can sometimes serve as useful coordinating tools just because they aren't too closely tied to the intricacies of work in any particular location. Engineering codes, for example, are not expected to completely specify each step in the design process — a non-engineer would be hard pressed to get any useful information on how to actually design anything from a code. Instead, the expectation is that the designer will bring his or her expertise to bear on the design problem, using the code as a tool to be adapted to particular problems and local circumstances. As Bucciarelli explains, with codes,

> like common law, or any other human construct, there is always a need for a reading and interpretation in their application and always more than one way to meet their intent. Far from being ex cathedra commandments, these human constructs derive their meaning out of their continual exercise and redesign.[13]

When codes are used in this interactive way, their abstraction is not an impediment to design work. Rather, it is a way of formalizing what can easily be formalized, freeing the designer to exercise professional judgment in other directions.[14] This does mean, however, that design codes cannot be counted on to standardize practice and coordinate the design process on their own.

Another limitation of formal representations of work is that they are often relatively inflexible instruments. As a result, they can be slow to respond to changes in practice. Codes can be particularly inflexible because they are used by different people within an organization, even different designers, in different ways and for different purposes. Because of this, code development is often a process of extended negotiation between these different actors. As Bucciarelli notes,

> the problem is not simply in the drafting; it lies as much in getting the regulation accepted as legitimate and relevant to [engineering] work . . . The process of constructing and implementing a regulation works best when it is, like the design process itself, one of negotiation and exchange across the myriad of constituent interests. This is no light task.[15]

In addition, the fact that codes are supposed to standardize practice, and are often legally sanctioned, means that change cannot easily be introduced at a local level — instead, some kind of formal institutional process is usually necessary. The relative inflexibility of codes is not usually a problem, however, since design practice itself usually changes slowly enough that codes can keep up. But codes aren't always able to stay so close to practice during periods of rapid change like the one examined here.

Distributions of competence

In classic sociological theory, the complexity of human activities is often discussed in terms of the division of labor: how work tasks are divided up among social actors. More recent work in the field of science and technology studies and in the sociological theory of modernity tries to draw attention to the ways in which human divisions of labor are mediated through technology and formal representations. In place of a division of labor, we can refer to what Bruno Latour has called a "distribution of competences" between people and things.[16] For example, Latour and others have argued that science and engineering work involves the creation and extension of networks linking together human and nonhuman elements.[17] Similarly, cognitive scientist Edwin Hutchins has shown how complex cognitive tasks can be spread out across various social actors, tools, and representational devices in patterns of distributed cognition.[18]

In a slightly different vein, social theorists Niklas Luhmann and Anthony Giddens have noted that modern society is characterized by increasing social complexity and by the increasing extension of social relations from familiar local settings to broader expanses of time and space.[19] Maintaining such complex and dispersed systems of social relations requires us to place trust in unfamiliar people and in technologies the workings of which we do not fully understand. This trust is increasingly mediated through abstract, impersonal systems for certifying expert knowledge and constraining individual actions[20] — in engineering, for example, professional certification and design codes serve these purposes. Both authors, however, point out that the importance of such abstract systems is often exaggerated, and that modern social relations still depend greatly on personal familiarity and on the face-to-face interactions that foster it.[21] These arguments suggest that the social coherence of complex work processes may be described in terms of a distribution of competences between various sorts of face-to-face interactions and symbolic representations of skills and practices.

The form that a distribution of competences takes is not infinitely flexible, because a given task cannot equally well be delegated to a human being, a machine, or a representational device. Hutchins, for example, discusses this in terms of the distinct computational capabilities of people and their tools, and looks at how patterns of distributed cognition change in response to environmental conditions and the task at hand. The argument of this chapter draws on this insight, but instead of describing how distributions of competence shift to accommodate specific changes in the work environment, it looks at how they respond to the overall rate of change. Specifically, a more rapid pace of change in engineering practice tends to shift the burden of coordinating the design process away from relatively inflexible formal representations like codes and toward more personal and socially interactive mechanisms. Among these mechanisms are face-to-face meetings and reliance on known and personally trusted experts.

5.3 Changes in Caltrans design practice

In the years following the Loma Prieta earthquake in 1989, a number of changes were introduced into Caltrans practice from various sources. A great number of the changes made had to do primarily with design details — for example, how much and what configuration of reinforcing steel should be used in column foundations. But these changes, while important at an aggregate level, were fairly incremental in nature and did not reflect a fundamental change in design methods. When Caltrans engineers talk about the most significant changes that have occurred over this time period, they usually refer to changes in the analytical methods that are used as a basis for design calculations. It was the introduction of new analytical methods which posed the most difficult problems in terms of re-educating designers, and the inclusion of these methods is one of the primary goals of current code revisions.

Caltrans seismic design methods rely on the basic concept that the strength of a structure, defined as its ability to resist a certain amount of force (i.e., its *capacity*), must be greater than the force it is likely to experience in use (i.e., the *demand* placed on it). In order to assess the viability of a particular structure, therefore, methods must be found to estimate both demand and capacity. Engineers find it useful to discuss these forces in terms of *moment*, which is the component of the force acting to bend a structure (say a column) relative to a certain fixed point (say the point where it meets the ground). (I introduce this term here only because it is used in the names of various techniques; the reader unfamiliar with this concept can simply read “force” in place of “moment” and get the basic idea.)

For the purposes of seismic design, the most important property of a structure is its *ductility*. When an engineer designs a structure for normal loads, such as the weight of vehicles, he or she tries to make sure that it is strong enough to respond *elastically* — that is, without suffering permanent deformation. It would be rather costly to design structures to respond to much more powerful seismic forces in an elastic manner. Instead, engineers assume that structures will exhibit some degree of deformation— also known as *plastic* behavior. For reinforced concrete structures, plastic behavior entails the stretching of steel reinforcing bars and the cracking of concrete. The challenge for the designer is to ensure that a structure can deform plastically over the many back-and-forth cycles of a large earthquake without losing too much strength and collapsing. Ductility is this ability to be repeatedly deformed without losing strength.

The problem with plastic behavior is that it is fundamentally *nonlinear*: unlike elastic behavior, the relationship between the force applied and the movement produced cannot be plotted as a straight line. This complicates engineering calculations considerably, so in many cases designers will assume linear behavior instead. But ductility improves a structure's ability to absorb the energy of an earthquake, so such an assumption considerably underestimates the amount of force a structure can take. If linear methods are used, this has to be taken into account through various approximations. Alternatively, more sophisticated and complex techniques of nonlinear modeling can be employed that calculate ductility effects directly.

In the period following the 1989 earthquake, the major change in design procedures at Caltrans was a switch from linear to nonlinear methods for estimating the capacity of a structure. However, the method for estimating the demand on a structure remained essentially unchanged. First, engineers calculate the period of vibration of the structure, using either a simple mathematical formula or a fairly sophisticated linear computer modeling program called STRUDL. Once the period of vibration is known, designers can determine the force a structure will be subject to from a given earthquake by using a chart known as the Acceleration Response Spectrum, which shows the period-force relationships that can be expected given the soil conditions at a particular site. Although some linear assumptions are made in this process, they aren't as significant as the linear assumptions made to calculate structural capacity, so the method is still regarded as being quite accurate.

The moment overload approach

Prior to 1989, Caltrans designers relied mainly on what is called the "moment overload" approach for comparing demand and capacity.[22] In this approach, the capacity of a structure — again, in terms of the moment, or force, it can stand up to — is calculated from simple linear models of the behavior of steel and concrete. The demand is calculated using the methods described above, and then the two forces are compared to one another. If the calculations were exact, you would want demand to always be just a little less than capacity. But the effects of ductility actually make a structure able to resist many times the force that linear methods would predict. Depending on certain characteristics of a structure, Caltrans code specifies that demand may exceed capacity by an "overload" factor between 4 and 8.[23] Although these factors are generally considered to be quite conservative, allowing a good margin of safety, they are a fairly imprecise way of taking the effects of ductility into account.

The displacement ductility approach

In the early 1990s, for reasons that will be outlined below, Caltrans engineers began to use a different approach for calculating capacity, using more sophisticated nonlinear methods to model the effects of ductility directly. In this approach, capacity and demand are expressed in terms of units of ductility, or amount of displacement beyond the elastic state, rather than force.

The first step in this process is to model the behavior of individual beams or columns using a "moment-curvature" analysis program, which calculates the force necessary to produce a given amount of bending. This calculation is complicated by the fact that a beam or column does not start to exhibit nonlinear, plastic behavior at the same time over its entire length. Instead, this behavior will usually start at one or more specific locations called *plastic hinges*. For reasons of stability, engineers try to design structures so that hinges will form only at specified locations, usually at the tops and/or bottoms of columns. The program assumes that hinges will only occur at these designated locations, but is able to calculate the amount of bending required to cause them to form. This analysis makes it possible to model the degradation of strength in the beam or column as bending increases in order to determine its ultimate capacity. This is also known as "pushover" analysis, because in the case of a column it essentially shows the sequence of failure that would occur if it were pushed over by a sideways-acting force. To model a complex structure, the moment-curvature relationships for individual beams and columns, along with their layout in space, are plugged in to another program which can then perform a pushover analysis on the entire structural frame. This type of analysis makes many assumptions about where nonlinear behavior will occur, but is still far more accurate than a simple linear approach.

Once the ductility capacity has been calculated using this method, it is simply compared directly to the previously calculated ductility demand. If capacity is greater than demand, the structure is considered strong enough. Because ductility is calculated directly, no “moment overload” factor is necessary, although a separate margin of safety is sometimes added on. Because ductility is derived from the displacement of a structure, this method is referred to as *displacement*-based, in contrast to the old approach, which was *force*-based.

Because the displacement ductility method produces more precise results and has a firmer analytical basis than its predecessor, it typically results in less conservative designs. In other words, it makes it possible for engineers to design structures with less excess capacity over the minimum required to meet demand. While this provides a smaller margin of safety in many cases, it also eliminates some of the uncertainty associated with the earlier approach. One effect of this is that the new method often results in significant cost savings over the older method, an important consideration at the height of the seismic retrofit program.

5.4 A history of displacement ductility analysis at Caltrans

Development and acceptance

In 1991, Caltrans was working on developing a retrofit strategy for the Santa Monica Freeway Viaduct, an 4-mile long elevated portion of Interstate 10 in central Los Angeles.[24] Bridges and elevated freeway structures are generally supported by sets of columns, along with associated beams and bridge deck supports, at various points along their length. Each of these structures is called a “bent.” In July of that year, Caltrans initiated a contract with two University of California, San Diego professors, Nigel Priestley and Frieder Seible, to serve as peer reviewers and in particular to carry out seismic analysis of two representative bents of the structure in order to “demonstrate the consequences of a more detailed and less conservative assessment procedure than currently adopted by Caltrans" — specifically, a displacement ductility-based approach like that described above, simplified by considering each bent as a stand-alone structure.[25] The contract was formally with a consulting firm the two professors had put together, SEQAD, in which they were the principals.

From the late 1980s through the 1990s, professors Priestley and Seible were among the most active members of the academic community in terms of their interaction with Caltrans engineers and impact on their design practices. Priestley is a world-renowned expert on reinforced concrete, a laid-back New Zealander recruited from the University of Canterbury. He was well-known to some at Caltrans even before he arrived in the United States because of the innovative engineering testing program he directed in New Zealand. Seible is a somewhat stern but good-humored German who has a less exalted reputation than Priestley, but is greatly respected within Caltrans for his sharp analytical skills and efficiency in running the structural engineering laboratory at U.C. San Diego. Both are known for their ability to relate academic research to practical design problems.

The two professors presented the results of their analysis to Caltrans project engineers in a meeting at U.C. San Diego on August 29, 1991. The minutes from this meeting report that, for one of the representative bents, "this preliminary analysis shows no retrofit is required for this bent. Conservatively, Nigel would recommend a steel casing around the lap splice of main reinforcement where the column and pile shaft meet. Although he feels this lap splice may be adequate even without the casing." Seible made a similar recommendation for the other bent.[26] These results were at odds with those produced by existing Caltrans methods. The designers had originally intended to do extensive retrofit work, in part involving the construction of bracing walls between the columns in each bent.

The new analytical method was debated at some length by Caltrans engineers at an internal strategy meeting two weeks later, on September 10. The marked discrepancy between the results of the two methods was the object of some debate. Some engineers gave the benefit of the doubt to the SEQAD results. One project designer, Mark Seyed, is noted as saying that “Caltrans appears to be overestimating moment demands and underestimating capacities as compared to U.C.S.D.” and another member of the design team stated that “our retrofit strategy is driven by excessive moment demands based on Caltrans criteria.”[27] When interpreted in this way, these discrepancies made the U.C. San Diego approach attractive, because it could result in great cost savings.

Mark Seyed[28] plays a prominent role in the events described here. He represents the younger generation of highly-trained structural specialists, with a Ph.D. degree from U.C. Davis and experience with the latest analytical techniques and computer tools. A low-key, technically-oriented engineer, Seyed was affiliated both with one of the design sections and with the special analysis group, and later with the Office of Earthquake Engineering. By virtue of this position, he came to serve as a mediator of sorts between the technical experts in earthquake engineering, both inside and outside of Caltrans, and designers. It was Seyed who saw a need to develop in-house computer programs and written documentation to guide designers in the application of the new analytical methods introduced by Priestley and Seible.

The two senior seismic experts, Ray Zelinski and Jim Gates, were more skeptical about the new approach. One participant in the meeting noted that Ray Zelinski had some concerns about the method. Jim Gates, the head of the Seismic Analysis group, was at the meeting himself, and he argued that “displacement demands procedure is used in the building industry and is a correct procedure, but we should be using the tools we have.” He was also skeptical of the “no retrofit” recommendation because SEQAD had only analyzed motion in the transverse direction. But, he conceded, “we can back off a lot, and [the] SEQAD results make some sense if we can accept some damage.” Taking a less cautious approach, Mark Seyed noted that the “SEQAD results show not only that no collapse will occur but that no major damage will occur. Caltrans can’t get our numbers to show that.” This prompted Gates to elaborate further on the reasons for his skepticism:

> In a long viaduct there will always be some surprises. That’s why we use a global model to see the ’big picture’ [when doing demand calculations]. We want to pinpoint those locations. This is why Jim Roberts and I have a problem with SEQAD assessment procedure because it only analyses [sic.] one bent at a time; it doesn’t look at the total structure. This goes against

> the Housner group and AASHTO. It is a good tool to look at an individual bent, but we can't throw out the global model because it pinpoints the weak areas that need closer attention.

In the end, one of the "key conclusions" listed in the meeting minutes was that

> Caltrans will use [the linear modeling program] STRUDL as a global model which will pinpoint any weak areas in the structure. The SEQAD seismic assessment procedure might be used to analyze those particular weak points but as a general practice has not yet been accepted by Caltrans.

At the next strategy meeting, on October 7, Mark Seyed presented his and another designer's interpretation of the SEQAD assessment methods, and by this time both Ray Zelinski and Jim Gates were less skeptical about the method, although Gates still emphasized the need for global analysis. He also thought the designers should get Priestley's concurrence on their interpretation of the approach. Another engineer reported on a meeting with Jim Roberts. Roberts was cautiously optimistic about the approach now, noting that he "does not want [the] Santa Monica Viaduct out of service because there is no convenient alternative route," but that the engineers "should take advantage of what we are learning, and not be over conservative in our retrofit designs." He suggested further testing to verify the method, and also offered "to write something in the future that says 'this is our criteria' so that designers can say they designed to 'the criteria of the day.' "[29]

By this point, the decision seems to have been made to go with the SEQAD approach. A few days later, the design team went to San Diego to meet with Nigel Priestley in person and discuss the implementation the new methods.[30] In the end, the initial "no retrofit" recommendation was not followed. Caltrans engineers apparently used some combination of the frame analysis proposed by Priestley and Seible and their own global modeling techniques. The final design called for a greatly reduced but still significant amount of retrofitting, putting five foot tall steel jackets on one out of every five columns and adding link beams, instead of walls, between some of the columns. The result was a total cost savings of between 30 and 40 percent compared to the initial Caltrans strategy.[31]

Extension and formalization

Displacement ductility analysis quickly began to become a standard design method at Caltrans. The story of how this happened centers primarily upon the work of Mark Seyed. Prior to the Santa Monica Viaduct project, Seyed had developed a basic column moment-curvature analysis program for use on other retrofits. Such programs became necessary, he recalls, because the basic purpose of steel jacket retrofits is to make columns more ductile by improving their ability to bend — their "curvature capacity." Although he had access to a moment-curvature analysis program written by researchers at U.C. San Diego, it was only capable of analyzing columns with circular cross-sections, and he needed a program that was capable of analyzing a rectangular column with an unusual rebar layout. So Seyed developed two computer programs in the BASIC programming language, COLx and BEAMx (where x stands for a version number), for analyzing columns and beams, respectively. These programs were based largely on moment-curvature programs he had written as a graduate student.[32]

These basic moment-curvature tools were already entering into design practice at Caltrans by the time SEQAD introduced their method of displacement ductility analysis for complete frames. As part of his effort to interpret this method for use at Caltrans, Seyed developed his own frame program, FRAMEx. This program used the moment-curvature analyses produced by COLx and BEAMx as input for generating a displacement ductility analysis of an entire frame. In writing this program, he also drew on his own academic background, not just on the SEQAD methods. It was at least partly adapted from work he had done in graduate school, and also from a course he had taught at Sacramento State University, in which he asked students, as an exercise, to produce a frame analysis program based on existing moment-curvature analysis programs. This reflects the fact that these methods, which were considered to be fairly radical by Caltrans engineers, were reasonably normal practice in the academic world.[33]

The displacement ductility approach began to be promoted throughout Caltrans by early 1992. A brief introduction to the method was included in the March issue of an internal earthquake engineering newsletter, appropriately named *What's Shakin'*, which was circulated to all of the design sections.[34] Information and user's manuals for COLx, BEAMx and FRAMEx were reproduced in the July issue. By this point, a number of engineers were using the programs, and papers by Seyed and others about the method were circulating within the organization. It is hard to say exactly how far and how quickly the method entered into design practice, but Seyed estimates that 30 to 40 percent of designers were using it by 1992. In April of 1993, a package was made available to designers which included many of these papers, along with the user manuals for each of the programs. This package included a memo, signed by design supervisors, which sanctioned the displacement ductility approach as an "approved alternative method."[35]

As the program became more widely used by designers, Seyed continued to add new features — new shapes, new rebar layouts — and it soon became apparent that BASIC was no longer adequate to the task.[36] To develop the programs further, Seyed would have to use the C programming language. This created some problems, because all programming at Caltrans is supposed to be done by the Division of Information Services, not by individual engineers. As long as he used BASIC, Seyed had been able to avoid scrutiny, but when the Special Analysis unit requested a C compiler, the Information Services people were not happy about it. After an extended period of negotiation, the unit was allowed to buy some C compilers, but of a brand that did not have the features Seyed needed.[37] So he had to purchase his own C compiler and avoided breaking the rules against programming by developing new versions of the programs on his home computer on his own time. In the package which accompanied the programs, he was careful to indicate that the source code was copyrighted and proprietary to him.[38]

The programs that Seyed developed in C, xSECTION and wFRAME, were introduced in 1994 and offered a number of improvements over the previous programs.[39] xSECTION replaced both COLx and BEAMx as a general-purpose moment-curvature analysis tool which could be used to analyze members of any arbitrary cross-section, including what Seyed refers to as "cruciform," "hammerhead," and "dogbone" sections. wFRAME also allowed more elaborate modeling of soil conditions, and allowed designers to break individual members down into segments with different cross-sections, which was useful, for example, for approximate modeling of flared columns.

Displacement ductility methods have entered into Caltrans design methods in much the same way as had been discussed in the Santa Monica Viaduct meetings. Global STRUDL models, as well as more sophisticated models, continue to be used on the demand side to identify points of vulnerability in complex structures. Displacement ductility programs have, however, become a standard method for calculating frame capacities. Seyed estimates, based on his experience providing support for the programs, that by 1998, 75 to 80 percent of designers were using his programs routinely.[40] Another engineer, more familiar with work in the design sections, put the figure closer to 65 or 70 percent.[41] These figures, while significant, do reveal a certain lack of consistency in the use of the method throughout the organization.

5.5 Managing change

In the atmosphere of pervasive and constant change at Caltrans during this time period, the state of the art of design practice became increasingly distant from what was recorded in formal codes. As these codes fell further and further behind, they became increasingly useless as a way of regulating design practice, and they played virtually no role in introducing new design methods. In response to this situation, a number of new strategies emerged for coordinating design work. First, the new design methods were introduced and standardized largely through face-to-face interactions, often in the context of particular design projects. Second, designers increasingly placed a great deal of trust in particular seismic experts, relying on them to render the new ideas coherent and workable. Finally, even as these new strategies were taking hold, Caltrans engineers were constantly working to develop written documentation of the new procedures, starting at a very informal level but eventually moving toward the development of new code provisions.

Face-to-face interactions

The Governor's Board of Inquiry into the Loma Prieta earthquake introduced a number of measures that sought to reform Caltrans design practices from the top down. One of these was a code rewriting project called ATC-32. This project, organized by a non-profit engineering group called the Applied Technology Council, brought together a number of prominent seismic design experts, many of whom also served as peer reviewers for Caltrans, to produce a complete revision of Caltrans codes. But this project, like all code writing processes, involved a lot of negotiation and did not produce a finished document until 1996.[42] Meanwhile, most of the important changes had already been introduced into Caltrans design practice through informal contacts among Caltrans engineers and between Caltrans engineers and outside peer reviewers on specific projects.

This was clearly the case, for example, with the introduction of displacement ductility methods in the context of the Santa Monica viaduct retrofit. Here, face-to-face contact and personal familiarity played a dominant role. The initial skepticism of senior seismic experts at Caltrans seems to have been overcome when other Caltrans engineers were able to come up with their own interpretation of the approach introduced by the U.C. San Diego professors. The soundness of this interpretation was further established through project strategy meetings and by the designers meeting personally with one of the professors. This personal interaction between Caltrans engineers and the outside reviewers continued throughout the design phase of the project. Introducing change in the context of specific projects like this seems to have afforded more opportunity for Caltrans engineers and outside reviewers to develop new design procedures collaboratively, helping to ensure that new methods were integrated with the existing routines and practical knowledge of designers.

This kind of face-to-face interaction was typical of design work at Caltrans during this period. One indicator of this was a great increase in the number and frequency of meetings and presentations:

> We would have, for example, design seminars, we'd have consultants coming in, we'd have Caltrans engineers making presentations, or consultants coming and making presentations, and all the work the consultants did, at some point during the work, they had to make presentations for the strategy committees, the peer reviews, many presentations, so it was a very open era.[43]

There was also a tendency for these meetings to cut across different levels of the organization. This proliferation of meetings was largely the result of the seismic retrofit program, which created a necessity for meetings simply because it greatly increased the number of design projects. But it also seems to be a strategy to maintain control over the design process during a period in which many of the relevant design procedures were changing frequently.

