Institutional Ecology, `Translations' and Boundary Objects: Amateurs and Professionals in Berkeley's Museum of Vertebrate Zoology, 1907-39

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Social Studies of Science 1989 19: 387
DOI: 10.1177/030631289019003001

• *ABSTRACT*

Scientific work is heterogeneous, requiring many different actors and viewpoints. It also requires cooperation. The two create tension between divergent viewpoints and the need for generalizable findings. We present a model of how one group of actors managed this tension. It draws on the work of amateurs, professionals, administrators and others connected to the Museum of Vertebrate Zoology at the University of California, Berkeley, during its early years. Extending the Latour–Callon model of interessement, *two major activities are central for translating between viewpoints: standardization of methods, and the development of 'boundary objects'. Boundary objects are both adaptable to different viewpoints and robust enough to maintain identity across them. We distinguish four types of boundary objects: repositories, ideal types, coincident boundaries and standardized forms.*

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Most scientific work is conducted by extremely diverse groups of actors — researchers from different disciplines, amateurs and professionals, humans and animals, functionaries and visionaries. Simply put, scientific work is heterogeneous. At the same time, science requires cooperation — to create common understandings, to ensure reliability across domains and to gather information which retains its integrity across time, space and local contingencies. This creates a 'central tension' in science between divergent viewpoints and the need for generalizable findings. In this paper we examine the development of a natural history research museum as a case in which both heterogeneity and cooperation are central issues for participants. We develop an analytical framework for interpreting our historical material, one which can be applied to studies similarly focused on scientific work in complex institutional settings.

Social Studies of Science (SAGE, London, Newbury Park and New Delhi), Vol. 19 (1989), 387–420

The plan of the paper is as follows. First we consider the ramifications of the heterogeneity of scientific work and the need for cooperation among participants for the nature of translation among social worlds. We suggest modifications of the *interessement* model of Latour, Callon and Law. We urge a more ecological approach and develop the concept of boundary objects to analyze a case study of a research natural history museum. We discuss the history of the Museum of Vertebrate Zoology at the University of California, Berkeley, and describe conceptions of it by participants from several distinct social worlds, including those of professional scientists, amateur naturalists, patrons, hired hands and administrators. Our discussion is meant to be suggestive rather than conclusive at this stage, outlining an approach to case studies as well as providing a partial analysis of the case at hand. We conclude with further discussion of boundary objects and the allied issue of methods standardization.

The Problem of Common Representation in Diverse Intersecting Social Worlds

Common myths characterize scientific cooperation as deriving from a consensus imposed by nature. But if we examine the actual work organization of scientific enterprises, we find no such consensus. Instead, we find that scientific work neither loses its internal diversity nor is consequently retarded by lack of consensus. Consensus is not necessary for cooperation nor for the successful conduct of work. This fundamental sociological finding holds in science no less than in any other kind of work.[1] However, scientific actors themselves face many problems in trying to ensure integrity of information in the presence of such diversity. One way of describing this process is to say that the actors trying to solve scientific problems come from different social worlds and establish a mutual *modus operandi*.[2] A university administrator in charge of grants and contracts, for example, answers to a different set of audiences and pursues a different set of tasks, than does an amateur field naturalist collecting specimens for a natural history museum.

When the worlds of these actors intersect a difficulty appears. The creation of new scientific knowledge depends on communication as well as on creating new findings. But because these new objects and methods mean different things in different worlds, actors are faced with the task of reconciling these meanings if they wish to cooperate. This reconciliation requires substantial labour on everyone's part. Scientists and other actors contributing to science translate, negotiate, debate, triangulate and simplify in order to work together.

The problem of translation as described by Latour, Callon and Law is central to the kind of reconciliation described in this paper.[3] In order to create scientific authority, entrepreneurs gradually enlist participants (or in Latour's word, 'allies') from a range of locations, re-interpret their concerns to fit their own programmatic goals and then establish themselves as gatekeepers (in Law's terms, as 'obligatory points of passage').[4] This authority may be either substantive or methodological. Latour and Callon have called this process *interessement*, to indicate the translation of the concerns of the non-scientist into those of the scientist.

Yet, a central feature of this situation is that entrepreneurs from more than one social world are trying to conduct such translations simultaneously. It is not just a case of *interessement* from non-scientist to scientist. Unless they use coercion, each translator must maintain the integrity of the interests of the other audiences in order to retain them as allies. Yet this must be done in such a way as to increase the centrality and importance of that entrepreneur's work. The *n*-way nature of the *interessement* (or let us say, the challenge intersecting social worlds pose to the coherence of translations) cannot be understood from a single viewpoint. Rather, it requires an ecological analysis of the sort intended in Hughes' description of the ecology of institutions:

> In some measure an institution chooses its environment. This is one of the functions of the institution as enterprise. Someone inside the institution acts as an entrepreneur... one of the things the enterprising element must do is choose within the possible limits the environment to which the institution will react, that is, in many cases, the sources of its funds, the sources of its clientele (whether they be clients who will buy shoes, education or medicine), and the sources of its personnel of various grades and kinds. This is an ecology of institutions in the original sense of that term.[5]

An advantage of the ecological analysis is that it does not presuppose an epistemological primacy for any one viewpoint; the viewpoint of the amateurs is not inherently better or worse than that of the professionals, for instance. We are persuaded by Latour that the important questions concern the *flow* of objects and concepts through the *network* of participating allies and social worlds. The ecological viewpoint is anti-reductionist in that the unit of analysis is the whole enterprise, not simply the point of view of the university administration or of the professional scientist. It does, however, entail understanding the processes of management across worlds: crafting, diplomacy, the choice of clientele and personnel. Our approach thus differs from the Callon–Latour–Law model of translations and *interessement* in several ways. First, their model can be seen as a kind of 'funnelling' — reframing or mediating the concerns of several actors into a narrower passage point (see Figure 1). The story in this case is *necessarily* told from the point of view of one passage point — usually the manager, entrepreneur, or scientist. The analysis we propose here still contains a managerial bias, in that the stories of the museum director and sponsor are much more fully fleshed out than those of the amateur collectors or other players. But it is a many-to-many mapping, where several obligatory points of passage are negotiated with several kinds of allies, including manager-to-manager types (see Figure 2).

FIGURE 1

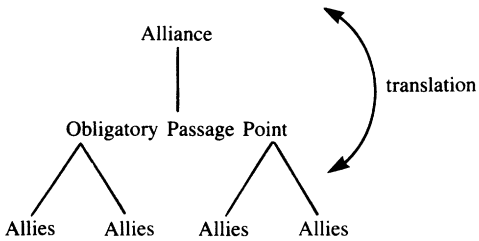
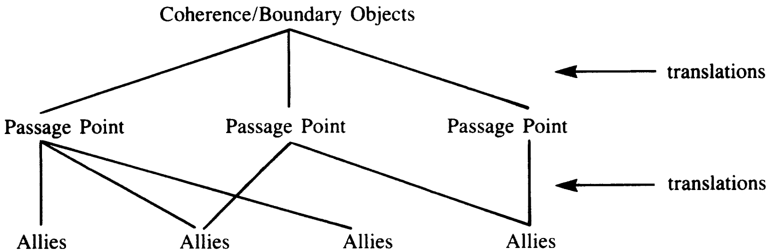


FIGURE 2



The coherence of sets of translations depends on the extent to which entrepreneurial efforts from multiple worlds can coexist, whatever the nature of the processes which produce them. Translation here is indeterminate, in a way analogous to Quine's philosophical dictum about language.[6] That is, there is an indefinite number of ways entrepreneurs from each cooperating social world may make their own work an obligatory point of passage for the whole network of participants. There is, therefore, an indeterminate number of coherent sets of translations. The problem for all the actors in a network, including scientific entrepreneurs, is to (temporarily) reduce their local uncertainty without risking a loss of cooperation from allies. Once the process has established an obligatory point of passage, the job then becomes to defend it against other translations threatening to displace it.

