BEAMTIMES AND LIFETIMES

The World of High Energy Physicists

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An Anthropologist Studies Physicists

The public and political role of high energy physicists, the role of science as a secular religion, as well as the history, sociology, and philosophy of physics are the subjects of many important books by eminent scholars. This book is not about how physicists have shaped our world, or why our society has given them power and prestige. Nor is it about the current state of knowledge and inquiry in high energy physics. Instead it is an account of how high energy physicists see their own world; how they have forged a research community for themselves, how they turn novices into physicists, and how their community works to produce knowledge.

Since World War II physicists have maintained a special hold on the American imagination. Their discoveries are front-page news; *Time* magazine tries to describe the latest theoretical developments. Autobiographies of leading physicists reach best-seller lists. The image of Albert Einstein is still used as an emblem of intelligence and creativity. When Prime Minister Fukuda of Japan and President Carter met during the energy crisis of the 1970s, they decided to fund a ten-year, multimillion-dollar research project in high energy physics. During the budget-cutting years of recession all the major industrial countries continued to increase the funding of the enormously costly research in this field; and in 1987 President Reagan announced his support for a new "superconducting supercollider" many times larger than the largest high energy physics facility ever before built.[1] How is it that physics and physicists have so strong a claim on our society?

Part of the answer is war. Competition for novel weapons during World War I led to the organization of research and development labs staffed by scientists and engineers. The modest successes of these labs brought funding during World War II to diverse and esoteric projects, and this time the yield was much greater: it included radar and, most decisively, the atomic bomb. The search for the fundamental secrets of nature was suddenly a matter of national power and prestige. The bomb brought political influence to the "atom smashers," and with that influence came money; neither has declined in the succeeding forty years, in spite of the fact that since the war almost all high energy physicists have refused to do secret research or to work on weapons.[2]

Part of the answer is organization. In the new mission-oriented labs of World War II, high energy physicists learned to administer large interdisciplinary teams of researchers, manage huge budgets, and speak the language of government agencies. At the end of the war, many of these same scientists were called in to help reorganize and redefine the goals of the agencies, which have funded basic research ever since; high energy physicists have maintained personal ties and influence in Washington. At the same time, their organizational skills and political acumen have not gone unnoticed in the universities: the expansion of the resources of physics departments is the envy of other disciplines, and many senior high energy physicists have become university deans, provosts, and presidents.[3]

Yet another part of the answer is the emotional power of cosmology. The physicists' calling is awesome: memoirs and biographies often present this corps d'elite as unique, Promethean heroes of the search for truth. Traditionally the mysteries of the universe have been the province of theologians and priests. Physicists of course do not see themselves as writing the cosmology of some secular religion: for them, religion is about belief rather than knowledge. But they do see their own profession as the revelation and custody of fundamental truth, and to a surprising degree Western culture confirms them in this privileged role. They bring news of another world: hidden but stable, coherent, and incorruptible. In times of bewildering and threatening change, this gospel, however esoteric, has a very deep appeal. (Fear may confuse these feelings, but it does not weaken them.) The extraordinary scale and costliness of much physics research if anything reinforces its cultural value. The great accelerators, for example, are like medieval cathedrals: free from the constraints of cost-benefit analysis.

The physicists who work in these great accelerators study the basic constituents of matter and the elementary forces that operate between them. This field of study is called particle physics.[4] (Nuclear physics is a separate subject area, dealing not with the components of the nucleus, but with the relations between nuclei. Its implications lie mainly in nuclear energy, material science, and medicine.)

Though strong in influence, high energy physicists are not strong in number. According to the international leaders of the community, there are about eight hundred to a thousand very active researchers in the world in their field. They suggest that perhaps two thousand more are abreast of the latest developments. Three or four hundred of all these people know one another quite well, and all the other practitioners want to.[5] About half of the group are theorists; they work at blackboards alone or in small, short-lived collaborations of two or three people. The other half are experimentalists; they work with big machines in long-lived groups of twenty to fifty people. The experimentalists and theorists need the others' contributions to solve their scientific problems, but they keep a predictable, friendly distance from each other, and they are readily distinguishable at any laboratory by their styles and habits. In this book I will be writing primarily about experimentalists.

