THE PURSUIT OF NATURE: DEFINING NATURAL HISTORIES IN EIGHTEENTH-CENTURY BRITAIN

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This dissertation is the result of my own work and includes nothing which is the outcome of work done in collaboration with others.

This dissertation does not exceed 80,000 words, including footnotes.

The pursuit of nature: defining natural histories in eighteenth-century Britain

Many histories of natural history see it as a descriptive science, as a clear forerunner to modern studies of classification, ecology and allied sciences. But this thesis argues that the story of unproblematic progression from eighteenth-century natural history to nineteenth-century and modern natural history is a myth. Eighteenth-century natural history was a distinct blend of practices and theories that no longer exists, though many individual elements of it have survived. The natural history that I discuss was not solely about collecting, displaying, naming and grouping objects. Though these activities played an important part in natural history (and in many histories of natural history) this thesis focuses on some other key elements of natural history that are too often neglected: elements such as experimenting, theorising, hypothesising, seeking causes, and explaining. Usually these activities are linked to natural philosophy rather than natural history, but I show how they were used by naturalists and, by extension, create a new way of understanding how eighteenth-century natural history, natural philosophy and other sciences were related.

The first chapter is about the end of eighteenth-century natural history and looks at the role of the Linnean Society of London. It argues that this society tried to homogenise British natural history through the promotion of the Linnean sexual system of plant classification and through the suppression of the kinds of experimental and theoretical work described in this thesis. To understand that experimental and theoretical work, and to see what British natural history really entailed in this period, three central chapters focus on specific case studies. The second chapter shows how English-based naturalists such as John Ellis (1710-1776) approached the problem of distinguishing plants from animals, and especially about how they used chemical experiments to decide whether things such as coral and corallines should be placed in the animal or plant kingdom. The third chapter discusses sensitive plants and the overlaps between natural history and natural philosophy. It draws on case studies of naturalists who investigated things like plant motion and apparent plant sensitivity with different observational and experimental methods, and tried to explain them using various mechanical and vitalist explanations. The fourth chapter focuses on the controversy over whether plants (like animals) can be male or female and shows the theoretical and experimental tools that naturalists used to address this issue. Together, these chapters give a very detailed insight into the everyday practices and theories used by eighteenth-century naturalists and show the variety of activities that made up the field. The next two chapters focus on the identity and interactions of naturalists and show how they created a distinctive science: the fifth chapter is about how someone in England could go about becoming an authority on natural history in the late eighteenth century; and the final chapter looks outwards from Britain and examines how British natural history influenced, and was influenced by, European natural history; it uses correspondence to examine how British naturalists communicated with their overseas counterparts and what each party gained from those exchanges.

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INTRODUCTION

What is natural history? Or, more precisely, what *was* natural history in the eighteenth century? Though these two questions appear similar they in fact have very different answers. Many histories of eighteenth-century natural history see it as a descriptive science, as a clear forerunner to modern natural history. But here I argue that the story of unproblematic progression from eighteenth-century natural history to nineteenth-century and modern natural history is a myth. Eighteenth-century natural history was a distinct blend of practices and theories that no longer exists, though many individual elements of it have survived. The natural history that I discuss was not solely about collecting, displaying, naming and grouping objects. Though these activities played an important part in the subject (and in many histories of it) this thesis focuses on some other key elements of natural history that are too often neglected. These elements are things like experimenting, theorising, hypothesising, seeking causes, and explaining. Usually these activities are linked to natural philosophy rather than natural history, but I will show how they were used by naturalists and, by extension, create a new way of understanding how eighteenth-century natural history, natural philosophy and other sciences were linked.

In the epilogue of *Cultures of natural history*, the most comprehensive collection of work on the history of natural history, Jim Secord concludes that the 26 essays that make up the book cannot answer the question 'what is natural history?'.[1] This is not because of any fault on the part of the authors, but because of the very nature of the subject. The definition of natural history has always been contentious; Secord believes that this is because definitions must centre around "acts of exclusion and inclusion". In this thesis, I examine practices and theories that have subsequently been excluded from the history of natural history and show: how they fitted into the natural history of the eighteenth century; how they interacted with better-known natural historical practices such as collecting and classifying; how they related to other scientific disciplines; and how and why they began to be removed from natural historical discourses towards the end of the century. Despite renewed interest in the history of natural history in the past few decades,

[1] Secord [1996] 448.

and despite the publication of wide-ranging survey works, these experimental and theoretical aspects have still not been fully considered and understood by historians.[2]

There is little agreement among historians as to exactly what natural history was in the eighteenth century. It has been variously described as a branch of history with a particular focus on description; the foundation of natural philosophy, responsible for the creation of 'facts'; an aesthetic activity that centred on collection and the creation of elaborate cabinets; a religious activity that formed the basis of natural theology; a set of practices, particularly observational practices, that gave the practitioner special insights into nature; an attempt to impose order on the natural world; and an attempt to see the order created by Nature herself. [3] There are almost as many definitions of natural history as there are historians of natural history. Wider interest in the history of natural history is a fairly recent phenomenon; earlier historians of science generally considered it to be beneath their notice with Charles Gillispie, for example, writing that taxonomy and classification (two key activities within natural history) were unlikely to appeal to historians and that attempts to study them "did not ultimately prove interesting".[4] More recent work has gone some way to explaining how natural history was encumbered with this reputation as a dull science, not much more intellectually challenging than stamp-collecting.[5] G.S. Rousseau and Roy Porter have described how early nineteenth-century intellectuals rebelled against eighteenth-century sciences which they painted as "boring, unoriginal, lacking in rigour, and over-speculative"; Rousseau and Porter attribute this rebellion to the growth of the Romantic and counter-French-Revolutionary movements and describe how it has negatively impacted many studies of eighteenth-century knowledge.[6] Paul Farber has described a similar phenomenon whereby nineteenth-century pioneers in the new field of biology sought to distance themselves from earlier natural history; so successful was their propaganda that natural history has been undervalued by historians.[7] Paradoxically, this nineteenth-century tendency to see a gulf between 'descriptive' natural history and 'scientific' biology came about partly because of

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[2] This is particularly true for Britain. Experimental traditions by naturalists in France have received far more attention; see the works of Spary [2000] and Terrall [2002] for recent examples of such scholarship.
[3] Rappaport [2003]; Bacon [1834]; Allen [1993]; Daston & Vidal[2004].
[4] Gillispie [1960] 170.
[5] Johnson [2007].
[6] Rousseau & Porter [1980] 3.
[7] Farber [1982a].

an attempt to make natural history more 'scientific' by focussing primarily on classification, as I show in this thesis.

Other problems have also affected the historiography of natural history. Rhoda Rappaport has pointed out that natural history could expand to accommodate almost any topic – she cites the example of David Hume's 1757 *Natural History of Religion* – and that the sheer breadth of natural history makes it almost impossible to write a coherent history of it.[8] Even if it were possible to write a history that encompassed all of eighteenth-century natural history, the historian would encounter other historiographic problems: Simon Schaffer has complained that the dominant historiography of the eighteenth century shows natural philosophy and related traditions as a coherent and unified set of theories and practices; meanwhile Richard Yeo has lamented the fact that historians seem to have divided into two camps on this issue, with some believing that eighteenth-century sciences were a unified whole and others believing that the period is notable for the separation of scientific disciplines.[9]

This problem of whether different branches of eighteenth-century knowledge formed a united whole or a set of separate discourses is a significant one. To what extent were there separate 'disciplines' in this period, or, to what extent did knowledge in seemingly different areas actually overlap? These questions are central to understanding what natural history was in this period, but answering them satisfactorily is difficult because the diversity and complexity of eighteenth-century intellectual pursuits makes it almost impossible to generalise. Rousseau and Porter, when editing a volume about the historiography of eighteenth-century sciences, constantly came up against the problem of overlapping areas of knowledge: they would commission an essay on one topic, such as natural history, and receive a finished product that also delved into medicine and physiology. Almost anyone who has studied the period will be familiar with such occurrences. Rousseau and Porter also found it near-impossible to map eighteenth-century branches of knowledge onto modern disciplines and vice versa. Shifting boundaries make the historian's task infinitely more challenging, but also more worthwhile.[10] Several other historians have also written about this difficulty: Schaffer has

[8] Rappaport [2003] 418.
[9] Schaffer [1980] 55; Yeo [2003] 243.
[10] Rousseau & Porter [1980] introduction.

published on the problem of deciding whether Joseph Priestley's work on airs counted as physics, chemistry, natural philosophy or some science unique to Priestley; Yeo has described how the explosion of knowledge in the eighteenth century made it almost impossible to map disciplines or areas of expertise[11].

Many years ago Thomas Kuhn discussed the problem of the historian who wishes to write a history of a modern subject such as electricity. Electricity did not exist as a field of study until the seventeenth century but it is possible for the historian to use a range of works "ordinarily described as works of philosophy, literature, history, scripture, or mythology" to create what looks like a plausible account of its development; however, this ignores the fact that the phenomena described (such as lightning or electric eels) were not believed to be related by the authors of those various works. Kuhn warned against falling into the trap of mapping older sciences onto modern disciplines.[12] Porter has emphasised that scientific disciplines are essentially cultural and historical products and that in order to understand the spaces within, and the boundaries between, fields of knowledge we must try to see them as they were seen in the eighteenth century.[13] Mary Terrall's work on the French savant Pierre Louis Maupertuis which shows how his career seamlessly moved between physical and life sciences, literature, mathematics and philosophy is a good example of how this approach can work.[14]

Some authors have taken a different line and see not unity but disparity between the different branches of knowledge in this period: John Lyon and Phillip Sloan have stressed how eighteenth-century knowledge separated into discrete categories which formed the basis of nineteenth-century disciplines; Thomas Hankins too has focussed on differences, rather than similarities, between nascent disciplines.[15] How does a historian decide when to emphasise unity of knowledge, and when disparity? Those who study the nineteenth century as well as the eighteenth seem more likely to see disciplines forming and separating out from one another. Perhaps because my work is mostly concerned with eighteenth-century natural history, I tend to see the ways in which natural history spreads out and blends into many other fields. My research on the work of self-proclaimed

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[11] Schaffer [1984] 152; Yeo [2003] 242.
[12] Kuhn [1977] 31-5.
[13] Porter [1980] 318-9.
[14] Terrall [2002] chapter 1.
[15] Lyon & Sloan [1981] 2; Hankins [1985] 11.

'naturalists' touches on various kinds of natural histories, natural philosophies, observational practices, experimental practices, exact sciences, inexact sciences, pure description, and the search for causes. No two characters in this story conduct their natural history in quite the same way; none seems to feel constrained to act within the imagined bounds of an archetypal 'natural history' or to shun ideas or practices from other fields of knowledge. The natural history that I describe in this thesis is very much part of a bigger scientific discourse, not a separate entity.

This leads to one of the primary questions of this dissertation. The problem of describing the relationships between all fields of knowledge in the eighteenth century is too big to be tackled here, so I focus instead on a narrower part of that question: how did natural history interact with other branches of the arts and sciences? Before that can be answered, a more fundamental question must be dealt with: what was natural history in this period? As I have indicated, there is no simple answer to this question and historians have many different ways of approaching it. One of the commonest definitions of natural history came from Francis Bacon (1561-1626). In his *Descriptio Globi Intellectus* of 1612, Bacon decreed that natural history was primarily a science of description and that its products were the raw material for natural philosophy.[16] Bacon's belief that natural history should largely concern itself with descriptions of natural objects has been taken by some historians to mean that natural history actually *did* only concern itself with descriptions of natural objects. Lyon and Sloan, for example, adopt this view and write that Bacon "made" natural history a descriptive science with almost no independent theoretical content.[17] But Bacon's definition included another element – that of 'experimental history' which he saw as "an inventory of extant operations or 'experiments' in the arts and crafts and in everyday life, which complemented natural history"; he encouraged not just the recording of common experiences of nature, but also the collection of "deviating instances" in order to avoid making rash generalisations and axioms.[18] Ursula Klein has shown how this 'experimental history' functioned in the work of Herman Boerhaave (1668-1738) working in Leiden and Staffan Müller-Wille has discussed the work of Swedish naturalist Carl Linnæus (1707-1778) in relation to the

[16] Bacon [1612] *Descriptio Globi Intellectus*.
[17] Lyon & Sloan [1981] 2.
[18] Klein [2003] 539; Daston [1998] 23-4; for more on Bacon and the different methodologies of natural history, see also Pomata and Siraisi [2005] introduction.

Bacon's views of natural history, but a similar study of British natural history does not yet exist.[19]

Many histories of natural history neglect Bacon's idea of the science as one that involved both description and experiment. Like Lyon and Sloan, Rappaport writes that eighteenth-century natural history "meant description", although she concedes that, though natural history was principally a descriptive enterprise, it occasionally dealt with causal explanation.[20] Many other historians also see a very clear divide between the activities of natural history and natural philosophy: R.W. Home understands natural philosophy as a field concerned with "broad principles rather than particular natural effects"; John Pickstone creates three scientific 'ways of knowing' and clearly separates the natural historical style from analytical or experimental ways of understanding the world; and Alastair Crombie likewise distinguishes natural history from experimental traditions.[21]

Yeo is an example of a historian who does not take definitions involving description at face value; his approach to discovering the essence of eighteenth-century natural history involves trying to reconstruct its meaning by using contemporary dictionaries and encyclopaedias.[22] Encyclopaedias are physical embodiments of the act of classifying knowledge so they seem an obvious source for defining fields or disciplines. Yeo looks at three particular encyclopaedic works: those of John Harris (1666-1719); Ephraim Chambers (1680-1740); and Jean-Baptiste le Rond d'Alembert (1717-1783) and Denis Diderot (1713-1784). Harris's *Lexicon Technicum* (1704-10) divided natural knowledge into three parts: natural philosophy and physick; chemistry; and botany, natural history and meteorology. His definition of natural history was largely Baconian and saw it as primarily descriptive. Chambers's *Cyclopaedia* of 1728 also saw a distinction between natural history (which fell into the category of 'sensible') and natural philosophy (which fell into the category of 'rational'); furthermore, experimentation and the search for causes were exclusively linked to natural philosophy. The 1751-72 *Encyclopédie* of Diderot and d'Alembert placed natural history and natural philosophy much closer

[19] Ibid.; Müller-Wille [2008].
[20] Rappaport [2003] 417.
[21] Home [2003] 354; Pickstone [2000] introduction; Crombie [1994] chapter 5. It should be noted that Pickstone and Crombie are aiming to distinguish styles of science, rather than identify discrete disciplines, so their works still allow for an overlap in terms of actors or methodologies.
[22] Yeo [2003] 253-263.

together, with subjects like zoology, botany and mineralogy being placed in the same category as the physical sciences, and with natural history and chemistry being placed on the same continuum. Partly, this reflects differences in the sciences between Britain and France, but it also, more generally, reflects different possibilities for how people thought about what natural history was and how those views developed over time.[23]

Not all historians favour Yeo's approach; some prefer a definition of natural history that is based not on what people *say* they do via the written word, but, rather on evidence about their practices. Gunnar Broberg looks at the importance of quantification in natural history and sees it as a science that brought together mathematics and encyclopedism.[24] Emma Spary's *Utopia's Garden* shows how it is impossible to separate the practices of naturalists from the political and cultural milieus in which they worked.[25] Lorraine Daston and Anne Secord also focus on the daily activities of the naturalist when studying, for example, observational practices within natural history. Theirs is a methodologically-based understanding of natural history that looks at how "regimens of experience", rather than larger theoretical frameworks, may have shaped the field.[26] Closely linked to practices, are objects; one cannot talk of, for example, observational practices without also considering what was being observed. A material view of natural history often leads to the study of collections and cabinets. Creating displays and collecting were extremely popular activities and David Allen has pointed out that many people were drawn to natural history for visual reasons rather than intellectual or scientific ones, so natural history can be seen as an aesthetic activity, and many popular works of the time emphasised this.[27] Allen has described many of the features of eighteenth-century natural history; indeed he has probably written more extensively about English natural history than any other historian. In *The naturalist in Britain* he has stressed that natural history needs to be understood within its social context before its "true character" can be known, but there is no one, simple distillation of this character and Allen writes that he has found this "convenient vagueness" useful.[28] But in order to write a book about naturalists, Allen required a working definition of the terms and so he

[23] Later works tend to have broader definitions of natural history.
[24] Broberg [1990] 45-71.
[25] Spary [2000] introduction.
[26] Daston & Vidal [2004] 100-115; Secord [1994].
[27] Allen [1993] 344-7.
[28] Allen [1976] preface.

correlated all seventeenth-, eighteenth- and nineteenth-century natural histories with the twentieth-century disciplines of ecology and systematics as well as any practices still used at London's Natural History Museum.[29] This kind of definition is built upon mapping modern sciences onto older traditions and leads to the neglect of large areas of those older traditions; this exclusion has caused a distorted picture of natural history to emerge from many histories of natural history.

Other historians take a different tack and create definitions that attempt to mirror the true breadth of eighteenth-century natural history. Nicholas Jardine turns most accounts of natural history upside-down when he looks not at the purposes or doctrines of natural history, but at the *questions* (especially the ones that now seem unreal) that naturalists asked.[30] Farber creates four categories to describe the activities of naturalists: the first relates to naming and categorising natural objects; the second relates to description; the third to morphology; and the fourth to experimentation and physiology. There is much boundary-crossing between these four groups, and much interaction between these four and related fields such as natural philosophy and medicine.[31] Farber's work consistently points to the diversity of natural history. Michel Foucault too acknowledges the diversity, and difficulty of pigeon-holing, eighteenth-century natural history. Rather than define it by its aims or practices, he prefers to link it to a broad *episteme* which, he claims, lay behind much of eighteenth-century intellectual life. He sees similarities between the thought-patterns of naturalists, economists and linguists, and these overlaps allow him to construct an epistemological space within which natural history operated.[32]

These broad views forwarded by Farber and Foucault are a good place to start a meaningful investigation into the nature of natural history, and they mesh well with the definitions that seem most relevant: the ones used by naturalists themselves. The characters in this story come from a diverse range of backgrounds; many had trained in medicine, some were clergymen, some had received a university education while others were self-taught. Their methods were diverse: these characters were variously interested in observing, collecting, arranging, naming, dissecting, and experimenting on natural

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[29] Allen [1976] 2.
[30] Jardine [1991] chapter 2.
[31] Farber [1982a] 398-9.
[32] Foucault [1970] foreword and chapter 5.

objects. Despite their different aims and practices, these men are linked by how they saw themselves: each of them referred to himself as a naturalist, or natural historian. From this comes a better way of seeing natural history: natural history becomes the set of activities undertaken by naturalists. This allows a more flexible approach and removes the need for a rigid definition of the field. One possible problem with this approach is that social identity can be a difficult thing to pin down, either through written works or through practices. The perception of the naturalist (both by self-declared naturalists, and by other commentators) could occasionally be a troubled question. John Gascoigne discusses this problem in a book chapter titled 'From virtuoso to botanist' in which he explores topics like: caricatures of naturalists such as Sir Joseph Banks and Daniel Solander; the problem of natural history being seen as a science for 'amateurs' (meant in a pejorative sense); and the desire of naturalists to present themselves as serious men of science. Gascoigne's analysis shows how loaded terms such as 'virtuoso' and 'botanist' could be in the eighteenth century.[33]

Thinking about natural history using the ideas of Farber and Foucault, it is easier to address the question of how it interacted with other fields. Most particularly, I am interested in how natural history and natural philosophy were related. If Bacon is to be believed, then natural history is responsible for the creation of facts for the use of natural philosophers. But is this subservient role a true reflection of how the two fields were actually related? Some recent work by Klein and Wolfgang Lefèvre looks at eighteenth-century sciences and argues that seeing them as two disparate fields does justice to neither. To give a more balanced view, they create a "third domain of learned experiential inquiry" called "experimental history" in which practitioners mixed methods from both natural history and natural philosophy.[34] Michael Bycroft likewise examines the relationships between these two fields of knowledge but, where others focus on their differences or common causes, he tries to build a picture of how they could mutually reinforce each other.[35]

The history of natural philosophy suffers from some of the same problems as the history of natural history. Schaffer has written about historiographical issues with respect

[33] Gascoigne [1994] chapter 3.
[34] Klein and Lefèvre [2007] 21-28. The examples given in this book are mostly drawn from the chemical sciences.
[35] Bycroft [forthcoming] 3-11. Bycroft uses the work of Charles Dufay to illustrate this mutual reinforcement.

to natural philosophy, and specifically about the tendency of earlier historians to discuss an abstracted 'natural philosophy', removed from any practical context.[36] By looking at the *practices* of natural philosophy as well as its theories, systems and concepts, Schaffer has changed how we view many parts of the field. Examining practices also allows us to think about the relationship between natural history and natural philosophy; classification is one such practice that can be used in this way. Classification, as I have already mentioned, was a very important activity for many natural historians, and Schaffer (in his discussion of practices) points out that it was also central to the work of many natural philosophers.[37] He cites examples from eighteenth-century botany, zoology, chemistry, electricity and psychology to show how the concepts of speciation and classification pervaded many fields. Closely related to speciation and classification are a range of activities which Schaffer groups together under the heading of "fact-gathering" – these practices were common to people who considered themselves natural historians and those who thought of themselves as natural philosophers. One of Schaffer's best examples of crossover between what some rigidly think of as natural history and natural philosophy is found in his work on William Herschel (1738-1822).[38] Herschel is known for his astronomy and is generally discussed in relation to other astronomers and those working in the physical sciences. But Schaffer, using Foucault's idea of the discourse, stresses Herschel's identity as a natural historian. He points out that Herschel's first scientific paper at the Bath Philosophical Society in 1780 was on the problem of classifying corals, and goes on to show how Herschel used many ideas usually associated with natural history in astronomy – a branch of knowledge generally considered to be part of natural philosophy. So we see that it is possible to use scientific practices to broaden our view of natural philosophy and natural history so that the two fields appear less distinct.

The problem of classifying corals (which illustrates how a practice commonly associated with natural history shaped Herschel's astronomy) can also be used to show how ideas that many historians see as natural philosophical shaped natural history. Corals are difficult to classify because they seem to lie on the border of the animal and plant

[36] Schaffer [1980a] 55-6, 72.
[37] Ibid. 85.
[38] Schaffer [1980b].

kingdoms. In my research, I have found such problematic boundary objects especially useful in showing how different scientific fields overlapped. The difficulty of understanding a controversial object often led naturalists to use a wide array of methods and theories, and so gives a particularly clear-cut example of how the practices and ideas of natural history, natural philosophy and other sciences interacted. The status of such objects in natural history has been discussed by Susan Leigh Star and James R. Griesemer in the case of a twentieth-century museum, but their work is also applicable to eighteenth-century natural history studies. For Star and Griesemer, a boundary object is one that can "inhabit several intersecting social worlds ... and satisfy the informational requirements of each of them", they are adaptable enough to serve many functions in many contexts, but robust enough to maintain a common identity across boundaries.[39] Corals and other objects from the plant/animal boundary were used in a variety of contexts by eighteenth-century men of science: they were used to answer philosophical questions about the differences between the kingdoms; they were used to answer practical questions about classification; they were the subject of experiment in laboratories, and of observation on the sea-shore; they had identities that straddled both the natural historical and natural philosophical realms, and they can be used to show how closely related these fields really were. Here, I use such objects to describe what the relationship between these fields was, how its effects were seen and felt, and what it meant for natural history. I show that the relationship between natural history and natural philosophy was not simply one in which one discrete 'discipline' created facts for the use of the other.

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My story is not confined to dusty museums or rigid systematic gardens. We see this natural history in public coffee houses and private meeting rooms, through the eyes of a cloth-merchant on an English sea-shore, behind the tall hedges of an Edinburgh garden, in the cabinets of wealthy London collectors, on the workbench of a famous chemist, in the creation of a gardener asked to turn his hand to carpentry on a country estate, on the Siberian tundra and in the Welsh countryside, in a lecture hall that smelled of burnt coralline, by a mint plant grown upside-down, in a west-end gallery, and in bell jars filled with campion flowers. The breadth of this natural history is reflected in the

[39] Star & Griesemer [1989] 8.

range of characters, locations, practices and objects whose stories are told in this thesis. Though these elements are diverse and may seem unrelated at first glance, I will show how this broad natural history linked them together. The six chapters which follow are designed to highlight some of the key activities of natural history that have been neglected by other historians. I use these neglected areas to deal with the more general questions I have raised in this introduction – what was natural history, how did it function, how did it relate to the other fields of scientific knowledge, what was a natural historian, what happened to the natural history of the eighteenth century and why is today's natural history so different?

The setting for this story is Britain and, more particularly, England. There are several reasons for choosing to look at English natural history in the eighteenth century. One is that certain aspects of it have been almost entirely overlooked by historians of natural history; the use of theory and experiment in natural history is far better documented for other European countries as we see in the cases of naturalists like Georges Louis Leclerc, Comte de Buffon, Carl Linnæus, Abraham Trembley, or the Jussieus. Roy Porter's work has ensured that the theoretical and experimental histories of British geology are well understood, but zoology and botany have received less attention. James Larson's work addresses the complexities of eighteenth-century studies of life in France and Germany but he has pointed out that there have been few comparable studies outside those countries. He believes that in the English-speaking world, most studies of pre-nineteenth-century life sciences have been "anchored in the Darwinian synthesis". He further believes that examinations of the period 1740-1790 have failed to deal with the problems actually facing practitioners but have been used as a "quarry" for establishing the origins of 'biology'.[40]

But a more compelling reason comes not from what historians have and haven't documented, but from the very nature of the science in Britain. All eighteenth-century natural history was idiosyncratic to a certain extent: nowhere was it a formal discipline with set training or career structures or much prospect of providing a livelihood, but British natural history seems to have been especially individualistic. Compared to Sweden which had a singular figurehead in the form of Linnæus and an economic programme in botany, Germany where cameralism was an important consideration, or France where the

[40] Larson [1994] 1-5.

Royal gardens and the *Académie des Sciences* provided a reasonably formal structure for the science, British naturalists had far fewer formal strictures in place.[41] Even Scotland, which had far closer links to French science through its medical schools than England did, never had a formal route into natural history. Perhaps the most unifying force in British natural history was natural theology, but natural theology acted as a justification for undertaking research rather than a strict framework requiring particular methods to be used, particular objects to be studied, or particular results to be obtained. An obsession with classification is sometimes seen as another unifying force of British natural history, but here I show that while classification was important, its importance was deliberately exaggerated towards the end of the century after the founding of the Linnean Society. The informality of British natural history, coupled with the fact that it never revolved around a single figure, group, school, institution or philosophy, allowed a wide variety of practices to flourish there. And this is what makes it an ideal place to see the true breadth of natural history. I have chosen to study the latter part of the eighteenth century as it is often seen as a time when a simple transition from 'old-fashioned' natural history to 'modern' biology began to take place; but the research I present in the following chapters shows that such a straightforward trajectory never existed.[42]

The first chapter deals with the question of the end of this wide-ranging, open-minded, experimental, theoretical natural history. I argue that this once broad field began to diverge into different natural histories, some of which were not compatible with each other. The natural history that was to become dominant (and from which the natural histories of the nineteenth and twentieth centuries were derived) was one that grew out of the classification system of Linnæus and was formalised by the Linnean Society of London (established 1788). The Linnean Society set out to homogenise British natural history and in the process created a new definition of the field. Its definition did not include the kind of speculative theories, philosophising or experimental work that this thesis describes; rather, its view of 'scientific' natural history rested on naming and grouping objects according to the Linnean rules.

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[41] For more on Sweden and Linnæus see Koerner [1999] and Müller-Wille [2003]; for more on German cameralism see Lindenfield [1997] chapter 1; for more on French naturalists see Spary [2000].

[42] For more on the transition from natural history to biology, see Nyhart [1996] 426-443.

I then turn to the idea of crossing supposed scientific boundaries; the second chapter revolves around zoophytes (those creatures that, like coral, lie on the plant/animal boundary) and how they were studied in Britain. The principal character here is London-based naturalist John Ellis who, having failed to establish whether certain creatures were animals or plants using the classical ideas of Aristotle, turned to chemical analysis for an answer. I use Ellis's work as a way to discuss the overlaps of natural history and chemistry, as well as the existence of experimental natural history. The controversy over zoophytes forced practitioners to consider methods and theories more carefully than they might otherwise have done and so gives a useful window into the work of the naturalist. This case study also shows how theory and practice were combined in natural history and emphasises how easily these theories and practices moved between seemingly different branches of science. This chapter also contains a section on the problem of classifying these boundary creatures and the problem of creating a 'natural' order, thus showing how well-recognised natural historical activities such as classifying fit with the natural historical practices I am describing.

I continue the discussion of problematic boundary objects in the third chapter, and show what such objects reveal about natural history. This time, the objects are sensitive plants, and I use them to illustrate how theories that most historians associate with natural philosophy were also used to answer questions in natural history. I present case studies of men such as Thomas Percival, Robert Townson, James Edward Smith, James Perchard Tupper and Thomas Andrew Knight who investigated things like plant motion and apparent plant sensitivity with different observational and experimental methods, and tried to explain them using various mechanical and vitalist theories. This chapter shows that natural history was not just a descriptive science; it could also deal with causes and explanations. Again it shows the fluidity of natural history in this period and the lack of a clear boundary between it and other sciences.

In the fourth chapter I present a case study about the sexes of plants; the controversy about whether or not plants can be male or female gives further evidence of the kinds of experimental and theoretical evidence that naturalists used to explain the world around them.

Next, I move away from the difficulties of dealing with animals that behave like plants, and vice versa, and turn to the problem of identifying naturalists and understanding their working lives. The fifth chapter tells the story of how an Englishman might go about becoming a naturalist in the decades around 1800. It is based around case studies of the author, artist, collector and museum-owner Edward Donovan, collector Alexander Macleay, and lecturer and museum curator George Shaw. This was a time before natural history was an established profession and yet, due to a variety of social, political and economic factors, it was a time when many people were drawn to natural history and hoped to make a living from its pursuit. By following the careers of Donovan, Macleay and Shaw I show the multitude of natural historical activities in which they could engage.

The final chapter looks outward from Britain to see how naturalists in different locations shared their work and ideas, to see how European natural history travelled into Britain and how Europeans received British natural knowledge. Communication involved not just letters but also books and specimens; it was a costly and time-consuming business. Here, I look at what benefits such communication conferred on participants and show the mechanisms by which particular people such as Thomas Pennant interacted with their European counterparts. I also discuss the use of a key French natural history book – Buffon's *Histoire naturelle* – by British naturalists.

These six chapters reveal a natural history that other histories of the field do not. They show a broad science that did not see itself as subservient to natural philosophy; they show a diverse range of practitioners who did not feel they had to operate within strict boundaries of a clearly-defined discipline; they show how natural historians freely used techniques and theories now commonly associated with other sciences; they show a natural history that was compatible with well-known activities such as collecting, displaying and grouping objects, but that was also at ease with trying to understand and explain those objects. This natural history did not simply segue into the natural history of the nineteenth century; rather, it was deliberately marginalised. But elements of it came to be incorporated into other fields and much of it lived on under the new name of biology.

CHAPTER ONE

The Linnean Society of London and the end of eighteenth-century natural history

> "Permit me to take this opportunity to congratulate you on the effects which your *Systema* has had among followers of natural history here in London, the number of which, altho' not equal to those found in many other countries are yet every day increasing to such as degree as could not have been expected a little time ago by its most sanguine well wishers".
>
> Dru Drury to Carl Linnæus, 30th August 1770[43]

Eighteenth-century natural history did not have clear boundaries. It was not a distinct discipline. It overlapped with many related fields such as medicine, natural philosophy, chemistry and physiology; practitioners freely moved between these different fields of knowledge, sharing both ideas and methods. But towards the final years of the eighteenth century, and into the beginning of the nineteenth, this began to change. In this chapter I will tell the story of how and why that happened. I will begin this story at its end; for the end of this broad natural history, and its replacement by something quite different, has shaped our perception of the science. The period of change at the close of the eighteenth century highlights some key elements of these two kinds of natural history by showing what was distinct about them, and why they might have come into competition. Here, I will examine a force that played a major role in the end of the broad natural history of the eighteenth century and inspired the rise of a narrower British natural history – the Linnean Society of London.

The Linnean Society became the most famous, most well organised, most influential and, arguably, the most important group of naturalists in eighteenth-century Britain. It was a group founded for the improvement, promotion and preservation of British natural history, and one that also had the power to shape it. The society was established in 1788 and took its name from the Swedish naturalist Carl Linnæus (1707-1778). Earlier in the century, Linnæus had proposed the idea that plants, like animals, had male or female parts and that these parts were so essential to the nature of plants that

[43] Drury to Linnæus letter L.4392.3.312-313, the Linnean Society Archives.

they could be used to classify the whole vegetable kingdom in a logical order. From that idea sprang the Linnean system of classification. This system and its spin-offs would go on to dominate most European discourses on natural history for the rest of the century. Such was the reputation of both Linnæus and his system that almost no-one questioned the propriety of what was essentially the British national academy for natural history adopting a Swedish naturalist as its inspiration. With the foundation of the Linnean Society had come a new sense of stability and continuity in British natural history. Before its foundation, many smaller groups of scholarly naturalists had existed and often struggled to keep hold of members and resources. But the new society - being built around the material centre of Linnæus's own library and collections, creating a definition of 'scientific' natural history, standardising terminology, providing a controlled platform for the announcement of new discoveries and a reliable means of preserving them for future generations - brought an austere formality to British natural history. With this formality came stability. The fellows of the Linnean Society believed that maintaining this stability was the best way to advance their science. In order to achieve this, they had to preserve the foundation on which the society was built – the reputation of Linnæus and his sexual system of classification.

This chapter covers the kind of natural history practiced by the Linnean Society of London; in later chapters, I will contrast this to other styles of natural history which had preceded it. I begin with a discussion of the Linnean Society's history and pre-history, covering the rise of the Linnean system, natural history and natural history clubs in Britain before moving on to the foundation of the new society and its early days in Great Marlborough Street. I then use the society's aims, its members, meetings, journal, correspondence and private visits between fellows to build a picture of what the society did, how it functioned, and what place it occupied in British natural history. While there have been some studies of the Linnean Society, they tend to focus on the biographies of its more famous members, or on general surveys of natural history societies of the time; one or two more analytical studies have been undertaken recently such as Paul White's work on the importance of the Linnean collections within the society.[44] I hope to fill a

[44] The standard account of the history of the Linnean Society is Gage and Stearn [1988], for information on other natural history societies of the time, see Allen [1987], for more on clubs in general, see Clark [2000]. White [1999] 126-129.

gap in the historiography by considering the natural history of the Linnean Society as a deliberately distinct science from earlier natural histories and by using Linnean natural history as a foil that emphasises many of the features of those earlier natural histories. I will argue that the fellows of the Linnean Society deliberately narrowed the scope of natural history in order to ensure their success and will show why the Linnean struggle against these earlier kinds of science would shape the field of natural history throughout the nineteenth century and beyond.

The desire for a 'scientific' natural history, and the Linnean sexual system in Britain

Linnean taxonomy lay at the centre of the Linnean Society. Though taxonomy was an important activity in eighteenth-century British natural history, it had never been the only one. But with the rise of the Linnean Society more and more attention became focussed on it. Arranging plants and animals into groups and naming them according to Linnean principles was the single most important activity for the fellows of the society. In the presidential address given at the first official meeting, James Edward Smith (1759-1828) defined 'scientific' natural history as being dependent upon taxonomy.[45] A natural history that did not rely on taxonomic rules was not really a natural history worth doing, according to Smith and his fellows.[46]

The desire to appear 'scientific' may have stemmed from comparisons between the mathematical sciences and the life sciences which had caused controversy in the Royal Society just a few years before. There, criticism had been levelled at Sir Joseph Banks (1743-1820) that under his leadership the Royal Society was neglecting serious mathematical and physical science in favour of frivolous butterfly-collecting. Tension between Banks and some fellows of the Royal Society had existed since he had first become its president; some of this could be attributed to his wish that new fellows be admitted for their social standing as well as their philosophic abilities, but some was due to disquiet that a naturalist should preside over a society that had once been led by Newton. In 1783-4 this tension escalated into a series of hostile exchanges between the

[45] Smith [1791a] 54.
[46] For more on the analysis of public statements made by scientific societies, see Miller [1986] 231-2 in relation to the Geological Society of London.

sides: Banks's opponents accused him of being an "amateur"; of trying "to amuse the Fellows with frogs, flees and grasshoppers"; of making the Royal Society "a cabinet of trifling curiosities"; and of supporting botany – "a study to be preferred above all others, as it furnishes the whole human race with amusement delicious, without either wasting the spirits, or hurting the brain".[47] But Banks's supporters outnumbered his critics and in 1784, following a campaign by some of the society's mathematicians to have him removed, a motion of confidence in Banks's presidency was passed by 119 votes to 24.

Aware of this incident, naturalists such as Smith became anxious to give a more rigorous appearance to their studies; and so they turned to classification. For them, classification was, like the mathematical sciences, a rational activity that sought order in the natural world through the application of logical rules. Because earlier British natural histories had used a variety of methods to achieve their various aims, they appeared less like the mathematical sciences than classification which could, for some practitioners, be reduced to a formula used to determine kingdom, class, order, genus and species. Classification also had the advantage of deflecting attention away from some of the more troublesome areas of natural history. The contents of natural history could give rise to speculation and philosophical questions about such loaded topics as the meaning of life, the relationship between God and the natural world, and the existence of animal, vegetable or mineral souls. While European naturalists were often willing to engage with such questions, British naturalists were wary of them. Classification was a practice that circumvented much speculation: once a classification system was accepted as useful, practitioners did not have to think about its underlying principles if they did not wish. Ideas about classification varied hugely between naturalists and across national boundaries and the Linnean Society's approach reflected many national concerns. The emphasis on Linnean sexual classification was distinctly different to the French approach. In France, naturalists mostly preferred the 'natural method' – i.e. one that took as many characteristics of the object being classified into account as possible – or agreed with Buffon that classification systems were a distraction from more important elements of natural history.[48] Tensions between Britain and France were running high in the closing

[47] Gascoigne [1994] 10-13, 62. For an account of tensions between Royal Society mathematicians and naturalists in the earlier part of the century, see Feingold [2001].
[48] Spary [2000]198-202; Farley [1982] 11; Schiebinger [1993] 28; Smith [1791] 38, 47; Roger [1997] 360, 479.

decades of the eighteenth century, as Britain looked warily at the social and political turmoil that presaged a revolution. English naturalists were keen to characterise a national natural history through their emphasis on classification and through deliberate avoidance of French-style speculation.

If the Linnean Society wished to advance British natural history in a particular direction, it needed stability. This stability could be drawn from its use of a universal taxonomic system; once that system was accepted, much controversy could be eliminated from discussions among naturalists. In order to further bolster the status of this system (and so increase stability), competing ways of doing natural history had to be suppressed. Some elements of natural history, particularly those that involved speculating, theorising, hypothesising or new experimental methods were likely to cause controversy; with controversy would come debate, in-fighting and splintering. If the Linneans could limit natural history to taxonomy and related areas such as naming, they would avoid many of these problems and ensure that natural history had a stable base in Britain. The concern with stability came about for a few different reasons: one was the break away from the Royal Society; another was the knowledge that many earlier natural history clubs had failed; and a third was that as the sciences began to separate out into nascent disciplines, they needed some kind of coherence in order to avoid confusion and excessive splintering.

The work that the Linnean fellows drew their much-desired stability from was Carl Linnæus's *Systema Naturæ* - a book that would shape natural history in Europe for the best part of a century. This short book took a simple idea and expanded it to create a new basis for natural history. Classifying natural objects was already an important part of natural history at this time, as were artificially-constructed classification systems, but Linnæus took this further. His idea was to reduce a plant to one classifiable characteristic – its flower – and to arrange the entire vegetable kingdom around this feature. Linnæus did this by identifying parts of the flower which he, and some others[49], claimed were responsible for reproduction, by then using analogies with the animal kingdom to label these 'female' and 'male', and finally by dividing the vegetable kingdom into groups based

[49] These others include English, French and German naturalists such as Thomas Millington (1628-1704), Nehemiah Grew (1641-1712), John Ray (1627-1705), Sebastien Vaillant (1669-1722) and Rudolf Jakob Camerarius (1665-1721). For more on naturalists who disagreed with this theory, see chapter four of this thesis.

on the number and arrangement of these parts in different plants. For Linnæus, the most important parts of the flower were the pistil (which he considered female) and the stamen (which he considered male).

Here I wish to look specifically at how Linnæus's sexual system of classification was received in Britain. Though some parts of the theory were contentious, many were readily accepted. Perhaps the most controversial aspect of the sexual system was the way in which Linnæus had framed it: he described relationships between flowers in human terms – Linnæus's flowers could love, court, marry, and even engage in clandestine affairs. By describing so-called marriages (often between one wife and several husbands) in some detail, Linnæus scandalised many. Even those who believed in the system often felt the need to tone down its metaphors. William Withering (1741-1799) in his *Botanical arrangement of British plants* wrote that he intended to downplay the sexual part of the system for the benefit of any ladies who might be reading:

> From an apprehension that Botany in an English dress would become a favourite amusement with the Ladies, many of whom are very considerable proficients in the study ... it was thought proper to drop the sexual distinctions in the titles to the Classes and Orders.[50]

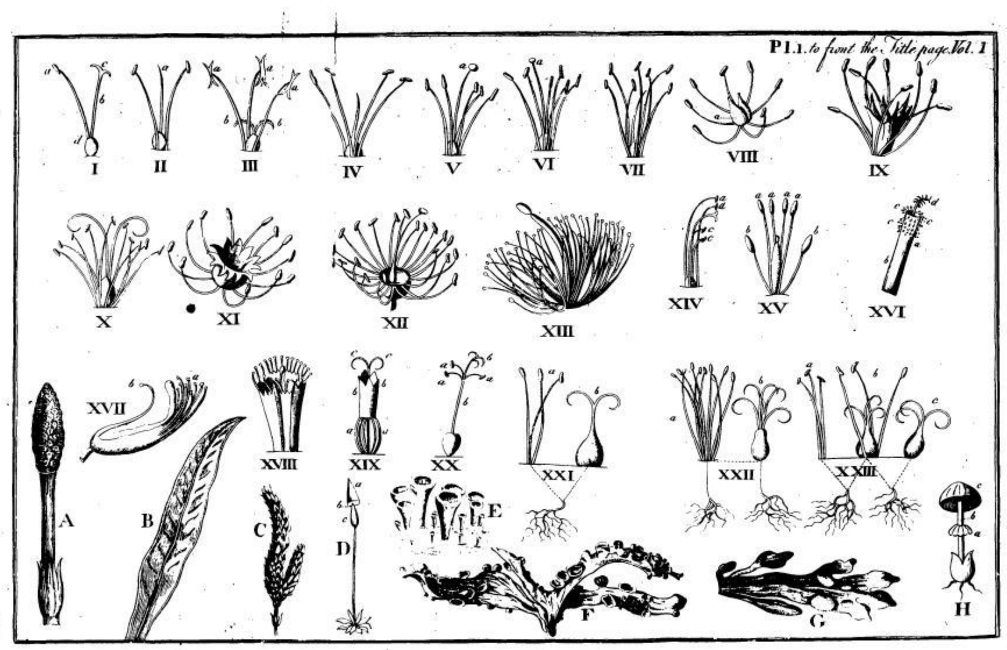

Illustration from William Withering's 1787 *A botanical arrangement of all the vegetables naturally growing in Great Britain* showing the Linnean arrangement of plants according to the number of stamens and pistils. The first class had one stamen, the second had two and so on; these were then subdivided into orders based on the number of pistils. Image courtesy of ECCO.

[50] Withering [1787] xv.

Likewise, the Reverend Samuel Goodenough (1743-1827) was a firm supporter of the Linnean classification system but he made several moral objections to Linnæus's language.[51] A very small number of British naturalists actively embraced both Linnæus's system and his racy language; most famous of these was Erasmus Darwin (1731-1802). In 1789, Darwin published his poem *The loves of the plants* in which he dramatised Linnæus's system and described plants as though they were people engaged in love affairs; this poem, faithful to many of Linnæus's ideas and metaphors, was also controversial.[52] Müller-Wille has argued that the racier elements of Linnæus's system may have contributed to its popularity and success, though perhaps this is more true for Europe than it is for Britain.[53]

But language and imagery were not the only controversial parts of the sexual system; for some, its artificiality was a much larger philosophical problem. In England, naturalists were often happy to ignore philosophical issues. Many of the authors who translated or interpreted Linnæus for a British audience were explicit about this: Withering wrote in the preface to his Linnean arrangement of British plants that "all controversies about system are here studiously avoided. Mankind are weary of such unprofitable disputes"; while John Berkenhout (1726-1791) who wrote an English-language lexicon of Linnean botanical terms also explicitly ignored the philosophical aspects of Linnæus's system.[54] Outside Britain, naturalists were less forgiving about the artificial nature of the sexual system; the system was particularly unpopular in France where desire for a more natural system was far stronger. But although the artificiality of the system was seen as a great weakness by many of the more philosophical naturalists, it was perhaps the system's greatest strength in the eyes of the practical naturalist. In the eighteenth century, natural history became increasingly popular across broad sections of society; it began to open up more to the middle and working classes and to women – i.e. to those without the benefit of wealth, a university education or proficiency in Latin and Greek. For these people, who may not have been able to procure or understand many of the key natural history texts of the time (which were frequently expensive, written in Latin, or both), the Linnean system was an accessible route into natural history. Its

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[51] Schiebinger [1993] 30.
[52] Darwin [1789]; Browne [1989] 596, 600.
[53] Müller-Wille [2007a] 268.
[54] Withering [1787] xv; Berkenhout [1764] preface.

simplicity was its key strength; in order to understand and classify the entire vegetable kingdom, a naturalist simply had to be able to count the number of stamens and pistils in a flower. The basics of the system were straightforward enough to be explained in short, cheap pamphlets and field-books, while books such as Berkenhout's lexicon allowed research to be carried out in English. So while the system may not have given a true representation of nature, and Linnæus himself freely admitted this, it had many practical advantages and quickly gained popularity in Britain. Towards the end of the century, many naturalists happily reflected on its near-universality: Withering declared that "the system of Linnæus is now very universally adopted", while James Edward Smith wrote that Linnæus's system "is really in many respects more agreeable to nature than many which had preceded it, and which, for facility and universality, has a decided superiority over all hitherto invented".[55] And even Linnæus's detractors had to admit that "there is not a notion more generally adopted than that vegetables have the distinction of sexes".[56]

Organisation and aims

The utility and accessibility of Linnæus's sexual system of classification and the growing popularity of natural history led to the formation of many clubs and societies dedicated to the practice and promotion of this branch of knowledge. Many such groups sprang up around Britain from the late seventeenth century and throughout the eighteenth, their numbers increasing even further after the 1760s – something that David Allen attributes to the spread of Linnean ideas.[57] Most of these early natural history societies had quite short life-spans, perhaps due to their informality, lack of funding, or shifting tastes in natural history. Examples of the early clubs include Temple Coffee House Botanic Club (founded c.1689, this is the oldest known natural history club in London) and many other small coffee house clubs, the Botanical Society (1721-1726), the Aurelian Society (which ceased to exist in 1748 when a fire destroyed all of its collections, records and library), the Society of Entomologists of London (1780-1782), and the

[55] Withering [1787] xv; Smith [1791] 29.
[56] Smellie [1790] 245.
[57] Allen [1987] 245.