Within Caltrans, there have been a variety of working groups, committees, and organizational units which deal with seismic-related problems. The group which generally dealt with seismic issues prior to the 1989 earthquake was called SASA, which is variously described as standing for the Seismic and Structural Analysis unit or the Seismic and Special Analysis unit. It may have started out as the first and changed to the second, since meeting records from 1992 list SASA and Special Analysis as separate units, and Special Analysis merged with SASA later on. In addition to these two related groups, each of the 14 design sections had a senior seismic specialist. As the retrofit program became a priority, yet another group was created, the Seismic Technology unit (Seitech), which was specifically charged with developing retrofit techniques, although in practice it sometimes had a broader role.[44] Finally, there was a long-standing Earthquake Committee, one of a number of committees focusing on particular aspects of design — for example, there is also a foundations committee, a concrete committee, et cetera. This committee had 15 members in 1992, and was composed of engineers from SASA, Special Analysis, and Seitech, along with representatives of design and construction.[45]

While many of these groups existed prior to 1989, they increased in importance and became better staffed as the retrofit program developed. They also became more closely integrated throughout this period, culminating in the creation of the Office of Earthquake Engineering in the mid-1990s, which combined SASA and Seitech. There is other evidence that intra-organizational communication about seismic issues became more intensive during this time period. For example, despite the presence of design representatives on the Earthquake Committee, initially there was not very good communication between the two seismic units, SASA and Seitech, and the senior seismic specialists in the design sections. It also appears that these design specialists may not have communicated extensively amongst themselves. In 1992, a member of the Earthquake Committee was delegated to coordinate meetings of the senior seismic specialists in the design sections. This accomplished two goals: first, it allowed the senior seismic specialists to share information amongst themselves, and second, it provided an avenue of communication between the design units and the earthquake committee. The lack of communication between the seismic groups and the design sections was itself a recurrent topic in these meetings.[46] Creating more extensive networks of communication that cut across organizational units was one way of making sure that the latest seismic design methods were widely understood even if they weren't yet formally codified.

Another important example of the use of face-to-face meetings as a way of coordinating the design process was strategy meetings for individual retrofit projects. Strategy meetings are an adaptation of a standard element of the design process at Caltrans, the type selection meeting, in which engineers from design, construction, maintenance, and other groups are assembled in the early stages of the design process to decide on the basic structural concept for a particular bridge. In the context of seismic retrofit, however, these meetings took on added importance. Strategy meetings were supervised by members of SASA and Seitech, or later the Office of Earthquake engineering, and included the usual contingent from design, construction and maintenance, as well as district representatives and Caltrans geologists, if necessary.[47] The project designer, whether they were a Caltrans engineer or a consultant, would present a proposed retrofit strategy, discussing analytical results, proposed design details, background geological information, and usually a preliminary cost estimate. Engineers from the various divisions would then discuss the strategy, pointing out potential problems or suggesting alternate approaches.

This entire process was extensively documented, particularly when the designer was an outside consultant. For the meetings, designers prepared written reports detailing their retrofit plans. Afterwards, they were required to write up meeting minutes, including all the problems that were raised and their proposed solutions. If this was deemed satisfactory, the design could proceed without further extensive review; otherwise, another strategy meeting might take place. When designers were Caltrans engineers, this process might be relaxed a bit for more straightforward projects; sometimes a strategy could be approved just on the submission of a “strategy memo” to the head of SASA or the Office of Earthquake Engineering.

Jim Gates and Ray Zelinski describe several different reasons for making strategy meetings such an important part of the design process. One reason, Zelinski explains, was that

> we thought the best thing would be that if we had a core group of people that would be in on each and every strategy meeting, we could carry a constant theme through from designer to designer, you know whether it was in house or a consultant . . . it was just everybody was learning the stuff, and so as long as we had people that were seeing what was going into every project . . . we could help others by using that experience and passing it on to them.[48]

This concern with consistency in design across projects had several motivations. In the very earliest stages of the retrofit program, before it became a political priority, Caltrans had a very small budget to work with. According to Zelinski, part of the reason for having strategy meetings was simply to ensure that individual designers or design teams did not retrofit any one bridge more extensively than necessary, taking money away from other bridges that were equally in need. Even after the retrofit program was put on a fast track, keeping retrofit spending under control was still a concern, simply because of the huge number of bridges involved. It was also at this point that many projects were handed over to consultants, so strategy meetings became increasingly important as a means of overseeing and educating consultants who had little experience with Caltrans design methods.

Another reason for making strategy meetings so important was the complexity of the retrofit task itself. Retrofitting a bridge is a much more complicated process than designing a new one from scratch, because the engineer has to work around the unique features of the existing structure. Also, existing structures are quite variable because they were built at different times and according to different code provisions. Gates remembers that

> when I was in design [long before the retrofit program], the toughest jobs we had to do were widenings. They still are difficult. Where you have an old bridge, and you want to put an extra lane on one side, or two lanes . . . you have to deal with the old bridge and widen it. Well, retrofitting is a degree of magnitude more complex than widening, in that now you're dealing with the total structure, not just necessarily where the connection [between the old and new structures] is . . . it's just really a mess.[49]

The complexity of dealing with an existing structure, coupled with the complexity of the analytical methods common in earthquake engineering, created problems which taxed the ability of many design engineers. Having seismic experts in on the design process provided feedback to these designers, and served as a check that they were all following proper procedures.

Although some of the reasons discussed here for the use of face-to-face meetings to coordinate the design process have to do with the specific nature of retrofit work, underlying all of them is the difficulty of coping with rapid change in design methodology using formalized standards. A large part of the problem in this case was not the complexity of the retrofit task *per se* but the fact that retrofit projects had to be designed according to a constantly-evolving knowledge base, as research results flowed in and new analytical techniques were developed. Increasing the amount of personal interaction within the organization helped deal with this rapid change in two ways. First, since design was being carried out according to methods that were not in codes, there was an increased need for participants in the design process to communicate with one another directly in order to coordinate their activities. Second, designers, because of their wider responsibilities and lack of specialized training in seismic issues, could not always keep up with all of the latest developments on their own. Seismic experts, however, were in a much better position to keep up with the state-of-the-art, in addition to the fact that they saw many more retrofit cases than the average designer. They were therefore in a good position to ensure that the newest design guidelines were being followed consistently. Transcribing the knowledge of these engineers into written documents would have been a time-consuming process, when time was in short supply.

Experts and wizards

While the increase in face-to-face interaction throughout Caltrans engineering practice in this era no doubt promoted a sense of trust between all kinds of individuals, it had an interesting tendency toward focusing a great deal of trust and a great deal of the responsibility for regulating change on a select few. These individuals tended to fit one of two profiles: the “expert,” a person who is just known as being extraordinarily knowledgeable and competent, and the “wizard,” a term Susan Leigh Star uses to describe people in an organization who are able to move freely between different levels of technical work.

In public settings, expertise is increasingly established through the possession of academic credentials. Within the relatively small community of engineers and outside peer reviewers at Caltrans, however, the imputation of expertise seems to be based on more personal factors. The in-house seismic experts at Caltrans, for example, are not always the most highly-educated, but they do tend to be well-respected, experienced engineers who are known to many people throughout the organization. This became increasingly true through the course of the retrofit program, when many if not most Caltrans engineers interacted with them at one time or another.

But perhaps the best example of expertise being defined through personal qualities is the case of professor Nigel Priestley of U.C. San Diego. His name comes up more frequently than any other academic expert in conversations with Caltrans engineers at all levels, and he is almost always mentioned in a very positive light.[50] Many of the senior engineers and managers at Caltrans have had extensive personal interactions with Priestley, going back even before he moved to the United States, and know him very well. Among this group, he is seen as an individual with extraordinary qualities as a researcher. Jim Gates was initially impressed with Priestley on a visit to his laboratory in New Zealand:

> The thing that impressed us the most was the fact that they had one-year projects . . . and they had a huge amount of support staff at the university to help these guys get their projects done . . . and so these guys were cranking out really magnificent research projects in one year. And Nigel said he was gonna do that in San Diego, and when we heard that, we said listen, this is really great. And it turned out to be really good. They're turning out research, still, twice as fast or even more than that, than [another university] . . . Nigel cranks it out. Now, you get a report, you get something you can use right away, it's really good stuff. No comparison. . . . It's just that Nigel is just, then again, he's one of those guys that's just way above everybody else. He's just faster, better.[51]

Ray Zelinski, speculating on the reasons for the high quality of research at San Diego, expresses a similar view:

> I think a lot of it is from Nigel, from his background in New Zealand, I think the New Zealand people by nature are, the engineers there are more practical . . . they actually in their reports make the transition from laboratory to

> practice in their reports, they come up with practical means of implementing. In this country we don't do that. You know, we do a report, and you say this is the results of this, and then the conclusion is always to do more research to figure out how the heck to do this, you know, and it just drives us practicing engineers crazy, because we pay all this money and we don't always get a workable product. Whereas the stuff coming from Nigel, and I assume Frieder [Seible] because of his association with Nigel, is very much like that, you know, he's very good.[52]

It is apparent from these descriptions that the respect that Gates and Zelinski have for Priestley is based not simply on his credentials or some abstract knowledge of his reputation. Instead, it is based on personal knowledge of his research skills, his work habits, and his leadership style. It is also significant that their assessment of Priestley is based largely on the characteristics of the work coming out of the laboratories he has been associated with — with most of the credit going to him personally. This may legitimately reflect Priestley's role at these institutions. However, it may also have something to do with the fact that it is Priestley, and not the graduate students, technicians, or laboratory managers working under him, who is known personally to Gates and Zelinski. They place their trust in him, not in the laboratory. It also indicates that Priestley's expertise has a certain charismatic component to it. It is natural to attribute the success of these laboratories to Priestley because he is seen as being an extraordinary person.[53]

Positive opinions of Priestley also seem to have worked their way down the heirarchy at Caltrans, to designers and other engineers who have not had so much personal contact with him. In some cases this seems to come about through a general sense of Priestley's reputation, as with one Caltrans consultant who referred to him as a "concrete god," one of several people known to anyone working in the field. In other cases it seems to have more to do with the knowledge that people at higher levels in the organization have a positive opinion of him. For example, after meeting with the senior seismic specialists in the design sections, the delegate from the Earthquake Committee reported that "designers feel they are to do what Nigel Priestley recommends because they feel management is behind his opinion."[54]

Wizards are "both repositories of local knowledge about the social and technical situations, and simultaneously, they know enough of more than one layer [of technical work] to perform rare cross-layering coordination."[55] Though such people may be particularly important in computer engineering, the context in which Star describes them, almost any kind of design work involves a hierarchical division of labor that only certain individuals can cut across. During the 1990s, Caltrans design practice became even more hierarchical than usual because individual projects had to fit together to make up a much larger, coordinated retrofit program.

This created a greater need for wizard-like people, such as Mark Seyed. His central role in coordinating changes in the design process was not linked so much to his personality traits and reputation as it was to his unusual position at the intersection of several levels of technical work. First, Seyed was in a position to bridge the gap between Caltrans design work and the academic world because of his graduate degree and university teaching experience. Second, his work spanned layers within Caltrans. He was included among the highly-trained seismic experts in the Office of Earthquake Engineering, but his office was located on a different floor, near the design offices, and much of his work involved consulting with designers about how to apply his computer programs in particular design situations. These characteristics put him in a unique position to shape design practice while keeping up with the latest analytical developments.

During the period being discussed here, an extraordinary amount of trust was placed in individuals like Nigel Priestley and Mark Seyed. As Luhmann notes, trust can be a mechanism for simplifying what would otherwise be an overwhelmingly complex world.[56] Trusting a code, for example, frees a designer from a great deal of complicated analytical work which could strain the cognitive capacity of any one person. Trusting people can serve a similar purpose. Instead of learning everything about new design approaches themselves, designers relied on people like Seyed and Priestley to digest the relevant information and then tell them how to approach particular design problems. Wizards succeed in this role because they are able to perform the same kind of integration across work settings that is involved in writing good design codes. Experts like Priestley play an integrative role in design practice because they are widely perceived as authoritative figures, so their ideas are more easily accepted and implemented throughout an organization. But people, unlike codes, can be very flexible, continually integrating new methods and new data into their thinking. This is why individuals like Priestley and

Seyed tend to take over the coordinating role of codes during periods of rapid change.

Back to formalism

Shifting from codes to personal interactions as the dominant way of regulating design practice comes with certain costs. The two approaches are not strictly interchangeable. As the 1990s wore on, some at Caltrans began to feel that more informal, personalized mechanisms were not producing a sufficient degree of uniformity in design practice. One problem was with a lack of consistency between sources. Much of the information that engineers communicated to each other informally, the standards developed for particular projects, and various papers about design methods that were circulating among the designers were in some degree of conflict with one another. In the 1992 meeting of senior seismic design specialists, the inconsistency in design methodology between projects is discussed repeatedly. Specifically, some designers complained about the fact that the new standards were being applied to retrofit work, but not to the design of new bridges.[57] Now that the retrofit program is winding down, some engineers have tended to revert back to the requirements of the old code in their design work.[58]

More personalized mechanisms, then, evidently cannot standardize design practice to the degree that codes can. If new methods are too closely associated with particular projects and people, or with the particular context in which they emerged (in this case, seismic retrofit), designers may not think of them as universal standards of practice. One of the features of codes that makes them particularly attractive as agents of regulation is that they carry with them an implied universalism — in part deriving from their explicit legal and organizational sanction — which more informal standards may not, no matter how authoritative or trustworthy are the individuals who promote them.

Caltrans design codes

Caltrans has a complex, hierarchically organized set of codes. The highest-level, most generalized code document is the *Bridge Design Specifications*. This is an extremely formal document, broken down into numbered sections, each describing very general elements of the design process, such as loads, foundations, and reinforced concrete design, and giving specific design requirements in each area. The very terse format of this code demands a great deal of prior knowledge on the part of the designer. It is accompanied by a set of numbered commentaries corresponding to each section of the code. These commentaries are much more informal, and attempt to give some background on the reasoning behind code provisions.

On a level below these commentaries, there are *Memos to Designers*. These memos focus more narrowly on specific design issues and do not correspond to specific sections of the design specifications. Memos deal with issues like widenings, abutment design, and seismic retrofit procedures, and are written in a relatively informal and loosely-structured way. They are meant to be flexible instruments which can incorporate the latest developments in research and design practice.[59] The intended pattern is for memos to gradually become incorporated into commentaries and specifications, with the more informal material going into the commentaries and more specific requirements, when they are finally standardized, being incorporated into the specifications themselves.

Patterns of formalization

Attempts to formalize the new design procedures started almost as soon as they were introduced, even as practice continued to evolve rapidly. In the case of the displacement ductility approach, this process began with a memo written by Mark Seyed and another designer, distributed after the October 7, 1991 Santa Monica Viaduct strategy meeting, which was their effort to interpret the methods introduced by the U.C. San Diego professors.[60] This document lays out, in a simplified way, the steps involved in designing a structure using displacement ductility methods, and was instrumental in convincing high-level engineers and managers within Caltrans of the viability of the technique.

After this memo, perhaps the most important early formalization of the displacement ductility method was Mark Seyed's computer programs. Computer programs can be a particularly powerful type of formalization because they automated a significant portion of the design task. Although designers are ultimately responsible for knowing how the tools they use work, these programs were probably less amenable to idiosyncratic use than a written procedure would have been, particularly one without the status of a formal code. So the fact that the new techniques were introduced in the form of computer programs probably helped standardize their use.

The computer programs did not stand alone, however. They were accompanied by written materials of many different kinds. In 1993, Mark Seyed brought together a "Seismic Bridge Analysis Package" which was intended to be distributed along with the computer programs.[61] The structure of this package provides some interesting clues about the formalization process. Part of it was very basic documentation on how to use the programs COLx, BEAMx, and FRAMEx. But the bulk of the material consisted of many different free-standing papers describing different aspects of seismic design, dating back as far as the end of 1991. Significantly, a number of these papers had very little specifically to do with the use of the computer programs. Instead, the package appears to be an attempt to bring together all of the ideas and methods that a designer might need in the course of an overall design effort based on the displacement ductility approach. However, at this stage, there was very little attempt to try to mold these individual documents into a coherent whole. This was still a task each designer had to face in their own practice.

The second version of this package, released in 1996, had a new title, "Procedures in Seismic Analysis and Design of Bridge Structures," and had changed significantly.[62] Instead of a photocopied assemblage of stand-alone papers, most of the material has been set in the same type face. At the beginning of the package, there is a table of contents listing 11 Chapters, each organized conceptually with titles like "Structural Idealization," "Initial Proportioning" and "Data Synthesis." The document appears to be the beginning of an attempt to develop a complete set of design procedures based on the displacement ductility analysis, almost like a code, although the first page of the document makes it clear that this is not official policy, and it is still the work of just one person. Interestingly, though, the chapter format breaks down after Chapter 4, and the package once again degenerates into many independent papers, although many of them are now in the same typeface. In this document, we can see the formalization process in action, as one person attempts to reconcile a number of different sources into a single document. However, this kind of document is different than most codes because, although it does attempt to reconcile some divergent sources, it is has not been through the consensus-forging process of code writing, and it does not carry the organizational sanction that a code would — designers were still free to follow these procedures or not, as they saw fit.

By 1998, with the retrofit program winding down and the pace of change falling back to more normal levels, engineers at Caltrans saw an opportunity to start integrating the accumulated changes into a new seismic design code. They began an effort to bring all of the new approaches together, culminating in the publication of a new code document, "Caltrans Seismic Design Criteria," in July 1999.[63] Though part of Caltrans code, this document does not fit into any of the existing categories of code documents; its format falls somewhere between the formality of the Bridge Design Specifications and the looser structure of Memos to Designers. The hope is to eventually standardize the new seismic procedures to the point where they can be fully integrated into the Bridge Design Specifications, but this will involve another round of effort.[64]

The sources on which the Seismic Design Criteria are based reveal some of the other formalization efforts that were going on concurrently with Seyed's. Besides his work, they draw on the ATC-32 effort to revise Caltrans codes, research results from the universities, and procedures introduced in the course of individual projects.[65] The results of university research include not only the reports on particular tests, but also documents like one written up by U.C. San Diego, a higher-level formalization of design procedures which gives step-by-step instructions on retrofit design.[66] The criteria attempt to bring these disparate sources together into a single, coherent set of procedures for designers to follow.

Code development, at least at Caltrans, is just one among many steps in a continuous process of formalization. As design practice evolves, various issues crop up which suggest a need to produce some kind of written document. Sometimes, these documents can take the form of papers or notes that are circulated informally; other times, brief informal memos are written to communicate a particular issue to designers throughout the organization. In this case, informal documents like these went through an intermediate stage, in which they were compiled and some effort was made to reconcile them. Finally, these sources are brought together in several layers of code documents, which become increasingly generalized and abstract. At each stage in the process, more references to design work as it is experienced by designers in the course of working on particular projects are deleted. Extending the formalization process over time in this way makes it possible to gradually accommodate written documents to design practice and to adjust them to the needs of different parties, making the final stage of code writing a bit less complicated.

5.6 Conclusion

Both codes and more informal, personalized mechanisms can play a role in coordinating the design process. In most cases, design work depends upon a closely integrated combination of the two: the practice of a designer may be shaped by codes, interactions with colleagues, and expert advice all in the course of a single design task. At Caltrans, the period following the 1989 Loma Prieta earthquake was somewhat unusual because this close connection was temporarily disrupted, and many aspects of design that had been formalized were carried out instead through more informal social mechanisms for an extended period of time until codes caught up with changes in practice. This disruption in organizational routine demonstrates, in a particularly vivid way, that shifting certain tasks — in this case, the task of coordinating the design process — from representations to people is not a neutral event. It changes the way work is organized and forces individuals to alter their work practices in significant ways.

To engineers, coordinating the design process is largely a problem of regulating or controlling the practices of individual designers. Design is made more tractable at the collective level at the expense of restricting the autonomy of the individual practitioner. Codes and collegial interactions are distinct mechanisms for regulating design practice which succeed at restricting the actions of individual designers in different ways.

Codes are simultaneously very strong and very weak means of regulation. They are strong because they are typically the result of a process of negotiation and consensus-building, and are generally perceived as legitimate by designers. Because of this, they are slow to change, and this stability often reinforces their legitimacy. They are also strong because they are very abstract, divorced from the specific details of any single design project. This makes them very broadly applicable, which enhances their authority. Because of this abstraction, they may also be able to reconcile and render coherent divergent elements of the design process, which is part of what makes them such useful tools for designers. The legal protection which codes afford to designers also should not be overlooked.

The abstraction of codes is also part of what makes them weak agents of regulation. The more abstract a code is, the more interpretation it requires of the designer to apply to any particular case. Paradoxically, the very broad applicability of codes limits the degree to which they can specify design practice, leaving room for more variation in certain elements of the design process. Because of this, codes may be most effective for setting minimum design standards.[67] They are not particularly effective as tools for regulating the whole design process in a comprehensive way. Also, as this chapter shows, their stability makes it difficult for codes to respond to dramatic changes that are not introduced in an incremental fashion. During periods of rapid change, therefore, codes may come to be seen as static and outmoded.

This chapter has examined several alternate modes of regulating the design process which, in contrast to codes, derive their power from their embeddedness in local circumstance and personal interaction. Design practice was regulated during this period of change through the introduction of new methods on a project-by-project basis, by an increased reliance on meetings and face-to-face transmission of information, and through reliance on respected experts and on “wizards,” individual engineers who have the ability to move between different levels of the organization.

Like design codes, these forms of regulation have their strengths and weaknesses. The strength of project-specific and face-to-face modes of regulation lies in the fact that they make it possible to exert comprehensive control over the design process, and not just to set minimum standards. The groups who are in positions of authority — in this case, Caltrans management and seismic experts, and outside peer reviewers — are in direct contact with designers and are able to intervene wherever they see a problem. The local character of this sort of regulation is also a source of weakness, however. One reason for this is that it depends upon a kind of continuous surveillance of each individual project to operate. The effectiveness of this kind of regulation tends to diminish dramatically outside of the immediate realm of surveillance, as demonstrated by the failure of some designers to carry the new methods, introduced in the course of the retrofit program, into their standard practice. This may also make it more applicable within the context of a particular organization — it would be harder to apply this kind of regulation at the same broad level as nationally recognized building codes.

Another problem with local and personal forms of regulation is that they tend to produce many different guidelines for practice, each tailored to the specifics of a particular project. These guidelines are often in conflict with one another, making it difficult for designers to follow any particular standard. Experts can help reduce this conflict if their ideas are seen to be more widely applicable than others, but this only seems to work to a certain point.

Because both codes and more informal forms of regulation have strengths and weaknesses, the most thorough way of maintaining certain common standards in engineering practice is a combination of the two. If a route of excessive routinization and codification is followed, there is a danger that only minimum quality standards will be maintained, and that engineering organizations will lose some of the flexibility needed to cope with significant change when it does occur. If, on the other hand, such codification were abandoned, engineering practice would become more irregular between and even within organizations — not the kind of image a modern profession wants to project — and that individual engineers would be deprived of a set of tools that helps them make their practice more efficient.

