**Exploring Nature**

**Geographies of Science’s History**

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When people tell the stories of the Scientific Revolution, they often use the language of travel and exploration. We describe the voyages of discovery that figures such as Galileo and Harvey took as they encountered novelties in inner and outer space, we speak of the quest to understand planetary movement begun by Copernicus and completed triumphantly by Newton, and we discuss the difficulties encountered on the journeys away from superstition and the pursuit of a reasoned way of defining and exploring the natural world. These descriptions could be dismissed simply as colourful metaphors – but they could, of course, also be considered literally. The era commonly associated with the period of the Scientific Revolution is also the dawn of the European age of exploration and discovery: from the mid 16th century to the end of the 18th century, nascent nation-states and houses of commerce began and intensified programmes of internal and external exploration. Voyagers brought back specimens of unfamiliar flora and fauna, drawings and descriptions of strange peoples and landscapes, all of which needed to be accounted for and to find their places in the European world view. Travellers surveyed the (sometimes even stranger) peoples, practices and landscapes that were to be found closer to home, and strove to tie them more tightly to urban centres and institutions as part of the practices of nation-building. And by the end of the 18th century, the sciences that were to become known as geology, botany, zoology, ecology, ethnology, archaeology, meteorology, cartography, hydrology, oceanography, ichthyology, amongst others, were beginning to emerge from the catch-all discipline of natural history.

But at the same time, the idea of the laboratory was fast-becoming the iconic image of science, and the experiment its defining practice. Even today, when most people think about science, the pictures that come to mind are ones of test-tubes, lab tables, white coats and (possibly) caged rats, all located within the enclosed, indoor space of the laboratory. When surveys of scientific literacy ask how one should do science, correct answers tend to include reference to hypotheses, variables and experimental trials. For field sciences, the dominance of this particular notion of manipulative practice as the emblematic essence of ‘science’ can be a problem – because the field sciences, by their nature, are very different. Most obviously, they were and are dependent on work carried on outdoors, in the open air, often far from the urban centres where the laboratories and other institutions of science are usually found. Frequently, these places were and are hard to get to (either physically or politically) and make strenuous corporeal demands (both emotionally and materially) on scientists working there. In almost every respect, the characteristics of these places could not be more different from those associated with those of the laboratory.

Consider these points: where laboratories were designed to be spaces for the experimental manipulation of the natural world, more often than not, it was the techniques of observational recording and measurement that tended to be privileged in field sites and spaces. Where laboratories were valued for their uniformity and similarity to each other, field sites were valued for their individual variability and unpredictability. While laboratory workers were trying to translate some aspect of the natural world into a form that could be scrutinized, displayed and made to run through its paces at human convenience, field workers were concerned to look at ‘nature’ under ‘natural’ conditions, seeking to understand the interrelations between organism and environment, to find ways of reading and recording the landscape, and to establish means of tying information from this incredible variety of sites and scenes together into authoritative laws of nature. The differences between the two sites should not be over-stressed – but undoubtedly, doing science successfully in either space required the researcher to develop and deploy a very different set of skills. But the real problem lay in the fact that the laboratory was privileged as the iconic scientific space. This meant that results from the field were often treated with suspicion: what warrant was there for believing that they represented an accurate account of some aspect of the natural world? This was exacerbated by the fact that – again in contrast to the laboratory – field workers were not in complete control of the place in which they were trying to do research.

At a very basic level, the laboratory is an indoor space, meaning that access to the lab can be restricted by something as simple as a locked door or cabinet; its population and conditions are regulated by its researchers, who are normally operating under commonly held scientific conventions. The field, on the other hand, does not have walls, and this means that the population of the site is far less susceptible to scientific governance. Not only might the phenomena under investigation decline to manifest or choose to migrate, but attempts to study it/them could be actively disrupted by the presence at the site of other (non-scientific) humans. All field workers have to deal with the presence at ‘their’ site of other residents: game wardens, or national park managers, hunters, farmers, fishermen, loggers, tourists, agents of local or national governments, soldiers, sailors or eco-warriors. These represent a bewildering variety of different tribes, each with their own agenda for the use of the place in which the scientist/s want to work. On the positive side, however, they also bring with them an equally wide variety of practices for accessing and extracting the resources of the land, sea, and air, a pool of potential skill-sets on which scientists have eagerly drawn. Field workers have adopted and adapted the strategies and technologies originally developed for divers, mountaineers, balloonists, film-makers, surveyors, arctic explorers – and sometimes, themselves perfecting techniques that fed back into the habits of other professions or pursuits.

As a result, looking at the history of field science can require orientation to a rather different set of concerns than those normally associated with the study of science indoors. Historians of science have long been concerned with the places where science gets done – but studying field science means putting the variability and volatility of these places, and the demands that working in them makes on human bodies, at the forefront of understanding the process of knowledge production there. And the story of field science does not stop at knowledge production. Understanding field science also requires an abiding attention to the circulation of knowledge, and the uses to which knowledge is being put. Not only are field scientists not in control of events at their sites and dependent for access to those sites on successful negotiations with potential funding bodies and the owners and inhabitants of these spaces, but they are also not in control of the way in which field-based knowledge is put to work. This is particularly problematic when most people’s understanding of ‘how science works’ is based on a particular – and even then, not entirely accurate – picture of laboratory science, where experiments can be done and hypotheses can be directly tested. Field science – being concerned with observation, rather than manipulation – is not susceptible to such apparently clear-cut checks, and as a result, can appear far more uncertain, and sometimes, far more vulnerable to political or ideological interpretation, than its practitioners would wish. When dealing with field science, as this chapter will show, one is dealing not just with the history or the geography of science, but also with the profound political, economic and social consequences of such work.

***Section 1 Why do field science?***

In the first place then, if the field is often so hard to get to and work in, why go there at all? If field sites are such uncertain places, replete with variability and unpredictability, then how were scientists to make authoritative knowledge claims based on their experiences in such ambiguous locations? The answers to these questions depended on the nature of the work that needed to be done. Scientists, travellers, natural philosophers all went to the field because it enabled them to do work under conditions that either could not be obtained, or could not be accurately replicated, in the laboratory. There are many different sites at which field work was and is carried out: on mountains, moors or the decks of ships; in forests, mines or space; at the seaside or specially constructed field stations – the list can be as long as that of the different environments which exist on, in and under Earth. And despite the clear differences between field and lab, it should be remembered that they exist only as categories at either end of a continuum of scientific spaces, between which scientists themselves travel. Researchers who spend the summer vacation working on the savannah will return in the autumn to teach undergraduates and subject their gathered samples to laboratory analysis. But it is useful to consider how different kinds of spaces are linked with, or perhaps produce, different kinds of approaches to the study of the natural world – and different kinds of responses to the knowledge produced there.

*Categorising field science*

Very broadly, historians have identified four different categories of field science: survey science, historical/observational science, salvage science and, for want of a better word, ‘extreme’ science. In the first part of this chapter, we will look at how these have developed over the course of the last two centuries.