Our interest in problems of coherence and cooperation in science has been shaped, in part, by trying to understand the historical development of a particular type of institution: natural history research museums. Museums of natural history originally arose when private collectors in the 17th century opened their cabinets of curiosities to public view. The display of wealth, polite learning and emulation of the aristocracy, as well as development of reference collections for physicians and apothecaries, were common motives for making cabinets. Many such cabinets were arranged to display, and evoke wonder at, the variety and plenitude of nature or to represent the universe in microcosm. Such museums, in other words, developed as part of popular culture.[7] In the nineteenth century, many new museums were developed by amateur naturalists, rather than by members of the 'general public', through their participation in societies for the amateur naturalist. These societies filled an important role in the development of the museum-based science to come.[8] The museum we studied, the Museum of Vertebrate Zoology (MVZ) at the University of California, Berkeley, is important as an example of a museum devoted to scientific research from its inception, aided by the alliance of an amateur naturalist/patron and an early West Coast professional scientist. The MVZ did not take on scientific research as an adjunct to public instruction or popular edification as had many eastern museums; if anything, the reverse is true. (A symbol of this tradition of research is the evident pride with which current museum staff draw attention to an advertisement on the front door stating that there are 'NO PUBLIC EXHIBITS'.)

As such, the development of the *research* natural history museum represents an important stage in the professionalization of natural history work, as well as an example of the changing relationship between amateurs and professionals after the professionalization of biology in America had already begun. Unlike many well documented cases of eastern institutions which looked to the European scientific community as a model and for legitimation, western biologists had to struggle to gain credibility in the eyes of the already professionalizing biological community in the eastern US itself. Successful pursuit of the research problems through which the Museum of Vertebrate Zoology's scientists hoped to gain recognition depended on an evolving set of practices instituted to manage the particular sort of work occasioned by the intersection of the professional, amateur, lay and academic worlds.[9] There, several groups of actors — amateurs, professionals, animals, bureaucrats and 'mercenaries' — succeeded in crafting a coherent problem-solving enterprise, surviving multiple translations.

Joseph Grinnell was the first director of the Museum of Vertebrate Zoology. He worked on problems of speciation, migration and the role of the environment in Darwinian evolution. Grinnell's research required the labours of (among others) university administrators, professors, research scientists, curators, amateur collectors, private sponsors and patrons, occasional field hands, government officials and members of scientific clubs.

Some objects of interest to all these social worlds included:

- species and subspecies of mammals and birds
- the terrain of the state of California
- physical factors in California's environment (such as temperature, rainfall and humidity)
- the habitats of collected animal species

Methods Standardization and Boundary Objects

It is normally the case that the objects of scientific inquiry inhabit multiple social worlds, since all science requires intersectional work. Varying degrees of coherence obtain both at different stages of the enterprise and from different points of view in the enterprise. However, one thing is clear. Because of the heterogeneous character of scientific work and its requirement for cooperation, the management of this diversity cannot be achieved via a simple pluralism or a laissez-faire solution. The fact that the objects *originate in*, and continue to inhabit, different worlds reflects the fundamental tension of science: how can findings which incorporate radically different meanings become coherent?

In analyzing our case study, we see two major factors contributing to the success of the museum: *methods standardization* and the development of *boundary objects*.

Grinnell's managerial decisions about the best way to translate the interests of all these disparate worlds not only shaped the character of the institution he built, but also the content of his scientific claims.[10] His elaborate collection and curation guidelines established a management system in which diverse allies could participate concurrently in the heterogeneous work of building a research museum. It was a lasting legacy. Grinnell's methods are looked upon as quaint and overly fastidious by current generations of museum workers,[11] but they are still taught and practised at the Museum of Vertebrate Zoology. (They were also adopted by several other museums around the United States during the first part of this century.[12]) For example, his course handouts for 1913[13] are similar to current field manuals for students in Zoology 107 at Berkeley.[14] There was an intimate connection between the management of scientific work as exemplified by these precise standards of collection, duration and description, and the content of the scientific claims made by Grinnell and others at the museum.

The second important concept used to explain how museum workers managed both diversity and cooperation is that of *boundary objects*. This is an analytic concept of those scientific objects which both inhabit several intersecting social worlds (see the list of examples in the previous section) *and* satisfy the informational requirements of each of them.[15] Boundary objects are objects which are both plastic enough to adapt to local needs and the constraints of the several parties employing them, yet robust enough to maintain a common identity across sites. They are weakly structured in common use, and become strongly structured in individual-site use. These objects may be abstract or concrete.[16] They have different meanings in different social worlds but their structure is common enough to more than one world to make them recognizable, a means of translation. The creation and management of boundary objects is a key process in developing and maintaining coherence across intersecting social worlds.

In the next section, we provide some background to the museum's evolution, then turn to a discussion of both methods standardization and boundary objects.

Grinnell and the Museum of Vertebrate Zoology, 1907–39

The biological sciences in America were undergoing a number of transitions during this period. The educational and cultural functions of natural history were being subsumed under the research goals of scientists. Biological research was increasingly conducted in academic institutions such as universities and specialized research stations rather than in societies formed by amateurs. Professional biologists sought international credibility by distinguishing themselves from amateurs, establishing advanced degrees as credentials, establishing specialized journals for the dissemination of results and by increasingly eschewing the public's eclectic interests in science. For organism-based subdisciplines (for example, ornithology, mammology, herpetology), the central transition was a shift from studies of classification and morphology to studies of process and function. With this change of focus, methods and practices diversified. From mostly observational and comparative approaches, biological methods came to include experimental, manipulative and quantitative techniques and natural history methods were refined so as to focus on increasingly specialized research problems.[17]

At the same time, a number of 'inventorying' efforts of the federal government were coming to fruition in reports of collecting and surveying trips to the west. The Bureau of the Biological Survey, founded in 1905 as an arm of the US Department of Agriculture, for example, made a massive effort to chart the flora and fauna of the states and territories. As the nineteenth century closed, these reports were used by their authors and others to go far beyond mere catalogues of materials. Their data were used, for example, to begin to develop general biogeographic principles of animal and plant distribution, most notably that of C. Hart Merriam, later an important influence on the Museum of Vertebrate Zoology workers.[18]

The participation of ecology in these changes meant both distinguishing itself from its basis in descriptive natural history and adopting new methods. On the one hand, ecologists adopted a set of problems originating in evolutionary theory (adaptation, natural selection), geography (distribution and abundance) and physiology (effects of physical factors such as heat, light, soil and humidity on life-history). On the other, they learned new methods of quantification and analysis and the use of biological indicators.[19] Ecology emerged from the last century as a subdiscipline distinct from systematics, morphology and genetics. Ecologists are concerned with (1) the bases for adaptation, (2) extending physiology to consider the dynamics of interacting groups of organisms and (3) the quantification of the physical (physiographic) environment as it affects the life-histories of organisms. New theoretical work was beginning to emerge which distinguished ecology as well.[20]

Joseph Grinnell (1877–1939) extended his work in natural history to include ecological problems during this period of disciplinary shift. He studied at Stanford University under the tutelage of Charles H. Gilbert and David Starr Jordan.[21] At the turn of the century Stanford naturalists were bringing together problems of habitat and distribution along with evolutionary theory to form an emerging geographical conception of speciation. This merger was to become of central concern to evolutionists and ecologists alike later in this century.[22]

Alexander and Grinnell

The Museum of Vertebrate Zoology was founded at Berkeley in 1908 by Annie Montague Alexander (1867–1950). Alexander was heir to a Hawaiian shipping and sugar fortune and a dedicated amateur naturalist.[23] Inspired by paleontology courses she took at Berkeley and by safari experience with her father in Africa, Alexander decided to build a museum of natural history. As her first director, she chose Grinnell, at that time an instructor at Throop Polytechnic Institute in Pasadena (later to become CalTech).

