# Collaboration in the Museum of Vertebrate Zoology

JAMES R. GRIESEMER

*Program in History and Philosophy of Science*
*University of California*
*Davis, California 95616*

ELIHU M. GERSON

*Tremont Research Institute*
*458 Twenty-Ninth Street*
*San Francisco, California 94131*

In November 1947, at the age of eighty, Miss Annie M. Alexander (1867–1950) of Oakland, California, loaded her Dodge Power Wagon equipped with four-wheel drive, eight forward speeds, a front bumper winch, and a wire body cage and set out with her companions, Louise Kellogg and Annetta Carter, for a three-month collecting trip to Baja California.[1] The trip was one of the last in the long career of an amateur naturalist whose vision, patronage, and collecting work led to the creation, design, and development of research museums for vertebrate zoology and paleontology at the University of California. Annie Alexander contributed personal and purchased collections totaling 20,564 specimens and sponsored every professional and student member of her Museum of Vertebrate Zoology through endowments and direct support from its founding in 1908. The story of her work with Joseph Grinnell (1877–1939), the western naturalist, ecologist, and conservationist who became the first director of the Museum, raises significant questions about the appropriate categories for describing scientific collaboration and challenges some misconceptions of museum science.

Scientific collaboration is usually conceptualized as the work of teams of scientists with shared goals, such as formulating or testing particular empirical hypotheses, and with shared products, such as coauthored papers. But the work of research involves more than scientists. There are many kinds of joint effort that serve

1. Hilda W. Grinnell, *Annie Montague Alexander* (Berkeley: Grinnell Naturalists Society, 1958), p. 24.

*Journal of the History of Biology*, vol. 26, no. 2 (Summer 1993), pp. 185–203.

science without being limited to scientists or to shared "scientific" goals. On the other hand, science is not an *undifferentiated* network of social relations. Our purpose in this paper is to suggest that we need a wide view of collaboration, and of the products and productions of scientific work, in order to understand some important limitations on (and opportunities for) research. We categorize several kinds of joint effort that help illustrate this wide view.

## MAKING A MUSEUM FOR "THE WORK THAT COUNTS"

Alexander was heir to a Hawaiian shipping and sugar fortune. She had become an ardent hunter and collector of mammals and fossils through big game hunting trips with her father, and through courses in paleontology from John C. Merriam (1869–1945) at Berkeley.[2] In 1907, upon returning from a trip to Alaska to collect bears, Alexander wrote to Grinnell (at that time a young naturalist in Pasadena), who had given her valuable advice prior to the trip:

> What you say of the lack of a large working collection of Birds, Mammals and Reptiles on this coast is only too true. The two Dr. Merriams have talked about it and Dr. Ritter and others but that hasn't seemed to remedy things.[3]
>
> I should like to see a collection developed (more especially of the California fauna) and would be glad to give what support I could if I could find the right man to take hold; some one interested not only in bringing a collection together but with the larger object in view, namely gathering data in connection with the work that would have direct bearing on the important biological issues of the day, work systematically and intelligently carried on is the work that counts. But there is so much to be said in this connection I won't discuss it further here.[4]

2. Ibid., p. 7. John C. Merriam (1869–1945) founded the Paleontology Department at Berkeley in 1912 and left to become president of the Carnegie Institution of Washington in 1920.

3. C. Hart Merriam (1855–1942; no relation to John C.) was a mammalogist and biogeographer who founded the Bureau of Ornithology and Mammalogy in the U.S. Department of Agriculture (later the Biological Survey); he left the government and came to California in 1910. William Emerson Ritter (1856–1944), a zoologist, was first chairman of Berkeley's Zoology Department in 1891, and founding director of the Scripps Institution in 1902.

4. Annie Alexander to Joseph Grinnell, October 18, 1907, Alexander Papers, 67/121c, Bancroft Library, University of California, Berkeley (cited hereinafter as Alexander Papers).



Grinnell was born in Oklahoma on February, 27, 1877; he received a B.A. from Throop Polytechnic Institute in Pasadena in 1897, and an M.A. at Stanford in 1901. When Alexander approached him in 1907, he was continuing his graduate studies at Stanford with ichthyologist Charles H. Gilbert (1859–1928). He was also an instructor at Throop and editor of *The Condor*, the principal bird-watching and ornithological journal on the West Coast.[5]