Survey science – the kind of work first associated with the exploratory voyages and expeditions of the late 18th and early 19th centuries, and with which we might now link cartography, meteorology or biogeography – was the Big Science of its day. It was most closely linked with the work of the German explorer-naturalists Alexander von Humboldt (1769-1859), and is, in fact, often described by historians as ‘Humboldtian science’. It was characterised by absolute attention to meticulous measurement, as travellers and explorers crossed sea and land, accompanied by multiple versions of scientific instruments such as thermometers, magnetic compasses, scales, barometers. In Britain alone, dedicated voyages of scientific discovery, such as that of *HMS Beagle* (1831) or *HMS Challenger* (1858), and overland expeditions such as those sponsored by the African Association, the Geographical Society of London, or the Royal Society were sent out: all recorded information in situ so as to apply it on a global scale. There was nothing new about surveys, of course – people like John Ray (1627-1705) or Edward Llwyd (1660-1709) had travelled through North-West Europe in the late 17th century in order to record botanical, geological and linguistic details about the different lands and their peoples. What made the situation different a hundred years later was the turn to instrumentation and the drive for accuracy in measurement. The acceptability of observations as authoritative depended, amongst other things, on their repetition, often using many different versions of the same instrument, so as to minimise error and maximise range. It was no longer sufficient simply to describe what was seen on a journey: instead, travels had to be quantitatively recorded. What’s interesting here is that notions of measurement and standardisation could be held to apply to the field just as they did to the laboratory – as long, that is, as the person doing the work could be trusted. After Humboldt, the notion of the ‘scientific traveller’ had been created, constantly on the move, harvesting information as ‘he’ went, in order to establish universal laws of nature through the combination and culmination of observations made in particular places

In contrast, the observational/historical field sciences such as botany, ethology, geology or palaeontology, involved and involve spending more time in the study of particular places. The influence of the laboratory can be seen here in the *selection* of these sites – one might, for example, choose to work in a place where fewer (potentially) confounding variables might be present, as a result of altitude, isolation, or the ways in which local people used the area. Alternatively, one might seek to work at a range of sites, chosen so as to complement each other, with variations in one site being accounted for by constancy elsewhere. In this way, one might find in comparisons between these sites the functional equivalent of a series of lab-based experiments. As with survey work, the need for exactitude in sampling, observing and recording was a key theme, along with the desire to be able to extrapolate from the close examination of the constellation of characteristics in specific locations to other areas of the natural world. Geology, in particular, relied on this, as people like Adam Sedgwick (1785-1873), Henry de la Beche (1796-1855), Roderick Murchison (1792-1871), as well as the young Charles Darwin, used intensive study of particular places to predict the sequence of rock formations elsewhere, while Abraham Werner (1749-1817) taught students how to identify valuable deposits from external physical characteristics – a reminder that mining was as significant as theology to geology’s history. Being able to identify the location of coal-bearing strata or ground-water reserves, for example, was to prove central to the institutional independence of geology – and depended on extended fieldwork in the areas in question, whether these were the Welsh Marches or the Ural Mountains. Paleoanthropological knowledge, too, could be utilised in this way, as with the Leakeys’ efforts to use their knowledge of the fossil history of East Africa to identify potential diamond deposits in Angola.

For the botanical and zoological field sciences, increasingly the impetus for field work came from the need to examine organisms within their natural environment. By the early 19th century, for example, debates on the nature of evolution between figures such as the Comte de Buffon (1707-1788), Jean-Baptiste Lamark (1744-1829) and Georges Cuvier (1769-1832) which turned on the relationship between function, form and environment, encouraged naturalists to examine animals and plants in situ, whether that be in gardens, woods, moors or sea-shore. Later on, the work of Humboldt (1769-1859), Darwin (1809-1882) and Joseph Hooker (1817-1911) on the reasons for and barriers to the geographical distribution of organisms was also emphasising the importance of context and the tangle of relationships in which individual entities existed. Studying these kinds of questions could only be done in the field. By the 20th century, such work was both benefiting from, and sometimes coming to require, a long-term commitment to the study of a particular place. In these cases, doing field work successfully meant making a virtue of the singularity, the uniqueness of that specific space, in a way that turned the field site itself into a crucial scientific tool.

For lab science, the context in which scientific work was done was relatively irrelevant: the uniformity of lab space across space and time was meant to minimise the potential impact on that work of uncontrolled variables. For the field, context was essential to understanding the meaning of the work done there. In some cases, it was the detailed description of the particularities of a given field site that could be used to lend authority to the work that was being carried out there – and by the 20th century, this was especially the case in relation to ecological and ethological studies. At the most basic level, the inclusion of such detail provided reassurance to the reader that one had actually been to the place in question. But far more importantly, and especially in relation to sites where work had been carried out for a long time, the accumulation of quantities of detail meant that there could be a sea-change in both the quality of the data and the understanding of phenomena. Once a site had a recorded history – where variables such as rainfall, plant cover, species counts and so on had been recorded for an appreciable period of time – that data then provided the constant backdrop against which the significance of current events could be judged. For ethology/zoology in particular, prolonged work in a specific place was important for two reasons. First, long exposure meant the animals under observation would become used to human presence, and would therefore behave more naturally – and it was natural behaviour that the observers had come to see. But it also meant that scientists could identify and recognise individuals, and thus come to record the animals’ own histories. This was important, because knowing, for example, that what one was seeing was not an embrace between two sexually mature monkeys, but between a mother and her adult son, could make a major difference to the interpretation and significance of their behaviour. In both cases, the longer studies had been carried out in a particular place, the more valuable and authoritative such observations became.

Yet another category of field science, however, relates to observations that are valuable because of their fragility, rather than their longevity, usually because of the activities of other humans. Although many examples of botanical, ecological and even geological field science can fall into this area, perhaps the disciplines of archaeology and anthropology present the best examples. This is due, of course, to the extent of global political and economic development during the course of the 19th and 20th centuries, which wrought tremendous changes in human societies and their relationship with each other and with the land. Anthropologists, for example, found themselves in a position where they were studying ways of life that were in the process of deliberate or inadvertent eradication: archaeologists were confronted with a situation in which the physical integrity – such as it was – of their subjects of study were threatened by agricultural and industrial development. In both cases, information had to be salvaged before human activities damaged or altered it beyond recognition, although – ironically – it was often these potentially destructive human activities that permitted the scientists to gain access to the information in the first place. The same networks of ships, railways and political institutions that were marginalising traditional ways of life were the routes that anthropologists used to access the field. Archaeologists and paleoanthropologists found that mines, quarries, and construction sites could become very useful places to search for fossils and artefacts, as long as the companies involved were willing to accommodate scholars: indeed, some quarry owners discovered that archaeological interest in their diggings meant that another extractive industry could be tapped, as tourists came to see their results. As such, salvage science is a good – but by no means singular – example of the ways in which the history and practice of the field sciences were thoroughly embrangled with commercial and industrial development