Grinnell had been an enthusiastic and dedicated bird and mammal collector since his boyhood in Indian Territory. His father was a physician who worked with American Indians and he grew up with Oglala Sioux Indian playmates.[24] He was a founding member of the Cooper Ornithological Club, a major western bird-watching and ornithological association. (Their bulletin would later become the journal *The Condor*, edited by Grinnell for many years.) When Alexander first met Grinnell in 1907, he had already made significant theoretical contributions and was an established scientist.[25] David Starr Jordan, for example, included him in a survey of zoologists supporting his 'general law of distribution', discussed in his famous 1905 paper.[26] Grinnell became the founding director of the Museum in 1908. In 1913, he finished his Stanford PhD and was appointed to the Berkeley Zoology Department.[27]

Beginning with Grinnell's and Alexander's own collecting efforts, the museum developed into an important repository of regional specimens of vertebrates. Alexander's contributions alone came to over 20,000 specimens.[28] As part of this work, Grinnell and his staff codified a precise set of procedures for collecting and curating specimens.[29] Many of Grinnell's descriptive monographs on the systematics, geography and ecology of birds and mammals are still used today as important reference works. Grinnell also contributed important concepts to the literature on geographic distribution, ecology and evolution. He extended C. Hart Merriam's life-zone concept to a hierarchical classification system for environments, and developed an important and influential concept of the 'niche'. He argued for trinomialism in systematics as essential to studies of speciation. He also worked toward a 'two layer' theory of evolution which incorporated evolution of the environment as part of an explanation for natural selection.[30]

Different Social Worlds and their Perspectives

We have been talking so far about the goals and interests of only a few of the people essential for the museum's success as a going concern. The work at the museum, like that of scientific establishments everywhere, encompassed a range of very different visions stemming from the intersection of participating social worlds. Among these were amateur naturalists, professional biologists, the general public, philanthropists, conservationists, university administrators, preparators and taxidermists, and even the animals which became the research specimens.[31]

It is not possible to consider all these visions equally in this essay, so we are forced to consider most fully those of the entrepreneurs like Grinnell and Alexander. However, by considering the work of Grinnell and Alexander as a part of a network which spans a number of intersecting social worlds, we can begin the task of tracing the network into those other social worlds. An adequate account of *n*-way translation in this case awaits the results of tracing our way out and back again. It also requires conducting such tracings from a variety of starting points (that is, including some starting points which would be considered 'peripheral' or 'subsidiary' on a one-way translation model, such as the work of commercial specimen houses or taxidermists). Only with tracings from multiple starting points can we begin to test the robustness of the network.

The more limited work discussed in this paper is, in part, conditioned by the historical record — for us as scholars, scientific publications *are* the boundary objects which are also obligatory passage points! Records concerning the entrepreneurs who served as administrators of the museum are kept in the central archives of the university which housed the museum. Records concerning the many other elements of the network of such amateur collectors who contributed specimens to the museum and articles to naturalist society newsletters are not equally centralized. Nevertheless, it is important not to mistake the search heuristic of starting with the centralized records for a theoretical model of the structure of the network itself. In the following section we adumbrate the central features of several visions of the museum and its work.

Grinnell's Vision

One of Grinnell's passions was the elaboration of Darwinian theory which was to be derived from the work of the museum. Darwin had argued that natural selection is the chief mechanism by which organisms adapt, but had said little about the precise nature of the environmental forces of change. Grinnell wanted to extend the Darwinian picture by developing a theory of the evolution *of the environment* as the driving force behind natural selection.[32]

Sadly, Grinnell died before he was able to express his views on evolution and speciation in a major theoretical monograph. (Some of his more important views are excerpted in a book posthumously produced by his students.[33]). While in the field, he did outline such a volume and his research programme is perhaps best characterized by its title, *Geography and Evolution*. Grinnell's overarching theoretical concern was to bring the study of both physical and biotic environmental factors to bear on the problems of evolution. The chapter titles of his book outline serve to summarize the topics to which Grinnell had devoted his career. He felt that their synthesis would have fulfilled his theoretical programmatic:

1. The concept of distributional limitation; chronological versus spatial conditions.
2. The nature of barriers; examples of different sorts of barriers in mammals and birds.
3. Distributional areas defined: realms, life-zones, faunal areas, association; the ecologic niche.
4. Bird migration as a phase of geographic distribution.
5. Kinds of isolation; degrees of isolation as influencing results; the significance of geographic variation.
6. 'Plasticity' versus 'conservatism' in different groups of birds and mammals.
7. The pocket gophers and the song sparrows of California.
8. Reconcilability of geographic concept with that of genetics; species and subspecies in nature defined.
9. 'Orthogenesis' from the standpoint of geographic variation.
10. The bearing of geography and evolution upon human problems.[34]

From these titles, it is clear that Grinnell's approach to questions of evolution differed radically from, say, those of experimental genetics. His natural world was a large-scale, topographical one; his units of analysis and selection were subspecies and species, habitats and niches. This vision required vast amounts of highly detailed data about flora, fauna and aspects of the environment. He needed a small army of assistants to collect these data.

Prior to the establishment of the museum, Grinnell and Alexander exchanged many letters in which they expressed their hopes and visions for its future. In one of these letters Grinnell stated his scientific and political goals:

> First, as regards the working up of the Alaska mammals, it seems to me it should be done as far as possible by our own men. We want to establish a *center of authority* on this coast. I take it that was one purpose you had in mind founding the institution. I will grant that it would take our man, whoever he may be, longer to work up the paper, than the BS [Biological Survey] people. But in the former case we would be ever so much the stronger and better able to tackle the next problem...
>
> I believe in buying desirable material where *definitely* in hand and subject to selection and inspection. I have more faith, however, in the *salaried* field man who turns in everything he finds.[35]

Grinnell was clearly concerned to ensure that the materials collected by others met his scientific requirements. The odd specimen collected from here or there might serve as backup for work in taxonomy, but collection for ecological and evolutionary purposes required more thorough documentation. This included documenting the presence of groups of animal species in a particular place at the same time of day and season of the year. It also required comparisons of samples over time — hence Grinnell's preference for the salaried field man. In other words, conducting scientific research on problems in ecology and evolution in a museum setting required more than just a change of interests and training on the part of the scientific staff; it also required changes in basic collecting and curating procedures. Moreover, Grinnell clearly had an institutional goal as well as a research goal and that was to build a centre of authority. One means of doing this would be to build collections of scientific value which are not easily duplicated elsewhere, or which are tailored to particular research problems not well served by collections elsewhere. Grinnell focused his collecting efforts on the American west, a place distinguished from the east by its great geographical diversity, and asked scientific questions which could only be answered by careful consideration of such geographically-based organic diversity on a finer scale than is available in museums pursuing world-wide collections.

Grinnell needed accurate information in the form of carefully preserved animal specimens *and* documented native habitats over tens or hundreds of years. This placed constraints on the museum's physical organization.[36] In an essay titled 'The Museum Conscience', Grinnell argued that the order and accuracy are the chief aims of the curator (once the specimens are safely preserved). On the subject of order, he wrote that,

> To secure a really practicable scheme of arrangement [of specimens, card indexes and data on specimen labels] takes the best thought and much experimentation on the part of the keenest museum curator. Once he has selected or devised his scheme, his work is not done, moreover, until this scheme is in operation through all the materials in his charge. Any fact, specimen, or record left out of order is lost. It had, perhaps, better not exist, for it is taking space somewhere; and space is the chief cost initially and currently in any museum.[37]

On the second aim, accuracy, Grinnell continued,

> The second essential in the care of scientific materials is *accuracy*. Every item on the label of each specimen, every item of the general record in the accession catalog, must be precise as to fact. Many errors in published literature, now practically impossible to 'head off', are traceable to mistakes on labels. Label-writing having to do with scientific materials is not a chore to be handed over casually to '25-cent-an-hour' girl, or even to the ordinary clerk. To do this essential work correctly requires an exceptional genius plus training...By no means *any* person that happens to be around is capable of doing such work with reliable results.[38]

Grinnell's vision of environmental evolution reinforced his conception of collection and curation.[39] He designed the museum so that sampling from restricted locations over long periods of time would capture evolution in progress as environments changed.

Fulfillment of Grinnell's theoretical vision required that specimens and field notes collected over many years be painstakingly curated. In this fashion comparisons of materials could be made by scientists who would come to work for the museum after Grinnell himself was long gone. This concern was not unique to Grinnell or his museum,[40] but Grinnell was a master at articulating both the 'museum conscience', as he called it, and his scientific goals.[41] That is, both preservation for posterity and hot new theoretical findings must be protected.