In 1908, the Museum of Vertebrate Zoology was doubly new. In planning and organizing a museum of natural history, Alexander and Grinnell were engaged not only in founding an organization, but in developing a way of conducting research as well. The nature of natural history museums as institutions at that time was unsettled, with many researchers and administrators engaged in reformulating every aspect of museum practice.[6] A substantial literature was developing on the aims of natural history museums and the principles of their administration. These sometimes lengthy handbooks described many aspects of the workings of museums, including curation (acquisition, specimen preparation, labeling, storage, exhibition), staffing, accessibility to the public, funding, and architecture. It was often noted that the aim of the work is to produce collections for research, for exhibition, or both. A unified perspective on "the museum method" emerged from these treatises and the practices they described, which promoted the professionalization of museum administration. Even "Books, manuscripts, pictures, maps, etc.," wrote George Brown Goode (1851–1896) in his treatise *The Principles of Museum Administration*, "become specimens when treated in the museum

5. Hilda W. Grinnell, "Joseph Grinnell, 1877–1939," *Condor, 42* (1940), 3–34.

6. See T. Barbour, *A Naturalist's Scrapbook* (Cambridge, Mass.: Harvard University Press, 1946); George Brown Goode, "The Principles of Museum Administration," *Annual Report of the Board of Regents of the Smithsonian Institution, Report of the U.S. National Museum, Part II* (Washington, D.C.: U.S. Government Printing Office, 1895/1901), pp. 193–240; F. M. Chapman, "What Constitutes a Museum Collection of Birds?" in *Proceedings of the Fourth International Ornithological Congress*, ed. R. Bowdler Sharpe, E. J. O. Hartert, and J. L. Bonhote (London: Dulau, 1905), pp. 144–156; Ernst Hartert, "The Principal Aims of Modern Ornithology," in ibid., pp. 265–270; J. Grinnell, "The Uses and Methods of a Research Museum," *Pop. Sci. Mon., 77* (1910), 163–169; idem, "The Museum Conscience," *Mus. Work, 4* (1922), 62–63; C. C. Adams, "Some of the Advantages of an Ecological Organization of a Natural History Museum," *Pro. Amer. Ass. Mus., 1* (1908), 170–178; idem, "The Relation of Field Excursions to the Activities of Local Museums," *N.Y. State Mus. Bull., 284* (1929), 43–52; A. Ruthven, *A Naturalist in a University Museum* (Ann Arbor: privately printed, 1931).

method."[7] Goode, director of the U.S. National Museum from 1887 to 1896, sought the essence of museums of various types, writing, for example, that "An efficient educational museum may be described as a collection of instructive labels, each illustrated by a well-selected specimen."[8]

Most contributors to the turn-of-the-century literature stressed that museums are dynamic institutions: "A finished museum is a dead museum, and a dead museum is a useless museum."[9] They also stressed free access to museum material by professionals, amateurs, and the general public as a central goal of administration. Many advocated an exchange of duplicate specimens, and specialization: each museum to concentrate on the local fauna and perhaps some additional specialties, so that full coverage of taxa and localities would be achieved collectively while containing costs.[10] Some administrators also rejected the "stamp-collecting" approach of amassing specimens as an end in itself, choosing to focus instead on scientific problems of newly emerging biological disciplines (including economic and conservation aspects) as the justification for collections.[11]

From the first, Alexander and Grinnell agreed that the new Museum should concentrate on Western mammals and birds; Stanford University, under the leadership of David Starr Jordan (1851–1931), had specialized in fishes, and the California Academy of Sciences in San Francisco had substantial collections of sea birds.[12] But differences between Grinnell and Alexander soon emerged. Alexander's vision of the Museum was focused on organizing and displaying nature. She favored elaborate exhibits that would interest the public and help to preserve representatives of California's diverse fauna. In line with this, she discussed several exhibit "groups" she was having built for installation at Stanford and at Berkeley.[13] Grinnell, by contrast, focused on the Museum as a research organization, dedicated to capturing the variety of nature for study in the face of economic development and the inevitable extinction and evolution of species.

It soon developed that the services of an expert exhibit preparator

7. Goode, "Principles," p. 220.
8. Ibid., p. 220.
9. Ibid., p. 201.
10. Ibid., p. 203 ff.
11. See the works by Adams, Goode, Grinnell, and Hartert, cited above (n. 6).
12. Grinnell to Alexander, February 2, 1907, Joseph Grinnell Papers, C-B 1003, Bancroft Library, University of California, Berkeley (cited hereinafter as Grinnell Papers).
13. Alexander to Grinnell, October 18, 1907, Alexander Papers.



would cost three times as much as those of an experienced curator, in addition to the costs of space, public access, and the like,[14] and Alexander became convinced of the wisdom of allowing the California Academy of Sciences (then preparing its new facilities in Golden Gate Park, San Francisco) to carry the burden of public education. Thomas Barbour and Sally Gregory Kohlstedt have both described the development of a similar division of labor in 1913–1914 between the Museum of Natural History in Boston and the Museum of Comparative Zoology in Cambridge, Massachusetts.[15] Here, then, is the emergence of one kind of joint effort – the *segregation of functions among organizations* serving overlapping constituencies. This is not collaboration among scientists, but rather cooperation among organizations, which allows each to specialize along the lines of work of greatest importance and interest to it.