High energy physicists gather together to do research at five major accelerators, in Western Europe, the USSR, the United States, and now Japan. The first major American accelerator was constructed at the University of California at Berkeley in the 1930s under the leadership of Ernest Orlando Lawrence, who was indefatigable in his search for money to support the lab. His accelerator received financial support from several sources: Lawrence publicized the medical applications of procedures developed for basic research in order to gain funding from an eclectic collection of individuals, governmental agencies, private business, and philanthropic foundations. He claimed that the knowledge gained by the new machine was the "beginning of an economic revolution"; when his staff created gold from platinum Lawrence announced that "the information we are getting is worth more than gold."[6] His public appeal that physics should be funded because it ultimately enhances the public good has been copied by today's physicists.

Lawrence's role in the design, construction, research, and funding of the accelerator at Berkeley established a style in laboratory research that is maintained now throughout the particle physics community. The close bonds between a laboratory, its research, and its director is found in nineteenth-century science; Lawrence's innovation was a shift in scale and an adroit use of public opinion to gain funding.

Funding for the high energy physics community is directly determined by national governments: no private sponsorship could maintain a field dependent on machines so massive and so constantly changing. Science funding in both Japan and the United States is determined first by the national government as part of annual budgets. Before this budget with its science research component is voted upon, particle physicists gather into a national high energy physics advisory panel (HEPAP) to determine which projects they will advocate, and hence what allocations they will seek, as a concerted community. Construction of new facilities and operating budgets for the laboratories are established at this level. This committee reports to the Department of Energy (DOE) and the National Science Foundation (NSF) in the United States, and to the Ministry of Education (Monbusho) in Japan. Membership on this committee is an honor and a position of great power. Once priorities have been established, various members begin to advocate the projects and their funding to appropriate government agencies and legislative committees; certain individuals are known to be particularly effective at this crucial stage. A few physicists regularly encourage some of their students to become science advisors in Washington so that they will have loyal representatives working with key committees.

Each laboratory in turn has its own program advisory committee (PAC), which, in principle, sets long-range science policy for the lab and decides which proposed experiments will be done and how much accelerator time will in principle be allocated to each experiment. All accepted experiments are funded entirely from the laboratories' overall budgets. Established groups within the labs also receive funding from the lab budgets for maintenance and development of their research equipment, known generically as "detectors." Access to the accelerator and a detector is crucial in experimental particle physics, and it is the lab's program advisory committee that decides which groups will have that access.

Once experiments are accepted by the program advisory committee they are scheduled by the "long-term scheduling committee," which determines how to coordinate the various experiments simultaneously at the laboratory. Groups want a certain number of pulses per second from the accelerator beam over a number of months ("beamtime") directed to their detector; in addition, they also want scheduled access to computer time and other laboratory facilities. The long-term scheduling committee has the power to allocate time within the PAC's guidelines. As the slate of experiments draws near, the "short-term scheduling committee," another powerful group, assumes control over distribution of time, including reallotments of accelerator beamtime if they are needed because of accelerator or detector failure. The "beam switchyard," which delivers accelerator beam pulses to the diverse detectors, becomes the final arbitrator in last-minute revisions of time distributions. Conflict at this stage is intense. Access to beamtime is a precondition of power in experimental particle physics.

The community I have just described is scarcely the sort of group that is generally thought of as material for anthropological study. Classical anthropologists studied small communities that were non-Western, nonindustrialized, nonliterate . . . in short, as the string of negatives suggests, not like us. The methods and theories that evolved in such situations have in fact been applied to "our" world—to the sort of "developed" societies of which anthropologists themselves are a part—for at least fifty years. A notable early example is Ruth Benedict's *Chrysanthemum and the Sword*, a study of Japanese values undertaken during World War II to assess the role of the emperor in Japanese society. But such studies have been marginal to the main enterprise of anthropology until quite recently.[7]

The new, "repatriated" anthropologists study people with power as well as those without, corporations as well as ghettoes.[8] The status of the inquiring anthropologist changes accordingly, from a knowing, benevolent visitor, backed by technical and political power, to a tolerated, perhaps amusing, marginal presence. These days "informants" offer employment to anthropologists, not the other way around. The physicists I study are always shocked at the low pay of anthropologists and our small research grants; they regularly offer advice on "using the system" more effectively. Most significantly, we have become informants ourselves; our reports and interpretations are read and debated in the communities we study. The anthropologist no longer has the last word in the dialogue of fieldwork: when I submit a manuscript for publication, those who are asked to review its merits always include physicists. And since an anthropologist often studies the same community throughout her entire career, the conversation between us may go on for forty years.

