

Introduction

In 1859 and 1860, at the tail end of the era historians have called the “Great Reconnaissance” of the West—just before the building of the transcontinental railroad—Captain William F. Raynolds of the US Army Corps of Topographical Engineers led an exploring expedition from the Great Plains to the Wind River Mountains of present-day Wyoming, part of the larger Rocky Mountain region. Despite being guided by legendary mountain man Jim Bridger, the expedition failed in its goal of reaching the fabulous Yellowstone Park area.¹ Yet the enterprise was not entirely a failure for science. In the late spring of 1860, a young Ferdinand V. Hayden, the expedition’s geologist and naturalist, found something novel in those mountains that he would publish in a national scientific journal almost a decade later, in the same year that the transcontinental railroad was completed. “One day,” he recounted, “I noticed a group of singular tracks on the snow which seemed different from any I had ever observed in the West, and they appeared to belong to an enormous species of hare. Descending the western slope about a third of the way from the summit, we saw a number of these animals in the little patches of pine forests, and succeeded in capturing several of them, old and young.” Hayden described this new species, which he named *Lepus bairdii* after Spencer Baird of the US National Museum, not only by its physical traits and habitat but also by its fine taste: “Its meat is very white and tender, affording the most delicate food for the traveller. For tenderness and fineness of fibre, the meat of this hare not only differs from, but surpasses all others of the West.”²

The year 1860 also marks the starting point for this study of the field sciences in the US Great Plains and Rocky Mountains of the American West. Hayden’s scientific work in the Wyoming Rockies illustrates some key features of field science on the cusp of the railroad era. In the origins and funding of the expedition itself, as well as in Hayden’s naming of the species, it suggests the early subordination of regional scientific development to the institutions and scientific centers of the East. But it

also foreshadows the region's environmental distinctiveness, as well as the shaping of knowledge production by a challenging material context. At a more practical level, the case shows how scientists themselves used that environment's resources for their own subsistence in the field. Finally, it symbolizes the era of natural history before more rigid disciplinary divisions, when multipurpose expeditions identified new plant and animal species, minerals, geological formations, and native peoples to be reported in general-interest publications such as *American Naturalist* that appealed far beyond narrow specialists.

Now shift forward sixty years to 1920. In that year Francis Ramaley, a biology professor at a regional institution, the University of Colorado, published a paper in a disciplinary journal, the research for which involved ecological field work at a series of subalpine lakes, all in the vicinity of the university's "Mountain Laboratory," a biological field station at the small village of Tolland, located in a high mountain valley within the towering Front Range of the Rocky Mountains and conveniently accessible by railroad. "The area in which lake-shore vegetation has been studied," wrote Ramaley, "is a strip about 5 miles wide, along the eastern slope of the Rocky Mountain National Park south to Parry Peak, a distance of 24 miles."³ In his paper, Ramaley described the formations of vegetation in the technical language of the emerging discipline of ecology. Moreover, his botanical field work at the Mountain Lab involved the prolific use of disciplinary innovations such as the quadrat, pioneered by Frederic Clements and Roscoe Pound at a neighboring state institution, the University of Nebraska, in the 1890s.⁴ After more than ten years of field work in that high mountain valley, Ramaley had produced scientific knowledge using intensified and systematic methods at a site that extended the reach of one of the interior West's own institutions into its fabulous mountain landscapes.

The contrasting example of Ramaley at Tolland reveals a geography of knowledge production quite different from Hayden's discoveries on the Reynolds expedition. By the 1910s, regional institutions—state universities, agricultural experiment stations, civic museums, geological surveys, and the like—had gained a substantial foothold. They were joined in the field by powerful institutions from outside the Great Plains and Rocky Mountains, most often from the eastern United States, and including major research universities, large urban museums, and sprawling federal science agencies such as the US Geological Survey and the US Department of Agriculture. Moreover, the practices used had grown more intensive, rigorous, and systematic, with field scientists adopting

the cosmopolitan standards set in distant scientific centers and applying them in the distinctive environmental context of the West. Finally, most practice had shifted from multipurpose expeditions studying the whole gamut of natural phenomena of the region—its rocks, fossils, plants, animals, weather, artifacts, and people—to research now almost always divided by disciplinary boundary lines, connecting the region’s environmental resources to specialized sciences. In one sense, this book seeks to show how a region’s scientific practice in the field developed from the mid-nineteenth century to the early twentieth century. Yet while change over time in field practices is a significant part of this narrative, it is also intended to reconstruct a world we have lost—a world of field science that existed during the era when railroads dominated long-distance transport. “Field life” during the railroad era was produced through intertwined environmental and technological systems, the organization of work, and a geographical division of labor. But it is the analysis of science in regions and the historical articulation of its diverse modes of practice that bind and define its subject matter.