In organizing their museum, Alexander and Grinnell faced additional issues as well. Grinnell favored locating the Museum on the Stanford campus, which had good physical facilities for both research and exhibition, first-rank zoologists, an excellent technical library, and a primary commitment to basic research. Alexander felt that Stanford was physically remote, and that the public university (where she had substantial ties) would better serve the purposes of her museum.[16] Grinnell was not disposed to contest the issue, and they decided to locate the museum on the Berkeley campus. Alexander and her lawyer then instructed President Benjamin Ide Wheeler on her wishes, and they were duly accommodated after the usual administrative delays.

The formalities observed, Alexander and Grinnell settled down to the difficulties of building and organizing collections. Grinnell contributed his own collections of mammals, birds, and eggs and planned regular collecting expeditions as part of the Museum's work. In a letter responding to Alexander's request for "a rough plan of campaign," Grinnell sketched the duties of his proposed staff:

14. Grinnell to Alexander, November 20, 1907, Grinnell Papers.
15. Barbour, *Naturalist's Scrapbook* (above, n. 6); S. G. Kohlstedt, "From Learned Society to Public Museum: The Boston Society of Natural History," in *The Organization of Knowledge in Modern America, 1860–1920*, ed. A. Oleson and J. Voss (Baltimore: Johns Hopkins University Press, 1979), pp. 386–408.
16. Grinnell to Alexander, February 2, 1907, Grinnell Papers. (Cf. Alexander to Grinnell, November 2, 1907, Alexander Papers.)

Duties of Officers

Director. – To promote the usefulness of the Museum along educational and scientific lines in every way practicable.

To inaugurate and maintain the most efficient systems of equipment, storage of material, & cataloging.

To have direct charge of the ornithological collections.

To publish, as time permits, the results of research in the ornithology of the West Coast of the United States, particularly of California.

To edit The Condor, making it a worthy exponent of Western ornithology.

To have immediate charge of, and be responsible for, ~~immediate~~ regular expenditures.

To map out the itinerary for field collectors and keep constant watch of their results, and to study out and maintain the most productive methods of carrying on field work.

To carry on field work in person at such times as other duties permit, collecting mammals, birds, and reptiles.

First Assistant. – To assume the duties of the Director at the Museum in case of absence of the latter, as when in the field.

To have direct charge of the mammalogical collections.

To publish results of study of West Coast mammals as time permits.

To engage in regular field work, at propitious seasons, collecting mammals, birds, and reptiles.

Second Assistant. – Largely field work, collecting mammal, birds, and reptiles; perhaps also working in the Museum at certain seasons when field-work is least advantageous.[17]

This plan, which was largely carried out in the establishment of the Museum, exhibits a second kind of joint effort: *stratification of authority*.

Alexander carefully reviewed the financial and administrative affairs of the Museum. A substantial amount of correspondence between Grinnell and Alexander concerns the justification of requests for funds to conduct fieldwork. At times, misunderstandings led to confusion. At the end of June 1909, for example, Alexander had the accountant from her family's firm audit the Museum's accounts. The accountant reported that certain expenditures were not carried on the Museum's books. Grinnell hastened to point out that the "missing" entries were for expenses that Alexander had reimbursed directly. The confusion was resolved by requiring Grinnell to submit a monthly itemized statement of expenses, which he did until his death in May 1939.

17. Grinnell to Alexander, November 2, 1907, Grinnell Papers.



## THE WORK OF COLLECTING

Donors rarely take a direct role in carrying out the research programs they endow, but Alexander was an active collector. "It was Miss Alexander's pleasure," wrote Hilda Grinnell, "in her desire to be useful in collecting plants and small mammals, to visit remote places in California in late fall and early winter, a time when campus duties kept most collectors occupied at home."[18] Although it was she who paid the piper, Alexander willingly subjected herself to Grinnell's authority in technical matters – indeed, this was one of the most striking things about their relationship over the years. She was willing to take technical direction not just from Grinnell but even from junior staff members, on such matters as suitable equipment and techniques of collecting, as well as the choice of her expeditions' itineraries and the kinds of specimens to be collected.