A final category of field science could be characterised as ‘extreme’ science: fieldwork done under conditions of great hardship, or in settings exceptionally inimical to human life – but which were also, ironically enough, often linked with popular leisure-time activities, sometimes making it hard to see if scientists were at work or play in these areas. Such environments included those of mountain, marine, polar and space science, where altitude, element or temperature made it hard for human beings to survive without technological assistance, much less conduct scientific research. Mountains, for example, became places to do science from the mid-18th century on. Sometimes described as ‘laboratories of nature’, they were places where the influence of altitude and pressure on physical variables and biological variation could be studied, astronomical observations made, and the impact of the mountain itself on both global and local climate – as well as the body of the observer – could be considered. Additionally, they were increasingly fashionable places to see and be seen, as upper- and middle-class citizens discovered the pleasures of mountain climbing and sketching. In a similar manner, the oceans had increasingly become the subject of scrutiny by the beginning of the 19th century. A number of economic and cultural factors combined to account for this, including such developments as the expansion in the size and range of national fishing fleets supported by improvements to ship and harbour design in the late 18th century, as well as the laying of the trans-Atlantic cable lines in the 19th century. Additional encouragement, as with the mountains, came with the discovery by the middle-classes of Europe and America, that the sea-side was a pleasant, uplifting and potentially stylish place to be. By the 20th century, undersea exploration had taken a central place in marine science, especially after Jacques Cousteau and Emile Gagnan developed the aqualung in the 1940s – which in turn was taken up as an increasingly popular leisure activity. In contrast, neither the poles nor space were available for exploitation by the casual visitor, but as with the survey science of the 19th century, work in these areas was facilitated by the interests of nation states and commercial operators, and supported by the avid and abiding public interest in consuming the accounts of the triumphs and disasters of the polar and space expeditions.

*Embodying field science*

What the cases of both salvage and extreme science make clear, however, is the extent to which doing science in the field was dependent on the body of the fieldworker. When people think of science, they often think of it as an overwhelmingly intellectual activity: in contrast, looking at field science shows how important the body and its physical and emotional resilience or adaptability was to investigating nature. This was the case on a number of levels, ranging from managing to remain physically active in a stressful environment to training one’s eyes and hands to record accurately what one saw.

Before they could go about the business of studying what they had come to see, fieldworkers had to habituate themselves to a novel, and usually unfamiliar environment. This was not just related to the fact that fieldwork required movement through space, meaning bodily strength and mastery was a key element in successful fieldwork – but just as importantly, also connected to the use of the body itself as a scientific tool. Fieldworkers often had to be extremely creative in developing technologies to extend their physical access to different areas of the natural world, in process that wasn’t just physical, but often involved both imagination and emotion as crucial resources. In the case of mountain science, for example, the 18th century ‘discovery’ of the Alps for science also involved their aesthetic discovery: climbing became a literally uplifting activity, both morally and physically. Various expeditions to Mount Everest, for example, cited not only the geological and cartographical advantages that an attempt at ascending the mountain would accrue (not to mention the adventure, or the kudos, that awaited the first team to reach the summit), but also the spiritual benefits of such an effort. In the case of the 1922 expedition, it may be that this element was deliberately over-emphasised as part of a strategy to gain the approval the lama of Rongbuk monastery; however, it is certainly the case that members of later expeditions described their efforts as a pilgrimage. Imagination and embodiment also had a role to play in the case of anthropology. Trained as an experimental psychologist, but based on his experiences on the Cambridge Torres Strait Expedition (1898), W H R Rivers (1864-1922) argued that one must share the life of the anthropological subject, doing as they did in order to understand from the inside out how they lived their lives, before one could go about analysing their social structures.

More generally, fieldworkers had to train themselves to observe and record in the field: not just what to record, but how to observe it in the first place. Geological students like the young Darwin had to learn to walk the land and to distinguish meaningful observations from accidental deposits: it was necessary to learn that the discovery of a tropical shell in a Shrewsbury gravel pit did not mean that all previous interpretations of the geological history of the area needed to be jettisoned. The significance of this process of learning to see can be judged from the emphasis that was consistently placed by learned societies and individuals on the necessity of teaching people how to do it. From the *Directions for Sea-men, Bound for Far Voyages*, published by the Royal Society in the 1660s, through the various editions of the *Hints to Travellers* (first published by the Royal Geographical Society from 1854), and the manuals of inquiry put out by various branches of the British Association for the Advancement of Science, scholars regularly tried to advise and instruct travellers on what their eyes should look for and their hands should record. The log- and sketch-book were key tools of knowledge, and for naval surveyors like John Roe (1797-1878), their most precious possessions often included treatments for eyestrain brought on by the effort to record as accurately as possibly what they saw. Learning to use one’s body effectively in the field was a matter of constant practice: Frank Fraser Darling (1903-1979) bemoaned the fact that only a few weeks absence from his Highland field-site damaged his skill at recognising deer against a hillside. And sometimes bodily skill was only noticed in its absence: only when suffering from a heavy head-cold did Darling realised that he had been using scent and taste as much as sight and sound to track the deer.

Some kinds of fieldwork were, of course, more stressful than others, both physically (in terms of acclimatising to new environments) and emotionally (in terms of getting equipment and research strategies to work in that environment). For those maritime observers new to the sea, learning how to cope with sea-sickness and accustoming oneself to the physical challenges of life aboard ship were fundamental to social and scientific success on the voyage. Altitude sickness could be an issue for those conducting mountain-based research (as could simply taking photographs in the era of collodion technology), malaria and other tropical diseases were constant threats to those working at lower altitudes and closer to the equator, and just doing science at sea, underwater and in the air usually required more extensive technological interventions and support. But all fieldworkers faced less tangible threats to their survival as scientists, threats that emerged from the particular nature of field-based research, and which centred around the question of scientific *identity*. Their very status as scientists was a matter that was often rendered equivocal, usually as a direct result of the hybrid and ambiguous nature of the places in which they worked, and the social groups with which they shared that space.

We have seen the ways in which various political, economic and national interests made it possible for field scientists to access the subjects they wanted to study, and the role of culture in making certain aspects of field science fashionable and popular. We will now go on to look rather more closely at the political economy of field research and at the different cultures and societies that helped to structure the spaces in which scientific work was carried out.

***Section 2 The political economy of field science***

Doing field science with a global reach was expensive: what this meant was that those interested in studying such matters had to find ways of enlisting the support of other people and institutions in the funding of this research. To put it crudely, the political and economic benefits that would or could accrue from scientific research had to be made clear. But the trouble was that the interest that other groups (politicians, civil servants, soldiers, sailors, entrepreneurs, financiers) had in these matters might not be wholly scientific – and this had consequences for when and what kind of research was done, as well as who carried it out. We can see this if we look at the kind of science that was done at sea.

*Magnetic measurements and the maritime economy*

At the beginning of the 19th century, for the military, the seas and oceans were largely just hostile environments over which they travelled and on which they fought. By the mid-20th century, however, changes in the nature of international warfare had turned the sea floor into a theatre of conflict, and focused military attention on the constitution and structure of the oceans themselves. Science and scientists had key roles to play in this shift – but this was not a simple process of mutual symbiosis. Those with an interest in pursuing scientific problems – establishing the shape of the earth, for example, mapping the ocean floor or searching for evidence of the giant squid’s existence – had to enlist the support of others in their endeavours, to seek the political or economic backing that would lead to financial support for their studies. But especially during the 19th century, the period during which science was becoming a professional activity, it was not always clear who could best be relied on to identify and solve the scientific questions that seemed to present themselves.