Anthropologists study relatively small communities, usually not larger than three to five thousand people. A community is a group of people who have a shared past, hope to have a shared future, have some means of acquiring new members, and have some means of recognizing and maintaining differences between themselves and other communities. The high energy physics community meets this definition. The first condition of anthropological study is that the anthropologist live in a community long enough to observe a full cycle of routine activities. In an agricultural society, it takes the four seasons of a year to observe all the activities associated with food production. In a research laboratory, a full cycle comprises the planning and execution of an experiment.

Completing an experiment in high energy physics can take three to five years. First, a research group selects a physics question of current interest to the community and decides on a procedure for confirming one of the proposed answers. It then designs equipment for carrying out this procedure; as I shall discuss in more detail later, high energy physics experiments almost always depend on innovations in equipment. The entire experiment, with costs, is described in a written proposal, typically of about 150 pages, which is submitted to the lab's Program Advisory Committee (PAC). At the same time group members give lectures at other laboratories and university physics departments, describing the virtues of their proposal.

If the PAC accepts the proposal, the research group begins "tooling up," drafting detailed descriptions of the equipment and software and assigning primary responsibility to individuals for various components of the experiment. Production and assembly will usually take one to three years. Conducting the experiment itself may take about one year; during that time the group monitors, repairs, and modifies the equipment and begins to analyze the data being collected. The group may revise the equipment, the software, or

the shape of the experiment, if early data suggest that a change is needed. After the experiment is completed, a year or so of intensive data analysis begins. When the group members agree that they have found interesting data, they arrange to give another round of lectures, trying to persuade their colleagues of the significance of their results. If their arguments are well received, the speaker and other members of the group "write up their data" in articles submitted to scientific journals. All the members of the group, in alphabetical order, are listed as authors, although informally everyone comes to know who contributed what. Copies of submitted articles are circulated as "preprints"; as a rule active researchers read preprints, not journals. Graduate students write up for their doctoral thesis their own contribution to the experimental design, to the equipment, or to the analysis, but theses are seldom read except by the candidate's teachers. The important communications are made by word of mouth: by informal talk and by lectures and seminars. In five years of fieldwork I witnessed each of these stages of producing an experiment.

The account written as an outcome of anthropological fieldwork, an ethnography, usually includes information about four domains of community life.[9] The first is ecology: the group's means of subsistence, the environment that supports it, the tools and other artifacts used in getting a living from the environment. The second is social organization: how the group structures itself, formally and informally, in order to do work, to form factions, to maintain and resolve conflicts, and to exchange goods and information. The third is the developmental cycle: how the group transmits to novices the skills, values, and knowledge that constitute a sensible, competent person; the stages of a life and the characteristic attributes of a person at each of those stages. The fourth is cosmology: the group's system of knowledge, skills, and beliefs, what is valued and what is denigrated.

These four domains can be separated only in an artificial way, for purposes of analysis: in all human action all four are present, in some configuration distinctive to the group. Any ethnography, whatever its primary focus, must address all four domains and the relations between them if it is to contribute to an account of the culture of the group. One current definition, developed in the work of David Schneider and Clifford Geertz, makes "culture" a group's shared set of *meanings*, its implicit and explicit messages, encoded in social action, about how to interpret experience.[10] The ethnographer tells how those meanings are generated, maintained, and transmitted in different ecological settings, and how they affect the group's ecology. An ethnography describes *patterns* of explanation and action, the meanings people bring from one situation to another, the connections and distinctions people make between certain actions, feelings, ideas, things, and their environment; these patterns make up the culture.

The fieldworker's goal, then, is to find out what the community takes to be knowledge, sensible action, and morality, as well as how its members account for unpredictable information, disturbing actions, and troubling motives. In my fieldwork I wanted to discover the physicists' "common sense" world view, what everyone in the community knows, and what every newcomer needs to learn in order to act in a sensible way, in order to be taken seriously. I wanted to understand how young physicists in Japan and the United States learn the ethos of their community, how they are immediately recognizable to others as high energy physicists; how the young person comes to display good physics judgment, commitment and trustworthiness; how physicists, research equipment, and data are taken to be reliable and trustworthy; why these qualities are so valued in the community; and what happens to those who violate these expectations. The first goal is to discover what counts as being the right kind of person in the community one studies.