REGIONS OF SCIENCE, MODES OF PRACTICE

Why, one may ask, should we study the history of science in regions at all? The region has been a rare unit of analysis in the history of science; national or disciplinary traditions have received far more attention. While many excellent histories have been written about national or disciplinary science, this book is grounded in the belief that the region can be a useful scale at which to examine the environmental and working conditions of science, as well as a middle level of analysis between the local and global that does not presume the nation-state to be the only important intermediate unit.⁵ One of my principal aims is to more systematically develop a regional approach to the history of science, further sharpening the analytical tools necessary to make it a compelling scale for research. Moreover, by placing greater emphasis on the natural environment and the work organization of knowledge production than some previous regional accounts in the history of science, I seek to strongly emphasize place and practice.

Regions exist in a global context, of course. Scholarly approaches to capitalism can be instructive by analogy in expressing how global processes interact with place. “Capitalism,” notes geographer David Harvey, “does not develop on a flat plain surface endowed with ubiquitous raw materials and homogeneous labour supply with equal transport facility in all directions. It is inserted, grows and spreads within a richly varie-

gated geographical environment which encompasses great diversity in the munificence of nature and in labour productivity.”⁶ The same could be said for the geographical development of science.⁷ By the 1860s the American West was already beginning to be opened to the production of scientific knowledge. Expeditions led by the US Army, which included some naturalists, had begun to circulate knowledge of this region more widely by the mid-nineteenth century. Topographical engineers, as well as the scientific assistants they sometimes took along with them, provided valuable observations in their reports and collected specimens from a region that had hitherto been largely outside the domain of cosmopolitan science. In the late 1860s the building of the transcontinental telegraph and railroad opened a new frontier for the expansion of scientific practice, beginning the process of converting nature into knowledge that could travel to metropolitan centers.

By conceiving of the expansion of science regionally, we can also help address another problem that has vexed the history of science, and science and technology studies more generally, for the past several decades: the tension between the contextualized, empirical richness of the local case study and the explanatory, weighty allure of the “big picture” global grand narrative.⁸ In examining scientific practices that extended over many local places within larger regions, we can generalize beyond the local while still preserving a fidelity to the texture and specificity of the past. One way that historians have negotiated this tension is to focus on the nation-state, an emphasis that has generated significant insights, sometimes even through the explicit comparison of “national styles” in science.⁹ By focusing on regions, however, the roles of environmental context and the closely connected technologies and practices deployed by field scientists become more salient. At the same time, despite focusing on environmental regions, I have deliberately confined this study within the borders of the United States, not because tracing the story into Canada—where both the Rocky Mountains and Great Plains extend—is any less worthy, but because I prefer to sidestep making cross-national comparisons the central focus of my analysis, which would be a significant, and probably irresistible, temptation. Transnational histories of science of environmental regions are likewise well worth doing, but I have chosen to resist making *Field Life* a book about transnationalism, which could easily overshadow other important themes. In focusing on environmental regions, even within one nation-state, this book operates at the middle range, between the general and the particular. By doing so, it seeks to avoid having to “trade off abstraction and generalization

with the demands of remaining faithful to the complexity of [our] empirical material.”¹⁰

Just as a regional frame of analysis permits an analytical balance between the local and the universal, and between abstraction and empiricism, so too does it help locate a middle ground between what people were physically doing on the ground and their cognitive goals. One way to accomplish this is to focus on midlevel patterns in the practice of science—in other words, what I shall refer to as *modes of field practice*. Each differing mode indicates the organization of both people and techniques and thus is simultaneously social and material, and at once practical and methodological.¹¹ By defining modes of knowledge production in the field as they emerged historically and regionally, this history articulates a taxonomy of scientific practice that is intentionally close to the ground. While most histories of scientific practice have started with specific disciplines and ideas, this book starts from the practices themselves, spanning diverse disciplines and theoretical perspectives. In particular, it focuses on the four most prevalent new modes of field science that were emerging during the railroad era: lay networks, surveys, quarries, and stations, each of which will be treated in a separate chapter.¹² As an analytical strategy, dividing “field practice” into four distinct modes has the advantage of highlighting the diversity of ways that scientific work was accomplished in the field, while at the same time making a serious effort to generalize and seek larger patterns beyond local case studies.

By putting practice at the center of the analysis, this book approaches science as a form of *work*. Scientific work is partly mental, of course, but it also involves moving through the environment, making observations, taking specimens, and sometimes transforming the earth itself. And like the history of miners, farmers, or factory workers, the work history of field scientists is also a form of social history. A central phenomenon of social history has been the emergence of status levels, some of which have been called classes. The closest analogy to a class division in the production of knowledge is probably the line separating those who publish or otherwise culturally display it from those who participate in making knowledge—as field assistants or local collaborators—without gaining such rewards. Similar to class-based exploitation under capitalism, scientific appropriation across the class lines of knowledge work may or may not be regarded as such by participants.