For example, interest in pendulum experiments which could determine the shape of the earth had intensified in the years following the Congress of Vienna (1815). Captain Henry Kater (1777-1835) had published a series of papers on pendulum design and observations in the Royal Society’s *Philosophical Transactions*, which closed by calling for a series of globally comparable pendulum measurements that needed to be taken from geographically distant locations. This was a project that required not just national, but international support and cooperation – but fortunately, it was also a problem that could help solve immediate questions of national prestige and military practicality. As more and more ships were made of iron, magnetic studies and means of improving navigation became more and more interesting to the world’s navies. Additionally, in the post-Napoleonic era, voyages of survey and exploration would give at least some of the suddenly under-employed British navy something to do: no longer required to fight the French, being assigned to such a journey was one way of keeping one’s job and even successfully pursuing promotion.

Commander John Ross (1777-1856)’s initial voyage in search of the North West passage (1818-19) was the first to carry out extensive pendulum work, primarily conducted by Edward Sabine (1788-1883). Sabine, educated at the Royal Military Academy and an officer in the Royal Artillery, was the expedition’s astronomer, having been personally trained by Kater in operating the new kind of pendulum. In the years that followed, observations from the ships searching for the North West passage – a potentially highly lucrative trade route, and for which discovery the Admiralty offered prizes – were an important, although not sufficient, source for the accumulation of pendulum observations. By the 1830s, then, Sabine was at the forefront of a campaign to establish a global network of geomagnetic observatories under British leadership. The second secretary of the Admiralty, John Barrow (1764-1848), who had been a key figure in promoting both the search for the North West passage and in African expeditionary ventures, had also sponsored and facilitated several more voyages of Arctic exploration. For both men, however, the issue of who was qualified to make accurate observations about the natural world remained a problem, and it was one that was tied to the broader relationships between science, exploration and commerce in the period.

Sabine became the personal target of criticism from Charles Babbage, inventor of the Difference Engine, who was then in the midst of a quarrel with the Royal Society, which had failed to appoint him as junior secretary. In his *Reflections on the Decline of Science in England* (1830), Babbage argued that most of the Society’s Fellows were elected for their social, rather than scientific, qualifications – and attacked, in particular, Sabine’s appointment as scientific advisor to the Admiralty. His condemnation was based on his contention that Sabine was “an officer of artillery on leave of absence from his regiment”: a military man, not a scientist, whose observations were not to be trusted. Babbage had looked closely at Sabine’s recorded measurements and results from the Ross voyage, and concluded that they were uncannily and suspiciously in agreement. They must, he argued, be the product of an amateur observer who was not even competent to fake observations convincingly – indeed, an entire section of the polemic was devoted to the ‘Frauds of Observers’ who wish to be considered as accurate witnesses for science. This use of Sabine’s military identity to pre-empt his scientific credibility was part of Babbage’s wider concern with the social and institutional status of science – part of his wider attack on the state of the Royal Society and its relationship with the Admiralty. But the issue of identity and the proper qualifications for observation was also one that troubled the Admiralty Secretary John Barrow.

In Barrow’s case, the question was whether anyone *other* than a member of the Royal Navy was qualified to carry out observations and measurements on the ocean. He felt very strongly that exploration and survey work was the prerogative of the Navy, despite the fact that it was whaling ships, in search of economic profit, that were returning with vitally important news and observations from the far North. In fact, it was the reports of whaling ships that had first suggested that Arctic ice might be sufficiently fragmented to permit the resumption of the search for the North West passage. Barrow was particularly hostile to one William Scoresby Jr. (1789-1857), the son of a Whitby whaler. Scoresby, a correspondent of Joseph Banks, had made extensive charts of Greenland’s coasts, as well as many observations of polar ice, sea temperatures and marine life as it related to the northern whale fisheries – but his request for government finance for a voyage of discovery under his command was rejected at Barrow’s behest. Arctic observations and explorations were not to be trusted to a commercial operator: nothing daunted, Scoresby borrowed against the voyage’s profits and went ahead anyway. The venture was a success, with Scoresby reaching further north than any previous sailor –but Barrow’s hostility to the ‘mere whaler’ meant that ships were soon despatched to sail a few degrees further along Greenland’s coast, potentially overshadowing the geographic and biological discoveries that Scoresby had made.

What both these examples show is the extent to which economic, military and scientific identities and interests were entangled during this period of survey and exploration, when the sciences themselves were in the process of disciplinary formation and professionalization. Over a hundred years later, the story of sub-surface exploration illustrates the continuing close relationship between science, national interest, the military and commercial operations.

*Science and the sub-marine*

By the 20th century, war was being carried on in, not just on, the water. The increasing use of submarines both during and after World War 1 meant that it became progressively more important to understand the properties of the sea itself, as a medium that things travelled through, as well as over. Naval officers needed to be able to draw on expertise in order to understand the behaviour of waves at the sea-shore, to appreciate the military applications of thermoclines and the correct operation of bathythermographs, to understand underwater acoustics, whether produced by submarines or cetaceans, as well as (in peace time) being able to identify the species of fish that fishing boats were permitted to harvest. But this interest in, and support for, oceanography and maritime studies did not come without potential costs both at the individual and the institutional level, particularly in relation to the Navy’s approach to basic, as opposed to applied, research and the role of national security.

In the United States, for example, two key oceanographic institutions had been established on either coast by the early 1930s – the Scripps Institution of Oceanography, founded in 1903 in La Jolla, California, and Woods Hole Oceanographic Institution in Massachusetts, founded in 1930. It was Athelstan Sphilhaus of Woods Hole that invented the bathythermograph in 1938. This instrument was a means by which the reliability of sonar could be estimated, since the movement of sound through water was affected by temperature and depth, and they could be fitted to the exterior of submarines to aid them in both attack and defensive manoeuvrings. This development inaugurated a productive and reliable relationship, based on the exchange of data, instruments and personnel, between the US Navy and Woods Hole that persisted throughout the Second World War and beyond. It was the presence of oceanographers on Navy ships and Navy funding for research that facilitated the study of a range of geodetic and oceanographic data, from the shape of the ocean floor to the behaviour of waves at the sea shore – key scientific information and observations that also had clear military applications when it came to marine landings and sub-surface nuclear warfare. But the relationship between basic and applied research, and the Navy’s interest in supporting the former, was not always clear, as the development of manned deep sea submersibles illustrates.