I wanted to find out how the physicists generate the shared ground that all members of the community stand upon; how they define the established terrain within which debate can occur, the recognized strategies for making data and equipment and reputations, and the ground rules for contesting data, machines and reputations. Describing and explaining how knowledge in science and technology is *contested* is the subject of many books and articles published during the past fifteen years. Through meticulous case studies researchers have shown how scientists and engineers use their accumulated resources (reputation, funding, students, technicians, and laboratory space, equipment, and techniques) to make strategic experimental choices, how they make and make use of data to construct and defend their intellectual positions, how they recruit supporters and defeat critics, and how the written accounts of their work reflect and conceal this elaborate and stylized combat. I have chosen to describe how scientists and engineers construct

the ground on which this contest is waged, how they all can agree on what can be contested, how they all can agree on what is an interesting or a boring contest. I believe that to understand how scientific and technological knowledge is produced we must understand what is *uncontested* as well as what is contested, how the ground state is constructed as well as how the signals called data are produced. When I speak of the shared ground I do not mean some a priori norms or values but the daily production and reproduction of what is to be shared.[11] In my research I wanted to find the forces of stability, the varieties of tradition, in a community dedicated to innovation and discovery.

Anthropologists collect life histories, stories about knowledge, legends, myths, and theology, as well as information about networks, generational relations, negotiation, leadership and followership, conflict, change, and stability; they also observe the construction of artifacts ("material culture") and collect descriptions of them. This kind of fieldwork, conducted in settings arranged and structured by the community, not by the researcher, is known as participant-observation. The "observation" aspect calls for detailed attention to how the people in the community conduct themselves in daily life: the goal is a "thick description" of settings, language, tone of voice, posture, gestures, clothing, distance, arrangement of movable objects, and how all this changes from one interaction to another. The "participant" aspect calls for the fieldworker to take account of how the group responds to her, the stages by which she gradually comes to be accepted or at any rate tolerated.

The anthropological literature is rife with "fables of rapport," feelings of communion with the subjects of investigation. Whether we like or dislike our informants, whether they like or dislike us, does not determine the validity of our observations or interpretations. A good fieldworker explores the foundations of empathy and antipathy and uses them to examine the local commonsense notions of what counts as the "obvious" responses, the adult way to act in everyday interactions. She explores the local social construction of emotion, of gender, how people come to want to do what they should, and how they cope with anomalies and transgressions.

The fieldworker needs to remain marginal. If she were to become a fully integrated participant in the community, its sociocultural assumptions would no longer stand out in the foreground of her attention; and in any case it would no longer then be appropriate for her to be asking questions about the meaning of social actions. On the other hand, if she "learns" too slowly, her informants may become exasperated, impatient, or bored. It is part of the job to make social "mistakes," to note which of her actions are accepted as worthy, which are treated as inappropriate, repulsive, or ridiculous, and how approval or disapproval is conveyed. All this is taken as information about how the group maintains its boundaries and guides its own members toward acceptable behavior. The fieldworker learns to ask if her own embarrassment and anxiety are culturally induced emotions, designed to make one wish to do what one should do in a given culture.

Clearly, inquiry by participant-observation cannot maintain the distinction between subjective and objective knowledge. It does not assume that the relations between investigator and subject should be distant or dispassionate, still less that the investigator should control that relationship or the subject. To the contrary, it requires the investigator to have a close and complex relation to the subject, and to be rigorously conscious of her "objective" and "subjective" understanding of the community as well as the interaction between her observations and her affective responses. Physicists, of course, adhere to a strikingly different model of inquiry—which has led to many, many discussions between us about the nature of knowing and knowers.

The major fact of this kind of research is that the fieldworker lives her days and weeks and months within the patterns of the community's life, moving in spaces shaped by the community and taking part in its activities on its terms. As a fieldworker she learns what the informants take to be interesting, boring, useful, catastrophic, funny, fortunate, troubling, exciting; she learns the right actions for each situation, how things fall apart and how they are mended. While participating she must try to observe and remember every detail. This intense awareness is quite difficult to sustain, in part because she receives no direct appreciation for it. The lack of reinforcement and the inability to lead her life on her own terms are the fieldworker's burden. At the beginning she chafes; toward the end she feels torn from the life she has learned to lead. This separation is rehearsed daily as she retreats to write up the day's and night's observations. It is from these field notes, from the records of interviews, from collected documents and artifacts, and

from the experience etched in her memory that the process of analysis and the process of writing the ethnography begins.