The relationship between formalism and design practice is in a constant state of flux, more so at some times than at others. Changes in practice are continually being incorporated into various formal documents and computer programs, and eventually into codes. These specifications are, in turn, used by designers as tools, and in this capacity have an influence on practice — and in use they are often transformed or incorporated into new formal representations. The dynamic tension here is not simply between codes and practice. At Caltrans, it is apparent that practices are constantly being turned into representations of various sorts. Even during a period when codes were not being significantly updated to reflect changes in practice, engineers were continually turning out informal papers, newsletters, and computer programs in order to communicate with each other and as tools for their design work. Engineering practice, in its modern form, could scarcely survive without such formalizing activity. On the other hand, engineering practice clearly cannot be sustained through reliance on formal representations alone: when formalisms attempt to specify too much of practice too rigidly, engineers often find ways of working around them.[68] Since neither of these extremes is desirable, practices are constantly being translated into representations, which are themselves transformed and refined over time, and then feed back into practice. The moments when a new practice emerges at a completely informal level and when a code provision is adopted are just two endpoints of this very complex and dynamic process.

Notes

[1] Governor's Board of Inquiry on the 1989 Loma Prieta Earthquake 1990, 48.

[2] Interview, Mark Seyed Mahan, August 27, 1998.

[3] Printout of electronic message, Apostolo to Weaver, May 20, 1991, "Subject: Outstanding Teamwork on Embarcadero/Central," EF 29. All ellipses and capitalized words in the original.

[4] Interview, Thomas Ostrom, August 27, 1998.

[5] Latour 1987, 227-228.

[6] See Star and Griesemer 1989.

[7] Hutchins 1995, chapter 3.

[8] Henderson refers to these drawings as *conscription devices* to emphasize the power relations involved in their use as organizing tools. See Henderson 1999, 51-57.

[9] Bucciarelli 1994, 132.

[10] Collins 1992, Collins 1990.

[11] Collins derives this argument from Winch 1958.

[12] Star 1995, 98. See also Suchman 1987 on the relationship between plans and situated actions and Vaughan 1996 on the connections between rules, norms and decisions in large engineering organizations. On the problematic relationship between organizational rules and work more generally, see Gouldner 1954.

[13] Bucciarelli 1994, 132.

[14] This is what Ed Hutchins has in mind when he says of navigational devices, "These tools permit the people using them to do the tasks that need to be done while doing the kinds of things the people are good at." (Hutchins 1995, 155.) It should be noted that the ability of codes to work in this way depends greatly on the fact that codes are generally written primarily by engineers and for engineers. When rules, codes, and other representations of work practices are imposed from the outside in a way that is not sensitive to local work practices, problems can sometimes result. See, for example, Henderson 1999, which describes the negative effects of the imposition of a CAD/CAM system developed by computer specialists on design and production at turbine company.

[15] Bucciarelli 1994, 132.

[16] Latour 1992, 233.

[17] See, for example, Latour 1987, Latour 1996, Callon 1987, Law and Callon 1992.

[18] Hutchins 1995; this argument is summarized on pages 353-374.

[19] Luhmann 1979, 46; Giddens 1990, 16-21.

[20] Luhmann 1979, chapter 7; Giddens 1990, 21-36, 88-92.

[21] Luhmann 1979, 45-46; Giddens 1990, 79-88.

[22] The descriptions of Caltrans design methods in this section follow Mark Seyed, "Ductility Analysis for Seismic Retrofit of Multi-Column Bridge Structures, Santa Monica Viaduct," pages 12-19 in Seyed, "Seismic Bridge Analysis Package," April 1993.

[23] Caltrans *Bridge Design Specifications*, June 1990, section 3.21.1.2.

[24] "Overview: Santa Monica Viaduct Retrofit," November 7, 1991, EF 28.

[25] Seismic Safety Review Panel, "Seismic Safety Review of Santa Monica Viaduct Retrofit," November 8, 1991, EF 28.

[26] "Minutes — August 29, 1991 Peer Review Meeting at U.C. San Diego, Santa Monica Viaduct Seismic Retrofit Br. No. 53-1301," EF 28.

[27] These quotations and those in the subsequent paragraph are from "September 10, 1991 Strategy Meeting Minutes, Santa Monica Viaduct Seismic Retrofit Br. No. 53-1301," EF 28.

[28] Later known as Mark Seyed Mahan.

[29] "October 7, 1991 Strategy Meeting Minutes, Santa Monica Viaduct Seismic Retrofit Br. No. 53-1301," EF 28.

[30] "Minutes — Oct. 11, 1991 — Review Meeting at U.C. San Diego — Santa Monica Viaduct Seismic Retrofit Bridge No. 53-1301," EF 28.

[31] Rosenbaum 1995.

[32] Interview, Mark Seyed Mahan, August 27, 1998.

[33] Ibid.

[34] Mark Yashinsky, "Calculating the Displacement Capacity of Columns," WS, March 1992.

[35] Memorandum, "re: Displacement Ductility Approach," signed by Jerry A. McKee and Floyd L. Mellon, April 30, 1993, page 3 in Mark Seyed, "Seismic Bridge Analysis Package," April 1993.

[36] Mark Seyed, Memorandum, "re: Engineering Software for Seismic Design of Bridges," March 7, 1994, section 2, page 1 in Seyed Mahan, "Procedures in Seismic Analysis and Design of Bridge Structures," May 31, 1996.

[37] Interview, Mark Seyed Mahan, August 27, 1998.

[38] Mark Seyed, Memorandum, "re: Engineering Software for Seismic Design of Bridges," March 7, 1994, section 2, page 1 in Seyed Mahan, "Procedures in Seismic Analysis and Design of Bridge Structures," May 31, 1996.

[39] Ibid.

[40] Interview, Mark Seyed Mahan, August 27, 1998.

[41] Interview, Thomas Ostrom, August 27, 1998.

[42] Applied Technology Council 1996.

[43] Interview, Mark Seyed Mahan, August 27, 1998.

[44] Interview, Ray Zelinski, August 25, 1997.

[45] Committee members are listed in a number of Earthquake Committee meeting minutes, which were included in the *What's Shakin'* newsletter during this time period.

[46] Memorandum, Brian Maroney to Steve Mellon, March 18, 1992, "re: Notes from the First Meeting of the Seismic Senior Design Specialists," WS, April 1992.

[47] "Strategy Meetings," undated memo obtained from a designer at the Caltrans District 11 office, May 30, 1997.

[48] Interview, Ray Zelinski, August 26, 1997.

[49] Interview, Jim Gates, August 28, 1997.

[50] Some caution is in order in interpreting this phenomenon, however, since the author was a student from the same university as Priestley.

[51] Interview, Jim Gates, August 28, 1997.

[52] Interview, Ray Zelinski, August 25, 1997.

[53] This is an aspect of what Bruno Latour refers to as the "secondary mechanism," by which one person comes to be seen as responsible for the actions of many (Latour 1987, 119). This phenomenon is discussed in connection with Louis Pasteur in Latour 1988, 42-43 and 71-72. Charismatic leaders in technical fields are discussed in MacKenzie and Elzen 1996, which focuses on computer designer Seymour Cray, and in Thorpe and Shapin 2000, which examines Robert Oppenheimer's role at Los Alamos.

[54] Memorandum, Brian Maroney to Steve Mellon, March 18, 1992, "re: Notes from the First Meeting of the Seismic Senior Design Specialists," WS, April 1992.

[55] Star 1995, 107.

[56] See Luhmann 1979, chapter 1.

[57] Memorandum, Brian Maroney to Steve Mellon, March 18, 1992, "re: Notes from the First Meeting of the Seismic Senior Design Specialists," WS, April 1992.

[58] Interview, Thomas Ostrom, August 27, 1998.

[59] In addition, *Bridge Design Aids* provides detailed descriptions of design procedures in a format much like Memos to Designers, and *Bridge Design Details* give examples of standard structural types and configurations — showing, for example, standard methods for connecting reinforcing steel. These two documents seem to be considered as auxiliary to the main thread of the code, however.

[60] Mark Seyed and Ali Asnaashari, "Ductility Concepts Applied to Santa Monica Viaduct." Attached to "October 7, 1991 Strategy Meeting Minutes, Santa Monica Viaduct Seismic Retrofit Br. No. 53-1301," EF 28.

[61] Mark Seyed, "Seismic Bridge Analysis Package," April 1993.

[62] Mark Seyed Mahan, "Procedures in Seismic Analysis and Design of Bridge Structures," May 31, 1996. Thought it is labeled "draft," I believe this version of the package was widely used at Caltrans.

[63] "Caltrans Seismic Design Criteria," Version 1.1, July 1999. Available (May 14, 2000) on the Caltrans Office of Earthquake Engineering and Design Support web site at http://www.dot.ca.gov/hq/esc/earthquake_engineering/SDC/sdc.pdf.

[64] Interview, Thomas Ostrom, August 27, 1998.

[65] Ibid. See also Applied Technology Council 1996.

[66] Priestley *et al.* 1992.

[67] Gouldner 1954, 174-177, argues that bureaucratic rules in general may enhance worker apathy about job performance by providing a minimum set of standards they can work to meet but not exceed.

[68] See Henderson 1999, 98-102. Henderson describes how a very inflexible CAD/CAM system created problems for designers and production workers, which they responded to by developing informal methods for circumventing the system.

Chapter 6

A Chain of Practices: From Laboratory to Design Floor

6.1 The setting

The Charles Lee Powell Structural Research Facility at the University of California, San Diego — known as the Structures Lab to those familiar with it — occupies several buildings near the center of the campus.[1] The two principal facilities are large, warehouse-like buildings that sit across the street from one another, set back by walled concrete aprons which are used for storage and construction. The fronts of the buildings open out onto these aprons via massive sliding doors, forming a continuous indoor-outdoor workspace (Figure 6.1).

Most of the work being done at the laboratory on a given day is related to the construction of large concrete test specimens (Figure 6.2). These range from fifteen foot tall column sections to full-scale columns of thirty feet or more. On one occasion, a full scale model of a five-story building section was tested inside the laboratory. While outside contractors are brought in to construct some of the largest test specimens, most of the construction work is done by the technicians and graduate students. They tie steel reinforcing bars into cages, build wooden forms, and pour concrete, all physically demanding tasks. Standard work clothing for everyone includes a hard-hat, concrete-spattered jeans, and steel-toed boots. When testing finally begins, the instruments,

Figure 6.1: An exterior view of one of the main buildings of the Structures Lab. Photograph by the author.

cables, signal-conditioning cabinets and computer equipment which are more typical of laboratory research make an appearance, but they look somewhat out of place sitting on the dirty concrete test floor. This kind of juxtaposition is what prompted the laboratory manager to describe the Structures Lab as a "construction environment that does scientific research."[2]

Most, though by no means all, of the research done at the laboratory is funded by Caltrans. The laboratory's close relationship with Caltrans stemmed initially from connections between Caltrans engineers and professor Nigel Priestley established when he was still working in his native New Zealand. Caltrans was eager to continue the relationship when Priestley moved to U.C. San Diego in the mid-1980s, before the Loma Prieta earthquake. After the earthquake, and as the seismic retrofit program became a major priority, Caltrans came to rely on Priestley and his colleague at the laboratory, Frieder Seible, for advice on seismic matters. It also channeled a great deal of research work their way, in part because of a perception that the laboratory provided more practically-useful results than facilities at other universities. Research on concrete

Figure 6.2: The laboratory as a construction site: A technician and a graduate student pour concrete into wooden forms to cast a test specimen. Photograph by the author.

columns encased in steel shells that was begun in New Zealand by Priestley and others, and continued at the laboratory, became the basis for Caltrans' standard retrofit approach.

Passing by the laboratory buildings, one's attention is immediately drawn to a forest of variously mangled concrete beams and columns which loom over the walls surrounding the driveways. Several similarly mangled columns have been set up in rows on either side of the pedestrian walkway approaching the laboratory, as strange monuments to what goes on within the laboratory. Large chunks of their surface concrete are missing, revealing the skeleton-like steel reinforcing bars within. In this Southern California setting, these ruined columns make a powerful statement, standing as tangible symbols of the fragility of the built environment in an area prone to earthquakes. The symbolism is not accidental. The damage that has been done to these columns is meant to stand for, in a methodologically rigorous way, the damage which earthquakes may cause to California's transportation infrastructure. This sort of projection from the controlled world of the laboratory to the performance of technology in the field is a central characteristic of all technological testing.

6.2 Testing and projection

Projection as a similarity relationship

Substantial progress has been made toward understanding the distinct epistemological issues which testing can raise. In particular, Donald MacKenzie and Trevor Pinch have drawn attention to the idea that a test's credibility is secured by establishing a similarity relationship between a test situation and the conditions a technology faces in actual use. Pinch introduces the term "projection" to describe this relationship.[3] However, the STS literature generally has not looked in detail at how testing practices are situated within particular material and organizational settings. In the sociology of science, these aspects of experimental research have been addressed in a number of laboratory studies.[4] By placing scientific experiments in the organizational contexts of specific laboratories, these studies demonstrate how the construction of scientific facts is grounded in the mundane, everyday work of laboratory personnel. Following this example, this chapter seeks to explain how testing, and the process of projection which it involves, is embodied in work practices in and around an engineering laboratory.

The study of testing in a laboratory context highlights the importance of two aspects of scientific and engineering research which have not been extensively studied: the division of labor among differently-skilled people both inside and outside the laboratory, and the distribution of the work of producing and interpreting experimental results across culturally distinct work settings. As the preceding brief description of the laboratory indicates, testing there draws upon and is relevant to a wide range of social settings, from the world of construction work to design engineering to state transportation policy. I argue that projection should be understood not as the establishment of a similarity relationship between testing and use of technology, but as a more general process through which the work practices and local knowledge of the test site are made understandable in other social settings through the movement of people and things between them. In this way, what happens at the test site is made to have an impact on the practices of engineering researchers, designers, policy makers, and others involved in technological production.

MacKenzie and Pinch, each writing generally in what is known as the "SSK" ("Sociology of Scientific Knowledge") tradition, make similar arguments based on the premise that "all the issues that recent sociology of science has raised about *experiment* in science can be raised about *testing* in technology."[5] In particular, they draw on the fundamental work of H.M. Collins on the replication of experiments.[6] Collins argues that an experiment, in itself, can never resolve a point of scientific contention because it is always possible, in principle, to challenge elements of experimental procedure. When an experiment produces a controversial result, it is generally possible for scientists to find fault with the experimenter or the methodology and dismiss the result as spurious. When an attempt is made to replicate an accepted experimental result, negative results can be explained away on similar grounds. In these disputes, judgments of similarity often play a crucial role: for example, important questions can arise about whether or not one experiment has been done under similar enough conditions to count as a legitimate replication of another.

Both Pinch and MacKenzie argue that projection can be analyzed in these terms because it also involves judgments of similarity: not between two tests, but between a test situation and the actual working conditions of a technology. In order for a test to produce meaningful information about the performance of a technology in use, practitioners must believe that these two circumstances are similar in certain essential respects. They take this to be the central issue that must be addressed in the sociological study of testing.

MacKenzie provides a detailed case study of the controversies surrounding nuclear missile testing, which shows how projection can become problematic when the similarity between test conditions and operating conditions is called into question. The tests he describes are meant to simulate, as closely as practically possible, the conditions a missile would face upon launch at the Soviet Union in time of war. In these tests, the warhead of a selected, operational nuclear missile is removed, and the missile is transported from its silo to a test range, where it is instrumented and then launched. In debates about whether to rely primarily on bombers or missiles, critics pointed out a number of ways in which these missile tests failed to accurately duplicate wartime conditions. For example, they claimed that the missiles themselves were given special treatment and maintenance prior to testing, and that there could be significant differences in the earth's gravitational and magnetic fields on the test range and along a flight path toward the Soviet Union. In this way, the results of tests that may have seemed straightforward and credible to some were seen as invalid by others. From the perspective of the second group, a projection could not reasonably be made between a test and the performance of a missile in war.

Projection is mediated and embodied

MacKenzie's case study of missile testing, like other examples given by Pinch, presents a very convincing case that the credibility of testing often depends upon social convention and judgments of similarity. This observation is a good starting point for the sociological analysis of testing, but it focuses mainly on issues of language and the representation of reality, as indicated by MacKenzie's claim that the product of "(socially) successful testing" is "a credible statement of a certain form."[7] This reflects MacKenzie and Pinch's general concern with questions of knowledge and belief, which is in keeping with SSK's origins in a critique of positivist and rationalist approaches to the philosophy of science. While these issues are certainly important, they don't fully capture some of the subtleties of how testing relates to technical work. Testing doesn't only generate new technological *knowledge*; it also generates new forms of technological *practice* — tacit knowledge, embodied skills, and other forms of working knowledge of technological artifacts. Projection, therefore, should not be understood just as a process of establishing similarity relationships, but rather as a more general process by which the practices of testing are more or less effectively linked to a wider context of technological practice. As I will explain in more detail below, this linkage is crucially dependent upon the movement of skilled people, material objects and symbolic representations across social settings.

The importance of embodied skill in scientific practice is convincingly demonstrated in the work of H.M. Collins on the replication of experiments.[8] Collins studied the experimental replication of a new type of laser, the TEA laser, in various laboratories.[9] He found that successful replication depended largely upon unarticulated "tacit" or "skill-like" knowledge. This was reflected in the fact that research groups had difficulty making working lasers if they relied on written information alone. Generally, only those scientists who were able to maintain extended personal contact with personnel from other laboratories who had already produced a working laser were able to build their own. While such a strong dependence on personal contact might not be found in every case, Collins makes an important point: there is an aspect of experimental practice which is embodied, and not easily formalized. So it is not enough to analyze replication just in terms of similarity judgments; some description of the way people move between work sites is necessary as well.

Applying these considerations to projection introduces some new complexities. The replication of experiments generally involves establishing a common set of practices and artifacts across different laboratories. These laboratories may be geographically dispersed, but they are very similar social settings: they are organized around common goals, populated by comparable groups of people, and often make use of identical tools and instruments. The similarity judgments scientists make when comparing experiments generally presuppose this kind of cultural homogeneity. In contrast, projection involves making a connection between the practices of the test location and a larger world of technological practice, which may include social settings which are quite different from the test setting. These social settings may be organized around very different goals, be populated by different types of people, and involve quite different sets of practices and technologies. The missile tests which MacKenzie discusses, for example, may be carried out by specialized research staff on test ranges in the Pacific Ocean, but the results are taken up (or not) by missile designers, the nuclear strategy community both inside and outside the military, and by policy analysts and politicians who operate in a more public arena. Because these settings are differentiated specifically by the fact that they involve distinct sets of practices and distinct ways of relating to technology, projection, by definition, cannot be accomplished simply by establishing a common set of practices across different locations. Instead, for projection from test to use to be successful, some kind of translation or coordination has to take place between social settings which remain distinct.

The coordination of work across social settings is also one of the central topics of the “social worlds” perspective within interactionist sociology and the sociology of science and technology.[10] This approach is a very useful starting point for analyzing projection. As Anselm Strauss and others have argued, one of the central tasks for an analyst of social worlds is to look at how these worlds and the smaller segments within them intersect, and describe the various mechanisms that make such intersection possible.[11]

One way that these intersections can be managed is through “marginal people” who are members of more than one social world.[12] Such people can help solve the problem of projection, because by virtue of their membership in adjacent social worlds, they may possess the skills and tacit knowledge of both. Because they understand the work practices of different worlds, they are in a unique position to help coordinate or translate work practices between sites. Also, by moving between social worlds, they may bring them closer together — for example, by bringing elements of the skill and knowledge of one world to the practices of another. Susan Leigh Star and James Griesemer have introduced an original variation on this idea. In the context of scientific research, they argue that marginal objects, as well as marginal people, can be used to connect different social worlds. These "boundary objects" are "those scientific objects which inhabit several intersecting social worlds . . . *and* satisfy the informational requirements of each of them." They are "both plastic enough to adapt to local needs and the constraints of the several parties employing them, yet robust enough to maintain a common identity across sites."[13]

Projection can be understood, then, as a process in which the practices of different social worlds, or segments of these worlds, are linked through the movement of certain people and objects across the boundaries between them. This creates a continuous *chain of practices* between social settings. It is this chain of practices which makes it possible for test results to have an impact on the practices and ways of thinking of a wide range of social actors, both inside and outside of technical work.[14] In other words, it is a central feature of projection. Returning once again to MacKenzie's case study, we might be able to describe how test results are made available to missile designers through statistical analyses and test reports, or through the participation of designers in the testing process. The missiles themselves could be described as boundary objects between the test setting and the world of missile silo operations, carefully selected and transported to the test range so that the test launch can be considered (at least by some) to be just the same as a launch from a working silo. At the other end of the process, it might be that certain Congressional staff members with backgrounds in nuclear weapons policy play a crucial role in translating the technical details of missile performance into a form which is useful to members of Congress as they work to formulate positions in the public arena.

The stability of the projection relationship — that is, the strength of the chain of practices — may depend on a number of factors. For example, links between social worlds which involve more intense circulation of people and objects are likely to be stronger. In the hypothetical scenario presented above, the link between the social worlds of missile testing and missile design is likely to be particularly strong because actors from these worlds may work together and exchange information frequently. On the other hand, politicians and their staff members may have little interaction with missile testers or even designers, getting most of their information from written reports. Because of this lack of interaction, they may be more likely than designers to be skeptical of test results. But this need not be a universal tendency, particularly in cases where the testers might have a great deal of cultural authority.

Also, test results can more open to challenge the more complex the chain of practices becomes.[15] If a long chain of measurements, calibrations, and modeling techniques is necessary to bridge the gap between testing and use, there may be many points at which critics could challenge the connection. But a very simple test, such as Pinch's example of saying "testing . . . one, two, three . . . testing" to see if a microphone is working, may be difficult to dispute.[16] Of course, the simplicity or complexity of a chain of practices is itself dependent upon social convention, as when certain elements of a chain are "black boxed" to the extent that they cease (at least temporarily) to be seen as sources of additional complexity.[17]

6.3 Work settings at the Structures Lab

In my analysis of the Structures Lab, I describe a chain of practices that extends between a series of *work settings*, rather than distinct social worlds. Work settings, like social worlds, can be characterized by certain activities, by the particular sets of skills and artifacts employed in these activities, and by a common working relationship to technology on the part of participants.[18] However, work settings generally correspond to particular locations in which specific work tasks are carried out, rather than to a broader arena of activity. This unit of analysis is more appropriate for the detailed description of testing and engineering practice given here.

In the Structures Lab, there are four work settings which play a role in the testing and projection process (Figure 6.3). The first setting is the laboratory itself, which is located within the buildings described in the introduction. This is the setting where test specimens are built and the actual activity of testing takes place. This work

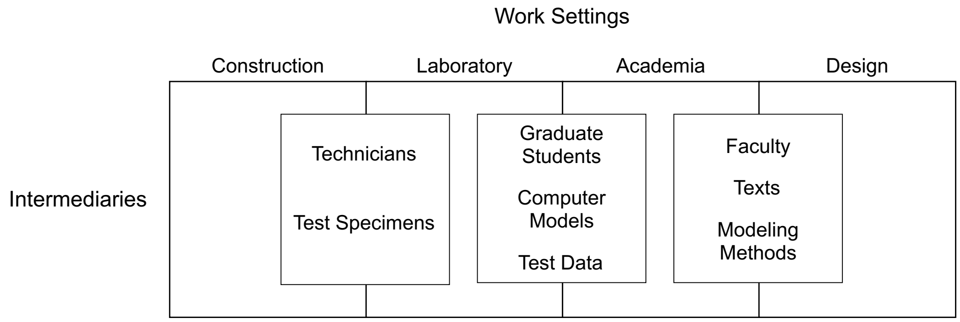


Figure 6.3: A simplified diagram of the chain of practices that links the Structures Lab to other work settings, making projection possible.

is done by technicians, as well as by assorted graduate students and postdocs carrying out specific research projects. Next is the academic work setting, which is associated primarily with the graduate student and faculty office area, which is separate from the laboratory. Here, work revolves around planning tests, processing data using computers, writing papers and otherwise communicating with colleagues. Most of this work is done by graduate students under the supervision of faculty members. Finally, the design work setting is associated with Caltrans offices in Sacramento. Here, engineers design new freeway structures, in part based on information gained through testing. Academic researchers occasionally appear in this setting as consultants or design reviewers.