But just what is scientific “field work”? Here I adopt an operational definition that emphasizes its distinction from laboratory work.¹³ While

work in the laboratory is intended (if not in actuality) to take place in a controlled, placeless setting—“simplified and standardized, stripped of all context and environmental variations,” laboratories “are places apart from the world,” writes historian Robert E. Kohler—field work is always tied in some way to the specific geographical place in which it is done.¹⁴ In other words, the environmental and locational features of place are explicitly part of reports of the knowledge produced, rather than remaining unmentioned. Often field work has involved the publishing researcher actually traveling into the field—for example, on an organized expedition—but some field work has relied on surrogates in the field who communicate with and send specimens to a scientist located elsewhere. Either way, however, field work is more or less place bound.

Historical work on scientific practice is, like all human practice, constrained by the availability of documentary evidence about everyday activities and interactions. Laboratory work, for its part, produces very little permanent written record of the face-to-face conversations among those who participate in it.¹⁵ Archival records of correspondence in field science, on the other hand, provide access to communication between field agents and their metropolitan supervisors, which can reveal abundant, rich detail about scientific practice. Moreover, key insights about field work are often included in reports and publications, as well as more fully in programmatic statements and purposely written reflections. In other words, a wide range of primary source materials is available to reveal the texture and development of scientific field work. In this book, then, my task is to analyze the historical emergence of these four material modes of field practice during the railroad era. The exposition will combine a synchronic, historically ethnographic approach, seeking to reanimate the social and material world of scientific field practice, with some attention within each chapter to diachronic transformations within each mode, between the 1860s and 1910s. Finally, each chapter will feature two distinct case studies to propel the narrative forward and provide flesh-and-blood texture to the analysis. These case studies have been chosen to represent some of the diversity of subject matter within each mode of practice, and their presence, along with many other comparative examples, is intended to prevent the conflation of each mode of practice with any particular example.

AN ENVIROTECHNICAL INFRASTRUCTURE

The title of this book indicates that it takes place during “the railroad era.” In part, this is simply a convenient shorthand for a period that

could also be labeled as the Gilded Age and Progressive Era, or the period from the Civil War to World War I. But this technological label also conveys something important about the American West from the late 1860s to around 1920: to understand the practice of field work requires diligent attention to the infrastructure of transport and communication. The railroad not only provided for rapid movement into and out of the field, as well as within it; the railroad often defined the very point of entry into the field.¹⁶

Yet, as Richard White has observed, while the railroads “made the far near . . . they did so unevenly and chaotically.”¹⁷ Despite the dominance of railroads in the long-distance transport infrastructure, for more localized movement it was still in many respects a “landscape for horses,” in the words of historian Ann Norton Greene, “with living machines and steam machines each occupying a unique niche in nineteenth-century industrialization.”¹⁸ The botanist A. Isabel Mulford expressed this transport bifurcation succinctly in 1894, reporting that for summer travels in the Rocky Mountains of Idaho she had “traveled by railroad to the principal points, and by stage, carriage, or horseback to the more distant ones.” (In common with most other field scientists, she also “walked a good deal.”)¹⁹ The railroads undergirded a new travel geography of the West that was as critical to the practice of science as to any other activity.

In communications, the most important technological infrastructure was provided by the US Post Office, which by then generally followed the railroad, though with a significant supporting network of horse-drawn mail stagecoach lines and postriders to reach outposts beyond the rail. Postal historian Richard R. John has convincingly argued for the early bureaucratic development of the post office prior to the 1830s—a “communications revolution” before the advent of rail or steam. In his view, “it was neither the railroad nor the telegraph nor even the commercialization of fossil fuel, but rather the stagecoach and the postrider that liberated Americans from their crippling dependence on the vagaries of geography, wind, and water.” Moreover, steamboats of this era played a very limited role, even where there were pliable waters, given their unreliable schedules, in part due to the perpetually “changing river conditions.”²⁰ At the same time, historian David Henkin has aptly described the period from 1840 to 1860, too, as a “revolutionary era . . . when a critical mass of America began reorganizing their perceptions of time, space, and community around the existence of the post,” and he highlights the “substantial reductions in the cost of sending a letter” in 1845 and 1851. The number of items posted during this time accordingly ex-

ploded “from about 27 million to about 161 million,” which meant a tripling of the per capita rate, even when adjusted for population growth.²¹ Moreover, while the increasing movement of mail by railroad, which began in 1831, may not have changed all aspects of postal practice at first, by the 1860s the advent of “railway mail,” based on “continuous sorting on specially outfitted railroad cars,” signaled further transformation.²²