Society for Promoting Natural History, founded in 1782.[58] Many had quite general aims and hoped to provide a meeting space for naturalists and to encourage interest in the science while some, such as the Lichfield Botanical Society, had very specific aims. The society in Lichfield consisted of just three members – Erasmus Darwin, Brooke Boothby (1744-1824) and William Jackson (1735-1798) – and its only substantial project was to support Darwin's translation of Linnæus's *Systema Vegetabilium*.[59] None of these groups tended to survive for more than a few years. But one society survived, flourished, and continues to meet today – this is the Linnean Society of London. This society was founded in 1788 and Allen credits its success to its novelty: it had much grander ambitions than any other natural history society of the time; it was intentionally international; it was based around a specific library and collection; and it published its own journal.[60] The Linnean Society came to represent continuity and stability and, as we shall see, its ability to attract new members and survive beyond its founders was unique amongst British natural history societies.

The founding members of the Linnean Society first met at the Society for Promoting Natural History. But they were dissatisfied with several aspects of that society, such as its particular interest in fossils at the expense of other parts of natural history and its officiousness; informing Smith (who was travelling in Italy) about what was happening at the society, Goodenough wrote:

> The present society goes on in the usual way, of having a fossil or a plant go round the table: nothing is or can be said upon it. It is referred to a committee to consider of it; the committee call it by some name, and send it back to the society. The society desire the committee to reconsider it: the committee desire the society to reconsider it. In the mean time nothing is done; indeed it does not appear to me that any of them can do anything.[61]

The old society's inability to form a library due to lack of funds and its reputation for heavy drinking were also sources of dissatisfaction.[62] This, in part, created the impetus to found a new natural history society; a second important impetus for establishing a new society was James Edward Smith's purchase of the Linnean collections. Smith was born in Norwich in 1759, the son of a wealthy merchant. Norfolk at that time was an active

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[58] Ibid. 244-5; Gage and Stearn [1988] 1-4.
[59] Uglow [2002] chapter 32.
[60] Allen [1987] 245.
[61] Goodenough in Smith [1832] 214.
[62] Gage and Stearn [1988] 4, 6.

centre for botany and horticulture and the young Smith was acquainted with many followers of Linnæus such as Hugh Rose (c.1716-1792). In 1781, Smith moved to Edinburgh to study medicine and began attending the botany lectures of Dr. John Hope (1725-1786) – one of the earliest teachers of the Linnean system in Britain.[63] While in Edinburgh, Smith founded a natural history society with some friends.[64] Following his time in Scotland, Smith went to London in 1783 where he studied under John Hunter (1728-1793) and William Pitcairn (1712-1791) and was introduced to Sir Joseph Banks by Hope.[65] It was through Banks that Smith came to own the Linnean collections; the two were breakfasting together when Banks received a letter offering the collections for sale. Banks himself did not wish to purchase them, but suggested that Smith might benefit from owning such a collection. With a loan from his father, Smith purchased the collections for £1088.[66] These collections consisted of an array of books and objects collected by Carl Linnæus himself. The largest part of the collection was botanical, containing over 14,000 plant specimens, but it also contained thousands of fish, shells, insects, 1600 books and over 3000 letters and manuscripts.

The collections provided a physical centre around which a new society could form. Considering the provenance of the collections, it made sense that the society would focus principally on promoting a very Linnean kind of natural history. Paul White has written that the collections formed a social and material basis from which to build the Linnean system in Britain.[67] And so, between their arrival in London and the dissatisfaction that Smith and others felt with the Society for Promoting Natural History, the desire for a new natural history club began to make itself more and more apparent. Smith began to discuss this possibility with two other fellows of the Society for Promoting Natural History – Samuel Goodenough and Thomas Marsham (d.1819) – and in February 1788 (after Smith had returned from a tour of the continent and further medical studies in Leiden) the three called a meeting of seven naturalists and declared the

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[63] Smith, a Unitarian, was unable to enrol at either of the English universities.
[64] Allen [1978].
[65] Though Banks became an honorary fellow of the Linnean Society, he in fact favoured French attempts at natural classification, particularly that of Antoine Laurent de Jussieu. See Gascoigne [1994] 106-118 for more on Banks's views on classification. The Linnean Society was unique in securing Banks's support – he opposed the formation of other specialist societies such as the Geological Society of London and the Astronomical Society of London for fear that they might encroach on the activities of the Royal Society, see Miller [1986] 235.
[66] Boulger [2004]; Gage and Stearn [1988] 4-5.
[67] White [1999] 126.

founding of the Linnean Society.[68] From this initial small meeting in a coffee house, the society soon grew larger and began to meet at Smith's own house on Great Marlborough Street where the Linnean collections were also housed.

In an introductory address given on 8th April 1788, Smith discussed the aims and purposes of the society. As well as generally promoting natural history as the most basic and useful science, Smith wished to promote Linnæus and his system. Smith credited Linnæus with bringing about a "golden age" in natural history through his classification system and his desire to define species based on "philosophical principles"; Smith wished to continue this golden age.[69] He believed he was living in a time that was "one of the most propitious to the study of nature, on the most solid and philosophical principles" and that a new society could capitalise on these fortunate circumstances for the improvement of natural history (and especially botany) in his country.[70] A particular strength of a society, said Smith, was its inherent capacity to foster cooperation between individuals, and he hoped that the fellows of the Linnean Society would harness this in the furtherance of their science. He wrote that

> all who pursue the same studies should labour together for the common good: every degree of assistance, every deserved commendation which they give to each other, is the most probable means of advancing their own fame; while every atom of usurped honour, if it does not immediately cover its vain possessor with opprobrium, is almost certain to be deducted with interest from his character by a discerning and impartial posterity.[71]

Other stated aims of the society were: advancing understanding of British plants and animals (which Smith claimed were sadly neglected when compared to foreign specimens); providing a single central forum for presenting new knowledge of natural history; creating a specialised space just for natural history (Smith complained that the Royal Society was too broad in its scope and that this necessarily led to the neglect of his favourite study); and preserving research in a journal in order to "prevent all the pains and expence of collectors, all the experience of cultivators, all the remarks of real observers, from being lost to the world".[72] Two final aims were particularly important to

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[68] Boulger [2004]; Gage and Stearn [1988] 6-7; the four other naturalists were Jonas Dryander (1748-1810), James Dickson (1738-1822), John Beckwith (dates unknown) and John Timothy Swainson (1756-1824).
[69] Smith [1791a] 29-31.
[70] Ibid. 50.
[71] Ibid. 51.
[72] Ibid. 52. On the Royal Society's view of natural history, see Gascoigne [1994] 7-14.

the identity of the society; the first was its very Linnean approach to natural history - Smith wrote that "nothing will be with more reason expected from the members of this society than a strict attention to the laws and principles of Linnæus", and this certainly fits with White's claim that Smith and his colleagues promoted the Linnean system "like a religious orthodoxy".[73] The second especially important aim of the society, said Smith, was to distinguish between 'scientific' and 'non-scientific' natural history. Smith said that he hoped

> never to see any descriptions sent into the world by this society without specific differences; they are what distinguish a true scientific naturalist from an empiric, and nothing but incapacity in an author can make us pardon the want of them. Without a strict attention to this maxim, the science will soon relapse into its original barbarism, nor can any thing but another Linnæus restore it.[74]

Specific differences are crucial in Linnean classification, so Smith was essentially identifying 'scientific' natural history with Linnean natural history and claiming it as the particular province of his new society.[75]

So the Linnean Society of London was founded explicitly for the cultivation and dissemination of 'scientific' British natural history of a Linnean bent. Smith's address gives a grand overview of the history and state of natural history and outlined many high-minded aims, but it gives little idea of the day-to-day business of the society and its fellows. To get a better idea of how the society actually functioned and what it really did, it is worth looking at some of its members and their activities. A membership list appeared in the first volume of the *Transactions of the Linnean Society* and shows over 100 members. Of these, 50 were regular fellows, 56 were foreign members, 24 were associates and three were honorary members. Even a quick glance at the list reveals much about the make up of the society. Of the 50 fellows, 20 were also fellows of the Royal Society, 10 had medical degrees, nine were clergymen and two had aristocratic titles: the society was intended for the well born and the well educated. A contemporary reviewer was impressed by this membership list; he supported his effusive praise for the work of young society by referring readers to the articles it published and "the list of the members who

[73] White [1999] 128.
[74] Smith [1791a] 54.
[75] Linnæus's philosophical works (such as *Philosophia Botanica*) were also held in high regard by the Linnean fellows, but were rarely, if ever, discussed or analysed in their written works.

adorn it".[76] But there is one strange omission for a society wishing to promote *British* natural history: the fellows were drawn from the length and breadth of England and Wales, but the Scots were barely represented. Archibald Menzies (1754-1842) was the only Scot listed as a fellow (though there were also four Scottish associates). The large number of foreign members indicates that the society wasn't aiming to be exclusively English, and yet, despite the existence of many active naturalists in Scotland and despite Smith's links with Edinburgh, there always remained a tension between Scotland and the Linnean Society. There were also some notable British naturalists who chose to exclude themselves from the society. The highly regarded naturalist Thomas Pennant (1726-1798) declined Smith's repeated invitations to join the new society, ostensibly on account of his age, though perhaps also for other, unstated reasons. Pennant's determination not to become a Linnean fellow was a great disappointment to Smith who wrote:

> I regret very much your determination respecting our Society & yet know not how to help it. Your ideas are too just for me to controvert them – we alas are the only losers! – I still indulge a gleam of hope – in the mean time shall keep the matter to myself.[77]

The list of foreign members tells us something about the links that London naturalists such as Smith were cultivating across Europe (many of those on this list were people that Smith had met while on his tour of the continent[78]). Of these foreign members, five were also members of Swedish natural history societies, 10 had connections to Paris, five to Montpellier, six to Switzerland and 12 to Italy. The number of Swedish members suggests a reasonably strong relationship with Linnæus's homeland, while the French members (especially those linked to Montpellier, famous for its medical school) highlight the links between natural history and medicine. The same reviewer who praised the society's membership list also drew attention to the international nature of the society and noted with approval how many of Linnæus's disciples were fellows.[79]

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[76] Anon. *Monthly Review* [1792] 166.
[77] Smith to Pennant, 5th September 1791, letter # CR2017/TP364.1, Warwickshire County Archive; Pennant to Smith, 10th August 1791, letter 8.33, general correspondence, Linnean Society Archives.
[78] Boulger [2004]; Gage and Stearn [1988] 13.
[79] Anon. *Monthly Review* [1792] 172-3.

Meetings and *The Linnean Transactions*

The two principal ways in which these fellows, associates and foreign members visibly fulfilled their functions within the society were through its formal meetings, and through its publications. Another important aspect of the society was its fostering of correspondence networks and a final, less formal, function of the society was to facilitate introductions between its members so that they might meet and work together for the advancement of natural history.

The Linnean Society held two kinds of meeting: general meetings were open to all members and their guests and took place on the first Tuesday of each month, and fellows-only meetings occurred on the third Tuesday of each month – both were held in the early evening at Smith's house on Great Marlborough Street. The fellows-only meetings were largely administrative; it was at the general meetings that the business of disseminating natural history research was done. Gage and Stearn have described the austerity of these meetings: pre- and post-meeting socializing was not encouraged, members simply arrived, listened to a paper being read aloud for about an hour, and then departed.[80] There was no time given for asking questions or discussing the contents or merits of the papers. In part, this format was adopted as a way to minimize controversy within the society and to eliminate the negative effects of infighting that had plagued many other societies.[81] Perhaps this sense of reserve and decorum, although it may appear at first glance to have had a stultifying effect on the advancement of natural history, played a role in the success of the Linnean Society. Peter Clark's wide-ranging survey of English-language clubs and societies in the eighteenth century (and he estimates there were about 25,000 in total, ranging from political and religious groups to temperance societies to groups dedicated to the arts or music, merchants' clubs, cock-fighting clubs and at least one ugly face club) reveals the importance of sociability to the functioning of many of these groups. Most clubs and societies met in coffee houses or taverns, or centred their meetings around food and drink; this tended to add a degree of informality to the proceedings, and Clark has drawn a link between the informality and the short life-spans of particular societies. As mentioned above, the Society for Promoting Natural History was criticised because of its members' proclivity for heavy

[80] Gage and Stearn [1988] 14.
[81] Clark [2000] 234; Gage and Stearn [1988] vi.

drinking; that social element was seen as a distraction from the society's scientific business and a possible reason for its demise.[82] As groups became more formal from the 1780s on, they tended to become more stable too.[83] By ensuring that its meetings were strictly business, and by meeting in Smith's own house, the Linnean Society avoided many of the problems of less formal societies: its formality was an important factor in its longevity.[84] Gage and Stearn admit that a history of the Linnean Society may appear "unexciting" and "lethargic" but insist that Smith and the fellows never aimed for sensation or controversy, and that there is still much of interest in the society's history.[85]

Apart from the meetings, the Linnean Society's most visible manifestation was its journal – *The transactions of the Linnean Society* – which was first published in 1791. This journal, issued annually, was a high quality publication containing a large number of plates, many of them coloured. It was very well received: both *The critical review* and *The monthly review* carried positive reviews not just of the journal but of the society and its mission to promote natural history more widely. *The critical review* noted approvingly that "almost every article in this [first] volume is illustrated with a plate, and sometimes with two or three, always very clearly and accurately engraved ... On the whole, this first volume appears a very interesting and useful one".[86] While *The monthly review* praised "so respectable a volume", singled out particular authors for commendation (for example, Smith's introductory discourse was complimented for its richness, its utility and for using language suited to "a gentleman and a scholar") and concluded that the publication as a whole was "excellent".[87]

The main purpose of the *Transactions* was to preserve the papers that had been read at meetings and bring them to a wider audience; few other natural history societies published and (as Allen has pointed out) this ambitious journal distinguished the Linnean Society from them and probably contributed to its strong identity and early success. Papers generally appeared in print about two years after being read at a general meeting. The published papers give an idea of the kinds of subjects studied by the fellows, about the kinds of people doing research and about some of the more general concerns of late

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82 Gage and Stearn [1988] 4, 6.
83 Clark [2000] 95, 101, 225.
84 Ibid. 240.
85 Gage and Stearn [1988] vi.
86 Anon. *Critical Review* [1792] 13.
87 Anon. *Monthly Review* [1792].

eighteenth-century naturalists. The first volume, for example, contained about 25 scientific papers, of which the largest number (13) related to botany and slightly fewer to zoology and entomology, there was also one article on fossils and one on the language of science.

Despite its stated commitment to studying all of natural history, the Linnean Society always had a more pronounced interest in botany than in the other branches of natural history. There are several possible explanations for this. One is that it is simply due to the predilections of Linnæus or Smith who both specialised in botany above any other branch of natural history. Another explanation which takes the broader context of Britain into account is that botany had always been the preferred science there. Even a brief survey of published works shows this: between 1700 and 1800 approximately 360 books were published in English containing the word 'botany' in their title, while in the same period, only about 30 books appeared containing the word 'zoology'.[88] The popularity of botany has been linked to utilitarian and economic reason: for example, plants were used daily for practical purposes such as thatching, and the increasing sophistication of agriculture and horticulture meant that plants were becoming more important commodities across society.[89] Botany also tended to be more accessible than zoology as plants were generally easier to procure, cheaper and more disposable than animals.

Most of the papers published in the *Linnean Transactions* were about describing, naming and classifying; occasionally the *Transactions* carried papers that were not taxonomic (such as Robert Townson's paper on hydraulics in plants, discussed in chapter three of this thesis) but these were the exception rather than the rule. More common were papers that described new species, re-named or re-classified known species, or observed new properties of species. To get a better idea of the style and content of the *Transactions*, it is worth looking at a few papers in some detail. I have chosen papers by Smith and Marsham to illustrate what a typical paper might contain and how it might be presented in an early volume of the *Transactions*. Marsham's paper was titled 'Observations on the phalæna bombyx lubricipeda of Linneus [sic.] and some other

[88] Eighteenth Century Collections Online search.
[89] Fissell & Cooter [2003] 151-156.

(v)

C O N T E N T S.

a VI. *Some*

vi C O N T E N T S.

8 XIV. *Account*

C O N T E N T S. vii

a 2 XXII. *Remarks*

viii C O N T E N T S.

LIN-

Contents pages from the first volume of the *Transactions of the Linnean Society of London* (1791) showing the range of topics covered by this publication. Image courtesy of moths allied to it'.[90] The first four pages were written in English and contained general remarks about Marsham's desire to promote entomology, the utility of entomology, the importance of good communication and a common language between naturalists, and the usefulness of Linnæus's system. But the main point of Marsham's paper was that Linnæus had made a mistake when describing a particular moth and so confused it with other species. Marsham was keen to stress that Linnæus almost never erred and that this particular mistake was an easy one to make. There then followed two pages of specific descriptions in Latin of the moths in question; for example,

> PHALÆNA BOMBYX.
> ERMINE A. '*tab. l.f.i. Cream Ermine.*
> B. Alis albis punctis nigris sparsis, abdomine quinquefariam nigro punctato
> *Linn. Syst. Nat.* 829. 69. lubricipeda. *Faun. Suec.* 1138. fœm.
> *Fab. Syst. Ent.* 576. 68. *Sp. Ins.* 190. 93.
> *Gœd. Ins. vol.* I. *tab.* 23. *fig.* 38. *List. Gœd.* 96. *Rai. Ins. fig.* 195.
> *n.* 40. *Albin. Ins.* 24. f. 36. g—k. *Wilkes 20. t.* 3—5.
> *DeGeer. Ins. I.t.II.f.8. Roes. Ins.* 2. *t.4.6. Esper. tom.* 3. *tab.* 66.
> *fig.* 6—10 Menthastri. *Harris Aur. pl.* 38. g—b. *Ernst.*
> *Pap. d'Europe, pl.* 158. *n.* 204.
>
> Habitat in arboribus pomiferis, urtica, atriplici, quercu.
> Expansio alarum 1 unc. 6 lin.
> *Descrip. Femora,* præsertim antica, lanugine ferruginea vestita; *Corpus* album; *Alæ* adspersæ punctis nigris plurimis in superiorum pagina superiore; *Abdomen* luteum quintuplici macularum nigrarum ordine, quorum unus dorsalis, duo utrinque laterales—Ano albo quo certo certius, a Ph. lubricipeda dissert.

This begins with the genus and species name (*phalæna bombyx*) as well as the common local name (cream ermine) and a one-line description of the moth as having white wings sparsely dotted with black spots and an abdomen marked with black. Then follows a list of works by other authors such as Linnæus, Fabricius and Roesel who have also described this species along with the book and page-number where their description can be found. Finally comes a longer description containing information about habitat, and more details about the creature's size, appearance and anatomy. This was repeated for each moth that Marsham wished to discuss and served to illustrate his point that Linnæus had confused the different species. The article was accompanied by a plate showing the insects in question in their caterpillar and moth stages. The final part of the paper was a

[90] Marsham [1791] 67-75.

two-page analysis in English about why different authors may have confused the species. Smith's paper was similar in that it dealt primarily with specific descriptions, but it lacked Marsham's English-language preamble on the importance of natural history and a closing commentary (though perhaps this was because he had already set out his ideas on natural history in the volume's opening paper). This article was titled 'Descriptions of ten species of lichen collected in the south of Europe', and quite simply gave those ten descriptions, in Latin, without any additional analysis, for example:

> L. tumidulus.
> L. crustaceus albus lobatus: lobis deflexis tumidis, tuberculis atris difformibus.
> Habitat in fissuris rupium Galliæ australis.
> Crusta alba, lobata; lobi rotundati, valde deflexi, ut farcti vel inflati apparent, supra minute tessellato-rimosi. Tubercula in interstitiis loborum, atra, irregularia.[91]

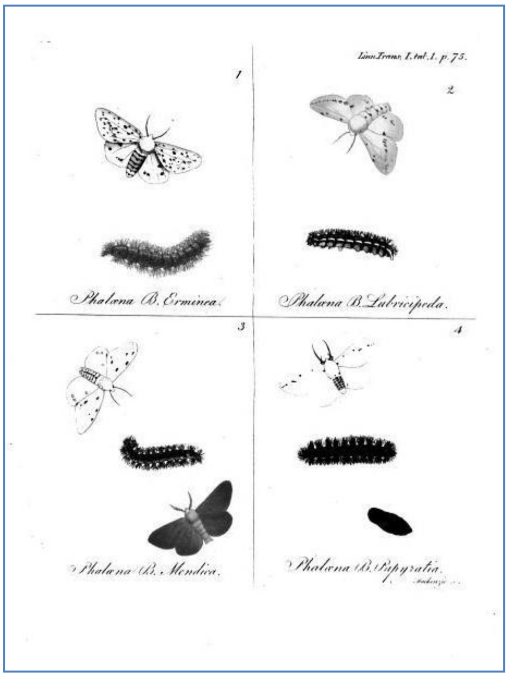

Plate accompanying Marsham's article on moths in the first edition of the *Transactions of the Linnean Society* (1791). Image courtesy of Google Books.

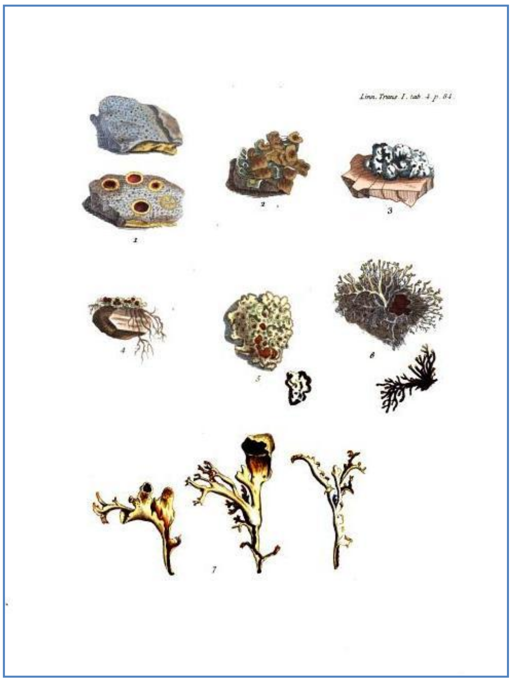

Plate accompanying Smith's article on lichens in the first edition of the *Transactions of the Linnean Society* (1791). Image courtesy of Google Books.

The L. stood for the genus *lichen*, while *tumidulus* was the species name. The description related to the colour and texture of the lichen (in the case of the *lichen tumidulus*, it had hard white lobes, some twisted and swollen lobes and was partly marred by black

[91] Smith [1791b] 81-85.

tubercles) and its habitat in the clefts of cliffs in southern France. The text was accompanied by a coloured plate showing the lichens. This paper was intended as new information rather than a correction of old information like Marsham's had been; presenting new findings in this one central journal was a key function of the Linnean Society.

It is also worth noting that many authors had multiple papers within each volume; for example, George Shaw (1751-1813) – assistant keeper of natural history at the British Museum – was responsible for a significant number of the zoological articles in the early volumes. Shaw, being in the unusual position of having a paid position as a natural historian, may have had more incentive to publish than many of the other fellows who, with a small number of exceptions, were not attempting to make a living from natural history. Without Shaw's contributions, the zoology content of the *Transactions* would have been considerably smaller. Other papers in the journal came from a range of naturalists: some were clergymen; many had medical qualifications; some were attached to universities or other learned institutions, but more were independent scholars; all tended to be of reasonably high social standing.

A high level of education was assumed of the readership as articles were published in French and Latin as well as English. Although this would have appealed to an international audience (and, as the large number of foreign members shows, this was a key aim of the society), it was symptomatic of a larger problem in natural history. The problem of language and terminology was one with which the Linnean Society struggled constantly in its early years. Almost every issue of the *Transactions* carried a paper which dealt with attempts to standardise the words of natural history. Though botanical terms, for example, tended to have origins in Latin or Greek, there was no standard way to import or translate these terms into other languages. Throughout the seventeenth and eighteenth centuries, Latin had given way more and more to modern languages in scientific texts and articles, but the problem of bringing useful technical Latin terms into these languages had never been successfully resolved. Because one of the stated aims of the Linnean Society was to communicate new scientific information to the world, it is natural that its fellows should spend time trying to perfect one of their main instruments of communication – their language. In the first volume of the *Transactions*, for example, the Cambridge Professor of Botany Thomas Martyn (1735-1825) published an article in which he tried to establish a set of rules for importing "the excellent language which Linnæus invented" into "the mother tongue into which it is to be received".[92] He formulated these rules as simply as he could: "First", he wrote, "we should adhere as closely as possible to the Linnean language itself: and secondly, that we should adopt the terminations, plurals, compounds and derivatives, to the structure and genius of our sterling English".[93] The use of Linnean terms would, according to Martyn, take advantage of their universality and allow British botanists to be intelligible to their European counterparts; while the use of English would allow those without a university education, as well as women, to play a more active role in natural history.[94] The *Transactions* are a very useful window into eighteenth-century natural history: they allow us to see not just what was being studied by the Linnean fellows, but also to see who was studying what, and what kinds of wider concern were influencing the practice and spread of natural history.

As a material resource, the early volumes of the *Transactions of the Linnean Society* are invaluable. But a history of the Linnean Society that relied solely on them (and on the minutes of meetings that were kept deliberately austere and free of any kind of controversy) would neglect a large part of the society's activities. Small informal interactions between individual fellows were as much a part of the society as formal readings and leather-bound journals. Private correspondence of some of the early fellows of the Linnean Society has been preserved; the contents of these letters can impart direct information about what naturalists wished to discuss, and indirect information about their dealings with each other, as we see with the Macleay archive. Alexander Macleay was elected a fellow of the Linnean Society in 1794 and became its secretary in 1798.[95] His extant correspondence with other members of the society reveals much; not just the sharing of natural historical information such as lists of species names, drawings and descriptions, or offers to proof-read each others' writings before publication; but also feelings of camaraderie and friendship. We see an example of this in an exchange between Macleay and Thomas Marsham, the co-founder of the Linnean Society. Marsham required some assistance for his new book in which he would classify beetles

[92] Martyn [1791] 148.
[93] Ibid. 148.
[94] Ibid. 147-8.
[95] See chapter five of this thesis for more information on the work of Macleay.

according to the Linnean system. He wrote to Macleay, renowned entomologist, for help with species identification and names. His letter began:

> Every letter from you makes me more ashamed of the trouble I give you. Chrys. hinnulia is certainly made the other sex of Cervinia by [Edward] Donovan. I had quoted the plate to hinnulia with that remark. I have no objection to the name being altered to Cinerea, but I think I have made another error of calling them both var β of Payk. it is so in my book & if I have copied it so strike it out of Cervinia – The name hinnulia was from D^{r}. G –

The letter was mostly concerned with technical detail, but it ended on a more personal note:

> Time sure passes merrily with you – what day am I to dine with you, your letter yesterday said Monday Jan^{y} 4. Your letter today is dated Jan^{y} 21, but luckily no anno Domini is added so I may take for 1801 or 1802 as I please. Tell M^{rs}. M^{c}Leay my visit on Monday will be to her & that I will not once go into the Sanctum Sanctorum, as I am anxious to keep her in good humour, I won't mention Crawley bugs the whole day.[96]

The Macleay correspondence is littered with similar examples of fellows helping and supporting each other, visiting each other to see specimens or read books, dining together, asking after wives and children. These interactions mostly took place in private homes, both in London and in country houses, but occasionally also in coffee houses. These letters give a much better insight into the relationships between fellows than the dry minutes of the society's meetings. They also give a sense of how collaborative natural history was in this period.

A study of the Linnean Society is useful for exploring some of the activities of late eighteenth-century British naturalists. It tells us what they considered important, how they went about promoting their science, how they arranged themselves into formal groups and informal alliances. At the society's heart was a group of naturalists who wished to present a united front and maintain stability: we see this in the use of Linnæus as a figurehead; in the unquestioning adoption of his system of classification; in the central position of the Linnean collections and library; and in the deliberate avoidance of discussion, debate and controversy. In many ways the Linnean Society gives an accurate representation of much English scholarly natural history at the turn of the century, and by the early nineteenth century their definition of the science was becoming more

[96] Marsham to Macleay, 31st December 1801, letter 84, Macleay Correspondence Archive, Linnean Society Archive.

common. Many British naturalists accepted the teachings of Linnæus, saw the utility of his artificial system, and wished to avoid the deeper (and possibly divisive) philosophical debates of natural history. But, as we shall see in subsequent chapters, this was not the only kind of natural history that existed in eighteenth-century Britain.

Conclusion

The success of the Linnean Society and its success in defining a particular kind of natural history have been explored in this chapter. It shows how a society for natural history could become *the* society for natural history; it shows what had to be done to ensure that that society would thrive; and it shows how natural history began to move from being a broadly-defined field of knowledge that used a wide range of techniques and theories, to being a science of taxonomy.

The society grew out of the increased popularity of natural history in the eighteenth century, out of the success of Linnæus, out of the utility of his classification system, out of the failure of so many other natural history clubs, out of the desire to create a 'scientific' natural history, out of a mission to improve, promote and preserve British natural history. Its success came about because it was able to maintain a stable base when so many other societies could not; this stability was drawn from regular meetings, a fixed location, a material collection at its centre, a methodical system of presenting papers, a consistent journal, a feeling of fellowship, and from a complete suppression of debate, discussion and controversy in all of its forums. The Linnean classification system that lay at the foundation of the society became popular because of Linnæus's need to classify large numbers of plants quickly, because of new entrants to the field requiring a simple methodology, because of a desire to standardise systems and terminologies. Its success was dependent on the fact that a large number of people could easily learn its basics and apply them to any plant in the world; its universality was what made it popular initially, and once it gained such popularity it could play a role in stabilising natural history. As a stabilising tool, it was inherently useful to a group such as the Linnean Society who wished to minimise controversy: here was a way that any fellow or foreign member could find a common language; here was a way to standardise a core activity of natural history; here was a way to avoid just the kind of disputes and controversies that might destabilise a society; here was a classification system distinct from those used in politically-turbulent France; here was something that must be defended.

The Linnean Society allowed for better communication, access to a wider circle of contacts, standardised language and regular publication; all of which furthered the spread of natural history. But this came at a price: the society also stifled debate and controversy, and was generally unwilling to discuss the more philosophical parts of natural history or any theory that might undermine the status of Linnean classification. Many British naturalists believed that the benefits of the society far outweighed any perceived disadvantages; for them, the tools provided by the society, and the sense of unity that it gave its members, were far more important than the consideration of the more speculative aspects of natural history.

The Linnean fellows' desire for stability (which was partly inspired by the knowledge that so many earlier natural history societies had failed, partly by the aspiration to be seen as a scientific equal to the mathematical sciences, partly as a reaction to the awareness that larger fields might splinter as they became specialised, and partly as a way to distance themselves from free-thinking French naturalists who reflected the political turmoil of their nation) resulted in a narrowing of natural history. And because of the success of the society, it is the Linnean version of natural history that most people think of when they think of eighteenth-century British natural history. This has profoundly affected the way the science has been viewed by later practitioners, and by historians of science. By looking at the rise of the Linnean Society, we also look at the demise of another kind of natural history and understand why those two facets of the same science were incompatible. I have begun this thesis with the story of the Linnean Society's natural history so that it can be juxtaposed to the broader form that I will focus on for the rest of this thesis; the next chapter will explore part of that natural history through the study of zoophytes.

CHAPTER TWO

On being an animal; or, the eighteenth-century zoophyte controversy in Britain

> "Your discoveries may be said to vie with those of Columbus. He found out America, or a new India in the west: you have laid open hitherto unknown Indies in the depths of the ocean."
>
> Carl Linnæus to John Ellis, 8th November 1769[97]

Zoophytes, a group of strange creatures that existed somewhere on, or between, the boundaries of the plant and animal kingdoms, were the subject of considerable debate in the eighteenth century. They were believed by some naturalists to be a blend of plant and animal; others considered them to be entirely plant, albeit with some animal characteristics; and others still argued that they were wholly animal, but conceded that they occasionally behaved like plants. These disagreements about the nature of the zoophyte allow us to understand a wide range of issues in late eighteenth-century natural history: how the plant and animal kingdoms were defined; the relationship between the two; the meaning of the 'chain of being'; and the construction of taxonomic systems. The 'chain of being' is an ancient concept which, Lovejoy argues, is based upon the principles of plentitude, continuity and gradation; it had long existed as a metaphysical concept, but it was only in the eighteenth century that the order of nature became the subject of empirical study and lead to questions about artificial and natural systems.[98] It raises a host of questions that eighteenth-century naturalists had to grapple with: were species real; were there 'missing links' in the chain; were there different levels of perfection in nature? The study of this metaphysical idea of 'perfection' was difficult, but zoophytes allowed naturalists to approach some of the biggest questions raised by the concept of the 'chain of being'.

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97 Smith [1821] 240.

98 See Lovejoy [1964] chapters 6, 7 8 for a detailed discussion of the chain of being in the eighteenth century, and see Bynum [1975] for a discussion of Lovely and subsequent historiography of the chain of being.

Zoophyte studies brought together natural history, natural philosophy, and practices from the experimental and observational traditions in a unique way. And so zoophytes can be used to elucidate questions about methodology within eighteenth-century British natural history; about the relationship between theory and practice in natural history; and about the ways in which naturalists formulated arguments and dealt with controversial issues. The strangeness of the creatures necessitated a distinctive approach; the problem of the zoophyte could not be resolved unless one combined practical knowledge of specimens with theoretical knowledge about the nature of animal life or the 'natural' order of creation. By examining this approach, one begins to see some of the ways in which natural history interacted with other fields of knowledge. The fluidity of eighteenth-century branches of knowledge has been well documented, but little work has been done specifically on the overlaps between natural history (especially within botany and zoology), natural philosophy, and the chemical sciences.[99]

A large number of organisms fell into the category of 'zoophyte': not just polyps, corals and sponges, but also starfish and earthworms were placed in this group by different naturalists.[100] In ancient and early modern times zoophytes were generally seen as a rather insignificant part of nature. It was only in the eighteenth century, following the discoveries of Abraham Trembley (1710-1784), that larger numbers of naturalists began to study them seriously and to see them as potentially useful in answering questions about the natural world. Trembley, a Swiss naturalist, had begun his researches on polyps in the 1730s and his key discoveries centred on the regenerative powers of those tiny creatures. A polyp is an organism, generally less than a centimetre in length, shaped like a bell or, the description more commonly employed by eighteenth-century naturalists, like the severed finger of a glove. Its single opening is surrounded by tentacles and leads to a central cavity (later discovered to be its stomach). They are generally found in stagnant ditches or similar locations. When, in March 1741, Trembley wrote to the French savant René-Antoine Ferchault de Réaumur (1683-1757) to announce his discovery that these polyps could reproduce from cuttings, it caused consternation in the learned circles of Europe. Especially in France, where Trembley's findings had been reported in the *Histoire*

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99 On the fluidity of boundaries between eighteenth-century sciences and fields of knowledge, see the introduction to Rousseau and Porter [1980]; Schaffer [1980]; Yeo [2003].

100 In this chapter, I will focus just on zoophytes and not on the related question of animalcules which were also subject to questions about their true nature and proper kingdom.

de l'Académie des Sciences, discussion of polyp regeneration re-ignited debates about materialism and vitalism.[101] Since this thesis aims to see how British natural history functioned, I focus primarily on the zoophyte studies of British-based naturalists. Many British authors tried to minimize the emphasis on philosophical questions about what a zoophyte was, preferring to supply technical description of its appearance and habits.[102] But it is possible, sometimes, to discern the problems these authors faced when studying polyps – particularly by looking at the ways in which they classified these organisms. The first decision to be made when classifying related to kingdom: was the specimen animal, vegetable or mineral? Most of the time the answer to this question was so obvious that naturalists gave it little thought. But occasionally, as in the case of zoophytes, it was more difficult to establish the correct answer and naturalists were forced to think carefully about definitions often taken to be self-evident.

This chapter moves between the English seashore, London laboratories, genteel cabinets of curiosity and Scottish print-shops to show how polyps and similar organisms shaped natural historical thinking. The first section will discuss how zoophyte studies blurred the boundary between the animal and vegetable kingdoms, and how naturalists re-conceptualized animal and vegetable in the wake of these studies. The second section looks at John Ellis's (c. 1710-1776) use of chemical experiments to try to solve a problem in natural history. The third section deals with the problems zoophytes posed for taxonomy and systematics. And the final section will address the use of Georges-Louis Leclerc, Comte de Buffon's (1707-1788) theories by British-based naturalists, and the popular presentation of zoophyte studies. There have been several historical studies published on well-known figures such as Trembley and Ellis, but little has been written on the meaning of their work for natural history and natural philosophy.[103] Here, I will use the zoophyte controversy to examine the ways in which these areas of knowledge interacted, and the knowledge produced by these interactions.

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[101] Dawson [1987] chapter 1.Trembley's discoveries were propagated not just through his letters and publications, but also through his important 'strategy of generosity' which entailed sending live polyps to naturalists across Europe so that they could perform the experiments for themselves, see Ratcliffe [2009] chapter 5.

[102] Some authors, such as Schiebinger [1993] 28, attribute this British, and most particularly English, preference for practice over theory to the decline of academic natural history which had been ongoing since the 1720s.

[103] For a recent work on Ellis and Trembley see Ratcliffe [2009] which focuses primarily on their microscopic work.

The animal in the eighteenth century

Zoophyte studies reached something of a zenith in the decades after Trembley first published his accounts of polyps. But, even during these years of intense research and debate, the study of zoophytes remained a minority interest within natural history. Zoophytes were placed at the lower end of the chain of being and were popularly seen as 'nauseous', 'despicable' and 'imperfect'.[104] In an age when much natural history centred on aesthetics, on cabinets and collections, on public displays of the wonders of nature, the zoophyte could not compete for popular attention. Compared to the beauty of a butterfly, the majesty of a lion, or the wonder of a kangaroo, the zoophyte seemed small, dull and ugly to many. It was primarily scholarly naturalists, natural philosophers and theorists who took an interest in the zoophyte. In Britain, only a few people published on zoophytes. Foremost among these was John Ellis. Ellis was born in Ireland but spent most of his life in London; there, he began his career as an apprentice to a cloth-maker before setting up a textile business of his own and becoming reasonably wealthy. Ellis's wealth allowed him sufficient time and resources to indulge his principal interest – natural history. Ellis was interested in many branches of natural history and was well known to his contemporary naturalists; in 1754 he was elected a fellow of the Royal Society and in 1755 he published his first major work: *Natural history of the corallines*.[105] This high-quality work was one of the first original British publications on zoophytes since Trembley's results had sparked interest in these creatures; it set the standard for British works on zoophytes and it was still being referenced by naturalists well into the nineteenth century.[106] The book, with its thick paper, large print, wide margins and numerous plates was expensive and its audience would have been scholars, gentleman-naturalists and natural history clubs or societies.

In the introduction, Ellis described how he had become interested in zoophytes; despite the fact that many found them ugly, he had first been drawn to them for aesthetic reasons. In 1751 a friend had sent him some sea-plants and corallines. Ellis had preserved them and arranged them in a frame to form a landscape. The natural philosopher

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104 Goldsmith [1774] Vol. VIII, 164-5.

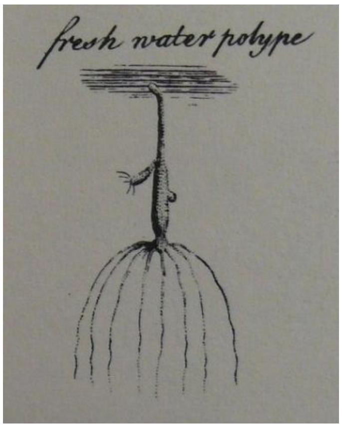
105 Ellis [1755]. In modern terms, corallines are algae with calcareous jointed stems. For more on Ellis's background and his place in British natural history in this period, see Gascoigne [1994] chapter 3.

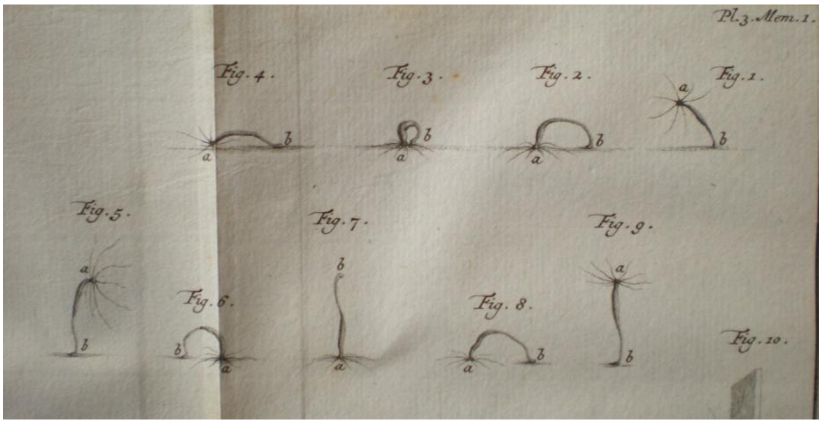
106 In, for example, Robert Grant's 1825-1826 papers on sea sponges published in the Edinburgh New Philosophical Journal. A less original British work on zoophytes was Baker [1743] in which he plagiarised much of Trembley's work; for more on this incident, see Ratcliffe [2009] 109.

Stephen Hales (1677-1761) had seen this and suggested that Ellis make some for the Princess Dowager of Wales; thus encouraged, Ellis began to collect seriously and wrote that "the great Variety [of sea-plants], that came through my Hands, determined me to separate all the different Species, and to dispose them in proper Classes". Ellis described how he went about this:

> In order to distinguish their proper Characters with the greater Accuracy, I found it necessary to examine them in the Microscope; by which I soon discovered, that they differed not less from each other, in respect to their Form, than they did in regard to their Texture; and that, in many of them, this Texture was such, as seemed to indicate their being more of an animal, than vegetable Nature.

This was how Ellis first stumbled upon the problem of distinguishing animal from vegetable. He created three categories into which to place his problematic 'sea-plants': those that he considered animal; those that he considered plant; and "a third Class, which seemed to partake of the Nature of both".[107] Ellis, early in his career, did not have any conceptual objections to the existence of a grey area between the plant and animal kingdoms.





Above left: drawing of a polyp from Ellis's 1755 *Natural history of the corallines*. Above right: drawing showing polyp locomotion through (top) an inch-worm-like motion and (bottom) a series of somersaults from Trembley's 1744 *Mémoires pour servir à l'histoire d'un genre de polpes d'eau douce*. Images courtesy of the Whipple Library, University of Cambridge.

The defining characteristics of an animal had been debated since at least the time of Aristotle. In *Historia animalium*, Aristotle described the four factors he would use to define an animal: nutrition; reproduction; sensation; and physiology. In order to be

[107] Ellis [1755] introduction v-vii; the book was dedicated to the Princess Dowager of Wales. For more on the use of the microscope in the study of zoophytes and other small organisms, see Ratcliffe [2009] 103-5 and chapter 5; this book looks at microscopy in Europe as well as Britain and deliberately focuses on experiments performed with the microscope rather than their metaphysical interpretations.

considered an animal, a creature first required a digestive system: "All animals have in common the part by which they take in food and the part into which they take it. In addition to these, the majority of animals have other parts in common as well – first, the parts by which they discharge the residue that comes from their food...". It also required a reproductive system; in his list of parts essential to animals, Aristotle included "a part by which they emit semen". Further, it had to experience sensations, Aristotle wrote: "One of the senses, and only one, is common to all animals, *viz.*, touch". And finally, he declared, "every animal contains fluid... and further, there must be some receptacle in which this fluid exists... these parts, respectively, in some animals are blood and blood-vessel; in others, parts analogous to these; but the latter are imperfect, e.g. fibre and serum".[108] It was not necessary for all four of these factors to be present simultaneously; often, the presence of one or two was enough for an object to be placed in the animal kingdom.

In the eighteenth century, Aristotle's definition was still widely used in zoology and he was frequently cited by naturalists – the concept of animality had changed little. We can see this by looking at Trembley's work. Part of Trembley's reason for undertaking the experiments in which he cut polyps was to determine whether they were animal or vegetable. Differences in plant and animal reproduction meant that plants could re-grow from cuttings but animals could not. When the cut polyps regenerated their lost parts that should have allowed Trembley to place them in the vegetable kingdom – but some of their other properties marked them out as animal. The first of these was the movement of their tentacles.[109] Polyps in water moved their tentacles independently of any motion in the liquid. They were also sensitive to touch: touching the polyp or shaking the jar in which it was placed caused it to contract.[110] The criteria relating to nutrition also indicated that polyps were animals. Aristotle had suggested that the presence of a mouth and stomach were central to the definition of an animal and this was a belief held by many eighteenth-century naturalists. For example, in his 1732 work *Elementa chemiae* Herman Boerhaave (1668-1738), professor of medicine and chemistry at

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[108] Aristotle [1979] 19-21. Lloyd [1996] gives an excellent overview of Aristotle's doubts about the correct way to classify zoophytes.
[109] Although Aristotle had not mentioned motion in the list of animal characteristics in *Historia Animalium*, he did write an entire treatise on *The Movement of Animals* and motion was widely considered a standard animal property.
[110] Dawson [1987] 97-8.

Leiden, wrote that the principal distinction between plants and animals was their method of obtaining nourishment. Trembley used Boerhaave's definition in his 1744 work on polyps, *Mémoires pour servir à l'histoire d'un genre de polpes d'eau douce*:

> No more respected authority can be cited at this juncture than the renowned Boerhaave. ... It seems that he has found but a single general and essential difference between these two classes of organisms. This difference... consists in the manner in which plants and animals draw their nourishment. '*The nourishment of plants,'* says Mr. Boerhaave, '*is through external roots, that of animals through internal roots'*.[111]

Since Trembley had observed polyps grasping food with their tentacles and placing it in their central cavity he could prove, according to this definition, that a polyp was an animal. Another reason to view polyps as animals was their power of locomotion – polyps were capable of travelling in the manner of an inch-worm or by means of a series of somersaults. For Réaumur, this was the most convincing proof of the polyp's animal nature.[112] On Aristotle's final point – the presence of blood or an equivalent fluid – experiments were inconclusive. Sometimes dissections revealed the presence of green globules in a transparent liquid but sometimes there were none and Trembley had difficulty discovering whether this substance was more analogous to animal blood or plant sap.[113] So, according to Aristotle's criteria, the polyp was an animal in its nutrition, motion and sensation; a vegetable in its reproduction; and ambivalent in its structure and physiology.

John Ellis, animal chemistry and the problem of the zoophyte

This was the theoretical background against which Ellis began thinking about the animal and vegetable properties of the corallines. As has already been mentioned, it was their texture that first made Ellis question the idea that they were plants. This wasn't something that had occurred in the definitions of Aristotle or Trembley, so why did it become so important to Ellis? We can speculate that improved microscopes and more reliable chemical analysis in this period allowed Ellis and his contemporaries to develop

[111] Dawson [1985] 326.
[112] Ibid. 327.
[113] Dawson [1987] 102.

new ways of studying organisms and thinking about animal nature. Ellis and others had noticed that the corallines' unusual texture was due to the presence of a layer of calcareous material on their surface. Because the coralline was entirely devoid of an animal digestive system or powers of sensation or motion, texture was the only obvious non-vegetable feature, and so it became central to its classification. Ellis rarely elaborated on the reasoning behind his zoophyte work or gave a clear definition of all the features he considered to be characteristic of animal life. His published writings consisted mostly of careful, exact descriptions of his fieldwork, methods, subjects and results. In his private letters he explained that he tried to avoid conjecture wherever possible, moving from factual description to final conclusion without excessive discourse. Discussing the possible existence of vegetating animals he wrote that "the introduction of the doctrine of this mixed kind of life will only confuse our ideas of Nature. We have not proof sufficient to determine it; and I am averse to hypotheses".[114]

In his 1755 work Ellis did not explicitly state the characteristics he considered to define a plant or animal, but his discussion of the zoophyte researches of Trembley and other naturalists, as well as comments made in his correspondence, show that he was aware of the intricacies of the debate. In the opening pages of the book, Ellis tried to explain why he considered the texture and calcareous surface of the corallines to be important in deciding their kingdom:

> [Corallines] differ from Sea-Plants in Texture, as well as Hardness, and likewise in their chymical Productions. For Sea-Plants, properly so called, such as the *Algae, Fuci,* &c. afford in Distillation little or no Traces of a volatile Salt: Whereas all the Corallines afford a considerable Quantity; and in burning yield a Smell somewhat resembling that of a burnt Horn, and other animal Substances: Which of itself is a Proof that this Class of Bodies, tho' it has the vegetable Form, yet is not intirely of a vegetable Nature.[115]

Ellis's ideas about chemical distinctions between plant and animal may have been inspired by the great Swedish botanist and taxonomist Carl Linnæus (1707-1778): in Ellis's (posthumously published) *The natural history of many curious and uncommon zoophytes* of 1786, he mentioned Linnæus's belief that "all calcareous substances are most truly of

[114] Smith [1821] 260-1. This belief, expressed in 1771, shows how Ellis had changed his views since 1755 when, in *Natural History of the Corallines*, he had created a class that was a mixture of animal and vegetable.
[115] Ellis [1755] 2.

animal production".[116] Linnæus and Ellis were good friends and regular correspondents and in a letter to Ellis written in 1761 Linnæus had stated his criteria for distinguishing the kingdoms: "animals differ from plants merely in having a sentient nervous system, with voluntary motion". But in 1767 Linnæus used chemical distinctions to define the coralline as an animal: "that Corallines belong to the Animal Kingdom, I never had any doubt, on account of their calcareous crust; being well convinced that lime is never produced by vegetables, but by animals only".[117] Here, Linnæus did not appear to regard the corallines' lack of a nervous system or the absence of voluntary motion as an impediment to calling them animal. In the case of zoophytes, different criteria of animality were applied to different species. The corallines especially, which Ellis considered "the most difficult part of all the Zoophytes to explain", were difficult to classify based on simple observation and so were more often subjected to chemical and microscopic analysis than other species.