Besides these three discrete work settings, work in the laboratory setting is based on the practices of some work settings which are so widely distributed that they could almost be considered social worlds in themselves. The most prominent of these is the construction industry. Technicians hired from this setting bring to the laboratory skills in carpentry and steel and concrete work which are necessary for the construction of test specimens. Other technicians bring in expertise gained through outside work in a variety of other areas, such as electronics and hydraulics.

The relationship between work settings and the human, material, and symbolic intermediaries which connect them can be a complicated one. In one sense, work settings are like segments of social worlds, organized around a particular set of activities, but in other ways they are more like zones of intersection between social worlds. Laboratories are a good example. On the one hand, they are their own little social worlds, populated by various groups of people working around a common goal. On the other hand, the different groups of people involved in laboratory work can themselves be considered part of distinct segments of a larger social world. Scientists, for example, may see their laboratory work as continuous with a set of related activities, like attending conferences and writing papers for publication. Some technicians, however, participate only in laboratory work, but may be involved with aspects of this work that scientists do not or cannot participate in. Each group is likely, as well, to make use of distinct sets of technological artifacts. In the laboratory, these worlds intersect.

The role of work settings as intersections between segments of social worlds makes them similar to what Peter Galison has called "trading zones": locations in which members of divergent social groups are able to successfully interact while maintaining distinct cultural identities.[19] Because of this characteristic, the work settings described here are connected to one another in a subtly different way than social worlds. Although social worlds are connected primarily through marginal people and marginal objects, this is not necessarily the case with work settings. Work settings are much smaller units than social worlds, so it is much easier for a given actor to be a participant in more than one work setting; in fact, most actors will participate in more than one. Work settings are therefore connected through a generalized division of labor and circulation of technology, not just through certain marginal people and objects.

6.4 Technicians and the division of labor

While many ethnographic studies of scientific laboratories have been oriented toward understanding the work routines of scientific research, very few actually discuss the distinct roles that faculty, technicians, and students play in the division of labor in the laboratory, even if their work practices are described in great detail.[20] The Structures Lab is a particularly good location for addressing this issue. In part, this is because it is a fairly large laboratory with a somewhat hierarchical organization. But also, because it is a testing laboratory run by engineers, work there is connected to diverse bodies of practice which have origins well outside the immediate research community. It is difficult for any one individual to understand or be competent in all of these areas, which leads to a more extensive division of labor. Technicians, graduate students, and faculty members all play quite distinct roles in research at this laboratory.

The division of labor in the laboratory has not been entirely ignored, however. The most significant discussion of this issue can be found in the growing literature on technicians. Steven Shapin, for example, sifts through the historical record to reconstruct the role of "invisible technicians" in 17th century English science.[21] Chandra Mukerji similarly discusses the role of technicians in 20th century oceanography.[22] Both note that technicians' work seems to be devalued because of its routine, manual character and association with machinery. The work they do is often viewed, in the end, as a mere extension of the will of the scientists they work for.

The precise nature of the division of labor between technicians and scientists is discussed in more detail in the work of Stephen Barley.[23] Barley and a group of collaborators conducted a comparative study of technicians in a variety of fields, ranging from laboratory technicians to emergency medical technicians. This study found that technicians' work, in whatever field, generally occurred at an "empirical interface" where the material world is manipulated to produce symbolic representations.[24] In their role as mediators between these two realms, technicians fit into the division of labor in two distinct ways. Some technicians, like those who fix computer problems, serve as "brokers."[25] They are responsible for taking care of the technological systems that others use to do their work. The people they serve often have little knowledge of the technology in question, and as a consequence generally inhabit a different social world from the technicians, according to Barley. These technicians generate symbolic representations primarily for use in their own work. Other technicians serve as "buffers" between professionals and the material world.[26] Their job, essentially, is to ensure that there is a reliable correspondence between the material world and the symbolic representations that they produce, so that professionals can use these representations to add to their own knowledge. Laboratory technicians are a paradigmatic example of this type. Barley notes that technicians in this role generally share a social world with the professionals they work for, and are able to discuss problems in a common language. In a pinch, the professionals might even be able to take over some of the work of technicians, although they might not do it well.

Although the technicians in the Structures Lab do appear, in some respects, to work at a boundary between material and symbolic worlds, they are not alone in this respect. Each of the different groups of actors at the laboratory — faculty, graduate students, and technicians — serves, in some substantial aspect of their work, as an intermediary between the material and the symbolic. Also, an "empirical interface" can generally be more fully described as an interface between work settings. Human beings never enter into unmediated confrontations with the material world. These interactions are always shaped by traditions of practice and skill. The empirical interface at which technicians work, for example, is more importantly an interface between a body of practices dominated by highly skilled manual work and a body of scientific practices dominated by the manipulation of symbols. So while the concepts of "buffer" and "broker" can help explain the texture of work at the Structures Lab, they do so within a broader framework in which each actor serves as an intermediary between distinct work settings, making it possible to establish a continuous chain of practices between settings. It is the need for this kind of mediation that shapes the division of labor in the laboratory setting.

6.5 A tradition of testability: The quasi-static method

Before I move on to discuss the specifics of the chain of practices at the Structures Lab, it is important to understand the basic principles of the testing method used at the laboratory. In earthquake engineering research, what might be called a "tradition of technological testability" has developed.[27] There are several widely used methods of laboratory testing in the field.[28] Most of the tests done at the Structures Lab are of the "quasi-static" variety. Purely static testing, which is rarely used in this sort of research, simply involves subjecting a test specimen to a constant level of force or weight. Quasi-static testing, on the other hand, attempts to replicate the side-to-side shaking of an earthquake, although in a simplified form and at a very slow speed.[29] Another common method is "shake table" testing, which attempts to simulate the dynamic motions of an earthquake in real time, using a hydraulically-driven table.[30]

Both quasi-static and shake table testing have advantages and disadvantages.

While shake table testing simulates the motions of an earthquake in a very sophisticated way, most shake tables can only accommodate relatively small-scale models of structures, and the tests go by so quickly that it is difficult to obtain certain kinds of data from them.[31] Quasi-static testing, however, simulates certain key aspects of the effects of earthquakes on structures, but uses less complicated equipment and is done at a slower speed which facilitates data collection. For these and other reasons, quasi-static testing has become the dominant form of laboratory testing in earthquake engineering: one source estimates that 85 to 90% of all published experimental research in the field is based on this method.[32]

One of the most important long-term research projects being done for Caltrans is directed toward studying the behavior of reinforced concrete bridge columns of the sort which support freeway overpasses. A given series of tests generally proceeds through a process of parameter variation.[33] For example, the main focus of my participant observation in the laboratory was a series of tests being done on flared columns.[34] This series included tests of models representing an older column design with flares, the same design with the flares removed, and the same design again with the flares partially cut away as a retrofit measure; it then moved on to newer-style Caltrans columns, including one without flares, one with flares, and one with the flares partially cut away.

Column tests are typically done on 40% scale models, although some full-scale tests have been done as well. The Structures Lab, like many other laboratories that do this kind of testing, is built with a massively reinforced and prestressed concrete wall and floor, often called a “reaction wall” and “strong floor.” The columns are tested in an inverted position. The bottom of the test specimen (which would be the top of an actual column) is bolted down to the floor, and a large hydraulic actuator — about fifteen feet long when extended — is bolted to the wall and to the top of the specimen. This actuator is hooked up to a computer control system which can precisely control the forces and displacements which are applied to the test specimen. The steel reinforcing bars inside the column are outfitted with hundreds of strain gauges which will measure the deformation of the steel during the test; these are complemented by other externally attached instruments that measure column curvature, top displacement, and

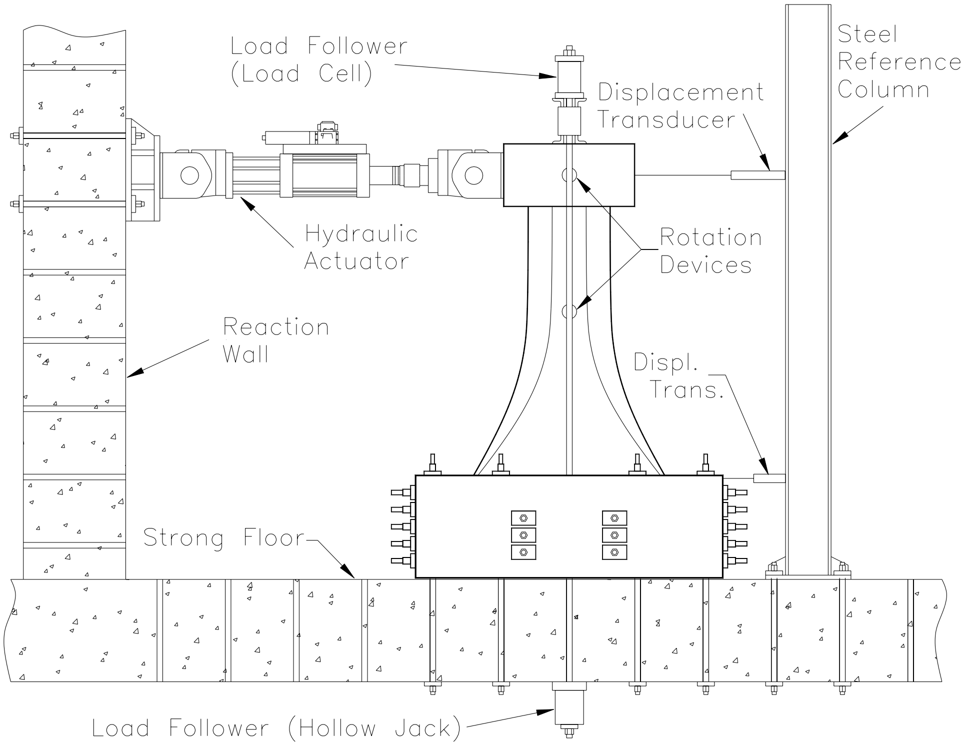


Figure 6.4: Configuration of a typical column test. The column is approximately 15 feet tall. Source: Anthony V. Sánchez, Frieder Seible and M.J. Nigel Priestley, *Seismic Performance of Flared Bridge Columns*, Structural Systems Research Project vol. SSRP-97/06 (La Jolla, CA: University of California, San Diego, Division of Structural Engineering, 1997), p. 22. Used by permission.

base slippage (Figure 6.4).

Tests begin slowly, with very small displacements well within the "elastic" range of behavior of reinforced concrete. Elastic displacements are those which do not permanently deform the concrete and steel. The displacements increase in a series of steps, with three back-and-forth cycles generally performed at each level. As the cycles continue, they soon move past the point of "first yield," when the steel reinforcing bar ("rebar") begins to deform permanently, and into the area of inelastic behavior. The test continues to progress, cycling to ever larger displacements. The concrete begins to fall off in chunks after a while, exposing the rebar, which at this point is still able to hold the interior concrete together. As the test nears its end, the bars begin to snap, with loud booms. Now the concrete begins to lose its integrity completely, which marks the end of the test. This whole process is quite slow, involving in the neighborhood of

Figure 6.5: A column test in progress, nearing completion. Note computer monitor, which is reporting data from hundreds of gauges, and plotter, which keeps a continuous hysteresis record during the test. The test is controlled from another computer terminal to the right of the plotter. Photograph by the author.

twenty back-and-forth cycles over as long as an eight-hour period. Most of the time is actually taken up in pauses between cycles, during which the students and technicians who are conducting the test carefully look over the specimen, mark any surface cracks with felt-tipped pens, and photograph the resulting patterns (Figure 6.6). By the end of the test, the column is covered with a network of marked cracks which radiate outward from the areas where the concrete has disintegrated. These photographs, along with the numerical data from instruments, make up the data which are taken away for further analysis.

Most of the existing case study literature on testing focuses on what some engineers call “proof tests.”[35] A proof test is designed to test a complete technological system under conditions as close as possible to those it would experience in use, in order to make a projection about whether it will work as it is supposed to.[36] The nuclear missile testing described by MacKenzie is a particularly clear-cut example of this kind of test.[37]

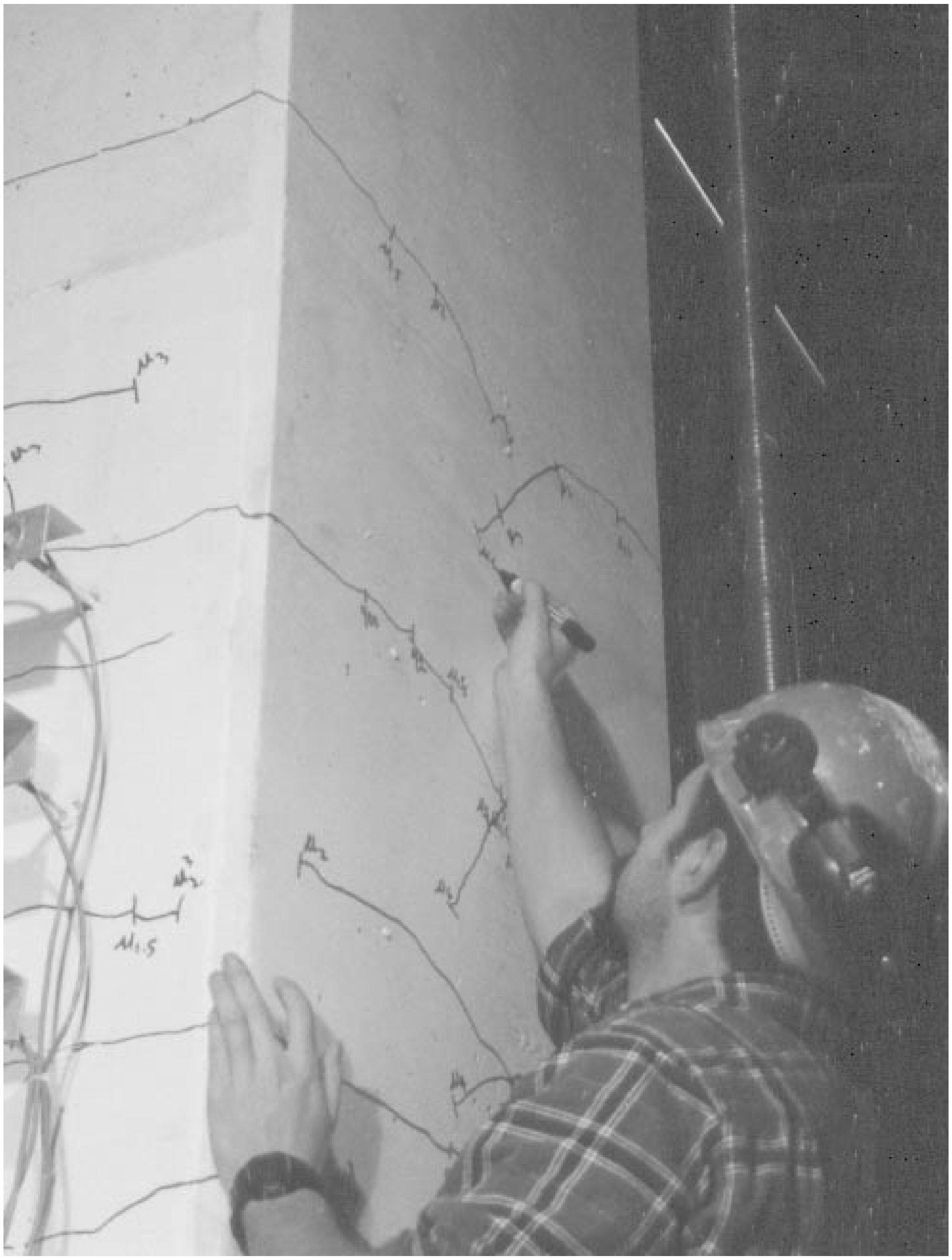


Figure 6.6: Marking cracks with a felt-tipped pen between cycles of a column test. Photograph by the author.

The testing done at the Structures Lab, by contrast, plays a much more ambiguous role. While some tests are done to determine whether a particular design will work in the field, many others are at least partly oriented toward gaining a better understanding of the fundamental behavior of building materials like reinforced concrete. These tests have some of the characteristics of experiment, in that they are oriented toward gaining new basic knowledge, rather than just proof of functionality.[38] One result of this is that there is more of an emphasis on replicability than may be found in some test situations, even though, on the whole, projection is a more important problem to these researchers. Also, an analysis of testing in terms of similarity relationships becomes less convincing for this kind of testing, because maintaining the similarity between test objects and objects in the field is often secondary to other methodological concerns. This sort of testing makes a particularly good case study precisely because the similarity between test and use is full of ambiguity, yet projection seems to occur anyhow. If testing is to be understood at the most general level, it is important to look carefully at this kind of research-oriented testing.

6.6 The chain of practices: Three intersections

In this case study, I focus not on specific work settings or bodies of practice, but rather on the interfaces between them. By examining how objects, representations, and actors move across these interfaces, it will become clearer how the practices of one setting can have an impact on the practices of another, even as the settings themselves remain distinct in certain ways. At each of these intersections, establishing a relevant similarity relationship is in some way problematic. Instead of focusing on this problem, therefore, much of the work at the interfaces between work settings is devoted to finding creative ways to manage ambiguity where there are no clear similarity relationships. In each case — for example, in making the connections between test specimens and structures in the field, test results and the performance of structures in earthquakes, or the practices of researchers and designers — we find that actors have organized boundary objects and boundary-crossing people to ensure that relevant connections are made between the practices of different work settings despite these ambiguities.

Outside practices and the laboratory

The point at which the laboratory work setting connects with outside work practices and technologies is a particularly crucial location, because it is through this intersection that most of the "raw materials" of research make their way into the laboratory. This includes many of the skills that are necessary to laboratory work. The crucial problem faced by researchers at this intersection is that of bringing the technology to be tested into the laboratory in a form that is amenable to the demands of research, yet still able to reasonably "stand for" the technological infrastructure outside of the laboratory. To this end, an ensemble of "hybrid objects" — in the form of test specimens — and skilled technicians is deployed.

Test specimens

A key feature of the specimens tested in the laboratory is their commonality with structures found outside of the laboratory. Indeed, in some cases the test specimens are meant to represent specific, existing freeway structures. But in order to be useful to research purposes, test specimens have to satisfy a number of conflicting demands. As a result, researchers find it necessary to have these models built on-site, so that they can be built in a way which reconciles some of these conflicting demands. There are a number of ways in which test specimens differ from their counterparts outside the laboratory. At the most straightforward level, they differ because a great number of measuring devices have to be incorporated into their construction. But this issue aside, test specimens are also constructed and sometimes designed differently to meet specific research needs.

The main difference in the construction of test specimens and the construction of structures in the field is a matter of precision. One of the laboratory technicians, a former construction worker, explained:

> You have to do it here [with] more precision than you would on the outside, because this is a test, everything has to be set, the steel has to be just set at a certain spacing, where if the steel is off an inch or so it doesn't make any difference in the real world, you're not in a test. And when you start to test something, everything should be just as drawn. You can't slide a bar a couple of inches and get away with it, you know, because that's not going to give you the exact reading that they want.[39]

The point here is not that the construction industry has low quality standards, but that most structures are designed with enough redundancy that minor deviations in the placement of reinforcing steel will not compromise their integrity.[40] In the test situation, however, the goal is to determine precisely how a structure fails, so researchers want to know the physical dimensions of the test specimens to a high degree of accuracy. Also, as Seible pointed out, structural tests are not done in great enough numbers to be able to simulate the actual range of variation in construction in a statistically rigorous way. Instead, researchers prefer specimens to be built precisely enough that test results can be used as a reliable basis for developing computer models. These computer models can then be used to simulate the actual variability of structures, if this is deemed necessary.[41]

While all test specimens are built according to these higher standards of precision, some specimens are actually designed differently than anything that exists in the field, in order to highlight certain aspects of structural performance. Priestley gave an example:

> [If] we want to be close to a balance point between two different failure modes, flexural failure and shear failure mode, for example we design the test specimen to be right close to that balance point so we can examine that critical area. If we were designing real structures, we'll design to be away from that area.[42]

This is a typical case in which the need to understand a fundamental aspect of the behavior of reinforced concrete takes precedence over the need to make the test specimen as similar as possible to artifacts outside the laboratory.

The test specimens, then, are a particular kind of boundary object. Some objects can cross boundaries easily because they are flexible enough to be adapted to a variety of local circumstances. These test specimens, however, are built as hybrid objects which incorporate elements relevant to two quite different work settings. In these objects, the concrete and steel of real infrastructure meet the electronic measuring devices and epistemological demands of science. Objects like these do not have to move in order to cross boundaries; their existence implies a prior boundary-crossing movement on the part of those who build them. It is the laboratory technicians who make this transition.

Technicians

As the laboratory has expanded, an initially very simple division of labor has grown increasingly complex. Technicians who were hired early in the history of the lab — in the mid-1980s — tend to have experience and skills in more than one area. One of these technicians, for example, worked in oceanography for fourteen years as a shipboard electronics technician, then ran his own concrete business for several years before coming to work at the laboratory. But it proved difficult to find people with this range of skills, and as the laboratory grew, a new hiring strategy emerged. Instead of seeking individuals who could do many things, the laboratory began to hire technicians with specialized skills in two distinct areas: construction, and electronics and hydraulics.

Electronics and hydraulics technicians are generally responsible for test setup and instrumentation, and for the maintenance of instruments and testing equipment. These technicians often have very diverse backgrounds, because their work revolves around an array of machinery which is fairly particular to the laboratory. For example, one of these technicians had an undergraduate degree in bioengineering, worked for several years as a theatrical lighting technician, and then worked for a while as a general support technician for the mechanical engineering department before being hired by the laboratory.[43] The head technician in this category is unique in having a Ph.D. in structural engineering, acquired through graduate work at the laboratory.[44]

Here, I focus mainly on the work of construction technicians. These technicians are primarily responsible for the actual construction of test specimens, as well as for operating heavy machinery like cranes and forklifts, and typically have extensive experience in the construction industry. One of the most valued construction technicians was hired by the laboratory after working in the construction industry around southern California for over forty years. He was perceived by researchers as being especially well suited for laboratory work because of his extremely perfectionist attitude.[45] The laboratory is crucially dependent upon technicians like these, because of the need to construct specimens for testing purposes which incorporate features of construction in the field. These technicians bring skills in steel work, carpentry, and concrete pouring which professors and graduate students usually do not possess to any great degree.