Supplementing the postal network was the expanding telegraph, which provided a more rapid (and expensive) means for communication into the field.²³ As Elliott West has noted, “the West was acquired, conquered, and largely consolidated into the nation coincident with the greatest breakthrough in the history of human communication . . . the telegraph.”²⁴ While we must be cautious not to overdramatize the role of the telegraph—after all, most scientific messages, forms, circulars, and material specimens still traveled through the mail—it is nonetheless significant that a form of nearly instantaneous communication became available during the railroad era at some locations in the Plains and Rockies, beginning with the first transcontinental telegraph route, which opened in late 1861. The telegraph could be a powerful tool or instrument of science, just as much as for any other activity, when time was of the essence.

There has been much written more generally on the roles of tools, technologies, and instruments in the history of science. In considering the field sciences especially, the key “instruments” need not be solely conventional artifacts that might be displayed in science museums or hidden away in the back rooms of scientific institutions, although these were obviously important. Tools, technologies, and instruments may always have some material component, of course, but these might range quite broadly, even in laboratory and theoretical science, as other historians of science have shown—for example, legal technologies reflected in the demarcation and enforcement of property boundaries, or the example of paper and pencil techniques in mathematical physics or Berzelian chemical formulae as “paper tools.”²⁵ Tools in the field deriving from the natural history tradition, and often carried over into the field sciences, also included various “written categorization procedures” such as lists and reports and “material collection techniques” such as boxes and capsules.²⁶ Other technologies and instruments in the field sciences that developed during the railroad era were quite distinct, as we shall see. Often these were integrated into large technological systems link-

ing everything from a transcontinental railroad line to a single paper form delivered by railway to be held in one person's hand.

Moreover, shifts in infrastructure need not be related merely to technological hardware, such as railway or telegraph lines. In the case of the post office, for example, simple changes in policy or fare structure—what we might call the “software”—have had significant implications for the practice of science. An instructive example of such a change is noted by Martin Rudwick in his study of early nineteenth-century gentlemanly geology in Britain. He observes that these “gentlemen of science” inhabited a social world “in which spontaneous and fluent letterwriting was a routine accomplishment.” Yet at first the high cost of postage beyond London—a cost paid by the recipient, not the sender—gave correspondents about geological matters a strong incentive, even a social “obligation,” to provide “good value for [recipients’] money, to fill the sheet with interesting and worthwhile material.” While such reliable and rapid communication was in fact made possible by a relatively well-developed postal infrastructure, compared to earlier times and other places, it was still a cultural-technical system requiring considerable economy and care. “All this changed,” Rudwick points out, “with the introduction of the cheap prepaid penny post throughout Britain in 1840; as soon as a low and uniform cost was incurred by the sender, the informative quality of long-distance scientific letterwriting fell off abruptly.”²⁷

While the transport and communication infrastructure receive top billing in *Field Life*, they do not constitute the entire material basis of scientific field work. Some other technologies were crucial to particular modes, such as the paper reporting form in lay networks, camping gear for surveys, property control (legal technologies) for quarries, and laboratory buildings for stations. Other technical means and tools have been important to particular sciences, such as farm equipment for agricultural science, thermometers and rain gauges for meteorology, and indeed the whole gamut of specialized measuring tools for each field discipline. While these more specific technologies underlying more specialized systems of production will not be my primary focus, they will come up from time to time, and they are certainly important in case studies of the particular systems of knowledge production that flourished within each discipline.

Thinking about the tools of science in the field purely as technology is not enough, however, even when it incorporates both hardware and software. The actual use of technologies of transport, communica-

tion, and everything else that we might consider important was deeply intertwined with the particular natural environments in which those technologies were deployed. For thinking about this relationship, a conceptual framework from two fields closely related to the history of science—environmental history and the history of technology, or, more precisely, from the intersection between them—can be useful: the “envirotechnical system.” One leading envirotech historian, Sara Pritchard, has defined *envirotechnical systems* (deliberately pluralized) as “historically and culturally specific configurations of intertwined ‘ecological’ and ‘technological’ systems, which may be composed of artifacts, practices, people, institutions, and ecologies.”²⁸ Note that “enviro” and “tech” are not merged into complete indistinguishability: they are “intertwined.” As another influential envirotech historian, Tim LeCain, has argued, “the goal of envirotechnical analysis . . . is to demonstrate how this system that is both human and nonhuman, artificial and natural, technological and ecological, does actually exist even if our culturally constructed ideas and words often keep us from recognizing it.”²⁹