This relationship was established early on, even before Alexander formulated the notion of founding a museum. In an early letter to Grinnell regarding her 1907 trip to Alaska, she explained that she intended to collect bears, and went on:

> Through the agency of Indians [in Admiralty, Baranoff, and Chickapoff Islands, Alaska] I hope to secure the bulk of the bear specimens. This leaves time open for other work and I would be glad to collect birds as well as small mammals if I could have your promise to work up the material. Your advice as to just how to tackle a region would be valuable – as you know, the islands above mentioned are mountainous.[19]

A similar interaction with a junior member of the staff is evident in correspondence from 1932, when graduate student Seth Benson wrote to Alexander in the field concerning the collection of mammals in the Southwest:

> It is hard to say which lava field would be the best to visit.

18. H. W. Grinnell, *Annie Montague Alexander* (above, n. 1), pp. 18–19.
19. Alexander to Grinnell, February 2, 1907, Alexander Papers.

The best chance to obtain new things would probably be had on the lava flow east of the Elephant Butte Reservoir. An intermedius should be found there.

It would be worth while to obtain a series of Eutamia from the lava beds near Carrizzo [?]. They were quite numerous among the junipers at French's Ranch. I was only able to obtain one, but it was darker than those from Ricedero [?].

Dark woodrats of several species have been obtained in the lava beds of Valencia County. Goldman ~~secure~~ records dark albigula from San Rafael + Rio Puerco, N. mexicana fallus [?] from Copperton, Mt. Taylor, and Grant, N. stephensi relicta from Grant. Most of these localities are on or close to Highway 66 between Gallup and Albuquerque. Woodrats from any of these lava beds are desireable.

Any well marked lava bed, distinct from the surrounding country is likely to harbor a [*sic*] unknown race of some kind of mammal.

According to a report on the geology of New Mexico by Darton, "On the east side of the Pecos Valley in southern New Mexico there are very extensive sand hills formed of deposits known as the 'Mescalero Sands'" ——. You might find some interesting mammals there. We have no pocket mice, or anything else, for that matter, from that region.[20]

Here is a third kind of joint effort – *complementarity in the division of labor* between "professional" scientists and curators, on the one hand, and "amateur" collectors and hobbyists who provided specimens, information about potential sites for fieldwork, and every kind of assistance in the field.

This sort of complementarity in the division of labor is apparent in Museum correspondence and field notes in a way that it is not in the published work alone. Alexander, for example, published no scientific papers, although her collecting work was clearly central to the scientific research of a number of biologists (including C. Hart Merriam as well as Grinnell).[21] And while she was an expert collector, Alexander showed little interest in trying her hand at the sort of analysis that engaged Grinnell.

A second kind of complementarity in the division of labor is exemplified by the pair of papers published separately by Kellogg and Grinnell describing and analyzing the results of Kellogg and

20. Seth B. Benson to Alexander, July 26, 1932, Alexander Papers.

21. C. Hart Merriam, "The Museum of Vertebrate Zoology of the University of California," *Science, 40* (1914), 703–704.



Alexander's expeditions to the Trinity Mountains in Northern California.[22] Kellogg's paper is a workup of her field notes, giving descriptions of species and localities, including the description of a new species; it is of the same basic sort as much of Grinnell's own published field work. Grinnell's paper analyzes Kellogg's data (along with those of other museum staff) according to his theoretical scheme for faunal analysis. A brief description of that theory is necessary to understand the Museum's work.

## GRINNELL'S RESEARCH PROGRAM FOR THE MUSEUM

Grinnell envisioned the Museum as more than a "storehouse of facts." Early in his career, he developed a theory that related organisms and their environments, and his program for the Museum reflects that theory. Grinnell's theory had three closely related parts, which together formed an integrated vision of the relationships among species, environments, and evolution. One part, the faunal theory, was concerned with the classification of environments – that is, the grouping of biologically similar regions into categories. Another part was concerned with the dynamics of geographical distribution – that is, with how different species come to occupy specific geographical ranges, and the factors that set limits to those ranges. The third part drew implications from both the faunal and distributional models to form a model of the evolutionary process.

Grinnell's interest in faunal analysis stemmed from a model in which changes in the environment drive organic evolution by determining which species come into contact: "Environments are forever changing – slowly in units of recent time, perhaps. Yet with relative rapidity they circulate about over the surface of the earth, and the species occupying them are thrust or pushed about, herded as it were, hither and thither. If a given environment be changed suddenly its more specialized occupants disappear – species become extinct. . . . The course of organic evolution has been molded and is being molded by environmental circumstance."[23] Building on the work of C. Hart Merriam, Grinnell developed a classification of environments. Merriam classified "life zones" on the basis of average annual temperature; Grinnell extended Merriam's scheme by classifying "faunas" on the basis of humidity as well as

22. Louise Kellogg, "Siskiyou and Shasta Counties, California," *Univ. Calif. Pub. Zool., 12* (1916), 335–398; J. Grinnell, "An Analysis of the Vertebrate Fauna of the Trinity Region of Northern California," ibid., pp. 399–410.