Many of the construction technicians working at the laboratory are at or near retirement age. Researchers value these older workers because of their greater skill, but laboratory work is also attractive to these workers because it is quite a lot less strenuous than construction work in the field. It also pays considerably less, which may drive away younger workers. The technician mentioned in the previous paragraph compared the physical demands of the two types of work:

> You're doing it here on a much smaller scale, and the heavy work is done by my friend, the crane, instead of my back. A lot of things that you do out on a bridge, well, you can't reach with a crane, or they only give you X number of hours to finish the job, and you better have all that heavy work done, or otherwise you have to find another way of doing it. Most of the time it's with the muscles in your body.[46]

Many of the construction technicians are also drawn to laboratory work because they find the research environment interesting. One technician described his lifetime strategy in the construction business, which carried over into his interest in laboratory work: "when I see something being built, [a] new type structure . . . or a new way of doing it, I try to get on and learn how."[47] Another technician explained that "the part I really like about it [is] every day you learn something . . . you get to learn things that you wouldn't when you build something out in the field."[48] Part of the reason for this is that the laboratory brings engineers and construction workers together in a way that rarely happens in the field, as this technician described:

> People building whatever out in the field, houses or bridges or whatever, they're always saying look at this, look at the engineers and architects, this is stupid, why did they do this? . . . And a lot of it is, [the engineers and architects] don't understand how things go together when they draw the drawings, and they'll draw things that can't work. . . . but a lot of times they have a reason that doesn't make sense to you out in the field, and now, after being here [in the laboratory] for a while, you can see a lot of times that there is a reason, some of the things that appeared stupid before.[49]

This statement reflects the important intermediary role that these technicians take on in the laboratory. If their work were limited to building and moving test specimens, they would fit the "broker" role described by Barley, since they are taking care of the physical infrastructure of laboratory work. But their work is not limited in this way.

Construction technicians also play a more active role in shaping laboratory practices. In particular, they interact extensively with graduate students in the laboratory, and play a major role in training them for laboratory work. Partly, this involves teaching graduate students some of the skills and "tricks of the trade" that they need to assist in the construction process. For example, on one occasion I worked with a graduate student who was trying to assemble wooden forms around an assembled "cage" of steel reinforcing bars, prior to pouring the concrete. The column was to have about a dozen steel bars protruding from it to serve as instrument attachments, and the wooden forms had holes drilled in them to fit over these bars. However, the student had a great deal of trouble maneuvering the large plywood forms so that the holes and bars all lined up at once. He consulted one of the construction technicians, who told him to fit lengths of metal pipe into the holes, then slide the pipes over the ends of the bars before finally positioning the forms. This solved the problem. Besides this sort of troubleshooting, it is the technicians who usually instruct students in basic construction techniques and in the use of machinery.

Through experiences like this, technicians and students start to share a common social world, which begins to make the technicians' role more like that of a "buffer" between work settings. Technicians also act like buffers because they do play an active role in translating between the worlds of material and symbolic practice. They do this mainly through working with students on design drawings. Being able to produce design drawings which can be successfully translated into built structures is one of the most important aspects of laboratory work, just as it is in structural design. Interpreting these drawings, whether in the laboratory or in the field, almost always involves some degree of interaction between engineers and builders. In the Structures Lab, the problem is aggravated by the fact that some graduate students do not have a great deal of practical design experience. When they are working on a specimen with a new design, graduate students frequently consult with technicians to make sure their design drawings are in accordance with standard construction practice. A technician described how this process worked with one inexperienced student:

> [He] didn't quite know how to set things up, I mean . . . his drawings suffered a little bit, so you have to make some interpretations to make it work. . . . I'd

> say to him, we can't do it this way, we have to do it this way, and he would change the drawings and make it all work.[50]

Through interactions like these, construction technicians help ensure, in one important respect, that there is a reliable connection between symbolic representations used by the engineers and the material objects in the laboratory. They do so as part of their larger role of bringing the work practices of the construction industry into the laboratory setting.

Construction technicians and test specimens relate to each other in rather complex ways as they bridge the gap between work settings. At one level, technicians play a key role in the construction of test specimens, which themselves serve as hybrid boundary objects. But in so doing, they also come to serve an important role by making certain skills and practices available to everyone who works in the laboratory, in the role of teachers and advisors. The test specimens, in this case, play their mediating role only with the close support of skilled human beings. At the same time, however, as common objects of work they play a pivotal role in the transfer of knowledge and skills between technicians and graduate students.

The laboratory and academia

There are two central problems that researchers face in making the work done in the laboratory relevant to the broader academic field of earthquake engineering. The first problem is how to organize testing methodology and computer modeling techniques in such a way that test results are made relevant to the entire field, which is after all interested mainly in the behavior of the built environment outside the laboratory, not test specimens in the laboratory. The second problem is a somewhat narrower subset of the first: how to make sure that the data taken from the laboratory to the academic offices for further processing are, in fact, accurate representations of the performance of the specimen during the test. In part, this is done through an process of independent calibration and testing of components of the test setup. Even with this calibration, however, researchers feel that ability to interpret data is somehow enhanced by direct experience of the testing process, and for this reason graduate students are required to take a major role in both producing and interpreting data.

Test methodology

Earthquake engineering researchers think that quasi-static tests produce results that are similar to the effects of an actual earthquake on a structure, but only at a fairly general level, as one researcher explained:

> We believe that if you take a structure through three complete cycles at the maximum level of displacement, that's more severe than would ever happen in an earthquake in which that displacement was the maximum ... by definition you're only going to have one cycle or one half cycle to the maximum displacement [in an earthquake] ... so by doing three cycles plus and minus to that level, you're pretty conservative in terms of the response.[51]

But researchers are not satisfied with this very general similarity relationship; they want to be able to use test results to accurately predict the behavior of specific structures in real earthquakes. Here they face a dilemma: for various methodological reasons, explained below, both the test specimens and the test method are highly idealized, making them manifestly different from any particular structure or earthquake. This makes it difficult for researchers to see a direct correspondence between any particular test and the performance of a structure in an actual earthquake. As a result, when researchers explain this correlation, they do not usually talk about direct similarity relationships, but about intermediary devices that serve to connect the two domains. The most important of these devices are computer programs which enable researchers to model the behavior of any given structure. In conjunction with the test methodology described here, these models play an important role in rendering the practices of the laboratory useful to academic researchers.

The quasi-static testing method itself is designed to mimic the forces of an earthquake only in a very general way. Unlike shake table tests, quasi-static tests put a structure through a very regular, predetermined sequence of steadily increasing displacements which is not believed by researchers to directly imitate the effects of an earthquake. Although one of the reasons that this method is used at the laboratory is because it is simpler and less labor-intensive than shake-table testing, this choice is not just a practical trade-off. In fact, quasi-static testing is felt to provide certain advantages in terms of the overall usefulness and applicability of test results.

One such advantage is that it standardizes test results, and therefore makes tests easier to replicate. Priestley described why this is important for the generation of knowledge in the field:

> Doing standardized testing is rather important if you're going to be able to compare one test to another, and one laboratory to another, so if you put a particular structure through one earthquake record, and somebody else does another structure through another earthquake record, you really can't compare them. But if we both work to a standardized testing pattern, which simulates or represents the response in earthquakes, then you can at least compare . . . and increase the database.[52]

But it is not just concerns about standardization that make quasi-static testing useful. The idealized character of the method is also felt to make the test results applicable to a wider range of actual earthquake conditions:

> You have to make it so it doesn't really have the characteristics of an individual earthquake, because you don't know what the characteristics of the earthquake that's going to hit the structure will be, you have no means for really being able to tell that. So what you try and do is something or other that has generic characteristics of relatively increasing earthquakes.[53]

In this case, drawing a tight, one-to-one comparison between a test and a particular earthquake is not seen as a methodological advantage; in fact, it is seen as a disadvantage because the precise nature of an earthquake that is likely to hit a particular structure cannot be easily predicted, given the present state of knowledge about earthquakes. Because of this uncertainty, it is felt that simulating earthquakes in a very general way provides a better connection between the tests and the performance of actual structures than could be achieved through more direct methods.

Computer models

This reliance on a very generalized testing method, however, creates a new set of problems. Now, in order to get the kind of precise, quantitative correlation that researchers want between tests and the performance of structures in actual earthquakes, an intermediate step is required: computer modeling. According to the Structures Lab researchers, the primary purpose of the testing they do is to confirm, or calibrate, these models.[54] These analytical models, which are usually realized on computers, can then be used to more accurately simulate the effects of a specific earthquake on specific structures. Almost all the research done at the laboratory involves either the generation of new modeling tools, or the modification and refinement of existing tools to cover a broader range of cases or to be more accurate. This is consistent with trends in the entire field of structural engineering, where computer models are routinely used in design work; indeed, they have become so prevalent that there are now engineering firms which focus almost exclusively on the development and application of these models. At the Structures Lab, most of the actual work of developing models is done by graduate students.

There are a variety of modeling techniques in current use in earthquake engineering. One example is "finite element" analysis, which essentially breaks a structure down into small pieces, each of which can be modeled fairly simply. Models can be either linear — that is, modeling only the elastic behavior of structures — or nonlinear, in which case they attempt to simulate effects such as the deformation of steel and the cracking of concrete. In order to simulate the dynamic response of structures to earthquakes, techniques of "time-history analysis" are often used. This method can be used to simulate the response of a whole structure to specific ground movements. Test results are used to help refine the assumptions built in to these models about the nonlinear behavior of materials and about the interactions between materials, such as the degree of slippage between concrete and steel.

Researchers feel that this process of calibration between tests and models has led, over time, to very good correlation between models and test results, according to Priestley:

> Our computer models enable us to predict the force-displacement response, and we typically do that before the test, we plot it on to the paper that is going to be used for automatically plotting the x-y response [of the test specimen] so we can compare a true prediction of response and what we actually get, and we typically get excellent agreement of the whole curve now. Now this is something or other which we couldn't get ten years ago. . . . We really believe we have very good models, just because of the fact that we see the agreement between theory and experiment.[55]

However, this correlation between tests and analytical models is not always perfect, particularly in situations where the structural element being modeled has an unusual design or complex shape. For example, on one occasion a bridge pile was tested in the laboratory which had an unusual design: a pre-stressed, cylindrical shell made of high-strength concrete, surrounding a reinforced core of normal concrete. Two independent computer models each failed to predict certain aspects of the pile's behavior under stress. After the test was completed, the models were refined so that they could accurately duplicate the performance of the pile. In such cases, the hope is that the refined models will be able to predict the outcome of future tests.

Although the close correlation between test results and computer simulations has made earthquake engineering researchers very confident in their ability to project between the two, they do not have quite the same degree of confidence about connecting these models to the performance of actual structures in earthquakes. Data on the effects of earthquakes on structures come mainly from field observations of earthquake damage. After a major earthquake in any built-up area, earthquake engineers from research groups all over the world try to get into the area as quickly as possible in order to take pictures of the damage before it is cleaned up. While it is usually only faculty members who are able to make international trips, a team of faculty and students from the laboratory visited and photographed all of the sites of major damage to bridges in the 1994 Northridge earthquake in the Los Angeles area within hours of the event.

The data that can be gathered about the effects of earthquakes through these investigations are largely qualitative observations about the nature of the damage. This can be supplemented by seismographic readings giving a general idea of the forces to which a given structure might have been subjected. Analytical models can then be used along with these data to try to generate probable failure scenarios. After the Northridge earthquake, faculty and graduate students from the Structures Lab put together a report which analyzed a number of structural failures in this way.[56] Such exercises give researchers some measure of confidence that their analytical models can be applied to real-world situations, but they usually do not produce the kind of tight correlation that researchers believe exists between analytical models and test results.

The uncertainty of this connection is reflected in the fact that earthquakes still quite frequently present researchers with unanticipated effects. As a result, each new earthquake in a built-up area is seen as an important learning experience. Seible explained:

> We are by no means at the stage where we can say look, we know everything about it. Events like this help by pushing the state of the art, it's like a turbocharge . . . every time you have an earthquake then research jumps again a couple of steps, but we still need to do a lot of work there.[57]

Data

The production of the data that goes into computer models poses some significant problems in itself. The most significant data obtained from a test come from the numerical readouts of the many gauges and measuring devices which are attached to, or built into, the test specimen. Throughout a test, readings from these instruments are digitally recorded every few seconds. These readings are recorded by a computer system, and then saved on floppy disks for further processing on computers in the office area. The most commonly used representation of test results is a plot of the force applied to the column by the hydraulic actuator versus the displacement of the top of the column, called a "hysteresis" graph (Figure 6.7). Good results can be seen fairly easily on such a graph. Very generally, if the loops on the graph become large and rounded, this shows that the specimen is able to absorb the energy of an earthquake while retaining its ductility, or ability to resist repeated deformations. More vertical, compressed loops, however, reflect a lack of ability to absorb energy and lower ductility, which can make a structure more likely to fail in an earthquake. These graphs can be compared to predictions of force-displacement response in order to test analytical models, as Figure 6.7 indicates.

Researchers try to ensure the accuracy of the data used to produce these graphs, as well as higher-level analytical results, through a variety of checks, tests, and calibrations. Strain gauges, for example, convert variations in strain into variations in voltage. Prior to a test, each data channel to which a gauge is connected has to be carefully calibrated so that a given voltage reading corresponds to the same strain in every case. Also, readings from different instruments can serve as a check on one another. For example,

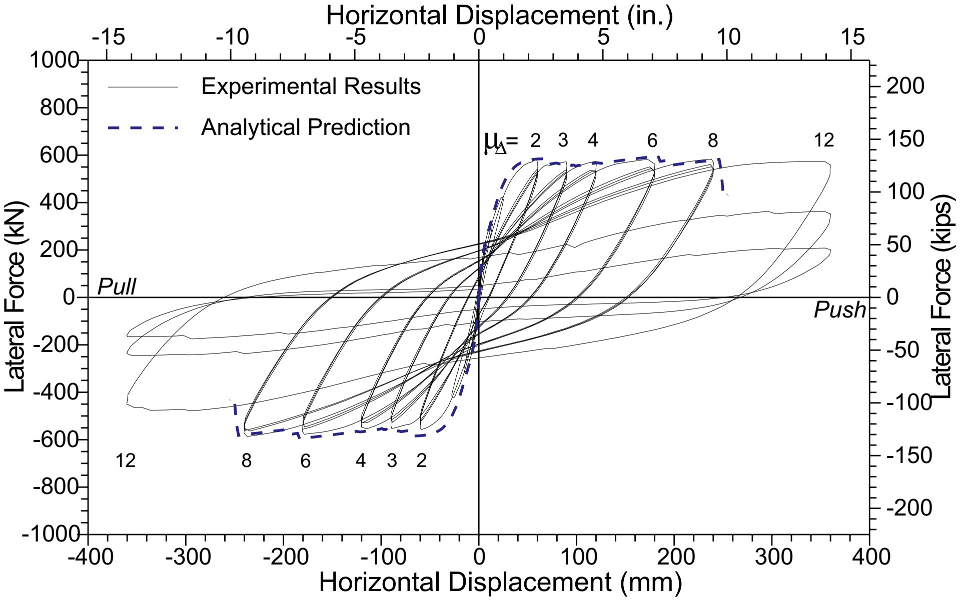


Figure 6.7: Hysteresis graph for unflared column test, showing the close correlation that can often be obtained between analytical predictions and test results. Source: Anthony V. Sánchez, Frieder Seible and M.J. Nigel Priestley, *Seismic Performance of Flared Bridge Columns*, Structural Systems Research Project vol. SSRP-97/06 (La Jolla, CA: University of California, San Diego, Division of Structural Engineering, 1997), p. 585. Used by permission.

while the main sources of data from the test are strain gauges and devices for measuring the displacement and force on the top of the column, there are also instruments that measure the angle of the column, its curvature, and the displacement of the bottom of the column. The readings provided by these instruments are somewhat redundant, but if a problem arises, they are sometimes used to verify the accuracy of other instruments.

The readings of the instruments themselves, however, cannot be accurately interpreted if the basic properties of the test specimen — particularly, the strengths of the materials — are not known. Steel is manufactured so reliably that its strength can usually be determined with sufficient accuracy just from data provided by the manufacturer. But concrete is a material which can vary drastically in strength depending on its moisture content and the amount and nature of the aggregate it contains. When the concrete is poured for the specimen, therefore, it is carefully mixed to contain exactly the proportions called for in the design. One way this is measured is through a "slump test," in which a sample of concrete is packed into a specially-designed cone which is upended on the ground; the distance the wet concrete sinks as it spreads out is then measured. The test is repeated until the mixture is exactly right.

Still, it is understood that the concrete will exhibit a certain variability even if its composition is carefully adjusted during construction. To account for this variability, samples of concrete are taken at several points during the concrete pouring process. These samples are packed into foot-long plastic cylinders with lids that are carefully labeled with the source of the concrete. The cylinders are then placed in storage to cure. Then, on the day that the specimen is tested, they are removed from storage and their strength is measured using a standard materials testing machine. This parallel testing process enables researchers to calibrate their test results in relation to the independently measured strength of the concrete.

All of these checks and calibrations are oriented toward the problem of metrology. That is, how to ensure that a measurement made in one place and time can be accurately compared to a measurement made elsewhere: for example, in a different part of the specimen, at a different point in the progress of a test, or in a different test or a different laboratory altogether.[58] As we have seen, the ability to compare test results is crucial if researchers are to be able to use test results, in aggregate form, as a basis for the development of analytical techniques and computer models.

Graduate students

The key actors in the mediation between the laboratory setting and the academic world are graduate students. While technicians work almost exclusively in the laboratory, and faculty members spend most of their time in the offices, graduate students routinely move between the two settings. After taking classes for a year or two, most graduate students begin a laboratory research project which will be the basis for their dissertation. Seible explained that graduate students function as "project managers." While faculty members are responsible for carrying out the overall research program, each specific series of tests is delegated to a graduate student.[59] The students supervise these projects from beginning to end: they design the test specimens, make drawings, and consult with technicians on matters of construction; they make sure that all the necessary materials are available; and they do a great deal of the construction, instrumentation, and test preparation themselves, with substantial help from technicians. They also oversee the testing process itself and the collection of data, and do most of the analysis, computer modeling, and writing of papers and reports.

Graduate students are given such a wide range of responsibilities intentionally. This professor explained that graduate students at some other structural engineering laboratories are responsible mainly for designing tests and processing data, relying on technicians to do most of the actual work in the laboratory. He argued that it is much better to involve students in the whole process, for two reasons. First, the process is made faster and more efficient, since the graduate students and technicians are able to coordinate their work much more effectively. Second, graduate students end up with a better understanding of the whole testing process, which he believes is reflected in the higher quality of the papers and reports they produce.[60] Although they did not mention graduate students specifically, Caltrans engineers generally did agree that reports produced by the Structures Lab were more immediately useful than those produced by other laboratories.

Graduate students are usually well-prepared for the analytical side of their work by the classes they have taken, but laboratory work is often quite a learning experience for them. Some students come to graduate school with previous experience in engineering practice. A typical example was one student who had worked for a bridge contractor as a field engineer on construction projects during college.[61] Such students are generally well-prepared to take on the "project manager" role. Others seem to find the transition from classroom to laboratory more difficult. Students also have varying degrees of experience in the use of common tools. Even those with a great deal of general experience usually do not know much about the tools and techniques of steel and concrete work. As I have described previously, it is the technicians who take the lead role in socializing graduate students into these aspects of laboratory practice. Despite the challenge, however, it is the rare student who is unable to learn enough relevant skills to be useful in the laboratory.

In the broader division of labor at the Structures Lab, it is the graduate students, rather than the technicians, who emerge as the primary "buffers" between the materiality of laboratory work and the symbolic products which make this work relevant to the academic and design communities. They play a slightly different role than the technicians described by Barley, however, because they are less fully immersed in the world of the laboratory, and are responsible for manipulating the symbolic data as well as producing them.

While graduate students may not take on such an intermediary role in every laboratory, it is no accident that they do so at the Structures Lab. Researchers there appear to be specifically dissatisfied with the traditional division of labor between laboratory technicians and researchers, in which the technicians simply supply the data and maintain the material infrastructure which scientists use for their work. Instead, they seem to believe that it is important that some of the tacit knowledge gained through laboratory work travel with the data, and that such knowledge is important for its correct interpretation. In particular, researchers who understand the circumstances of production of the formalized knowledge they generate may be better able to give advice to designers, especially if the designers need to apply this knowledge under circumstances not anticipated by laboratory testing. This is a role in which graduate students may often find themselves when they become faculty members.

Academia and design engineering

Although many aspects of research at the Structures Lab are oriented toward producing results which will contribute to the development of a body of academic knowledge, most people in the field would agree that the ultimate purpose of earthquake engineering research is to develop techniques which can be applied to practical design problems. Perhaps more importantly, much of the funding for research in this field comes from organizations like Caltrans, whose main interest does not lie in supporting basic research. However, academic researchers and design engineers do work in different settings, and sometimes see engineering problems very differently. Because of this, a certain amount of translation has to take place in order for research results to have an impact on the practices of designers. One way this is done is through the movement of texts and computer models from the academic to the design setting. These sources are sometimes aggressively reinterpreted and modified in the process. A certain degree of continuity in interpretation is maintained, however, through personal interactions and negotiations between researchers and designers.

Texts and computer models

Besides their role in academic research, computer models serve, to a limited extent, as links between the academic and design settings. Usually the programs are not transferred between these settings as inviolable “black boxes.” Instead, practicing engineers with expertise in analytical methods may use the code of these programs as a basis for writing their own programs that are more useful for design purposes. One case in which this was done at Caltrans was described in detail in Chapter 5. Designers are also influenced by written documents from academic sources. Published papers are one kind of document that designers read. But Caltrans engineers, because of their funding of university research, often rely on more informal documents, such as test reports or general reports they have commissioned on particular design topics, such as one widely-distributed report on retrofit design by Priestley, Seible and another U.C. San Diego researcher.[62] Caltrans designers have also been influenced by a textbook on seismic design by Priestley, Seible, and a well-known Italian researcher.[63] These resources, too, are actively reinterpreted according to the needs of designers, as Ray Zelinksi explained:

> Sometimes when design references come out of academia, there's a tendency to be a little too theoretical and running more calculations than what we want to spend time on . . . usually what happens [is] we get somebody aside that would take that report . . . and look at it, and run a few examples on a few bridges, and say gee, if we just kind of ignore this part of it and just put in this little shortcut in here, that it will still end up with the right answer, maybe a little bit on the conservative side, but we'll be able to save several days of calculations.[64]

If designers were simply making these choices arbitrarily, or based solely on their own experience, there might be cause to question the usefulness of funding laboratory research in earthquake engineering. But it is not as if design engineers are completely ignorant of the ways of academic research. Personal contact plays a crucial role in establishing a connection between the research and design settings.