Ellis, as well as being familiar with Linnæus's ideas on animal and plant chemistry, was familiar with the work of the Berlin-based naturalist Peter Simon Pallas (1741-1811) who, in 1766, published a work called *Elenchus zoophytorum*. Pallas believed that corallines were vegetable because of their structure and chemistry. In 1767, Ellis published a letter "On the Animal Nature of the Genus of Zoophytes, called Corallina" in the Royal Society's *Philosophical Transactions* in which he directly tackled Pallas's claims. Pallas had written that burned corallines smelled like vegetables and that, like vegetables, they did not contain a volatile salt. Ellis responded by performing several public experiments in which he burned corallines and plants to demonstrate the very different smells produced – when he burned a piece of coralline "it filled the room with such an offensive smell like that of burnt bones, or hair, that the door was obliged to be opened, to dissipate the disagreeable scent, and let in fresh air".[118]

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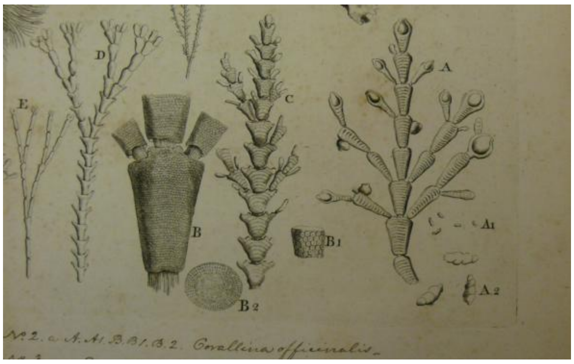
[116] Ellis [1786] 108. This work was co-authored by Daniel Solander (1733-1782).
[117] Smith [1821] 152, 208.
[118] Ellis & Woulfe [1767] 404-27. Ellis was later awarded the Royal Society's Copley Medal for this and the following paper.



Corallina officinalis – this was the species analysed by Woulfe. From Ellis's 1755 *Natural history of the corallines*. Image courtesy of the Whipple Library, University of

In order to counter Pallas's claims that corallines did not contain any volatile salt (which were based on the experiments of Count Luigi Ferdinando Marsigli (1658-1730)[119]), Ellis requested the assistance of the chemist Peter Woulfe (1727?-1803). Woulfe, also a fellow of the Royal Society and "a gentleman distinguished for his great knowledge in chemistry", had studied chemistry in Paris, and mineralogy in France, Germany, Hungary and Bohemia. Woulfe was known as an inventor and improver of compound distillation apparatus – and distillation was the key technique in proving or disproving the existence of volatile salts in a sample. Ellis sent Woulfe a quantity of c*orallina officinalis* so that he could "have fair and accurate experiments made on this substance". Over the course of about two months, Woulfe performed a series of distillations on samples of the corallines. The samples were distilled in three stages: first they were heated gently for eight hours; then they were heated at a higher temperature for six hours; finally the temperature was increased again and the sample heated for a further six hours. At the end of each stage, Woulfe would extract and set aside the liquids and crystals produced by the distillations. The liquid produced during the first stage "slightly effervesced with spirit of salt, and changed syrup of violets green, certain proofs of a volatile alkali". The distillates produced during the second and third stages reacted more strongly with spirit of salt, showing that they too contained volatile alkalis. Woulfe, commenting on the importance of his methods, remarked that "had this distillation been conducted in a hurry, there would have been no concrete volatile alkali; for then this would have been confounded and dissolved in the first liquor that came over". This not only served to reinforce Woulfe's position as a leading chemical experimenter, but may have also been intended to explain why Pallas had not found volatile salts in corallines. In a letter of May 1767, Woulfe recounted the details of these experiments to Ellis. This

[119] For more on Marsigli's experiments and fieldwork relating to corals, see McConnell [1990] 51-66.

letter was published in Ellis's 1767 paper on the animal nature of corallines. By publishing Woulfe's results just as he had received them, Ellis was displaying their authenticity, and was also giving a privileged place to knowledge produced by the experimental sciences.[120]

In this 1767 paper, Ellis also discussed Pallas's arguments about the pore size of corallines, their places of habitation, and their manner of reproduction. On each point, Ellis argued for the animal nature of the organism where Pallas had insisted upon it being a vegetable. But in his conclusion, Ellis made it clear that he felt his proofs regarding chemistry and texture were the most compelling evidence for the animality of the corallines. He encouraged the fellows of the Royal Society "to analyse these bodies chemically, and with care; and likewise to view them with the same attention, that I have done, in the microscope; if so, I am perswaded they will be of our opinion".[121]

Trembley's work on polyps and Ellis's work on corallines addressed the same kinds of questions in quite different ways. This is primarily because polyps and corallines, although both considered zoophytes, had little in common by way of appearance, structure or mode of life. The group named 'zoophyte' contained such a diversity of organisms that it was difficult to generalize, or to apply the same standards of animality to all. This was what made zoophyte studies so interesting to these naturalists – for every species one had to consider all possible animal and plant characteristics, weigh the relative importance of each characteristic, consider the arguments that other naturalists had advanced for or against these characteristics being animal or vegetable, and then produce a methodology that could be used to determine to which kingdom the species belonged. In other branches or botany or zoology, classification disputes usually existed at the level of genus or species, occasionally at the level of class or order, but almost never at the level of kingdom.

Controversies about zoophytes and disputes about the boundary between the animal and vegetable kingdoms were difficult to resolve not just because of the large variety of zoophytes, but also because of the varying criteria for being considered animal or vegetable, and the different ways of studying them. While Trembley's analyses of the polyps were observational rather than chemical, Ellis's work on coralline classification

[120] Ellis & Woulfe [1767]; Campbell, [2004].
[121] Ellis & Woulfe [1767] 417-8.

was largely dependent on burning and chemical distillation. Increasingly sophisticated methods of scientific analysis did not necessarily make the task of distinguishing plant from animal any easier. Ellis may have been able to prove that his coralline samples contained a volatile salt but to a naturalist who considered the presence of a digestive system or the power of motion to be the defining characteristic of an animal, Ellis's results would have been meaningless. The use of chemistry to define the animal and vegetable kingdoms was a controversial topic in eighteenth-century natural history. The community of naturalists was divided on whether there was an innate chemical difference between a plant and an animal. While the Dutch physiologist Jan Ingenhousz (1730-1799), referring to his work on pond slime, wrote "only a weak argument can be drawn from chemical analysis, a fallible conjecture, in judging if a substance is animal or vegetable", the Italian naturalist Marsigli declared that "chemical analysis must terminate the question so often asked, that is, if coral is or is not a plant".[122] Ellis did not directly discuss his use of chemical analysis in his published works. Goodman has accused Ellis of being "unaware that the interpretation of the chemical evidence was not as straightforward as he had presented it" but because of Ellis's reluctance to discourse on his choice of methodology, it is impossible to say whether this is true.[123]

The discussion about the definitions of plant and animal, and the use of chemistry to separate the kingdoms, continued into the nineteenth century. In *A genuine and universal system of natural history*, a compendium compiled from a wide variety of sources, the discussion of the nature of the zoophyte was framed in the same way as it had been in the works of Ellis and his contemporaries.[124] The powers of motion and sensation were seen as key animal characteristics and the author wrote:

> [The zoophyte] seems destined by nature to connect the animal with the vegetable life; and hence the individuals classed under this arrangement, have been occasionally denominated the *last* of animals, and the *first* of plants. Most of them take root, as it were, and grow up into stems; multiplying life in their branches and deciduous buds, and in the transformation of their animated blossoms or polypes, which are endued with spontaneous motion. Plants therefore resemble zoophytes, but are destitute of animation, and the power of locomotion; and zoophytes are as

[122] Goodman [1971] 23-44, 36, 39.
[123] Ibid. 41.
[124] Volume XIII of this work relates to zoophytes and would have been published c. 1807. It was published under the name of Ebenezer Sibly but he had died c. 1799 and the actual identities of the authors/editors are disputed. The title page said that the work had been "methodically incorporated and arranged by the editors of the Encyclopædia Londinensis".

> it were plants, but furnished with sensation, and the organs of spontaneous motion.[125]

The book also described some recent chemical experiments undertaken "in order to ascertain the true nature of the substance wherewith corals are formed".[126] This work had been done by Charles Hatchett (1765-1847) – a famous chemist, fellow of the Royal Society and co-founder of the Animal Chemistry Club.[127] Just as Ellis had included Woulfe's results in his 1767 paper, so the editors of *A genuine and universal system of natural history* included Hatchett's. The aim of the experiments was "to ascertain in these animal substances [corals], the presence of carbonat and phosphat of lime, which are the materials employed by nature to communicate rigidity and hardness to shell and bone".[128] Hatchett's conclusion was that

> the varieties of bone, shell, coral, and the numerous tribe of zoophytes with which the last are connected, only differ in composition by the nature and quantity of the hardening or ossifying principle, and by the state of the substance with which it is mixed or connected.
>
> Thus, then, it is evident that coral is the bone of zoophytes, analogous to bone in all other animals.[129]

This use of chemistry to define the kingdoms was part of the same tradition as Ellis's chemical experiments, but where Ellis had looked for volatile salts, Hatchett tried to build an analogy between the hard structures of zoophytes, and animal bone. Nineteenth-century animal chemistry tended to be less about the search for volatile salts, and more about establishing analogies between different animal substances.

While chemistry may have been useful, eighteenth-century naturalists did not rely upon a single method for the analysis of all specimens; rather, they mixed several techniques to assess the creatures they were studying. When we look at Ellis's papers on polyps and similar beings, we see that he did not always rely on the chemical analysis that had been so important in his coralline work. In another paper, "An Account of the *Actinia Sociata*, or Clustered Animal-Flower", also published in 1767 in the *Philosophical Transactions*, Ellis's methodology was based on observation and minute description rather

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125 Sibly [1807] 592.

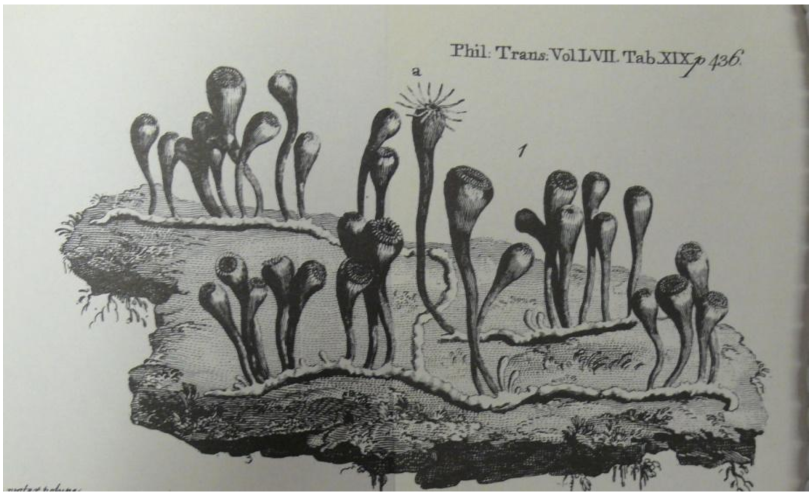
126 Ibid. 595.

127 This club was founded in 1808 as an 'Assistant Society' to the Royal Society to encourage research in chemistry, anatomy and physiology. It was dissolved in 1825.

128 Sibly [1807] 595.

129 Ibid. 597.

than chemical experiment. The animal-flower had recently been discovered in the West Indies – Ellis described it as a "compound animal" which consisted of many tubular bodies rising from a common base, each of which ended in a bulb surmounted by a mouth surrounded by tentacles. Because the animal-flower, unlike the corallines, had a mouth and digestive system, Ellis did not have to resort to chemical tests to prove that it was an animal. But the species was problematic because of its clustered, or compound, nature. Ellis admitted that "an animal compounded of many animals has not a very philosophical sound". This mode of existence was much more common in the vegetable kingdom, and Ellis had to use various arguments from zoophyte studies and from botany to try to prove that the organism was an animal. He wrote that "it is well known to those, who understand the nature of zoophytes; that there are many kinds of these animals... that have a great many mouths in the form of polypes, and yet are but single animals". He also made a comparison to a tree "that sends out at a distance round it many suckers coming in time to be trees, these may and will, with propriety, be reckoned so many distinct trees, though connected at their roots with the parent tree, and that without any absurdity". It seems slightly incongruous that Ellis would use a botany analogy to argue for the animal nature of the animal-flower. He concluded that the compound nature of the creature and its resemblance to a flower were not proofs of vegetative life, but rather that the presence of muscles, tendons, a stomach and intestines constituted "the strongest proof that has yet appeared to convince the learned world, that zoophytes are true animals, and in no part vegetable".[130]



A clustered animal flower. From Ellis's 1767 article in the *Philosophical Transactions*. Image courtesy of the Whipple Library, University of Cambridge.

[130] Ellis [1767] 428-37, 431, 433, 435.

Ellis considered himself a natural historian but often his work, which relied heavily on experimenting and actively manipulating natural objects, appears more closely allied to practices usually thought to be part of natural philosophy. The wide range of techniques that Ellis used were indicative of the fact that natural history in this period was not enclosed within rigid boundaries; it was not a discrete discipline and Ellis's techniques show his willingness to use whatever tools necessary from many different fields of knowledge to solve the problem of the zoophyte. The use of chemical analysis to answer questions in natural history is particularly striking and points to a much stronger experimental tradition among naturalists than is usually acknowledged.[131]

Taxonomy, systems and the chain of being

Much of eighteenth-century natural history was concerned with system. The 1735 publication of *Systema naturae* by Carl Linnæus intensified interest in taxonomies and systems of nature. Although there were naturalists who continued their studies without explicit reference to system, Linnæus's work changed the landscape of natural history, and the search for a natural system dominated the work of many eighteenth-century naturalists. For Linnæus, classification was a central activity of natural history. Naturalists had been ordering natural objects for millennia, but often as an activity auxiliary to the study of those objects; the works of Linnæus re-prioritized classification and emphasized it as a worthwhile activity in its own right. Linnæus was revered by many throughout Europe, and was held in particularly high regard in England. Under the influence of Linnæus, classification systems proliferated. Naturalists, swayed by arguments about the importance of system but unsatisfied with existing taxonomies, often created their own; this is especially true for zoology as Linnæus's zoological systems were not as well-received as his botanical ones. By the end of the century, there were hundreds of taxonomic systems in use, some of which differed from each other only to a tiny degree. These systems were intended to simplify natural history but frequently just created confusion. Some responded by trying to create a more definitive system while others

[131] Chemistry was often employed in the mineralogical parts of natural history, but far less often in zoology and botany.

denounced system entirely. In the "*Premier discours*" of his 1749 *Histoire naturelle*, Buffon argued against all classification in natural history and, although few naturalists were willing to abandon system, some conceded that Buffon had a number of valid points.

The large numbers of contradictory systems shows the difficulty of establishing a 'natural' order. The case of zoophytes, which were difficult to fit into any taxon, is particularly useful for highlighting some of the issues in eighteenth-century classification. I have already discussed the problem of fitting zoophytes into either the plant or animal kingdoms but, since this issue related only to a relatively small number of species, naturalists were able to invent solutions on a case-by-case basis. A much more general problem that arose from attempts to classify zoophytes was whether any man-made system could ever accommodate all of nature. Oliver Goldsmith (c.1728-1774) and William Smellie (1740-1795), two British-based authors, wrote explicitly about this problem. Both had been inspired to write about natural history by Buffon so it is unsurprising that this issue was given so much thought in their works.

The printer William Smellie was a key figure in the 'Edinburgh Enlightenment' and is said to have taught himself French for the express purpose of translating Buffon.[132] His 1780 translation of Buffon's *Histoire naturelle* was one of the most complete English editions of the text. Alongside his translation of the work, Smellie included a lengthy preface containing some of his own ideas about taxonomy in natural history. Discussing the profusion of new systems, Smellie wrote:

> the justly celebrated Linnæus...unfortunately turned the attention of most naturalists, though contrary to the learned author's design, from the great views of Nature to the humble ambition of system-making. ... Every philosopher must have observed, with regret, that inundation of methodical distributions which have successively appeared during the course of these last thirty or forty years. Since Linnæus's works were published, the attention of Naturalists has been principally occupied with criticising former arrangements, and fabricating new ones. The philosophy of the science has, of course, been almost totally neglected.[133]

Smellie was not against classification per se, simply concerned that it deflected attention away from more important aspects of natural history. He admitted that classification systems could be useful: first, as a way of formalizing the distinctions between natural

[132] Brown [2004].
[133] Buffon [1785] xii.

objects; and second, as a way to establish links between objects and to ascertain their positions on the chain of being. Smellie had little interest in the first use, declaring instead that:

> the second species of system is more elevated and sublime. ... Natural objects are wonderfully diversified in their structure, œconomy, and faculties. But, in these, as well as in many other circumstances, they are no less wonderfully connected. Here, then, are foundations for constructing the system of Nature, ... to ascertain the great chain that unites the numerous tribes which people and adorn the universe.[134]

This passage exemplifies some aspects of the debate about natural and artificial systems. The distinction between the two kinds of system was often a subtle one. Many believed that there was a natural order in the plant and animal kingdoms, but that any attempt to discover that order could only result in an artificial system. There was a natural chain of being, and there was a preferred way of studying it (the natural method), but there was no natural system. Dividing the chain into smaller sections was useful when naming plants or animals but was incompatible with understanding natural laws. The vastness of creation, however, meant that any naturalist wishing to make sense of the world was obliged to divide nature into workable groups. Peter Stevens, discussing the systems used by late eighteenth-century French naturalists, has written that "although the order as a whole (the arrangement) was that of nature, because it was the naturalist who decided which characters should be emphasized, groupings circumscribed using those (or any other) characters – the classification itself – must be alien to nature".[135] Smellie believed that by focussing less on division, and more on the unity of the chain, it would be possible to create a more philosophical natural history; he further believed that Buffon's works represented the beginnings of this improved science.

Oliver Goldsmith, like Ellis and Woulfe, was born in Ireland but spent much of his life abroad, mostly in London. He had studied medicine in Edinburgh and Leiden but when his medical career foundered he began to write full-time for journals such as the *Monthly review* and the *Critical review* and began to establish his reputation as an author, poet and playwright.[136] Goldsmith too, in the preface to his 1774 *An history of the earth, and animated nature*, addressed some of the problems of system. He believed that there were

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[134] Ibid. xiv-xv.
[135] Stevens [1994] Ch. 2, 18.
[136] Dussinger [2004].

two parts to natural history: discovering and naming natural objects, which he considered dull but necessary; and "describing the properties, manners, and relations" of those objects, which he considered much more amusing. Goldsmith described how, in order to deal with the first part of the science, naturalists had devised many "artificial systems". Being a man of letters, he turned to a literary metaphor to explain these artificial systems; he wrote that "a system of natural history may, in some measure, be compared to a dictionary of words. Both are solely intended to explain the names of things". The separation of ordering from description worried Goldsmith, as we see from his account of how:

> [naturalists] have been content to give, not only the brevity, but also the dry and disgusting air of a dictionary to their systems. Ray, Klein, Brisson, and Linnæus, have had only one aim, that of pointing out the object in nature, of discovering its name, and where it was to be found in those authors that treated of it in a more prolix and satisfactory manner. Thus natural history at present is carried on, in two distinct and separate channels, the one serving to lead us to the thing, the other conveying the history of the thing.

Although Goldsmith was not particularly interested in classification and tried to use as little as possible in this book, he did believe that it was important and did not fully agree with Buffon's rejection of system.[137]

The weaknesses of particular classification systems were often exposed when naturalists tried to fit newly-discovered species into them. So it was with the polyp. Goldsmith explained how, in the mid-eighteenth century:

> many found their favourite systems overthrown by the discovery [of the polyp], and it was not without a wordy struggle, that they gave up what had formerly been their pleasure and their pride. At last, however, conviction became too strong for argument, and [older systems were] given up in favour of the new discovery.[138]

Zoophyte studies threw taxonomy into a state of some confusion and fuelled the debate about natural and artificial systems. For naturalists who believed that all classification was artificial, the existence of zoophytes was useful in picking apart existing taxonomies. Many had placed zoophytes below the orders of insects – the lowest link on the chain of animated being – but often their specific characteristics implied that they belonged somewhere else entirely.

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[137] Goldsmith [1774] Vol. I, preface, viii-x; Vol. II, 289-300.
[138] Goldsmith [1774] Vol. VIII, 165.

Ellis also struggled with zoophyte classification. In his 1755 *A natural history of the corallines*, he had described how the large number of corallines he encountered inspired him "to separate all the different Species, and dispose them in proper Classes".[139] Ellis's solution to the problem of fitting corallines into any existing category was to create a new taxon for them. He subdivided this taxon into four groups: vesiculated, tubular, celliferous and articulated corallines. These groups had been used by Linnæus in his classification of "coral-like Bodies" and Ellis's modification of a Linnæan group is typical of many naturalists in this period. In order to lend authority to his new groups, Ellis worked hard to produce plausible generic and specific characters for his corallines. In his 1767 paper on corallines, he outlined his project: "[Solander and I] have made a description of each species: to do this with more exactness, I have taken care to dissect them minutely, and to pass them in review under his eye in the microscope, in order to establish a true general character of this genus".[140] Another way to fit awkward creatures into a pre-defined classification system was to merge genera together to accommodate a wider range of generic characteristics. In the opening paragraph of his 1767 paper on the clustered animal-flower, Ellis described how this animal "seems to bring together two remarkable genera in the system of nature, which Professor Linnæus had removed far from each other".[141] Ellis then went on to argue convincingly that there was considerable overlap between the genera *actinia* and *hydra*, and so accommodated the animal-flower in his system. This method of merging genera fitted neatly with the idea of a chain of being that Ellis frequently alluded to in his work.[142]

It was a reasonably popular belief, particularly among French naturalists, that the lower divisions of taxonomic systems (such as species) might be real, natural groups but that higher divisions (such as genus, order and class) must be artificial constructs. The chain of being was supposed to be made of species separated from each other by only tiny gradations. As more species were discovered, and as new species could be shown to overlap between two other species, the gaps in the chain were slowly filled. Zoophytes were particularly useful in linking species together, and particularly difficult to classify into genus, order or class – so, for some naturalists, they were further evidence that there

139 Ellis [1755] vi.
140 Ellis & Woulfe [1767] 404.
141 Ellis [1767]428.
142 Ellis [1765] 280-9, 280.

was a complete chain of being but that it was impossible to fit this natural chain into an artificial system.

Buffon and his followers in Britain

Natural historians continued observing, collecting, preserving, describing and classifying zoophytes, but, although more and more species were discovered and studied, the definitions of plant and animal developed very little over the rest of the century, and the question of how to classify zoophytes could not be satisfactorily answered. The late eighteenth-century naturalists who attempted to address the problem of distinguishing plant from animal were often dependent on the research of Ellis, Trembley, and on the ideas of authors like Buffon. Buffon began publishing his *Histoire naturelle, générale et particulière* in the 1740s and this epic, 35-volume work became one of the key texts of the century. In the book's "*Premier discours*" Buffon questioned the very idea of classification in natural history. His work directly opposed that of Linnæus. In Britain, where Linnæus's taxonomy had been very popular (especially with botanists), naturalists tended to ignore Buffon's radical "*Premier discours*" but were highly enamoured of his wonderful descriptions and illustrations of thousands of species of quadruped, bird, fish and reptile. English translations of the work began to appear shortly after the original French edition came out and were popular with British naturalists.[143] In the early chapters of the book, Buffon discussed the relationships, similarities and differences between plants and animals.[144] He named three principal characteristics for distinguishing the kingdoms: the power of progressive motion; the ability to experience sensation; and mode of nutrition. But of these three, he found that progressive motion was "neither general nor essential" and that definitions based on nutrition were unsatisfactory; he wrote: "From this investigation we are led to conclude, that there is no absolute and essential distinction between the animal and vegetable kingdoms; but that nature proceeds by imperceptible degrees from the most perfect to the most imperfect animal, and from that to the

[143] For more on the English-language translations of Buffon, see chapter 6 of this thesis. For a comparison of the main English translations of Buffon, see Loveland [2004] 214-235.
[144] As I am discussing the reception of Buffon in Britain, I have used one of the English-language translations in this section to present Buffon's words and ideas as many British naturalists would have read them. The quotations and chapter and volume references in this section come from the second edition of William Smellie's translation which appeared in 1785.

vegetable". He also discussed the similarities between the two kingdoms; he considered the three principal areas of overlap to be reproduction, asexual reproduction (as in the case of polyps) and growth. On the subject of growth, he wrote that "the foetus, in its first formation, may be rather said to vegetate than to live". From looking at these similarities, he was able to conclude:

> that animals and vegetables are beings of the same order, and that Nature passes from the one to the other by imperceptible degrees; since the properties in which they resemble each other are universal and essential, while those by which they are distinguished are limited and partial.

In a later chapter on the nature of animals, Buffon wrote that in order to create a theoretical framework for the understanding of animal nature:

> the qualities possessed in common by plants and animals ought... to be rejected. It is for this reason that we have treated of nutrition, of growth, of reproduction, and even of generation, properties common to the plant and animal, before entering upon those qualities which are peculiar to animated bodies.

Then, despite his earlier assertion that it was neither general nor essential, Buffon decided that motion, along with sensation and certain physiological characteristics, were the key animal attributes. He also declared that "animation, or the principle of life, instead of a metaphysical step in the scale of being, is a physical property common to all matter". Levels of animation varied slowly as one moved along the chain of being and Buffon used the metaphor of sleep to convey this:

> An oyster, or a zoophyte, which appear not to possess either external senses, or the power of progressive motion, are animals destined to sleep continually. A vegetable, in this view, is a sleeping animal: And, in general, every organized being, deprived of sense and motion, may be compared to an animal constrained by Nature to perpetual sleep.[145]

Although naturalists in England tended to shy away from such theories, Buffon's work was still popular among them – but primarily for its descriptions rather than its theorizing. An example of this is to be found in the work of Goldsmith. In 1769 Goldsmith was commissioned by William Griffin to write an eight-volume natural history. This appeared in 1774 under the title *An history of the earth, and animated nature.* Goldsmith's background was more literary than scientific and he intended the book to be amusing and educational; he admitted that "professed naturalists will, no doubt, find it

[145] Buffon [1785] Vol. II, 6-9, 15, 209, 211-2.

superficial" but he hoped it was "not wholly trite or elementary". Goldsmith had initially been inspired by Pliny's natural history writings and had planned to translate Pliny and add some commentary but, he wrote, "upon the appearance... of Mr. Buffon's work, I dropped my former plan, and adopted the present, being convinced by his manner, that the best imitation of the ancients was to write from our own feelings, and to imitate nature". Goldsmith described how he would use Buffon:

> I have taken him for my guide. The warmth of his style, and the brilliancy of his imagination, are inimitable... [O]nly availing myself of his information, I have been content to describe things in my own way; and though many of the materials are taken from him, yet I have added, retrenched, and altered, as I thought proper.[146]

Although Goldsmith used many of Buffon's descriptions, he did not necessarily agree with his theories. Goldsmith's view of the relationship between the plant and animal kingdoms was partly inspired by Buffon, but was also strongly coloured by his own beliefs. He wrote:

> But though it is very easy, without the help of definitions, to distinguish a plant from an animal, yet both possess many properties so much alike, that the two kingdoms, as they are called, seem mixed with each other. Hence, it frequently puzzles the naturalist to tell exactly where animal life begins, and vegetative terminates; nor, indeed, is it easy to resolve, whether some objects offered to view be of the lowest of the animal, or the highest of the vegetable races... Still, therefore, the animal kingdom is far removed above the vegetable; and its lowest denizen is possessed of very great privileges, when compared with the plants with which it is often surrounded.[147]

So for Goldsmith there was a clear gap between the animal and plant kingdoms.

But Goldsmith had to rethink this belief in a distinct gap between the two kingdoms when he came to the zoophyte problem. He classed these beings as a fifth order of insects and, citing the regenerative power of the polyp, described them as "a set of creatures placed between animals and vegetables, and make the shade that connects animated and insensible nature".[148] This idea seems to indicate that Goldsmith may have been swayed by Buffon's notion of imperceptible gradations along a chain of being.

Goldsmith and Buffon also differed when it came to selecting the principal characteristics needed to define animals. There is a passage in *An history of the earth* in

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146 Goldsmith [1774] Vol. I, xi, xiv.
147 Ibid. Vol. II, 3.
148 Ibid. Vol. VII, 243-4.

which Goldsmith discussed the animality of polyps and compared them to sensitive plants:

> The sensitive plant, that moves at the touch, seems to have as much perception as the fresh water polypus, that is possessed of a still slower share of motion. Besides, the sensitive plant will not re-produce upon cutting in pieces, which the polypus is known to do; so that the vegetable production seems to have the superiority. But, notwithstanding this, the polypus hunts for its food, as most other animals do. It changes its situation; and, therefore possesses a power of chusing its food, or retreating from danger.

This gives us an idea of what traits Goldsmith saw as intrinsically animal; his use of motion and nutrition to class the polyp as an animal, while using its mode of reproduction to link it to the plant kingdom, is in line with the ideas of Ellis, Trembley, Buffon and others. He clarified his definition even further in another passage:

> Every animal, by some means or other, finds protection from injury; either from its force, or courage, its swiftness or cunning. Some are protected by hiding in convenient places; and others by taking refuge in an hard resisting shell. But, vegetables are totally unprotected; they are exposed to every assailant, and patiently submissive in every attack. In a word, an animal is an organised being that is in some measure provided for its own security; a vegetable is destitute of every protection.[149]

This is a considerably more complicated definition than any of the ones already mentioned. Motion, nutrition, reproduction, sensation, physiology, the presence of certain chemical compounds, are all essentially stand-alone characteristics. But self-defence must rely on several of these characteristics working in tandem; an animal might typically employ sensation, motion and physiology in evading a predator. Goldsmith's definition demanded not just that animals have several key characteristics but also that those characteristics were coordinated within the creature in a particular way. It is worth noting that each of the other naturalists mentioned so far in this chapter formulated their definitions of animality based on classical ideas, contemporary scholarship, and *their own observational and experimental work*. As far as is known, Goldsmith did not conduct any original research for this book, so his definition was based on a combination of others' research, and deliberation on their findings.

Goldsmith returned to this definition again at the end of the book. The last few chapters of the final volume were given over to zoophytes and Goldsmith admitted that

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[149] Ibid. Vol. II, 2, 3.

"some historians have been at a loss whether to consider them as a superior rank of vegetables, or the humblest order of the animated tribe". Musing on "what it is that lays the line that separates those two great kingdoms from each other", Goldsmith once again dismissed the power of motion (as in sensitive plants) and method of reproduction (as in polyps) and looked to "self-preservation" as the defining feature of the animal. The ability of, for example, an earthworm or a polyp to evade a predator meant that they were "placed many degrees above the highest vegetable of the earth", but the hierarchy of nature decreed that "though these be superior to plants, they are very far beneath their animated fellows of existence". So when Goldsmith had spoken of zoophytes as a group that connected the animal and vegetable kingdoms, he didn't mean to imply a continuum of species; his kingdoms were clearly delineated and zoophytes were merely a subset of the animal kingdom.[150]

In a chapter on the polyp, Goldsmith discussed the work of Ellis, Trembley and Réaumur in some detail. Like the others, he was particularly interested in polyp generation and wrote that:

> their manner of propagation, or rather multiplication, has for some years been the astonishment of all the learned of Europe. They are produced in as great a variety of manners as every species of vegetable. Some polypi are propagated from eggs, as plants are from their seed; some are produced by buds issuing from their bodies, as plants are produced by inoculation, while all may be multiplied by cuttings, and this to a degree of minuteness that exceeds even philosophical perseverance.

These multiple modes of generation were the most striking feature of the polyp; they were the characteristic most likely to render them interesting to a non-specialist reader. Goldsmith used the ability of something so small and so seemingly insignificant to evade "even philosophical perseverance" as a way to evoke a sense of wonder at nature; such evocations helped to move zoophytes from the realm of the specialist to a more mainstream arena. Having thus introduced the fresh-water polyp, Goldsmith went on to describe the various experiments that Trembley and others had performed to demonstrate their animality and vivacity. He also discussed the more recently-discovered animal-flower and agreed with Ellis that it was "no other than an animal of the polypus

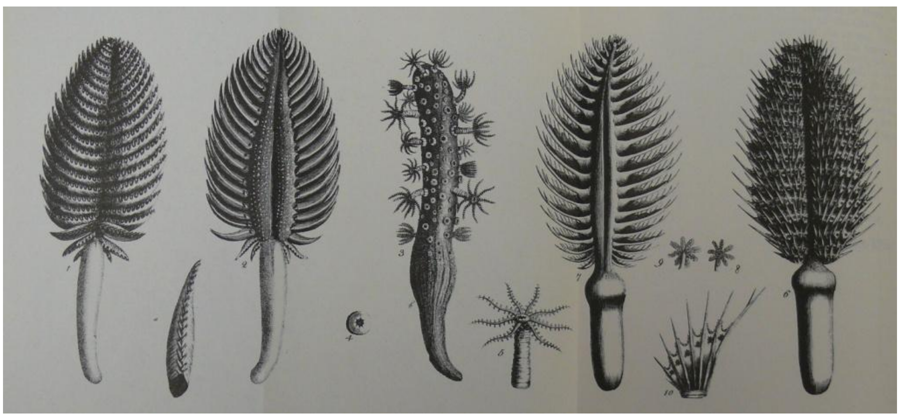
[150] Ibid. Vol. VIII, 161-4.

kind".[151] The final chapter of the work dealt with sponges. The true nature of the sponge had been debated since Aristotle's time. Aristotle had considered the sponge to be a stationary animal endowed with sensation – "this", he declared, "is indicated by the fact that it is more difficult to dislodge, unless the effort to do so is made surreptitiously".[152] In his 1755 *Natural history of the corallines*, Ellis said that he had examined many sponges and yet could not give a satisfactory account of them but, he wrote:

> from many obvious Resemblances to divers other classes of Sea-productions, which are found to be of animal Construction, and from the chemical Analysis of Sponges in general, there seems to be sufficient Reason to induce us to give them a Place here with the rest.[153]

So we see that in his early works Ellis had expressed the belief that a sponge, like the shell of a snail or bivalve, was constructed by an animal living within it, but he was unable to prove this or to find the creatures responsible for sponge-building. Ellis re-visited this problem in a 1763 paper entitled "An Account of the Sea Pen". Having carried out further observations, Ellis was able to say something more concrete about the nature of the sponge and to question the ideas of other naturalists:

> I much doubt whether Sponges have such polype-like suckers as the Corals, Alcyonia, and Pennatulæ, or are even produced by Worms, as the late ingenious Dr. Peysonel informs us... but I am inclined to believe he took this for granted from the similitude they bear to Corals, Alcyonia, &c. rather than from actual experiment. I rather take these holes, which I have observed in them, to be so many mouths upon the surface of the animal.[154]



Sea pens. From Ellis's 1763 article in the *Philosophical Transactions*. Image courtesy of the Whipple Library,

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[151] Ibid. Vol. VIII, 188-91.
[152] Aristotle [1979] 11.
[153] Ellis [1755] 79.
[154] Ellis [1763] 433.

Ellis then described some field-work he had conducted on the Sussex coast in 1762 with his friend Daniel Solander in which they had taken sponges from the sea, placed them in salt-water-filled glass vessels and observed that the sponges opened and shut their surface pores but that no smaller animals were seen to reside in the pores.

Ellis discussed this further in his 1765 article "On the Nature and Formation of Sponges" in which he referred to the sponge as "the lowest being that I have yet observed to have the appearance of animal life".[155] Ellis lamented that the zoophytes and other imperfect animals were so often overlooked by naturalists and sought to draw attention to this "dark part of nature". His aims in this article were three-fold: to describe his sea-side sponge experiments; to discuss the findings of other naturalists, modern and ancient; and to show the similarities between sponges and alcyoniums (a group placed one step above sponges on the chain of being). Ellis travelled to the south coast of England regularly to carry out field-work; his observations of sponges allowed him to challenge the common views that they were either plants or animal fabrications. Naturalists such as Jean André de Peysonnel (1694-1759) had insisted that sponges were constructed by tiny animals, so Ellis was surprised that "instead of seeing any of the polype-like suckers, or any minute animal figure, come out of the papillæ, or small holes with which they are surrounded, we only observed these holes to contract and dilate themselves". Ellis, having repeated these observations many times, was able to conclude that "the sponge is an animal *sui generis*, whose mouths are so many holes or ends of branched tubes opening on its surface; with these it receives its nourishment, and by these it discharges, like the polypes, its excrements". The presence of a digestive system allowed Ellis to classify sponges as animals.

Goldsmith, in his chapter on sponges, described how:

> Philosophers...till of late, thought themselves pretty secure in ascribing these productions [sponges] to the vegetable kingdom... This opinion, however, some time after, began to be shaken...by the ingenious Mr. Ellis who by a more sagacious and diligent enquiry into nature, put it past doubt, that corals and sponges were entirely the work of animals.[156]

Goldsmith was probably only familiar with Ellis's 1755 book, rather than the later papers. This erroneous belief of Goldsmith's (that Ellis had proved that polyps fabricated

[155] Ellis [1765] 280-4.
[156] Goldsmith [1774] Vol. VIII, 194.

sponges) was propagated in several other works of natural history.[157] This was a problem not just because it overlooked Ellis's actual results, but also because it complicated the debate about the plant-animal boundary by introducing the mineral kingdom into the arena.

Goldsmith's *An history of the earth, and animated nature* sold well enough to be reprinted many times.[158] It was popular not just with the reading public but also with other natural history authors – this is evident from the extent to which they borrowed Goldsmith's words and ideas. Just a year after Goldsmith had published his natural history, the Reverend Samuel Ward published *A modern system of natural history*. In his chapter on zoophytes, Ward used several direct quotations from Goldsmith (without acknowledgment): writing of the regenerative abilities of worms, Ward quoted, "This is the most astonishing phenomenon in all natural history, that man should have a kind of creative power, and out of one life make two, each completely formed, with all its apparatus and functions".[159] Likewise, in his 1787 *Surveys of nature*, Charles Taylor (publishing under the pseudonym Francis Fitzgerald) extensively quoted and paraphrased Goldsmith's chapters on zoophytes.[160] Another work that used Goldsmith was a heavily abridged 1791 English edition of Buffon's *Natural history* produced by an anonymous translator. The translator, feeling that Buffon had not written extensively enough on the lower orders of the animal kingdom, "had recourse to that agreeable writer, Dr. Goldsmith, from whose entertaining History of Animated Nature, several of the latter chapters are chiefly extracted".[161] It is significant that, without conducting any original research on zoophytes, Goldsmith appeared to become as respected an authority as Ellis or Trembley. It was his fame as an author and his literary style, as much as his scientific ideas, that contributed to the proliferation of Goldsmith's natural history work. The three books mentioned in this paragraph were all intended for popular audiences.

Buffon's theoretical works, translated and interpreted by writers like Goldsmith for a British audience, thus had a significant impact on ideas about the boundary between

[157] Such as Ward [1776] and Taylor [1787].
[158] Reprints with new illustrations were still being published in the 1850s.
[159] Ward [1776] 177; cf. Goldsmith [1774] Vol. VIII, 171.
[160] Taylor [1787]325-32; cf. Goldsmith [1774] Vol. VIII , Chapter VIII-XII.
[161] Buffon [1791] vi.

animal and plant kingdoms. This theoretical approach, alongside the observations and chemical analyses of naturalists such as Trembley and Ellis, contributed to the overall conception of what it meant to say that something was an animal.

Conclusion

Understanding zoophytes was not straightforward. In order to know these creatures, a naturalist had to combine theoretical knowledge about the plant and animal kingdoms and their organisation with practical skills such as observation and experimentation. Here I have highlighted several kissues; showing that the concept of 'animal' was a nebulous one, and that factors such as nutrition, reproduction sensation, physiology, motion, texture and chemical constitution could be used to determine whether or not something was to be placed in the animal kingdom – but there was no particular consensus among naturalists about which of these factors was most important. I have also emphasised the variety of methods that naturalists could use to investigate each of the above factors; observing, recording and describing the lives of the creatures under consideration was a common approach, but experimentation and laboratory analysis also took place – a fact that is often omitted from histories of natural history. By highlighting the problem of defining a clear boundary between the animal and vegetable kingdoms, and by examining the ways in which naturalists tried to solve this problem, I have been able to consider the relationship between theory and practice in eighteenth-century natural history. This relationship was a complicated one, but studies of creatures at the plant-animal boundary help to elucidate it: by looking at several theories about the nature of zoophytes, and various methods of studying them, we see some of the ways in which natural history ideas and practices interacted.

All of these things – definitions of 'animal', natural history practices, and the relationship between the theoretical and the practical – can be used to broaden our understanding of eighteenth-century natural history in Britain. More particularly, these things can be used to figure out some of the connections between natural history and other branches of scientific knowledge. The case of the zoophyte exemplifies the fact that eighteenth-century natural history was an inherently complex field. Only by combining elements from different branches of science could Ellis and his contemporaries understand these beings: Ellis's work is an excellent example of how fluidly a practitioner could move between natural history, natural philosophy and the chemical sciences, and demonstrates the breadth of science in this period. In many ways, Ellis's work seems closer to that of someone like Stephen Hales (usually classified by historians as a natural philosopher) than it does to the work of well-known eighteenth-century naturalists such as Gilbert White, Daniel Solander or James Edward Smith. Yet Ellis always considered himself a straightforward naturalist, and frequently referred to himself as such in his published works. If we are to accept Ellis's self-definition, then we must concede that neither natural history nor any of the other branches of knowledge discussed in this chapter was a clearly-delineated discipline. Rather, these fields overlapped with each other, all aiming to understand the natural world, and their practitioners did not feel constrained by artificial boundaries.

Zoophytes are especially useful for understanding British natural history in this period because they were particularly difficult to understand and so they forced naturalists to think carefully about issues in natural history often taken for granted: what was a plant, and what was an animal; was there a natural chain of being; was it possible to classify nature? The controversy about what a zoophyte was, and about the best methods for discovering the answer to this question, give useful insight into eighteenth-century natural history precisely because they were controversial. In order to resolve a controversy using empirical research, practitioners must set out their ideas, aims, and practices more clearly than they might ordinarily do. Especially in England, where naturalists tended not to be explicit about their philosophical views, and tried to avoid speculation, zoophyte studies are particularly useful for gleaning information about the theoretical underpinnings of natural history. The reasons for this reticence are not fully clear; it may have that the decline of academic history in England throughout the eighteenth century meant that naturalists there were not equipped to deal with in-depth philosophical speculation; it may have been that Britain was more socially and religiously conservative than other European countries and didn't wish to engage with the complex questions that could arise from zoophyte studies to do with, for example, the nature of individuality or the existence of a soul within an animal body.

Zoophytes were often unprepossessing things studied seriously by only a small number of naturalists. But, despite their low status, their usefulness in answering fundamental questions about the natural world meant that they were particularly important to natural historians; and their usefulness in shedding light upon the practices of natural history means that they are particularly valuable to historians of natural history. This story of the quest to understand zoophytes forms an extremely significant case-study, not just for historians of natural history, but also because the ideas presented in this chapter for looking at one small part of natural history can be extrapolated and used to examine many other eighteenth-century areas of knowledge.

CHAPTER THREE

Newtonian vegetables and perceptive plants

> "My dear friend, I know that every discovery in nature is a treat to you; but in this you will have a feast."
>
> John Ellis describing the Venus flytrap to Carl Linnæus,
> 23rd September 1769[162]

Sensitive plants, like zoophytes, were objects of extraordinary fascination to eighteenth-century naturalists for they existed on the boundary between two kingdoms: the vegetable and the animal. Their fabric and structure, the presence of roots, stems and leaves should have allowed them to be placed within the vegetable kingdom; but their ability to feel, move and react to their environment meant that they could also be considered partially animal. Carl Linnæus, the Swedish botanist and systematist, had codified the divisions between these kingdoms in his famous maxim "stones grow; plants grow and live; animals grow, live and feel", and many naturalists used this formula when classifying specimens.[163] But others considered this definition too simple.

Here I reflect on the problem of understanding the different kingdoms and the relationships between them by examining how several eighteenth-century naturalists approached the problem of sensitive plants. This study not only tells us something about the plants themselves or about definitions of the kingdoms of nature and the workings of the so-called 'chain of being' that connected them; but also, significantly, allows the historian to examine some important aspects of natural history and again demonstrates that eighteenth-century natural history was not a clearly-defined, discrete discipline. Many authors have written about the difficulties of classifying eighteenth-century knowledge, and of drawing boundaries between sciences; here, I wish to examine one particular way

[162] Ellis [1770] 37.
[163] This line from the introduction to Linnæus's *Systema Natura* [1735] reads '*lapides crescunt; vegetabilia crescunt et vivunt; animalia crescunt, vivunt, et sentiunt*' in the original. N.B. the words 'plant' and 'vegetable' were used interchangeably in the eighteenth century and I adopt that usage here.

in which natural history expanded beyond a perceived boundary.[164] In examining zoophyte studies we have already seen that naturalists used a wide range of methods in their work; this work on sensitive plants builds on that but also adds another dimension – the search for explanations. Collecting, describing, naming, categorising and displaying objects from the natural world were not the only activities undertaken by natural historians; for many, trying to understand causes was just as important. Here I will argue that seeking causal explanations was not an activity exclusive to the natural philosopher; many natural historians also sought to explain the world around them.

The study of sensitive plants was undertaken by different naturalists, botanists and plant physiologists who were aware of many species of plant (some native, some recently arrived in the gardens of Europe from far-flung corners of empires) that displayed these curious hybrid properties. Plant physiology was a developing field in this period and its practitioners had to grapple with a host of fundamental questions: what is a plant?; how does it live?; how similar is plant life to animal life?; how can such questions be answered observationally, experimentally, or theoretically? Because of the breadth of its subject matter and research questions, plant physiology could not be an isolated science; its techniques, practices and theories were shared with many other scientific fields of study. Most of the practitioners of plant physiology had trained in medicine, had a good grounding in natural history, and primarily considered themselves 'naturalists'. But these naturalists did not feel constrained by any idea of disciplinary boundaries; they were willing to use whatever tools necessary to understand the lives of plants. They were also willing to create theories, to propose hypotheses and to speculate in their quest to explain plant life.