Grinnell, too, had a sense of urgency about 'preserving California'. Whereas the Smithsonian Institution in Washington and the American Museum in New York had the entire world's natural species for their purview, Alexander, Grinnell and their associates limited themselves to Californian birds and mammals and, later, reptiles and amphibians.[42] As he wrote to Alexander in the early years of the museum:

> ...there is nothing attractive about collecting in a settled-up, level country. But it *ought* to be done, and the longer we wait, the fewer 'waste lots' there will be in which to trap for native mammals...[43]

Again in May of the same year, Grinnell wrote,

> It would surely be a fine thing if we could acquire a collection of fresh-water ducks, geese, waders, etc. All the species, with the possible exception of killdeer and herons, are decreasing in numbers rapidly, and it is at least certain that specimens will never be obtainable to better advantage than now. All thru [sic] San Joaquin Valley, many of the former marshy areas are not ditched or diked; and the great fields, where geese grazed, are being cut up into farms.[44]

However, to Grinnell, the important feature of preservation was recording information. The important preserved objects were ecological facts, not mere specimens used to educate the public about a vanishing wilderness.[45] Indeed, shortly after its founding, the museum decided not to pursue displays of its objects at all. Nevertheless, it was essential to Grinnell's success as a research scientist that he continue to attract Alexander's patronage. Grinnell shaped research problems which were suited to work in the region which Alexander wished to document in the form of collections. By seeking to establish a centre of authority for problems well-served by this regional focus, Grinnell simultaneously shaped his research goals and increased the value of Alexander's continued support — not only would she be preserving a sample of California's native fauna for posterity, she would be contributing to the establishment of a research centre.

Alexander's Vision

Annie Alexander, too, saw the flora and fauna of California disappearing under the advance of civilization. She felt that it should be meticulously preserved and recorded.[46] As a passionate and single-minded patron of science, Alexander contributed funds and oversight sufficient virtually to control the museum as an autonomous organization on the Berkeley campus. She intended her museum to serve as a demonstration project to the public about what could be done in conservation and zoological research.[47]

As a rich, unmarried woman, Alexander had a degree of autonomy unusual for women during this period. Alexander's trips were primitive by comparison with the 'ladylike' expeditions to Africa made by aristocratic women in a somewhat earlier time. Her scrapbooks and the museum archives contain pictures of her camping out, toting rifles and scaling mountains. She was an indefatigable amateur collector. Along with her lifelong companion and partner, Louise Kellogg, she conducted many expeditions to gather specimens for the MVZ, the Museum of Paleontology and the Herbarium.

In addition to collecting, Alexander served the museum in other capacities. She was its primary *patron*, funding the museum building, staff salaries, specimen and equipment purchases and expeditions. She was as well a day-to-day *administrator* who approved expenditures in minute detail, including operating expenses and budget reports, hiring and firing personnel, reviewing productivity of the staff and approving the nature and location of their expeditions (Grinnell, for example, reported to her and sought her advance approval of expeditions).

In none of these roles was Alexander a theoretical scientist. While she read some evolutionary theory, her primary 'take' on the job of the museum came from her commitments to conservation and educational philanthropy. The museum was a way of preserving a vanishing nature, of making a record of that which was disappearing under the advance of civilization. For her, as for many social élites of the period, natural history was both a passionate hobby and a civic duty.

The Collector's Vision

In addition to its museums, California was imbued with a particularly vigorous conservation and nature-loving amateur constituency. John Muir and the Sierra Club, the Cooper Ornithological Club, the Society of Western Naturalists and the Save the Redwoods League, among other organizations, all brought amateurs and academics together for purposes of collecting and conservation.

Amateur collectors wanted to play a role in the scholarly pursuit of knowledge by professional scientists. They sought legitimacy for their conservation efforts. They shared with both Alexander and Grinnell the sentiment that what was unique to California and the west should be described, preserved and made available to the public.[48] The intrinsic beauty of nature should be shared and protected. The expeditions themselves were at once opportunities for peaceful observation and enjoyment of the natural world and a battle of wits between collectors on the one hand and recalcitrant animals and environments on the other.

How does one persuade a reluctant and clever animal to participate in science? For the natural historian, there is a delicate balance between capturing an animal at all costs, and capturing an animal with the integrity of its valuable information unassailed. The animals must be brought in physically intact; their habitats must be detailed so that the specimen has scientific meaning. ('Without a label', says one zoologist friend, 'a specimen is just dead meat'.) The animals, as mentioned above, must be caught quickly, before the larger ecological balances change and they adapt to new conditions. In order to measure changes, Grinnell and other theorists also needed baseline data from which to proceed.

Animals within the museum present another kind of recalcitrance: they must be preserved against decay. The littlest allies, the dermestid beetles which clean the captured specimens so that skeletons can be used for research, are often the most difficult to discipline! They escape their bounds, eat specimens they should not and eat parts of specimens that are needed for other work. Such allies are coaxed and managed through containment and a certain amount of brute force.

A typical example of the struggles with recalcitrant animal allies may be found in Louise Kellogg's field notebook from an expedition in 1911:

> March 20. We left the house at six and went to the Stop Thief traps first. Both had been robbed of their bait and the tracks of two animals, probably a civet and a coon were visible — in the one place the creature had reached through the trap and taken the bait without springing it and in the other had pushed aside a rock and got the bait out from above but in the scuffle the bait was caught in the trap and was found lying on one side partially eaten. I caught two *microtus* out of 21 traps set in the grass. The bait was eaten from two sets of dipodomys and the others were untouched.[49]

The Trappers' Vision

In fulfilling their interests in natural history and collecting, the amateur collector was often on the front line, making contact with a host of other social worlds. These included farmers and townspeople on or near whose land the collectors searched for specimens, and trappers and traders who could provide them with specimens that were rare or difficult to capture. These people were often invaluable sources of information and other sorts of help (food, camping places) — sometimes for a price.

Many of the backwoods trappers being 'interessed' by the amateur collectors or the museum workers had little or no interest in either conservation or science as such. Their coin of exchange was money, information about hunting, or possibly the exchange of a less scientifically interesting but edible specimen for one valued by the collectors. Friction between viewpoints here was smoothed by such exchanges. For example, Alexander described a set of problems with a recalcitrant trapper who wanted to sell skins to the museum:

> You will notice that two of the skulls are broken. It seems next to impossible to persuade a trapper to kill an animal without whacking him on the head. The bob cat is in rather

a sorry plight that has a history. I am holding on to Knowles [a trapper] a little longer in the hope that he may get a panther and some coyotes. He has dogs with him. He set the no. 3 traps but the coyotes as he expressed it 'did not throw them' although they walked all over them. He will have to set them finer. Knowles is about as good as the ordinary run of trappers who can't see anything in a skin except its commercial value — and the little extra care in skinning that we demand frets them.[50]

The University Administration's Vision

Another important participant in the museum enterprise was the university administration. Their vision of natural history and California was different yet again from that of the staff of the museum and the amateur collectors. The University of California during this period was trying to become a legitimate, national-class university, and was also trying to begin seriously to compete with the eastern universities for scientific resources and prestige. It was at the same time clearly a local school, a pet charity for many of the San Francisco Bay Area élite, and a training ground for local doctors, lawyers, industrialists and agriculturalists.

The university was willing to accommodate a natural history museum as long as Alexander was willing to fund it. The administration accepted Alexander's funding of the museum as part of this vision, measuring the museum's contribution to this goal by its own criteria: level of funding and prestige returned to the university as a whole. They had similar arrangements with local philanthropists Phoebe Hearst and Jane Sather for charitable research or library endeavours on campus. In turn, Alexander enjoyed an administrative power almost unheard of for single individuals at major universities today. She hired and fired museum staff, chose expedition sites and managed administrative liaison with the Regents of the university.

The different visions and economic values of participating worlds is clear from the sometimes stormy correspondence between Alexander and the Regents and university administration over autonomy for the museum. Here, Grinnell responds to Alexander's chagrin about the university president's vision of the museum in monetary terms:

I think the letter from President Wheeler is fine. You must consider his limitations (and those of the Regents) in forming any conception of the methods and aims of such an institution as the Museum. It seems nothing more than natural that these men should measure your work for the University in terms of the dollars involved. Money is the common standard, and, too, it *is* the money that makes the major part of our work possible. You deserve all the credit expressed, and more, on this score alone. It is nothing to be ashamed of, or to resent, if their appreciation seems to be prompted only by a recognition of the money cost of the Museum. They don't know any better, and the intrinsic value of the Museum and your work for it remain the same.[51]

The museum was administratively separate from the department of zoology. It was publicly active in natural history circles and it was the home for meetings of local natural history clubs such as the Cooper Club and the Society of Western Naturalists. In this sense it helped meet the university's goal of being a local cultural centre.