23. Joseph Grinnell, "Geography and Evolution," *Ecology, 5* (1924), 225–229, at p. 153 of 1943 reprint.

temperature. He also recognized "associations" based on the kind of vegetation in a region.[24] The schema permitted Grinnell to study the relationship between environmental and species characteristics.

The second part of Grinnell's theory focused on the dynamics of geographical distribution in an evolutionary perspective. In 1904 he addressed this problem in a paper, "The Origin and Distribution of the Chestnut-Backed Chickadee."[25] The immediate motivation for the paper was the unusual distribution of the chestnut-backed chickadee, *Parus rufescens*, whose range covers 2000 miles from Sitka, Alaska, to forty miles south of Monterey, California, but is confined to a strip within a few miles of the coast. Grinnell used the bird's distribution to generate an abstract and general model of relationships among population structure, geographical distribution, and speciation.

Grinnell noted that each species has a center of population, the place where it enjoys the best environmental conditions for population growth (food supply, climate, and so on). Because of these favorable conditions, the center of population is also the place where the species shows the highest population density and intraspecific competition; as a result, there is continual migration away from the center, as individuals seek better conditions. This migration expands the geographical range of a species until environmental conditions become too extreme for it, or until it hits an impassable mechanical barrier:

> The geographical range of any species of animal may be likened to a reservoir of water in a canyon. The confining walls are of varying nature. A concrete dam, absolutely impervious, may retain the water at one end. Along either side the basin's walls differ in consistency from place to place. The substratum varies in porosity, at some points impervious like the dam, at others permitting seepage of water to a greater or less distance from the main volume. The water continually presses against its basin walls, as if seeking to enlarge its area. And it may succeed in escaping, by seepage through such portions of its barrier as are pervious or soluble, or by free flow through a gap in its walls, if such offers. The area occupied by the water will extend itself most rapidly along the lines of least resistance.
>
> Every species has a center or centers of abundance in which favouring conditions usually give rise to a rate of reproduction more than sufficient to keep the critical area stocked. A tendency to occupy a larger space results because of competition within the species, and individuals and descent-lines multiply and travel radially, extending those segments of the frontier where least resistance is offered. Such radial dispersion takes place slowly in some directions, more rapidly in others, according to the degree of passability of the opposing barriers. These barriers consist of any sort of conditions less favorable to the existence of the species than those in the center of the abundance.[26]

It is important to keep two points in mind here. First, Grinnell's focus was on the species vis-à-vis its environment. He was not concerned with the hereditary continuity of species or the problem of heritable variation; he simply took both continuity and variation for granted. Second, in Grinnell's model, environments as well as species change and move:

> Theoretically, sooner or later and in all directions, every species is absolutely stopped. But as a matter of undoubted fact most barriers are continually shifting, and the adaptability of the animals themselves may be also undergoing continual modification; so that perfect adjustment is beyond the limits of possibility so long as topography and climate keep changing. The ranges of species may thus be constantly shifting. Descent-lines may move about repeatedly over the same general region, like sparks in the soot on the back of a brick fireplace.[27]

Hence, neither species nor the boundaries of their ranges are fixed; distribution and speciation are wholly intertwined with one another. Grinnell's model tied population factors (size, rate of increase) to geographical distribution, the properties of environments, and speciation via natural selection.

Grinnell's evolutionary theory followed from his faunal and

24. C. H. Merriam, *Results of a Biological Survey of the San Francisco Mountain Region and Desert of the Little Colorado, Arizona*, North American Fauna, no. 3 (Washington, D.C.: U.S. Department of Agriculture, 1890). For a discussion of Merriam's work, see K. Sterling, *Last of the Naturalists: The Career of C. Hart Merriam* (New York: Arno Press, 1977). Grinnell's faunal theory was developed in his dissertation: Joseph Grinnell, "An Account of the Mammals and Birds of the Lower Colorado Valley, with Especial Reference to the Distributional Problems Presented," *Univ. Calif. Publ. Zool., 12* (1914), 51–294.