These naturalists, with their magpie inclination to gather many diverse methods and theories, likewise gathered together an assortment of philosophies to underpin them. Two of the principal ideas in physiological thinking in this period were mechanical philosophy and vitalist philosophy. These two philosophies could be used by naturalists to explain how plants worked and to question the clear-cut plant-animal divide in which

[164]On the difficulties of distinguishing disciplines or drawing boundaries between eighteenth-century areas of knowledge, see Rousseau and Porter [1980] introduction; Schaffer [1980]; Yeo [2003].

Linnæus and so many others believed.[165] Animal physiology in this period has been the subject of several studies by historians, but plant physiology has been largely neglected; while there is some overlap between the two fields, there are also many differences.[166] The naturalists whose work I will discuss here tended towards one of two theories of plant life: plants were most likely to be either called 'Newtonian' and so described as hydraulic systems that followed mechanical laws; or they were living, feeling, perceptive beings that were capable of a certain degree of voluntary action.[167] These two ideas of plants are useful to the historian because they were controversial, and the ensuing debate about which was the true representation of the plant kingdom forced naturalists to think carefully about their methods, results and arguments. Much of the impetus for Newtonian theories of vegetables had come from the works of Stephen Hales (1677-1761) and particularly his *Vegetable staticks* of 1727. This book claimed that plants were hydraulic machines entirely explicable in terms of internal fluid (sap) flow; because plants were simply machines they could be described in numerical terms and Hales's experiments focussed largely on measuring and weighing plant fluids. Throughout the eighteenth century, mechanistic theories of life coexisted with vitalist ones. Vitalism, generally, refers to the belief that the origins and functions of living beings are dependent on some kind of life force that is separate from material, physical, hydraulic or chemical causes, or that there exists 'vital matter'.[168] There were many different strains of vitalism in the eighteenth century, but I will discuss the two particular aspects of vitalism most common in plant physiology – the existence of a vegetable life force and the problem of irritability. The idea of a life force was a very ancient one; strains of vitalism had run through the works of Aristotle and Galen. The concept of irritability had become

[165] My aim here is not to discuss the theories of mechanism or vitalism by themselves, but rather (drawing on more recent historiography about natural historical practices) to contextualise the theories in relation to practice. For more on mechanical theories of life, see Schofield [1970]; for more on vitalism, see Bechtel and Richardson [1998].

[166] One of the main differences is the changing dominance of mechanical and vitalist theories. See Brown [1974] 7, 179-216 for how the two different theories were used in animal physiology, and later sections of this article for their uses in plant physiology. Another difference is the fact that vegetables do not have a heart (equivalent to a pump) and so their internal fluid flow is very different to that of animals, because of this it is more difficult to subject them to mechanical explanations.

[167] The word 'Newtonian' has many uses, I use it here not in the sense of an action-at-a-distance theory, or in the sense of organisms-as-machines, but in relation to hydraulics. In late seventeenth- and eighteenth-century England the word was often used in medicine to denote the use of theories of hydraulics and fluid flow, particularly in relation to secretion and muscle movements. See Guerrini [1987] and Guerrini [1996] for more on this kind of Newtonianism.

[168] See Roe [1981]15 and Lenoir [1982] 9 for more on 'vital matter'.

popular in the mid-eighteenth century. Its main proponent was Albrecht von Haller (1708-1777) who popularized the theory in his 1752 oration, and subsequent publication, *De partibus sensilibus et irritabilibus*. According to Haller, irritability was simply an unconscious reflex of muscle fibres which took place in the exact place where a stimulus had been applied, while sensibility involved nervous transmission so that a reaction was observed in places which had not been directly subject to stimulus. Sensibility was believed by Haller to be linked to nerves, the brain and the soul.[169] This model of how natural beings work was different from earlier models in three important ways: first, it proposed a force that existed in muscle fibres independent of the nervous system or the soul; second, it distinguished the mechanisms of movement and perceptions; and third, it proposed a correlation between structure and function.[170]

In this chapter, I show how mechanical and vitalist theories were applied to the plant kingdom in the later part of eighteenth century; little has been written about this subject.[171] As well as looking at the debate about whether plants were machines or animated beings, at the purposes of natural history, and at natural history's relationship with natural philosophy (and other sciences), I look at specific examples of theory and practice, and at how naturalists investigated plants, interpreted results and formulated arguments. Natural history was primarily a practical activity, so it is important to link the theoretical aspects of naturalists' works with their day to day practices. To this end, I have divided the chapter into two principal sections. The first looks at particular papers written by Thomas Percival (1740-1804) and Robert Townson (1762-1827). In 1785, Percival published a paper in which he used vitalist theory to argue that plants are capable of perceptivity; a few years later, Townson responded with a paper in which he tried to counter Percival's arguments using mechanical theory. These two authors are particularly useful as examples of naturalists who incorporated mechanical and vitalist theories into their work. Their published work also shows how similar results could be used to support very different conceptions of nature. The second section relates largely to theory and practice and shows how four different authors - Francis Penrose (1718-1798), James Perchard Tupper (fl. 1797-1821) James Edward Smith (1759-1828) and Thomas Andrew Knight (1759-1838) - approached the problem of explaining plant behaviour and

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[169] For more on Haller's work, see Steinke [2005].
[170] Ibid. 7
[171] Most works about eighteenth century studies in plant physiology relate to Stephen Hales and the earlier part of the century.

particularly the apparent sensitivity of plants using hypothesis, observation and experiment. The methods used by these naturalists demonstrate the different approaches available to practitioners. These two sections act to show how studies of the vegetable kingdom were carried out, and how they related to the different sciences and different ways of knowing.

Starting from a perspective that sees natural history both as a practical and as a theoretical activity, this study provides a way of investigating how eighteenth-century scientific subjects interacted and overlapped with each other. The characters in this story operated at different levels of natural history expertise and acceptance, from the renowned Smith and celebrated Knight, to the well regarded Townson on to the more obscure Penrose. They also operated using different research methods. But, in spite of their many differences, they had in common a desire not just to describe but to *explain* the things they saw when they looked at the plant kingdom. The overall aim of this chapter is to re-evaluate the idea that natural history was purely a science of description. I will show that natural historians were not solely interested in collecting, describing, naming and grouping objects; many were also interested in understanding causes.[172] The desire to understand causes is often seen as a hallmark of natural philosophy, but I argue that many natural historians of the eighteenth century were unperturbed by any supposed difference between the two branches of knowledge.

Percival's perceptive plant

Thomas Percival believed not only that plants had a life force, were capable of spontaneous motion and experienced sensations, but also that they had genuine powers of perceptivity. In his work, he described plants that were aware of their surroundings and able to respond to them. Robert Townson believed that Percival's work was overly fanciful and that his results, if seen through the lens of mechanical philosophy, could be reinterpreted in a more 'scientific' manner. Here, I will examine each of their papers on the subject in order to show how naturalists tried to understand the vegetable kingdom and how they utilised different philosophies.

[172] For views of natural history as a descriptive science see the introduction to Lyon and Sloan [1981], Pickstone [2000], Rehbock [1983], Rappaport [2003].

Percival had trained as a physician in Edinburgh, London and Leiden. In 1767 he moved to Manchester and became a central figure in the cultural and scientific life of the city; in 1781 he co-founded the Literary and Philosophical Society of Manchester. There, he wrote dozens of books and articles on topics ranging from medicine, chemistry and the sciences to taxation, population growth and morality.[173] Percival was from a Unitarian family and, before training as a physician, had studied at Warrington Academy, one of a number of important dissenting academies in England. Time spent in Edinburgh's medical school further exposed Percival to dissenting and radical views.[174] His theories on the existence of a life force in plants and on their ability to perceive their surroundings reflected his radical views. Vital materialism in the late eighteenth century was not just a scientific theory; its implications for generation theory (especially preformation theory) and the idea of the fixity of species could have important political and social resonances.[175]

In 1785 Percival's *Speculations on the perceptive power of vegetables* was published. In it he gave little indication of what had drawn him to the subject of plant life, he simply declared that he would "attempt to shew, by the several analogies of organisation, life, instinct, spontaneity, and self-motion, that plants, like animals, are endued with the powers, both of perception and enjoyment".[176] Percival's overall aim seems to have been to prove that there was little essential difference between the vegetable and animal kingdoms and so to demonstrate that plants were capable of perceiving their environments and deriving pleasure from them. His motive for this seems to have been natural theological; several times he mentioned the belief that God wished to create a universe in which "the greatest possible sum of happiness exists" and, in order to maximize this, it would make sense that all of creation could experience happiness. So it was necessary that plants could feel.

In the early parts of this paper, Percival appealed to the idea of a "living principle" to support his ideas, writing that

> organization evidently belongs not to inanimate matter; and when we observe, in vegetables, that it is connected with, or instrumental to the powers of growth, of

[173] Nicholson & Pickstone [2004].
[174] For more on Percival's time in Warrington, Edinburgh and Manchester see Haakonssen [1997] 94-120.
[175] See Lenoir [1980] for an example of how vital materialism was used in German scientific and social contexts, and Desmond [1989] for the English and Scottish story.
[176] Percival [1785] 4. This article was published in the Memoirs of the Literary and Philosophical Society of Manchester but the copy I quote here was one of a number privately printed "for Distribution amongst the Author's Friends".

> self-preservation, of motion, and of seminal increase, we cannot hesitate to ascribe to them a LIVING PRINCIPLE. And by admitting this attribute, we advance a step higher in the analogy we are pursuing. For, the idea of life naturally implies some degree of perceptivity.[177]

Unfortunately Percival said little more about what he saw as the self-evident relationship between life and perceptivity.

Percival disagreed with the idea that there was a rigidly fixed boundary between the animal and plant kingdoms; he blamed such a notion on the rise of artificial classification systems in the eighteenth century. He rejected Linnæus's simple formula of *lapides crescunt; vegetabilia crescunt et vivunt; animalia crescunt, vivunt, et sentiunt* and claimed that no-one had yet gathered enough evidence to establish a clear boundary between animal and vegetable. He cited his contemporaries' works on zoophytes and especially corallines and sponges to show how easily a boundary could be moved; it had been only a few years since the researches of John Ellis and others had promoted certain zoophytes from the animal to the vegetable kingdom.[178] Percival began his argument for the perceptivity of plants by citing analogies between members of the two kingdoms.[179] He was particularly interested in plant movement and animal movement. Many naturalists believed that spontaneous motion was something found only in the animal kingdom but Percival hoped that by showing that some plants also exhibited spontaneous motion he could more closely link the kingdoms. In this way, he would be able to argue that plants were likely to have other 'animal' characteristics such as sensitivity and perceptivity. He chose the example of the water lily to illustrate movement in plants. The lily, growing in a pond,

> pushes up its flower-stems, till they reach the open air, that the *farina fecundans* may perform, without injury, its proper office. About seven in the morning, the stalk erects itself, and the flowers rise above the surface of the water: In this state they continue till four in the afternoon, when the stalk becomes relaxed, and the flowers sink and close. The motions of this plant have been long noticed with admiration, as exhibiting the most obvious signs of perceptivity.[180]

He argued that there was no essential difference between this kind of motion and animal motion, and that to attribute special meaning to animal motion while disregarding plant

[177] Ibid. 4.
[178] Ibid. 5-6. See chapter two of this thesis for more on Ellis and his work.
[179] For more on the use of analogies in eighteenth-century life science, see Ritterbush [1964] pp. 1, 2, and chapter 3. Ritterbush believes that analogy, along with electrical theories, lay behind all conceptions of life in this period and that these two things distracted naturalists from serious inquiry. He also says that analogy was used to escape empiricism, p. 64.
[180] Ibid. 12-13.

motion was to "deviate from the soundest rules of philosophizing".[181] Percival also cited the example of an East Indian plant in the order *decandria* whose leaves are in a state of constant motion; even without a stimulus "they are continually moving either upwards, downwards, or in the segment of a circle".[182] Percival considered this to be a sign of "vegetable animation". For many, the idea of 'vegetable animation' would have been an oxymoron; an animal was animated, a vegetable was not, and if it were shown that a vegetable did possess animation (as in the case of sponges, for example) then it was reclassified as animal. Percival supported his belief by quoting Cicero's maxim that *inanimum est omne quod pulsu agitatur externo; quod autem est animal, id motu cietur interiore et suo*.[183] Here, he was giving the power of animal motion to vegetables, and yet maintaining two distinct kingdoms.

Percival also used the concept of 'instinct' to discuss vegetable perceptivity. He defined instinct as "a propensity, or movement to seek, without deliberation, what is agreeable to the particular nature, actuated by it; and to avoid what is incongruous, or hurtful".[184] Percival was keen to emphasise that instinct was not a product of mind; he wrote that "it is a practical power, which requires no previous knowledge or experience; and which pursues a present or future good, without any definite ideas or foresight".[185] As examples of instinct in plants, Percival cited their tendency to grow their roots downwards and their shoots upwards, regardless of the position of the seed (now known as geotropism), their tendency to grow towards a light-source or the sun (now known as phototropism), and the existence of carnivorous plants such as *dionæa muscipula* (also known as the Venus flytrap). In the next section of the paper he linked instinct to 'spontaneity' and wrote that "the impulse to discriminate and to prefer [instinct], is an actual exertion of that principle [spontaneity], however obscure the consciousness of the feeling may be".[186] For Percival, this spontaneity implied volition, "and such volition presuppose[d] an innate perception, both of what is consonant, and what is injurious to the constitution of the individual".[187] At this point, perhaps realising that he was becoming increasingly vague, Percival pulled himself away from "metaphorical

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[181] Ibid. 13.
[182] Ibid. 14.
[183] This line from Cicero's *De natura deorum* translates as "all those moved by external impulses are inanimate; but that is animal that is moved by internal self motion".
[184] Percival [1785] 6.
[185] Ibid.
[186] Ibid. 9.
[187] Ibid.

considerations" and returned to safer ground – concrete experimentation. He related some experiments he had performed that demonstrated geo- and phototropism; in the case of a sprig of mint that he suspended upside-down by the root, he saw the plant's attempt to right itself by curving its shoot upwards as evidence of volition. Earlier in the paper he had ascribed such an effect to instinct, so we see again that Percival saw little difference between instinct and volition. This relationship, between two entities that many saw as entirely disparate, formed the core of Percival's argument for the perceptivity of plants.

Percival, towards the end of the paper, briefly discussed irritability. He disagreed with Haller's belief that irritability and sensibility were distinct from each other; he considered this view to be "evidently a solecism" because "the presence of irritability can only be proved by the experience of irritations, and the idea of irritation involves in it that of feeling".[188] Percival did not engage with the details of Haller's work (and glossed over the three ideas, mentioned earlier, of a muscle force independent of nerves or the soul, separation of movement and perception, and correlation between structure and function) and did not clearly define his terms 'irritability' and 'sensibility' thus making it difficult to see exactly where his disagreements with Haller lay. He did, however, mentioned several experiments in which plants exposed to volatile alkali vapour or sulphur fumes underwent contractions in their fibres; he saw this as evidence of irritability and, by extension, of sensitivity. Thus Percival had used a range of things to argue that plants were capable of perceiving their surroundings: that they were alive; that they had similarities to animals; that they could move; that they could grow in the direction of light or good soil; that they were irritable. But in spite of all of this evidence in favour of his belief in plant perceptivity, Percival still had some doubts. In his conclusion, he admitted that "I review my speculations with much diffidence; and that, I dare not presume to expect that they will produce any permanent conviction in others, because I experience an instability of opinion in myself".[189] But, in the end, his belief that the Creator had wished to ensure "that the greatest possible sum of happiness exists in the universe" convinced him that his theory was the correct one.[190]

[188] Ibid. 13.
[189] Ibid. 15.
[190] Ibid.

In 1792 Robert Townson, who had studied medicine at Edinburgh and natural history at Göttingen, read a paper to the Linnean Society entitled "Objections against the perceptivity of plants, so far as is evinced by their external motions, in Answer to Dr. Percival's memoir in the Manchester Transactions". In this short paper, Townson argued against the kind of vitalistic explanation favoured by Percival and in favour of a return to mechanical thinking. Though this focus on theory may appear to be at odds with the Linnean Society's preoccupation with classification, it was fully in line with their desire to make natural history seem more 'scientific' by emulating the mathematical and physical sciences.[191] Lamenting the state of his science, Townson wrote, "if physiologists have been unsuccessful in many of their enquiries into the animal œconomy, they have been still more so with respect to vegetables: for how little do we know at this day of the course of their fluids, and of the power by which they are moved?". Here, we hear an echo of Stephen Hales; Townson, like Hales, believed that plants could be most fully understood by studying the motions of their sap and he used this mechanical approach to counter Percival's arguments. "It is", wrote Townson, "from their [plants] not having been explained upon mechanical principles that mind has been resorted to. Mind is in general our last resource when we fail in explaining natural phænomena". Indeed, Townson seemed to have had little patience for Percival's approach. Although he admitted that if one were "favourable to the supposition of the existence of a complete chain of beings", it was possible to deduce the possibility of perceptive plants. But he considered such deductions to be the product of "men of warm imaginations, who, prepossessed in favour of an opinion, were grasping at every distant analogy to support it". Townson did not believe that plant motion constituted a proper locomotive faculty, and so any attempt to use it to prove the existence of volition, mind, perception or sensitivity was bound to fail. Townson saw a plant's absorption of fluids as the primary cause of all its motions and, if he could prove this, he could "exclude volition from having any causation in these phænomena".[192]

It was generally agreed among physiologists at this time that plant absorption took place by capillary action, although some maintained that it was due to "active open-mouthed vessels, which in the common opinion takes place in the animal œconomy" –

[191] See chapter one of this thesis for more on the Linnean Society's view on 'scientific' natural history.
[192] Townson [1794] 267-9.

Townson's theory of sap motion fitted with either condition.[193] This theory was based on three suppositions: first, "that an inert fluid is in motion"; second, "that, possessing no motion in itself, it owes this motion to the plant"; and third, drawing on Newton's laws of motion, "that as action and reaction are equal, whilst the plant draws the fluid towards itself, it must be drawn towards the fluid, and that in the reverse ratios of their respective resistances". Capillary action drew fluid into the vessels; the resulting interplay of forces arising from the fluid's effect on the vessels and the vessels' effect on the fluid not only drove the fluids through the vegetable but also caused movement in the plant. Townson could use these simple mechanisms to explain everything that Percival had considered indicative of perception and volition. For example, the tendency of plants to grow their roots in the direction of good soil and their shoots in the direction of light was ascribed to the absorption of water and light, nothing more. The force caused by absorption was small, but it was constant and so could produce the noticeable effects mentioned above. From his mechanical analysis, Townson was able to conclude that plants were entirely explicable in hydraulic terms and that attempts to prove that they were capable of feeling should be numbered "amongst the many ingenious flights of the imagination". Fittingly for a paper presented before the Linnean Society, Townson ended with Linnæus's famous maxim, *vegetabilia crescunt et vivunt; animalia crescunt, vivunt, et sentiunt*.[194]

Townson's views derived primarily from his readings of Hales and Newton. He saw the same effects Percival had seen, but ascribed them to very different causes. The works of Percival and Townson allow us to appreciate how the same phenomena could be interpreted in very different ways and there was little space for meaningful interaction between the two sides. The work of sociologists such as Gieryn has addressed how people like Percival and Townson had to struggle for credibility and negotiate 'epistemic authority'. Gieryn describes a situation in which individuals debated how to decide which camp had jurisdiction over the facts of nature; the debate over whether mechanical or non-mechanical theories were the best way to understand plant motion are a good example of this kind of problem.[195]

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[193] Capillary action refers to the motions of fluid in narrow tubes; because the adhesion of the fluid to the surface of the tube is stronger than the fluid's internal adhesion, the fluid can move up the tube against the force of gravity.
[194] Townson [1794] 269-72.
[195] Gieryn [1999] xi, 15.

As well as looking at the dispute itself, it is interesting to see how the participants used techniques from across an array of scientific fields. While most of Percival's methods fall within what is usually seen as the remit of the naturalist, this is not the case with Townson. Townson applied laws of mechanics to the study of plants. Nowhere in his work is there any indication that this might be a problematic thing to do. He published this paper in the *Transactions of the Linnean Society*; and, although Townson's work was obviously distinct from the taxonomic papers that surrounded it in that journal, the journal's editor accepted it as unquestionably natural history. Furthermore, though Townson deviated from the Linnean Society's norm of publishing primarily on classification, his use of Newton must have struck the society's fellows as a good way to make natural history seem more like the mathematical sciences whose supposed rigour they so wished to emulate. This use of Newton's ideas on fluid forces to solve a problem in natural history shows the lack of boundaries between sciences in this period.

Sensation, irritability and gravity: cause and effect in the vegetable kingdom

Percival and Townson were not alone in seeking explanations for the seemingly odd behaviour of plants. Nor were they alone in relying on a diverse array of methods. Other practitioners interested in apparently sensitive plants, notably James Edward Smith, used the observational methods of botany, while some, such as Francis Penrose, relied mostly on theoretical considerations. Others still, especially Thomas Andrew Knight, developed elegant physical and physiological experiments in order to investigate the workings of the second kingdom. James Perchard Tupper used a combination of all three to argue for the sensitivity of plants. By looking at the works of these men, it becomes clear that naturalists in this period quite cheerfully used techniques and ideas from a range of scientific fields and that natural history was not a closed system.

In 1788, James Edward Smith, founder and president of the Linnean Society, published a paper titled "Some observations on the irritability of vegetables" in which he examined the causes of apparent sensation in plants.[196] Smith was first and foremost a botanist and the work he presented in this paper adhered more closely to standard

[196] This paper was published in the *Philosophical Transactions* of the Royal Society; the first volume of the *Transactions of the Linnean Society* was not published until 1791.

botanical practice than the work of Hales, Percival or Townson. For these men, it was often the case that the specimen under scrutiny was a philosophical tool first, and a plant second; but Smith's work put the plant at centre stage. His style was more conventionally natural historical than that of the others (containing, as it did, many precise descriptions of the physical structures of the plants) and so was possibly more immediately comprehensible to the majority of naturalists and fitted well with the aims of the Linnean Society.

Smith went along to Chelsea Garden on May afternoon to experiment on a barberry shrub. He had heard that this plant had irritable stamens and decided to investigate for himself.[197] He described the plant and his methods:

> the stamina of such of the flowers as were open were bent backwards to each petal, and sheltered themselves under their concave tips. No shaking of the branch appeared to have any effect upon them. With a very small bit of stick I gently touched the inside of one of the filaments, which instantly sprung from the petal with considerable force, striking its anthera against the stigma.[198]

Smith took home three branches of the barberry to continue his investigations. He was trying to answer two particular questions: first, in which part of the stamen did irritability reside; and second, what was its purpose? Although others had written about this plant, Smith was unsatisfied with their work; he wrote that "they have not pursued their inquiries with any great degree of accuracy, but seem mostly to have copied one another".[199] In order to remedy this state of affairs, Smith began his experiments; he removed a petal from the barberry flower without touching the adjacent stamen and began his search for the seat of irritability. He described how:

> with an extremely slender piece of quill, I touched the outside of the filament which had been next the petal, stroaking [sic] it from top to bottom; but it remained perfectly immoveable. With the same instrument I then touched the back of the anthera, then its top, its edges, and at last its inside; still without any effect. But the quill being carried from the anthera down the inside of the filament, it no sooner touched that part than the stamen sprung forwards with great vigour to the stigma.[200]

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[197] The stamen is the male part of a flower, consisting of a pollen-bearing anther atop a filament.
[198] Smith [1788] 158.
[199] Ibid. 161.
[200] Ibid. 159.

Drawing of a barberry flower showing the irritable stamens. By Arthur Henry Church, 1908.

Smith repeated this process many times and with many different instruments and was able to conclude that

> the motion above described was owing to an high degree or irritability in the side of each filament next the germen, by which, when touched, it contracts, that side becomes shorter than the other, and consequently the filament is bent towards the germen. I could not discover any thing particular in the structure of that or any other part of the filament.[201]

Smith did not specify which definition of 'irritability' he was using but it is probable that he was using Haller's – that irritability was just contraction caused by stimulation – or something similar.

Having ascertained which part of the stamen was irritable, Smith next turned to the question of why it was irritable. This he found more straightforward; he explained:

> When the stamina stand in their original position, their antheræ are effectually sheltered from rain by the concavity of the petals. Thus probably they remain till some insect coming to extract honey from the base of the flower, thrusts itself between their filaments, and almost unavoidably touches them in the most irritable part: thus the impregnation of the germen is performed.[202]

So irritability was necessary for the propagation of the species. Smith suggested an experiment to test this theory – if a barberry bush isolated from insects and other stimuli was unable to produce offspring, then Smith's theory would be verified.

Smith was careful to point out that the irritability and subsequent motion of the barberry was a function only of mechanics, he wrote: "we must be careful not to confound them with other movements, which, however wonderful at first sight, are to be

[201] Ibid.
[202] Ibid. 160-1.

explained merely on mechanical principles".[203] He disagreed with the view that all parts of a plant possess irritability and that this was responsible for the motion of their fluids, but he encouraged others to experiment with this. He clearly distinguished between irritability (as in the barberry) and spontaneous motion (as in the *Ruta chalepensis* which can move its stamens without a stimulus) and held that these two phenomena were never observed acting together in the same plant. He used this to draw a boundary between plants and animals: "There still remains then this difference between animals and vegetables, that although some of the latter possess irritability, and others spontaneous motion, even in a superior degree to many of the former, yet those properties have hitherto in animals only been found combined in one and the same part".[204] For Smith, the difference between irritability and sensitivity separated plants from animals. Smith's views, perhaps on account of his celebrity, were influential on other naturalists and many fellows of the Linnean Society adopted a mechanical theory of the vegetable kingdom.

Publishing at approximately the same time as Smith was Francis Penrose. Penrose, like Smith, had trained in medicine. He earned his living as a surgeon, medical writer and pamphleteer. Unlike Smith, his approach to understanding the vegetable kingdom did not rely on acute observation and an in-depth knowledge of the workings of real plants. Much of his work was highly speculative and theoretical. He greatly admired Hales's work and wrote that Hales was "judicious" and that he was "accurate in making, and faithful in relating [experiments], and had no farther Intention to answer but that Truth might appear".[205] But, despite this admiration, Penrose's work was very different from that of Hales. Penrose's *Letters, philosophical and astronomical, in which the following operations of nature are treated of and explained in the most simple and natural manner, according to Isaac Newton's opinions, (viz.) the creation; the deluge; vegetation...* was published in 1789 and received favourable reviews in the *Critical review* and the *Analytical review*.[206] Though this epistolary work was largely concerned with astronomy and calendar reform, it also contained several letters explaining the properties of vegetables in Newtonian terms. Penrose set out to use Newton's idea – that there was an "ethereal Fluid, which filled the Pores of all

[203] Ibid. 162.
[204] Ibid. 163.
[205] Penrose [1789] 178, 185.
[206] Penrose [1794] Advertisement.

Bodies" and that this fluid existed in two conditions (namely hot and cold) - to explain vegetation.[207] He also borrowed from Herman Boerhaave (1668-1738) who had written "that Fire and Motion are synonymous terms" (he gave the example of a metal bar expanding when heated to justify this).[208] Penrose would use the work of these two men to examine "the Phenomina produced by natural Operations" and, because he believed that observing nature was "less equivocal" than performing experiments, he chose vegetation as a case study from which he could extrapolate for other parts of nature.[209] He used experiment and observation to a limited extent in his investigations, and relied heavily on some of Hales's experimental results to support key points of his theory, but his primary tool for his study of nature was the use of hypothesis about particles and natural forces. On the title page of the *Letters*, Penrose included quotations from Newton and Hales that highlighted some of the ideas he would use in this book: from Newton's *Opticks*, "There are AGENTS in Nature able to make the Particles of Bodies stick together by very strong Attractions, and it is the Business of Experimental Philosophy to find them out"; from Hales's *Vegetable staticks*, "We see that Nature exerts a considerable but secret Power in carrying on her Productions, which demonstrates the Wisdom of the Author of Nature, in giving such due Proportions and Directions to these Powers, that they uniformly concur to the Productions of natural Beings"; the final quotation was Hales's comment on the importance of numbering, weighing and measuring (already quoted above). Penrose's interest in causes and in particle interactions was a central theme of his writings on plant physiology.

In three letters to his neighbour and fellow-surgeon John Heaviside (c. 1717-1787) Penrose attempted to explain, in Newtonian terms, how plants grew, lived and reproduced. Only measurable physical entities would be given a place in Penrose's theory of vegetation; he reminded his reader that "we have already observed from *Sir Isaac Newton*, that *Light*, and its perceivable *Effect*, *Heat*, and *Spirit* or *Cold*; together with the *Atmosphere*, *Water* and *Earth*, are the Things required to produce and carry on Vegetation".[210] Penrose's account is largely concerned with heat and cold – he refers to a plant as a "hydraulic machine" which is controlled by the responses of its sap to changes

[207] Penrose [1789] 204.
[208] Ibid. 169, 205.
[209] Ibid. 206.
[210] Ibid. 169.

in temperature. In the spring, according to Penrose, the warmth of the Sun allows buds to break free of their gummy covering,

> the same Heat which dissolves the Gum, penetrates the Bark of the Tree, thins and makes fluid the Juices contained in the *vascular Series* of Vessels, between the Bark and the Wood, and expands them... This expansive Force continually increasing by the fresh addition of Heat, enters the Bud...where the Resistance of the gummy Substance being taken away, it expands his Parts, and spreads open its Leaves.[211]

Penrose appears to have based this part of his theory on some of Hales's experiments; here, Penrose quoted Hales's Experiment XX (relating to temperature measurements and the growing-conditions of plants) at length. In that experiment, Hales had concluded that the Sun's warmth was necessary to supply moisture to the roots of trees.[212]

Penrose also used heat and fluids to explain how a seed germinates: he planted a kidney bean and then described how

> the warm ascending Vapour from the Earth enters its Pores, and forcibly expands the Lobes of the Bean: - This expanding Vapour...is forced from these Lobes into the capillary Vessels of the Radicle, which by continually receiving a fresh Supply, is forced out and elongated. ... The expanded Seed Leaves are forced out of the Ground; after this the Plume being uncovered...the expansive Force opens and unfolds its Leaves, and when they are sufficiently enlarged, so as to throw off the superfluous Moisture, becomes a perfect Plant, and hydraulic Machine, *Sui Generis*".[213]

Thus, Penrose was able to explain all of the key functions of a plant (such as growth and nutrition) using only forces and substances that are quantifiable and explicable in mechanical terms.

In his next letter to Heaviside, also included in the *Letters*, Penrose discussed plant respiration. Again he used Hales's experimental work to support his theory. Penrose wrote that

> after the Plume of the Seed, and the Leaves are pushed out, they perspire or inspire, according *to the Heat*, the *Coldness*, the *Dryness*, or *Moistness* of the Air; in the Day-Time, when the Air is hot and dry, it dilates, and expands their Pores, (most especially when the Sun shines) and rarefies their Juices, whereby it ascends in the Atmosphere; thus a *Vacuum* is made, and Room is given, for the Sap ascending from the Roots![214]

[211] Ibid. 172-3.
[212] Hales [1961] 37.
[213] Penrose [1789] 182-3.
[214] Ibid. 187.

Penrose also used the warmth of the Sun to explain phenomena like the shape of trees, and the fact that plants tend to grow towards the Sun or a light-source such as a window.

Throughout the *Letters* Penrose sought to prove that "a Plant and a Tree are hydraulick Machines, and are supported and made to grow by the two Agents *Light* or *Heat*, and *Spirit* or *Cold*". Animals too, according to Penrose, were machines that responded only to physical stimuli such as heat. Indeed Penrose believed that there were many similarities between plants and animals, writing that "when there is Heat enough to make Vegetables grow, their Juices are in continual Motion, as well as the Blood in Animals" and that "Leaves are formed wide, thin, and full of Pores, and answer the same Purpose to Plants, that the Lungs do to Animals".[215] This mechanical view of the kingdoms of nature is reasonably typical of the time. Penrose's mechanical theory of vegetation did not directly address the problem of sensitive plants, but it could quite easily have been extended to do so; it was part of a larger Newtonian-style physical theory of everything and was intended to answer all sorts of questions about nature.

Smith had located the irritable part of a flower, and he had postulated a reason for the existence of that irritability; but, although he firmly attributed this irritability to mechanics, nowhere did he discuss the exact mechanisms by which it might occur. And Penrose's approach was not sufficiently grounded in observation or experiment to be truly useful to most practitioners of natural history. One author who did explicitly discuss mechanisms affecting plant physiology, and who combined this interest in mechanical theories of nature with rigorous experimentation, was Thomas Andrew Knight. Knight had studied at Oxford but failed to take a degree; his interest in natural history, horticulture and agriculture did not develop until later years. It was later still that he began to study plant physiology. Sir Joseph Banks (1743-1820) encouraged him to send papers to the Royal Society and in 1805 he was elected a fellow of that organisation. In 1806 he was awarded the Copley medal for his work on plant physiology. And in 1807 he was elected a fellow of the Linnean Society. His 1806 *Philosophical Transactions* paper "On the direction of the radicle and germen during the vegetation of seeds" was one of the most successful attempts to discuss plant behaviour with respect to Newtonian mechanics. In this paper he discussed the tendency of plants to grow their roots downwards and their shoots upwards. Many of the authors I have discussed in this

[215] Penrose [1789] 192, 207-8, 213-4.

chapter had tried to explain this phenomenon – some used instinct or volition to account for it, and some used mechanical ideas; but Knight, using an ingenious experiment of his own devising, was able to describe it in terms of Newtonian laws. Although others had tried to explain this tendency as a product of gravity, Knight considered that "the hypothesis of these naturalists does not, however, appear to have been much strengthed [sic] by any facts they were able to adduce in support of it, nor much weakened by the arguments of their opponents".[216] So Knight decided to adduce new facts and so *prove* that this phenomenon was caused only by gravity.

Gravity could only cause roots to grow down, and shoots up, if a "seed remained at rest, and in the same position relative to the attraction of the earth", and Knight "imagined that its [gravity's] operation would become suspended by constant and rapid change of the position of the germinating seed, and that it might be counteracted by the agency of centrifugal force".[217] To test this idea, Knight and his gardener constructed a set of water wheels and set them in a stream running through their garden.

> Round the circumference of [one of the wheels], which was eleven inches in diameter, numerous seeds of the garden bean...were bound, at short distances from each other. The radicles of these seeds were made to point in every direction, some towards the centre of the wheel, and others in the opposite direction; others as tangents to its curve, some pointing backwards, and others forwards, relative to its motion; and others pointing in opposite directions in lines parallel with the axis of the wheels.[218]

Such was the force of the water that the wheel, and the attached seeds, revolved more than 150 times per minute. After a few days the seeds began to germinate and Knight reported that he had

> the pleasure to see that the radicles, in whatever direction they were protruded from the position of the seed, turned their points outwards from the circumference of the wheel... The germens, on the contrary, took the opposite direction, and in a few days their points all met in the centre of the wheel.[219]

Knight then extended the experiment and left three of the plants on the wheel. As they grew, the three shoots crossed at the centre, reached the opposite edge of the wheel and then turned and grew back towards the centre. Knight repeated these experiments with different wheels in different configurations and consistently found that centrifugal force

[216] Knight [1806] 99.
[217] Ibid. 100.
[218] Ibid.
[219] Ibid. 101.

affected the direction of plant growth. He concluded that he had "fully proved that the radicles of germinating seeds are made to descend, and their germens to ascend, by some external cause, and not by any power inherent in vegetable life: and I see little reason to doubt that gravitation is the principal, if not the only agent employed, in this case, by nature".[220] Knight was also able to say why the same force caused the root to grow one way, and the shoot another; he did this by explaining that while the root "increased in length only by new parts successively added to its apex or point", the shoot, "on the contrary, elongates by a general extension of its parts previously organized" – because of their different modes of growth, the root and shoot were affected by gravity in different ways.[221] In his conclusion, Knight denied that there was "any power inherent in vegetable life" that caused plants to send their shoots upwards and roots downwards. Like Penrose, Townson and Smith, he was arguing that plants were simple hydraulic machines. They were not capable of voluntary acts such as sending their roots into particularly nourishing soil or their leaves towards bright light – such phenomena were entirely explicable in mechanical terms. Furthermore, Knight's work was largely dependent on experiment; thus his work was less theoretical and speculative than that of others, and he could elegantly show the effects of gravity on different parts of the growing plant. Knight's papers on plant physiology were very well received and very influential on plant physiologists.[222]

James Perchard Tupper, like many of the other characters in this chapter, had trained in medicine. He developed his interest in botany while still a student at St. Thomas and Guy's Hospital in London. There, the Botanical Chair was held by James Edward Smith who encouraged Tupper in his botanical interests and later admitted him as a fellow of the Linnean Society. Unlike his mentor Smith and the other authors I have discussed here, Tupper did not believe in mechanical explanations for plant behaviour. In 1812, Tupper published *An essay on the probability of sensation in vegetables; with additional observations on instinct, sensation, and irritability*. This work received favourable reviews with, for example, *The medical and critical journal* stating that Tupper's writings "evidently shew

[220] Ibid. 103.
[221] Ibid. 103-4.
[222] Shull & Fisher Stanfield [1939] 1-2.

that he is capable of thinking for himself", that his work was "original", "interesting" and displayed "ingenuity".[223]

Like Percival and many others, Tupper found it almost impossible to see a clear boundary between the animal and vegetable kingdoms. He believed that God's creation was arranged in a descending chain of being and that

> so gradual is this descent throughout the whole system of living beings, that the most inferior of a species resembles in many respects the most perfect of that which is next below it. Hence the transition from the animal to the plant is effected by shades so imperceptible that it is difficult, and perhaps impossible to determine what are those beings which actually form the last link in the scale of animal existence, and the first in that of vegetables.[224]

Tupper discussed several characteristics that other naturalists had used to define animals. These included the power of progressive motion or the presence of a brain or a stomach but Tupper found these definitions wanting. He also disputed Linnæus's definition, *lapides crescunt; vegetabilia crescunt et vivunt; animalia crescunt, vivunt, et sentiunt*, as he was not entirely convinced that plants were destitute of sensation. And, as he said, even if Linnæus's definition were correct, it did not "point out the means, or afford us any practical rule by which we are to ascertain the *existence* or *non-existence* of *sensation* in these beings".[225] Tupper set out to establish the existence of sensation in the plant kingdom. His arguments fell into three broad categories: those from plant-animal analogies; those about instinct and volition; and those about irritability and nerves.

Because Tupper believed in a continuous chain of being, it was easy for him to say that "the œconomy of generation in vegetables is regulated by the same laws as that in animals, [and] plants, like animals, have an internal organization and internal powers of growth".[226] In order to justify his belief in the similarity of the two kingdoms he pointed out that plants, like animals, are affected by climate and season; that both can generate heat; that both are damaged by cold; that both require particular nourishment; that both require air; that both can fall victim to disease, and so on. He used these examples to argue that

> nature in the formation of each has acted upon the same general plan and governs both by the same general laws: and although these circumstances may not be

[223] Anon [1812] 417-8.
[224] Tupper [1812] 4-5.
[225] Ibid. 8.
[226] Ibid. 9-10.

> sufficient of themselves to prove the existence of sensation in plants, they are at least very presumptive evidences in favour of that opinion.[227]

Tupper had great faith in the power of arguments by analogy and later in the paper he explained that "analogical evidence often leads to the discovery of very important truths, and therefore has claims on our attention, until we have better testimony to appeal to".[228]

Tupper saw motion as a particularly interesting characteristic shared by plants and animals. He admitted that vegetables were not capable of locomotion but, nonetheless, they were "endued with certain powers of motion, and many of them even to a greater degree than some of the inferior orders of animals, several species of which are also destitute of *all* locomotive power. This is the case with sponges, sea-pens, and various other zoophytes".[229] Tupper pointed out that the limited motion of plants was entirely suitable for their particular needs and contributed to their well-being. With this in mind, he wondered, "may it not be fairly inferred that they are likewise endued with *instinct*, and consequently with *sensation*?".[230] This question led to Tupper's second line of argument – this one concerned with instinct and volition. Tupper, unlike Percival, was quite certain that plants were not capable of voluntary actions; he agreed with the philosopher John Locke (1632-1704) that "volition is the actual exercise of the power the mind has to order the consideration of any idea, or the forebearing to consider it".[231] Volition, being a faculty of mind and rationality, was not a plant characteristic. Tupper drew a clear line between instinct and volition and so his arguments focussed exclusively on the link between instinct and sensitivity; because of this, his argument is less sprawling and more convincing than Percival's.

Instinct, according to Tupper, was

> a particular disposition or tendency, in a living being, to embrace *without deliberation* or *reflection*, the means of self-preservation, and to perform on particular occasions such other actions as are required by its œconomy, *without* having any *perception* for what end or purposes it acts, or any idea of the utility or advantages of its own operation.[232]

[227] Ibid. 12-13.
[228] Ibid. 54.
[229] Ibid. 13.
[230] Ibid. 14.
[231] Ibid. 15. Tupper quoted this line from Locke's 1690 *Essay Concerning Human Understanding*.
[232] Ibid. 16.

A central theme of Tupper's essay was to determine whether certain plant behaviours were due to this kind of instinct, or to the mechanical principles used by Hales, Penrose, Knight and others. Discussing phototropism, Tupper wrote that

> some naturalists, however, ascribe these effects to the mechanical operation of light; but the evident benefit which a plant derives in consequence of those particular actions, as well as the circumstances attending these, render it most probable that they are the *spontaneous exertion* of that being to avoid what is prejudicial, and to obtain that which is more salutary.[233]

Tupper also saw a plant's reaction to cold weather more as an act of instinct than one of mechanics: he wrote that "many flowers also fold up their leaves on the approach of rain or in cold cloudy weather, and unfold them again when cheered by the reanimating influence of the sun".[234] Where Penrose or Townson saw such reactions as merely hydraulic, Tupper ascribed them to instinct. Tupper also used the example of the water-lily which raises and lowers its stalks at certain times of the day. Several others, including Linnæus, Smith and Percival, had also written about this phenomenon. Smith had explained the cause of this motion as a mechanical effect "entirely owing to the stimulus of light" but Tupper believed that "it is also in part referable to instinct, and that light operates only as an auxiliary to that phænomenon".[235] Sleeping plants also divided naturalists in this way. Tupper described the night-time actions that he considered to be indicative of sleep in plants:

> in some plants the leaves hang down by the side of the stem; in others, they rise and embrace it; and in some they are disposed in such a way as to conceal all the parts of fructification.
>
> Motions of a similar kind also take place in the flowers. Some of these during the night fold themselves up in their calices; some only close their petals, while others incline their mouth or opening towards the ground. The mode of sleep varies, therefore, in different species of plants.[236]

Tupper acknowledged that some naturalists believed that light was the sole cause of such actions, "but", he argued, "although this may have some share in producing those effects, yet, it can only act as a partial cause, which indeed operates in a very similar manner on animals; for the absence of light is also favourable to *their sleep*".[237] Erasmus Darwin (1731-1802) went so far as to claim that sleep was indicative of volition in plants but

[233] Ibid. 20-1.
[234] Ibid. 24.
[235] Ibid. 26-7.
[236] Ibid. 29.
[237] Ibid. 30.

Tupper disagreed and preferred to attribute it to instinct. On this topic, Tupper concluded that "sleep probably indicates the presence of sensation but not necessarily of volition".[238] More generally, he concluded from the evidence he had presented

> that plants have the power of *self-motion*; and as they contribute thereby to their well-being, it is reasonable to conclude that they are, like animals, also capable of instinctive actions: and if *instinct* is the consequence or the necessary adjunct of sensation in *the one*, it is more than probable it is so likewise in *the other*.[239]

Tupper's third line of argument related to irritability and nerves. He acknowledged that there existed "some naturalists who ascribe the motions of which vegetables are capable to *irritability*, a property which they say may exist in organized matter independently of sensation".[240] Tupper set out to refute this idea and he began by re-defining 'irritability'. He believed that the general definition – that irritability was a property that caused muscles to contract when subjected to certain stimuli – was inappropriate and so he created his own. Like Percival, Tupper did not clearly set out exactly why he disagreed with Haller's definition of irritability, but the following conception rejects Haller's idea of separation of mindless muscular contraction from nervous response. Believing that "*irritability* is a particular power, which is *coëval* with the *living principle* itself of the individual, and *continually* operating in a greater or less degree, so long as the principle of life exists", he re-defined irritability as "a particular *inherent power* or *property*, which is *continually* operating in a living body, and in consequence of which its *natural actions* may be *more* or *less* increased by some fresh exciting cause".[241] Perhaps it was because viewing irritability as a function of sensibility fitted better with his idea that plants, by analogy with animals, must be sensitive that Tupper rejected Haller's definition of irritability. Although Tupper linked irritability to a 'living principle', he chose not to discuss this principle or its source; rather than discussing its cause, he would consider its effects. By examining these effects, he hoped to show that

> the *irritability* of which plants are possessed is another, and very powerful, evidence of their sensation. Surely, it would be very inconsistent to suppose that a living being, so nearly allied to animals in organization, should be *destitute* of sensation, and yet at the *same* time susceptible of impressions.[242]

[238] Ibid. 34.
[239] Ibid. 32.
[240] Ibid. 42.
[241] Ibid. 43-4.
[242] Ibid. 45.

By linking irritability and sensation in this way, Tupper was in direct disagreement with Haller's definitions. He also disagreed with the notion that plant irritability was inherently different to animal irritability; he used the concept of a 'living principle' to argue that

> *irritability*, whether in the plant or the animal, cannot exist independently of life: therefore, if the principle of *irritability* is not of the same nature in both, then, it must necessarily follow that the *living principle* of *the one* is *different* from that of *the other*. But this is an idea which would be inconsistent with philosophy and in opposition to her laws, which direct us, *not to ascribe similar effects to different causes*.[243]

So, according to Tupper's use of the terms 'irritability' and 'living principle', plants were sensitive.

Tupper next sought out the seat of this sensitivity: he looked for plant nerves. Generally, organs that perform the same functions in plants and animals do not have the same structure; we see this in the case of the organs of reproduction or respiration. So Tupper believed that plant nerves would not necessarily look like animal nerves. He admonished those who denied the existence of plant nerves, he compared them to those who

> contend that fishes are destitute of lungs, because their organs of respiration are different in structure, and even differently situated with regard to the other viscera, from the lungs of terrene animals. But we know that those particular organs to which we give the name of *gills* are to the fish, what the lungs are to the animal destined to live on land.[244]

Tupper was not able to find plant nerves by dissection or observation, so he relied primarily on analogies from the animal kingdom to argue for their existence. He also used the irritability of plants to argue that they must have nerves. As we have seen, Tupper disagreed with Haller's separation of irritability from the nervous system. He wrote that many physiologists believed that "[irritability] is derived from the nerves; and if so, as plants are *irritable*, and as the irritability appears to be of the same nature in them as in animals, may we not reasonably infer that they have also a nervous system, or at least something analogous to it, and consequently some kind of sensation?".[245]

In this section, I have examined in some detail the methods and conclusions of four men studying the vegetable kingdom at the end of the eighteenth century. There was not one single way of investigating this kingdom. Nor was there a straightforward way of

[243] Ibid. 48.
[244] Ibid. 51.
[245] Ibid. 57.

comparing and evaluating the methods used. Knight's work, which relied heavily on ingeniously constructed experiments, seems to have received the most praise, and he was awarded the Royal Society's Copley medal for these, and other, experiments. Smith, whose work was more observational than experimental, was also well received. Penrose's highly speculative work also received some positive reviews. The works of these four authors show that naturalists used a wide variety of techniques in their quest to understand the natural world. As well as showing the breadth of natural historical practice, these case studies highlight some of the ways in which practices feed into theories.