Analysis of Methods Standardization and Boundary Objects

The worlds listed above have both commonalities and differences. To meet the scientific goals of the museum, the trick of translation required two things: first, developing, teaching and enforcing a clear set of methods to 'discipline' the information obtained by collectors, trappers and other non-scientists; and generating a series of boundary objects which would maximize both the autonomy and communication between worlds. Different social worlds maintained a good deal of autonomy in parallel work situations. Only those parts of the work essential to maintaining coherent information were pooled in the intersection of information; the others were left alone. Participants developed extremely flexible, heterogeneous economies of information and materials, in which needed objects could be bartered, traded and bought or sold. Such economies maximized the autonomy of work considerations in intersecting worlds while ensuring 'trade' across world boundaries.

From a purely logical point of view, problems posed by conflicting views could be managed in a variety of ways:

- via a 'lowest common denominator' which satisfies the minimal demands of each world by capturing properties that fall within the minimum acceptable range of all concerned worlds; *or*
- via the use of versatile, plastic, reconfigurable (programmable) objects that each world can mould to its purposes locally; *or*
- via storing a complex of objects from which things necessary for each world can be physically extracted and configured for local purposes, as from a library; *or*
- each participating world can abstract or simplify the object to suit its demands; that is, 'extraneous' properties can be deleted or ignored; *or*
- work in the worlds can proceed in parallel except for limited exchanges of standardized sorts; *or*
- work can be staged so that stages are relatively autonomous.

The strategies of the different participants in the museum world share several of these attributes; below, we focus on two major varieties.

Methods and Collectors

> What do you think of the system? It seems complex at first reading. But it means detailed, *exact* and easily get-at-able records. And the better the records the more valuable the specimens.[52]

Specimens are preserved in a highly standardized way, so that specific information can be recovered later on when the specimen is stored in a museum. For example, it makes a great deal of difference to ease of measurement, handling and storage whether the limbs are 'frozen' at the sides of the body or outstretched, straight or bent. Colour of pelage, scales, and so on, are usually not preservable, and colour photographs or accurate notes may be the only feasible solution to this preservation problem. Whether soft parts (internal organs and fatty tissues) are preserved depends on the availability of techniques, the conventions for preserving external structures and the parts commonly studied. If precise measurements of long bones are desired, for example, the animal must usually be taken apart to expose them.[53]

For geographical distribution work, and more especially for the ecological problem of inferring environmental factors limiting species' ranges from distributional data, the taxa to which specimens belong must be linked to a geographical location and to each other. The objects of interest are *collections* of taxa represented in a particular geographical location. Study of the factors responsible for presence or absence of particular taxa from a local area proceeds according to a method outlined by Grinnell and his colleagues:

> In practice, the method used in this survey to get at the causes for differential occurrence as observed was, first, to consider the observed actual instances of restriction of individuals of each kind of animal; and second, to compare all the records of occurrence with what we know in various respects of the portion of the section inhabited, this is an attempt to detect parallels between the extent of presence of the animal and of some appreciable environmental feature or set of features.[54]

Thus, it is necessary to translate specimens into ecological units *via* a set of field notes. This creates a tension or potential incoherence between collectors and theorists. Let us examine the process of preservation of information. Once faunas are represented as lists of species (and subspecies) linked to a location, their distributional limits are established in terms of the overlapping ranges of their member taxa (or a subset of indicator species). These collections must in turn be linked to a distribution of potentially responsible environmental factors. Hence, in addition to the translation work of creating abstract objects (lists of species, lists of factors) from concrete, conventionalized ones (locations, specimens, field notes), a series of increasingly abstract maps must be created which link these objects together.

Reports of field work begin with an itinerary and often a topographical map of the region explored. Taxa represented by specimens can be plotted on these maps, and if they also serve as indicators of life-zones, faunas or associations, an ecological map of these units can be constructed. In parallel, maps of environmental factor isoclines (quantitatively or qualitatively expressed) can be constructed from field notes and geographic maps. Then the environmental factors maps can be superimposed on the maps of ecological units, and the strongest concordances used to rank environmental factors as delimiters of species distributions.[55]

The specimens *per se* are not the primary objects of ecological study — the check-lists of taxa represented in a local area are. These check-lists are then mapped into ecological units (geographically identified groups of species and subspecies) by finding subsets of the check-list which are limited to a geographical subarea. A map is constructed in terms of ranges of the ecological units set by the species ranges (within the geographical area studied) of species taken to be indicators for the zones.

Grinnell and Alexander were able to mobilize a network of collectors, cooperating scientists and administrators to ensure the integrity of the information they collected for archiving and research purposes. The precise set of standardized methods for labelling and collecting played a critical part in their success. These methods were both stringent and simple — they could be learned by amateurs who might have little understanding of taxonomic, ecological or evolutionary theory. They thus did not require an education in professional biology to understand or to execute. At the same time, they rendered the information collected by amateurs amenable to analysis by professionals. The professional biologists convinced the amateur collectors, for the most part, to adhere to these conventions — for example, to clearly specify the habitat and time of capture of a specimen in a standard format notebook.

Grinnell's insistence on, and success with, standardized methods of collecting, preserving, labelling and taking field notes is a testament to his skilful management of the complex multiple translations involved in natural history work. The methods protocols themselves, and the injunctions implied, are a record not only of the kinds of information Grinnell needed to capture for his theoretical developments, but of the conflicts between the various participating worlds. In this sense, each protocol is a record of the process of reconciliation.

Propagating methods is not an easy task. In working with amateur collectors, a major problem is to ensure that the data coming back in from the field is of reliable quality; that it does not decay en route through sloppy collecting or preserving techniques; that the collectors give enough information about where they got the beasts from so that the locations can be precisely identified. On the other hand, directions for collectors cannot be made so complicated that they interfere with the already-difficult job of camping out in the wilderness, capturing sneaky little animals or bribing reluctant farmers to preserve intact their saleable specimens.

Another way of saying this is that the allies enrolled by the scientist must be disciplined, but cannot be *overly*-disciplined. Each world is willing — for a price — to grant autonomy to the museum and to conform to Grinnell's information-gathering standards. It is only gradually that a scientist in Grinnell's position comes to be an authority. Part of this authority is exercized through the standardization of methods.

Standardizing methods is different from standardizing theory. By emphasizing how, and not what or why, methods standardization both makes information compatible and allows for a longer 'reach' across divergent worlds. Grinnell was thus able to accomplish several things at once. First, and perhaps most important, methods standardization allowed both collectors and professional biologists to find a common ground in clear, precise manual tasks. Collectors do not need to learn theoretical biology in order to contribute to the enterprise. Potential differences in beliefs about evolution or higher-order questions tend to be displaced by a focus on 'how', not 'why'. The methods thus provided a useful 'lingua franca' between amateurs and professionals. They also allowed amateurs to make a substantial contribution both to science and to conservation. The standardized specimens, fieldnotes and techniques provided consistent information for future generations or for researchers at a distance.

Grinnell's methods emphasis thus translated the concerns of his allies in such a way that their pleasure was not impaired — the basic activities of going on camping trips, adding to personal hobby collections and preserving California remained virtually untouched. With respect to the collectors, Grinnell created a mesh through which their products must pass if they want money or scientific recognition, but not so narrow a mesh that the products of their labour cannot be easily used.

One consequence of this strategy is that Grinnell created a large area of autonomy for himself from which he could *move into* more theoretical arenas. His carefully crafted relationship with Alexander involved them both in making a commitment to methods and preservation techniques. As a sponsor, Alexander was concerned with preserving a representative collection of Californiana, both for posterity and as a demonstration of good scientific practice. It is clear from their correspondence that Alexander had little concern for the contents of the scientific theory — but that she was quite concerned with curation and preservation methods.