25. Joseph Grinnell, "The Origin and Distribution of the Chestnut-Backed Chickadee," *Auk, 21* (1904), 364–378.



26. Grinnell, "Lower Colorado Valley" (above, n. 24), pp. 107–108.

27. Ibid, p. 108.

distributional models and his commitment to Darwin's model of adaptation and speciation. The evolutionary theory can be summarized as follows: At the margins of a species' range, temperature and humidity differences, reduced crowding, different kinds and numbers of predators, and other environmental factors make for a different selective regime. This gives the advantage to a combination of characters different from those optimal at the center of population. Individuals with these characters have a reproductive advantage. Hence, subspecies adapted to different environments emerge on the extreme margins of a species' range. The areas settled by the emergent subspecies in turn become new centers of population. As the new subspecies expands its range, it comes into contact with the parent stock along the common border. Barriers "enforced" by selection maintain a clear border between the two stocks, with a zone of intergradation between the two. The border is a dynamic equilibrium: species push against one another, with neither able to extend very far into the other's territory, as barriers to migration act to isolate them from one another. A version of this notion later became known as the "competitive exclusion principle" – that is, very similar species do not inhabit the same region, because one will drive the other to (local) extinction.

## VARIETIES OF COLLABORATION AND THE MUSEUM'S RESEARCH PROGRAM

The Museum's theoretically motivated research program had practical consequences for the Museum's work on a day-to-day basis. One major consequence of Grinnell's theories was a great concern with the development and distribution of small differences among species and subspecies. This required the collection and comparison of many specimens of the same species in many different locations. The quality of these specimens, with their associated field notes on the state of the environment, was just as important to Grinnell as the total size or taxonomic scope of the collection. Hence, close supervision of every detail of the work was required. In 1910, Grinnell wrote:

> The museum curator only a few years since was satisfied to gather and arrange his research collections with very little reference to their source or to the conditions under which they were obtained. In fact it is surprising to find how little information is on record in regard to collections contained in certain eastern institutions as accessioned previous to about 1885. The modern method, and the one adopted and being carried out more and more in detail by our California museum, is to make the record of each individual acquired, whether it comes in from an outside donor or whether, as is the most usual case, it is secured by the trained museum collector, as complete a history as practicable.[28]



Retaining the comparative value of many similar specimens taken from multiple similar environments required the careful preservation of specimens. The theory also required that careful records be kept on the place and circumstances of capture, since knowledge of the environment from which specimens came was as important as the specimens themselves. Hence, the nature of the research problem imposed on the researchers a substantial burden of record keeping and record preservation. The original field notebooks of each expedition are kept today in the Museum's library, and are routinely consulted by researchers.

The records, collections, and technical practices required by the theories in turn posed certain organizational constraints on the Museum. We usually think of organizational specialization, complementarity, and stratification as "outside" the realm of research proper – as something that doesn't really have much to do with the important results of research. And that is perfectly true, when the research under way is routine "stamp collecting" of a kind that has been done many times before: cross-breeding to capture and map the latest interesting mutation, sequencing the nucleotides of another strand of DNA, or generating the next subatomic particle. In these cases, the arrangements for doing the work routinely have long since been made. But when new phenomena, instruments, concepts, and models are being opened up for exploration, then new arrangements for dealing with them reliably have to be made. For, it must be clear, anything that interrupts or facilitates the research is pertinent to the science – and thus arrangements for "buffering" the research from extraneous interference are part of the science too. In consequence, reliable means of anticipating the effects of university administrators, game wardens, and other difficulties are as consequential to the success of the research as procedures for (say) anticipating the effects of vibration or chemical impurities in a physical science experiment. In this view, segregation of function (both within and between organizations), stratification of authority, and complementarity in the technical division of labor are *the means by which this buffering is accomplished.*

28. Grinnell, "Uses and Methods" (above, n. 6), p. 35.

Of course, research is concerned with much more than minimizing the effects of contaminants, whether they be noxious chemicals or government inspectors and regulators. It also requires the routine coordination of different skills, possibly in new ways. In museum work, the core skills are collecting, preparing, and curating specimens. Museums are not simply warehouses; they are also dedicated to furthering the various branches of biology: recognizing and describing new species, analyzing geographical distributions, analyzing comparative morphology, and so on. Hence, all the other skills of the working scientist come into play as well. Collecting specimens requires a certain amount of biological knowledge, at least of a rough-and-ready sort – knowing, for example, what kinds of bait are likely to attract species of interest, where and how to set traps, and what sort of ammunition to use for which kinds of animals. It also requires knowing how to live off the land safely for days and weeks at a time, a skill that was common enough to be taken for granted in 1908 when Grinnell and Alexander were deciding how to staff their museum. Curating specimens, by contrast, requires skills more like those of the bibliographer or bookkeeper than those of the hunter and cowboy; scrupulous attention to repetitive detail is the sine qua non of adequate work.