Faculty

Academic engineering faculty, particularly in very "applied" fields like structural engineering, tend to play an intermediary role simply because of the positions that they hold. They have to seek research money and publish just like other academics, but they are also responsible for training the next generation of design engineers. Many faculty members at the Structures Lab also have some previous experience as working designers. Because of this, their research is often very directly informed by design concerns, even if designers sometimes disagree with their specific recommendations. Since Caltrans is primarily oriented toward the design and maintenance of civil infrastructure, rather than basic research, the Structures Lab is perhaps more oriented than most toward producing results that will be immediately useful to design engineers. The analytical models and written reports produced by the laboratory are intended to facilitate this communication with Caltrans. However, as we have seen, these formalized representations seem to require a great deal of reinterpretation to be useful on the design floor. The transfer of knowledge between the two settings appears to depend much more upon a system of extensive face-to-face contact between Structures Lab researchers and Caltrans design engineers.

Some of these face-to-face contacts occur in the context of specific testing programs: researchers must work with the Caltrans engineers who oversee their research contracts. These engineers often travel to San Diego to observe tests, particularly those deemed to be particularly crucial. To some extent, they serve as conduits to communicate test results to others at Caltrans in a timely way. Faculty members also frequently make the 500-mile journey to Sacramento to consult with Caltrans engineers. One situation which often requires such a trip is when a research contract is being negotiated. These negotiations are very much a two-way process, in which faculty members have considerable power to shape the research agenda. One of the professors gave an example:

> Sometimes we think that they need to know more about a subject than they think they need to know . . . we perhaps see a variety of things that not necessarily every Caltrans engineer sees as being significant design issues. An example of this is in the new project that we're just going forward [with on] the behavior of hollow columns. . . . When we suggested this initially there was a feeling at Caltrans that it really wasn't of great interest to them, because they basically . . . build solid columns. And we point out there's a lot of controversy at the moment about the performance of hollow columns in existing structures . . . and that they're looking towards the prospect of using hollow columns in some more major structures. . . . There are some specific design issues there which we have some concerns about. . . . [It changed] from being a very low priority to being quite a high priority [at Caltrans], after our discussions with them.[65]

Part of the reason academics have such an influence over the Caltrans research agenda is because they also play a role on peer review panels. Both Priestley and Seible have, over time, played prominent roles on many of these panels, and continue to do so. Between the panels and their research work, they sometimes commute to Caltrans headquarters in Sacramento on a weekly basis. In the process, they have cultivated a wide range of informal contacts within the organization. Through these interactions, both in their role as formal advisors and in their role as trusted individuals, the professors have an impact on design practice at Caltrans which goes far beyond the simple reporting of research findings. This influence seems to depend largely upon their reputation within the organization as people who have an exceptionally deep understanding of seismic design issues, precisely because they are involved in cutting-edge research rather than routine design work. Even though their written recommendations may not be adopted wholesale by Caltrans engineers, the researchers ultimately have a great deal of power over how they are interpreted because of their prestige and personal connections within the organization, as described in more detail in Chapter 5.

In their work, both academic researchers and designers rely on a great deal of tacit knowledge gained through experience on the job. If knowledge gained through research is to be effectively exploited by designers, therefore, formalized representations of this knowledge, alone, will not be sufficient. This has clearly been a driving problem in relations between Caltrans and the research community. Before Caltrans had developed such close ties to researchers through funding academic research and organizing peer review panels, designers had to rely primarily on written sources to find out what was going on in earthquake engineering research and practice. Under these circumstances, many engineers outside Caltrans, including members of the Governor's Board of Inquiry on the Loma Prieta earthquake, felt that the department had become cut off from the mainstream of engineering practice. Significantly, it was felt that this problem could best be resolved by establishing forums, like peer review panels, in which personal contact could take the place of reading articles. This need to go beyond formalism may characterize many cases of projection from laboratory to field.

6.7 Conclusion

This case study shows that projection, when analyzed in terms of local practices, can be a very complicated process, managed through heterogeneous means. The simplified representation in Figure 6.3 can only capture the chain of practices which connects laboratory testing in the Structures Lab to other work settings in the most schematic way. In this particular case, potential similarity relationships between the materials and representations used in different work settings are full of ambiguity. At each interface between work settings, this ambiguity is dealt with through the use of particularly flexible modes of representation, like computer models, as well as through hybrid objects which help resolve the tension between work settings in their physical form. In every case, however, the boundary objects are not, in themselves, sufficient to bridge the gap between work settings; human beings, with their complement of skills and tacit understandings, have to join them in their boundary crossing. This is why it makes sense to talk about projection in terms of the translation of practices across work settings. This is the most useful level of analysis for capturing the full complexity of the relationships between humans and machines which go into the process of projection.

The division of labor

While some authors have analyzed the distinctive role of laboratory technicians, and others have examined the division of labor in the laboratory setting as an element of scientific culture, I argue that the division of labor can play an important epistemological role as well. The methods of laboratory research are not invented in isolation from the rest of our culture. This is particularly clear in the Structures Lab, where skills in construction, electronics, hydraulics, and operating heavy machinery mingle with the practices of engineering science. In order for research to proceed, these bodies of practice have to be brought into the laboratory, and in order for laboratory work to have a coherent flow, they have to be integrated with one another. The knowledge that laboratory research produces depends upon this kind of integration of disparate sources of knowledge and skill. In the Structures Lab, it is usually the technicians who bring outside practices into the laboratory, and the graduate students who try to reconcile these bodies of practice with the practices of engineering research. Faculty members have their own role in integrating academic research and design practice.

A study of testing and projection brings these aspects of laboratory work to the fore, perhaps because testing often takes place as an integral part of a larger process of designing and manufacturing technology which occurs outside the laboratory. But these issues are not unique to testing. A central task of research in any scientific laboratory is to somehow bring natural phenomena into the laboratory where they can be controlled and manipulated. This sometimes involves bringing outside bodies of skill into the laboratory. Some examples might be skills drawn from veterinary surgery which are important to biomedical research, or the wide variety of engineering skills necessary to build a working experimental apparatus in high-energy physics.[66] There is also a division of labor between graduate students and faculty in many laboratories, since representing the laboratory to the scientific community and to funding agencies often takes faculty members away from active involvement in research. Of course, laboratory work need not be divided up in any particular way. One would expect that each laboratory would develop its own characteristic division of labor. The important point is that the role of the laboratory in the production of knowledge cannot be fully understood without some analysis of where research skills come from, how different research tasks are distributed among the various actors in the laboratory, and how these actors manage to coordinate their activities.

Testing

When the activities of testing and projection are opened up and examined closely, they emerge as messy, contingent processes in which the management of ambiguity, rather than clear-cut relationships of similarity or dissimilarity, is the norm. Yet MacKenzie and Pinch are able to identify significant cases in which the credibility of test results clearly does depend on judgments of similarity or dissimilarity between test and use. In such cases, I would argue, some crucial simplification has already taken place: people have chosen to ignore some of the complexity of the local circumstances surrounding testing. This can happen when a judgment of similarity is made, as when nuclear missile supporters claim that missile tests are credible because they are performed just like an actual launch. In the process they reduce the work of the people who remove the missile from its silo, take out its warhead, instrument it, and set it up for launch at the test range to necessary but epistemologically insignificant background activity. Judgments of dissimilarity, while they may bring out important aspects of testing practice, often depend upon a comparable assumption. Those who believe missile test are inaccurate, for example, assume that tests ought to be just like an actual launch. Practices that are particular to the testing process itself are therefore seen as deviations from the prescribed similarity relationship. Both forms of argument proceed on the assumption that the practices of testers ought to be left out when describing a successful test.

But this kind of simplification is not always necessary for a test result to be credible. Judgments of similarity and dissimilarity are likely to take on a particularly important role in two situations. First, when test results are taken up or debated in a social setting far removed from the test situation and the practices of testers, as in political debate or courtroom arguments. In these situations, most of the participants are not members of the same technical community as the testers, and do not share their nuanced understanding of the uses of test results. As a result, they may tend to rely on simple outward signs of similarity or dissimilarity between a test and the situation of use of a technology when judging the credibility of the test. Second, when both the testing procedure and the technology being tested are standardized to such an extent that they have become largely, but not completely, "black boxed." Such is the case in Pinch's example of the microphone test at a rock concert. In this situation, testing may be necessary to assess functionality, but the results are not debatable because the characteristics of a functioning system are already well known.

The complex nature of projection as practice, by contrast, is likely to come to the forefront under the sort of circumstances I have described here. That is, first of all, in situations where testing is oriented toward research and the development of new methods of analysis and design, rather than toward a simple functional evaluation of an established technology. It also becomes particularly significant in situations where test results are produced, analyzed, and applied within a relatively well-integrated technical community. The Structures Lab embodies both characteristics, since it does testing as research, and test results have significance primarily within the earthquake engineering community, which is probably more close-knit than many sub-fields of engineering. If test results were to become an object of political controversy or legal proceedings, which could certainly happen, arguments might well be made in terms of the similarity or dissimilarity between tests and earthquakes. But this has not happened yet, and it appears to be more important to the earthquake engineering community that researchers be able to give practical assistance to designers than that laboratory tests exactly simulate the effects of an earthquake.

Projection is always a process in which one social setting is linked to another. Under certain circumstances, important aspects of this connection can be described in terms of a similarity relationship between a test and the situation of use of a technology. However, to describe testing at the level of local work processes, or to explain how projection works within certain technical communities, it is necessary to focus on how the connection between social settings is embodied in a chain of practices. Examining such chains provides important insights into the ways in which knowledge and material practices are generated, transmitted, and put to use not only in the laboratory, but in technical communities in general. In particular, it highlights the division of labor and the need to coordinate work across different sites as important issues in modern technical practice. In the case of the Structures Lab, this approach makes it possible to understand how it is that construction workers' practical ways of understanding and working with concrete and steel can be put to use in the laboratory, and ultimately play a role in the development of the more abstract professional knowledge of academic researchers and design engineers.

Figures 6.4 and 6.7 are used with the permission of Anthony Sánchez.

Notes

[1] This chapter originally appeared, in somewhat different form, as "Concrete Practices: Testing in an Earthquake-Engineering Laboratory," in the journal *Social Studies of Science*, Volume 29, Number 4 (August 1999), pages 483-518.

[2] Interview, Larry Berman, November 4, 1997.

[3] The idea is actually first presented in MacKenzie 1989, 409-435; see also MacKenzie 1990, 340-381. Pinch 1993 presents a more general argument about the nature of projection, based in part on MacKenzie's work.

[4] See, for example, Knorr-Cetina 1981, Latour and Woolgar 1986, Lynch 1985, Traweek 1988 and Mukerji 1989.

[5] MacKenzie 1989, 411 (emphasis in the original).

[6] Collins 1974, Collins 1975, Collins 1981a. These articles are summarized and tied

together in Collins 1992.

[7] MacKenzie 1989, 414.

[8] This is an aspect of Collins' work which is not clearly addressed in MacKenzie and Pinch's work on testing.

[9] See Collins 1974 and Collins 1992, 51-78.

[10] See Strauss 1978. For examples of how this approach has been applied to science and technology, see Kling and Gerson 1978, Gerson 1983, Star and Griesemer 1989 and Fujimura 1992. A useful overview of the approach is given in Garrety 1997. See also Becker 1982.

[11] Strauss 1978, 122-123.

[12] Star and Griesemer 1989, 411. Here they draw on the work of Chicago school sociologists Robert Park and Everett Hughes.

[13] Star and Griesemer 1989, 393 (emphasis in the original). Also see Fujimura 1992 for an elaboration of this idea through the concept of "standardized packages."

[14] This sort of analysis has certain obvious parallels with other work, such as the actor network approach of Bruno Latour, Michel Callon, and others. However, these authors are centrally concerned with issues of technoscientific change, and in the exercises of power, translations of terms, and simplification and "black boxing" that go into the construction of sociotechnical networks. See, for example, Latour 1987 and Callon 1987. While these concerns could be brought into my analysis, I am more interested in examining the detailed workings of a well-established network. I believe this approach makes it possible to examine the normal work of testing in greater depth.

[15] I thank Steven Epstein for pointing this out to me.

[16] Pinch 1993, 26.

[17] See Latour 1987, 2-3, 130-131, and Callon 1987, 93-95.

[18] The participants in these work settings may share a common way of understanding technology that is similar to an "object world," as described in Bucciarelli 1994 in relation to engineering design.

[19] Galison 1997, 781-844.

[20] Knorr-Cetina 1981 and Latour and Woolgar 1986 are both consistent with this trend. Lynch 1985 looks at the work of a group of graduate students, but not in the context of an overall division of labor in the laboratory. In part, this lack of consideration of role differentiation could stem from the relatively small scale of the laboratories studied in these works. Traweek 1988 and Mukerji 1989 are two notable exceptions to the general trend, and both deal with relatively large-scale laboratories in high energy physics and oceanography, respectively.

[21] Shapin 1989 and Shapin 1994, 355-407.

[22] Mukerji 1989, 125-145. For another contemporary study focusing on technicians, see Orr 1996.

[23] Barley 1996. For a detailed discussion focused specifically on laboratory technicians, see Barley and Bechky 1994.

[24] Barley 1996, 418.

[25] Barley 1996, 422.

[26] Ibid., 420.

[27] This term is proposed in Constant 1983.

[28] These methods are summarized in Popov 1986. The reasons for the widespread use of quasi-static testing are discussed in some detail in Leon and Deierlein 1996.

[29] This method is also sometimes referred to as "pseudo-static" testing. See Popov 1986.

[30] In addition to the test methods discussed here, a hybrid method known as "pseudo-dynamic" testing has recently been developed, which uses multiple hydraulic actuators controlled by a computer program which simulates the motion that a structure would undergo during a specific earthquake, but at the slow speed of a quasi-static test. This method is still being refined and is not yet in widespread use. See Seible *et al.* 1994.

[31] Leon and Deierlein 1996.

[32] Ibid., 87.

[33] On the use of parameter variation in engineering tests, see Vincenti 1990, 137-169.

[34] This research is presented in Sánchez *et al.* 1997.

[35] This term was introduced to me by Nigel Priestley. Interview, November 7, 1997.

[36] This is generally similar to what Glenn Bugos has called "acceptance testing." See Bugos 1993.

[37] MacKenzie 1989. See also the tests described in Latour 1996 and Law and Callon 1992.

[38] An excellent historical example of this sort of testing can be found in Rosenberg and Vincenti 1978, a case study about the design of the Britannia Bridge. The bridge featured an unprecedented tubular design, so an extensive series of tests was carried out in order to find the fundamental causes of failure in metal tubes, and to see which configurations were most stable. The results of this research subsequently proved useful in many different areas of engineering.

[39] Interview, Page Nelson, November 4, 1997.

[40] The smaller scale of the test specimens may also be relevant here, but Nelson did not see it as a major factor.

[41] Interview, Frieder Seible, November 7, 1997.

[42] Interview, Nigel Priestley, November 7, 1997.

[43] Interview, Michael Baumann, March 19, 1997.

[44] Interview, Christopher Latham, March 10, 1997.

[45] Interview, Frieder Seible, November 7, 1997.

[46] Interview, Page Nelson, November 4, 1997.

[47] Ibid.

[48] Interview, Charles Stearns, April 7, 1997.

[49] Ibid.

[50] Ibid.

[51] Interview, Nigel Priestley, November 19, 1996.

[52] Ibid.

[53] Ibid.

[54] Interview, Nigel Priestley, November 7, 1997; Interview, Frieder Seible, February 27, 1997.

[55] Interview, Nigel Priestley, November 19, 1996.

[56] Priestley *et al.* 1994.

[57] Interview, Frieder Seible, November 8, 1996.

[58] See O'Connell 1993, Schaffer 1992 and Latour 1987, 250-254.

[59] Interview, Frieder Seible, November 7, 1997.

[60] Ibid.

[61] Interview, Anthony Sánchez, October 23, 1996.

[62] Priestley *et al.* 1992.

[63] Priestley *et al.* 1996.

[64] Interview, Ray Zelinski, August 25, 1997.

[65] Interview, Nigel Priestley, November 7, 1997.

[66] On the latter, see Galison 1997.

Chapter 7

Conclusion: Retrofit and Our Technological Inheritance

7.1 Professional practice, standards, politics and personal interaction

Projection, as described in the previous chapter, is all about making connections between situations that are distant from one another so that the experience gained in one setting becomes useful in another. Though raised in the context of testing, this issue is really central to all kinds of work we call technical, and some we usually don't. Extraordinarily complex engineering and organizational projects must be built on an extensive division of labor among scientists, engineers, and skilled workers, each of whom may have a very narrow area of expertise. Typically, these groups also work in very different settings that do not overlap very much. Because these work settings are so different, each group develops a distinct local culture which may lead them to very different interpretations of the concepts and the artifacts they all work with. When politicians, the media, and local communities get involved, even greater cultural differences come into play.

Symbolic representations, such as written documents or computer programs, and more tangible objects, such as laboratory equipment or test specimens, frequently play a role in bridging these cultural gaps. But close examination of a social world like earthquake engineering reveals that the transfer of knowledge and the coordination of work between settings ultimately depends on extremely complex networks of personal interactions that cut across social boundaries. Despite the current proliferation of information and communications technologies, much of this interaction takes place face-to-face, perhaps necessarily so. Because face-to-face communication is very flexible and often seems to build trust between participants, it makes it easier to convey information between settings even if there is no consensus or even explicit discussion about what makes two situations similar. Things like ideas, practices, and test results may end up taking on quite different meanings in the translation, but enough cultural continuity can be maintained to permit coordinated action. In fact, the diverse interpretations may serve a purpose by permitting groups to analyze a problem from perspectives better adapted to the nature of their work. This emphasis on the coordination of work within and across social settings through chains of personal interactions came out in various ways throughout the thesis.

For example, one of the key factors that shaped definitions of seismic risk at Caltrans over time was the expansion of the "risk community" to include university researchers. Even though Caltrans engineers had read the academic literature before, and it had influenced their views, actually interacting with the researchers in person had a dramatic impact on the the way seismic risk was defined. This social event had a much greater impact on Caltrans definitions of seismic risk than the 1989 Loma Prieta earthquake itself.

In general, increased interactions between Caltrans engineers and outside peer reviewers brought out latent tensions between different segments of the profession. This is probably often the case when different social worlds come together, and in each situation the participants have to develop ways of managing these tensions if they are to successfully work together. In many peer review situations in engineering, participants try to minimize tension by acting according to norms of civility and disinterestedness. Peer reviewers also seek to avoid the appearance of competing with their colleagues for business by making sure they do not in any way seem to be usurping the role of the designers or questioning their competence. In the Caltrans case, academic members of peer review panels were able to cross this boundary freely, in part because they were not perceived as having commercial interests.

Any large engineering organization requires some means of regulating the design process if work is to be coordinated and standardized. As in the interaction between distinct segments of social worlds, both formal representations and personal interactions are important for this coordination, though symbolic representations can rarely get the job done by themselves. In the design context, these representations usually take the form of codes or other similar documents. Codes and less formal personal communications have very different capacities to shape design practice, capacities which give them distinct roles in the regulation of the design task. Codes can provide broadly accepted minimum design standards, but they are slow to adapt to changes in practice. On the other hand, individual people can assimilate new design approaches relatively quickly and pass their understanding along to colleagues, though discontinuities in the network of personal interactions can mean that some designers never adopt methods that are promoted in this way. In situations where practice is changing quickly — here, as a result of increased interaction between designers and the academic community — codes become increasingly irrelevant to the state of the art in design practice, and designers tend to rely more and more on their interactions with certain people within the organization who have access to the latest information and can communicate it effectively.

Chains of personal interaction aren't just useful for coordinating work, however: they may also serve as means for coordinating the production of authoritative interpretations of events, making them a tool for the consolidation of power, professional and otherwise. For example, media interpretations of the causes of structural damage in the Loma Prieta earthquake were heavily influenced by the views of the engineering profession. This was partly because engineers had already developed a more coherent view of what happened than any other group, and partly because, as credentialed experts, they fit the profile of reliable journalistic sources. But the media initially relied on political figures for interpretations of the events, because these were the sources they were familiar with. A crucial factor in bringing media accounts around to the view of the profession was Caltrans engineers' deliberate efforts to make themselves personally known and available to reporters. Though this exposed some disagreements in the organization, it was a dramatically successful strategy which had a decisive influence on public debate.

The engineering profession had a similar influence on the political response to the Loma Prieta earthquake. After setting a few ground rules, state government turned the investigation over to a board of inquiry composed almost entirely of engineers. Government agencies are generally comfortable turning these kinds of political-technical problems over to panels of researchers and professionals, because they give like-minded experts a chance to negotiate and reach consensus on whatever differences they may have. This provides a form of public accountability while avoiding the divisive debates between experts that often emerge in more public forums. Government officials and experts both gain a means for protecting and possibly consolidating their power to shape interpretations of events.

Finally, examining the process of testing and the use of test results in other social contexts demonstrates that the local skills and knowledge of one setting — in this case, the laboratory — can travel very far indeed through chains of representations, objects, and personal interactions that link work settings and social worlds. This provides a general view of how work and interpretations are coordinated within and between social worlds. Modern scientific research and technical work raise these characteristic issues in a particularly dramatic way because they depend upon extremely complex divisions of labor, but also seek to maintain a certain epistemological coherence. They therefore build up very complex and well-organized chains of interactions that support the widespread dissemination of knowledge and new technological artifacts. Though it is easy enough to focus on the global spatial scope of these interactions and the ability of modern scientific and technological institutions to transcend location, the crucial characteristic of these institutions is that the global connections they make are embedded, at nearly every point, in local, interactive circumstances.[1]

Returning to technology

As this brief overview of the thesis suggests, it has largely focused on the nature and organization of technical work, and on the effects of rapid social and intellectual change on engineering practice. Seismic retrofit has been considered as one source of this rapid change. However, the very idea of retrofit also raises important questions about the nature of technology itself and the mechanisms that produce technological change. In order to give retrofit the attention it is due, in this chapter I return to the more traditional subject matter of the social study of technology: the nature of the interaction between humans and technology, and the question of whether we control technological change or it controls us. Examination of civil engineering work, particularly as it relates to the retrofit of existing structures, makes it difficult to sustain a view that we freely choose what path technological development will take under all circumstances. Neither does it support any kind of sweeping technological determinism. In the course of addressing these issues, this concluding chapter provides further evidence of the continuing importance of local circumstances and interactions in modern technical work.

7.2 Engineering within the grid

An engineer designing a telephone or an automobile usually has a great deal of creative flexibility. Besides the technical standards of engineering practice, the forms of these kinds of objects are limited mainly by generic interface requirements — a telephone should be able to work with standard phone lines, a car should fit within a standard traffic lane, etc. — and by the availability of parts and materials. Working within these relatively broad constraints, the designer is free to give the object a wide range of different forms, depending on the anticipated wants of the purchasing public. Civil engineers work in a more limiting environment because every bridge or building they design has to fit uniquely into a particular local landscape, taking into account the existing infrastructure and geographical and geological features. But in either case, the designer is still working mainly with abstract representations stored in a computer or drawn on paper. Through these relatively flexible representations, a technological artifact presents itself to the designer as a malleable entity, changeable with a few strokes of a pencil or clicks of a mouse. There is a sense that the designer is in control of the form of the object, able to experiment with any number of variations to come up with one that seems to respond most elegantly to the constraints at hand.