While necessary for the sort of sweeping ecological work undertaken by the MVZ, 'methods control' alone was not sufficient. Other means were necessary to ensure cooperation across divergent social worlds. These were not engineered as such by any one individual or group, but rather emerged through the process of the work. As groups from different worlds work together, they create various sorts of boundary objects. The intersectional nature of the museum's shared work creates objects which inhabit multiple worlds simultaneously, and which must meet the demands of each one.

Boundary Objects

In natural history work, boundary objects are produced when sponsors, theorists and amateurs collaborate to produce representations of nature. Among these objects are specimens, field notes, museums and maps of particular territories. Their boundary nature is reflected by the fact that they are simultaneously concrete and abstract, specific and general, conventionalized and customized. They are often internally heterogeneous.

We have, in the management strategies of the MVZ, a situation with the following characteristics:

1. many participants share a common goal: preserve California's nature. Those that do not share this goal participate in the economy via a neutral medium — direct monetary exchange (note: this includes the university administration!);

2. all participants come to agree literally to preserve samples of its flora and fauna, as intact and as well-tagged as possible;

3. for some participants (amateur collectors, general public, trappers and farmers) this literal, concrete preservation of animals is sufficient for their purposes;

4. for others (Grinnell, university administration), literal concrete preservation is only the beginning of a long process of making arguments

to professional audiences and establishing themselves as 'experts' in some theoretical domain.

So, in the case of the museum, the different worlds share goals of conserving California and nature, and of making an orderly array out of natural variety. These shared goals are lined up in such a way that everybody has satisfying work to perform in each world. How does this happen?

In building the theories and in building the organization, Grinnell had to maintain the conventionality of the objects so that future collecting could go on. The concerns and technologies of the amateurs, farmers, and so on, needed to be preserved if they were to continue fully participating. At the same time, he had to overcome the conventionality in order to make his objects scientifically interesting. It would not be enough if all the worlds collected objects which were in some sense challenging old ways of thinking about nature, nor arguing with other parts of science. How did Grinnell balance the need for argument with the need for building on the very conventional understandings about California that the amateur collectors and clubs had? How did he escape being limited by their concerns?

Grinnell and Alexander quite brilliantly began their enterprise by building on a goal they shared with several participants (the university presidents, nature-lovers, sponsors and local social élites): draw a line around the west (sometimes even around the state) and declare it a nature preserve. (As one current member of the museum staff has wryly stated: 'When you get to the Nevada border, turn around and drive the other way!') For Grinnell, then, California became a delimitable 'laboratory in the field' giving his research questions a regional, geographical focus. For the university administration, the regional focus supported its mandate to serve the people of the state. For the amateur naturalists concerned with the flora and fauna of their state, research conducted within its bounds also served their goals of preservation and conservation. This first constraint is a weak one with many advantages. It gives California itself the status of a boundary object, an object which lives in multiple social worlds and which has different identities in each.

Grinnell then transformed this agreement into a resource for getting more money. He became one of the primary people in charge of preserving California. He made extensive alliances with conservation groups. This provided him with a definite but still weakly-constrained and weakly-structured base. Furthermore, the geographical concepts he wanted to advance were built on this kernel of support for California preservation. He needed a baseline for his geographical theories and comparisons, as the conservation movement needed and wanted information about the natural baseline threatened by development interests. At the core and beginning of his work, then, he placed a common goal and conventional understanding, with boundaries from several different worlds which coincide. These coincident boundaries, around a loosely-structured, boundary object, provide an anchor for more widely-ranging, riskier claims.[56]

From the standardized information which Grinnell collected, he built an orderly repository. And from this library of specimens, he was able to build ecological theories different from those being developed in the rest of the country. His autonomy in this regard rested on solving the problems of boundary tensions posed by the multiple intersections of the worlds which met in the museum. Grinnell's work was highly abstract, with a strong empirical base and strikingly strong support from participating worlds.

In analyzing these translation tasks represented by the MVZ undertaking, we found four types of boundary objects. This is not an exhaustive list by any means. These are only analytic distinctions, in the sense that we are really dealing here with systems of boundary objects which are themselves heterogeneous.

1. *Repositories*. These are ordered 'piles' of objects which are indexed in a standardized fashion. Repositories are built to deal with problems of heterogeneity caused by differences in unit of analysis. An example of a repository is a library or museum. It has the advantage of modularity. People from different worlds can use or borrow from the 'pile' for their own purposes without having directly to negotiate differences in purpose.

2. *Ideal type*. This is an object such as a diagram, atlas or other description which in fact does not accurately describe the details of any one locality or thing. It is abstracted from all domains, and may be fairly vague. However, it is adaptable to a local site precisely because it is fairly vague; it serves as a means of communicating and cooperating symbolically — a 'good enough' road map for all parties. An example of an ideal type is the species. This is a concept which in fact described no specimen, which incorporated both concrete and theoretical data and which served as a means of communicating across both worlds. Ideal types arise with differences in degree of abstraction. They result in the deletion of local contingencies from the common object and have the advantage of adaptability.

3. *Coincident boundaries*. These are common objects which have the same boundaries but different internal contents. They arise in the presence of different means of aggregating data and when work is distributed

over a large-scale geographic area. The result is that work in different sites and with different perspectives can be conducted autonomously while cooperating parties share a common referent. The advantage is the resolution of different goals. An example of coincident boundaries is the creation of the state of California itself as a boundary object for workers at the museum. The maps of California created by the amateur collectors and the conservationists resembled traditional roadmaps familiar to us all, and emphasized campsites, trails and places to collect. The maps created by the professional biologists, however, shared the same outline of the state (with the same geo-political boundaries), but were filled in with a highly abstract, ecologically-based series of shaded areas representing 'life zones', an ecological concept.

4. *Standardized forms*. These are boundary objects devised as methods of common communication across dispersed work groups. Because the natural history work took place at highly distributed sites by a number of different people, standardized methods were essential, as discussed above. In the case of the amateur collectors, they were provided with a form to fill out when they obtained an animal, standardized in the information it collected. The results of this type of boundary object are standardized indexes and what Latour would call 'immutable mobiles' (objects which can be transported over a long distance and convey unchanging information). The advantages of such objects are that local uncertainties (for instance, in the collecting of animal species) are deleted.

People who inhabit more than one social world — marginal people — face an analogous situation. Traditionally, the concept of marginality has referred to a person who has membership in more than one social world: for example, a person whose mother is white and father is black.[57] Park's classic work on the 'marginal man' discusses the tensions imposed by such multiple membership, problems of identity and loyalty.[58] Marginality has been a critical concept for understanding the ways in which the boundaries of social worlds are constructed, and the kinds of navigation and articulation performed by those with multiple memberships. The strategies employed by marginal people to manage their identities — passing, trying to shift into a single world, oscillating — provide a provocative source of metaphors for understanding objects with multiple memberships. Can we find similar strategies among those creating or managing joint objects across social world boundaries?

A social world, such as the world of amateur natural history collectors, 'stakes out' territory, either literal or conceptual. If a state of war does not prevail, then institutionalized negotiations manage ordinary affairs when different social worlds share the same territory (for instance, the

United States Government and the Mafia). Such negotiations include conflict and are constantly challenged and refined. Everett Hughes has talked about such overlaps and has described organizations which manage collisions in space sovereignty as 'intertribal centers'.[59] Gerson's analysis of resources and commitments provides a general model of sovereignties based on commitments of time, money, skill and sentiment.[60] Gerson and Gerson, drawing on Hughes' earlier work, have discussed the complex management of such overlapping place perspectives.[61] In their analysis, the central cooperative task of social worlds which share the same space but different perspectives is the 'translation' of each others' perspectives.

In this paper, we are interested in that sort of *n*-way translation which includes scientific objects. In particular, we are interested in the kinds of translations scientists perform in order to craft objects containing elements which are different in different worlds — objects marginal to those worlds, or what we call boundary objects.[62] In conducting collective work, people coming together from different social worlds frequently have the experience of addressing an object that has a different meaning for each of them. Each social world has partial jurisdiction over the resources represented by that object, and mismatches caused by the overlap become problems for negotiation. Unlike the situation of marginal people who reflexively face problems of identity and membership, however, the objects with multiple memberships do not change themselves reflexively, or voluntarily manage membership problems. While these objects have some of the same properties as marginal people, there are crucial differences.