Conclusion

The boundary between the plant and animal kingdoms was not fixed. Nor were the kinds of evidence that naturalists could use to determine truths about the essential characteristics of plants and animals. I have shown that naturalists had many different ways to distinguish between, or link together, the plant and animal kingdoms: the ability to move; the presence of a brain or stomach; the existence of a chain of being; structural and functional analogies; living principles; irritability; instinct; and sensitivity. But these were used differently by different naturalists; according to how each naturalist ranked the importance of these characteristics, they formulated different theories about what the kingdoms were and how they were related. We see that Percival and Tupper, who both strongly believed in a continuous chain of being, were more likely to attribute irritability, instinct and sensitivity to the vegetable kingdom. Generally, strong belief in a continuous chain of being implied belief in the existence of life force in the plant kingdom, and this implied the belief that characteristics usually attributed to animals could also be found in plants. Naturalists who did not strongly believe in continuity between the kingdoms, were less likely to use the idea of a life force in their theories, and more likely to use mechanical principles to explain plant behaviour – we see this with Townson, Smith, Penrose and Knight.

The developing science of plant physiology did not have a separate disciplinary identity in this period; it was deeply intertwined with natural history, medicine, animal physiology and natural philosophy. This chapter shows how studies of the physiology of plants used techniques, practices, ideas and theories from a range of other scientific fields. Naturalists used a range of methods to try to legitimate their work and gain credibility for their beliefs. Naturalists could also use their research as a basis for applying mechanistic or vitalist theories to the vegetable kingdom. The mechanical and vitalist theories that had been developing throughout the previous century were put to use in very different ways by men like Percival and Townson. These men applied different theories to similar sets of results and came up with contrasting visions of nature. The struggle to produce a comprehensive view of the vegetable kingdom made naturalists think more clearly about their aims and methods. Townson's critique of Percival highlights what he considered most important in natural history. A rational, scientific, mechanical view of nature would save naturalists from having to resort to mind, volition, will, perception, or other such "flights of the imagination".[246] Townson, and many of the other authors cited here, firmly believed that seeing natural objects as hydraulic machines was the key to understanding nature. There was much at stake for these men: not only was their authority and influence as naturalists on the line; but also their right and ability to explain the workings of nature.[247] These debates were so hotly contested due to the larger ideological implications of materialism and vitalism – especially for the medical community. Townson and Percival had both studied medicine in Edinburgh and on the continent and would have been aware of debates over different kinds of matter, over generation theories, and over the essence of life.[248] The existence of two competing world views forced naturalists to clarify their own arguments and criticize those of others; these clarifications and criticisms are useful to the historian in understanding the content and mechanisms of the different branches of natural history.

Another aspect of this chapter has been to discuss the theoretical aspects of studies of the vegetable kingdom in relation to the *practices* of natural history. I have mentioned the importance of mechanical and vitalist interpretations of the results of investigations of plants; but ancillary to how naturalists interpret their results are the methods they use to produce those results. The second section gave examples of how Smith, Penrose, Knight and Tupper used different methods and techniques to produce a

[246] Townson [1794] 272.
[247] Gieryn [1999]15.
[248] One obvious example of such a debate is that between Haller and Caspar Friedrich Wolff (1734-1794) over whether generation occurs through the existence of pre-formed animalcula, or through the process of epigenesis – see Roe [1981] for more details. For an overview of similar debates in Britain and their moral and politcal implications, see Jacyna [1983].

range of results – the methods ranged from observation, description and analogy to experiment. These were then used to support or refute theories of vegetables as hydraulic machines or of sensitive plants as possessing a life force. The number of possible methods is indicative of how natural history was conducted in this period.

From these three things – understanding some of the ways in which naturalists conceptualised the plant and animal kingdoms, understanding how plant physiology was studied, and understanding the interactions of theory and practice – we develop a new appreciation of what natural history was in the eighteenth century. The study of plants, and of sensitive plants in particular, gives rise to useful examples concerning the conduct of natural history; we see the extent to which natural historians were willing to engage with philosophical questions, and the extent to which natural history interacted with other fields, most especially natural philosophy. The authors featured in this chapter were mostly medical men who considered themselves 'naturalists'. I have shown how open these naturalists were to using methods and theories from all areas of the sciences. The case studies given here demonstrate the fluidity of the sciences in the eighteenth century. It is impossible for historians to give a simple definition of any of these fields of knowledge because the actors themselves did not recognize any such clear-cut definitions. It is evident from these authors that naturalists can and did concern themselves with causes; the desire to explain nature was not the exclusive province of the natural philosopher. With this in mind, it is possible to create a broader view of natural history that encompasses the aspects of the subject that were most important to naturalists. Many naturalists were preoccupied with collecting, classifying, naming and displaying objects, but to define natural history based only on these activities is to exclude a significant part of the field. Naturalists used methods from physical, chemical and other sciences, and developed many theories of life.

CHAPTER FOUR

The sexes of plants

"BOTANIC MUSE! who in this latter age
Led by your airy hand the Swedish sage,
Bad his keen eye your secret haunts explore
On dewy dell, high wood, and winding shore;
...
First the tall CANNA lifts his curled brow
Erect to heaven, and plights his nuptual vow;
The virtuous pair, in milder regions born,
Dread the rude blast of Autumn's icy morn;
Round the chill fair he folds his crimson vest,
And clasps the timorous beauty to his breast."

Erasmus Darwin
The loves of the plants, 1789[249]

Looking at studies of zoophytes and sensitive plants shows how important experimenting and hypothesising were in eighteenth-century British natural history. I have deliberately focused attention on natural historical activities that are commonly disregarded or neglected by historians; but even more obvious activities such as classification can be used to demonstrate the practical and theoretical breadth of the science. As the first chapter showed, many British naturalists embraced the Linnean system of sexual classification; they did this for its utility, its simplicity and its easy applicability and rarely had any cause to question its basic principles. But some naturalists asked deeper questions about this system; here, I explore why these naturalists attempted to shake the foundations of the Linnean system that was quickly gaining popularity throughout Britain, and how they achieved this through ingenious experiments on ordinary plants such as spinach, hemp and pumpkins in the gardens and greenhouses of Edinburgh. This story highlights two very different kinds of natural history that were going on and emphasises the heterogeneity of the science.

[249] Darwin [1789] 3-4.

According to Linnean botanical taxonomy plants, like animals, had sexes. Linnæus's *Systema Naturæ* had introduced a new classification system in which plants were grouped solely on the structure of their flowers. The system, which Linnæus readily admitted was artificial, rested on the belief that certain parts of the flower were responsible for reproduction and that this reproduction took place in a manner analogous to that in animals – i.e. that both male and female elements were necessary for the production of offspring. The number and arrangement of these so-called male and female parts of the flower (the stamen and pistil, respectively, according to Linnæus) were used to define taxonomic groups. For example, flowers with one stamen made up the first class, named monandria, flowers with two stamens resided in the second class, diandria, and so on; these classes were then subdivided into orders based on the number of pistils, the first order was made up of flowers with a single pistil and called monogynia, each subsequent order contained an additional pistil. This simple system made botany accessible to many and it could be practically employed without having to consider any of the deeper implications about the nature of the vegetable kingdom or its similarities to the animal kingdom.

But some naturalists, though aware of the utility of Linnæus's system, were not happy to accept its basic assumptions. Here I use the work of two Scottish naturalists - Charles Alston (1685-1760) and William Smellie - to show why the idea that plants have sexes might be questioned, to show how logic and experimentation could be used to examine this idea, and to see what kinds of natural history were practiced by different actors. I contrast their beliefs and findings with those of Linnæus himself and of James Edward Smith, one of Linnæus's main supporters in Britain. While there have been some studies of the sexual theory of plants they have largely been written through the lens of gender studies, literary studies, or history of ideas.[250] Though plant sex has been

[250] Londa Schiebinger has written extensively on the theory in her work on gender and science, e.g. see *Nature's body* [1993]. For a more literary approach, see Janet Browne's 'Botany for gentlemen: Erasmus Darwin and *The loves of the plants*' *Isis* [1989]. For a more theory-focussed account, see Philip Ritterbush's *Overtures to biology* [1964], John Farley's *Gametes and spores* [1982] and James Larson's article 'Linnæus and the natural method' *Isis* [1967].

extensively studied, it is rarely written about in relation to the wider practices of natural history or used to make more general statements about the sciences.[251]

Linnæus, Smith and *A dissertation on the sexes of plants*

This is a story about different ways of seeing nature. On a smaller scale, it is about the difference in believing that flowers are really male, female or hermaphrodite; on a larger scale, it is about the difference in believing that nature is best understood through cataloguing her productions or through deeper philosophical considerations. Here, I discuss the works of Carl Linnæus, Charles Alston and William Smellie on the sexes of plants. In some ways, the three works are quite similar: they all grapple with problems such as the appropriate use of analogy and ways to eliminate errors in experiments. But they differ in their goals, reception and uses. The version of Linnæus's work that I examine is a translation by James Edward Smith, the Linnean Society's founder and president. It was very much intended as a tool in the spread of the Linnean system in Britain. Alston and Smellie (like so many other Scots) were not involved in the Linnean Society and their works were produced in a different natural historical tradition; Edinburgh natural history tended to have more in common with French natural history than with its English counterpart and Alston and Smellie's works reflect this.[252] Their desire to understand truths of nature rather than to have a convenient (but artificial) classification system is reminiscent of French authors such as Joseph Pitton de Tournefort (1656-1708), Buffon and the Jussieus of the Parisian *Jardin des Plantes*. But for Linnæus and Smith, making natural history accessible to a larger number of people and providing a simple universal system took priority. These two different ideologies lay behind the two different natural histories to be examined in this section. These ideologies would also affect how the two natural histories interacted. Alston and Smellie believed that classification was an important activity and had no wish to detract from it; they simply thought that some of Linnæus's basic tenets were misguided. They were also not

[251] One notable exception to this is Larson's work on the German botanist Koelreuter's studies of plant hybrids. Here, Larson argues that Koelreuter's work was part of an eighteenth-century drive to quantify the objects of natural history. Larson [1990].
[252] For more on Scotland's (and particularly Smellie's) scientific relationship to France, see Loveland [2004b].

part of a group such as the Linnean Society which wished to drive British natural history in a particular direction. On the other hand, Smith and his fellows thought that engaging in philosophical lines of enquiry, or anything that might lead to dispute, was unprofitable for natural history, and they had established a group for the express purpose of eliminating such threats. The natural history of the Linnean Society was not compatible with many other kinds of natural history; the story of plant sex illustrates why this was and how that affected British natural history.

From as early as the 1720s, Linnæus had been promoting his theory that plants had distinct male and female parts, that these parts were the most essential elements of plants, and that they should therefore be used as the basis of a classification system.[253] Both of these ideas – that plants had male and female parts and that a classification system could be based on a single characteristic – had already existed in natural history. Several naturalists such as Thomas Millington, Nehemiah Grew and John Ray in England, Sebastien Vaillant in France and Rudolf Jakob Camerarius in Tübingen had suggested plant sexual reproduction in the late seventeenth and early eighteenth centuries.[254] And the seemingly impossible task of discovering a natural system of classification had led most naturalists to use artificial classifications instead. Although Linnæus had combined a sexual theory of plants with an artificial classification system as early as the 1720s and 1730s, it was not until 1759 that he fully explained the reasoning and experiments that had led him to this result. In that year, the Imperial Academy of St. Petersburg offered a prize for the best dissertation on the theory of the sexes of plants. Many believed that the Academy offered the prize expressly to encourage Linnæus to explain his beliefs more fully; Linnæus responded to the challenge with his *Dissertation on the sexes of plants*.[255]

This work was written in Latin but in 1786 James Edward Smith translated it into English. Smith owned most of the copies of the original edition of the *Dissertation* since he had purchased the Linnean library. He worried that the tract was not sufficiently well known and had been overlooked by other scholars, so Smith determined to popularise it

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[253] See Müller-Wille [2007b] for more on Linnæus's views on natural and artificial classification, and for more on the common belief that Linnæus was an essentialist. See Ritterbush [1964] 109-122 for a detailed analysis of Linnæus's theory of plant sexuality.
[254] Schiebinger [1993] 5-8; Farley [1982] 41.
[255] Linnæus/Smith [1786] v.

through a translation.[256] In his preface to the work, Smith addressed some of the controversies that had surrounded Linnæus's ideas: Lazzaro Spallanzani (1729-1799), for example, had conducted some of the same experiments as Linnæus but with very different results; Michel Adanson (1727-1806) had attacked the system, but Smith claimed that this was for personal rather than scientific reasons.[257] Smith had little time for these detractors and wrote that:

> [Linnæus's] opponents of this kind, as well as those who on making use of their judgment disapproved of his publications, are now, with their works, for the most part gone off the stage. The futility of their objections has been repeatedly shown by many of Linnæus's pupils and admirers, he himself scarce ever deigned to notice them, and trusting for his justification to time alone. Its decision has been most completely in his favour.[258]

Smith attributed any naturalist's disapproval of Linnæus's work to personal reasons or professional jealousy and refused to entertain the notion of any scientific basis for their objections to the sexual system. But Smith's counter-attacks were hardly impersonal: about Adanson, who preferred more natural classification systems, Smith wrote that

> in spite of all opposition, the system of Linnæus is even now become universal, every part of the world abounding with his disciples; while the "Familles des Plantes" of Monsieur Adanson, professedly written to supersede it, is only occasionally read by those who are disposed to amuse themselves with whimsical paradoxes, presenting themselves in a preposterous orthography, which renders them still more ridiculous and unintelligible.[259]

Image of a date palm in Friedrich Johann Bertuch's *Bilderbuch für kinder*, published between 1790 and 1830. Image from panteek.com.

[256] Ibid. v-xiii.
[257] Ibid. vi-x.
[258] Ibid. x.
[259] Ibid. xii-xiii.

This was the kind of ridicule to which one risked exposing oneself by questioning Linnæus's doctrine, and yet some did question it. Before looking at the objectors' arguments against Linnæus, it is worth looking at his own explanation of the theory. *A dissertation on the sexes of plants* is a 62-page work in which Linnæus uses analogy, morphology, case studies, hybrid theory, physiology and 13 experiments to argue that plants have male and female parts. Linnæus claimed that this was the case for every single vegetable and that the historical record showed that many different cultures had long been aware of this – particularly in countries where the date palm was cultivated.[260] Linnæus explained that he began to turn his attention to the parts of a plant that seemed to be responsible for fructification for practical purposes. Up to that point, several different artificial systems had been in use – some naturalists used the petals to distinguish species, and some used the fruit. But, wrote Linnæus, these naturalists "did not take time duly to consider the minuter parts of the flower, till they found the larger quite insufficient to discriminate the immense numbers of vegetables, which were daily augmenting the catalogue of Flora".[261] The need to distinguish large numbers of plants easily was what led Linnæus to look at stamens and pistils in a new way. He considered these parts to be "essential" – no flower existed without them. The ubiquity of pistils and stamens formed the first strand of Linnæus's argument.

The second strand of the argument was drawn from the great chain of being – that supposed link that connected all parts of creation, running from man at the top, down through all the animals and on to the vegetable kingdom. Linnæus used this chain as a justification for analogies between plants and animals. He argued that the bodies of humans and the higher animals consisted of two principal parts: the nervous system (which was made from a medullary substance) and the vascular system (made from a cortical substance). Linnæus insisted that an analogy could be drawn with the plant kingdom: plants too had a cortical substance that was responsible for nourishing them by transporting fluids, and a medullary substance "which is the other essential part of vegetables, is multiplied and extended without end; and whenever it is entirely lost, the death of the plant necessarily follows".[262] There were other analogies too: wood was

[260] 'Female' palm trees had to be exposed to branches from 'male' trees before they could produce fruit. See also the discussion of the Berlin date palm later in this chapter.
[261] Linnæus/Smith [1786] 4.
[262] Ibid. 9-13.

equivalent to bone; the development of a flower from a plant was likened to the development of a butterfly from a caterpillar. Flowers and butterflies existed just to propagate the species and the only real difference between them, according to Linnæus, was that flowers were stationary while butterflies could move.[263]

A third part of the argument came from studies of generation and hybrids. Hybrids were useful in showing what each parent contributed to the offspring. Linnæus believed that studies of hybrid creatures such as mules showed that the mother supplied the medullary substance, or nervous system, while the father gave the cortical substance, or vascular system. More important than which parent contributed what was the fact that each parent contributed something. Each parent was responsible for some part of the offspring; and Linnæus believed that this was also the case with plants. He argued that a plant's stamens (the 'male' part according to the sexual system) originate from its woody part and inner bark which are derived from a cortical substance.[264] Pistils, on the other hand, which were 'female' and located at the centre of the flower, were derived from a medullary substance. Therefore both pistils and stamens had to contribute something to the seed in order for a whole plant to be produced. Linnæus also noted that no plant existed without stamens and pistils.[265] From these facts he concluded that "the stamina are the male organs of generation, and the pistilla the female".[266] Linnæus then went on to explain the mechanics of how pollen, or "fecundating powder", was transferred from the stamen to the pistil, and on to the stigma, so stimulating the production of viable seeds.[267]

In addition to these three arguments – from the ubiquity of stamens and pistils, analogy, and hybrids – Linnæus also used a series of experiments to confirm his theory of the sexes of plants. These experiments mostly involved removing pistils or stamens from plants, isolating plants, or introducing foreign pollen, and then observing whether fertile seeds were produced. For example, the first experiment related how

> one evening in the month of August, [Linnæus] removed all the stamina from three flowers of the *Mirabilis longiflora*, at the same time destroying all the rest of the flowers which were expanded; [he] sprinkled these three flowers with the

[263] Ibid. 22.
[264]Ibid. 20-1.
[265] Though he did concede that he had not yet found stamens or pistils in plants such as mosses, he called these cryptogamia and accused them of having clandestine marriages.
[266] Linnæus/Smith [1786] 25.
[267] Ibid. 26-7.

> pollen of *Mirabilis Jalappa*; the seed buds swelled, but did not ripen. Another evening [he] performed a similar experiment, only sprinkling the flowers with the pollen of the same species; all these flowers produced ripe seeds.[268]

Most of the other experiments were along similar lines (though each was subtly different). Linnæus was keenly aware of possible counter-arguments to his sexual theory and used the experiments to dismiss them. He knew, for example, that Tournefort had not believed that stamens played any very significant role in generation and so Linnæus performed several experiments to show that generation did not occur if a plant's stamens were removed.[269] Linnæus was not the only naturalist to perform these kinds of experiment, but not all naturalists' results agreed. In one of Linnæus's experiments, he planted hemp seeds in two different pots in separate locations. In the first pot he left the 'male' and 'female' plants to grow together while in the second, he removed the 'males'. He gathered seeds from the first set of hemp plants, sowed them and successfully raised a new generation; the second set of plants never produced ripe seeds. But some naturalists had had a different result: they had found that the 'female'-only pot of hemp plants produced viable seeds. Linnæus attributed their result to the pots not being sufficiently isolated and suggested that the plants had probably been fertilised by wind-borne pollen.[270] By suggesting such possible sources of error in others' methods, Linnæus was hoping to add credence to his own experiments and theories.

Linnæus ended his description of these experiments with the declaration that "all nature proclaims the truth I have endeavoured to inculcate, and every flower bears witness to it. Any person may make the experiment for himself, with any plant he pleases".[271] Linnæus was certain about his results and conclusions. But not everyone shared his certainty. Smith included a footnote at this point in which he admitted that "a charge ... has been brought against Linnæus, of asserting the generation of plants in too absolute and positive a manner, from a few experiments only". But Smith defended his hero against such charges, claiming that they were entirely without foundation. [272] Linnæus's certainty about the sexual theory of plants was not only drawn from the kinds of analogy and scientific experiment mentioned above. It was also dependent on tacit

[268] Ibid. 29-30.
[269] Ibid. 30.
[270] Ibid. 32-3.
[271] Ibid. 43-4.
[272] Ibid. 43.

knowledge gleaned from nurserymen and gardeners, from common knowledge of common plants, and from ancient sources such as Theophrastus. Towards the end of the *Dissertation,* Linnæus returned again to the subject of hybrids – for here he had found the most conclusive evidence in favour of his theory. He listed four hybrid species – such as the *veronica spuria* which "agrees perfectly with its mother in fructification, and with its father in leaves" – that he believed provided the final pieces of evidence needed to verify his theory.[273] All of these four plants exhibited some characteristics inherited from each parent and so seemed to be perfectly analogous in their modes of generation; from this, Linnæus drew his final conclusion "that the sexes of plants admit of a proof *a priori* from experiments, appears therefore from hybrid productions".[274]

Linnæus's theory of plant reproduction and his supporting arguments were the foundation of his classification system. It was for this reason that Smith felt it necessary to popularise this work. Smith's translation was made up of two parts: a faithful translation of Linnæus's scientific theory; and numerous footnotes and additional comments of Smith's own. The analogies, experiments and case studies of Linnæus seemed to give a solid basis for the sexual system of classification; they could therefore also be used to legitimate the work of the Linnean Society. The additional comments by Smith were mostly personal attacks on naturalists – Spallanzani, Adanson, Scopoli, Alston, Tournefort, Pontedera – who had criticised Linnæus's theory; and occasional notes on discoveries made since the original 1759 publication of the dissertation. For Smith, the treatise was more than just a scientific document; it was an instrument that could be used to further advance the agenda of the Linnean Society. Smith's translation was published in 1786 – after his purchase of the Linnean collections and after he had conceived the idea of founding a society around them; it came two years before his opening presidential address to the new society in which he declared that "nothing will be with more reason expected from the members of this society than a strict attention to the laws and principles of Linnæus".[275] White has suggested that a "religious orthodoxy" permeated the early Linnean Society.[276] But Smith and the fellows were aware of this characterisation and fought against it; for example, when the Dutch naturalist Petrus

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273 Ibid. 52-4.
274 Ibid. 57.
275 Smith [1791] 53.
276 White [1999] 128.

Camper (1722-1789) refused an honorary membership of the new society because it was explicitly linked to Linnæus, Smith assured him that

> The Linnean Society is a body of naturalists associated for the purpose of cultivating the Science, not to enlist themselves as the followers of any person whatever, any further than truth directs them... We consider [Linnæus's] works as a good foundation to work upon, we are best able to determine the different objects he described, to correct his errors & improve what he has left imperfect. On this ground we call ourselves the Linnean Society...[277]

But though the Linneans were willing to admit that Linnæus had made mistakes and wished to correct them, they only ever made minor corrections of species names or placement in classification schemes. They did not seek to question Linnæus's larger programme (the promotion of the sexual system) or its basis (the sexual theory). Commentators of the time recognised this: a reviewer of the first volume of the *Transactions*, writing in *The critical review*, felt that perhaps the Linnean Society had a personal as well as professional devotion to Linnæus. The review began:

> The possessors of the Linnean collection consider, very properly, that with it the task of cherishing the author's fame and defending his system has devolved. They do not decline it; and, while as natural historians, in general, they confess his merits, they seem to feel the more intimate connection, which excites their zeal and adds to their ardour;

and it ended by stating that "the great object of the Society [is] to establish and correct the immense system of the Swedish naturalist".[278]

Scottish objections to the sexual system

Though Smith and the Linnean fellows were dedicated to the orthodoxy of their figurehead, and though Linnæus's arguments in favour of the sexes of plants seemed convincing to many, several notable naturalists questioned them. On the continent, Spallanzani, Giulio Pontedera (1688-1757), Adanson, Tournefort and many other (particularly French) naturalists rejected the idea of plant sexes. In Britain, most of the serious objections came from Scotland. Here, I examine some of the writings of two of Linnæus's most vocal critics – Charles Alston and William Smellie. Alston was the

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[277] Gage and Stearn [1988] 13.
[278] Anon. *Critical review* [1792] 1, 13.

Professor of Materia Medica and Botany at the University of Edinburgh, and Smellie was an Edinburgh publisher, printer and naturalist who had also studied at the university. I wish to see which parts of Linnæus's logic and evidence they questioned, what their own theories and methods were, why their objections to Linnæus's theory were never widely accepted, and how their style of natural history differed to that practiced at the Linnean Society.

In 1754, Alston published his *Dissertation on botany*. Although this appeared before Linnæus's 1759 *Dissertation on the sexes of plants*, Linnæus had already written about many parts of his theory and so Alston would have been familiar with his supporting arguments. Alston's *Dissertation* received reasonably good reviews; *The monthly review* was impressed by his "large fund of botanical knowledge" but when it came to what it considered the most attention-grabbing part of the book – i.e. the discussion of the sexes of plants – it remained neutral. The reviewer outlined Alston's aim but, though he agreed that "experience...is the only method of determining the controversy", said nothing about whether he agreed with Alston's results and conclusions.[279] In the *Dissertation*, Alston concurred with Linnæus that most fertile plants had stamens and pistils and that they were therefore probably an essential part of vegetables but, he continued, the precise purpose of the stamen was a topic on which botanists had yet to agree. Though he cited several authors such as Andrea Cæsalpinus (c.1525-1603) and Grew who believed that the purpose of the stamen was to fertilise a plant's seeds, he also cited some who disagreed such as Tournefort, Pontedera and Camerarius.[280] Camerarius had conducted experiments on hemp, dog's mercury and spinach in which 'female' plants were isolated from 'males' and yet still produced fertile seeds. Therefore, asserted Alston, stamens were not necessary for plant reproduction. To further prove this, he conducted some experiments of his own: for example, he placed three fruit-bearing spinach plants 240 English feet away from any other spinach plants and separated them with several hedges, but still the spinach produced viable seeds. He repeated this kind of experiment with dog's mercury and hemp, increasing the separation by up to a mile, and found the same results – the plants still bore fertile seeds.[281] Alston also found that many other naturalists

[279] Anon. *Monthly Review* [1754] 387.
[280] Alston [1754] 40-2.
[281] Ibid. 43-4.

– Tournefort, Philip Miller (1691-1771) and Claude Joseph Geoffroy (1685-1752) – had had similar results. Linnæus had tried to nullify these results in the *Sponsalia Plantarum* by claiming that 'female' hemp plants occasionally carried 'male' flowers, but Alston disputed this and protested that even an authority such as Linnæus could not prevail over the results of good experiments.[282]

Alston then began to pick apart the kinds of experiments used by the supporters of the sexual theory of plants. The most common was to remove a flower's stamens. This frequently resulted in the flower's inability to grow fertile seeds and was interpreted by followers of the sexual system as evidence in its favour. But Alston had two arguments against this: the first was that it had only been tried in a small number of species and so could not be assumed to be a universal truth; the second was that injured plants, due to loss of sap and vitality, were often unable to produce seeds – and what was the removal of the stamens if not a serious injury to a flower? As a further refutation, Alston tried the experiment on some tulips; despite having isolated the flowers and carefully removed the stamens before pollination could occur, the tulips produced fertile seeds.[283] Alston next attacked the ancient authors that the so-called sexualists were so fond of quoting: accounts from both Theophrastus and Herodotus were questioned and mocked. There appeared to be no consensus in either ancient or modern times on whether crops such as dates, figs and hops needed both 'male' and 'female' plants in order to produce fruit and seeds.

Then Alston turned his attention to one of the most central pillars of the sexualists' argument: analogy. Alston was adamant that comparisons between plants and animals proved nothing. To illustrate this, he chose an example where analogy clearly broke down. Those who believed that plants had male and female parts looked to seed production for evidence of this; the production of plant seeds was said to be exactly analogous to animal reproduction. But this overlooked the fact that much plant propagation took place without the need for any seeds. Many members of the vegetable kingdom reproduced by sending out shoots, by budding, or by growing from cuttings.

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282 Ibid.44.
283 Ibid. 45.

Alston seized on this as a way to undermine analogies between the animal and vegetable kingdoms.[284]

Alston also attacked other fundamental parts of the sexualists' argument. He attacked their claims of observation. Some writers claimed to have seen pollen from a flower's stamens sticking to its pistils, but Alston denied that this kind of observation could be used to assume causality; he wrote that "*the sight* therefore seems to tell nothing often, nor confirm any thing concerning the generation of plants tyed down to it".[285] He attacked arguments from morphology on similar grounds: the fact that stamens and pistils were in close proximity and sometimes angled towards each other proved nothing for Alston. He attacked Linnæus's group of cryptogamia – plants without flowers or, as Linnæus put it, plants engaged in clandestine affairs. Where did these fit into the sexual system? But, most importantly, Alston attacked Linnæus's classification system. Alston outlined his problems with the system thus:

> It would not be worth while to argue against the sexes of plants, unless it had given occasion to the specious contrivance of a System, or Method of plants, named sexual, which of all others, how many soever there are, is the most intricate, and involved, and unnatural. Because there is no system, whether it be orthodox, or heterodox, in which more dissimilar things are conjoined, and more similar separated; and the knowledge of which, by reason of an introduced dialect unknown to the Greeks as well as to the Latins, also by reason of the loosely changed familiar ideas of words and names, is acquired with greater difficulty.[286]

Alston did believe that classification systems were vital in botany, but he preferred natural systems like Tournefort's which grouped species in a more logical order and required fewer new technical terms. Perhaps Alston's biggest problem with the sexual system was one of language. He accused Linnæus of changing generic names "without any necessity" and of devising rules for naming which were "quite arbitrary, for most part useless, and frequently deceitful".[287] Following these comments were nine pages of examples of plants which had been poorly or confusingly named under the sexual system.

Alston's conclusion was that Linnæus had wasted his time, caused confusion, and needlessly complicated botany with his new system. It was really the practical elements of

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[284] Ibid. 53-4. This work was taking place at about the same time as Ellis's work on zoophytes – see chapter two of this thesis for more details. For more on analogy as a scientific instrument, see Ritterbush [1964] chapters 3 and 4.
[285] Alston [1754] 62.
[286] Ibid. 69.
[287] Ibid. 71.

the system that Alston objected to – new terminology and counter-intuitive grouping – but he decided to get to the root of the problem by attacking the basis of Linnæus's system: the idea that plants had male and female parts. Although Alston's work never gained very much support and few shared his beliefs on the Linnean system, he was not altogether alone. In 1790 another Edinburgh scholar launched a similar attack on the sexual system. William Smellie's *The philosophy of natural history* was intended for a much broader audience than Alston's *Dissertation on botany*, but it addressed some of the same concerns. Since the appearance of his translation of Buffon's *Histoire Naturelle*, Smellie had gained a strong reputation as a populariser of natural history. *The philosophy of natural history* was eagerly anticipated, so much so that the bookseller Charles Elliot (1748-1790) paid 1,000 guineas for the copyright – an unprecedented sum. Once published, the work proved successful and went through several reprints and translations. The subject of the book – philosophical natural history – was a loosely defined one; for Smellie, the term invoked a broader, more general way of studying natural history. He wished to give an overview of all nature rather than giving minute details of species or systems. There were already plenty of books on the market that gave specific descriptions of methods of classifying plants and animals; Smellie's book was different. Its chapters covered topics such as the differences between the kingdoms, how creatures eat and grow, how they live and die. He had long had an interest in the question of the sexes of plants and he gave a chapter to this too.[288] Smellie had first come across the debate while a student at the University of Edinburgh. Each year Dr. John Hope asked four students to present a lecture on some botanical subject and encouraged them to question or oppose commonly held theories. To Smellie, he assigned the topic of the sexes of plants, and so he began his research. Later, he recalled how,

> Being at that time a very young man, and a strict believer in the sexual system of plants, I willingly undertook the task, because I thought I had the chance of showing some little ingenuity in attempting to shake a theory which I then imagined to be established upon the firmest basis of fact and experiment. But, after perusing Linnæus's works, and many other books on the subject, I was

[288] Smellie's work on the sexes of plants was to have serious repercussions for his later job prospects; when competing for the Chair of Natural History at Edinburgh University in the late 1770s, Smellie's forthright views, alongside other factors, may have counted against him. The post was eventually awarded to John Walker (1731-1803). For an account of the politics behind this academic contest, see Shapin [1974]; for an account of the work of Walker, see Withers [1991].

> astonished to find, that this theory was supported neither by facts nor arguments, which could produce conviction even in the most prejudiced minds.[289]

Smellie's real problem with the sexual theory of plants was its reliance on analogy. He saw the sexual theory "as a striking example of the danger of rashly yielding assent to the alluring seductions of analogical reasoning" – his language left no doubt about his contempt for analogy. Much of the evidence used to support the sexual theory of plants was drawn from analogies with the animal kingdom and so, believed Smellie, was unreliable.

He turned to experiment, "the only test of natural truths", to overturn the evidence from analogy. Even simple observation was enough to undo analogy: Smellie cited examples of animals such as "vine-fretters, polypi, millepedes, and infusion animalcules" which were observed to reproduce asexually. If so many animals could generate without the need for males and females, why should plants require them? Another observation showed that the seeds of plants were already quite well developed by the time pollen was released; again, the analogy to animals (whose eggs can only be fertilised very early in their development) broke down.[290] Smellie cited the experiments of Alston, Camerarius and Tournefort on spinach and hemp, and the experiments of Spallanzani on pumpkins, as further proof against both analogy and the sexual system. He praised these naturalists for allowing "fair experiment [to triumph] over deep prejudice".[291] Conversely, he tried to discredit experiments which gave results that appeared to support the sexual system. There was a famous case of a palm tree in the garden of the Royal Academy of Berlin which never produced fruit until, one year, a branch from a 'male' palm tree in Leipzig was brought to Berlin and placed next to the 'female'; that year, the tree produced hundreds of ripe dates.[292] Many saw this as evidence of the sexes of plants, but Smellie believed that factors such as the climate of Berlin, the time taken for the acclimatisation of the palm tree and its level of maturity had not been properly taken into consideration. He suggested some controls that would have made the experiment more rigorous and conclusive.[293]

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289 Smellie [1790] 245-6.
290 Ibid. 246-7.
291 Ibid. 262.
292 Ibid. 249.
293 Ibid. 250.

Likewise, Smellie questioned the experimental results of his own mentor – John Hope. Hope was a supporter of the sexual theory of plants and had tried to prove it with an experiment on the Scottish plant *lychnis dioica* (popularly known as campion). This plant had two varieties, one with a white flower and one with a red. Hope planted a white 'female' and a red 'male' together under a glass bell so that they were isolated from all other plants. The seeds of the white 'female' were sown the following season and produced red flowers. Hope interpreted this as evidence of hybridisation and, from that, inferred the necessity of both male and female elements in plant reproduction, but Smellie disagreed. He produced five arguments against Hope's conclusion. First, he

Specimen of *lychnis dioica* in the Linnean Herbarium. Image courtesy of the Linnean Society of London.

questioned the assumption that white *lychnis* never produce red flowers spontaneously; second, he pointed out that in order to have a proper analogy with hybrid animals such as mules, the offspring of the *lychnis* should have been a mixture of red and white; third, he showed with an experiment of his own that red *lychnis* lost much of their colour if grown without sufficient light or air (such as when grown under a glass jar); fourth, he highlighted the need for several control samples before any conclusion could be reached; and fifth, he emphasised the existence of many naturally occurring varieties and the influence of environmental factors on seed production. Smellie looked to "chemical and philosophical principles" to explain the results of Hope's experiments rather than to "an hypothetical commerce of sexes".[294]

As well as picking apart others' experiments, Smellie also performed some of his own. He took a seed-bearing *lychnis* and it isolated indoors, away from all other plants. But, perhaps due to insufficient light, air or moisture, the flowers died before any seeds could ripen. Smellie re-thought the experiment and asked for assistance from his friend Daniel Rutherford (1749-1819) who had succeeded John Hope as Professor of Botany at Edinburgh. Rutherford had a small garden "in the heart of the city, which was surrounded with houses of five and six stories high, and distant from any male *lychnis* about an English mile". The seed-bearing *lychnis* was planted here and it was found that

> she not only ripened her seeds, but these seeds vegetated, without the possibility of any male impregnation; for the Doctor, after the young plants were in a state of discrimination, uniformly extirpated all the males, and never could discover the vestige of a single male upon the female plants. Her female progeny, however, continued to bear fertile seeds for several successive generations.[295]

This experiment, according to Smellie, not only raised doubts about the philosophical concepts behind the sexual theory but also allowed him to consider the mechanisms that allegedly drove plant sexual reproduction. Many flowers contained both 'male' and 'female' parts; for these, it was relatively easy to explain how pollen might travel from the stamen to the pistil. Gravity, proximity, a slight breeze or a single clumsy insect could lead to fertilisation. But for plants such as the palm tree in Berlin or the *lychnis* in Rutherford's garden, vast distances separated pistil from stamen. Here, an external mechanism such as the wind or insects was needed by the sexualists to explain how fertilisation could take place. But for Smellie, such an explanation left far too much to chance: the wind was too "desultory and capricious", while there was nothing "more casual and uncertain than the wayward paths of insects".[296] According to Smellie's worldview, nature did not take such chances:

> ...the multiplication of species is one of the most important laws of Nature. All the laws of Nature are fixed, steady, and uniform, in their operation: None of their effects are abandoned to those uncertainties which necessarily result from chance, or from any fortuitous train of circumstances. ... The very supposition, therefore, that Nature has exposed the fertility of a tenth part of the whole vegetable

[294] Ibid. 254-7.
[295] Ibid. 258.
[296] Ibid. 251.

> kingdom, and many of them too, plants of the utmost importance to man, and other animals, to such accidental causes, is repugnant to every sound idea of philosophy.[297]

Smellie believed that he had done enough to raise serious doubts about the sexual theory of plants. He knew that his chapter did not constitute a full refutation of the theory and that there were still more experiments to be done, but he hoped that he had rendered "the sexual commerce of plants suspicious", that he had encouraged free thinking and that the "vegetable kingdom may again be open to impartial investigation".[298] Not everyone was swayed by Smellie's work. The Linneans were anxious to defend the sexual system against this kind of assault and shortly after the publication of *The philosophy of natural history*, a fellow of the Linnean Society published a pamphlet titled *The sexes of plants vindicated; in a letter to Mr. William Smellie, member of the Antiquarian and Royal Societies of Edinburgh; containing a refutation of his arguments against the sexes of plants*. The author was John Rotheram (c.1750-1804) who had studied medicine in Uppsala and had the distinction of being one of the only Englishmen ever to have studied directly under Linnæus. He esteemed Linnæus both personally and professionally and could not let Smellie's arguments against his mentor remain unchallenged. In the pamphlet, Rotheram reinterpreted the results of some of Smellie's experiments so that they were in line with Linnean orthodoxy – a project that was well received in London. *The monthly review*'s appraisal of the pamphlet is particularly interesting.[299] Though Smellie's work was published before Rotheram's, it had not yet been reviewed by that journal; a footnote promised that a review of Smellie's work "will be given soon" – but this never appeared. The reviewer considered Rotheram to be the best person to attack Smellie's stance, because he was "first, a Fellow of the Linnean Society of London, and, in the next place...a zealous disciple of his great master"; Rotheram hardly appears impartial, and the reviewer seems keen to stoke controversy and draw battle-lines between opposing camps. Though the reviewer is clearly more supportive of Rotheram's views than of Smellie's, he does occasionally try to give a balanced assessment and encourages his audience to read both works. Another London review – *The new annual register* – also took Rotheram's side; dismissing Smellie's ideas as "juvenile" while

[297] Ibid. 251.
[298] Ibid. 257.
[299] Anon. *Monthly Review* [1790] 88.

Rotheram was praised for the "force and propriety" of his attack.[300] The Linnean Society too made its feelings known; but in a more subtle way. Their library catalogue of the time shows that Rotheram's work was on their shelves, but the writings of Smellie and Alston were nowhere to be found in Great Marlborough Street.

By the end of the eighteenth century, the sexual theory of plants was so well established in Britain that even a respected naturalist like Smellie could not convince others to question it. Distinctions were beginning to form between natural histories. Where once curiosity had driven the naturalist to try different methods and ideas, now a new orthodoxy discouraged this approach. The purpose of the Linnean Society was to catalogue nature and to build upon the work of Linnæus, it was incompatible with the work of scientists like Alston and Smellie because they had different methodologies, different priorities, and because their work challenged the basis on which the Linnean Society was founded. It was not enough just to allow these two natural histories to co-exist, the Linneans had to actively suppress any controversy – we see this in Smith's ridiculing of Adanson or Spallanzani, and in Rotheram's attack on Smellie.[301]

Conclusion

The ordering of the natural world through the application of natural or artificial classification systems is often seen as a hallmark of eighteenth-century natural history; frequently, classification appears to be an act of enlightenment. But not all readings are so positive; many see classifying objects, along with the related activities of describing and naming objects, as a formulaic pursuit that requires little original thought, really as nothing more than stamp collecting.[302] Britain in particular, where the Linnean sexual system was so popular, is sometimes seen as a centre of mindless taxonomy where naturalists simply arranged their cabinets into neat Linnean patterns without considering any of the wider philosophical ramifications of their work. But here I have shown that the Linnean classification system was not always accepted at face value by British

[300] Anon. *New Annual Register* [1791] 212.
[301] For another analysis of the writings of Alston, Smellie and Rotheram on the sexes of plants, see Ritterbush [1964] 118-120. Ritterbush attributes Alston and Smellie's opposition to Linnæus's plant sexual theory to "blind adherence to the orthodoxy of graded function"; I argue that doubts about the correct use of analogy and worries about the truth of the sexual system were also important factors in their work.
[302] Foucault [1979]; Johnson [2007].

naturalists; these naturalists thought hard about the basis of this system and its implications for the natural world. If the system was accepted, that meant that analogy was to be considered a viable analytic tool and that the animal and vegetable kingdoms were related in very particular ways. But for naturalists who were troubled by the use of analogy or not convinced that the chain of being could really be interpreted this way, Linnæus's system was problematic. British naturalists like Alston and Smellie dealt with this problem head-on by conducting experiments that sought to answer a fundamental question: do plants really have sexes? The works of Alston and Smellie exemplify the broad British natural history that I seek to explain. They show how British naturalists engaged with the underlying philosophies of natural history and how they used a range of experiments, critiques and hypotheses to question them.

As well as telling us something specific about styles of natural history, this chapter also says something more general about British and English natural history in this period. The desire to avoid confrontation was quite typical of English natural history, but this was far less true of Scottish natural history. And although the Englishman Smith had studied natural history at Edinburgh (because his Unitarian beliefs forbade him from entering either of the English universities), his wish to maintain peace among the fellows of the Linnean Society meant that he tried to play down the arguments of the anti-sexualists by ridiculing them rather than engaging scientifically with them. Though Smith and Smellie had both been taught the sexual theory of plants under the same teacher – John Hope of Edinburgh – the English-based Smith and the Francophile Smellie reacted very differently to it. The strong links between Edinburgh and France (where the theories of Linnæus struggled to gain a foothold) meant that Linnæus was not treated with the same reverence in Scotland as he was in England. It was no coincidence that two of the most vociferous critics of the sexual system of plants were both Scottish.[303]

Essentially, this chapter shows how two kinds of natural history dealt with the controversy surrounding the sexual system. Linnæus and Smith on the one side, and Alston and Smellie on the other, conducted similar experiments and observed similar phenomena but produced very different conceptions of how the vegetable kingdom

[303] For more on the Linnean Society's desire to avoid confrontations, see the first chapter of this thesis, for more on a comparable Scottish society, see Shapin [1991] on the foundation of the Royal Society of Edinburgh which had broader aims and did not seek to stifle scientific debate. The relationship between British and French natural history will be discussed further in chapter six of this thesis.

functioned, and how it related to the animal kingdom. Their works highlight what kinds of methods were used by the Linneans and by the Edinburgh-based scholars, but they also contextualise their successes and failures. Natural histories did not stand or fall on scientific merit alone, many external factors influenced their reception, dissemination and use. The Linnean Society's desire for stability affected what kinds of natural history could be successfully practised in Britain and this led to the marginalisation of some works that did not fit with their programme, as we have seen with Alston and Smellie.

CHAPTER FIVE

The careering naturalist

> "In truth, he is properly entitled, in any degree, to the character of the Botanist, whose acquirements enable him to investigate, to describe, and systematically arrange, any plant which comes under his cognizance. But to these abilities, in order to compleat the character, should be united, an acquaintance with the Philosophy of Vegetables, and with the History of the Science, in all its several relations, both literary and practical ... attainments which require a competent share of general learning, and no small degree of painful toil and patient industry, both in the field and in the closet."
>
> Richard Pulteney on the attributes of the naturalist, 1790[304]

So far, I have used specific pursuits of natural history – from collecting and naming to experimenting and theorising – as practised by various individuals to show the breadth of this science. This chapter takes a different approach; it follows one particular individual who partook in many diverse activities and uses his career to show how that breadth could exist in the life and works of a given naturalist. Before the professionalisation of science in Britain, before the concept of a clearly structured career in science or natural history, how did naturalists go about organising their domain, publicising their work, establishing authority and making a livelihood from this often nebulous field of knowledge? In an attempt to answer some of these questions I look at the life of Edward Donovan (1768-1837) and his work as a writer, artist, engraver, collector, curator and populariser of natural history. Donovan's 'career' in natural history, like those of so many of his contemporaries, did not follow a clear or preordained path and is extremely useful in highlighting the many ways the life of the naturalist could be lived. It is also a valuable tool in helping us to think about definitions of natural history; here we see a broad field without clearly defined boundaries and we observe how practitioners negotiated their ways through it.

The question of how one became a naturalist arises not just because of a gap in the secondary literature; the problem was rarely addressed in contemporary works. This was not because terms such as 'natural history' and 'naturalist' were unproblematic. The

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304 Pulteney [1790] preface.

science had ancient roots and its adherents had long-standing practices; much of the work of the late-eighteenth century naturalist was descended from a continuous Aristotelian tradition. Naturalists were keen to stress this heritage; general introductions to or descriptions of the subject almost always mentioned ancient writers – the wisdom of early zoologists, botanists and mineralogists was a source of comfort to their more modern equivalents. But despite its respectable history, natural history seemed to feel the need to justify itself, and the references to ancient writers often appear to be defensive rather than exultant. In the late eighteenth and early nineteenth centuries, British natural history was undergoing changes that would affect its practitioners, its audiences, it methods and its content for some time to come. As I discussed in the first chapter, the rise of the Linnean Society was profoundly affecting the content and scope of natural history, but the idea of 'the naturalist' was also changing subtly. Social, political, religious and economic concerns had always played a role in shaping natural history, but in this period their effects came to be more keenly felt. Britain's growing economy, the expanding British Empire and increased levels of travel, exploration and trade combined to produce new wealth as well as new territories for naturalists to investigate. Increasing public interest in natural history, as well as the influx of specimens from new worlds, created a market for information and allowed more people to enter the field.

This expansion of natural history distorted its traditional structure; since the sixteenth century, the field had become more public, more communal, more commercial and more lucrative.[305] The possibility of making a living from natural history began to seem plausible. These changes affected not so much the conception of the science as that of those who participated in it and for that reason I set this chapter, which focuses so much on the possibilities of the practitioner, in this time period at the close of the eighteenth century and in the first few decades of the nineteenth. Much of the essence of natural history remained the same but the ways in which it could be legitimately practiced began to be re-imagined. By the end of the eighteenth century, the term 'naturalist' could reasonably be used to describe writers, illustrators, engravers, editors, collectors, taxidermists, curators, professors, lecturers, society-members, travellers, explorers and so on. Furthermore, many of the men engaged in those activities were also involved in

[305] See Cook [2007] for an example of how Dutch natural history operated in relation to economic interests, and see Ogilvie [2006] for an account of the development of natural history contextualised with respect to other intellectual and scientific movements of the sixteenth and seventeenth centuries.

associated trades such as publishing or bookselling. The boundary between the more traditional elements of natural history and the commercial elements had never been very well defined, but now it became even less clear. The large range of activities that came under the banner of 'natural history' makes both it, and its practitioners, difficult to define.