The idea of building such a vehicle, a means by which humans could reach hitherto impossible depths and pressures was, in its modern form, that of Allyn Vine (1914-1994): its realisation was the product of extended negotiations on finance, ownership and design between Woods Hole, the Office of Naval Research and Reynolds Metal Corporation. *DSV Alvin* was commissioned in June 1964, but spent its first few years on specific tasks for the Navy – carrying out practical tasks like inspecting hydrophone arrays, looking for a lost hydrogen bomb. It was not initially used to fulfil the research proposals that that had been put forward in order to address significant questions of marine biology and geophysics. By the early 1970s, there were serious conflicts over funding between the Navy and the other interested parties, which meant that the *Alvin* came near being axed. It did not help that there had been many sub-surface accidents and losses in the late 1960s – including the sinking of *Alvin* itself in 1968 – although the discovery of a still-edible bologna sandwich on board when *Alvin* was recovered almost a year later did stimulate microbiologists to expand their work on decay in the deep-sea, and eventually, to challenging the notion that the ocean trenches might serve as receptacles for human waste: a somewhat inadvertent contribution to blue-sky(sea?) research. But once the Navy commissioned its own dedicated submersibles, they cancelled *Alvin*’s funding, forcing (ex-Naval) researchers like Robert Ballard (born 1942) to seek other supporters. In this, they were remarkably successful: beginning in 1974, *Alvin* was used in a collaborative project between academic and military institutions and personnel, which not only discovered the deep sea hydrothermal springs, but established that there was life there.

Here, the various interested parties and institutions had managed to create a basis for cooperation and collaboration, resulting in an event with implications that ranged from the geological through the biological to the philosophical sciences, not to mention the consequences for space exploration. Other examples of deep sea work had less fortunate results. In particular, issues of security and bureaucracy caused problems, especially during the Cold War. Some scientists found their nationality or personal political histories restricted the projects with which they could become involved. In other cases, permission to publish data or results was withheld. For example, by the 1940s, the US Navy was restricting the nature and kind of information that could be published about the oceans – in particular, information about the ocean floor, which could potentially be used to disrupt both submarine traffic and long-distance communication and monitoring. Much of the data-points from which maps of the sea-bed could be built came from the automatic depth-recorders attached to naval ships – but these specific bathymetric details of depth contours and soundings represented knowledge that had to be restricted in its ciruculation. As a result, for example, the famous Heezen-Tharp map of the ocean floor was eventually published in a less specific, physiographic form. As it happened, this made the image popular with, and familiar to, a much wider audience, especially after an artist’s version of it appeared in *National Geographic* – but this does not detract from the fact that scientific work had effectively been censored. Many scientists had related concerns about the sources of the funding for their research and the potential impact that this might have on their academic independence.

But despite these examples of tension between exploration, politics, science and commerce in making observations of the natural world, what is clear is the extent to which the various groups interested in studying the sea were involved in establishing a shared culture of collaborative maritime science. At the outset, the emphasis on accurate and repeated measurements, for example, was an element already present in naval culture, officers and scientists alike kept journals, and the popularity of natural history as a middle-class past-time provided mutual ground on which they could meet. While social relationships aboard ships were spatially segregated, with access to certain areas dependent on rank, on at least some voyages – not just dedicated scientific expeditions such as that of *HMS* *Challenger* – space was made for scientists and their equipment. The question of recompense and financial support was always problematic: after all, the identity of the ‘natural philosopher’ was closely tied to that of ‘gentleman’ precisely because of the assumption that financial independence meant freedom to speak the truth. But by the mid-19th century, a class of ‘scientific servicemen’ had emerged, drawing not just on naval officers and military men, but also on colonial officials and other government employees who had a remit to collect information and specimens from the various posts to which they found themselves sent.

*Science in the State’s service*

In fact, a key element of the professionalization of science in the 19th century was the emergence of a number of institutions, often state-supported (in Britain at least), that were dependent on and would employ field based agents, such as hydrographic offices, national geological surveys, ordnance surveys, census bureaus and so on. Again, these operated both at home and abroad, and key figures in the development of the field sciences often moved between them. Joseph Hooker (1817-1911) is one such example. In his youth, he had participated in the 1839-43 voyages of Arctic exploration. On his return, in 1846, he took up a position as botanist to the Geological Survey of Great Britain, founded in 1835 under the supervision of Henry de la Beche, and at that point entering a period of intense activity, since an act of parliament in the previous year had called for the production of a complete geological map of the country. Hooker spent the next year studying fossil plants in the coal beds of Wales and the South West, before being sent by his father (who was soon to become the director of the Royal Botanical Gardens at Kew) on a plant collecting expedition to India and the Himalayas. His travel to India was facilitated by the long established military and commercial links between Britain and the sub-continent: he sailed on Navy ships and lodged with officials of the East India Company. In recompense, he managed to cause what could have been a serious international incident – and possibly the punitive execution of a border guard – when in his search for novelties he crossed into the forbidden territory of Tibet without permission. On his return to Britain, he was appointed as assistant-director at Kew in 1855, succeeding his father as director in 1865. In these positions, and from the connections he had forged through all these voyages at home and abroad, Hooker established a network of correspondents and representatives, formal and informal, who sent information and specimens back to London.

This work had both a scientific and an economic element: indeed, it was often very hard to distinguish between the two. Hooker was asked to recommend young men for positions in commercial, as well as state sponsored, operations: for example, it was on his word that the young Arthur Keith (1866-1955) was sent to Siam as the medical officer for an English mining company, where he spent much of his time carrying out the dissections of fresh-caught monkeys and apes on which his future contributions to knowledge of human evolution were based. Kew and the network of colonial botanic gardens were central, not just to the collection and breeding of new plants, but to their improvement with an eye to potential exploitation. Specimens came from designated expeditions, from civil servants stationed in remote outposts, from customs officials at ports and borders, from travellers and locals and even from street markets. They could be given, traded, bought or stolen – or even sabotaged, as disgruntled gardeners who had failed to germinate Chinese seeds liked to believe. The successful naturalisation of a valuable plant or animal could lead to immense profit. It was in Paris that the first *Societie d'Acclimatation* was established (in 1854 by Isidore Geoffroy Saint-Hilaire), but the economic and business significance of acclimatising colonial plants and animals – and of transplanting to the colonies productive domestic flora and fauna – was quickly recognised elsewhere. By 1900, there were more than fifty acclimatisation societies, mostly located in the colonies of Europe.

Trade in spectacular exotic animals and plants had always flourished alongside travellers, but these societies were interested in the possibility of deliberately using science and expertise to improve (make more profitable) indigenous ecosystems by means of species transplantation. Some advocates, such as Richard Owen or Francis Trevalyan Buckland, were keen on the prospect of introducing new food animals to European tables, but the greatest successes came in the colonies with the establishment of tea and coffee plantations, the export of salmon, trout and sheep (and rabbits) to Australia and New Zealand. Here, it was not just the scientists that moved from place to place, but the products of field-work and practices that travelled in support of the wider economy. This relationship between science and economic improvement – of which Scoresby’s interest in improving the Arctic whale fisheries was another example – was a significant factor in the development of the field sciences. As noted, surveys and expeditions, both at home and abroad, had been sent out not just to make a record of land-and sea-scapes, but as part of this process to identify potential sources of botanical, mineral and animal wealth. This process was not restricted to the early days of exploration, but was a key part of the imperial and colonial process: the African Research Survey (1929-39), for example, sought to identify and mobilise the natural resources of British colonial Africa. Nor was it always only carried out by travellers: by the early 20th century, European colonies were actively seeking to attract settlers with technical expertise in forestry, medicine, game and agriculture amongst other specialities, to manage and maximise local production.