Thus, without denying that technology and the environment are to some extent distinct and analytically separable elements, historians of science can immediately recognize, in agreement with the envirotech historians, that the natural environment and human technology were so closely interconnected in scientific practice—especially but not only in the field—that it makes sense to view them as elements of the same system. To take just one example, the railroads of the interior American West constituted a powerful, even dominant, technological infrastructure for the practice of field work during this period, but they were always used and understood in relation to the environmental landscapes in which they were located, built, maintained, used, and embedded. Railroads could, of course, be built in a wide variety of natural places (though not everywhere), and environmental regions such as the Great Plains have been inhabited by humans with a variety of technological systems, from horses and tepees to gasoline-powered vehicles and center-pivot irrigation equipment—both before and since the railroad era. Yet it is only by attending to the envirotechnical system as a relational whole that the conditions of practice can be fully understood, including the conditions of scientific practice.

MICRODIVISION OF LABOR: THE ORGANIZATION OF WORK

Complementing my emphasis on the envirotechnical basis of scientific field work—and closely overlapping with my attention to the relation-

ship between the environment and scientific bodies in the field—another significant focus in this book is on divisions of labor, including both those within the day-to-day practice of scientific field work at ground level and those of the larger geographical relations between regions of cosmopolitan science. For the first of these, while the history of science has traditionally focused on lead research scientists, this book aims to uncover the hierarchical division of labor that made up rigorous scientific field work, from leaders to subordinate contributors, and crossing the conventional boundaries that have divided publishing scientists from their technicians, field hands, and other assistants. Another important aspect of field work, which has distinguished it from the laboratory with its access restrictions, has been the involvement of lay people in the process of knowledge production, which I also examine in this book—for example, the reliance of survey parties and quarry prospectors on their knowledge of where to find promising natural phenomena and objects.

One conventional way of characterizing the division of labor in science—if truth be told, more in rhetoric than in actual practice—is to distinguish between head and hand work.³⁰ As Mary Louise Pratt argues, “natural history set in motion a secular, global labor that, among other things, made contact zones a site of intellectual as well as manual labor, and installed there the distinction between the two.”³¹ Some of the field work described and analyzed in this book descended directly from the natural history collecting of the eighteenth and early nineteenth centuries that Pratt analyzes, and such a division of labor carried over to the modes of field production that I describe. Indeed, within each of these modes, as the several chapters of this book will show, field researchers employed or voluntarily organized in their work collaborators who engaged in relatively more manual labor than they themselves did.

Nevertheless, this conventional formulation fails to tell the whole story: practical realities in the organization of field work often meant that researchers would be engaging their hands, while those whose status they attempted to subordinate sometimes contributed vital mental skills and experiential knowledge. Other historians have recognized that the grubby, hands-on character of field work extended across the lines of social class, so that Britain’s gentlemen geologists, for instance, loved to recount examples in which their gentlemanly status went unrecognized in the field.³² Even when assisted by a myriad of guides, cooks, packers, hired workers, and other proletarians, field work routinely involved an engagement with the natural world that belied any simple exclusion of manual tasks from the activities of naturalists and other sci-

entific researchers. Likewise, the local or experiential knowledge of lay people and hired laborers in the field, along with the mental habits that systematic field work often required of them, was a crucial component of its success. The chapters that follow will offer numerous examples of the mental aspects of subordinate labor in the field.

The organization of scientific work, again like other types of work, has been accomplished through hierarchical structures of authority. These go well beyond the simple division between research authors and their assistants. Just as managers and laborers in industry are further stratified, so too are scientists divided by myriad status distinctions. For example, museum directors have authority over department curators, who in turn guide field staff, who in turn direct assistants in the field. Different status levels pervade the “work history” of science. Besides status distinctions, field scientists are also divided socially by career status, institutional affiliation, sex, race, and geography. Overlapping significantly with all these status distinctions is the development of careers based on field science work. Some participants in knowledge making in the field established scientific careers, while others were involved only temporarily (e.g., students) or as an unpaid adjunct to another career (e.g., doctors, clergymen, and businessmen). During the period of this study, career makers gradually came to dominate, although some non-career participants remained visible into the twentieth century.

Historians have learned to treat the issue of scientific identity with some historical sensitivity. In fact, I have focused deliberately on the emergence of scientific careers in part to avoid relying on the more conventional category of professionalization. It is not so much that I want to continue flogging the category of professional, so much under attack during the past several decades—and, to be sure, I do sometimes use the terminology of *amateur*, which implies its professional antonym—but it is important to avoid teleological assumptions about what particular identities were coveted by the makers of natural knowledge in the nineteenth century. In this case, the most questionable assumption is probably that scientists in the nineteenth century sought to become professionals in the twentieth-century mold. As numerous historians have demonstrated, knowledge makers did not necessarily aim to become professionals, or even scientists.³³ Rather, the task here is to display the historical emergence of scientific careers or life paths through field work based on changing practices and identities.