In a single chapter, it will not be possible to address all the different kinds of naturalists and all of the careers that they might have followed. Instead, I focus primarily on Edward Donovan and contrast certain elements of his career with those of two contemporaries: George Shaw (1751-1813) and Alexander Macleay (1767-1848). These three men fall roughly into the category of 'gentleman naturalist': one had independent wealth, one had a university degree and one was a high-ranking civil servant who later held a government position; all were members of the Linnean Society. But even though they had much in common and existed at much the same social level, there were a variety of options open to them, as illustrated by the three different paths they took. There are several reasons for studying such characters: even though gentlemen naturalists have been more studied than artisan naturalists, there are still many gaps in the scholarship – famous figures such as Joseph Banks, Gilbert White and Erasmus Darwin have been the focus of much attention but lesser known naturalists such as Donovan have been almost entirely overlooked. Gentlemen naturalists also tend to have left more evidence of their work, either in the form of printed works, cabinets or correspondence archives, but in many cases these have never been examined by historians. Through an examination of Donovan's works and letters it is possible to get a good idea of how someone in his position went about constructing a career in natural history; from this we can begin to understand what such a career might look like, how and why one would pursue it, and what its implications were for this field of knowledge.

Defining the naturalist

Primary sources from the period rarely dwelt on problems of the definition of natural history or the identity of the naturalist. Within reason, almost any work that an author chose to call natural history was accepted as natural history, and any person calling himself a naturalist was accepted as such. This was apparent not just in published works, where a certain formulaic politeness was adhered to, but also in private interchanges where opinions could be expressed more honestly. The near-complete absence of debate on the topic is striking. In a period when so many new people were entering the field and so many new roles were being filled, discussion about who these people were, what they were doing and how and why they were doing it is surely to be expected? But although explicit definitions are absent, subtle references to particular individuals may occasionally give us clues as to what were considered the essential characteristics of a good naturalist. The use of words such as 'approved', 'ingenious', 'admirable' were most often applied to writers while 'skilled' was generally reserved for painters or engravers. Particular credit was given to those who were seen as 'original' or 'genuine'. With naturalists who wrote or published extensively, it is possible to ascertain which fellow naturalists they considered good or interesting by paying attention to such references, as well as their citations and synonyms. But this is more difficult in the case of those who did not publish.

Anyone could be a naturalist, but being considered a good naturalist was another matter: becoming a respected naturalist was dependent on social as well as scientific criteria. Even in an enlarging field, many naturalists of this period knew each other personally or through correspondence, as well as through reputation, publications or collections. Social hierarchies among naturalist closely mirrored those across British society and clear distinctions were made between artisan naturalists and gentlemen naturalists; strict codes governed interactions between the different levels. Although artisan naturalists were respected, their work was generally considered separately from that of gentlemen naturalists, and because they were less likely to publish widely or present their work, they tended to be somewhat invisible and ill-defined. This gap in the written record has led to a large number of practitioners being overlooked by historians. The scholarship of Anne Secord has addressed this problem for the nineteenth century and begun to focus attention on this often-ignored group of naturalists.[306] But even with this work, the problem of a comprehensive review of natural history remains.

Secondary literature is scarce; the late eighteenth century is frequently neglected by historians of natural history and few who study the period clarify exactly what they mean

[306] For more on artisan naturalists, see Secord [1994a], for more on the relationship between artisan and gentleman naturalists, see Secord [1994b].

when they use the terms 'naturalist' or 'natural history'. Where secondary literature exists, it is most likely to focus on botany. The economic importance of botany and its uses in medicine and pharmacy meant that it tended to dominate the field. Out of necessity, there were reasonably well developed career paths for aspiring botanists and quite a number of paid positions in the field. This was not true of most other specialised branches of natural history and definitely not the case for general natural history. Natural history as a profession in the late eighteenth and early nineteenth centuries was a fluid entity; most naturalists were free to carve out and define roles for themselves and they did this with varying degrees of success. David Elliston Allen has written extensively about the rise of the professional natural historian in Britain and about the differences, or perceived differences, between amateurs and professionals. He is one of the few historians to have discussed what it meant to be a naturalist at that time, and what it meant to be a professional. His article "The early professionals in British natural history" addressed the problem of discussing 'professionalisation' and the possible meanings of that term. In the eighteenth century, the word 'profession' related particularly to law, medicine, the church or the military. These were well structured, universally recognized careers that guaranteed a certain amount of security and status. There were also recognised trades that included such things as printing, bookbinding, engraving and other practical craft activities. The trades too had clear structures and prospects, but occupied a different social niche to the professions. Natural history did not fit easily under either heading. The recognised trades and professions all tended to have specific training or apprenticeships, clear aims, progressive career paths and reasonably reliable incomes; natural history had none of these.

There were several ways into natural history: anyone with an interest in the subject and the means to attend university was likely to take a medical degree and use their training in anatomy, dissection and the use of *materia medica* as a basis for future work in natural history. Dedicated professors of natural history or any of its constituent parts were rare and, even where they did exist, frequently had little interest in or knowledge of their subject.[307] Even with a degree, there were few prospects of a clear role or a paid position in natural history; university chairs rarely changed hands and while teaching or demonstrating were possibilities, they were low-status and unreliable. Those who did not

[307] Allen [1978] 16-17.

attend university had several options: if one had independent wealth, it was common to create a cabinet. These collections of objects varied widely according to their creators' tastes, and their uses varied too; some were assembled purely for aesthetic reasons, some as displays of wealth and some for scientific reasons. The more scientific collections might be used for educational display, for research or for scientific drawings and publications either by the owner himself or by others who he allowed to access the objects. Those without such wealth might have trained in a trade such as engraving and then focussed on producing botanical or zoological plates, thereby gaining a good working knowledge of the subject and contacts with others in the field. Others chose jobs such as collecting and dealing specimens; although this entailed no particular training, those involved often became very skilled at identifying species and often developed detailed knowledge of classification and taxonomy. There were also a small number of paid positions in natural history – these might involve cataloguing a private cabinet or curating a museum, but they were not a particularly reliable source of income. Often a practitioner would combine parts of each of these options and so carve out an individual role for himself; thus the natural historian was something of a hybrid form and this makes him difficult to define. Natural history was inherently idiosyncratic. All professions or trades have a measure of idiosyncrasy and individuals have unique careers, but in natural history it seems to have been an intrinsic feature of the field. By looking at enough individuals we may begin to see a pattern emerge; in the case of natural history the pattern is noticeably absent, or perhaps the absence *is* the pattern.

David Allen has argued that naturalists deliberately chose to maintain this non-professional status and has put forward several reasons why this might be so: respect for amateur scholarship; the relative ease and inexpensiveness of conducting natural history; and the stigma attached to employment coupled with the importance of being seen as independent.[308] In keeping their science amateur and unstructured, naturalists would have had more freedom to pursue their own interests in whatever way they pleased. This model worked reasonably well throughout most of the eighteenth century; but in the closing decades, when natural history became more popular and commercialised, its lack of structure began to cause problems. Although Allen's work is useful, the dichotomy between 'amateur' and 'professional' is unhelpful as the term 'professional' did not really

[308] Allen [1985].

apply to British science before the late nineteenth century. As the case studies below demonstrate, there were many pitfalls to be encountered when making one's way in such uncharted territory. Transforming what had been for so long a genteel activity into an activity on which one's livelihood depended, without stooping to crass commercialism, was a difficult process. There were many who attempted to negotiate a career in natural history but few who were unequivocally successful. Here, in an attempt to explore these issues, I examine the life and work of Edward Donovan - a self-employed, self-published naturalist working in London between c. 1790 and 1830. To give context to Donovan's life's work, I compare his publishing career to that of George Shaw – keeper of natural history at the British Museum from 1791, and also well known as a lecturer and author in natural history. And I compare his collecting career to that of Alexander Macleay who worked full time as a civil servant but, due to his position as secretary of the Linnean Society and his impressive natural history collections, was also a central figure in London natural history in the late eighteenth and early nineteenth centuries. The careers of these three men, through their differences, illustrate some of the possibilities open to aspiring naturalists in this period. They can also be used to tell us something about what it meant to be a naturalist and what it meant to pursue knowledge as a career.

Donovan's career in natural history was played out on several different stages. Books were his first avenue into natural history and during his lifetime he produced dozens of texts on a diverse range of subjects. But Donovan was not only a writer; he was also responsible for illustrating his books – a lengthy process involving drawing, etching or engraving, and colouring – and for publishing them. Collecting was another key element of Donovan's career, and one that would lead him to establish a public museum of natural history. Donovan also had aspirations to lecture publicly on natural history and to teach it at university, but he was unsuccessful in these endeavours. The broad scope of Donovan's career allows us to understand some of the different aspects of a life in natural history.

Nothing is known about Donovan's early life, family background or education, but certainly by the age of 21 he was in London. Judging by his ability to collect and self-publish, he appears to have had independent wealth. In London, he made his first recorded foray into natural history with the 1789 publication of *The botanical review, or the beauties of flora*. In some ways, this first work was typical of his later writing career. The title page of the *Review* declared that it was "Intended to consist of Accurate Coloured Figures, of the scarcest and most beautiful Foreign Plants, A delineation of their several Characters, and Parts of Fructification, either on a Scale – of the natural Size – or as they appear deeply magnified by a Microscope".[309] Indeed, those 'accurate coloured figures' were the main selling-point of this short-lived periodical which acted largely as a showcase for Donovan's high-quality illustrations. The mixture of aesthetic sensibility and scientific gravitas which Donovan tried to convey were characteristic of his work: a magazine subtitled 'The beauties of flora' was clearly intended to appeal to a certain kind of botanist, while his mention of specific characters and his use of a microscope were intended to show that the work was not a frivolous one. His descriptions of 'foreign' plants reflected the late eighteenth-century craze for overseas exploration. His interest in practical rather than theoretical botany is also apparent in the title page, which promised "A Description of and the Mode of cultivating each". The intended audience was "The *Amateur* in Botany of every Denomination" but a price of six pence per issue meant that the *Review* was not necessarily affordable to all. Donovan not only wrote the text and took responsibility for the laborious illustration process, he also published the work himself; the words "printed for the Author" which appeared on the title page would come to characterise almost all of Donovan's written work.

All of these features are indicative of Donovan's approach to a career in natural history. He merged his talent for illustration with his interest in science and natural history to produce something suitable for polite public consumption. Years later, recalling his earliest work, Donovan wrote:

> I began in 1789 and from that period to the present I have endeavoured to render my labours useful by their originality, to the Great Object which is now so proudly lauded throughout the country: the dissemination of useful knowledge, in the various Sciences of nature, philosophy & research to which my attention has been directed.[310]

[309] Donovan [1789] title-page

[310] E. Donovan to the Committee of The Royal Literary Fund, November 1831. Item no. 6 in Donovan's file in the Archives of the Royal Literary Fund.

The ways in which Donovan attempted to create and spread this 'useful knowledge' and to establish himself as an appropriate person to undertake the task will be the principal focus of this chapter.[311]

Writing, Illustrating & Publishing

The popularity of natural history had increased considerably throughout the eighteenth century and, corresponding to this increase, there was a noticeable growth in the publication of natural historical books, journals and articles. The new public appetite for natural history knowledge created a market for exactly the kinds of books that men like Donovan produced. David Allen has written about the link between newly-fashionable natural history and the expanding market for such lavishly illustrated books.[312] These two factors are certainly important in contextualising Donovan's career as an author. Another important consideration when thinking about writing careers in eighteenth-century natural history is audience. In a world with little clear distinction between experts and laymen, it was necessary for writers to carefully pitch their work for particular audiences. A single book could have more than one type of audience; we see this in many of Donovan's works - dry Latin descriptions of specific characters to appeal to the serious naturalist, and striking plates to capture the imagination of the amateur enthusiast. Since there was little precedent for natural historical texts that catered for multiple audiences before this period, authors had to tread carefully to make their books both sufficiently accurate and engaging. It was in such a context that Donovan began his career and these concerns are reflected in many of his written works.

Donovan's earliest works from the 1790s were indicative of the rise in public and professional interest in the natural sciences. Through his works we can see the difficulties of addressing different audiences while both maintaining scientific credibility and making monetary profit. In this period, for example, Donovan simultaneously published a beginners' instruction manual and began writing a seminal 16-volume work on British insects. The first of these books, his 1794 work entitled *Instructions for collecting and preserving*

[311] Although Donovan never defined what he meant by 'useful knowledge', it is apparent that it is quite different from the 'useful knowledge' that was propagated by organisations such as the Society for the Diffusion of Useful Knowledge in later decades. His idea of what it was will become clear as the discussion of his work continues.
[312] Allen [1993] 335-6.

various subjects of natural history; as animals, birds, reptiles, shells, corals, plants, &c. Together with A treatise on the management of insects in their several states was a slim volume containing detailed information on the practices and methods of the natural historian. It exhorted the beginner not just to examine dead specimens, but also to observe live ones. The book went into great detail about the practical requirements for collecting. The naturalist was advised never to venture into the field without, at the very minimum, "a large Bat-fowling-net, a pair of forceps, a number of corked boxes of various sizes, ditto small pill boxes, a spare box with cramps, and a pincushion well stored with pins of different sizes"[313]. He was told what kinds of places to collect, and the best times and seasons for collecting. On returning to his study or workbench with his newly-captured specimens, he was instructed to procure "a quantity of allum, arsenic, camphire, sulphur, and warm spices; tobacco, tanners bark, bitter aloes, and spirit of wine; some cotton, wool, fine tow, and oakum" and then given exact details about how to handle each specimen.[314] The instructions were clearly aimed at one with little or no prior experience of practical natural history. The book was generally well-received. Indeed, one reviewer wrote: "We have not met with instructions more judicious and satisfactory. The young natural historian will feel, in his progress, great obligations to the hints of Mr. Donovan".[315]

Instructions was manifestly different from Donovan's *The natural history of British insects*, the first volume of which was published about the same time. Where *Instructions* contained specific information on practice and little on the subjects of natural history, *British insects* was premised on the existence of a readership already familiar with (or perhaps simply uninterested in) the basics of work in natural history. There were many such books in this period and Donovan's principal entomology book was characteristic of the genre. Here, I use *British insects* to comment on this genre and its audience and to see how these books fitted into the general scheme of late eighteenth-century natural history. More importantly, I examine what role such a book could play in advancing Donovan's career and in establishing him as a reliable source for knowledge in natural history.

The first volume of *British insects* appeared in 1792 and a new volume was produced annually until 1801, another six volumes were produced later and the series

[313] Donovan [1794a] 22.
[314] Ibid. 2.
[315] Anon. [1794] 115.

ended in 1813. It was Donovan's first large-scale attempt at scholarly natural history. As in most of Donovan's books, the illustrations acted as the main focal point: each volume contained about 35 plates, all coloured. The text was of a less consistent quality than the images. Although the work lacked an introduction, its lengthy subtitle gave some clues about Donovan's objectives. We learn, for example, that he intended to explain insects "in their several states, with the periods of their transformations".[316] Many naturalists were particularly drawn to entomology because of the strange life-cycles of its subjects – a creature that developed from egg to larva to chrysalis to adult was of immense philosophical interest to zoologists and theoreticians. Donovan not only used this to stimulate intellectual interest in his work, he also exploited this four-stage process in its visual presentation. The first plate in most volumes of *British insects* depicted all the states a particular insect (usually a butterfly or moth). This showed something of scientific interest while also allowing an artist to create more visually complex groupings and to use a wider range of colours. The plates too were mentioned in the subtitle; it declared that the work would be "illustrated by coloured figures, designed and executed from living specimens". This insistence on using live specimens was intended to bolster the scientific credibility of the work. Many entomology works at that time relied on using preserved specimens as a basis for their illustrations or on copying illustrations from other publications – both produced unsatisfactory results.[317] The use of live specimens represented a genuine advance in entomology since it resulted in more accurate illustrations and so allowed for easier identification, easier classification and less confusion about synonyms. Also, as many of the illustrated species had not previously been presented in these kinds of text books (or, in certain cases, anywhere else), *British insects* created new knowledge and made it available to a new audience.

It has been said that *British insects* "forms a transition between the primarily artistic (albeit well-observed and accurate) productions of the eighteenth century and the more austere scientific works which were to follow".[318] This analysis appears to deny that a work could be both artistic and scientific simultaneously – but that was exactly what Donovan's work set out to achieve. The advertisement for the tenth volume said that

[316] Donovan [1792] introduction.
[317] Salmon [2000] 129. How far Donovan actually used live insects is difficult to say, many of the rarer species appear to have been drawn from preserved specimens.
[318] Ibid. 130.

"[Donovan's] chief object was to illustrate the science of Entomology on a more extensive scale than had previously been attempted in this Country" but also that "the Author has purposely avoided entering too deeply amongst the minutiæ of the Insect race, [but] he has been careful to include whatever is interesting".[319] Certainly the book appears to have set out to address both the scientific and artistic elements of natural history, as well as being an entertaining read.

The book opened with a very brief outline of the Linnæan classification system which consisted of seven orders of insect: coleoptera; hemiptera; lepidoptera; neuroptera; hymenoptera; diptera; and aptera. Although Donovan used Linnæan names throughout, the insects were not actually grouped together by order.[320] In the earliest volumes, the text was entirely in English. The use of the vernacular had become increasingly common in British natural history in the late eighteenth century – this made books more accessible to the public, but less relevant to foreign naturalists and to serious scholars.[321] Perhaps with this in mind, from the third volume onwards, specific characters were given in Latin as well as in English. Also, as the series went on, the synonym lists for each species became more extensive. These changes could indicate, perhaps, a desire on Donovan's part to be seen as a more scholarly writer or they might have been a more straightforward response to the demands of his audience. The text consisted of specific descriptions accompanied by passages on the life and habitat of that species, on how the specimen had been procured, extracts from other naturalists, or, occasionally, extracts of poetry. Donovan, especially early on, was capable of great prolixity on his subjects. His description of the death of a buff-tip moth (*Phalæna bucephala*) read:

> Its beauty avails not the race of birds who pursue them from necessity, or from an innate desire of cruelty and devastation; and whilst happy in its apparent security, ranging the plain to experience the pleasures of liberty, to banquet in the nectareous profusion of the vegetable kingdom, or catch the dew drop from the humid air, to inspirit and refresh his parched system from the mid-day heat, he becomes a dupe to his happiness, his pleasures at once fully, and he falls an unresisting victim into the devouring jaws of death.[322]

This incredibly evocative style must have been intended to appeal to a particular type of audience. At that time the boundary between natural history and literature was often

[319] Donovan [1801].
[320] But Donovan did provide a Linnean index as well as an alphabetical one.
[321] Allen [1993] 337.
[322] Donovan [1792] 11-12.

negligible, as can be seen in works such as Erasmus Darwin's *Botanic garden*. For many, there was an obvious connection between the beauty of nature, the awe that it could inspire and the emotions that good literature could arouse, and Donovan seems to have been following in this tradition when writing his early volumes. Such florid descriptions became less and less common over time and they almost completely disappear in the final volumes. It is hardly surprising that *British insects* changed in style as it progressed. 24-year-old Donovan was only beginning his career when he started work on it. Over the 21 years that it took to produce the complete work, Donovan consolidated his reputation as a naturalist, became more focussed, and refined his writing style. Early volumes, with their long, chatty notes, seem to be aimed primarily at beginners and amateurs while later volumes, which are drier and more focussed on conveying facts, seem more appropriate for the more seasoned naturalist. By 1810, a reviewer could write that "Mr. Donovan's critical annotations...are characterized by good sense and judgement".[323] It is interesting to observe that while the quality of writing changed, the standard of the images remained constant throughout. This meant that even as the text came to contain more detailed specialist knowledge, the books could retain a wide audience of non-specialists. It was not the case in Donovan's work that the plates were simply intended to illustrate the text, rather, the images and text were distinct, though related, entities. While this was not necessarily typical of natural history as a whole, it was representative of an increasingly common trend in natural history publishing.

British insects was instrumental in establishing Donovan as a reputable figure in British natural history. More specifically, it identified him as an expert in entomology. Entomology had been less popular in Britain than it was in continental Europe but its visibility was beginning to increase around this time, partly due to books such as *British insects*. Despite its growing popularity, it was never a serious rival to botany and was seen by many as less glamorous than other branches of zoology. Entomologists often had to go to great lengths to justify their science and few could make a living from entomology alone; entomologists usually also engaged in general natural history or another specialism. Although Donovan was a general natural historian, entomology was his preferred field, and it was generally acknowledged that he was best at entomological illustrations.[324]

[323] Anon. [1810] 240. This is a quotation from a review of Donovan's *Natural history of British fishes* – a work quite similar in style to *British insects*.
[324] Swainson [1840] 169, Jackson [1985] 189.

British insects sold well; some volumes even had to be to be reprinted to meet demand[325]. In the dedication of the tenth volume (originally intended as the final volume), Donovan wrote that the book had been "sanctioned with no inconsiderable share of public approbation".[326] The book was also received well by specialists and was referenced quite frequently by other naturalists; the works (both public and private) of many eminent entomologists are scattered with citations from Donovan.[327]

The success of these first ventures into natural history publishing gave Donovan enough confidence to pursue a career as a writer. As he appears to have had independent wealth, his decision to write and illustrate was possibly not driven by financial need. He could have chosen to simply collect for his own gratification but he always insisted on making his collections public – first through his publications and later through his museum. His books seem to have been motivated purely by the desire to share knowledge. The response to his first books encouraged him to produce more; just as the third volume of *British insects* was appearing in 1794, he began work on *Natural history of British birds*. This ten-volume work was published in 50 monthly parts and so proceeded at a slightly slower pace than *British insects*; it was not completed until 1819. Donovan was a competent ornithologist but, even so, he relied heavily on the work of others for his specific descriptions. He stated that *British birds* would contain "descriptions from the *Systema naturæ* of Linnæus; with general observations, either original, or collected from the latest and most esteemed English ornithologists".[328] This was not an uncommon practice at that time: many natural history books contained either original text with illustrations borrowed from other sources, or vice versa.[329] A reviewer, commenting on Donovan's reliance on other naturalists, wrote: "Mr. Donovan has frequently availed himself of the labours of Buffon, Pennant, Latham, and some other writers on natural history; and if he had done it more fully, we cannot think that his performance would have been less valuable, or afforded less entertainment".[330] But despite mediocre reviews, the book was popular enough to merit several volumes being re-printed. Its popularity was probably predicated on the originality of the plates. Thanks to his increasingly large bird collection,

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325 Salmon [2000] 129.
326 This volume was dedicated to Thomas Marsham and Alexander Macleay, respectively treasurer and secretary of the Linnean Society when it was published in 1801.
327 See, for example, Shaw [1800-1813] and the letters between Alexander Macleay and William Kirby in the Macleay Correspondence Archive, Linnean Society.
328 Donovan [1794b] title page.
329 As an example of an original text with borrowed illustrations, see George Shaw's *General zoology* [1800-13].
330 Anon. [1797] 447.

Donovan was able to draw many of the figures from real specimens.[331] And, as with his entomology plates, these drawings were beautifully coloured.

The model that Donovan had established in *British insects* was one that he used again and again throughout his publishing career.[332] We see it here with his *British birds*, and later in works such as *Natural history of British shells* (1800-1804), *Natural history of British fishes* (1802-1808), *Natural history of British quadrupeds* (1820) and *Natural history of the nests and eggs of British birds* (1826). Clearly the formula was a successful one and the books sold reasonably well. The formula was not simply a combination of good plates, Latin descriptions and verbose notes; subject matter was also important. The unusual thing about the subjects of this series is that they are drawn entirely from British natural history. At a time when British overseas expeditions were increasing, and when large numbers of new, exotic, brightly-coloured specimens were being sent back from far-flung regions in Africa, the Americas and Australasia, it was unusual for British naturalists to concentrate so intently on their own flora and fauna.[333] This interest in British natural history was driven partly by scientific motives (after all, much of Britain's flora and fauna had not been described and illustrated in this way before) but also by nationalistic motives. The audience could see Donovan's writings through the lenses of both science and through that of patriotism. One reader, writing about *British fishes*, praised

> every laudable exertion of which the object is to bring us acquainted with the productions of British soil and of British seas. ...He is the truest patriot, who explores and displays the intrinsic resources of the nation to which he belongs. At a crisis in which our inveterate enemy labours to impede our intercourse with foreign states, it is pleasing to remark that the attention of the discerning and enlightened portion of the community begins to be directed to our domestic capacities of prosperity.[334]

At a time when political relations between Britain and her neighbours were less than cordial, it was easy for nationalistic rivalry to spill over into science. In his works, Donovan made several references to the national importance of natural history and also dreamed of setting up a national academy for natural history, but beyond these oblique

[331] Jackson [1985] 182.
[332] An earlier example of an individual who carved a career out of natural history illustrating and publishing, and of whom Donovan was aware, was Maria Sibylla Merian (1647-1717). For more on her work see Wettengl [1998].
[333] Jackson [1985] 182. This focus on foreign specimens was also cited as a concern in James Edward Smith's *Introductory discourse* at the first official meeting of the Linnean Society, Smith [1791a]. For more on some of the reasons behind British imperial expansion in this era see Drayton [2000] 34, 43, 109-110.
[334] Anon. [1810] 245.

references, he seems never to have commented on politics.[335] This series of books earned him a reputation as a particularly British naturalist, but actually Donovan was probably little interested in international rivalry; not only did he rely heavily on the works of foreign naturalists, but he was also proud when some of his own works were translated into European languages.[336]

Indeed, although he was widely known for his books on British natural history, Donovan also published several books on foreign entomology. As part of a series entitled *General illustration of entomology* Donovan published three volumes: *An epitome of the natural history of the insects of China* (1798); *An epitome of the natural history of the insects of India* (1800); and *An epitome of the natural history of the insects of New Holland, New Zealand, New Guinea, Otaheite, and other islands in the Indian, Southern, and Pacific oceans* (1805). A prospectus for this series outlined several of Donovan's aims in undertaking it: to acquaint English readers with the latest developments in entomology; to increase usage of the Linnæan system; and to raise the profile of entomology.[337] In style, the three volumes were similar to *British insects* – brightly-coloured, detailed illustrations are accompanied by short descriptions. The images dominated - the advert for the volume on Indian insects correctly described it as "a work [of] extensive design and splendid embellishments".[338] The books were popular; writing about his decision to undertake a second volume, Donovan said, "the favourable reception which the Epitome of the Insects of China met with, was an irresistible inducement with the Author to undertake a similar illustration of the Insects of India".[339]

New editions of *Insects of China* and *Insects of India* were published after Donovan's death. These 1842 editions, edited by J.O. Westwood, secretary of the recently-formed Entomological Society of London, contained Donovan's original plates but almost none of his text. The "beautiful figures of Donovan" were still valuable but his writings, written in a now-outdated Linnean framework, were of little interest.[340] The enduring popularity of Donovan's drawings is a testament to his skills as an artist and craftsman.

[335] Donovan [1800b] advertisement; Donovan [1808] iii.
[336] Donovan [1809a] – the bibliography given as part of the section on the history of entomology is composed almost entirely of foreign works; Royal Literary Fund Archives, item number 9 in Donovan's file.
[337] Donovan [1800a] 1-2.
[338] Ibid. 3.
[339] Donovan [1800b] advertisement.
[340] Donovan [1842a] preface i; Donovan [1842b].

Contemporary naturalists held Donovan's works in high regard. We find evidence of this in the correspondence of members of the Linnean Society. Donovan became a fellow of the society in 1799. By that year he had published about 15 volumes on natural history. The details of his nomination for membership indicate that other naturalists highly esteemed his work: most new fellows of the Linnean Society had their membership supported by only three existing fellows, but Donovan was nominated by six - Alexander Macleay, James Sowerby, William Lewis, Thomas Marsham, George Milne and Frederick Kanmacher. Donovan's status within the community is further verified by occasional comments by these and other naturalists about his work. He seems to have been particularly known for the accuracy of his work for when he made a rare mistake in one volume of *British Insects*, Marsham wrote to Alexander Macleay in surprise: "Donovan's figure of Coc. 14 punct. must be struck out, I must have taken the Synonym from his Index, supposing that he could not so have blundered".[341] His illustrations were also highly regarded. When learning how to paint, William Kirby sent some of his illustrations to Alexander Macleay accompanied by a note that read: "...the figures now sent are my first attempt at painting insects, so they must not be looked at with the same Eyes you would Sowerby's or Donovan's...".[342]

Little direct evidence exists about Donovan's illustration processes, but quite a bit can be inferred from knowledge about contemporary natural history illustrators, from occasional references in his writing or letters, and from the illustrations themselves. Donovan was unusual in undertaking all parts of the illustration process himself. The process began with making a drawing of the specimen – in Donovan's case, he preferred to draw from live specimens; this was often possible in entomology, but more difficult in some branches of natural history, and almost impossible in areas such as ichthyology. The drawing then had to be transferred to a plate; this could be done by processes such as engraving, etching, stipple or aquatint. In engraving, the drawing was incised onto a sheet of copper using a sharp steel point. When the plate was inked and printed, it produced a sharp, crisp image. In etching, a metal plate was covered with an acid-proof

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[341] Thomas Marsham to Alexander Macleay, 23rd December 1801, letter # 81, Macleay Correspondence Archive, Linnean Society Archives. Marsham's underlining.

[342] William Kirby to Alexander Macleay, 21st June 1807, letter # 23, Macleay Correspondence Archive, Linnean Society Archives. For more on the work of Sowerby and the market for natural history images in this period, see Dolan [1998] 282-6, 293-304.

layer; the drawing was then cut into this layer using a steel point so that parts of the plate are exposed; the plate was then dipped in nitric acid until the acid sufficiently corroded the exposed lines. This process produced less crisp images than engraving, but allowed for more flowing lines. Etching was easier to learn than engraving and quite a few naturalists in this period learned the process.[343] Stipple was a form of etching particularly suitable for printing crayon drawings. The process was similar to that of etching, but the images were transferred as a series of tiny dots instead of lines. Aquatint was also based on the etching process, but through the use of many tiny circles and dots allowed for more tone and shading – prints produced from such plates do not have clear lines and most closely resemble watercolours.[344] All of these methods were in use during the period of Donovan's career. He seems generally to have favoured the clean-cut lines of etching and engraving over aquatint. The final stage of the process was colouring. Here, Donovan excelled. His meticulous attention to detail was noteworthy and he went to great lengths to acquire the correct pigments, even using gold for many of the iridescent insects.[345] Contemporary naturalists acknowledged that "great labour has been bestowed upon the colouring of the plates he published" while reviewers agreed that "the colouring of the plates ... seems to be executed with care and attention".[346]

Few naturalists of this period participated in this long, cumbersome process of illustration but Donovan has always been credited with undertaking the work himself. He seems to have managed this by keeping a workshop at either his house or museum and employing several workmen to help in the prodigious task of producing over 50 fully-illustrated volumes in a 40-year span. The only reference to these workmen is in a brief letter to fellow naturalist William Swainson (1789-1855). This letter appears to be a reply to one in which Swainson accused Donovan of having found fault with his colouring of entomology plates, Swainson then seems to have asked for Donovan's professional help with the plates. Donovan's reply gives a small window into the relationship between naturalist and workman, he wrote:

> ...If it lies in my power to meet your wishes it will give me much pleasure to do so. I have always workmen in that line about me but I am sorry to say that to be "content in your station" does not rank among their moral virtues. Good hands

[343] Jackson [1985] 29.
[344] Ibid. 250-5.
[345] Jackson [1998] 78.
[346] Swainson [1840] 169; Anon. [1797] 447. But some found his colouring too vivid, bordering on the gaudy – Salmon [2000] 130.

> feel their importance, & indeed their wants likewise, & perhaps it may be more than can be expected that the price of a periodical publication would allow a sufficient recompense for the labour after the "indispensible claims" of a bookseller are satisfied. ... It is rather more than twenty years since I engaged in colouring any work excepting my own publications...however if I can make any suitable arrangements with my workmen it will pass with other work in the ordinary course of business and so far I may have it in my power to see that it is done properly...[347]

From fragmentary evidence, we can piece together something of the illustration process and of the roles of those involved in it. It is difficult to determine the precise mechanics of the production process, but it is possible to see that Donovan was closely involved in every step of it.

This was true too of the actual publication of the works. Here, there is considerably more evidence about the process than there is in the case of illustration. Generally, there is little information about the relationships between naturalists and their publishers or booksellers in this period; but Donovan is unusual in that he wrote explicitly about the financial arrangements behind his publications. Donovan, from the beginning of his career, had co-published his books with a bookseller rather than working with a publisher. This seems to have been a slightly unusual but not completely uncommon practice. In 1833, Donovan published a seven-page pamphlet entitled *To the patrons of Science, Literature and the Fine Arts. Mr. E. Donovan...most respectfully solicits permission to submit to his former Subscribers, and to the Public generally, a brief memorial of his case with certain Booksellers with whom he has been associated in Literary Property for many years*. Towards the end of his career, Donovan had had a dispute with his booksellers – Rivington's of Saint Paul's Churchyard[348] – about payment for his works. In this pamphlet he laid out his case to the public and appealed for donations to help him take the case to the Court of Chancery. Such a document gives us a rare insight into the financial workings of natural history publishing in the late eighteenth and early nineteenth centuries.

In the pamphlet, Donovan explained the agreement that he had had with the booksellers. Writing of the 1792 publication of the first part of *British insects*, he said: "This was a partnership concern, of which, by agreement, two-thirds of the property was

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[347] Edward Donovan to William Swainson, 22nd November 1820, letter # 221, Swainson Correspondence Archive, Linnean Society Archives

[348] Rivington's was a family-run printing and bookselling business established early in the eighteenth century. Over the years, at least fifteen of the Rivingtons were involved. Donovan seems to have dealt initially with Francis I and Charles III, and possibly with John III, George and Francis II later.

to be my own, and one-third the property of the Booksellers therein associated".[349] This was superseded by a later agreement in which "it was agreed that these Booksellers should have an equal half with myself in that work; and afterwards the same proportion of the other publications which have been produced as partnership concerns".[350] Donovan did not make it clear why the arrangement had changed, nor how it was worked out that an equal partnership was more appropriate than the previous two-thirds/one-third split. For quite a long time, the partnership seems to have been amicable; certainly Donovan and the Rivingtons appeared to be still on good terms in the 1820s.[351] But at some point the relationship deteriorated dramatically and the terms of the original agreement came to be bitterly regretted by Donovan. The pamphlet was the end result of extended wranglings over disputed payments. Donovan claimed that the booksellers had withheld vast sums of money due to him. He further claimed that the booksellers were planning to withhold their account books indefinitely so that the Statute of Limitations would prevent Donovan from taking legal action. The case was also complicated by the fact that the law at that time required cases in which one partner sued a fellow partner to be heard before the Court of Chancery – an expensive legal process that Donovan, now nearly bankrupt, could not afford.

The sums in question were enormous. Donovan estimated that Rivington's owed him in the region of £60,000 to £70,000. It is difficult to ascertain exactly how Donovan arrived at the figures mentioned but he claimed to have documentary evidence, lodged with a London attorney, which contributors to his campaign were welcome to examine. The money owed was not entirely related to natural history publishing; Donovan seems to have made private, interest-free loans to the firm in the earlier part of their relationship.[352]

Another issue was that of copyright. Although Donovan was entirely responsible for producing the content of the books, the booksellers claimed that since they were joint-published, they held an equal share of copyright. A passage from the pamphlet described how Donovan

[349] Donovan [1833] 1.
[350] Ibid.
[351] Edward Donovan to William Swainson, 22nd November 1820, letter # 221, Swainson Correspondence Archive, Linnean Society Archives.
[352] Donovan claimed that these loans were in the region of £32,000. Over a 40-year period, this would be about £800 per year – a very substantial amount.

> had furnished, in addition to the literary matter of this extensive work, a series of engravings, executed in a peculiar and expensive style, at the direction of the bookseller, the costs of which amounted to nearly £700, expecting, upon the agreement, the repayment of such outlay. ... The work was at last finished; and then the bookseller refused the payment altogether, alleging that the plates belonged to the work, and that consequently they were entitled to them as partners in the undertaking, without any payment ... they not only refused to pay for them, but also declared their defiance against any use being made of them, excepting for that specific publication to which they belonged.[353]

The Copyright Act of 1814, enacted for the "encouragement of learning", would presumably have given Donovan some protection in such matters had the case ever actually been brought to court.

The pamphlet was essentially a public appeal to raise sufficient funds to allow Donovan proceed with the court case.[354] In laying out the events that had led to the appeal, Donovan not only gave a lot of information about the intricacies of publishing partnerships in natural history, but also highlighted what he considered to be the most important points of his career. Even the pamphlet's title, which gave equal weighting to science, literature and the arts, revealed something of Donovan's approach to his work. His focus on literature is especially interesting. The literary part of Donovan's career seems to have played an important role in the creation of his professional identity. He often referred to himself as an author first, and a naturalist second.[355] Throughout this essay, Donovan stressed the importance of his case to other authors. He wrote that the case was "of more vital importance to the true interest of British literature than has ever engaged the investigation of a Court of Equity... [without it] the many essential points of law which its results would establish, with regard to literary property, will remain, most probably, as at present, in a great measure wholly undefined".[356] Donovan definitely saw himself as part of a literary community; the pamphlet not only outlined his own grievances, it also showed solidarity with other writers and warned younger authors about the potential pitfalls of a career like his. Donovan wrote that "Authors inexperienced in the *finesse* of bookselling concerns may be induced to place their confidence in specific agreements. These agreements, from the very nature of literary property...are liable to many abuses". This was partly an expression of his own bitterness about the situation he

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353 Donovan [1833] 6.
354 In the end the case did not come to court; Donovan failed to raise sufficient funds and his health began to seriously decline.
355 This is especially noticeable in this pamphlet and in his correspondence with the Royal Literary Fund – both of which were pitched at an audience already interested in literary endeavours.
356 Donovan [1833] 2.

found himself in but it also showed his more general concerns about the state of the publishing business.

So far, Donovan's career has shown us how one could function as an independent naturalist: for much of his career he was largely self-sufficient; he wrote, illustrated and published his own books using his own specimens. Many others were in similar situations, but some naturalists had very different experiences of publishing; either through social connections or employment they found themselves with access to resources unavailable to someone like Donovan. In order to contextualise Donovan's publishing career and to see what other paths a naturalist could have followed, it is useful to make a direct comparison with a contemporary; I have chosen George Shaw for this purpose. Shaw also worked in London and moved in similar circles to Donovan, he too wrote a variety of popular books on natural history. Shaw began his career in the church before moving into medicine and then into natural history. Unlike Donovan, Shaw's education is well documented: he took his BA and MA from Magdalen Hall, Oxford; was ordained a deacon shortly afterwards; then went to Edinburgh to study medicine. His career began with a botany lectureship in Oxford, followed by a move to London to practice medicine. In London, he quickly rekindled an interest in natural history that he had had since childhood and was appointed assistant keeper of natural history at the British Museum in 1791, rising to keeper in 1807. He lectured at the Royal Institution in 1806 and 1807. Shaw was also an early member of the Linnean Society; he was associated with James Edward Smith who had also studied medicine in Edinburgh. In many respects Shaw and Donovan had very different careers – Shaw had a doctorate, a paid position at a national institution, and various lectureships – but they had much in common; they were both passionately interested in popularising natural history, both published extensively on similar topics and both were employed in the display of natural history collections.

Because of his position in the British Museum, Shaw was never financially dependent on his books. The Museum provided a secure (if not particularly high) salary as well as lodgings; moreover, it provided Shaw with access to some of the best natural history collections in the country. Having this access meant that Shaw himself didn't have to spend large sums of money on specimens and on storing or displaying them. Shaw was in a unique position among naturalists of this period; there were only a tiny number of salaried posts in natural history, and fewer still that gave access to such collections and sufficient time to write. Because of his education and the contacts that he had made in Oxford and Edinburgh, Shaw had many advantages and became more established more quickly than someone like Donovan. Being offered the British Museum job meant that Shaw never had to experience the kinds of financial trouble that plagued men like Donovan.

Here, I briefly discuss Shaw's publishing career and the popular nature of his work. Shaw's first book, published in 1790, was entitled *Speculum Linnæanum*. There are certain noticeable parallels between this and Donovan's earliest written work, *The botanical review*. The two were printed almost simultaneously in London, both stuck closely to Linnean orthodoxy and both were heavily dependent on images, as well as text, to convey meaning and to appeal to a wide audience. But there were differences too: where Donovan used his books as a showcase for his artistic skills, Shaw engaged dedicated professionals to provide the illustrations for his works. In the case of *Speculum Linnæanum* it was James Sowerby (1757-1822) who was responsible for the plates.[357] Shaw's approach was the more common one – few naturalists made their own drawings and plates for publication; it was more usual to contract such work to men like Sowerby. The Sowerby family had built for itself a strong reputation as artists and engravers in natural history; they ran a large workshop, took on many apprentices and illustrated hundreds of scientific books. This approach was far more cost-effective than Donovan's method. While Donovan emphasised plates over text, Shaw concentrated on words and trusted others to provide images. Another noticeable difference was language. Donovan used Latin only in specific descriptions, but in Shaw's first book alternate pages gave the same text in English and Latin. Shaw's text was also of a more consistent quality than Donovan's and his style was generally more conversational than scientific. Latin was only used extensively in this first book, afterwards, Shaw wrote primarily in English. His books were aimed a general, non-specialist audience and seem to have been reasonably popular.

Like Donovan, Shaw wrote many different kinds of texts: Shaw was responsible for the original guide to the Leverian Museum (which Donovan would later catalogue for sale) as well as an abridged collection of the Royal Society's *Philosophical Transactions*. His

[357] This was the same Sowerby who had nominated Donovan for fellowship of the Linnean Society.

most ambitious projects were the *Naturalist's miscellany* and *General zoology*. The *Miscellany* began in 1790 and was published annually until Shaw's death in 1813. *General zoology* was a more rigorous textbook based on the same material as the *Miscellany*, Shaw began it in 1800 and published eight volumes in his lifetime, another six volumes were published posthumously.[358] As with his other works, Shaw wrote the text and used the illustrations of others; the subtitle of the work read: "With plates from the first Authorities and most select specimens engraved principally by Mr. Heath". This octavo book contained long descriptions, often interspersed with digressions about other topics or debates about issues in zoology. Throughout, it was engaging and easy to read. The plates, having been executed by different artists, were of inconsistent quality and were uncoloured. Each volume of *General zoology* was dedicated to a different zoological class and began with a broad discussion of how that class was defined and constituted. It was manifestly different from Donovan's major works. And yet both authors wished to appeal to similar audiences – both could have been used by a reader with little prior knowledge of natural history. The fact that Shaw wrote until his death, and that the series was then continued by his colleagues gives some indication of the popularity of the series.

The lives and works of these two men illustrate some of the issues surrounding careers in natural history writing and publishing. Donovan became a naturalist at a time when there was no standard formula for such a career. The problems that he experienced with his booksellers were probably partly due to his lack of business experience when he first entered into the agreement, but perhaps were also attributable to the absence of a good model for the publication of these kinds of books. The growth in the market for such books in the late eighteenth century resulted in a proliferation of different publishing methods. Sadly, the one that Donovan chose ended in financial ruin. This was despite the fact that Donovan's books remained popular throughout his lifetime; they sold well and a full set would have cost in the region of £100 but Donovan seems never to have profited from them. Shaw fared somewhat better, although without his British Museum post he too would have struggled to make a living from his books. These cases illustrate the difficulties of pursuing a career in natural history writing at that time. Being essentially 'self-employed' in the early years of an emerging scientific discipline presented

[358] These later volumes were compiled and edited by the entomologists James Francis Stephens (1792-1852) and William Elford Leach (1791-1836).

an enormous challenge. There was no single clearly-established method of building a reputation, becoming an authority or even securing an income.

Collecting and Displaying

We have seen how Shaw's career and access to the British Museum collections allowed him to publish widely without worrying about creating a cabinet of his own. Few were in such an advantageous position; the majority of naturalists had to rely on their own collections or on specimens borrowed from friends or societies. Collecting, at least by gentlemen naturalists, was a serious undertaking that required vast sums of money, much time and large areas for storage or display. Donovan began his natural history collections before 1788.[359] When, in 1818, he was compelled to sell off his collections, the auction took eight days and consisted of 878 lots. In the intervening thirty years, Donovan had become one of the biggest natural history collectors in London. His collections were so extensive that when he formed them into a public museum, it was regularly cited as one of the most comprehensive natural history displays in Europe.[360] But Donovan was not simply one of the wealthy dilettante naturalists who, in the late eighteenth century, formed vast collections for their own amusement. Rather, his collecting project was tied into his writing career and his general aim of disseminating 'useful knowledge'. The way in which Donovan assembled his collections, used them and assimilated them into his overall goal of having a career as a naturalist is the subject of this section.

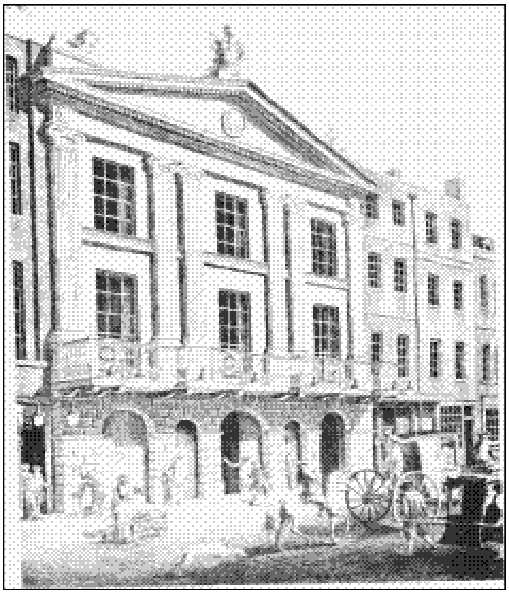
Little information exists about Donovan's earliest collections. His 1794 work *Instructions for collecting*... included sections on quadrupeds, birds, reptiles, shells, corals, plants and insects. This is probably an indication that even his initial exercises in collection had been very broad in scope. This book also showed that Donovan was familiar with field-work and with preservation techniques; unlike many collectors who only bought pre-prepared specimens, Donovan had the capacity to find and preserve his own. But Donovan was never averse to making substantial purchases at sales or engaging

[359] Donovan [1807].
[360] Donovan [1808] v-x.

in private transactions to achieve his *desiderata*. Museum catalogues, sale catalogues and correspondence all give information about why, what and how Donovan collected.

The most public expression of Donovan's love for natural history collecting was the founding of the London Museum and Institute of Natural History in 1807. This museum, located in London's west end, was set-up, financed and run entirely by Donovan.[361] The museum was open to the public and entry cost one shilling. In the first edition of the museum's catalogue, Donovan set out his objectives:

> The primary object of the London Museum, which under the auspices of an enlightened nation might be rendered the source of much rational amusement and instruction, is to concentrate within one general view a comprehensive and well-digested series of the various native productions of the British Empire, in the several departments of the Animal, Vegetable, and Mineral Kingdoms.[362]



View of Brydges, Street, Covent Garden where Donovan's museum was housed when it opened in

This idea of mixing 'amusement' and 'instruction' reminds us of Donovan's written works and his penchant for combining instructive prose with engaging illustrations. The museum was aimed at the general public rather than the specialist naturalist – we see that Donovan wrote of a 'general view' and a 'well-digested series'. He made it clear that he was interpreting the specimens and so making them available for public consumption. This juxtaposition of science and entertainment occurred several times in Donovan's descriptions of the museum:

[361] The museum occupied at least two locations: Brydges Street, Covent Garden (which was later renamed Catherine Street); and 197 Fleet Street.
[362] Ibid. iii.