Early contributors to what was becoming the science of ecology, such as Reginald Stapledon (1882-1960) were firmly rooted in this background of improving agricultural science, moving between domestic and colonial sites of study, and making theoretical contributions as they struggled with practical problems of production. One of Stapledon’s concerns, for example, was with the problem of grassland, which had long been an issue for British farmers – specifically, with the problem of how to turn arable land into pasture, since once land had been ploughed, returning it to a state in which it produced appropriate fodder for grazing animals was very difficult. In 1912, Stapledon began a comprehensive survey of the vegetation of Mid and North Wales, and in 1919, he became the first director of the Welsh Plant Breeding Station, a position of great significance given the importance of productive grassland to the policies of imperial Britain. His vegetation maps, together with the experimental work being done at places such as the Royal Agricultural College, made it possible to show how the quality of grassland (measured in yields of meat or milk) could be incrementally improved. But it was his travels in Australia and New Zealand that brought home to him one of the key insights that was to underpin ecological fieldwork – the need to consider relationships and contexts, and in this context, the recognition that when it came to productive pasturage, the actions of livestock animals were just as important, if not more so, than deliberate efforts at human management.

It was one thing to improve on nature: quite another for humans to produce it from scratch. The experience of scientists like Lauren Donaldson (1906-1994) in the American Northwest showed how impossible it was to experimentally recreate natural relationships. Donaldson believed that it was possible to manipulate nature in the field in order to create an economically productive ecology – specifically, to create a salmon run in the Pacific Northwest. By the 1920s, concern was growing at the damage done to fisheries by industrialisation and over-fishing. Donaldson tried seeding a nutrient-and-salmon deficient lake with minerals and fish ova, alongside the eradication or marginalisation of undesirable animals and plants. Ultimately, he failed. Salmon grew, and even bred in the lake, but they did not migrate: beavers could be shot, but they kept coming back and damming streams. Donaldson could create a fish farm, not a salmon run. For both Stapleton and Donaldson, ecological field work demonstrated the limitations that natural complexity placed on human intervention.

Untangling the convoluted and complex relationship between field science and economic and commercial demands over the past two hundred years is beyond the scope of this chapter. But it is worth noting that just as the identification and *extraction* of biological and mineral resources has been a central plank in this relationship (from the explorers of empire to Shell’s sponsorship of both the Royal Geographic Society and the National Geographic Society), so has the *defence* of these resources against development, in the name of conservation, biodiversity and democratic freedoms. Again, this is a problem that is closely related to the question of how scientific identity is established and maintained in the context of field science – how, when and why might a scientific researcher become better described as a scientific activist? What influences do, or can, field scientists have over economic and political development? And – perhaps most importantly – what consequences might the attempt at doing so have for their ability to speak with authority about the natural world? The next section of this chapter will consider these questions.

***Section 3: Activism, Authority and Field Science***

Doing fieldwork usually means working on territory, whether land or sea, that either belongs to, or is used by other human groups. This can cause both practical and epistemological problems. In terms of research methodology, for example, many researchers, especially those involved in observational/historical field sciences, had committed themselves to observing ‘natural’ behaviour or events. They went to the field in the first instance because they wanted to see how organisms related to each other within a ‘natural’ context, since recreating the complexities of natural relationships in an artificial context was impossible. Particularly valuable, then, were pristine sites that had remained relatively unaffected by human development, but since these sites were, almost by definition, impossible either to find or to access, most 20th century workers contented themselves with approximations. Nevertheless, the claim that a rival was doing work on a site where conditions had been materially affected by human alteration remained a rhetorical resource in controversial debates. Turning from epistemology to practicality, doing fieldwork virtually always meant getting permission from those groups that had a previous claim – whether legal or moral – on the space in which the researcher wished to work.

*Getting access to the field*

In other words, field sites are not just scientific spaces, but working landscapes. They are occupied or used by other human beings, and put to economic, practical, even spiritual uses. Depending on the nature of the site, this has a number of consequences for the scientist and for the research carried out there. First, there is the need to get permission to be at the place, and the related issue of what, if anything, the scientist must or should offer in return for that permission. Secondly, there is the issue of managing relationships with the other people at that site, and in particular, managing any contribution that they make to the conduct of the research there. In the third place, there is the problem of presenting the results of that research to the people back home, whether they be academic supervisors/colleagues, politicians, civil servants, business leaders or members of the general public. All three of these matters can have a significant impact on the field scientist’s ability to do research and the reception which that research receives.

It should be evident by now that field work is carried out in an incredibly varied range of places – from ship deck to mountain top to rainforest – and the political and bureaucratic process of getting permission to work in the field can be incredibly complex. In the later 20th and early 21st century, for example, permits to carry out work in national parks need to be obtained, research visas stamped to allow entry into foreign countries, licences to import technology or export samples acquired. Once in the field, and perhaps more informally, scientists have often had to offer inducements to ease their entrance to their actual site. These might range from offering credit, whether academic or monetary, to helping with a legal problem or supporting an economic claim, and their nature has changed over time. Anthropologists, for example, in the days of empire, were allowed access to tribal peoples not least because their work might help colonial officials manage the ‘natives’: in the 21st century, in contrast, locals might agree to cooperate with anthropological research in return for help in court-room battles to reclaim land from incoming settlers.

Archaeologists and paleontologists, especially in the American West, found themselves in a position where the most intellectually interesting sites were either already private property, or within Indian reservations, or were in the process of becoming so – as the director of the Carnegie Museum, William Holland (1848-1932) discovered in the early 20th century. Extremely rich fossil beds had been found around a ranch in Nebraska owned by the Cook family who, as it happened, were very willing to accept and support visiting scientists, although they were not keen on the Carnegie’s attempt to insist on privileged access. But it was when Carnegie researchers refused to give the Cooks credit for discovering the fossil beds in the first instance that the relationship between the family and the institution became badly damaged. It finally broke when Holland tried to obtain legal title to the land containing the fossil beds that was outside Cook’s property line, with the intent of excluding other scientists. Harold Cook promptly homesteaded the area, and was able to parlay his practical support for research into a scientific career for himself with institutions like the University of Nebraska and the American Museum – who were willing to be more accommodating to the contributions of amateurs than was the Carnegie at this point.

*Locals, laypeople and locating expertise*

But the contributions of people who did not primarily identify themselves as scientists, whether properly credited or not, had always had a major role to play in the conduct of field science. Earlier in this chapter, the achievements of Edward Sabine and William Scoresby were discussed, but even after science had largely professionalised, amateurs continued to be central to science in the field. Whether at home or abroad, field scientists found themselves drawing on a range of local talent – whether these were sailors aboard ship, local farmers or schoolchildren – in the collection of data and specimens. Perhaps one of the most famous amateurs in the history of science – certainly in the history of geology – was Mary Anning (1799-1847), although the use of the term amateur in her case is ironic. Fossil collecting was for her, above all, a business: her discoveries, whether of the iconic marine vertebrate fossils of *Ichthyosaurus* and *Plesiosaurus*, or the more common ammonites and coprolites, were sold to make a living for her and her family. And her approach to that business was utterly professional: she was widely known to be an expert and experienced excavator who was very familiar with the relevant scientific literature. She was recognised by naturalists and geologists like de la Beche and Murchison, but her skill at finding and unearthing fossils was not mirrored in formal credits: when her finds appeared in museums they did so not under her name, but those of their donors.