An important related trend was the displacement of independent field scientists by those with institutional affiliations at museums, uni-

versities, and government agencies. Nearly all field scientists at all status levels during the railroad era were men, although there were a handful of women, many of them conspicuous in part due to their sex. Nearly all of them were white, with the crucial exception of American Indian field assistants, especially in anthropology. Low-status participants such as cooks and packers were sometimes nonwhite, although the rarity of extended comment on such workers by scientific writers makes it difficult to say with any precision how many. Finally—and with great significance for the scientific development of the region—field scientists could be from inside or outside the West, and by the 1890s the early overwhelming dominance of Easterners was challenged by researchers from within the region.

Despite these consequential differences among the field scientists in the American West, they also had a great deal in common. All were involved in the production of knowledge out of Western nature, by converting their observations and material specimens into symbolic expressions—oral or written—that could circulate in a wider scientific community beyond the locality where they began. As such, these migrant knowledge makers were not fundamentally different from the many other new producer groups who came to the West in the late nineteenth century, such as miners, farmers, and ranchers. Like them, scientists subsisted on the natural resources offered by the land, attempting to earn a living through their own sweat and toil. Some carved out niches as old-middle-class-style independent producers, in the specimen business, for instance.³⁴ But as institutions such as universities and government agencies came to dominate employment of scientists, most would be better described as members of the new middle class of college-educated white-collar professionals and managers.³⁵

Although the producer groups provide the dominant metaphor underlying this project, we can also recognize a second comparison group, the tourists or vacationers. Scientific field work in the West shared a great deal with the emerging middle-class style of vacationing in the late nineteenth century, which Cindy Sondik Aron calls “working at play.” Middle-class vacationers sought to avoid the old upper-class style of vacationing, which they saw as luxurious, extravagant, and fraught with idleness. Instead, they sought self-improvement.³⁶ Participating in a scientific field expedition might well be called the extreme version of a middle-class, working-at-play vacation, in which tourists saw a lot of interesting sites—especially at popular field work locations like Yellowstone and the Colorado Rockies—but were doing scientific work too.

While spatial hierarchy in knowledge making has endured over time, the specific types of labor have shifted. As Henrika Kuklick has pointed out in her study of British social anthropologists, the nineteenth-century “division of disciplinary labor” between a recognized elite of armchair scholars and their collectors in the field reflected the community’s constitution, giving “distinct roles for foot soldiers who collected data and the scholars who interpreted them.” Yet such a division “dissolved” with the expansion of the university system, which opened up new career opportunities for researchers and made field work an “essential” activity for gaining prestige in anthropology. By the First World War—near the endpoint of this study—personal involvement in systematic field work was crucial for all different kinds of field sciences.³⁷ Scientists in the center still controlled the production and circulation of knowledge, but they did so increasingly by going into the field themselves.

Even while largely eschewing the amateur-professional distinction, as noted above, this book foregrounds another distinction with which it is often conflated: the lay-expert divide. While experts and professionals are often thought to be the same people today, the leading experts in a given field of study in the nineteenth century might or might not be professionals. Arguably, the professional identity was increasing in significance, especially by the turn of the twentieth century, but we need not place a great deal of weight on the category of the professional in order to recognize the analytical importance of the growing divide between lay people and experts. Of course, as Paul Lucier has reminded us, the term *expert* itself underwent a significant set of shifts during the nineteenth century.³⁸ Nonetheless, while it may be somewhat difficult to pin down the “expert,” the social role of the “lay person” in the production of knowledge, as an analytical category, is in my view unavoidable in examining field work during this period. The involvement of lay people—usually, locals who lived and worked near the field site—in knowledge production was simply too important, and sometimes contested, for the dynamics of such participation to be elided because of a fear of using anachronistic terminology.

Indeed, the lay person is a constitutive social category for understanding one of the period’s most important modes of knowledge production—what I have called the lay network—and lay people were nearly always important in other modes as well, acting as guides to survey parties, as well as hired laborers for many field stations and quarries. The lay person is a crucial social category for understanding the status hierarchies and patterns of work organization discussed in this book.

While I only rarely use the terminology of *expert*, leaving such a role implicit, it will thus prove to be crucial to talk a lot in this book about lay people. In some cases, for precision, it will be preferable to refer to amateurs instead, or to amateur and lay together, when the people involved possessed a clear avocational interest in the subject matter, or if some of them did, in order to convey the social and epistemic relations as closely as possible.