My fieldwork was conducted at three national laboratories over a period of five years: National Laboratory for High Energy Physics (Ko-Enerugie butsurigaku Kenkyusho, or KEK) at Tsukuba, Japan, Stanford Linear Accelerator (SLAC) near San Francisco, and Fermi National Accelerator Laboratory (Fermilab) near Chicago. I also visited other laboratories, including CERN in Geneva and DESY near Hamburg, and several university physics departments. My inquiry began several years before my formal commitment to it as an anthropological study. In 1972, while I was a graduate student in intellectual history, I took a part-time job in SLAC's Public Information Office. Along with three or four other graduate students, I explained the activities of the lab to visitors, who ranged from junior high school to college students, from the general public to special interest groups like safety experts, electrical engineers, and chemists, from new employees to visiting dignitaries. There was no training for this job: we were simply told to start asking questions, to find the right people to talk to. After about a month of learning how to learn, I began work. Over the next three years, I came to know a lot about the lab, and from seminars and conferences in the auditorium where I ran the sound system I learned more about physics. I also read textbooks and autobiographies, as well as the history and philosophy and sociology of physics. Increasingly, I took every opportunity to talk with people informally about the lab, and I began to take my own curiosity more seriously in the context of my own discipline of intellectual history.

My graduate research topic had been a study of social and technological change in the French Protestant textile industry in the early nineteenth century. Gradually it became clear that I wanted to change it: I planned to write a history of SLAC. As word of this spread around the lab, people who already knew me would stop me in hallways to tell me "important information" for my research; some began to show me files of memos, notes, and reports they had been saving. I was delighted by the promise of all these documents.

As I learned more stories about the past, I became fascinated by the ways they conflicted with one another. As a graduate student I knew that it was part of the historian's task to find the truth among these conflicting stories: which of them were "correct"? But I soon recognized that this was not where my own interest lay: what I wanted to know was why these conflicting versions had survived into the present, told by people who saw one another daily. As I studied the methods and models of oral history, I discovered that anthropologists were concerned with explaining why communities maintained competing versions of the past in the present. In 1975 I made the change to anthropology. Gregory Bateson, my dissertation advisor at the outset, advised me to go to Japan so that the American labs would become strange to me, to sharpen my observations of them. At SLAC I had met Japanese physicists during conferences and they had urged me to visit; I made the arrangements and went there in the spring of 1976, officially beginning my anthropological fieldwork.

It can be difficult to gain access to a community as a fieldworker. The group can be suspicious of the researcher's motives, skeptical of her intellectual or social or emotional capacities. Even if access is granted, informants may withhold real rapport. Inquiry can be stifled with politeness and formality; a fieldworker's endless questions about what seems obvious, natural, mere common sense, can be tedious or unnerving, since she is dwelling on the unexamined assumptions of the community. Since the people of the laboratory I set out to study already knew me well, the story of my fieldwork experience does not hinge on gaining access, but on the way the shift in my roles, when I reappeared at SLAC as an anthropologist, was managed by the group.

As a Public Information Officer I stood outside social divisions and yet was a familiar part of the lab. I knew and talked with many people in different parts of the lab, because I had to find out about their current work in order to give the tours. I was not associated with any particular group, as I had soon realized that in order to do my job well I needed to subtly dissociate myself from my supervisors in the Public Information Office, which was seen as tainted by undue preoccupation with the outside world. People joked that they did not recognize me without the rickety old bus in which I ferried visitors around the laboratory grounds. In the auditorium during seminars and lectures I was seen hooking up sound systems, fixing broken equipment, showing slides, and listening attentively. Versatility, especially around mechanical equipment, is a recommendation among experimentalists. Before I decided on the shift to anthropology, I already knew that people were willing to talk to me freely. As I described to some of the senior scientists my growing interest in studying the high energy physics community, they offered encouragement; they told me of the issues that concerned them, that they would like to see studied. When the time came, some wrote letters of support for my project, helping me to make the transition to the new discipline.