For people, managing multiple memberships can be volatile, elusive, or confusing; navigating in more than one world is a non-trivial mapping exercise. People resolve problems of marginality in a variety of ways: by passing on one side or another, denying one side, oscillating between worlds, or by forming a new social world composed of others like themselves. However, management of these scientific objects — including construction of them — is conducted by scientists, collectors and administrators only when their work coincides. The objects thus come to form a common boundary between worlds by inhabiting them both simultaneously. Scientists manage boundary objects via a set of strategies only loosely comparable to those practised by marginal people.

Intersections place particular demands on representations, and on the integrity of information arising from and being used in more than one world. Because more than one world or set of concerns is using and making the representation, it has to satisfy more than one set of concerns.

When participants in the intersecting worlds create representations together, their different commitments and perceptions are resolved into representations — in the sense that a fuzzy image is resolved by a microscope. This resolution does not mean consensus. Rather, representations, or inscriptions, contain at every stage the traces of multiple viewpoints, translations and incomplete battles. Gerson and Star have discussed a similar collision in an office workplace,[63] and considered the problem of evaluating the standards which apply as reconciliation takes place — a problem which computer scientist Carl Hewitt has called 'due process'.[64] Gökalp has described some of the processes of collisions which arise when multiple fields come together; he calls these 'borderland' disciplines.[65]

The production of boundary objects is one means of satisfying these potentially conflicting sets of concerns. Other means include imperialist imposition of representations, coercion, silencing and fragmentation.[66]

Summary

The different commitments of the participants from different social worlds reflects a fascinating phenomenon — the functioning of mixed economies of information with different values and only partially overlapping coin. Andrews has a compelling example of this from a natural history expedition of the period to Mongolia: natives there used fossils for *fang shui* (geomancy), and were in the habit of dissolving them in liquid and drinking them![67] The sacred fossil beds were well-protected against foraging paleontologists, who considered them equally valuable but for different reasons. The economy of the museum thus evolves as a mixture of barter, money and complex negotiations: money in exchange for furs and animals from trappers; animals in exchange for other animals from other museums and collectors; scientific classification in exchange for specimens donated by amateur naturalists; prestige and legitimacy for economic support; food and bait in traps in exchange for animals' unwitting cooperation.

As the museum matures, and becomes more efficient, the scientists have made headway in standardizing the interfaces between different worlds. In the case of museum work, this comes from the standardization of collecting and preparation methods. By reaching agreements about methods, different participating worlds establish protocols which go beyond mere trading across unjoined world boundaries. They begin to devise a common coin which makes possible new kinds of joint endeavour.

But the protocols are not simply the imposition of one world's vision on the rest; if they are, they are sure to fail. Rather, boundary objects act as anchors or bridges, however temporary.

The central analytical question raised by this study is: how do heterogeneity and cooperation coexist, and with what consequences for managing information? The museum is in a sense a model of information processing. In the strategies used by its participants are several sophisticated answers to problems of complexity, preservation and coordination. Our future work will examine these answers in different domains, including the history of evolutionary theory and the design of complex computer systems.

• NOTES

We would like to thank our colleague Elihu Gerson of Tremont Research Institute for many helpful conversations about the content of this paper. We would also like to thank The Bancroft Library, University of California, for access to the papers of Joseph Grinnell, Annie Alexander and the Museum of Vertebrate Zoology. David Wake and Barbara Stein of the Museum of Vertebrate Zoology have graciously allowed us access to the museum's archives and assisted us in locating material; Howard Hutchinson of the Berkeley University Museum of Paleontology generously provided access to the Alexander-Merriam correspondence in the archives there. Annetta Carter, Frank Pitelka, Joseph Gregory and Gene Crisman have provided valuable firsthand information about Alexander and Grinnell. We would also like to thank Michel Callon, Adele Clarke, Joan Fujimura, Carl Hewitt, Bruno Latour, John Law and Anselm Strauss for their helpful comments and discussions of many of these ideas. Star's work on this paper was supported in part by a generous grant from the Fondation Fyssen, Paris.

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6. W. V. O. Quine, *Word and Object* (Cambridge, MA: MIT Press, 1960).

7. See the excellent review by L. Daston, 'The Factual Sensibility', *Isis*, Vol. 79 (1988), 452–67.

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20. E. Cittadino, 'Ecology and the Professionalization of Botany in America, 1890–1905', *Studies in History of Biology*, Vol. 4, (1980), 171–98; Joel Hagen, 'Organism and Environment: Fredric Clements's Vision of a Unified Physiological Ecology', in R. Rainger, K. Benson and J. Maienschein (eds), *The American Development of Biology* (Philadelphia, PA: University of Pennsylvania Press, 1988), 257–80; William Kimler, 'Mimicry: Views of Naturalists and Ecologists Before the Modern Synthesis', in Marjorie Grene (ed.), *Dimensions of Darwinism* (Cambridge: Cambridge University Press, 1983), 97–128. See also Kingsland, MacIntosh, op. cit. note 19.

21. Hilda Grinnell, 'Joseph Grinnell: 1877–1939', *The Condor*, Vol. 42 (1940), 3–34.

22. Ernst Mayr, 'Ecological Factors in Speciation', *Evolution*, Vol. 1 (1947), 263–88; Mayr, 'Speciation and Systematics', in Glenn L. Jepsen, George Gaylord Simpson and Ernst Mayr (eds), *Genetics, Paleontology and Evolution* (Princeton, NJ: Princeton University Press, 1949), 281–98; David Lack, 'The Significance of Ecological Isolation', ibid., 299–308.

23. See Hilda W. Grinnell, *Annie Montague Alexander* (Berkeley, CA: Grinnell Naturalists Society, 1958). That she was an amateur naturalist, and not merely a financial backer is clear from Kohlstedt's (1976) discussion of the amateur tradition, op. cit. note 8. Amateurs were more interested in scientific investigation than the general public, which was largely interested in the exposition of ideas about nature as part of the general culture, but amateurs typically had a broad vision of the aims and nature of scientific research: Kohlstedt, ibid., 175. The dependence on amateur/patrons declined as universities and governments took over the financial stewardship of science, and eventually amateurs such as Alexander virtually disappeared from scientific academia.

24. See Grinnell, ibid.; Alden Miller, 'Joseph Grinnell', *Systematic Zoology*, Vol. 3 (1964), 195–249.

25. J. Grinnell, 'The Origin and Distribution of the Chestnut-Backed Chickadee', *The Auk*, Vol. 21 (1904), 364–65, 368–78.

26. D. Jordan, 'The Origin of Species Through Isolation', *Science*, Vol. 22 (1905), 545–62.

27. That Grinnell could become director of the Museum without appointment in the Zoology Department suggests that credentials for museum naturalists were not identical with those required for academic appointment. Grinnell's career marks a transition phase in the incorporation of research natural history into academic science. For histories of the Berkeley Zoology Department, including the MVZ, see Richard Eakin, 'History of Zoology at the University of California, Berkeley', *Bios*, Vol. 27 (1956), 66–92. (Reprint available from the Department of Zoology at Berkeley.)

28. See Eakin, op. cit. note 27.

29. These procedures included directives such as: to use a single serial set of identification numbers for all specimens collected during an expedition regardless of type, 'road-kills', nests, eggs, wet preservations, and so on; to give precise data on the location of capture of a specimen including altitude and county; to 'Attend minutely to proper punctuation'; to observe the proper order for reporting data on both field tags and in field notebooks; to pack 'Miscellaneous material. . . with as great care as skins or skulls. Cheek pouch contents, feces, etc., should be placed in small envelopes or boxes, *with labels inserted*, and such containers packed in a stout box to prevent crushing'; and most importantly, to 'Write *full* notes, even at risk of entering much information of apparently little value. One cannot anticipate the needs of the future, when notes and collection are worked up. . . . Be alert for new ideas and new facts'. These quotations were taken from a handout, 'Suggestions as to Collecting', used in Grinnell's natural history course, Zoology 113, Grinnell Correspondence and Papers, Bancroft Library, University of California, Berkeley. The handout was emended and used by a number of Grinnell's successors at Berkeley and elsewhere. See also, Hall, op. cit. note 12.