The design of new technological artifacts is the stereotypical engineering task, and both engineers and sociologists have tended to treat it as definitive of engineering practice. However, many — perhaps most — engineers aren't designers. Some are researchers or managers, but the group that is most often neglected are those in charge of maintaining existing technology. For consumer goods like copiers or automobiles, such work is usually carried out by technicians or mechanics, but for buildings or bridges or sewer systems, the responsibility often falls on civil engineers. Maintenance engineers, like technicians and repairmen, and unlike most designers, work with technology after it has taken material form. They don't experience technological artifacts primarily as flexible representations, as designers do, but as relatively inflexible material objects: design is replaced by working on what is already there.[2] As architectural critic Stewart Brand notes in the context of building maintenance and renovation, the best they can hope for is a "compromise with the *fait accompli*" of the object.[3]

In civil engineering, however, designers are increasingly being forced to look at structures from a position similar to that of the maintenance engineer. This is a relatively new phenomenon in North America, particularly in the west, where the continual expansion of population and infrastructure had until recently put most designers to work on brand new structures in areas that had not been extensively developed, culminating in the grandiose freeway, aqueduct, and dam-building projects of the mid-20th century.[4] As many areas have become more developed, however, engineers have gradually been forced to proceed in an atmosphere of constraint rather than limitless expansion. Environmental, historical, and cultural preservation laws passed since the 1970s have been one factor in this shift, as have newly restricted public works budgets. An equally important factor is that many projects have to be carried out within a matrix of existing infrastructure. The largest projects now tend to be things like replacements of aging freeway interchanges.[5] Such a project requires engineers to take into account everything that has happened around the interchange since it was built, such as the growth of population and business in an area (which brings more drivers who depend on that interchange) and commercial development of land near the freeway or even right under it that was once open.

The emergence of retrofit as a category of civil engineering activity is, in many ways, a culmination of these trends. The choice to retrofit existing structures that have been deemed inadequate implies that it would somehow be too costly to simply get rid of them or replace them. In the Caltrans case, eliminating certain bridges would have been costly because they are part of a large freeway system that is the central means of transportation in the state. The state was not willing to bear the costs of replacing all of the deficient bridges that had been built during the rapid expansion of the freeway system in the 1950s and 60s; the kind of funding that supported this expansion was simply no longer available, and in any case the political establishment didn't want to wait 20 years for a fix. Finally, it would have been costly to abandon the old bridges and build new routes, due to the cost of acquiring land that had already been developed, and given current environmental and cultural preservation rules. Seismic retrofit is an effort to make dramatic changes in the transportation infrastructure while avoiding these excessive costs.

The constraints that face designers of new structures are intensified dramatically in retrofit work because the designers now must operate not only within the limitations imposed by surrounding infrastructure, but also within those imposed by the particular structure they are retrofitting. This means that designers have to find ways of learning about the structure as it exists in the field, not just on plans; they have to develop a personal knowledge of it, much as maintenance engineers do. Instead of working with an object that is flexible and relatively easily shaped according to their intentions, designers are confronted with an object that has most likely never been under their control and often seems quite capable of frustrating their intentions. The very constrained nature of this work is evident from the observation of one Caltrans engineer that retrofit projects require significantly more effort than designing a new bridge, but the end result (for a common overpass) might be only three sheets of plans, compared to about fifteen for a new bridge. The bulk of the effort is in documenting and analyzing the structure that is already there. Because designers are not used to working with existing structures in this way, they are acutely aware of the ways in which a structure that already has material form limits their creative options. For this reason, retrofit is an ideal site for both engineers and social scientists to come to a better understanding of the circumstances in which technology limits human action rather than expressing our intentions and giving material form to our interpretations of the world.

7.3 Theories of technological change

Questions about how much technology can shape human action, or human action shape technology, are central to many theories of technological change. The two most important strands in Western technological thought take opposing views on the matter. What philosopher Langdon Winner has called the "voluntarist" view sees technological change as driven very straightforwardly by the rational application of technical means to solve objective human problems.[6] Change is driven by the emergence of new problems and by the continual refinement of the available technical means. The path that technological development takes is the direct outcome of informed human choices. The other important theory, technological determinism, turns the voluntarist approach on its head. It holds that technology evolves according to an inner logic, not as a set of rational solutions to problems we identify. In fact, it suggests, human decisions ultimately have very little impact on the overall course of technological development. But technology, resistant as it is to human control, has an enormous determining influence on human affairs.[7]

The voluntarist approach is significant because it suggests that technology should serve human needs, and that we are responsible for making the right choices about technology. But it falters in suggesting that the right solution to a problem can always be rationally identified, given the appropriate level of engineering expertise. The value of technological determinism is that it makes us aware that we are not always able to freely choose what shape technology should take next. It reminds us that the technology we develop today may have social consequences that cannot be completely controlled, even if they run counter to our future interests.

These views of technology, divergent as they are, share a common notion that the path of technological change can be comprehensively explained in terms of objective, rational principles. In the voluntarist view, new technologies follow directly from human needs: the problems to be solved can be clearly and objectively defined, and possible solutions are determined by fundamental scientific and engineering principles. In strong versions of technological determinism, technology evolves over time according to universal rules that can be identified by examining the historical record, and may be based in part on fundamental physical laws. In both theories, new technology emerges as the logical consequence of a given set of objective conditions.

As a result, neither has much to say about the activity of design itself. Because its results are explained largely in terms of objective forces, the particulars are presumed to be of little importance. This focus on objective forces also means that the design process is seen as largely free of social and cultural influences, even where society supplies the problem to be solved. But close examination of the work of engineering and invention, as a participant or as an observer, usually reveals that great creative effort is necessary to solve technical problems; that institutional arrangements and personal interactions can play important roles in the final outcome; and that engineers are not entirely insulated from prevailing cultural norms and assumptions. Looking at these circumstances, it is difficult to accept that new technology simply follows logically from well-defined social needs or existing technology.

A third, more recent view on the nature of technological change takes a social constructivist approach. Rejecting the idea that objective technological principles determine the outcome of the design process, social constructivists draw our attention to the "interpretative flexibility" of technology. They argue that different social groups attach radically different meanings to a single artifact, even to the point of having conflicting views about its basic operating principles.[8] These attributed meanings can lead to different definitions of a problem and to radically different solutions. Though it retains the voluntarist sense that technology can be adapted to serve a variety of human needs can be responsive to human needs, the constructivist approach suggests that the flexibility of the meanings we attribute to technology is just as important as the flexibility of technology itself in shaping technological change. These meanings take shape differently according to the cultural dynamics of different social groups, making the evolution of technology an irreducibly social process. From this perspective, constructivism firmly dismisses determinist claims that technology develops according to an inner logic and has an independent influence on human affairs.[9]

Social constructivism and interpretative flexibility

The view that technology is both materially and interpretatively flexible is the basis for a particular model of technological change put forward in the work of sociologists Wiebe Bijker and Trevor Pinch.[10] They describe technology as developing through an ongoing process in which an artifact is invented; different social groups attach different meanings to it; these meanings lead to variations on the original form; and these variations are in turn interpreted by different groups, leading to further variations. The cycle usually ends eventually when “closure” is reached — when an artifact develops a stable meaning across the relevant social groups, and hence takes on a stable form.[11]

Bijker and Pinch use the history of the bicycle as an example. The first pedal bicycles, what were later called “penny-farthings,” had pedals attached directly to a huge front wheel, with a tiny wheel in the rear for stability. Certain social groups, most notably young men, saw this bicycle as a “macho machine.” They wanted a large wheel for greater speed, and didn’t mind the bike’s height and tendency to throw the rider over the handlebars. Other social groups who weren’t interested so much in speed, like women and elderly men, saw the penny-farthing primarily as an “unsafe machine.” The first interpretation led to the development of bicycles with ever larger front wheels, while the second interpretation inspired designs with lower front wheels and air tires, more similar to the modern bicycle. Eventually, “macho” riders saw that bicycles with low wheels and air tires could be even faster than penny-farthings. One form of the technology now satisfied both the macho need for speed and the need for safety. Closure had been reached and the artifact became stabilized in something like its modern form.[12]

This basic approach has been elaborated by Bijker, Pinch, and others. Bijker expands the bicycle example and uses additional case studies of Bakelite and the fluorescent light bulb to paint a more nuanced picture of the institutional and industrial settings in which technology is developed. He introduces the concept of a "technological frame" — a dominant interpretation of a technology, usually backed up by certain tools and techniques — arguing that certain actors are included in a particular frame more than others, which explains why, for example, engineers and marketing executives have more influence over the design of fluorescent light bulbs than the average consumer.[13] Donald MacKenzie draws indirectly on Bijker and Pinch in his study of the history of nuclear missile guidance systems. He argues that institutions, rather than internal principles of technological development, shape the trajectory of technological change, and that different social groups — such as opposing pro-bomber and pro-ICBM groups within the military — developed radically different interpretations of missile tests and accuracy estimates.[14] Ronald Kline and Trevor Pinch show how users can reinterpret and modify already-stable technological artifacts, as when farmers modified early automobiles to power saws and washing machines or to serve as tractors.[15]

Interpretative flexibility and the context of invention

All of this work emphasizes the extreme flexibility of technology. Little attention is paid to the ways in which technology may sometimes frustrate our efforts to control it and constrain our actions. If a technology seems to be resistant to certain lines of development, this is usually explained in terms of social or institutional stabilization. Furthermore, Bijker and Pinch explicitly argue that the material flexibility of technology is an integral part of the idea of interpretative flexibility, and the other authors all seem to assume this to some extent. The work of invention and design appears to lie almost entirely in trying to understanding the interpretations and satisfy the needs expressed by various social groups, while the form of technology follows easily from these interpretative efforts.

This vision of technology makes sense mainly because all of these authors have chosen to look at a particular aspect of technological change: design and invention, particularly in reference to relatively inexpensive mass-produced consumer goods and weapons. These are usually replaced frequently over time — middle-class Americans keep buying new cars, the military continually procures new weapons. Because they are looking at these kinds of technologies, when these authors talk about different interpretations of an artifact — the bicycle, for example, or even one specific type of bicycle — they are not really talking about any particular artifact, but rather a succession of different artifacts, each copied many times over. No wonder the meanings we attach to technology appear to be so readily translated into material form in their work: the changes they describe do not involve modifying existing objects, but turning concepts into brand new products that will be built from basic parts and materials.

Most technological work is not nearly so open-ended. Civil engineers, I have argued, face a much more constrained design task, especially when they are engaged in tasks like seismic retrofit. In their work, technological artifacts often appear as independent entities, and it sometimes seems to take a great deal of engineering effort to bring them back under control. In order to account for this, the constructivist approach should not be so quick to throw out all vestiges of technological determinism. Specifically, it ought not to assume that interpretative flexibility entails material flexibility as well. These should be treated as two separate concepts, one referring only to the idea that people can attribute many different meanings to a given artifact, and the other referring to the extent to which we are able translate our interpretations into material form. We need to look more closely at circumstances in which technology doesn't seem to be as flexible as our interpretations of it. Seismic retrofit is one such circumstance.

7.4 Engineers confront bridges

There are at least three ways that engineers encounter objects — in this case, massive civil structures like bridges — as independent, inflexible entities through retrofit work: structures are durable in the face of changing engineering practice, they change over time in ways that can't be easily documented, and they become embedded in local material and social settings.

Durability and change

The principle of interpretative flexibility, so long as it is divorced from material flexibility, applies as much to buildings and bridges as to bicycles and light bulbs, and as much to the engineering profession as to any other social group. Principles of good engineering are constantly being updated in the light of experience and research. But when a new generation of bridges appears, we can't just throw the old ones away in landfills and junkyards, as we would with many consumer goods. They are too expensive and too crucial to a wide range of activities for that. So regardless of how much engineering practice has changed, relics of past practice will always remain, even though they may come to be considered outdated or defective. Bridges are durable even though engineers' interpretations of them are not.

There is no better example of rapid change in civil engineering practice than in the area of earthquake engineering during the 1970s and 80s. In the 1960s, engineers designed bridges in California with widely-spaced hoop reinforcement and unrestrained deck joints, checking only to see if they would stand up to a small horizontal force; at the same time, they used the latest computer tools to design bridges that soared more gracefully and had fewer support columns than before. At the time, of course, these bridges were seen as symbols of cutting-edge engineering and as models of seismic safety. Then in the 1970s, 80s and 90s, the specialty of earthquake engineering expanded rapidly and found new sources of funding, leading to more research and a much more detailed understanding of how structures respond to earthquakes; these new understandings were incorporated into more and more sophisticated computer programs made possible by increasingly fast computers; and, finally, major earthquakes caused damage to Caltrans bridges and provided the impetus for engineers to look again at the structures they and their predecessors had designed before. The definition of an earthquake-safe structure changed dramatically: columns should be designed with closely-spaced spiral reinforcement, joints should be restrained so they won't move too far, and underlying seismic risks should be analyzed more carefully.

It was through these changing interpretations of earthquake risk and structural performance that many of the older bridges in California came to be seen as unsafe. Like any engineers faced with such a situation, Caltrans bridge designers responded to these new interpretations by building new bridges differently. But they also began to consider ways in which the existing bridges could be made safer through retrofit. This task took on added urgency when geological and political circumstances combined to draw public attention to the fact that many bridges did not measure up to current engineering standards. But while designers can decide to do things differently with new bridges, they aren't able to simply forget the past while doing retrofit work. Instead, they are forced to grapple directly with the engineering practices and design decisions of their predecessors. Even engineers who would not ordinarily be interested in the history of their field get the opportunity to learn something about it through this work. Technology takes on an element of autonomy here simply because it can't change in response to our new ideas about it.

Documenting change

Structures also escape the control of engineers over time by changing, or being changed, in ways that are not or cannot be documented in the visual language of engineering plans. Engineers and architects try to maintain true representations of structures by retaining and updating what are commonly known as "as-builts": engineering drawings initially created during the construction process in order to document how a structure was actually built, including any deviations from the original plans. These, rather than design drawings, are usually retained on file for the life of the building or bridge. Theoretically, as-builts are supposed to be updated continuously to reflect modifications, but in reality they are often neglected. Brand notes that the as-builts for factories are usually scrupulously updated every time any change is made, simply because the building is one of the key assets of a manufacturing firm. But often design engineers and architects don't pay a lot of attention to as-builts, because most of the time they aren't directly concerned with what happens after something is built.[16]

Caltrans keeps as-builts for all their bridges on file, but keeping them up-to-date was apparently not a top priority, as engineers found out when they needed to retrofit. Sometimes the as-builts did not accurately reflect what went on during construction. This often had to do with irregularities that the engineers who were on-site during construction might not have known about — piles that ended up at not quite the right angle when they were driven into the ground, or footings that spread out into the surrounding soil more than usual when the concrete was poured.[17] Sometimes what was done during construction was simply not documented accurately. For example, designers working on the retrofit of the San Diego-Coronado bridge, one of the state's major toll bridges, needed to know whether lateral stiffeners on some of the bridge's huge girders had been welded to the top flange of the girder or not in order to put together an accurate computer model. In this case, the detail was probably left out because it is conventional not to make such welds, but the designers had to be certain.

In other instances, significant changes made after construction were never recorded in as-builts, or were recorded in some sets of as-builts but not others. For example, designers working on retrofits after the 1989 earthquake on at least one occasion determined by looking at as-builts that a bridge's deck joints needed retrofitting, only to find out during construction that retrofit devices had already been installed.[18] When I toured the San Diego-Coronado bridge with one retrofit designer, our tour guide, who worked in maintenance and was very familiar with the structure, pointed out a number of features that were not on the designer's as-builts, including electrical conduits that had been installed along the bottom of the bridge deck to supply power to a machine that moved lane dividers, and holes that had been drilled in the deck for the insertion of lane-marking poles and had caused the underlying steel to rust. These particular details weren't structurally significant, but could have interfered with the installation of retrofit devices.[19]

Structures also change over time without any direct intervention from people — from age, use, and exposure to the elements. Concrete, for example, tends to shrink with age, and other materials may deform in unexpected ways as well. Bridges have expansion joints and other movable parts that accommodate these changes. Caltrans maintenance engineers regularly inspect bridges and note any serious problems, but they don't usually make note of minor, expected changes, such as small movements at expansion joints. Inaccessible parts of structures, like underground or underwater piles, are inspected less regularly because of the effort involved, so less up-to-date information is available about them. Usually, any serious deterioration is noted before it poses a risk to the structural integrity of the bridge, but retrofit designers also need to know about less significant changes so they can properly model the structure.

Engineers cope with the uncertain relationship between as-builts and structures in the field in a number of different ways. One strategy is to dig through more archives — maintenance records or files at the Caltrans district office in charge of the bridge, for example — to try to get a more complete list of changes. Very often, though, the uncertainties are only fully resolved by direct inspection. For the Coronado Bridge retrofit, for example, divers were sent to document the condition of underwater concrete piles, and found cracking that had exposed some of the reinforcing steel, causing it to rust.[20] In this case, models of the piles were being tested in a university laboratory, and the cracks were replicated on the models. The retrofit team also took measurements themselves, in one instance shutting down some lanes of the bridge for a morning so they could go out with tape measures and determine how far the expansion joints had separated.[21] Even after such inspections, however, there are still some changes that only become apparent during the construction process, such as when workers try to put down new piles and find themselves hitting existing ones, or when they drill into the concrete to install seat extenders — metal bars placed across an expansion joint to prevent it from separating — and run into the bars that are already there.

These examples illustrate the difficulties engineers face in maintaining accurate representations of an object over time. In the case of Caltrans, it is true that engineering representations could have been made much more accurate in many respects simply by putting some effort into developing a more integrated and well-organized record-keeping system, as Brand suggests we do for buildings.[22] But the rapid accumulation of changes on an enormous structure like the San Diego-Coronado bridge would still stretch the limits of such a system. More fundamentally, engineers will always have to use some kind of criteria to decide which changes are important, because documenting every tiny change in a large structure could be an infinitely time-consuming task. And since choices about what aspects to document will inevitably reflect the engineering culture of the time, there is no guarantee that the records that are kept will be adequate for the needs of future engineers. Engineers' representations of an existing structure will always be at least a little bit out of date and a little bit inaccurate. Because engineers' control over the material world depends largely on their skills in manipulating representations, this means that objects are always just out of the control of engineers once they are built. When retrofit becomes necessary, designers are forced to go out and learn directly from the structure.

Embedding and resistance

Structures can also change independently of the intentions of engineers by becoming embedded in local material or social circumstances over time. In the material sense, structures often end up interpenetrating with other elements of the local infrastructure. Bridges are sometimes used to carry electrical or communications lines, or even water pipes. Lighting and other services might be mounted on the bridge structure. Most of these changes are of the sort that could be documented by an agency like Caltrans, but in practice they do not always make it into as-builts because they are installed under the authority of other local or state agencies.

Bridges and other structures also become embedded in a similar way in local history and social circumstances. Here I'm not referring simply to their functional role in enabling transportation and changing mobility habits, though these are important. I am also referring to the cultural meanings that people in the wider community invest in material objects as a result of interpretative flexibility. These meanings can stabilize over time, out of the control of engineers, placing serious constraints upon them when it comes time to retrofit. Of course, bridges begin to acquire such meanings the moment they are conceived in the mind of some engineer, planner, or politician. Therefore, even the design of a new bridge is an act of "heterogeneous engineering" that requires engineers to take a wide range of actors and meanings into account. It is important to understand how this process works in order to see how the retrofit design process is different. Take the San Diego-Coronado Bay Bridge as an example.[23]

Figure 7.1: The San Diego-Coronado Bay Bridge: View from Coronado toward San Diego. Note Navy ship passing under the bridge. Photograph by the author.

Building the Coronado Bridge

Various groups made plans as far back as the 1920s to build a bridge between the city of San Diego and the "island" of Coronado, which is actually the end of a peninsula that connects to the mainland well south of San Diego via a narrow, sandy causeway. These efforts were supported mainly by the business community and those with an interest in expanding tourism, but in every case they failed either because of intense opposition from Coronado residents who saw it as a threat to the small-town atmosphere on their "island" or because of concerns raised by the Navy that a bridge would restrict access to the naval base in San Diego Harbor. Since many Coronado residents were retired military officers, the two interests reinforced one another.[24] During the 1950s and 60s, planning for a bridge began in earnest with the support of Governor Edmund G. Brown, a champion of bridge-building as a tool of economic development.[25] During the planning stage, engineers were forced to take Navy concerns into account by including underwater tunnels as an option in place of a bridge. In the end, possibly because of cost concerns, a bridge was chosen over tunnels, and design began in the early 1960s. The Navy softened its stance against a bridge, possibly because of political pressure from Brown allies in Washington.[26]

Initial designs called for a straight bridge between San Diego and Coronado. With a 6% grade — which is considered very steep for a bridge — the bridge would have

Figure 7.2: View of the Coronado Bridge as it meets the coastline of Coronado. Photograph by the author.

180 feet of clearance in the center of the channel. The Navy, however, demanded at least 200 feet to accommodate its largest ships. The engineers' solution to this dilemma was to maintain the grade as it was but to make the bridge longer by putting in a 90 degree curve in the middle of the bay, making the added height possible.[27]

There were other important influences on the final shape of the bridge. For example, the location of the two ends of the bridge was dictated in part by the need to place the highest portion of the bridge directly over the main shipping lane, which is closer to the San Diego side. The bridge structure therefore touches down at the very edge of Coronado (Figure 7.2), while extending perhaps a quarter of a mile inland on the San Diego side to meet up with Interstate 5 (Figure 7.3). This was made possible, however, because the San Diego approach ran straight through a poor Mexican-American residential area. This was a "path of least resistance" at the time, where property values were low and where residents had little political voice.[28] Having the bridge come only to the edge of Coronado took care of a potential reason for opposition to the bridge from politically connected Coronado residents who wanted minimal disruption of their lifestyle.[29]

As this example shows, engineers balance a host of technical, political, and

Figure 7.3: View of the Coronado Bridge coming inland on the San Diego side. Photograph by the author.

economic considerations as they design a bridge. Because the shape of a bridge is very flexible as long is it remains on paper, designers are able to accommodate the form of the bridge to satisfy these heterogeneous constraints. The layout of the Coronado Bridge, for example, represents in material form a compromise between the conflicting interpretations of the Navy, politicians, community groups, and engineers, though it deliberately ignores the interpretations of less powerful groups. After a bridge is built, however, it loses much of its material flexibility. As it stands, though, it continues to collect new interpretations, and becomes part of the history and social fabric of a community. High-profile bridges like the Coronado Bridge, and perhaps most famously the Golden Gate Bridge in San Francisco, become symbols of a community, both to residents and to the world.[30] But bridge designers are no longer professionally concerned with these interpretations, having moved on to other projects, and may be unaware of the extent to which a structure has worked its way into the local culture. When they begin the task of retrofit design, engineers often have to confront a set of interpretations that have stabilized in their absence. There may be less room for negotiation and compromise with such entrenched interpretations.