In a sense, then, when my fieldwork began there were already figures standing ready to act as "key informants." For the anthropologist, key informants are crucial; they are people with whom one can try out tentative interpretations and hypotheses. People who are interested in consciously reflecting on their own culture tend to be atypical within it, whether leaders, geniuses, or simply marginal; they are willing to reflect on the differences between themselves and their fellows, with amusement, sympathy, bitterness, remorse, detachment, or condescension. (Ultimately, the anthropologist should be able to account for why certain kinds of action lead to those roles at the edge of a community.) Key informants are indispensable; but it is essential for the fieldworker not to become too closely identified with any specific informant or faction in the eyes of the community. The danger is that one's work will become an account of a particular viewpoint and of the community's reactions to that viewpoint.

As my fieldwork went on I noticed sharp differences in the responses of senior physicists and of their most junior colleagues. The senior people, once they had scheduled an interview with me, stopped phone calls and closed their office doors; anyone interrupting was asked to return later. They invariably expressed interest in my study, gave thoughtful responses to my questions, and often asked me how others were responding. Each of them seemed to feel responsible for setting me straight and helping me get the right picture; they were clearly disappointed when they felt I failed to understand that I was "wrong." My first task was to suppress my own conditioned impulse to show myself a good student—which they elicited so well. They liked my persistent inquisitiveness, although they did not like me to pursue questions they had decided were unimportant. Some of these men had experience of interviews with journalists and historians and questionnaires from sociologists. Occasionally they would discuss the similarities and differences between my approach and that of others.

By contrast, when I interviewed the youngest physicists in their offices very few of them closed their doors. Passersby would pause and stare, and often interrupt to carry on a conversation with my interviewee, who would break off our exchange without ceremony. The counterpart of this disregard was very valuable; they were much more unguarded in their responses than their seniors; indeed, they often spoke to me as if they were free-associating. It was as if—being both a nonphysicist and a woman—I was not in a position to use special knowledge to gain power. Once I asked what kind of information was generally considered worth keeping secret. "Well," my informant answered, "one thing we never tell anyone is . . ." It was clearly to my advantage that I was not anyone, but it was sometimes hard to take. I had to dissemble my anger, to resist devaluing myself and my work, and to take note as an ethnographer of the tremendous force of the division in the physicists' cosmology between outsiders, no matter how well-informed, and insiders. In 1987, when I am noticeably older than the junior physicists I interview, they still respond in the same way, in sharp distinction to the courtesy of the senior and midcareer scientists, who are both more secure in their own positions and more alert to the uses of the outside world.

Having done fieldwork in Japan during 1976 and again ten years later, I have found that there, too, prominent senior physicists are much more accessible and reflective about their community than their junior and less established colleagues. But there is a major difference: Japanese scientists in general are very interested in the differences between one national scientific community and another, not only in the organization and funding of research, but also in culture. Japanese physicists are quite conscious of belonging to an emerging scientific community, still building the so-called infrastructure of basic research: sound scientific education and modern laboratory equipment at all levels of the school system, excellent research laboratories at universities, state-of-the-art research equipment for basic researchers, an emerging "critical mass" of first-class scientists, and sustained funding to maintain all this. They know that they need these resources in place if they are to participate as equals in the international high energy physics community, and they are eager to learn how American physicists have gained and protected the social resources that have enabled them to build their own world-class laboratories and staff them with first-

rate scientists. They want to know how Americans make decisions among themselves about the allocation of these resources. They are interested in what parts of the American practice of science are distinctively American, and these are my concerns too. Hence, Japanese curiosity about my observations of the American community has often been the starting point of our conversations.

There are other differences also. At a Japanese laboratory there is no way that a five-foot-eight green-eyed auburn-haired woman can expect to fade into the background; and my activities are almost as strange as my appearance. Until quite recently there has been little public interest in high energy physics in Japan, and very little sociology of science; the idea of being interviewed and studied is new and surprising to Japanese scientists. I am always observed at least as much as observing.

It might seem that an American fieldworker would share the commonsense cultural expectations of American physicists. Traditional anthropologists have sometimes been suspicious of "repatriated" anthropology, arguing that without strangeness the fieldworker cannot identify the cultural assumptions of a community; they believe that shared common sense is transparent and hence invisible.