Anning is unusual in that she is so well known – other amateur contributors to field science, whether past or present, are largely forgotten, precisely because they tend to hand their data (specimens or observations) over to scientists for analysis and reporting. In this, they are following a pattern established in the very early days of scientific field research. Originally, the fieldworker’s role was simply to collect information, which they would then transmit back to university and museum scholars in the metropolitan centres of knowledge production. This was one key reason why institutions, learned societies and associations were so keen to produce manuals instructing people on how and what to observe: this was also a means of directing people at a distance to the points that these ‘armchair scholars’ thought were of interest. It was also, and remains, a means of collecting large quantities of data relatively efficiently, as in the case of Charles Elton (1900-1991)’s work with the Hudson Bay Company in the early 20th century: local agents forwarded to him data on the numbers of fur-bearing animals and their prey, which he was then able to use both to advise the Company and to elaborate his own ecological work. Digital developments in the 21st century have brought this practice back to the forefront of some kinds of fieldwork, as scientists use the web and webcams to enlist the help of citizens, as with the Zooniverse projects that ask volunteers to help identify animals, find fossils and classify galaxies. But these examples focus only on one aspect of fieldwork – what happens to information, specimens or data, *after* they have been extracted. What precedes that process is equally important.

Amateur, lay and local contributions to field work went far beyond simply the delivery of information – their skills and practices were, in fact, a crucial factor in the development of the reliable scientific field researcher. As previously noted, when scientists went to the field, they found it already populated by a variety of different social groups – and it was from the culture and practices of these groups that they learnt to navigate the field environment. This chapter has already looked at the relationship between marine science and the military, but ordinary fishermen also provided a resource for researchers. For example, the ecologist Stephen Forbes’ (1844-1930) study of the Illinois River drew inspiration and information from the local rivermen: he and his colleagues used their local techniques for working the river, collecting samples and so on, as well as listening to and profiting from vernacular versions of the river’s natural history. Eugenie Clark (1922 – present), sometimes known as ‘The Shark Lady’, paid tribute to the knowledge and help she had received from ‘the best spear-fisherman ’ Siakong, the Pacific Islander who tutored her in the technology and psychology of fish collection. This was not a phenomenon confined to the 20th century: Horace-Bénédict de Saussure (1740-1799)’s contributions to mountain science, Alpinism and geology are recognized with a statue in Chamonix – where he is shown alongside Jacques Balmat (1762-1834), the mountain guide who showed him it was possible to climb Mont Blanc. Expeditions and the survey field sciences benefited hugely from local knowledge and guidance, not just in terms of accomplishing the expedition’s aims, but often with regard to simple survival.

When it comes to the observational/historical field sciences, local knowledge and practice is extremely significant – not least because while the success of this work is often dependent on detailed knowledge of specific places, the work is usually carried out by people who are not local to that particular area. For ethology and zoology, for example, locating the animals that are of interest – especially if they are rare – often depends in the first instance on talking to local residents. Ironically, often the most knowledgeable people can be the ones who are involved in making the animals rare in the first place – such as the hunters, the loggers and the fishermen. Fishermen, for example were employed to bring samples to marine biological stations: scholars would travel to the sea shore, request specimens for study and dissection, and find them at their desks the next day. But increasingly through the 20th and 21st century, field researchers found themselves employing local assistants, not just to find the animals or plants in the first instance, or to provide guidance or protection – but to maintain the kind of long-term record keeping that was going to prove vital to the ultimate authority and acceptability of the work emerging from that field site. Most field scientists, after all, had commitments at home that they needed to meet – they either needed to return to write up and defend their theses at the end of their projects, or they needed to be back on campus to teach at the start of the academic year. What happened to the site and the study then became an issue, especially in relation to zoological and ornithological studies: was all the work done there in terms of habituating and identifying the animals to be abandoned, or was there a way of managing events so as to maintain continuity? One might pass over a site to an incoming colleague on a new project – but one might also try to ensure that local field assistants can maintain site records and ensure a permanent research presence at the site.

The history of primatology in particular is characterised by such efforts and transitions, although few of them have been unproblematic, and some have been intensely controversial, with questions raised about the qualifications of field assistants to make long term observations. The Gombe Stream Research Centre, for example, is one of the longest running sites of field research in the world, not least because of the ability of local assistants to maintain observations in the absence of researchers. In 1960, it was a condition of Jane Goodall (1934-present)’s permission to work at Gombe that she be accompanied while there by local men. Their role became more prominent after 1970 when it was decided that as well as accompanying students, they would collect basic data on chimp social grouping and behaviour. Their role grew with Gombe, as more and more students came to work with Goodall and the chimps - and became utterly essential when Zairean rebels took four students hostage in 1975. Research permission for foreigners to work at Gombe was promptly withdrawn by the Tanzanian government, and for the next years, the social and ecological data was collected by the field assistants. Goodall later noted the importance of instituting training programmes for such people, arguing that – in this instance – their ability to consistently and accurately identify the individual chimpanzees was the fundamental qualification for being able to record data reliably.

In many other examples of field science, providing training or employment for local people has become part of the process of acquiring permission to do research – as well as, for many scientists, a moral obligation. But the question of the reliability of these field results can remain, especially when they’re assessed by people far from the field site in question.

*Pleasure, pain and popular narratives*

Managing the problem of the role played in the research by people without formal qualifications was just one of the issues that researchers had to deal with when it came to the social relationships and identities of field science. They had to be relatively sociable, in order to – as we saw earlier in relation to marine science – build coalitions that would support their research. They had to convince people – whether scientists or members of the lay public – who lived and worked in places far distant from their research sites that their work represented an authoritative account of some aspect of the natural world – that they were, actually, *doing* science in the field. This could be a structural as well as an academic problem. When scholars went to the field, they usually did so in the ‘vacation’ from formal teaching. Marine biologists went to the sea-shore, glaciologists or astronomers to the mountains, botanists to the woods and heaths – and they did so using the railways and roads created for the leisure industry that had emerged by the late 19th century. They often travelled with their families, and stayed in the lodging houses and road inns used by tourist and other travelers: they camped, they went hiking, they sketched and took photos, and to a casual glance were indistinguishable from other middle-class professionals on holidays. Marine biology after the development of the aqualung by Cousteau and Gagnan is a particular case in point: scientists belonged to diving clubs, and diving clubs fed information to scientists. Were these people working, or having fun at someone else’s expense?