This combination of skills was unusual, and Grinnell argued forcefully that the Museum needed its own specialists on staff; it should not rely on outside collectors, whether they were hobbyists, contractors, or commercial supply firms. Grinnell wished to make the Museum a "center of authority" on the West Coast, by which he meant *scientific* authority. Here, he was looking to the Smithsonian and the great museums of Europe as a model. He wrote to Alexander:

> 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 in founding the institution. I will grant that it would take our man, whoever he may be, longer to work up the paper, than the B. S. [U.S. Biological Survey] people. But in the former case we would be ever so much the stronger and better able to tackle the next problem.[29]

29. Grinnell to Alexander, November 14, 1907, Grinnell Papers. Cf. the similar statement published in Grinnell, "Uses and Methods."



Here, Grinnell is arguing against segregation and complementarity of function in the routine technical work of the Museum, because the coordination of needed skills is so delicate a job that the work of describing and analyzing specimens can be done by only one person at a time. Which is to say, that the organization of research is not a problem of specialization per se, but specialization in ways that encourage and support the best-quality technical work. The different modes of collaboration thus have complementary requirements: exclude the extraneous influences *and* conjoin the needed techniques. It is this combination of included skills and excluded extraneous influences that is needed to establish a "center of authority."

Since the Museum was located in a university, relationships with other parts of campus were important. At first, Alexander insisted that the Museum operate independently of the Zoology Department. Grinnell's initial appointment did not carry faculty status, and his salary was paid from Alexander's annual donations. Hence, Alexander was irritated when Grinnell was asked to do some teaching:

> The more I think of it, the more I am surprised that you should have entertained for a moment Prof. Ritter's proposition that you undertake his Freshman classes. all this talk about the "New Zoology" and what it is going to accomplish for the student is hypothetical for the most part. Prof. Ritter knows this and shrinks from making the experiment himself.
>
> But after all the real truth of the matter is this: He goes to San Diego to follow up some pet research work while you undertake the drudgery of his class work. The responsibility is shifted, and the University gets a teacher for nothing. Your fresh vigor and enthusiasm might secure a few more recruits than ordinary for special work in Zoology, but freshmen are not going to study if they can help it and real students are rare. Teaching is thankless work. I am glad to see the Museum useful to the University – it's bound to be, without forcing matters. My ambition is to see it win a reputation all its own as a research center of the Pacific Coast. I think you agreed with me in this when we first inaugurated the work. It was for this purpose that I held out so persistently for our independence of the University, and I hate to see you put your hands out to be shackled the very first thing by selfish men who work on your enthusiasm and pride under cover of a grand crusade for the New Zoology. New Zoology can be taught through the very research work, the opportunities for which you were just thinking of

throwing away! And a reputation earned for the cause that will make the University proud of its Museum.[30]

After he received his Ph.D. in 1913, Grinnell was appointed to the faculty and took on the usual teaching duties. Alexander's sense that Grinnell would attract students was correct, but this required careful handling. On the one hand, students are untrained amateurs who require enormous amounts of time and attention. On the other hand, they are the most enthusiastic, devoted, and creative workers in the field. Grinnell developed procedures that brought students fully into the work of the Museum, and simultaneously trained them in all the different skills that the Museum required. Parts of the program were formulated as rigid exercises; for example, the procedures for taking field notes, which specified the precise headings, margins, format, and other details of the notebook page, even to the brand of ink used.[31] Other parts of the program were organized as "on the job" apprenticeship, with students learning from the Museum staff and one another in the context of a personal relationship.

The system that Grinnell and Alexander established worked well. A remarkably complete picture of the day-to-day work of the Museum can be traced through the correspondence files, due to the fact that field expeditions were limited largely to California, and that, in the second decade of this century, first class mail was routinely delivered on the next day, so that the "turn-around time" for communication between the Museum and the field was comparable to today's electronic mail. The Museum staff took good advantage of this, coordinating the details of collection, preparation, and administrative management very closely, advising one another on tactics and tricks of the trade, and resolving almost every kind of difficulty as it came up.

There is a clear pattern in this correspondence: the work, both in the museum and in the field, was organized in a very "flat" hierarchy, with almost no distinction as to organizational rank. Students and professors alike did "scut work." Everyone did many of the same tasks, offered technical suggestions, and learned from the others – although, of course, Grinnell was clearly first among

30. Alexander to Grinnell, February 28, 1909, Grinnell Papers.

31. J. Grinnell, "Suggestions as to Collecting," mimeographed handout, revised by Alden H. Miller, July 2, 1942, Grinnell Papers, 73–25c. See also the manual published by former MVZ curator E. R. Hall, *Collecting and Preparing Study Specimens of Vertebrates* (Lawrence, Kan.: University of Kansas Press, 1962); and S. Herman, *The Naturalist's Field Journal: A Manual of Instruction Based on a System Established by Joseph Grinnell* (Vermillion, S.D.: Buteo Books, 1986).