The premise is surely right, but the cases where it applies are rarer than they may seem. For one thing, repatriated anthropology is just that; one of the main reasons I worked in Japanese labs before returning to SLAC was to acquire strangeness. And it is important to remember that there is plenty of strangeness within the United States; in spite of the image of the melting pot, regional, class, ethnic, religious, and occupational differences give rise to sharply differing experiences of the world. In fact, many features of the Japanese physicists' social interactions—their expectations for men and women, their sense of family, what is said and not said, and their ways of expressing joy and anger, frustration, and relief—seem to me very familiar, evocative of the way I myself was raised by my traditional Southern family.

As I began to construct an account of physicists' culture, certain landmarks emerged. Three in particular persisted as key *symbols* of the culture: the physicists' experiences of time, the artifacts called detectors, and a way of thinking that is sometimes called "realism." I have used these symbols in mapping the field of acceptable strategies for making sense and being successful in the community.

There are, in fact, three intersecting cultures involved here: that of the international physics community, and those of Japan and the United States. In the context of their intersections, I have focused on two activities in particular: the training of novice physicists and the management of changes in the structure of laboratories. Both the training of novices and the renewal of institutions are examples of, and models for, maintaining tradition and achieving change—key problems in cultures dedicated to innovation.

It is during apprenticeship that traditions of research are passed on. Patterns of apprenticeship differ in United States physics and in Japanese physics, in ways connected to the larger national cultures in which they are embedded. Institutional change also, and resistance to it, follows different but related patterns in Japan and the United States. The two particle physics labs on which my study is concentrated, KEK and SLAC, face quite similar institutional challenges. Both continue to be confronted with the problem of balancing the interests of resident research groups—"insiders"—with those of visiting experimentalists, usually university based, known as "users." These interests concern decisions about, access to, and management of experimental facilities at each laboratory. Senior physicists are forced to reevaluate what is the best organizational environment for physics.

Two recurring themes in my account are gender and national culture. In the fifteen years I have been visiting physics labs, the status of women within them has remained unchanged—in spite of major transformations, in North America and Europe, in opportunities for women and attitudes about their roles. In this book, women remain marginal, as they are in the laboratory. The lab is a man's world, and I try to show why that is particularly the case in high energy physics: how the practice of physics is engendered, how laboratory work is masculinized.

Among national physics communities, the Japanese have also been peripheral. But they are moving away from their position on the sidelines, and their presence in this book reflects that. Until recently Japan sent money and its best researchers abroad to bring data home; now it is beginning to bring talented researchers and students from around the world to Japan. Japan may soon rank among the world centers of basic scientific research; and high

energy physicists are at the vanguard of this movement, struggling to maintain momentum. Their efforts to establish KEK, and to bring its equipment to world standards and keep it there, are part of the larger goal of moving Japan from the periphery to the core of the international community. I have tried to understand how their strategies for this campaign are related to Japanese research traditions.

Deeper even than submerged assumptions about gender and national identity are profound and deeply felt tensions about time that I find coiled at the center of this culture. In the course of a career a physicist learns the insignificance of the past, the fear of having too little time in the present, and anxiety about obsolescence in the face of a too rapidly advancing future. But the *content* of high energy physics, its explanations of the interactions of fundamental particles, screen out all consideration of elapsing, ephemeral time. Although the physicists speak of generations of particles, of lifetimes and decay, they represent the natural world as a stylized dance of only a few forces, endlessly repeating a small set of choreographed interactions. They have a passionate dedication to this vision of unchanging order: they are convinced that the deepest truths must be static, independent of human frailty and hubris. Simultaneously, they believe that this grand structure of physical truth can be progressively uncovered, and that this is the highest and most urgent human pursuit. Their everyday anxieties about the terrible loss of time—terrors that are carefully maintained in the culture of physics, as if they were essential driving forces for the good physicists—seem to me a mirror image of the cosmological vision that transcends change and mortality. I came to this view by spending many hours and months around detectors, coming to see them as embodying all their builders' divergent meanings and experiences of time. The detectors in the end are the key informants of this study; physicist and nature meet in the detector, where knowledge and passion are one.

exaggerated pride or self-confidence

TRAWEEK studied 3 diff cultures... US. JAPAN INTERNATIONAL PHYSICS COMMUNITY