EXPERIENTIAL AND COSMOPOLITAN KNOWLEDGE: THE EPISTEMIC RIFT

To situate field practice in its context, it will also be essential to take seriously the experiential knowledge possessed by lay people and amateurs in the field, which often complemented what we might call the “cosmopolitan” knowledge of field scientists who encountered them, collaborated with them, and occasionally conflicted with them in the practice of field science.³⁹ The distinction between experiential and cosmopolitan knowledge, and what we might call the “epistemic rift” between them, is one of the most important conceptual frameworks for understanding the practice of field science.⁴⁰ All modes of field science have attempted to bridge the rift between the knowledge of experience based on living, working, or playing in a particular place and the cosmopolitan knowledge that travels well between places and enables the facts from any one place to be situated in larger taxonomies, categories, and comparisons. For instance, while a farmer might know where and when to find various animals that live nearby, along with their locally peculiar habits, a visiting scientist with access to specialized knowledge and distant collections would be better equipped to classify and map those animals in terms of cosmopolitan scientific knowledge. We might furthermore think of this distinction as directly parallel to that between use value and exchange value in the material economy: the local use value of experiential knowledge within everyday life and work, likewise, contrasts with the long-distance exchange value of cosmopolitan knowledge within science.

The distinction between experiential and cosmopolitan knowledge, as well as the possibility of bridging the epistemic rift between them, not only is important for the historian’s analytical toolkit but was also sometimes present in discourse during the railroad era itself. During the late 1860s, for example, as the first transcontinental railroad was in progress, a new periodical called the *American Naturalist* began publication, with a stated aim to go beyond an audience of scientists and expert naturalists and become “equally welcome to the Farmer, Gardener, and Artisan,” by

emphasizing “the practical benefits resulting from the study of nature.” However, its editors pledged to offer readers content “drawn from original sources,” including “the most recent discoveries of general interest from the English, German, and French reviews and journals,—for science is cosmopolitan.”⁴¹ A quarter of a century later, near the midpoint of the railroad era, the naturalist C. Hart Merriam drew further attention to the distinction between different types of knowledge, writing in the pages of *Science* that he “would rather have the farmer’s boy who knows the plants and animals of his own home than the highest graduate in biology of our leading university.” Yet he also recognized a “chief disadvantage in manufacturing naturalists in this way”: in Merriam’s view, “they lack the education possessed by college-bred men,” which would be “sorely felt” over time. “To be well equipped for this work,” he concluded, “a naturalist or biologist needs a college education,” thus admitting the importance of cosmopolitan knowledge alongside the knowledge of experience possessed by the farm boy.⁴²

The adjective *experiential* is only one of many options for making this key distinction, and the term overlaps considerably with other commonly used terms such as *local*, *vernacular*, *indigenous*, or *folk*, which also identify this type of knowledge to varying degrees.⁴³ The term *local* is perhaps the easiest to use, and I will use it often, but we must be careful about the hierarchical assumptions built into the antinomy of local and global, which often implies that the one is geographically subsumed under the other. (In such a formulation, local knowledge is sometimes thought to have validity only insofar as it can be aggregated into global knowledge.) However, I do use the term *local* frequently in this book because of its simplicity and aptness for many specific examples, and occasionally I also employ other alternatives such as *folk*, *indigenous*, or *vernacular* when the connotation seems especially appropriate.⁴⁴

More broadly, as shall become clear at various points in this book, people of lower status levels—field collectors, amateurs, local collaborators, and the like—did possess some leverage that softened the more exclusionary, professional identity then emerging in the cosmopolitan scientific community, especially among laboratory researchers.⁴⁵ Perhaps one reason why so many researchers took to the field at the end of the nineteenth century—when both infrastructure and academic positions began to enable them to do so—was to exert a greater degree of control over the production of knowledge in the field, which had hitherto been under the partial authority of peripheral collaborators. Still, except perhaps for the example of intensive ethnographic field work,

most scientists did not live permanently in the field and had to rely on people who did.⁴⁶

MACRODIVISION OF LABOR: CORES AND PERIPHERIES

As this discussion of long-distance collaboration indicates, a patterned division of labor existed not just within the local practice of regional field science but also between larger geographical regions. In the end, whether in any part of the Americas, Africa, Asia, Australia, or even peripheral zones of Europe itself, those living and working in regions connected to the cosmopolitan science of Europe and eastern North America faced the challenge (and opportunity) presented by the geographical division of scientific labor between core and periphery. One of the underlying aims of this book is to explore the historical unfolding of that system of spatial hierarchy. A geographical division of labor has been crucial to the practice of science, just as a similar division of material labor has underpinned the spread of capitalist production and trade. Beyond the organization of the field work itself, then, this book also examines the geographical division of labor that characterizes the larger world of cosmopolitan science. As a new periphery for scientific practice in the nineteenth century, the Great Plains and Rocky Mountain regions were at first dominated, in the production of knowledge about them, almost completely by outsiders. Over time, regional institutions and researchers struggled to redirect the epistemic profits of research to the region itself, with partial success, so that by the mid-twentieth century it had arguably gained at least semiperipheral status. By discussing the division of labor both within research enterprises and between geographical regions, this book aims to highlight the role of hierarchies of labor and work in the history of science.