equals, and he edited many of the publications going out under other authors' names. The relatively flat organizational hierarchy extended even to publishing: as already noted, Alexander's companion, Louise Kellogg, published the field report of her 1911 expedition to the Trinity Mountains with Alexander under her own name; the Museum's secretary, Margaret Wythe, coauthored publications with Grinnell; and students, such as Walter P. Taylor, were able to make major theoretical contributions in their theses.[32]

## CONCLUSIONS: SCIENTIFIC COLLABORATION IN THE MUSEUM

Grinnell's theoretical models thus organized the work of the Museum; indeed, the Museum as an organization and set of collections must be seen as a model of the environment, capturing and recording changes in nature over decades and centuries.[33] In consequence, Grinnell's museum was to be a sensitive instrument for measuring evolution. The time scale of evolution is such that the museum instrument would need to operate far beyond Grinnell's own career and lifetime:

> It will be observed, then, that our efforts are not merely to accumulate as great a mass of animal remains as possible. On the contrary, we are expending even more time than would be required for the collection of the specimens alone, in rendering what we do obtain as permanently valuable as we know how, to the ecologist as well as the systematist. It is quite probable that the facts of distribution, life history, and economic status may finally prove to be of more far-reaching value, than whatever information is obtainable exclusively from the specimens themselves.
>
> At this point I wish to emphasize what I believe will ultimately prove to be the greatest value of our museum. This value will not, however, be realized until the lapse of many years, possibly a century, assuming that our material is safely preserved. And this is that the student of the future will have access to

32. Kellogg, "Siskiyou and Shasta Counties" (above, n. 22); J. Grinnell and M. Wythe, "Directory to the Bird Life of the San Francisco Bay Region," *Pacif. Coast Avif. 18* (1927); W. P. Taylor, "The Status of the Beavers of Western North America, with a Consideration of the Factors in their Speciation," *Univ. Calif. Publ. Zool., 12* (1916) 413–495.

33. J. Griesemer, "Modeling in the Museum: On the Role of Remnant Models in the Work of Joseph Grinnell," *Biol. Phil., 5* (1990), 3–36.

the original record of faunal conditions in California and the west wherever we now work.[34]

The Museum was designed as an institution for conducting "big" science – work that involves coordinating many people and substantial resources for long periods of time. This plan necessitated treating the organizational work, including the various collaborations, as integral to the research, since the one could not occur without the other.

How we interpret collaboration and such allied notions as cooperation and division of labor depends on how we view science. Is scientific research stratified, with scientists at the top of some kind of social hierarchy, making social relations with and among others peripheral? Or is science better thought of as a complex, possibly nonhierarchical, network in which a multiplicity of collaborations result in a heterogeneity of joint productions and products, some of which we now recognize as canonically "scientific"?

Clearly, various kinds of collaboration are involved in the museum work described here, but trying to locate this work between the two polar conceptions of social structure raises issues about museum science. First, are the collections themselves properly thought of as products of *science*? If not, then the collaborations involved in their production are irrelevant to our understanding of *scientific* research. The first of the two polar views suggests we should look to the published results of research on museum materials to understand what is scientific about this kind of natural history research. But if collections *are* products of science – perhaps as snapshot representations of what the environment is doing to species at each moment, with the collective representation over time embodying a theory of the causes of distribution and evolution – then all the kinds of collaboration we have discussed, and more, are directly relevant to understanding scientific research as practiced in the Museum of Vertebrate Zoology.

Although one case study does not adequately support generalization, several facts argue in favor of a broad view of collaboration in science. We have suggested that a coherent interpretation of what went on in the Museum of Vertebrate Zoology incorporates a view of collecting work as scientific research serving identifiable theoretical goals. Since the collecting work is part of a larger context of organizational collaboration that would not have served the theoretical goals outside such a context, it is important to think

34. Grinnel, "Uses and Methods" (above, n. 6), pp. 34–35.



of organizational collaborations – specialization, complementarity, and stratification – as "inside" the realm of scientific research. And once inside, it becomes an important task for historians to chart their course and influence.

*Acknowledgments*

This is a revised version of a paper read at the meetings of the History of Science Society in Seattle, October 1990. We are grateful to Barbara Stein for much helpful advice, and to the Museum of Vertebrate Zoology at Berkeley for permission to use the Museum's archives. We are also grateful to John Beatty and Jane Maienschein for comments on previous versions, and to M. Sue Gerson for continuing support of Gerson. Griesemer thanks the UC Davis Committee on Research and the Office of the Dean of the College of Letters and Science for support.