> ...The whole displayed in the order of scientific arrangement. In the present instance this assemblage comprehends nearly thirty thousand individual articles, and is collectively calculated to display, in the most pleasing and impressive manner, the grandeur, variety, beauty, and intrinsic value of the native riches of the country, and their various applications to the useful purposes of man.[363]

Donovan's museum career, like his writing career, seems to have been at least partly motivated by nationalism. He saw the museum as the first step towards creating a "National Academy of the Natural History of the Country, in the centre of the metropolis".[364] These nationalistic aims were combined with a measure of public-spiritedness to form the basis of the museum; he wrote in the catalogue that he was

> induced to establish this Museum for a purpose as laudable as it is novel in this country... He conceived the establishment of an Institute of this peculiar nation in every respect worthy the dignity and genius of the British Nation, and is free to confess that rather from motives of public spirit than by any other consideration, presumed to submit his design to the test of public discrimination.[365]

The Museum focussed primarily on British natural history and on that of countries under British rule. As we have seen with respect to Donovan's writing career, this intense focus on British natural history was unusual and marked out his museum from the many others nearby.

The contents of the museum were incredibly varied. The survival of several catalogues and prospectuses gives us some insight into what was on display and how the displays were arranged. The first catalogue was published in 1807. An extended second edition was produced the following year and gives considerably more detail about the exhibitions – acting as a virtual tour of the museum. The museum was arranged in a hierarchical order: it began with models or drawings of "primeval man" (subdivided into English, Scottish, Cambrian and Hibernian); then came preserved specimens of quadrupeds followed by cetaceans, birds, reptiles, fishes, insects, crustaceans, worms, echini, shells, zoophytes, plants and minerals. Each section appears to have been remarkably comprehensive: the catalogue lists almost 70 species of British quadruped, more than 300 species of British birds, 150 of fishes, over 100 of crustaceans, and 900 of plants. When it came to insects, Donovan's speciality, he wrote: "It is altogether impossible to enter with any degree of propriety upon such a multifarious and extensive

[363] Ibid. iv.
[364] Ibid. iii.
[365] Ibid. iv. Although this passage is in the third person, it was almost certainly written by Donovan – the style and the content are both characteristic of his work, as is the propensity to use the third person when discussing himself.

subject as the Entomology of Great Britain within the scanty limits of a catalogue... the Insect department alone comprising many thousand subjects".[366] The museum also contained large numbers of fossilised remains of animals and plants: there were more than 1000 fish fossils and several hundred antediluvian shells. Donovan also claimed that "The department of Recent and Antediluvian Botany, collectively considered, is allowed to form, beyond comparison, the most perfect assemblage of the Botanical productions of the British Isles that can exist in any Museum".[367]

An interesting feature of the museum was how much it contained that did not pertain to natural history. There was a respectable collection of British antiquities as well as several objects of art. The most noteworthy work of art in the museum was a panoramic oil painting of Jerusalem. This canvas (about which a special pamphlet was published by the museum) is interesting in its own right, but it is also helpful in giving a sense of the scale of the museum – it was more than 100 feet long and 18 feet high. The "Saloon of the Great Apartment" where it was located must have been quite substantial in size. The canvas was painted by Donovan himself and based on sketches drawn by his son, "Mr. J. Donovan", during a trip to the Holy Land in 1811 and 1812. The pamphlet about this painting also advertised a collection of models of sites in the Holy Land as well as maps, drawings, natural history objects, works of art, antiques and coins from the area (presumably all brought back by Donovan's son). This mixture of natural history with man-made artefacts was indicative of Donovan's desire to appeal to a wide audience.

The museum was intended as a place of popular entertainment as well as of learning. Its west-end location meant that it was in close proximity to many other similar establishments. Museums containing objects (both natural and man-made) from overseas, objects of curiosity, or other objects of scientific interest proliferated in this area during the late eighteenth and early nineteenth centuries; possibly the most famous example was the Egyptian Hall in Piccadilly. The site of Donovan's London Museum at 197 Fleet Street had previously been occupied by Rackstraw's Museum of Anatomy and Curiosities; just a few doors up had been Mrs. Salmon's Waxworks.[368] Several times in the catalogues, Donovan referred to the spectacle of his displays. In the case of the large number of antediluvian fishes, rather than listing them all, he stated that he would "briefly mention

366 Donovan [1808] 41.
367 Ibid. 54.
368 Beresford Chancellor [2005] 24.

those which form the most striking objects, and are likely to arrest the immediate attention of general observers".[369] But despite the popular nature of the venture, Donovan never lost sight of the science behind his displays. Wherever possible, animal displays contained male, female, and juvenile specimens together – this was in contrast to many museums that simply displayed the more brightly-coloured males.[370] He emphasised the "scientific arrangement" of the exhibits and outlined the Linnæan system of classification for museum-visitors.

Donovan's collections were particularly impressive. By the time he opened the museum, he had been collecting seriously for almost 20 years. The collections were formed through various methods – Donovan engaged in field-work, bought and exchanged individual specimens privately and bought pre-assembled collections at auction. We know little about Donovan's field-work for none of his notebooks survive, but occasional references in his printed works give some information. It is surprising how many uncommon specimens seem to have been casually chanced-upon: the beetle *Chrysomela coccinea*, for example, "was taken on a thistle in a field between Kennington Common and Camberwell"; and the moth *Sphinx chrysorrhœa* "was met with in Kensington Gardens in June".[371] Donovan lived close to Kennington in south London and must have been in the habit of searching for interesting insects nearby. This sort of very local field work is reminiscent of naturalists such as Gilbert White. It also chimes with Donovan's preoccupation with British natural history; many contemporary naturalists seemed to be primarily interested in insects sent from overseas but Donovan always had time for the local. Being constantly on the look-out for new specimens lead to some novel discoveries: in 1809 Donovan described how he found a previously unknown species of duck (*Aythya collaris*) in London's Leadenhall Market.[372] Donovan rarely travelled out of London, the only noticeable exception being a series of trips to south Wales between 1800 and 1804. In 1805 he published a two-volume book about these trips. This account, a lively and engaging travel narrative, contained some information on natural history but it avoided detailed discussions of the field-work undertaken on these trips.[373] Although Donovan himself never travelled very far to collect, some of his family

[369] Donovan [1808] 38.
[370] Jackson [1985] 183.
[371] Donovan [1795] vol IV, plates CXI, CXVI.
[372] Donovan [1809b] vol VI.
[373] Donovan [1805].

did. In 1812, his son brought back "a pretty considerable number of well preserved subjects taken by himself in the Mediterranean".[374] It seems likely that Donovan's children were well-versed in natural history collection and preservation from a young age but this is the only reference to any of them collecting for their father.

The second source of Donovan's collections – private sales and exchanges – took place primarily within a small, closely-linked group of naturalists in London. Donovan seems to have known many of these men through the Linnean Society, and it is in their archives that we find evidence about some of these collecting activities. Even brief references can give a lot of information. A letter sent in 1801 from the entomologist William Kirby (1759-1850) to Alexander Macleay, secretary of the Linnean Society, revealed something of the mechanics of these private sales. Kirby wrote:

> Mr. Donovan, after I last saw you in town, mentioned to me, that you wished to have the large Sphinx from Surinam. When I shewed the insects intrusted to me first to him I reserved the refusal of them for Mr. Marsham & yourself. I desired then, before I gave a final answer as to the fulgora to speak to the owner, he is willing to take the price Mr. Donovan offered for it, viz £1.1.0 – as to the others, if you like to take them, give for them what you think they are fairly worth, which I believe is a trifle scarcely worth mentioning, between you and I. The money may be paid in to Marsham's hands; will you tell Donovan this?[375]

The large number of people involved in the transaction is evident. The anonymous owner used Kirby as an intermediary to offer his specimens for sale to fellows of the Linnean Society. Once a sale was agreed, the purchaser paid the money to Thomas Marsham, then treasurer of the Society, who presumably forwarded it to the seller.[376] The whole was conducted along gentlemanly lines, with the purchasers being trusted to name a fair price, and money being deemed "a trifle scarcely worth mentioning". The passage also gave an indication of the large sums of money that Donovan was willing to spend on his collections: £1.1.0 was a considerable sum to spend on a single entomological specimen. As well as selling specimens to each other, this group would swap or loan particularly rare ones. References to such casual exchanges are scattered throughout these letters. It appears to have been common practice to lend specimens for the preparation of book plates and specific descriptions.

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[374] Edward Donovan to William Swainson, 24th September 1816, letter # 220, Swainson Correspondence Archive, Linnean Society Archives. Presumably this was the same son who had travelled to the Holy Land.
[375] William Kirby to Alexander Macleay, 14th October 1801, letter # 4, Macleay Correspondence Archive, Linnean Society Archives.
[376] Or perhaps not – Marsham suffered financial trouble in later life and "borrowed" heavily from the Linnean Society's accounts to pay his debts.

The final, and largest, source of specimens for Donovan's museum was the purchase of collections at auction. In the museum catalogue, Donovan listed the principal collections that he had bought, including:

> The Portland – Leverian – Edinburgh – Calonne – and Litchfield Museums. Collections of Earl Bute – Earl Donegal – Mr. E. Da Costa – the Rev. J. Lightfoot – Dr. Fordyce – Mr. Drury – Mr. Green – Mr. Keate – Mr. Cordiner – the Hon. Daines Barrington – Dr. Parsons – Mr. Plott, the Natural Historian of Oxford – Mr. Ingham Forster – Mr. Jacobs, Author of the History of Faversham – and the Rev. Mr. Parlby of Saffron Walden. Duplicate collections of Dr. Woodward – Sir Ashton Lever – and Dr. Latham &c.[377]

Of these, the collections from the Leverian Museum and those of Dru Drury merit special mention. Sir Ashton Lever (1729-1788) had begun his natural history collection with live birds before turning his attention to fossils, shells and preserved specimens in the 1760s. The collection grew so large that Lever opened it to the public and moved it to large premises in London's Leicester Square. The museum was not a financial success and shortly before his death Lever sold it, by means of lottery, to one James Parkinson (1730-1813). Parkinson too failed to make money from the museum and by 1806 had decided to sell the contents at auction. To facilitate the auction, he engaged Donovan to compile a sale catalogue. Through working on the catalogue, which listed almost 8000 lots, Donovan became well acquainted with the museum's contents. At the auction, which took place over 65 days, Donovan was the largest purchaser, buying several hundred lots, some of them very expensive.[378] Dru Drury (1725-1804), like Donovan, was especially interested in entomology. He assembled his collections by paying sailors and travellers to collect for him; the collections were not on public display, but were well known to naturalists, including Donovan. After Drury's death, the "most capital assemblage of Insects" – more than 11,000 specimens – was auctioned off by King and Lochee in 1805. An annotated sale catalogue shows that Donovan was one of the biggest purchasers.[379] These two collections formed a central component of the London Museum and Institute of Natural History.

The London Museum and Institute of Natural History received excellent reviews and testimonials. These were to be found in periodicals such as *Nicholson's journal* and John Aikin's *Athenæum* as well as in letters addressed directly to Donovan. The reviews,

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377 Donovan [1808] iv.
378 Donovan [1806].
379 Anon. [1805].

largely written by naturalists, focussed primarily on the scientific aspects of the museum. James Parkinson (1755-1824), a noted surgeon and amateur palaeontologist, wrote in *Nicholson's* that he wished

> to call the attention of the curious as well as scientific to the most complete collection of British natural history that has ever yet been formed; a museum not confined to any one particular department of nature... it will not be too much to say that this museum from the Science evinced in its arrangement, independently of its importance as a collection of [rare] and valuable specimens, must to those desirous of such knowledge prove a most instructive school and afford an inexhaustible fund of information to all those who think the natural history of their own country worth attending to.[380]

Aikin agreed that the "elegant museum... is unquestionably the most complete in its kind that exists any where & contains a greater number and much more valuable assortment of particular specimens than the richest cabinets of Europe would collectively afford".[381] Aikin also commented on the combination of scientific knowledge and aesthetics on show in the museum, he wrote that it was "arranged in scientific order & with an elegance of taste which, while it facilitates the inquiries of the student, charms the eye with an assemblage of the most splendid and delightful pictures".[382] One George Humphrey favourably compared Donovan's museum to royal and national collections in Paris, Madrid, St. Petersburg and Haarlem and praised Donovan for putting science first when constructing his displays. While Captain J. Laskey, writing in the *Medical and physical journal*, enthused at length about the collection:

> I have, at various times, had an opportunity of seeing almost every Cabinet and Museum, public as well as private, of any celebrity in this country: and I am confident in saying that so far from any one of these being comparable, the whole of them added together would not form a collection of British Natural History by any means so extensive, valuable, or instructive. I consider the divisions of birds and fishes the only perfect collections known. The organic remains of the ancient world consist of the most illustrative specimens; and I cannot help observing further, in every other department objects of the greatest rarity occur. ...[I] have no scruple in saying it would be impossible at this time for any collector, possessing the most unwearied attention, sanguine wish, and unlimited purse, to form another collection equal to that now before the public, under the appellation of the London Museum.[383]

[380] Parkinson, item # 17 in Donovan's File, Royal Literary Fund Archives.
[381] Aikin, item # 18 in Donovan's File, Royal Literary Fund Archives.
[382] Ibid.
[383] Laskey in Donovan [1808].

Although the London Museum was well regarded it did not enjoy the commercial success that Donovan had anticipated. Like the Leverian and many other natural history museums, it encountered financial difficulty. It is possible that if Donovan's publishing career had been more lucrative, he could have kept the museum afloat for longer. But it was not to be and in 1817 Donovan published a prospectus for the sale of the museum. He wrote that the museum was closing because of "the want of that due encouragement in the public generally, which the magnitude of such a design demanded", but he consoled himself with the knowledge that "his own endeavours individually, and unsupported, in the cause of science and his country, have been approved".[384] This document did not have the same tone of despair that characterised the 1833 pamphlet about booksellers, but Donovan's distress about the dispersal of his collections was evident. It was his wish that the collection should be preserved for the nation and he tried to raise a public subscription with the intention of allowing the British Museum to purchase it in its entirety. Sadly, this didn't happen and Donovan resigned himself to breaking up the collection and auctioning it off. The sale began on Thursday 30th April 1818 and continued for seven days. As with many other sales of natural history collections at this time, the auction was conducted by King's of King Street, Covent Garden. The most striking thing about the sale catalogue is the near-complete absence of entomological specimens. Of the 878 lots, only 25 contained insects. Donovan must have either been unable to part with his favourite specimens or else found a private buyer in the Linnean Society or elsewhere. Today, a few of Donovan's specimens survive as type specimens in the Natural History Museum, London, and the Hope Collection, Oxford. The location of the rest of this once great collection is unknown.

Donovan's collections were assembled for use in his publications and museum, but many naturalists collected for other purposes. Just as it is useful to compare Donovan's writing career to that of someone like Shaw, so it is useful to compare his collecting career to that of a contemporary in order to understand how different naturalists approached this activity. For this purpose I have chosen Alexander Macleay. Macleay was born in Scotland in 1767. Like Donovan, he was in London by his early twenties but, unlike Donovan, he did not immediately begin a career in natural history. Lacking the kind of family wealth that Donovan must have enjoyed, Macleay worked as

[384] Donovan [1817] 3.

wine merchant for a few years before joining the civil service in 1795. There he progressed steadily up the ranks – he joined the Transport Office as a clerk, was promoted to head of the correspondence department and then became secretary to the board. Despite the demands of a full-time post in the civil service, Macleay retained a keen interest in natural history and his collections appear to have steadily grown throughout this period. He must also have gained recognition as a naturalist from his peers, for he was elected a fellow of the Linnean Society in 1794 and became its secretary in 1798 – a position he held until 1825. Macleay never wrote, illustrated, published, demonstrated nor taught natural history; his reputation as a naturalist was built on two things – his collections and his position in the Linnean Society. [385]

Macleay's collections were built up in much the same way as Donovan's: a small amount of field-work produced some specimens; more came through private sales and exchanges; but the bulk of his collection was accrued through purchases made at auction. A letter written in 1819, when Macleay had been collecting for 20 or 30 years, gives some idea of the status of the collection; writing of a rumour that Macleay and his collections were about to leave London, the entomologist William Kirby wrote:

> That so princely (I might call it imperial) a collection of insects should with you, be withdrawn from the scientific world, & as it were buried alive, is an idea that quite petrifies me. ...[You would] not permit so vast a collection to be made in vain. Let it not be said that the mere *Amor habendi* urged you to collect.[386]

This also shows that Macleay, like Donovan, did not collect simply for the sake of it. Macleay's way of making his collections useful to science was different to Donovan's. He himself did not publish anything about them, but they were intended for scientific purposes. To this end, he was extremely generous about lending specimens to others. We see this particularly in the case of William Kirby – a vast correspondence between these two men is preserved in the archives of the Linnean Society and it shows the extent to which Kirby utilised Macleay's specimens in his publications.

Another difference between Macleay's collections and Donovan's was their content. Although entomology was Donovan's particular interest, he collected widely in all branches of natural history and his museum catalogues show how varied his collections were: quadrupeds, birds, reptiles, fishes, crustaceans, zoophytes, plants and

[385] Stacey & Hay [2007]; Boulger and Fletcher [2004].
[386] William Kirby to Alexander Macleay, 10th May 1819, letter # 149, Macleay Correspondence Archive, Linnean Society Archives.

minerals were all present in significant numbers. In contrast, Macleay's collection was composed almost entirely of insects. He had had a bird collection and a small number of quadrupeds, but he sold these off in order to buy more insects.

Donovan was devoted to making scientific knowledge public. From the earliest days of his career he explicitly stated that his work was intended to bring natural history into the public sphere. His books and collections were employed for this end. This was not the case for Macleay. Macleay never seemed to have considered putting his collections on public display. He did not see his role in natural history as a public one. As the secretary of a private society, Macleay may have been in a position to facilitate others' work in popularising natural history but he himself took no direct part in it. Many natural history societies in the late eighteenth century were open to all naturalists but the Linnean Society was an exclusive organisation and gaining fellowship was not an easy task. The Society's principal preoccupation was the propagation of the Linnean system of classification and the standardisation of natural history methods, purposes and terminology; its audience was primarily the scholarly naturalist rather than the general public.

The role and status of the naturalist was not clearly defined in this period and many, such as Donovan, moved freely between the public and the private, the elite and the accessible. It was not simply because his livelihood was dependent on natural history that Donovan had to be flexible and his work had to have broad appeal. His changing financial situation shows this. Even early in his career, while still a wealthy man, Donovan had chosen to combine education and entertainment; though later, as his fortune dwindled, it became imperative that his work appeal to a wide audience. The model that he had developed for the juxtaposition of science and spectacle in his work should have helped to achieve this. Macleay's situation was different: he had never had an independent income, nor was he ever financially dependent on natural history. He had not the means to publish independently, nor the time to create a museum. Instead, like many others, Macleay held a full-time job and devoted his spare time to natural history. In the Linnean Society, Macleay was a central figure. The secretary was responsible for administration relating to membership and also for the papers presented at meetings and published in the *Transactions*. Through this work, Macleay came to be well known by naturalists and developed an extended network of useful contacts. Macleay acted as a conduit for books, journals and specimens moving between Linnean fellows or different societies. His position as a central scientific administrator is reminiscent of someone like Joseph Banks.

In 1824, Macleay's situation changed dramatically. He had suffered some financial trouble in England after retiring from the Transport Office in 1817 so when he was offered a job as Colonial Secretary in New South Wales he quickly accepted it. Having for so long moved in the same London circles as Donovan, Macleay suddenly found himself facing a new scientific horizon. If making a career as a naturalist in London was difficult, it was even more so in Australia. As in London, Macleay's principal occupation was his civil service post but he devoted a considerable amount of time to entomology and natural history. The animal and plant life of the continent was still largely unknown to European naturalists and Macleay was eager to begin collecting. Not only that, he had also declined the Linnean Society's offer to store his collections – he took his specimens and library on the perilous six-month sea-voyage to Australia; so, from his disembarkation, he had one of the finest cabinets in Sydney. There, where there was little established science, few societies, museums or collections, Macleay was in a position to define a role for himself in natural history. On his arrival in January 1826, Macleay began to join as many societies as possible, but none was exclusively dedicated to natural history. He also began to think about setting up a colonial museum and was granted two offices in a civic building to facilitate this. After acquiring a 54-acre site in Elizabeth Bay, Macleay began work on creating a botanic garden. He later became first president of the Australia Club. In many ways his career in Australia was very similar to the one he had had in England: he was central to several societies, sat on various committees, collected extensively, but didn't write about or display his collections.

Despite their differences, the careers of Macleay and Donovan ended in similar circumstances. Even though he had a steady income as well as his pension from the Transport Office, Macleay began to experience financial troubles. It is not entirely clear what caused the problems but presumably his expensive collecting habits were a contributing factor. In 1839, William Sharp Macleay (1792-1865), Alexander's eldest child and well-known naturalist, moved to Sydney. He resolved to help his father. When a series of loans failed to alleviate Alexander's difficulties, William decided that the library and some of the specimens must be sold. It was enough to save Alexander from bankruptcy, but led to a serious falling-out between father and son. It was not unusual for naturalists to encounter such financial problems. Collecting books and objects was not a cheap hobby – at least not at the level undertaken by fellows of the Linnean Society. Several other colleagues of Donovan's and Macleay's had had similar experiences, most notably Thomas Marsham who acted as treasurer to the Linnean Society from 1798 to 1816. Marsham found himself in debt because of his collecting and publishing activities, secretly borrowed money from the Linnean accounts (with the intention of returning it as soon as possible), and was then unable to raise the funds to repay the Society and had to auction off his collections. For these men, natural history was not a lucrative enterprise. None of the three had entered into natural history for the sake of a livelihood, but all eventually discovered the heavy toll that it could exact.

So we see that the problems that could dominate a career in natural history writing were also present in natural history collecting. The lack of training, lack of structure, incoherency of the field and idiosyncrasies of practice that characterised natural history were seen as commonly among collectors as among authors and illustrators. Donovan, who became financially dependent on his work, must have felt this particularly keenly. For Macleay, with his civil service post, it was less important to have a structured career in natural history. We see also that a career in natural history was dependent as much on chance and opportunity as it was on skill and hard work. This is especially clear in the case of the large number of natural history museums that flourished in London during the first two decades of the nineteenth century; during these years the Napoleonic Wars isolated Britain from the scientific centres of Europe. It was surely not a coincidence that Donovan's museum opened in 1807 during the wars and closed in 1817, shortly after they ended. Seizing such opportunities to advance one's own career (and British natural history) was characteristic of naturalists like Donovan. The absence of a career structure may have had many disadvantages, but it also encouraged this kind of entrepreneurial spirit.

Conclusion

The conception and practice of natural history were undergoing dramatic changes in the late eighteenth and early nineteenth centuries. Donovan's story highlights the options open to a gentleman naturalist, the difficulty of negotiating a career in the sciences, and the activities that might fall under the auspices of natural history. The first section of this chapter briefly discussed Allen's work on this problem; his analysis was based on distinctions between 'amateur' and 'professional' naturalists. In contrast, I have shown that the idea of the amateur or professional was not applicable to this group. Few naturalists would have presumed to compare their career to that of a lawyer, clergyman, doctor or soldier, nor did they wish to; the concepts of structure, training, utility and salary that defined those groups did not relate to the experiences of most naturalists. However, that did not mean that naturalists were unconcerned about constructing roles for themselves and defining a clear identity. By looking at the various aspects of Donovan's career such as writing, illustrating, publishing, collecting and displaying, we understand the possible ways in which an individual could publicise their work, establish credibility and earn an income; moreover, we see how these things could coalesce to form the basis of one's identity as a naturalist.

The complexities of creating an identity and forging a career make it difficult to generalise about the naturalist or to create a comprehensive model for describing him. As we have seen, naturalists in this period borrowed heavily from different aspects of professions, trades, scholarship and commerce in order to make a viable career; they then had to balance their activities with their social standing. It is sometimes easier to say what a naturalist was not, rather than what he was. He was not some kind of proto-professional. He was not necessarily a straightforward gentleman. Nor was he an uncomplicated tradesman. The best model is one that balances gentility against commerce. And for each naturalist there was a slightly different balance; this is what makes it difficult to create a useful working definition. Even for an individual, there was unlikely to be much continuity within a career. Donovan's case illustrates this well; the fluidity of his occupations and the unpredictability of his working life would have been familiar to many of his contemporaries. Such idiosyncrasies were a recurrent theme in the lives of naturalists, and it is more helpful to think of natural history careers in terms of these things rather than in terms of broad categories such as amateur or professional.

This chapter also gives a detailed account of the working life of a fellow of the Linnean Society. Donovan joined this group in 1794, soon after its foundation; he was quite representative of its fellowship and was closely connected to many key figures in the society such as Macleay and Marsham. Donovan's published works almost all use a version of the Linnean classification system and his programme neatly fitted into the mould that the society was trying to promote in British natural history. But Donovan's story gives a far better feeling of how a Linnean fellow might go about the pursuit of natural history than the minutes of a meeting or a copy of the *Transactions* ever could. His work shows that even as the Linnean Society tried to narrow British natural history the complexities of a career in the science meant that, through the eyes of a typical practitioner at least, the field remained as diverse and nebulous as ever. Though the society might ensure that the work of someone like Donovan was carried out in a Linnean framework, it could not so easily control the myriad of activities in which he could engage.

CHAPTER SIX

Species of exchange: natural history in Britain and Europe

> "As it now seems determined that we shall have a commercial treaty with France, and restore the golden age, which I foolishly imagined was only in Utopia, I hope a few exports and imports in natural history may be both allowed and obtained *while it lasts*."
>
> Samuel Goodenough to James Edward Smith, 11th March 1787[387]

Eighteenth-century British naturalists could use a range of tools to further their science, to explain the world around them, and to earn a living. The previous chapters have discussed some of those tools and this chapter explores another one: correspondence. The exchange of letters and objects between British naturalists and their colleagues on the continent allows me to explore two important considerations: first, the idea of communication as tool of natural history in the same way that a butterfly-net or microscope might be; and second, the mechanisms by which British natural history interacted with continental European natural history. By seeing correspondence as a tool of natural history, I can show how its practice shaped the work of various individuals. In considering interaction, I do not tell a simple story where 'influence' acts at a distance through mysterious mechanisms but, rather, focus on examining how naturalists communicated, what they shared, how they viewed their exchanges, which parts of foreign natural history they valued, which they discarded, why they prized particular objects or information, how they used their newly acquired knowledge, and what its results were for British and European natural history. As well as talking about the tools of natural history and the factors that affected its practices, I also wish to contextualise British natural history with respect to the natural histories of other European countries. Though I have deliberately focussed on Britain thus far, and though Britain had distinctive traditions in natural history, those traditions did not exist in isolation. This

[387] Smith [1832] 212.

chapter begins to look outwards from Britain to show its place in a wider European discourse on natural history.[388]

Several historians have written about the ways in which natural history travelled; for example, on the topic of the movement of ideas, Wood has written about how Scottish academics used the ideas of Buffon in their writings and teachings, and Reill has studied the use of Buffon in German historical work.[389] Underwood has examined how teaching can cause both people and ideas to circulate; his work describes how students travelled to Leiden to study under Boerhaave, and then follows those students as they spread Boerhaave's ideas in their later works.[390] The works of Drayton, Mackay, Frost and Koerner focus on the utilitarian and empire-building uses of natural historical exchange around the globe.[391] I will use some of these ideas to build a picture of how natural historical information moved into and out of Britain, but I will also rely heavily on studies of personal relationships between naturalists; to do this I will be focussing principally on correspondence. Eighteenth-century naturalists, like so many others of this period, devoted much of their time to correspondence; their letters are one of the most essential resources for studying interactions between individuals, especially when those individuals were separated by distance or borders and therefore incapable of face-to-face contact. Recent scholarship had highlighted the point that correspondence was not necessarily a straightforward enterprise; as with most of eighteenth-century polite society it entailed a complicated system of rules and structures. Correspondents needed to be formally introduced or, if no such introduction was possible, needed to find alternative channels of communication through mutual acquaintances or societies. Once the correspondence was established, both parties would be expected to adhere to certain rules of politeness.[392] By examining the relationships of several naturalists through their letters and parcels, and by contextualising them within this understanding of polite culture, I hope to see what kinds of natural historical ideas, books, information and

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[388] Since much has already been written about how Scottish natural history interacted with its European equivalent, I will mostly be focusing on England and Wales – where such connections have not been so thoroughly studied; see, for example, Wood [1987] for work on the reception of French natural history in the Scottish universities.
[389] Wood [1987]; Reill [1992].
[390] Underwood [1977].
[391] Drayton [2000], Mackay [1996], Frost [1996], Koerner [1996].
[392] An excellent recent work on eighteenth-century correspondence is Meredith [2009]. Two other good studies of correspondence (which fall outside the time period of this dissertation but have been extremely useful nonetheless) are Goldgar [1995] on the Republic of Letters in the earlier part of the eighteenth century and Secord [1994] on scientific correspondence between artisans and gentlemen.

specimens were flowing into and out of Britain. More importantly, I wish to see how they were considered and used, and what significance they had for natural history in different locations.

The first part of this chapter deals with friendly, constructive and "practical" correspondence and exchanges between a British naturalist – Thomas Pennant (1726-1798) – and naturalists abroad. This shows how parcels of letters, books, and strange Siberian specimens sent between the imperial capital of Russia and a small town in North Wales could have important implications for natural history. We see this in the way that Pennant used his contacts in Russia and Scandinavia to become an authority on the Arctic, despite never having travelled there; or in the way that some of Pennant's European correspondents developed their interest in British classification systems in their own works. The second section deals with less amicable exchanges between Pennant and the French naturalist Buffon on the fraught issue of garden moles. Much has been written about the reception of Buffon's books in various places, but less consideration has been given to his personal relationships with foreign naturalists; this is especially true for Britain. While several historians have written about, for example, Buffon's collaboration with the British naturalist John Turberville Needham (1713-1781), the focus has been more on their methods and results than on the implications for French or British science.[393] Roger has also commented on some of Buffon's friendships with Britons and his respect for British scholars and their works, but the effects of these things on the works of Buffon or British naturalists have not been discussed.[394] The third section of this chapter is about how Buffon's *Histoire naturelle* was translated into English, and what that tells us about British uses and views of French natural history. Through a comparison of several translations of the original French text, we can see what British naturalists valued most in Buffon, and in natural history more generally. That translators focused on description at the expense of theory highlights some interesting facts about what was going on in British natural history in this period. These three sections allow me to look at various aspects of relationships between British and European natural historians, and to relate personal interactions to more public ones.

[393] Gottdenker [1980]; Roger [1997] 140-150.
[394] Roger [1997] 41-42.

Thomas Pennant's letters to Europe

Thomas Pennant was a Welsh-born, Oxford-educated, gentleman naturalist. Between the 1760s and 1790s he published over 15 books on natural history and travel, and was widely regarded as one of the finest British naturalists of his day.[395] His books ranged from the local – such as *British zoology* and *A tour of Wales* – to the decidedly foreign – such as *Arctic zoology*. Here, I wish to look at the ways in which a naturalist who had travelled abroad only briefly went about gathering enough information to become an expert on a specialist topic, like arctic zoology, so far removed from his own experience and locale. Pennant's main resource for such work was the large correspondence network he had built over many years with naturalists in diverse locations. In this section I will be looking at Pennant's relationships with two particular correspondents: Peter Simon Pallas (1741-1811) in St. Petersburg and Carl Linnæus in Uppsala, Sweden. Pennant did not simply exchange letters with these two men; rather, he traded a variety of tangible and intangible things: information, books, unpublished material, plants, animals, minerals, and goodwill. Here, I will examine how the flow of such things into and out of Britain had implications for British, and European, natural history.

In 1765, Pennant began a tour of the Continent. He left London on the 19th of February and crossed to France; he passed through St. Omer, Aire, Arras, Péronne and Chantilly before arriving in Paris. After Paris, Pennant travelled to Burgundy and then on to Switzerland and the Low Countries. It was there, in The Hague, that Pennant met the young Peter Simon Pallas - a native of Berlin who had studied medicine, anatomy and zoology at Berlin, Halle, Göttingen, Leiden and London, and who was in The Hague to pursue studies in natural history. Pennant and Pallas hit it off immediately, and later Pennant recalled how "from congeniality of disposition we soon became strongly attached. Our conversations rolled chiefly on natural history".[396] So congenial and useful did both men find their initial meeting that they continued their natural history conversations for the next 15 years, by letter. Seventeen letters from Pallas to Pennant are known, but Pennant's replies have been lost.[397] To balance this one-sided view of a correspondence, I will also look at some extant letters written by Pennant to another

[395] Withers [2004].
[396] Pennant [1793] 7.
[397] This collection of letters was edited and published in 1967 by Carol Urness.

naturalist – Carl Linnæus, the famous Swedish systematist.[398] Pennant's epistolary relationship with Linnæus had begun as early as the 1750s and continued for many decades. These two collections of letters reveal much about the relationship between British and European naturalists in this period; they show what was exchanged between naturalists across national borders, how these exchanges were negotiated, and why it mattered.

Communicating by letter and parcel was not an easy task for Pennant and his overseas colleagues. This was particularly true for Pennant and Pallas after 1767 when Pallas moved from Berlin to St. Petersburg. Discussion of the mechanics of moving things between countries forms a significant part of Pallas's letters: during the spring and summer months, Pallas would ask Pennant to send items via London to the English consulate in St. Petersburg or via his sister in Berlin, and Pallas would send post to London friends such as the publisher Benjamin White (1725-1794) who would forward it to Wales; during the winter, when the port at St. Petersburg was frozen over for several months, communication between the two men was almost entirely cut off.[399] The mechanics of moving books between Wales and Sweden was less complicated than between Wales and Russia, but still required several intermediaries. Pennant asked Linnæus to send parcels of books to Messrs. Charles and William Totty in Stockholm; they would then forward the parcels to a Mr. Rigby in Liverpool and Pennant could either collect the parcels from there or have them delivered to his home 30 miles away in Downing, Flintshire. To speed the delivery of books from Wales to Uppsala, Pennant asked Linnæus to name contacts in London to whom he could forward packages destined for Sweden, or he would give the books to a mutual friend such as Daniel Solander who travelled frequently between Britain and Sweden.[400]

In the correspondences between Pennant, Pallas and Linnæus all kinds of things were exchanged; here I will discuss them under three main headings – information about systems and objects, printed material, and natural historical specimens. The earliest surviving letter from Pallas to Pennant (dated 1766, when Pallas was still in The Hague)

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[398] Pennant's letters to Linnæus are available in digital form at the website of the Linnean Society (www.linnean.org); some of Linnæus's replies to Pennant are available at linnaeus.c18.net.

[399] Pallas to Pennant Letter II, 9, 15, 18; Pallas to Pennant Letter III, 21; Pallas to Pennant Letter IV, 26; Pallas to Pennant Letter V, 28 – all in Urness [1967].

[400] Pennant to Linnæus letters L.5263 and L.4747, the Linnean Society Archives. The Totty brothers (merchants of Scottish origin) are also referred to as Charles and Wilhelm Tottie by Linnæus.

deals mostly with the exchange of information about systems of natural history. When Pennant and Pallas had met, they had bonded over their shared esteem for the system of John Ray (1627-1705) and Pennant "proposed his [Pallas] undertaking a history of quadrupeds on the system of our illustrious countryman [Ray] a little reformed".[401] This first letter shows that Pallas had agreed with Pennant's proposal and in it he outlined a plan for elaborating Ray's system. He mentioned the cabinets and collections whose quadrupeds he would study, then discussed his plans "to dispose the Genera in a natural order", to give a full list of synonyms for all species, to provide information on the anatomy and economy of each species, and to correct the errors of earlier authors.[402] Above all, Pallas would emphasise system, and especially the system of Ray whom he called "the Father of Zoology, whose ideas about method and general subdivision where [sic] much sounder and nearer to nature, than those of modern Systematicks, and of those that affect to scorn all method in nat. History".[403] This interest in a 'natural' system and in Ray's taxonomy, along with the dismissal of Buffon's anti-system stance, is a recurrent theme throughout all of Pallas's letters to Pennant.

Pennant and Linnæus, as might be expected, also exchanged information about taxonomy and classification, as well as more general information about animals, plants and minerals. This information could take the form of lists of names of species, genera, orders and classes, sketches, engravings or descriptions. Much of it was particular to the location of each man; for example, when Pennant heard from a mutual acquaintance that Linnæus had made an error in his description of the British grouse he immediately sent him information and a list of references about it – illustrating again why it was so important to have contacts in a wide variety of places.[404] It might have been this kind of exchange that Pennant was referring to when he expressed his happiness at the "practical" nature of these letters.[405] These communications of information about natural history appear to have been the means by which they hoped to exert most influence on each other's works; for example, Pennant asked Linnæus to delay the publication of his

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401 Pennant [1793].
402 Pallas to Pennant Letter I, 10 – in Urness [1967].
403 Ibid. 11.
404 Pennant to Linnæus letter L.4924, the Linnean Society Archives.
405 Pennant to Linnæus letter L.2750, the Linnean Society Archives.

new system until he (Pennant) had sent more books and information from Britain. He wrote:

> I again write to beg you [to] postpone the publication of y^r^ system till you see my works. I am vain enough to think you will glean something out of them. Besides I will add m.s. notes to the copy of the Br[itish] Zoology for by y^r^ remarks in y^r^ last letter you oblige me to <u>alter</u> some of my numbers in the edition that is to go to the press.[406]

Not only did Pennant wish to affect Linnæus's work, he also freely admitted how Linnæus's views and comments had shaped his own. Return letters from Linnæus indicate that he too was keen to incorporate some of Pennant's work into his own: in a letter of 1763, Linnæus urged Pennant to send him some of his work before he began working on his new edition of *Systema naturæ* so that he could mention Pennant's illustrations and synonyms.[407]

Another theoretical aspect of natural history that is discussed in the letters of Pallas is the concept of species, and variation within species. This was a major question in eighteenth-century natural history: how were species to be defined; how were they related; were they completely distinct or did they sometimes overlap? In 1778, Pallas wrote to Pennant that he was collecting material for a treatise he wished to write on "varieties & changes of Species".[408] He especially wished to have information about species and varieties in different locations so he asked Pennant to tell him about "local varieties of Catle [sic], Sheep, Goats or Horses, about producing bastard animals, or mules, as well Quadrupeds as Birds, & mending one breed by another".[409] Pallas made a particular point of requesting information about British observations, as Pennant was one of his chief British-based correspondents. Sometimes, this extended from the more mundane cows, sheep, goats and horses into more unusual territory, as when Pallas asked Pennant to inform him about "an experiment having lately been made at London of coupling an Orang-outang with a common prostitute".[410] This story, which Pallas had heard of through Zimmermann's German edition of *Geographica zoologica*, shows how important personal connections were for naturalists in different countries.

[406] Pennant to Linnæus letter L.4783, the Linnean Society Archives.
[407] Linnæus to Pennant letter L.3289, linnaeus.c18.net.
[408] Pallas to Pennant Letter V, 29 – in Urness [1967].
[409] Ibid.
[410] Pallas to Pennant Letter XI, 128 – in Urness [1967].

Zimmermann's account appeared to be highly sensationalized but Pallas knew that information from a personal friend, and well respected naturalist, like Pennant was likely to be reliable. Information conveyed through publications was not always as useful as information conveyed through private channels.

Not only was this privately exchanged information often more reliable than published accounts of overseas happenings, it often arrived more promptly. It was common for naturalists to share unpublished work with each other; this served both as a means of proof-reading and fact-checking, as well as a sign of goodwill and openness. There are many instances in the Pennant/Pallas correspondence of Pallas sharing yet-to-be published findings with Pennant, and indications that Pennant was doing the same for Pallas. Even before the publication of material gathered during his six-year expedition through Russia and Siberia, Pallas happily acquiesced to Pennant's request of "having a full List of our Quadrupeds & Birds, tho' this is anticipating upon my Fauna of the Russian Empire"; in a 1779 letter, Pallas included a detailed list of over 100 Russian quadrupeds with notes about their habitats, a few days later he sent a corresponding list of Russian birds.[411] Without access to this kind of information, Pennant would have been unable to write his two-volume *Arctic zoology* of 1784-5. This work was one of Pennant's most important books; it was translated into several European languages and contributed greatly to his international reputation.

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Image of a moose from Pennant's *Arctic zoology*. Pennant depended on Pallas's information about this arctic inhabitant for publication. Image

[411] Pallas to Pennant Letter VII, 51, 56-95; Pallas to Pennant Letter VIII – both in Urness [1967].
[412] This particular image comes from an etching Peter Mazell made of a painting by George Stubbs. It features a Canadian (rather than Russian or Scandinavian) moose. For more on this image see Rolfe [1983a, 1983b].

As well as sharing unpublished information, Pennant also exchanged large quantities of printed materials with his European correspondents. Maps were a frequently-exchanged item: Pallas would send Russian maps to Pennant and "beg of [him] in return some English Books".[413] Books were almost always included in their parcels to each other. This allows us to see one significant way in which natural history publications moved around Europe; to see which books were available or unavailable in particular regions and countries; and to see how local naturalists received works from abroad. Pallas's lists of which books were already in his possession and which he required to be sent from Britain are extremely useful in determining how natural history books travelled in the eighteenth century. In a 1777 letter, Pallas listed which of Pennant's books he had been able to acquire in St. Petersburg:

> Of your publications I have the 1st & 2d Tour in Scotland & to the Hebrides, but did not receive and 3d volume, you mention. I have also your elegant synopsis of quadrupeds, which does very great honour to your Zoological Knowledge & judgment, & which I consult very often. ... Your Genera of Birds & Indian Zoology I could never procure, nor do I hear of the latter's being continued above the first set, which I saw in a Nobleman's Library here. I also saw the 8vo Edition of Your British Zoology & prefer it to the atlantik Edition, therefore desired last Summer a merchant (Mr. Kesley) that went to London, to procure it for me & hope I shall receive it in Spring. Besides these, & Drury's Illustrations, Hawkesworth's & Phipp's Voyages, I never saw any of your new English productions in our way.[414]

In the same letter, Pallas promised to send Pennant a copy of his three-volume work *Reise durch verschiedene Provinzen des Russischen Reichs*. Almost every surviving letter in the correspondence contains references to books sent or received by one or other party. Each regularly sent their own work as gifts, but they would also specifically request the purchase of other books. In 1779, for example, Pallas requested five works from England: Priestley's *Disquisitions on Matter and Spirit*; Forrest's *Voyage to New Guinea and the Moluccas*; Turner's *A view of the earth as far as it was known to the ancients*; Hunter's *Disputatio exponens quaedam de hominum varietatibus*; and Fize-Palmer's *Dissertatio de jaenia*. Payment for these books was sent via the English consul general in St. Petersburg, Walter Shairp, or via Mr. Porter (a merchant with W. Porter & Co.) through "any Russian Mercht in

[413] Pallas to Pennant Letter V, 28 – in Urness [1967].
[414] Pallas to Pennant Letter II, 16 – in Urness [1967].

London".[415] In return for these English books, Pallas would send Russian works such as the Commentaries of the St. Petersburg Academy of Sciences. There remains evidence of how closely Pallas scrutinized these treasured foreign books: in a 1778 letter he attached a list of corrections and criticisms of two overseas publications: Pennant's *Synopsis of quadrupeds* and Johann Reinhold Forster's (1729-1798) *Specimen historicae naturalis Volgensis*. For both works, he gave page-by-page criticisms which show how carefully he read these foreign volumes.

Plates of the bat *vespertilio caphalates* from Pallas's 1767 *Spicilegia zoologica* – a book owned by Pennant. Images courtesy of Cambridge University Library.

Pennant and Linnæus also frequently exchanged books and information about books. As with the letters of Pennant and Pallas, the correspondence of Pennant and Linnæus tells us about what books were available in different parts of Europe, and which books were most desirable. We see how quickly works by a celebrated figure such as Linnæus travelled from Sweden to Britain when Pennant, in a letter of February 1760, assured Linnæus that the 1758 edition of *Systema naturæ* had "long since reached my Lands", and in a second letter written that same month, Pennant included a list of books by Linnæus that were in his possession. These included two editions of *Systema naturæ*,

[415] Pallas to Pennant Letter VII, 52 – in Urness [1967].

major works such as *Hortus Cliffortianus*, *Genera*, *Classes*, and *Species plantarum* and less common works such as Linnæus's edition of Petrus Artedi's *Ichthyologia*.[416] Later that year, Pennant sent Linnæus a list of all the natural history books in his library in case Linnæus needed to refer to any of them.[417] The works of less well known European natural history authors were not as likely to travel to Britain as those by Linnæus, unless they were specifically required. Around 1760, Pennant began to become interested in entomology; in order to satisfy his "rising passion" for this subject, he began collecting insects and gathering books. But since many of the works he desired were foreign, they were not easily available in Britain and so he had to send abroad before he could read the German works of August Johann Rösel von Rosenhof (1705-1759), and those of Charles de Geer (1720-1788), a Swede.[418]

Related to the issue of moving natural history books around Europe was the question of translating them. Both Pennant and Pallas were keen to see each other's work in translation. In 1780, Pallas wrote to Pennant: "Your History pleases me so absolutely, that I should like to see it translated in French and German, & if your Printer would give a good bargain of the Plates, after the Original Edition is done, I believe I could get Booksellers to undertake such translations".[419] A year later, Pallas wrote to inform Pennant that Forster was preparing a German edition of Pennant's *Indian zoology*.[420] Pennant was also facilitating the spread of the works of Pallas and other German-language authors in English; Pallas often sent notes and maps from his journey which he encouraged Pennant to translate and publish, and Pennant expressed an interest in translating the German entomologist Miller's books on entomology into English.[421] These kinds of personal links were vital for naturalists who wished to disseminate their work overseas. It was far easier to be translated, published, read, cited and reviewed abroad if one had friends already in situ to help at each stage of the process and send encouragement.

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416 Pennant to Linnæus letters L.5263 and L.2675, the Linnean Society Archives.
417 Pennant to Linnæus letter L.2750, the Linnean Society Archives. Unfortunately, part of this letter appears to be missing – but the section of the list that survives lists approximately 40 books dealing with a broad range of topics such as fossils, plants, insects, travels, mineralogy and ichthyology.
418 Pennant to Linnæus letter L.2750, the Linnean Society Archives.
419 Pallas to Pennant Letter XIV, 140 – in Urness [1967].
420 Pallas to Pennant Letter XVI, 153 – in Urness [1967].
421 Pallas to Pennant Letter IX, 108-9 – in Urness [1967]. Pennant to Linnæus letter L.2750, the Linnean Society Archives.

As well as sending actual books and discussing translations, Pennant and Linnæus updated each other on developments in natural history publishing in their respective countries; and shared information about which naturalists were working on which topics, and what and when they would be publishing.[422] Pennant was particularly anxious that Linnæus should keep abreast of new British works involving classification. Several times Pennant asked Linnæus to defer publication of his updated system until Pennant could send him some new British books on the topic – these included books by Emanuel Mendez da Costa (1717-1791), Edwards and Pennant himself.[423] By this, it is clear that Pennant felt that British naturalists had much to offer their European counterparts when it came to systems of natural history and he was keen to ensure that Linnæus, the greatest of all systematists, knew their work and incorporated some elements of it into his own.

In addition to exchanging books (and information about books) as a reasonably straightforward way of sharing natural historical knowledge and of communicating ideas from geographical location to another, there were other reasons to swap books. They acted as signs of esteem and affection. They were also physical symbols of the relationships between individuals. In one of his later letters, Pennant (not really in need of any particular books for his work) asked Linnæus to send something: "I beg (more for the honor of the gift than anything else) any of your works which you can spare without much expence being [?] that posterity should find in my Library some marks of respect from Linnæus to his friend".[424]

For Pennant and Linnæus, book exchange was closely linked to specimen exchange, with Pennant writing, for example, that "I shall think myself amply repayed for the large collection of minerals I sent you, by a present of Books printed in your locality".[425] Pennant was quite clear about the fact that Linnæus owed him some kind of recompense for the mineral samples – they were not a gift; he continued, "I cannot resign my clame [sic.] to some Return for the valuable collection I had the Honor of sending you. Any of your works (except those I annex) will be most acceptable: as will be the Amœn. Acad. After them, any of these you think I merit – <u>Halmii Itinera</u>,

[422] Pennant to Linnæus letter L.5263, the Linnean Society Archives.
[423] Pennant to Linnæus letters L.2750, L.4868 and L.4783, the Linnean Society Archives. Edwards is possibly George Edwards (1694-1773).
[424] Pennant to Linnæus letter L.4888, the Linnean Society Archives.
[425] Pennant to Linnæus letter L.5263, the Linnean Society Archives.