Such hierarchies were evident to contemporary observers. On the occasion of the American Association for the Advancement of Science (AAAS) meeting in Denver in 1901—its first meeting in the region—an editorial report that appeared in *Science* advanced a suggestive historical comparison. “During the first hundred years,” it noted, “the nation was in scientific matters somewhat in the relation of a colony to Europe. Our students went abroad for study; we depended on Europe for our journals and books, and did not contribute our share to the work of the world.” Yet, in the last quarter century, “great progress” in American science led to a new set of relations: “The central and western states have been in the colonial relation to the Atlantic seaboard that it had previously held to Europe. Students have come to eastern universities, and

the scientific men for the central and western states have been drawn from the east.⁴⁷

East-West hierarchical relations in American science—especially as manifested in interactions between the Western states and the federal headquarters in Washington, DC, along with such metropolitan centers as New York, Boston, Chicago, and Pittsburgh—were in some respects similar to the relations of European capitals such as London and Paris with their peripheries.⁴⁸ Moreover, many of the dynamics and tensions discussed in this book were felt in other parts of the United States. But there was also an environmental dimension to East-West relations. Regional characteristics like the semiaridity of the interior West made it a different kind of continental periphery than the Midwest, the South, or northern New England. Such environmental differences between East and West—mediated through the environmental and technological infrastructures of transport, communication, supply, and provisioning—reinforced and even strengthened the more general geographical relations of core and periphery.

For the author of the 1901 report in *Science*, however, such a hierarchy was not unchangeable: further mobility could be imagined. The editorial report therefore concluded that “it is perhaps worth noting that the fiftieth meeting of the Association in the first year of the new century should celebrate an epoch in the development of science in America.”⁴⁹ Was the first meeting of the AAAS in the Rocky Mountains and Great Plains region a harbinger of a future liberation from the constraints of scientific colonialism? How would scientific work be organized to accomplish scientific development? In the end, rhetorical enthusiasm for progressive frontier scientific development must not be assumed to describe the actual reality; rather, it requires a clear-eyed confrontation with the historical evidence. It is the task of the rest of this book to supply that evidence and to interpret it in its larger context.

At its most basic level, this book is a historical reconstruction of the practice of scientific field work in the Plains and Rockies during the railroad era, oriented around a few simple questions: What was it like to do science in the field? How were natural environments and human tools and technologies brought together? Who was involved in field science and how was their work organized? How did they interact with others outside the West? This book aims to give readers a ground-level perspective on scientific practice in the field during an especially intriguing time and place in the history of science—the American West during the

late nineteenth and early twentieth centuries. The title, as many readers may have surmised by now, builds on the classic science studies book by Bruno Latour and Steve Woolgar, *Laboratory Life* (1981). Like *Laboratory Life*, *Field Life* is meant to provide a window onto the everyday practices involved in the production of scientific facts. There are a few key differences, however. First, I emphasize the diversity of ways of organizing the production of knowledge in the field, rather than identifying “field life” with any single mode. Second, my approach is consciously transdisciplinary in its subject matter, bringing together evidence from a wide range of field science disciplines, including paleontology, botany, zoology, ecology, archaeology, meteorology, geology, and agricultural science. Third, I have deliberately situated the field sciences both historically and geographically, so that the effects of period and place are a crucial part of the story. Finally, as suggested above, I aim to point out analogies and connections between science and the developing material economy even more emphatically, in order to suggest how field science has been in dynamic interaction with the larger capitalist political economy.

After an opening chapter on the making of the field as a place for doing science, which also considers the field in relation to the laboratory and the natural history tradition, the four remaining chapters are each oriented around thick description and analysis of a particular mode of field practice in the Great Plains and Rocky Mountains. Beginning with lay networks, followed by surveys, quarries, and stations, I consider the practice of science during the railroad era under each of the modes of field work. In each chapter, I consider the natural environment and technological systems, the organization of work, the ground-level division of labor, and the larger East-West relations, aiming to provide a synthesis of science practice across a range of disciplines, from the earth and physical sciences to the human and life sciences. Throughout, I pay close attention to how these configurations of work, environment, and technology bridged—selectively, distinctively, and partially—the epistemic rift between the cosmopolitan knowledge of science and the knowledge of experience. Finally, in the epilogue, I briefly consider the transformation and persistence of the field sciences after the end of the railroad era. By the end, I hope to demonstrate that “science in the field” in this period was a diverse, polyglot assemblage of practices, but that those practices followed common patterns we can trace.