Retrofitting a work of art

Engineers involved in the seismic retrofit of the Golden Gate Bridge and the replacement for the seismically-deficient San Francisco-Oakland Bay Bridge, for example, have had to carefully negotiate with a host of Bay Area planning and environmental commissions intent on preserving these bridges as city landmarks.[31] The City of San Diego has far fewer of these groups, and the Coronado Bridge, while well-loved by many, has not taken on as much symbolic value as the San Francisco bridges. But it ironically developed a great deal of cultural significance to the one neighborhood it nearly destroyed: the largely Chicano community known as Barrio Logan, bisected by the San Diego approach to the bridge. Barrio Logan had been a thriving neighborhood enclave, but by the early 1960s was already suffering from zoning laws that were changed to allow industrial use and from the construction of Interstate 5, which cut it off from the larger Logan Heights neighborhood. The construction of the bridge accelerated these changes and nearly destroyed the neighborhood.[32]

But by the late 1960s, Chicano political activism was on the rise nationally, and was becoming particularly important in California. In 1970, the Barrio Logan community, still upset about the building of the bridge, took action when they discovered that an enormous California Highway Patrol station was to be built on the already desolate empty land under the approach ramps to the bridge. The community was not happy with the prospect of such a heavy police presence, so several hundred residents occupied the construction site for twelve days, demanding that a community park be built instead. City officials intervened and granted the request, and what came to be known as "Chicano Park" was established under the approach ramps.[33] This action is remembered by many as a pivotal moment in the developing political and cultural consciousness of Barrio Logan residents and of the broader Chicano community in San Diego.[34]

Though the park was a focal point of community activity thereafter, it was a noisy and sometimes gloomy place, dominated by the gray concrete of the bridge structure. Partly in order to combat this gloominess, a loose coalition of local artists conceived of the idea of painting murals on the bridge columns in the park.[35] In this, they were part of a larger revival of Chicano "muralism," inspired by Mexican artists like Diego Rivera, Jose Orozco, and David Siqueiros, that saw murals as an expression of community solidarity, cultural heritage, and political resistance.[36] Over the next thirty years, no less than forty brightly-colored, symbolically-dense murals were painted on bridge columns and abutments in the park (Figure 7.4). Most were painted by local artists, but major muralists from throughout California and the southwest contributed as well. Some murals depict cultural figures and events, or display political messages like "VARRIO SI, YONKES NO" (barrio yes, junkyards no) or "NO RETROFITTING." Others make more purely artistic statements, such as one titled "Collosus" that depicts a muscular figure carrying the bridge deck on his back, his torso painted on the column and his arms outstretched along the beam supporting the deck. Another, "Tres Grandes y Frida," is a striking, impressionistic portrait of muralists Orozco, Rivera, and Siqueiros with painter Frida Kahlo (Figure 7.5).[37] These examples only hint at the diversity of themes represented in the murals in the park.

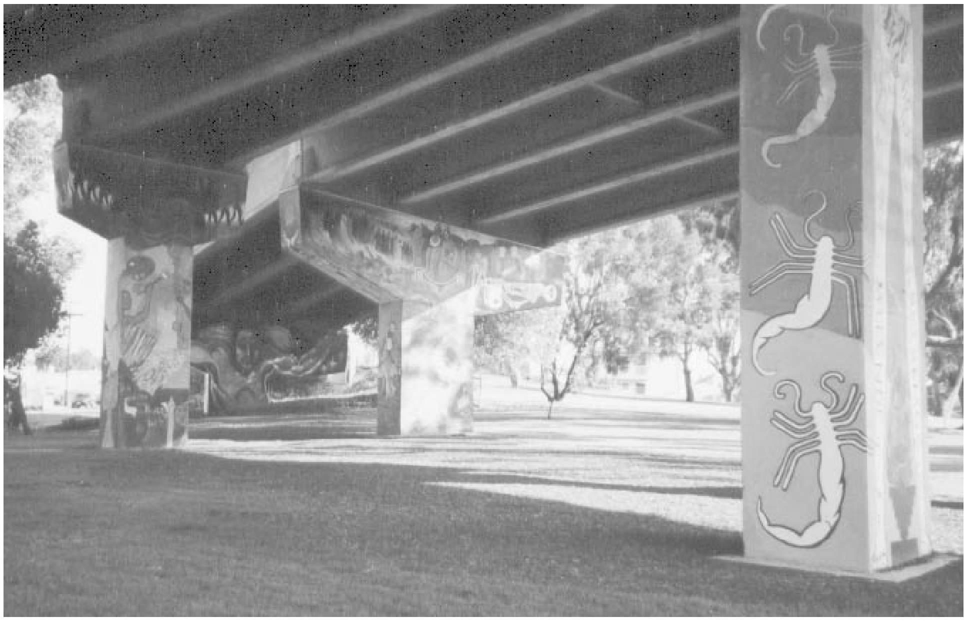


Figure 7.4: Chicano Park murals, painted on bridge columns. Photograph by the author.

As painting continued, the murals came to represent a certain degree of community ownership of the bridge that had so damaged the neighborhood. And to some, the bridge itself began to take on new aesthetic qualities. To many people in San Diego, the most striking feature of the bridge is the curving, bright-blue sweep of the deck girders as the bridge extends across the bay. But from Chicano Park, the most striking feature is the pairs of columns marching in a line toward the bay, oddly resembling the nave of a cathedral. This isn't completely coincidental, since the architects who consulted on the design of the bridge had intentionally designed the columns to resemble mission-style arches, a fact which is best appreciated from under the bridge.[38] The depth of this reinterpretation of the bridge by the artists and the community is reflected in comments made by muralist and community activist Salvador Torres to a newspaper reporter in 1989:

> When I look into the depth of the columns, as the arches flow toward the waterfront, I hear a sound, a mystical sound, like that of a living creature. To me, the bridge is life — reassurance, reaffirmation . . . love. I know what the birds must feel when they fly over. They feel *pride*, in a fortress of beauty and strength.[39]

While some Caltrans engineers and some of the local bridge engineers that were

Figure 7.5: Mural “Tres Grandes y Frida,” Chicano Park. Photograph by the author.

contracted to design the retrofit were aware that the murals could pose a problem, they do not appear to have been aware of the depth of the community's commitment to the artwork. Early in the design process Caltrans held public meetings where it presented a full range of possible retrofit measures, most of which would have had a significant impact on the murals — including completely replacing the columns, encasing them with steel jackets, and thickening the existing columns in the lateral direction. Both community activists and elected officials began to protest immediately. Decades of distrust between the community and Caltrans rose to the surface, and some questioned whether the bridge really needed to be retrofitted at all. An artists' group sent out a newsletter demanding "no retrofit, not now, not ever!"[40] Local politicians and newspapers soon took up the cause as well. Clearly, the power structure in the city had changed considerably since the 1960s.

Caltrans engineers and design consultants were in a difficult position: they were pretty sure they would have to retrofit the columns in a way that would affect the murals, yet this did not seem to be a viable option, politically speaking. Caltrans officials in Sacramento had meanwhile appointed a peer review panel to oversee the retrofit design. One member of this panel was Frieder Seible, structural engineering professor at U.C. San Diego. By this point, the Caltrans local district personnel who were in charge of community relations had realized they could not handle the situation on their own in the prevailing atmosphere of distrust. So they asked Seible to intervene, with the idea that he could explain the necessity of retrofitting to the community. Seible invited community activists to tour the UCSD structural engineering laboratory, where he explained to them the basic reasons for retrofit and showed them test specimens that had been put through simulated earthquakes. This made a big impression, and convinced many of those present of the need for retrofit. It was apparent to Seible that the community trusted him much more than Caltrans officials, perhaps because of his academic position and perceived independence from the department.[41]

In addition to convincing the community that retrofit was necessary, Seible also pressured project designers to do a very detailed analysis of the columns, instead of relying on standard Caltrans design methods and retrofit techniques. Something like steel jackets around the columns would be standard in this case, because there were "lap splices" in the reinforcing steel. What this means is that each vertical reinforcing bar was not a continuous piece of steel from the foundation through to the cap. Instead, each strand of vertical reinforcement consisted of two bars that overlapped for a few feet within the column, held together only by the surrounding concrete. This was standard practice in the 1960s, but engineers have since come to understand that lap splices can easily pull apart in an earthquake. This can be avoided by "clamping" the lap splices more firmly in the concrete, for example by placing a steel jacket around the column. In this case the lap splices had been staggered, so that some were in the middle of the columns, while others were very close to the footings, below ground level. Based on the more detailed calculations and new soil data, it was determined that the columns could be strengthened sufficiently simply by adding a new "mat" of reinforcing steel and concrete to the top of the footings, which would strengthen them and at the same time provide confinement for the lowest set of lap splices. It was also necessary to do some work near the top of the columns. But since most of the work would be done below ground level, the murals could be preserved. The retrofit designers could feel more comfortable taking this unconventional approach because on the peer review panel, Seible had main responsibility for the Coronado retrofit, and he had already endorsed the approach.[42]

Design under pressure

The case of the Coronado bridge retrofit and the Chicano Park murals is a vivid illustration of how the factors discussed above — the durability of structures in the face of cultural change, the undocumented changes that occur in them over time, and the way the become embedded in local material and social circumstances — can come together to constrain the designer's task. The murals, in particular, combine the social and material aspects of embedding; the bridge developed great significance to the community not only because of the social interactions that occurred around it, but through their material transformation of the structure and the land surrounding it. Caltrans engineers and officials faced a social and political problem that could ultimately be solved only within the material form of the retrofitted bridge. The murals were also a change that had been made to the structure outside of the control of engineers, a change which they had to assimilate into their practice. Finally, designers had to see how the columns had been built originally, and figure out how to analyze their outdated reinforcement configuration, which further restricted their range of action. After putting in so much engineering effort, and after all of the complex negotiations with the community, the end result was a fairly straightforward footing retrofit, along with some changes at the top of the columns. After working their way through all of the constraints, when the engineers got to the point of actually designing the reinforcement mat for the footing, there was little flexibility remaining. Most of the effort went into learning about the material and social structures that existed in the field and had long ago escaped the control of design engineers. Some of the project engineers felt they had been lucky to find any engineering solution to the problem at all.[43]

7.5 Autonomous technology and human choice

When they work on retrofit projects, engineers find that even after they have addressed the concerns of all relevant social groups and have satisfied the technical conventions of their profession, the range of possible solutions to the design problem is limited still further by the existing material object that is the focus of the retrofit effort. This is not to say that technology has an objective, internal logic that it unambiguously imposes on engineers, or that it possesses a human-like agency.[44] As constructivist studies of technology have shown, engineers base their design efforts on their own, culturally-conditioned interpretations of the world. They interpret the needs and desires of the relevant social groups, the conventions of professional practice in their field, and the relevant physical principles, and each of these interpretations further constrains the design. When they are working with an existing structure, they bring it into the design process through socially-mediated interpretations, as well. They look at maintenance reports and as-builts; they go out to the structure and inspect and measure it. The understandings they take away from these activities are always filtered through their professional experience and socialization. Still, just having another object to consider ultimately restricts their options even further. Through their interpretative activity, the existing structure has a causal impact on the final design. And through the medium of the structure, the designers are also grappling with specific engineering decisions made by their predecessors.

Our technological inheritance

Though the sort of technological determinism that claims there is a logically determined sequence to technological change is not supported by this analysis, another, more ironic, form of technological determinism is. This form of technological determinism is best explained in reference to large, durable objects like civil engineering structures, which are too expensive to simply get rid of or ignore. Given that an existing structure does seem to limit the range of actions we are willing to take, and given that many structures that were built, say, 30 years ago are still around, the decisions that engineers made 30 years ago, based on their interpretations of technology at the time, have an effect on the decisions we make today, even though our interpretations of technology have changed. In turn, our decisions, limited as they are by those taken 30 years ago, are limiting to future decisionmakers. Barring some unprecedented disaster, we will never be given the luxury of starting with a completely clean slate. Even though technological change is ultimately driven by human choices, the durability of our past choices limits our future courses of action, and technological change develops momentum in certain directions in ways that we don't have complete control over.[45]

Other researchers who have studied infrastructure have come to similar conclusions. Historian Thomas P. Hughes, whose work focuses on electrical power systems, argues that technological systems acquire a kind of momentum over time which predisposes them to evolve in certain directions. This is a result both of the inertia imposed by organizations and individuals that become committed to a particular direction of change, and of the durability of the material objects that make up a system, which are often too costly to replace in the short term.[46] Similarly, Susan Leigh Star and Karen Ruhleder, in their study of information infrastructure, note that new infrastructure is never designed "*de novo*" but always "wrestles with the inertia of the installed base."[47]

The study of infrastructure is important for understanding the nature of technological change because it draws attention to the fact that future generations will have to live with the technological choices that we make today, and won't be entirely free to ignore the mistakes we make. But it would be wrong to restrict this analysis to infrastructure. Even consumer goods with a limited useful life don't simply disappear into thin air when we are finished with them. Every insignificant plastic bag, lawn chair, or bicycle that has ever been made is still with us in one way or another, most likely taking up space in a landfill. Other technological byproducts, like radioactive and chemical waste, pose dangers that we have to address over time in a much more active way. Furthermore, goods that are assumed to have a limited useful life among the wealthier nations and social classes of the world are often far too expensive for others to simply discard; hence, people continue to maintain and drive older, more polluting cars even as we try to make new cars more and more environmentally friendly. Because technology is durable, we must always make new interpretations and choices amid the debris left by our ancestors.

7.6 The local embedding of globalizing technologies

Though the emphasis here has been on ways that technology can sometimes escape our control, it has also been noted at every step that technological artifacts are surrounded by locally embedded social interactions — interactions based on personal familiarity and face-to-face interaction. Engineers designing or retrofitting a bridge work in close-knit teams that are bound together by personal interactions, and the feelings of a community toward a bridge, for example, are articulated in the meetings of planning commissions and neighborhood activists. In fact, when a technological artifact is surrounded by embedded social circumstances in this way, it can be said to become locally embedded itself. People's interpretations come to be based not on abstractions or broad cultural symbolism, but on a kind of personal familiarity derived from direct contact with the artifact. Engineers confront a bridge in person in order to design a retrofit; a neighborhood makes an imposing bridge into a familiar focal point for social interaction. A bridge that increases people's geographical mobility, and hence further disembeds some social interactions, can simultaneously become part of a new set of embedded social relationships.

Anthony Giddens and others have argued that modernity is fundamentally about the dialectic between the local and the global. The disembedding brought on by increased globalization is said to open up new ways of attending to the local, a process Giddens calls reembedding.[48] But his analysis of reembedding draws on examples like this:

> The self-same processes that lead to the destruction of older city neighbourhoods and their replacement by towering office-blocks and skyscrapers often permit the gentrification of other areas and a recreation of locality.[49]

By focusing on such self-conscious attempts to recreate a lost sense of locality, theorists of modernity may miss a more fundamental aspect of the relationship between the local and the global. The very mechanisms that make disembedding possible — modern transportation or communications infrastructure, or social institutions like financial markets — aren't just abstract structures. They exist and are given meaning only through the efforts of the groups of people who run and maintain them. They can't be built up without the simultaneous creation of locally embedded networks of interpersonal interaction. This chapter has shown that technology, in particular, is always already a product of embedded social relations and locally-generated interpretations. Focusing on the social impact of technology — its potential to disembed and reembed social relations on a broad scale — at the expense of the social context of its production is, ultimately, to promote an unwarranted form of technological determinism.

Notes

[1] I take Peter Galison to make a similar point in his work on high-energy physics, particularly in his discussion of the importance of "trading zones." See Galison 1997, especially 781-844.

[2] On the work of repair technicians, see Henke 2000 and Orr 1996.

[3] Brand 1994, 2.

[4] On the politics and cultural significance of large water engineering projects in the western U.S., in particular, see Reisner 1993.

[5] For example, the enormous project to completely replace the I-40 - I-25 interchange in Albuquerque, New Mexico, which is supposed to be performed in record time, with two lanes always open in each direction in order to avoid massive traffic jams.

[6] Winner 1977.

[7] See definitions in MacKenzie and Wajcman 1985, 4; Winner 1977, 76; and Bimber 1994, 84.

[8] The term was originally used by H.M. Collins to describe variations in the interpretation of scientific findings. See Collins 1981b, 7.

[9] Misa 1988 argues that “micro” analyses of technology — such as design case studies — tend to confirm the social constructivist perspective, while “macro” historical works tend to lend support to versions of technological determinism.

[10] See in particular Pinch and Bijker 1987 and Bijker 1995.

[11] Pinch and Bijker 1987, 28-40.

[12] Pinch and Bijker 1987, 40-46.

[13] Bijker 1995.

[14] MacKenzie 1990.

[15] Kline and Pinch 1996; the focus on the user is partially in response to the criticism of SCOT in Mackay and Gillespie 1992.

[16] Brand 1994, 128. Brand notes that, ironically, the headquarters building of the American Institute of Architects in Washington D.C. was built (in 1970) without as-builts, “making the later all-too-necessary renovations even more of a pain,” 207.

[17] Interview with three Caltrans designers, May 30, 1997.

[18] Ibid.

[19] Author’s field notes, February 6, 1997.

[20] Caltrans Office of Materials Engineering and Testing Services, Corrosion Technology Section, “Condition Survey of Prestressed Concrete Piles for the San Diego-Coronado Bridge,” April 1997, EF 68.

[21] Author’s field notes, February 6, 1997.

[22] Brand 1994, 129.

[23] See Suchman 2000 for a detailed analysis of a Caltrans project to replace another toll bridge which raises many of the same issues discussed in this chapter.

[24] Fisher 1996, 2-3.

[25] Ibid., 3.

[26] Thomas K. Arnold, “The Sky Above, the Bay Below,” San Diego *Reader*, January 28, 1982, 10.

[27] For a description of Navy requirements and the grade of the bridge, see Michael O’Connor, “Coronado Span 2 Years Off — After Approval,” *San Diego Union*, April 21, 1964, A-1; for explanation of the curved shape, I rely on an interview with Charles Seim, April 8, 1997.

[28] Interview, Charles Seim, April 8, 1997.

[29] This is conjecture on my part, though it seems consistent with the political realities of the time. The bridge did have some impact, however: besides bringing traffic to the island, it bisected a bayside golf course.

[30] On bridges as civic symbols, see Petroski 1995, especially pages 3-21.

[31] See various documents on plans to retrofit and, later, to replace the San Francisco-Oakland Bay Bridge, EF 69.

[32] Fisher 1996, 5-6.

[33] Ibid., 13; a vivid contemporary account of these events, with photographs of the occupation, can be found in Coffelt 1973.

[34] Fisher 1996, 14; for an overview of the relationship between art and community activism in the San Diego Chicano community, see Brookman and Gomez-Pena 1986.

[35] Fisher 1996, 15.

[36] Ibid., 7-12. Fisher cites Garcia 1974 as a general overview of the connections between recent Chicano mural-painting and the Mexican tradition; for a review of Chicano murals in the larger context of community murals in the United States, see Barnett 1984.

[37] Fisher 1996, 17-19.

[38] Steven Allen, from the Bay Area, was the principle architectural consultant, but Caltrans also brought in San Diego architect Robert Mosher as a consultant. I ran across vague or contradictory accounts about whether Allen or Mosher, or both, were responsible for the design of the arches. Mosher says he was inspired by a bridge in Balboa Park in San Diego. Interview, Charles Seim, May 28, 1997; Interview, Robert Mosher, May 29, 1997.

[39] Quoted in Michael Granberry, “20 Years Later, Span is Loved and Loathed,” *Los Angeles Times*, San Diego Edition, August 2, 1989, Part II, 4. Ellipses in the original.

[40] Reproduced in California Department of Transportation, “Historic Property Survey Report, San Diego-Coronado Bay Bridge, Seismic Retrofit, Project 4,” 11-SD-75, P.M. R21.9/R22.3, 021941, January 1997.

[41] Interview, Frieder Seible, February 17, 1997.

[42] Interviews, Frieder Seible, February 17-18, 1997; author's field notes from Caltrans meeting with Barrio Logan community representatives, December 9, 1996; photocopied draft of the Coronado bridge section of the report of the Caltrans toll bridge peer review panel, given to me by Seible; see also Leonel Sanchez, "Bridge Work Plan Will Spare Murals," *San Diego Union-Tribune*, December 10, 1996, B-1.

[43] I heard this discussed at an employee seminar on the Coronado Bridge retrofit, Caltrans District 11 offices, San Diego. Author's field notes, April 1, 1997.

[44] On "nonhuman" agency, see Latour 1992 and Michel Callon's celebrated description of the negotiations between scientists and the scallops of St. Brieuc Bay, Callon 1986. On "material agency," see Pickering 1995.

[45] Even less durable technologies can have similar effects, though these often have to do more with historical accident. The classic example of this is David 1985, which examines the persistence of the QWERTY keyboard. In David's analysis, though, it was not the durability of technological artifacts that caused the system to stabilize (typewriters are, after all, mass-produced consumer goods), but rather the investment that would be required to change social arrangements — specifically, the training of typists.

[46] Hughes 1987, 76-80; see also the extended study of the history of electrical power systems in Hughes 1983.

[47] Star and Ruhleder 1996, quoted in Bowker and Star 1999, 35.

[48] Giddens 1990, 64-65, 79-80.

[49] Ibid., 142.

Note on Sources and Citations

Because of the diverse sources of information I use, all citations are given in notes at the end of each chapter. Published books and articles are cited by author and date and listed as references at the end of the thesis, while full details are provided in the notes for archival documents. This overcomes the difficulty of fitting these documents into standard bibliographic form, since many of them lack a clear author, date, or title.

Sources for archival documents are identified by the following labels:

Items labeled "GBI" are from the files of the Governor's Board of Inquiry on the 1989 Loma Prieta Earthquake, stored in the basement of the California Governor's Office of Planning and Research, 1400 10th Street, Sacramento, California. Numbers beginning with "CT," "PAS" and "VAR" are Board of Inquiry catalog numbers. Many of these documents are summarized in the annotated bibliography at the end of the Board's report "Competing Against Time" (Governor's Board of Inquiry 1990).

Items labeled "EF" are from the "Earthquake Files" file cabinets outside Jim Roberts' office, California Department of Transportation, Engineering Service Center, 1801 30th Street, Sacramento. Numbers correspond to file numbers listed on the drawers. The relevant files are labeled as follows:

7. DOS Information Bulletins
9. Recovery Efforts
19. Citizen Responses
25. Seismic Advisory Board
27. Seismic Retrofit Program - General

28. Peer Review Teams
29. San Francisco Viaducts - General
53. SASA - Office of Earthquake Engineering
55. Research Program
61. Phase II
62. Toll Bridges - General
68. San Diego-Coronado
69. San Francisco-Oakland Bay Bridge
72. ATC-32

The letters “BN,” “SN” and “WS” refer, respectively, to internal Caltrans newsletters “Bridge Notes,” “Structure Notes” and “What’s Shakin’.” The first two are successive titles for a newsletter focusing on activities within the Division of Structures, while the third focuses specifically on seismic issues. Many of these newsletters are available at the Caltrans Transportation Library, 1120 N Street, Sacramento. More complete sets are kept at the Division of Structures Technical Reference Center, 1801 30th Street, Sacramento.

Interview data is cited by interview subject and date. All interviews took place in California. Interviews with Caltrans engineers and managers were conducted in Sacramento, with the exception of three anonymous engineers who were interviewed in San Diego. Interviews with U.C. San Diego faculty members, graduate students, and technicians, and with Robert Mosher and Charles Seim, were conducted in La Jolla. Stephen Mahin and one anonymous source were interviewed in Berkeley. George Housner was interviewed in Pasadena.

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