30. See Griesemer, op. cit. note 9.

31. On museums in general, see E. Alexander , *Museums in Motion: An Introduction to the History and Functions of Museums* (Nashville, TN: American Association for State and Local History, 1979); Laurence V. Coleman, *The Museum in America*, 3 Volumes (Washington DC: The American Association of Museums, 1939); George Stocking, 'Essays on Museums and Material Culture', in G. Stocking, Jr. (ed.), *Objects and Others: Essays on Museums and Material Culture* (Madison, WI: University of Wisconsin Press, 1983), 3–14. On natural history museums in particular, see C. Adams, 'Some of the Advantages of an Ecological Organization of a Natural History Museum', *Proceedings of the American Association of Museums*, Vol. 1 (1907), 170–78; K. Benson, 'From Museum Research to Laboratory Research: The Transformation of Natural History into Academic Biology', in Rainger et al., op. cit. note 20, 49–83; E. Colbert, 'What is a Museum?', *Curator*, Vol. 4 (1961), 138–46; Joseph Grinnell, 'The Methods and Uses of a Research Museum', *Popular Science Monthly*, Vol. 77 (1910), 163–69; S. Kohlstedt, 'Henry A. Ward: The Merchant Naturalist and American Museum Development', *Journal of the Society for the Bibliography of Natural History*, Vol. 9 (1980), 647–61; Kohlstedt, 'Natural History on Campus: From Informal Collecting to College Museums', paper delivered to the West Coast History of Science Association (Friday Harbor, WA: September 1986); Kohlstedt (1988), op. cit. note 8; Kohlstedt, 'Museums on Campus: A Tradition of Inquiry and Teaching', in Rainger et al., op cit. note 20, 15–47; Ernst Mayr, 'Alden Holmes Miller', *National Academy of Sciences of the USA, Biographical Memoirs*, Vol. 33 (1973), 176–214; Ronald Rainger, 'Just Before Simpson: William Diller Matthew's Understanding of

Evolution', *Proceedings of the American Philosophical Society*, Vol. 130 (1986), 453–74; Rainger, 'Vertebrate Paleontology as Biology: Henry Fairfield Osborn and the American Museum of Natural History', in Rainger et. al., op. cit. note 20, 219–56; Dillon Ripley, *The Sacred Grove: Essays on Museums* (New York: Simon & Schuster, 1969); A. Ruthven, *A Naturalist in a University Museum* (Ann Arbor, MI: University of Michigan Alumni Press, 1963).

32. Joseph Grinnell, 'Significance of Faunal Analysis for General Biology', *University of California Publications in Zoology*, Vol. 32 (1928), 13–18.

33. Joseph Grinnell, *Joseph Grinnell's Philosophy of Nature, Selected Writing of a Western Naturalist* (Berkeley & Los Angeles, CA: University of California Press, 1943, reprinted by Freeport, NY: Books for Libraries Press, 1968).

34. Ibid., viii.

35. Joseph Grinnell to Annie Alexander, 14 November 1907, Joseph Grinnell Papers, The Bancroft Library, University of California, Berkeley.

36. Grinnell, op. cit. note 33. This essay, originally a director's report to the President of the University of California, was later published in *The Popular Science Monthly* as an article outlining Grinnell's vision titled, 'The Methods and Uses of a Research Museum'.

37. Grinnell, 'The Museum Conscience' (1922), in op. cit. note 33, 107–09, at 108.

38. Ibid.

39. Joseph Grinnell, 'Barriers to Distribution as Regards Birds and Mammals', *The American Naturalist*, Vol. 48 (1914), 248–54; Grinnell, 'An Account of the Mammals and Birds of the Lower Colorado Valley with Especial Reference to the Distributional Problems Presented', *University of California Publications in Zoology*, Vol. 12 (1914), 51–294.

40. See Rainger, 'Just Before Simpson', op. cit. note 31, for similar considerations by W. D. Matthew in the American Museum.

41. For another early example, see Adams, op. cit. note 31.

42. See E. M. Gerson, 'Audiences and Allies: The Transformation of American Zoology, 1880–1930', paper presented to the conference on the History, Philosophy and Social Studies of Biology (Blacksburg, VA, June 1987); Eakin, op. cit. note 27, reports the possibly apocryphal story that Jordan and Grinnell agreed that Stanford would get fishes and Berkeley would get birds and mammals.

43. Joseph Grinnell to Annie Alexander, 13 February 1911, Joseph Grinnell Papers, The Bancroft Library, University of California, Berkeley.

44. Joseph Grinnell to Annie Alexander, 11 May 1911, Joseph Grinnell Papers, The Bancroft Library, University of California, Berkeley.

45. See Grinnell, op. cit. note 31.

46. See Grinnell, op. cit. note 23, 7.

47. Annie Alexander to Joseph Grinnell, 6 January 1911, Annie M. Alexander Papers (Collection 67/121 c), The Bancroft Library, University of California, Berkeley.

48. See also Smith, op. cit. note 18.

49. Louise Kellogg, 1911 field notebook, Fieldnote Room, Museum of Vertebrate Zoology, University of California, Berkeley. See also Annie Alexander, 1911 field notebook, for similar observations.

50. Annie Alexander to Joseph Grinnell, 21 February 1911, Annie M. Alexander Papers (Collection 67/121 c), The Bancroft Library, University of California, Berkeley.

51. Joseph Grinnell to Annie Alexander, 27 March 1911, Joseph Grinnell Papers, The Bancroft Library, University of California, Berkeley.

52. Joseph Grinnell to Annie Alexander, 14 November 1907, Joseph Grinnell Papers, The Bancroft Library, University of California, Berkeley.
53. See Hall, op. cit. note 12, for a full discussion of these preparation and preservation techniques.
54. Joseph Grinnell, J. Dixon and Jean Linsdale, 'Vertebrate Natural History of a Section of Northern California through the Lassen Peak Region', *University of California Publications in Zoology*, Vol. 35 (1930), 1–594 and i–v.
55. See Griesemer, op. cit. note 9.
56. William C. Wimsatt, 'Robustness, Reliability and Overdetermination', in M. Brewer and B. Collins (eds), *Scientific Inquiry and the Social Sciences* (San Francisco, CA: Jossey-Bass, 1981), 124–63.
57. Robert E. Park, 'Human Migration and the Marginal Man', in his *Race and Culture* (New York: The Free Press, 1928, reprinted 1950), 345–56; E. C. Hughes, 'Social Change and Status Protest: An Essay on the Marginal Man', in his *The Sociological Eye*, op. cit. note 1, 220–28.
58. See also Everett V. Stonequist, *The Marginal Man: A Study in Personality and Culture Conflict* (New York: Russell & Russell, 1937, reprinted 1961).
59. E. C. Hughes, 'The Ecological Aspect of Institutions', in his *The Sociological Eye*, op. cit. note 1, 5–13.
60. Elihu M. Gerson, 'On "Quality of Life"', *American Sociological Review*, Vol. 41 (1976), 793–806.
61. Elihu M. Gerson and M. Sue Gerson, 'The Social Framework of Place Perspectives', in G. T. Moore and R. Golledge (eds), *Environmental Knowing: Theories, Research and Methods* (Stroudsberg, PA: Dowden, Hutchinson & Ross, 1976), 196–205.
62. See Star, op. cit. note 15.
63. Elihu M. Gerson and Susan Leigh Star, 'Analyzing Due Process in the Workplace', *ACM Transactions on Office Information Systems*, Vol. 4 (1986), 257–70.
64. Carl Hewitt, 'Offices are Open Systems', *ACM Transactions on Office Information Systems*, Vol. 4 (1986), 271–87.
65. Iskander Gökalp, 'Report on an Ongoing Research: Investigation on Turbulent Combustion as an Example of an Interfield Research Area', paper presented at the Society for the Social Studies of Science (Troy, NY, November 1985).
66. We are grateful to an anonymous referee for drawing our attention to the limits of the cooperation model, and the importance of conflict and authority in science-making.
67. Roy Chapman Andrews, *Across Mongolian Plains* (New York: D. Appleton, 1921).

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