This problem could be exacerbated by the fact that, while no scientific project necessarily produces reliable results, unproductive fieldwork can look particularly suspicious. If a lab project proves inconclusive or produces contradictory effects, then at least the researcher’s visible presence in the laboratory shows that they have tried their best to succeed. But how do you rate or reward the long periods of time when a fieldworker can’t find the data they’re looking for, or gauge, from the perspective of domestic academic institutions, the physical strain of getting to, constructing, working at and defending a research site in the rainforest? Letters between researchers in the field and those at home reveal the tensions that could emerge when the latter tried to exert too much control at a distance – for example, the problems caused for the Geological Survey when de la Beche tried to insist that survey officers account for their daily movements in detail (1850), or the acrimonious correspondence between Harvard astronomers William Pickering and Robert Black (1889-1890). Field researchers felt that their professional autonomy and dignity needed to be respected: administrators and more senior figures worried that they would not see an academic return on their funding. Field science was a much chancier business than was lab work.

In order to succeed, field workers had to possess or develop a range of personal characteristics and interpersonal talents. They had to be capable of making common cause and communicating with a range of different social groups, from their supervisors at home to the local assistants in the field. They had to be physically and emotionally robust and resilient. As noted earlier, by definition, doing fieldwork required movement through space: not just the ability to work long hours with concentration, but physical fitness and endurance, as well as the capacity to tolerate a range of different environments. As the primatologist Clarence Ray Carpenter (1905-1975) put it, to succeed, a fieldworker needs “the endurance and patience of a pack mule”. Accounts of fieldwork, from the explorer’s stories of the 19th century to the blogs of the 21st, often reference a kind of muscular science, with the body again as a tool for wresting data from the land and sea-scape. Bleeding feet, blistered hands, bouts of disease, monotonous or no rations, cold, heat, abrupt and uncomfortable encounters with large and decidedly unfriendly charismatic megafauna abound in these narratives. Emotional and psychological stress was also a potential problem, particularly where fieldwork was being conducted solo in unfamiliar places.

But critically, fieldworkers were also able to turn these problems to their advantage, particularly when giving accounts of their work to wider audiences, both academic and lay. In some contexts, they reported that prolonged solitude could be theoretically and intellectually productive: encountering new phenomena under conditions of complete absorption in one’s work could generate creative innovation. More often, it was the physical and emotional suffering they had endured in the field that was stressed, especially in works intended to be consumed by the general public. From the arctic and tropical narratives of early 19th century explorers to the 21st century accounts of herpetologists in the Congo, the difficulties and problems encountered and (sometimes) overcome by the field worker were foregrounded, whether they sprang from the physical or the social environment. Partly, this arose from the structural demands of writing for a mass audience: in 1752, John Hawkesworth (1715-1773) had argued that, in order to appeal, explorers’ stories needed to stress the heroic nature of the venture. They should show the traveller sacrificing self for knowledge, and should preferentially give first personal testimonials of eyewitness accounts of the natural world, giving their audiences the chance to watch over their shoulders as they met and mastered extraordinary challenges.

Despite the fact that the production of popular science became a steadily less respectable pursuit for scientists as the 20th century progressed, for a number of reasons, field scientists remained keen to publish such accounts. First, and most obviously, they were a potentially profitable means of raising their profile. More importantly for most writers, however, they were not just a means of publicising their work, but of showing the public the importance of their work, thereby making it more likely that financial support for their research would be maintained. John Barrow’s eagerness to promote Arctic exploration through the narratives published by Murray and sons showed his early awareness of the need to establish broad public backing for publicly funded research. By the late 20th century, it was not just support for research that scientists were seeking through popularising their work, but support for broader projects of conservation. By this point, the field sciences characterised earlier as examples of observational/historical science had begun to show some of the characteristics of salvage science, as researchers hurried to study organisms and environments before they disappeared forever. Particularly with regard to zoological work, what is notable about these accounts is the way in which scientists took Hawkesworth’s arguments about the need to dramatize the scientific story a step further. It was not sufficient to portray the scientist’s heroic struggle: in these accounts, the subjects of scientific research themselves become personalities, struggling to survive and protect their families in a hostile physical and social environment. As this chapter showed earlier, long term research meant that it was possible for scientists to know the individual histories of their research subjects, portraying them as characters with back-stories, motivations and emotions of their own. Such an anthropormorphic strategy did mean that the scientific status of the authors might be called into question: for some, this was a cost worth paying if it meant public support for conservation work.

*Conclusion*

Public interest in field science has always been intense, both in terms of information and practice. The 19th century – or more specifically, the period 1820 to 1870 – has been called the heyday of natural history, as botanising became a respectable leisure activity and drawing rooms became crowded with collections of living and preserved creatures, from aquaria to pressed flowers. But in terms of the content of scientific knowledge, it was and is the capacity of the field sciences to tell origin stories that was responsible for most of its appeal. From geology to astronomy to primatology, accounts of field work that seem to shed light on where humans came from and where we might be going have been transmitted through magazines, books, radio, film and blogs, and avidly consumed by the public. Of course, this adds another element of uncertainty to the role and identify of field scientist – as noted earlier, they are not necessarily in control of the information that emanates from their field site, and they are certainly not in control of how that information is deployed in public life by different groups. From Hawkesworth to the present day, accounts of life in the field have been edited and adapted – not with any nefarious purpose, since the construction of any narrative relies on the foregrounding of some elements and the marginalisation of others – but with consequences for the way in which the Western public thinks about nature and our relationship with it. Present in, but often treated as largely marginal to these accounts, are the networks of political support, commercial backing and intellectual obligation that underpin field work. Particularly over recent years, highlighted are the role of local peoples in both helping and hindering the research process.

But as this chapter has repeatedly shown, fieldwork is intimately tied up with issues of economic development, political maneuvering and policy-making: just as some fieldworkers went to the field to study relationships between organisms, so field science exists within a wider ecosystem of relationships, both reciprocal and denied, between humans and the rest of the natural world. This is not to argue that other kinds of science don’t show such characteristics: only that they are much more obvious in the context of field science, since field science is conducted out in the open, rather than behind laboratory walls. But of course, the combination of the fact that fieldwork happens within the public domain with the ready availability of the results and accounts of field-based research means that the expertise of field scientists can sometimes become blurred. This is exacerbated when field work is judged and assessed against a fairly simplistic account of the scientific method – one that assumes, for example, the ability to carry out clear-cut experimental tests of hypotheses. There may well be aspects of field science susceptible to such assessments, but most work in the field – done, as we have seen, in specific, unique places, and using methods of observation rather than manipulation to exert control over their environment – is not. And in addition, more than most scientists, fieldworkers are aware of the care that needs to be taken in the interpretation of their observations and measurements, and the need to be reflexively self-critical and cautious when considering the relationships between theory and observations, instrument and data. Whether dealing with global climate measurements or considering an interaction between two juvenile baboons, most modern field scientists show a self-awareness and humility that scholars in the humanities and social sciences would do well to emulate.

Understanding this aspect of field science is particularly important, given the significance that the results of field research have for the human future. As noted, accounts of field science have been consumed for what they can tell us about human origins: it is ironic that understanding more about the history of field science may well help us appreciate more the complexity of the political, scientific and cultural responses to the prospect of human extinction.

*Further Reading*

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