Hasselquistii et Osbeikii Itinera, Mus.Fr.Ad., Mus.Tessin."[426] Unhappy with the slowness of Linnæus's response, Pennant wrote again that month. This letter was ostensibly one of introduction for the Russian Baron de Demidoff and a Swiss gentleman named Valtravers who had just visited Pennant and were then travelling to Sweden. But Pennant also used it to point out that Linnæus had so far failed to repay him with specimens or books; Pennant explained that Demidoff and Valtravers had viewed his cabinet and that he was

> very unhappy in not having an opportunity of priding myself in showing them the favours I long expected from your cabinet ... you may have been so fully employed in the pursuit of your studies, as not to have leisure to make me a similar return to the Present I sent. Permit me then to beg a return in another kind that of Books; any of your much esteemed works will be chiefly acceptable; after those, any of those you think my gift of minerals deserve will be truely [sic.] acceptable.[427]

Pennant's hints seem to have paid off and eventually Linnæus did return Pennant's favours; unfortunately, details of what Linnæus sent have not survived, only the fact that it was so precious to Pennant that it was "placed among the most valuable ornaments of [his] family".[428]

Specimen exchange was much more central to Pennant's relationship with Pallas than it was in his dealings with Linnæus. The difficulty of moving things between Russia and north Wales meant that their exchanges had to be carried out as efficiently as possible. It was quite standard among naturalists to place higher value on specimens of particular rarity, or that were especially difficult to obtain; some collectors also valued beauty in specimens but this was not the case with Pennant and Pallas. It was standard too to exchange small numbers of rare specimens for large numbers of more common ones. But whereas naturalists within the same country could easily see each others' specimens and come to casual arrangements about lending or swapping specimens, those far apart had to have a more formal system. So Pallas and Pennant, having catalogued their own collections, drew up lists of the specimens they wished to acquire and sent these to each other. Pallas, at the beginning of his exchanges with Pennant, wishing to clarify his terms, wrote:

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426 Ibid.
427 Pennant to Linnæus letter L.2675, the Linnean Society Archives.
428 Pennant to Linnæus letter L.4888, the Linnean Society Archives.

> Of natural Curiosities from Siberia I can still exchange a good number of Birds & quadrupeds, elegantly stuffed, Insects, Ores & Plants. But these things have cost me so much trouble to collect & to carry some thousand miles by Land, that any reasonable Man will conceive the impossibility of exchanging them for trifling or common things. If therefore you should desire any such things for your Friends or own satisfaction, the best way will be to send a List of the things your Friends can furnish (provided they may be in perfect condition), & another of what they might choose out of the specification here enclosed.[429]

Many naturalists used this kind of system when swapping specimens but others were more casual in their dealings. Certainly within a given country, where naturalists were personally acquainted and in more frequent contact, specimen exchange was a less formal process. But when dealing with distant correspondents one had to exercise more caution, as Pallas had discovered to his cost. He complained to Pennant that "I have lost several sets of Curiosities by too much compliance & relying upon the honour & sincerity of Correspondents, which makes me so cautious. By dealing in the abovementioned way, troubles & expences will also be saved".[430] Along with these suggestions for how they should carry out their exchanges, Pallas included two lists: one was headed "Wanted" and the other, "Catalogue of Siberian Curiosities, that could be exchanged for others". These lists show that Pallas was interested in acquiring fauna and flora from the East and West Indies, the Americas, the South Sea Islands and the Arctic regions; they also show his particular interest in gathering specimens of zoophytes and metal ores. In return, he could supply at least 10 kinds of Siberian quadruped (including sables, polecats, rabbits, jerboas and flying squirrels), 55 species of Siberian bird, almost any native Siberian plant, several ores and minerals, and assorted Russian zoophytes.[431]

Once naturalists had negotiated which specimens would be given, and which received in return, they then had to arrange their transportation. For Pallas and Pennant this was no trivial matter. Shipping chests of precious, delicate and often irreplaceable specimens from Russia to Britain was a complicated business, as can be seen from remarks made in their letters. In one instance, Pallas described how he engaged the merchant Mr. Porter to ship a chest containing books and maps for Pennant and "about a Dozen Animals & Birds in fine preservation & a Box of Russian ores" for the English

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429 Pallas to Pennant Letter II, 17 – in Urness [1967].
430 Ibid.
431 Ibid. 18-20.

collector Anna Blackburn (1740-1793). This was to be brought by a brig to Blackburn's brother in Liverpool but Porter, by mistake, sent a different trunk on the brig and so had to send Pallas's chest to London with a trader. In London, the chest was delivered to Benjamin White's at Fleet Street. White would then have to repack the chest, "as the things were not packed for Land carriage", and forward the papers to Pennant and the specimens to Blackburn.[432] Shipments could only be sent infrequently between Pennant and Pallas. There were several reasons for this: the harshness of Russian winters caused the port at St. Petersburg to freeze annually; Pennant's nearest major port was Liverpool, and there were "only two or three Ships every year from that Port to Petersburgh"; and Pallas had to protect his specimens from pests by storing them in difficult-to-open, airtight boxes. He explained this to his friend: "The flying Squirrell [sic] I could send you, as I have still four or five Specimens; But am at a loss how to get it out of a large Chest, all tarred over, which I cannot venture to open now at the approach of Spring, without exposing all the Birds and animals in it, to vermine [sic]".[433] So Pallas's collections could only safely be accessed in the winter – the one time when he was unable to send parcels abroad.

"Bird catching at Orkney" from Pennant's *Arctic zoology* which shows some of the difficulties of procuring specimens of arctic wildlife and also highlights Pennant's desire to promote British natural history. By including northern Scotland in a book on the arctic, Pennant was trying to promote international interest in Britain's wildlife. Image courtesy of Cambridge University

[432] Pallas to Pennant Letter IV, 23-4 – in Urness [1967].
[433] Pallas to Pennant Letter VII, 53 – in Urness [1967].

Even though naturalists went to enormous amounts of trouble to exchange specimens, the results were not always satisfactory. As well as the problems of specimens being lost or damaged, there was also the problem of poor quality specimens being exchanged. Pallas and Pennant always seemed happy with the objects received from each other, but this was not necessarily the case with those received from other naturalists. In one letter, Pallas complained to Pennant that Lord Archibald Hope (1735-1794), "after a delay of 2½ Years, has at last sent me a parcell [sic] of wretched, dirty Spars & Cristals [sic], not worth the Customhouse expences they occasioned".[434] He contrasted this kind of exchange with those received from more reliable sources such as Anna Blackburn who sent parcels of specimens "which, tho' few in number, [were] very well chosen and extremely acceptable".[435] Pallas attributed the poor quality of Hope's specimens partly to his aristocratic status; he frequently bemoaned his dealings with the upper classes and cautioned against natural historical exchange with them. When the Duchess of Portland (1715-1785) requested some samples from him, Pallas wrote to Pennant,

> I am very ready to obey Her Grace's commands, & shall use my best endeavours to make up a fine parcel next year. But I am much afraid to deal with these great folks. ... Mr. Greville has not so much as answered to a Parcell [sic] of more than 100 Siberian specimens I transmitted to him last Spring. I could name a couple more of other Nations.[436]

Almost a year later, still awaiting a parcel from Greville, Pallas declared,

> Those honourable Gentlemen I will never meddle with any more ... It is the common fate of the poor, to be neglected by the opulent. If I could but get some tolerable specimens of Cornish Tin-ores, Cockles & black Granitoes, I would never trouble my head about Mr. Greville nor any Lord no more.[437]

But despite these unfortunate experiences of specimen exchange, Pallas persevered. To his trusted British friend Pennant, he continued to send lists of *desiderata* and parcels of strange Siberian creatures. His *desiderata* included large numbers of British geological and mineralogical items as well as zoological and botanical ones.[438] Pallas was also keen to exploit Britain's extensive trade network and asked Pennant if he could use his contacts throughout the British Empire to procure specimens: "As you are certainly

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[434] Pallas to Pennant Letter XII, 134 – in Urness [1967].
[435] Pallas to Pennant Letter XIV, 140 – in Urness [1967].
[436] Pallas to Pennant Letter IX, 106 – in Urness [1967]. This refers to Charles Francis Greville (1749-1809)
[437] Pallas to Pennant Letter XII, 134 – in Urness [1967].
[438] Pallas to Pennant Letter XIV, 142-7 – in Urness [1967].

acquainted with many Captains & Gentlemen travelling to the West & East-Indies, to America, & other distant parts of the World, it would be an easy matter to get specimens of the more curious or even the common rocks, pebbles & minerals, also volcanic productions, from those quarters".[439] Pallas was also eager to acquire more mundane items from Britain. These were things that did not require Pennant or other gentlemen natural historians to venture into the field, but rather could "be procured from the Druggist's Shops in London".[440] A list of items that Pallas desired included: fuller's earth; red stonecolour from the East Indies; coloured earths from England, Spain or India; emry and lead from Spain and Guernsey; tar or petroleum from Barbados; lapis hibernicus; borax; and lapis armenus.[441]

The correspondence between Pennant and Pallas, and between Pennant and Linnæus was important to all three men, but it was not only their only contact with naturalists abroad. Pallas was in touch with several British naturalists and scientific organisations (as well, of course, as being in contact with naturalists from the German lands and many other parts of Europe). In addition to sending specimens to collectors like Anna Blackburn and the Duchess of Portland, Pallas also engaged in correspondence with English scientific figures such as Sir Joseph Banks and Dru Drury, and Swedish-born, English-based naturalists such as Jonas Carlsson Dryander and Daniel Solander. Pallas was also a member of the Royal Society of London, and published work in its *Philosophical Transactions*. Linnæus, as well as corresponding with scores of naturalists around Europe, dispatched his students to distant corners of the globe in a bid to extend the reach of his knowledge.[442] Pennant too had many contacts around Europe; these included notable figures such as Albrecht von Haller and a particularly large number of correspondents in France, the most prolific of whom seems to have been Mathurin Jacques Brisson (1723-1806). Pennant was also a member of at least one foreign scientific society – the American Philosophical Society.[443] In these exchanges of Pennant's with naturalists all over the world, we see echoes of his exchanges with Pallas and Linnæus. The same kinds of thing – information, printed material and natural history objects –

[439] Pallas to Pennant Letter XVI, 151-2 – in Urness [1967].
[440] Pallas to Pennant Letter XIV, 141 – in Urness [1967].
[441] Ibid, 143-4.
[442] For more on Linnæus's networks, see Stafleu [1971].
[443] In a letter, Pallas offered to campaign for Pennant's admission to the St. Petersburg Academy of Sciences.

were being sent and received. With Brisson he exchanged books, engravings and mineralogical samples; with Buffon and Daubenton he exchanged zoological specimens. Through Pennant's writings, we also see how European natural history knowledge moved within Britain. Because European works were not always readily available in Britain, those naturalists fortunate enough to own any were often called upon to share their contents. When preparing his *Arctic Zoology*, Pennant asked Dryander to search through his collection of European books for information on particular questions relating to falcons, Dryander willingly complied.[444] Likewise, when William Smellie was writing a new natural history of animals, he requested Pennant's help. Smellie explained that

> in our progress, we have met with one great difficulty, which you are enabled to remove. In your numerous additions to Linnaeus, you often quote Schreber. The work of this author is not to be had in Scotland. We most anxiously wish to learn where it is to be procured and what is the value of it. If not to be had in London, might we presume to ask the perusal of your copy, till another be imported for you from the Continent?[445]

All of these links between naturalists from different nations show the importance of exchanges to the development of natural history. Pennant's links abroad especially show how things (both concrete and abstract) flowed into England and Wales and shaped its natural history, and how things sent from England and Wales to mainland Europe, Scandinavia and Russia affected natural history there. Without having travelled further north than Scotland, Pennant managed to become an expert on arctic zoology – this was made possible by his access to the specimens Pallas had collected in Siberia, and his ability to procure rare books and specialist information from his contacts overseas. The two-way flow of information also ensured that elements of British natural history appeared in continental works; for example, Pallas's early use of the classification systems of Ray was partly due to Pennant's nationalistic promotion of the work of his "countryman". Likewise, Pennant hoped that British work on classification would influence Linnæus to alter or amend some of his writings on the topic, and Linnæus's response shows how willing he was to consider British works; as Linnæus was the most celebrated taxonomist of this period, this was a significant achievement for Pennant and British natural history.

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[444] Dryander to Pennant Letter CR2017/TP217, Warwickshire County Archives.
[445] Smellie to Pennant Letter CR2017/TP362.1, Warwickshire County Archives.

Making a mountain out of a molehill

Pennant's amicable relationships with some European naturalists were not necessarily representative of all his dealings with overseas colleagues. While naturalists were generally given to cooperation, occasionally they found themselves embroiled in disputes. These might be minor tussles over species names or particular classifications, larger clashes stemming from the protagonists having fundamentally different views about the purposes and methods of natural history, or, sometimes, disputes arising from deeper causes such as religion or nationalism. In this section I will examine Thomas Pennant's relationship with Georges-Louis Leclerc, Comte de Buffon. This will highlight some of the ways in which British and French naturalists considered each other's work. The relationship was not always an amicable one, and to show some of the tensions and rivalries that existed between naturalists, I wish to examine particularly a dispute between the two that dominated many of their later interactions.

National, political and military rivalry between eighteenth-century Britain and France was oftentimes accompanied by intellectual rivalry and perhaps it was this that coloured the relationship between Pennant and Buffon. Their association appeared to begin on friendly enough terms: in 1765 Pennant began a tour of Europe and met Buffon in Paris. There, wrote Pennant later,

> [I was] made happy in the company of the celebrated naturalist *Le Comte de Buffon*, with whom I passed much of the time. He was satisfied with my proficiency in natural history, and publickly acknowleged [sic.] his favourable sentiments of my studies in the fifteenth volume of his *Histoire Naturelle*.[446]

But this easy friendship did not last long; the following year, in the preface to his 1766 *British zoology*, Pennant lamented the "unwearied diligence of our rivals the *French*" and grudgingly admitted that "if envy would permit" he would acknowledge that Buffon held "the first place among the modern zoologists".[447] Although the two naturalists cited each other reasonably frequently and held each other's work in high regard, they were prone to professional disagreements. The disagreements were of two kinds: more general ones relating to big questions about order in nature; and more specific ones relating to the

[446] Pennant [1793] 4.
[447] Pennant [1768] xi, xxiii.

anatomy, characteristics and modes of life of individual species. Of the more specific disputes, the mole dispute was perhaps the bitterest and longest-lived.

The mole dispute began with a reference in Pennant's *British zoology*. Pennant, in the introduction to this work, had stated that he would primarily use the classification system of the English naturalist John Ray to group the subjects of the book. So it was not surprising to find Ray cited in Pennant's section on the garden mole. Nor was it terribly surprising to find Ray's work compared favourably to that of Buffon. Pennant described the mole and then continued:

> thus amply supplied as it is, with every necessary accommodation of life; we must avoid assenting to an observation of *M. De Buffon*, and only refer the reader to the note, where he may find the very words of that author; and compare them with those of our illustrious countryman, Mr. Ray.[448]

Illustration of a mole from a French edition of Buffon's *Histoire naturelle.* Image courtesy of Cambridge University

In a footnote, Pennant included quotations from Buffon and Ray on the mole. Buffon's offending comment said that moles, living in the dark underground, had been given a sixth sense by nature to compensate for their lack of sight.[449] Ray did not refer to any such extra sense. This seemingly trivial remark of Pennant's caught Buffon's attention and in his *Histoire naturelle des oiseaux* (1770-1783) Buffon voiced his displeasure at Pennant's comment.[450] More than ten years later, this spat had still not been forgotten and Pennant's old friend Pallas had to admonish him, saying that "indeed you have been also, give me leave to tell you, been [sic.] a little too concise and serious in reproving Buffon & others in your Synopsis".[451] In the *Synopsis of quadrupeds*, Pennant had attacked

[448] Ibid. 110-1.
[449] Buffon's comment: "La taupe sans être aveugle, a les yeux si petits, si couverts, qu'elle ne peut faire grand usage du sens de la vûe: en dedommagement la nature lui a donné avec magnificence l'usage du sixieme sens...".
[450] Buffon [1770].
[451] Pallas to Pennant Letter VII, 51 - in Urness [1967].

Buffon for "the reflections he often casts on other Writers; the creation of his own gay fancy".[452] Twenty years after the initial comment, the incident was still fresh in Pennant's mind. In the opening pages of an index that Pennant had prepared of Buffon's *Ornithologie* and *Planches enluminées*, Pennant referred to Buffon as his "quondam friend" and recalled how,

> My remarks on a singular observation on the anatomy of the *Mole*, many years before I had the honor [sic.] of his acquaintance, was the irritating cause of his late resentment against me: but possibly the public will think ... that he has pursued me with too much acrimony.[453]

Even 25 years after the publication of *British zoology*, in his memoir *Literary life*, Pennant could still describe how he had

> made a comparison between the free-thinking philosopher and our great and religious countryman Mr. Ray, much to the advantage of the latter. The subject was a Mole, really too ridiculous to have been noticed; but such was his irritability, that, in the first volume of his *Histoire Naturelle des Oiseaux*, he fell on me most unmercifully, but happily often without reason.[454]

But this animosity co-existed with a certain level of mutual respect. In *Literary life*, Pennant continued his reminiscences about Buffon and the argument: "He probably relented, for in the following volumes he frequently made use of my authority, which fully atoned for a hasty and misguided fit of passion. I did not wish to quarrel with a gentleman I truly esteemed. ... Our blows were light, and I hope that neither of us felt any material injury".[455] Likewise, in the index to Buffon's *Ornithologie*, Pennant called Buffon "one of the most celebrated and illustrious writers in natural history which this age has produced ... a gentleman of first-rate abilities, great acquired knowledge, and of an eloquence which dazzles, delights and oftentimes instructs".[456] But the very fact that Pennant wrote this index was a double-edged sword: on the one hand it showed how much Pennant admired Buffon's work; on the other it was a kind of snub to Buffon's deliberate rejection of order. Pennant also seemed gleeful at the prospect of correcting the Count's errors; he wrote that "the dulness [sic.] of index-making has been a little

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452 Pennant [1771] vii-viii.
453 Pennant [1786] vi-vii. According to Pennant, he first met Buffon in 1765; *British Zoology* was published in 1766. But this comment seems to imply that the two did not meet until after the publication of *British Zoology*, or that Pennant delayed publishing the book for some years after writing it.
454 Pennant [1793] 4-5.
455 Ibid. 5.
456 Pennant [1786] iii.

abated by a few notes, which I have flung in, relative to the misconceptions, or misinformations, of ... Buffon".[457] So we see that the working relationship between Pennant and Buffon was a complex one. And the mole dispute which Pennant truthfully described as "ridiculous" was merely a focal point of a more complicated set of problems.

One of the central questions on which Pennant and Buffon differed was that of classification. Famously, in the *"Premier discours"* of his *Histoire naturelle*, Buffon eschewed the very idea of ordering nature. He attacked Linnæus and the systematists for their naïve belief that nature could be neatly subdivided into logical groupings. Pennant, on the other hand, was a firm believer in classification. In his works he used elements borrowed from the taxonomies of Ray, Linnæus and Brisson to classify species. The prefaces of works such as *Genera of birds* and *British zoology* show an unwavering dedication to the basic concept of ordering nature; in *Genera of Birds* particularly, Pennant discussed progress in ornithology as a function of improved taxonomic systems and praised Ray and his associate Francis Willughby (1635-1672) because "they made every species occupy their proper place".[458] This key ideological difference between these two naturalists created constant friction and resulted in occasional outbursts such as those outlined above. In the advertisement for the *Indexes to the Ornithologie of the Comte de Buffon*, Pennant was explicit about his views on Buffon's lack of method: "An immethodical author, says Mr. *Addison*, is like a duck, which dives and rises in places where you lest [sic.] expect its appearance. This simile may be very aptly applied to [Buffon]."[459] Pennant continued,

> Unfortunately, a contempt of system, and systematic writers, has taken full possession of [Buffon]. He flutters along the stream, and gracefully displays the elegancy of his plumage; and, having favoured us, as much as he thinks sufficient, with the pleasing spectacle, immerses and disappears, without leaving to common observers the lest power of guessing at the spot where he means to emerge to day.[460]

But, happily, Pennant, "by long and congenial study ... at last attained a knowledge of his ways", and he would clarify Buffon's meaning by distilling some of his natural historical works into a *methodical* index. He used the same system that he had outlined earlier in

[457] Ibid. vi.
[458] Pennant [1781] xx.
[459] Pennant [1786] iii.
[460] Ibid.

Genera of birds which divided birds into terrestrial and aquatic and subdivided these into orders based on characteristics such as feet.[461] In *Synopsis of quadrupeds* Pennant similarly criticised Buffon who "unfortunately seems to think it beneath him to shackle his lively spirit with systematic arrangement; so that the Reader is forced to wander thro' numbers of volumes in search of any wished-for subject".[462] Pennant was essentially claiming that Buffon's work could only be intelligible and useful if it were forced into a framework which was fundamentally antithetical to Buffon's ideology. By claiming this, Pennant was exposing some of the problems of their relationship. It was not just Pennant who resisted Buffon's ideas on classification; few, if any, British naturalists followed Buffon's lead and even in France his ideas on taxonomy were controversial.

There were several other reasons for friction between Pennant and Buffon. I have already mentioned national rivalry, and certainly Pennant was always quick to emphasise that he used the system of "*our countryman*", Ray.[463] In the preface to *Genera of birds*, he somewhat half-heartedly hoped that he would "not be accused of national partiality, in giving preference to that [system] composed by Mr Ray".[464] Pennant used parts of foreign systems in his works (notably those of Brisson and Linnæus), but generally only where Ray's work was lacking or needed updating. Another cause of friction may have been religion: when discussing the mole dispute in *Literary life*, Pennant pointedly termed Ray "religious" while Buffon was given the epitaph "free-thinking". In Britain, much natural history was undertaken within a natural theological context and Pennant's work was no exception.[465] Without this context, the purpose of Pennant's natural history would have been very different, and it is likely that he found Buffon's seemingly irreligious or deist approach to natural history problematic. Buffon's religious views are not particularly well documented; even Roger's biography of him mentions them only in passing. One early letter quoted by Roger shows Buffon's firm belief in the material nature of the soul, a later comment says that Buffon was quick "to underline his religious orthodoxy", but little further detail is given.[466] Regardless of the particulars of his beliefs,

[461] Pennant [1781] xix-xxii.
[462] Pennant [1771] viii.
[463] Pennant [1768] xiii, Pennant [1793] 4.
[464] Pennant [1781] xix.
[465] Pennant [1768] ii.
[466] Roger [1997] 43, 322.

Pennant may have associated Buffon's work with French materialism and been wary of its implications.

So we see that Pennant's relations with European naturalists were not always as straightforwardly cordial as his correspondence may lead us to believe. While the dispute with Buffon appeared to be about the number of senses garden moles possessed, it operated at a number of different levels: national rivalry, intellectual rivalry, and differing views on classification, on the purposes and limits of natural history, and on the role of religion in natural history were all factors in the discord that existed between Pennant and Buffon. In some ways, this dispute is quite representative of the relationship between many British naturalists and their overseas counterparts. French naturalists such as Buffon who eschewed the idea of ordering nature were looked on with suspicion in Britain; in contrast, naturalists who predominantly followed the teachings of Linnæus and believed strongly in classifying the natural world were often popular among British naturalists, particularly later in the eighteenth century when the Linnean system was gaining in popularity. This case study also shows that natural history was not a simple exercise in gathering facts which were independent of the naturalists doing the gathering. Although many naturalists were happy to cite Buffon's descriptions in their work, they sought to distance themselves from a character that they viewed as a controversial, atheistic and anti-system Frenchman by disputing the more philosophical or speculative parts of his work.

Buffon in Britain

Here, I expand upon some of the ideas raised in the previous section by looking at a larger question – how was Buffon's magnum opus, the *Histoire naturelle*, received in Britain? Tracing the spread of published works and their translations is a useful way of seeing how natural historical knowledge moves across national boundaries, of seeing how that knowledge is changed or interpreted as it travels, and of seeing how local naturalists respond to both the information and its author. The first part of *Histoire naturelle* was published in 15 volumes between 1749 and 1767; the first English translation was published in 1775 by William Kenrick (c.1729-1779) and John Murdoch (1747-1824), but even before this translation appeared many British naturalists were reading and using the original French text.[467] Many of these British naturalists used Buffon's work differently from how he had intended: it was common to use his natural historical descriptions, but to ignore his theoretical framework. British naturalists liked to classify and group the creatures and things that they studied, and they found it difficult to reconcile themselves to Buffon's system-free natural history. Here I will examine some parts of Buffon's original texts and compare them to English translations and interpretations to see how Buffon's ideas were changed when they moved out of France and into Britain.

There were three principal English translations of the *Histoire naturelle* in the eighteenth century, and one particularly important interpretation. The three translations were: Kenrick and Murdoch's of 1775 which was composed of six volumes and had only one edition; William Smellie's of 1780, this was in eight volumes and went through four editions; and James Smith Barr's (1769-1806) 1792 translation in 10 volumes, which also had four editions. The most popular interpretation of *Histoire naturelle* was Oliver Goldsmith's 1774 *An history of the earth, and animated nature* which contained eight volumes and was reprinted more than 20 times in the next half-century. According to Jeff Loveland, Buffon's *Histoire naturelle* (in the form of these four books, along with two anonymous abridgments that appeared in 1791) was one of the most popular books in Britain in the late eighteenth century.[468]

Each of these versions of Buffon's book had quite different aims and translation methods. Kenrick and Murdoch were both more interested in translation and the French language than in natural history, and Smellie criticised their translation for omitting important sections, lacking plates, being deficient in natural historical knowledge and being written in a poor style.[469] This appears to have been the least popular of the translations – it was never reprinted and was rarely cited by contemporary naturalists. Smellie, who was a well known naturalist and who is said to have learned French for the express purpose of translating Buffon, produced a translation that was far more fluidly written, and more accurate in its natural history facts and language. Barr's translation

[467] Despite Buffon's fame and status, and despite the fact that his work was read in Britain (as is evinced by citations in many British books), there were very few contemporary reviews of *Histoire Naturelle* in the English language press - see Lyon & Sloan [1981] preface.
[468] Loveland [2004a] 216. See Loveland's paper for a full comparison of the style and content of these three translations.
[469] Smellie in Buffon [1785] preface.

(though there is some question about whether Barr himself did the translation[470]) was partly drawn from the other two translation, but contained fewer errors and, like Smellie's, was more fluidly written than Kenrick and Murdoch's. But though the three read quite differently, they also had many similarities: all three omitted Buffon's *"Premier discours"* in which he renounced the use of system in natural history (Kenrick and Murdoch said that they omitted it because of its *a priori* nature, Smellie because it took too strong a line against Linnæus); they all also omitted the sections that had been written by Louis-Jean-Marie Daubenton which were drier, more starkly factual, and less elegantly written than Buffon's sections; all added some footnotes. Goldsmith's book was quite a different affair – he had not set out to write a strict translation of Buffon (indeed, he had set out to translate Pliny's *Natural history* but then read Buffon and, inspired by the warmth of his writing style, altered his planned book). Though he stated in his preface that "...only availing myself of [Buffon's] information, I have been content to describe things in my own way; and though many of the materials are taken from him, yet I have added, retrenched, and altered, as I thought proper", Goldsmith actually kept quite closely to Buffon's words for large sections of his work.[471] The alterations Goldsmith made tended to be either as footnotes or inserted in the body of the text in quotation marks or brackets, so readers could easily tell which parts came directly from Buffon.

There are many different elements to the *Histoire naturelle*: there are general discussions of the theory of natural history, and specific theories about the earth; there are chapters on what it means to call something an animal or plant; there are more focussed discussions on functions in nature such as growth, nutrition and generation, and on anatomical elements such as bones or muscles; and there are detailed accounts of the anatomy, lives, habitats, and economies of animals. Each of these elements was treated differently by the different English-language translators. As mentioned above, the *"Premier discours"* and discussion of general theory within natural history were entirely omitted. Buffon's view that animals and plants could only be classified at the level of species and that higher classifications (at the level of genus, class and order) were artificial constraints imposed upon nature was controversial even in France; and so controversial in Britain that it was felt that publicizing this view would detract from Buffon's work. But

[470] Loveland [2004a] 221; Barr & Buffon [1792].
[471] Goldsmith [1774] Vol. I xi.

the more focussed theoretical discussions of, for example, theories of the earth and on the distinction between the animal and vegetable kingdoms are included in all the English translations and seem not to have been especially problematic for either their translators or readers.[472] The chapters on functions in nature also tended to remain in the translations, though sometimes shortened. Finally, the detailed descriptions of animals and their lives were most warmly received in Britain. This was not just because of the wealth of detail that Buffon included, but also because of his fluid and enthusiastic writing style. Many of the translators of the *Histoire naturelle* commented on this. Kenrick and Murdoch declared that Buffon took the first place among natural historical authors, both for his content and style:

> To *Willoughby*, *Ray*, *Klein*, *Linnaeus*, and others, the world is indebted for classical arrangements of animal, vegetable, and fossil productions. By these writers, however, the reader is presented with a dry, unentertaining theory of their characteristic peculiarities and nominal distinctions, with little regard to their instincts, habits, properties and uses ... To supply these important *desiderata*...of the animal, vegetable, and mineral kingdoms, was the arduous task of DE BUFFON; in the execution of which he is universally allowed to have so eminently succeeded.[473]

Perhaps it is partly because of Buffon's mastery of natural historical prose that his descriptions of animals are more faithfully translated than his chapters on theory; though it is probably also partly due to British naturalists' general preference for description over theorizing.

Using Buffon's article on the lion as an example, we can see how his descriptions fared when they travelled from France to Britain. Buffon's description begins with remarks on how animals are influenced by their climate and he uses this to explain why the lion, "born under the burning sun of Africa and India" is so strong and fierce. He then goes on to discuss: the lion's populations; its relationship with man; its character; place in the chain of being; classification; Aristotle's views on lions; local names for lions; their skeleton, maternal instinct, senses; what they eat and drink; their roar; their sleep and motion; how to catch one; what they taste like; and how parts of their bodies can be used in medicine.[474] Kenrick and Murdoch, Barr and Smellie all give translations of this

[472] Though some small changes are made, and some ideas questioned, most notably in Goldsmith.
[473] Kenrick & Murdoch [1775] preface.
[474] Buffon [1761] Vol. IX 1-48.

passage that stick closely to the original, the one notable difference being that Smellie adds a short description of the lion at the beginning of his translation (taken from Pennant's *Synopsis of quadrupeds*). In this description, Pennant refers to the lion as a kind of cat – a link that Buffon clearly denied, stating that "classifying...the lion with the cat...is to degrade, deface nature".[475] In his translation, Smellie put part of this line in italics. Unlike Kenrick, Murdoch, Barr and Smellie, Goldsmith had not aimed to produce a strict translation and he freely added to and altered Buffon's words. Like Smellie, he classed the lion as a kind of cat and wrote that "the structure of the paws, teeth, eyes and tongue are the same as in a cat; and also in the inward parts these two animals so nearly resemble each others, that the anatomist's chief distinction arises merely from the size".[476] His two other principal additions related to the dangers of keeping lions as pets and their methods of hunting.

These findings are in agreement with those from the previous section of this chapter: just as Pennant cited Buffon's descriptions while disagreeing with his theories and publicly entering into disputes with him, these translations and interpretations of *Histoire naturelle* seemed most comfortable with Buffon's descriptions, and so perturbed by his theories that they deliberately omitted the *"Premier discours"*. Nor were the translators and interpreters above disagreeing with Buffon over questions of classification such as whether the lion and the cat should be placed in the same taxonomic group. In this way, Buffon's work was substantially altered when it moved from France to Britain; although much of the text was translated faithfully, key ideas that were present in Buffon's *Histoire naturelle* – such as the artificiality of classification systems – were lost in translation. By comparing British translations to the French original, we learn much about what was valued by British natural historians at this time.

Conclusion

Correspondence was a key tool of the natural historian. Pennant's correspondence networks allowed him to write natural histories that could not have existed otherwise; they also allowed him to share his ideas on British classification systems with overseas

[475] Ibid.
[476] Goldsmith [1774] Vol. III 221.

naturalists and so play a role in the natural histories they wrote. But the relationships between British and European natural history were complex. Saying straightforwardly that one 'influenced' the other does not do justice to the many thousands of interactions that took place between naturalists in many diverse locations, for many diverse purposes. Here, I have gone beyond a simple picture of influence and dissected several particular relationships to see how exchange may have played a role in the natural history of different nations. My analysis of the correspondence of Thomas Pennant with Peter Simon Pallas and Carl Linnæus looked at mechanisms of exchange, at words and objects exchanged, at friendship between distant naturalists. I showed how Pennant's correspondences with foreign naturalists had an effect on the contents of his natural history books; without the information and vast number of specimens that Pallas sent him from Russia, Pennant would have been unable to complete some of his most significant volumes. I also showed how information and ideas could travel the other way: Pennant's belief in the importance of British natural history (which he mentioned in the prefaces to many of his works) was reflected in his promotion of British ideas about classification systems to Pallas, Linnæus and other foreign correspondents.

Though many exchanges were successful there was always the possibility of tensions and difficultly, as is shown by an examination of Pennant's relationship with Buffon. Although some of their disputes appeared to be about very specific questions within natural history – such as moles' senses – in fact, they often involve bigger concerns such as anxiety about the purposes and methods of natural history, or even bigger questions about what was natural or artificial in natural history. In turn, these larger questions may have been coloured by unease about religious or national issues. The chapter's final section used a different approach to look at some of the same issues and again demonstrated the complexity of British naturalists' relationship with a foreign author such as Buffon. Although Buffon was held in high regard in Britain, there were parts of his philosophy to which British naturalists could simply not subscribe. Just as Pennant had struggled against Buffon's belief that classification was unnecessary, so the English-language translators of *Histoire naturelle* removed the parts of the text that explicitly dealt with this idea. The result was a book that contained much of the material from Buffon's original but without the underlying philosophy. The troubled relationship between these two nations is useful in highlighting many of the concerns of British natural history.

These three sections illustrate some of the ways in which British natural history interacted with European natural history. Through the contacts, meetings, letters, parcels, books, translations, friendships or enmities of individual naturalists, the natural history of one place interacted with that of another. While much has been written about the relationship between naturalists and their continental counterparts, and about how this affected natural history and associated fields, particularly in Scotland; far less has been said about how English and Welsh naturalists established and maintained connections in Europe, and about what those connections might have meant for the science. Here, I have chosen case studies that give an insight into the richness of the many relationships that men like Pennant conducted across Europe, and have shown how these relationships played an active role in natural historical thinking and writing. This story has shown a British natural history keen to engage with its overseas counterparts, but very much on its own terms.

CONCLUSION

The natural history of eighteenth-century Britain was a wide-ranging study that moved between beaches and laboratory benches, that could be pursued through the observation of a butterfly's wing or with the latest piece of distillation apparatus, where practitioners might watch how the petals of a flower unfurled or measure the airs expelled from a leaf. The science embraced many subject areas, used many methods, welcomed many kinds of practitioners, and encompassed many different ideas about the natural world. Although describing, naming and classifying objects were important activities, they were not the only ones: to define natural history using just those activities distorts our understanding of the past. The assumption that there was a clear progression from eighteenth-century natural history to nineteenth-century natural history has led to large parts of the science being written out of histories of the field. This thesis has focussed on the parts of natural history that other histories have neglected: it has shown that experimenting and hypothesising were key elements of that science; it has shown that naturalists were interested in understanding, as well as describing, their surroundings; it has shown that natural history and natural philosophy were not separated by a sharply-defined border but had many ideas, practices and practitioners in common. It has provided a new way of seeing natural history and a new way of thinking about how the sciences were linked in the eighteenth century.

The deliberate rewriting of histories of the sciences by nineteenth-century practitioners is something Roy Porter tackled in *The making of geology*. His introduction outlines how the new geologists sought to distance themselves from their seventeenth- and eighteenth-century predecessors. The new geologists believed that earlier studies of the earth were conjectural and unscientific; if they wished to build a new science, they had to move away from those speculative days and return to solid observation and data collection. More than that, they had to visibly repudiate the methods and conclusions of the older science. But Porter's work shows how important events dating back to the seventeenth century were for the development of modern earth sciences. Porter not only addresses the myth-building of nineteenth-century geologists; he also has to deal with those historians who believe the story that a new geology was created, out of nothing, in this period.[477]

Natural history has been afflicted by a somewhat similar problem. Nineteenth-century biology was not inherently at odds with the broad natural history that I have described in this thesis. Some early definitions of the word 'biology' show that it aimed to be a science which studied "the different forms and phenomena of life, the conditions and laws under which they occur and the causes whereby they are brought into being" – this was certainly compatible with much natural history.[478] But others had a different focus: Jean-Baptiste Lamarck saw biology as a science which "pertains to living bodies and particularly to their organization, their developmental processes, the structural complexity resulting from prolonged action of vital movements, the tendency to create special organs and to isolate them by focusing activity in a center" which seems to exclude or diminish some practices that were purely descriptive or classificatory.[479] Despite the fact that there were many overlaps between natural history and the new science of biology, the nineteenth-century desire to create new disciplines, seemingly from first principles, caused many biologists to reject natural history. As I showed in the first chapter, the founders of the Linnean Society were aware that natural history could be viewed as less 'scientific' than the mathematical and physical sciences. Just like the founders of the new geology, they tried to emphasise the importance of collecting facts and the dangers of speculating. But somehow the natural history they created, so reliant on description and classification, did not appeal to those who, like Lamarck, wanted to ask deeper questions. This Linnean natural history and the new biology – established at about the same time, and with similar desires to scientifically examine the living world – grew apart.[480]

In 1854, the biologist T.H. Huxley (1825-1895) gave a lecture on the educational uses of what he termed "the natural history sciences".[481] For him, these sciences equated roughly to physiology and their appeal lay in the fact that they could be used to understand what was special about living matter. Despite the use of the phrase 'natural history' in the title, Huxley spoke only of 'biology' in the talk itself. Huxley worried that

[477] Porter [1977] introduction.
[478] Coleman [1971] 2.
[479] Ibid.
[480] See Farber [1982b] for an account of the developments in nineteenth-century natural history.
[481] Huxley [2008].

biology was seen as an 'inexact' science and gave much time to showing that both its methods and results were every bit as exact as those found in the physical sciences. He believed that there were two reasons for the misconception concerning biology: first, that biology and the physiological sciences were so young – clearly a disavowal of the natural historical roots of these sciences; and second, that many believed observation, rather than experimentation, was the central method of biology. On the second point, Huxley let his feelings be known:

> A speculative philosopher again tells us that the Biological sciences are distinguished by being sciences of observation and not of experiment! Of all the strange assertions into which speculation without practical acquiantance with a subject may lead even an able man, I think this is the very strangest. Physiology not an experimental science? Why, there is not a function of a single organ in the body which has not been determined wholly and solely by experiment! ... Nay, how do you know even that your eye is your seeing apparatus unless you make the experiment of shutting it?[482]

Huxley wished to distance his science from observational techniques that were often associated with natural history, and link it instead to the other nineteenth-century experimental sciences. This rejection of what Huxley, and others, saw as the principal activities of natural history continued in his discussion of classification. He spent some time attacking William Whewell's (1794-1866) belief that "the Biological sciences differ from all others, inasmuch as in *them* classification takes place by type and not by definition".[483] Again, Huxley saw this way of classifying as part of an older natural history that he rejected. For Huxley, the fact that much natural history prior to the nineteenth century had used experimentation and had created classification schemes based on definitions was not important. It was enough that natural history had acquired a reputation as a science akin to stamp collecting. His biology had to be differentiated from this.

Just as Porter found that many historians had believed the myths of nineteenth-century geologists, I find that many believe those of nineteenth-century biologists. Philip Rehbock, writing about nineteenth-century British biology, describes the preceding century as a time of "malaise" in the life sciences. He sees eighteenth-century British natural history as a study "satisfied with the mere description of individual beings",

[482] Ibid. 29-30. That speculative philosopher was Auguste Comte (1798-1857).
[483] Ibid. 30.

isolated from the analytical traditions of continental Europe, and limited by "indigenous feelings which prevented the posing of new questions" and the popularity of natural theology. Rehbock's view of natural history excludes the possibility of it having any kind of theoretical framework and so he denies it the status of a legitimate scientific subject.[484] John Pickstone describes an "hegemony" of natural history in eighteenth-century Britain. For him, this natural history, based solely on describing and classifying objects, is distinct from analysis and experimentation - the two other 'ways of knowing' in the sciences.[485] William Coleman also argues for a clear break between eighteenth-century naturalists who focused on precise classification for its own sake, and nineteenth-century biologists who were more interested in functions of organisms.[486]

These historians, and many others like them, have a distorted view of the practices and scope of natural history. My research has set out to remedy that distortion and give a clearer picture of what natural history really was in the eighteenth century. But what happened to this natural history as that enlightened century drew to a close? Here it is more useful to think of natural histories, rather than one natural history. I have shown how some of these natural histories (such as that of the Linnean Society) had very focused goals that were not compatible with those of others; and so a sort of splintering (just the kind of splintering the Linnean Society had set out to avoid) began. The Linneans were so successful in promoting descriptive and classificatory natural history that they made people forget that there had been other, equally important, natural histories. The new biology was initially the province of those with training in medicine, physiology and anatomy – just as natural history had been. The experimental and analytical methods these men used and the explanations they proposed would have seemed familiar to many eighteenth-century naturalists. So there was a continuity of practitioner and practices across the turn of the century. As biology grew in status and began to coalesce into a discipline, it attracted more followers. Soon, where once there had been a spectrum of activities united under the name of natural history, now just two points on that spectrum remained: one for describing, naming and grouping; the other, for explaining.[487]

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[484] Rehbock [1983] introduction.
[485] Pickstone [2000] chapter 3.
[486] Coleman [1971] introduction.
[487] See Rehbock [1983] for more on the early years of the nineteenth century and the category of 'philosophical naturalist'.

The story I have told in this thesis moves beyond the creation myths of nineteenth-century biology to take a deeper look at how people studied the living world in the eighteenth century. I have built on the work of historians like Farber who show the diversity of eighteenth-century natural history, but my work does not rely on a rigid typology; rather, it is based on the practices of naturalists and so emphasises the fluidity of the field.[488] Naturalists who wished to understand their world faced a range of fundamental questions: was it more important to painstakingly catalogue and describe every species in Creation or to formulate a general idea of how those species related to each other; was it possible to find a natural order or was all classification inherently artificial; what were the essential differences between the kingdoms? These questions related to the theories that underpinned natural history, but there were many practical questions that arose from them: what was the best way to name natural objects; even if one knew the essential differences between the kingdoms, how did one test for them; how did knowledge gained through observation compare to knowledge gained through experiment? And, from these, arose even more immediate questions: how to display objects in the best manner; how to devise and perform a chemical, physical or observational test to distinguish the kingdoms? The second and third chapters used case studies of problematic creatures (seeming animals that can reproduce from cuttings, or plants that appear to sense their surroundings) to look at the kind of theoretical and practical obstacles that naturalists had to overcome in their daily work. The fourth chapter looked at the possibilities (and problems) of using analogies between the plant and animal kingdoms to explain how plants reproduced. The motivation for much of the work that I described in these three chapters came from problems with classification: Ellis simply wished to place corallines in their proper place in the chain of being when he stumbled on to a bigger problem – what is the nature of animal life. Faced with such weighty, and interesting, questions about the natural world, it was no wonder that naturalists chose to try to answer them. Their self-identity as natural historians, rather than natural philosophers, never inhibited them from seeking to understand nature.

If the scientific content of natural history required considerable thought and effort, its daily running was no less complicated. Natural history was not a straightforward venture and its practitioners had to overcome many obstacles – these

[488] Farber [1982a] 398-9.

ranged from the mundane, such as attempting to earn an income, to the practical, such as communicating ideas and objects, to the social, such as legitimating one's science to a wider audience. The fifth chapter told the story of Edward Donovan and some of his contemporaries and showed how, although natural history was not a formal discipline, this group created an identity for themselves as naturalists. They did this through sharing practices, as we see in Donovan's *Instructions for collecting and preserving various subjects of natural history*, or through using similar styles of publishing as we see with Donovan and Shaw, or through the ways in which they assembled collections as we see with Donovan and Macleay. Through these kinds of activities, naturalists could legitimate themselves, claim authority and feel a sense of community. But their path was not an easy one: Donovan and a number of his colleagues ended their careers in bankruptcy – unless one had considerable private wealth, becoming a full-time natural historian could be a risky business. Even when money didn't present a problem, naturalists still faced many practical problems. Natural history was a sociable field and depended heavily on good communication between naturalists; the natural world was too big for any one person to investigate and understand alone so natural history naturally tended towards collaboration. But, as the sixth chapter showed, communication was not always easy. The lengths that Pallas and Pennant went to in order to send information, books and specimens between Wales and Russia were considerable – but both men accepted them as necessary if their science was to advance.

Natural history was not an easy thing to pursue in Britain in this period. Nor is it an easy thing for a historian to describe comprehensively. It did not have a rigid structure that necessitated an obvious hierarchy or career path and, though it had some societies and groups, it did not have any formal institutions until the very end of the century. Eighteenth-century natural history consisted of a unique mixture of theories and practices that has not survived as a modern discipline. So my research spanned many different areas that are often categorised separately such as medicine, chemistry, physics and natural philosophy. Perhaps it is the complexity of natural history that has led historians to neglect certain aspects of it but, as I have shown, delving further into this complexity can produce important new insights. These relate not only to the daily business of natural history but also to bigger questions about how natural history fitted in with other sciences and about the fluidity of boundaries between the sciences in this period. I have argued that natural history was not just a subservient fact-gathering wing of natural philosophy but, instead, that both sciences were engaged in describing and explaining different parts of nature, often using methods and ideas common to both. While I have spent much time considering the connection between natural philosophy and natural philosophy, I have not had space here to properly examine the relationship between natural history and medicine. Most of the characters in this thesis had a background in medicine and there are clearly links between, for example, botany and knowledge about medicinal plants, or between zoology and anatomy. This is a question that merits further research.

This thesis has re-examined a field of knowledge too long regarded as a form of scientific stamp collecting. That image was partly created by nineteenth-century men of science who wished to associate themselves with the new biology. But, despite the work of many historians to show the incredibly active ferment of knowledge that existed in the eighteenth century, and despite historians beginning to write more nuanced accounts of natural history, a history of British natural history that fully reflects its scientific content, its social structures, and its place in the milieu of eighteenth-century sciences has not been written until now. The natural history that I have described here asked, and attempted to answer, all kinds of questions; it was one that engaged with some of the most important puzzles of the day – puzzles about the shape of the natural world, about the relationships between natural objects, and about the meanings of life; and it was a science that sought solutions using any and all available tools. This is a natural history that was almost impossible to define or pigeonhole because of its broad scope, diverse methods, countless theories and multitude of practitioners. And, though perhaps not always recognised, much of it survived into the nineteenth century.

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- Letter III: Peter Simon Pallas to Thomas Pennant, 9th May 1778
- Letter IV: Peter Simon Pallas to Thomas Pennant, undated
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- Letter XI: Peter Simon Pallas to Thomas Pennant, 28th April 1780
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