INTRODUCTION

This book reconstructs some of the assumptions shared by a group of London life researchers in the thirty-eight years before Charles Darwin's *Origin of Species.* It sets out to understand some of the questions that intrigued members of this community, looks at the research from their perspective and depicts their various theories and hypotheses as answers to these questions. In so doing it shows them to be part of a larger interlocking system of beliefs. Some are forgotten; others still live on.

This work can thus be seen as a history of a pre-Darwinian *mentality* in the life sciences. It does differ from earlier studies of mentalities because of its narrow range and focus – covering under forty years, looking only at those in a single city, dealing mainly with those who studied invertebrates. Yet *Styles of Reasoning* can be seen as a history of a mentality for other reasons. It studies collective and often unarticulated beliefs that formed a backdrop for the individual projects of London life researchers. It investigates many ordinary people too – not peasants or heterodox millers but instead now-forgotten life researchers who wrote textbooks, published in medical journals or taught comparative anatomy and physiology to medical students. These people shared assumptions and similar practices that formed an underlying context; a framework giving explicit theories and definitions their meaning and appeal. Finally, this book shows how many of these shared beliefs often reinforced one another, together forming a coherent system. If certain points were accepted, then one was constrained – though not forced – to accept other points.[1]

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These shared presuppositions can be explicated still further, using work from history and philosophy of science: instead of calling them mentalities they are called *styles of reasoning*. Styles of reasoning are here defined as self-reinforcing beliefs about what counted as good research. More durable than Kuhnian paradigms, they were rules about how one reasoned correctly, tacit positions that channelled researchers into deeming certain kinds of evidence to be more relevant to their investigations. A style of reasoning is a more historiographically precise tool than a mentality, because it acknowledges that different styles can coexist. It also more strongly emphasizes the role of scientific practice. Indeed

this book gets its title from Ian Hacking, who altered Alistair Crombie's 'styles of scientific thought' to highlight that scientific work is done just as much outside the head as inside it.[2]

This book uses other insights about styles of reasoning from historians and philosophers ranging from Robin George Collingwood in the 1930s to John Pickstone today. The ways in which they have used styles of reasoning to understand past science have several points in common. Seeing past scientific research as grounded in different styles of reasoning helps us to sympathetically understand that work. Styles of reasoning also show how past beliefs were structured in a coherent way, reinforcing one another to the point of self-authentication. Most relevant for the historian of science, styles of reasoning especially show how scientific disputes were often arguments over exactly what constituted evidence. They illustrate that past scientific disagreements often happened between practitioners who used different styles, constraining each side to see certain kinds of evidence as more relevant than other kinds.

Styles of Reasoning: Analysis:Synthesis and Palaetiology

This book examines Londoners using two different styles of reasoning in the life sciences between 1820 and 1858. The simplest way to tell this story is to focus on one style and then on the other. This does not result in an overly schematic history, if by this term one denotes a rigid and artificial set of historiographic structures. One reason is because of the agents' own self-descriptions and practices. Many researchers explicitly described themselves as practising one style or the other. In other cases, although they did not openly describe themselves as practising one style, researchers' methods and practices clearly show a use of one common style or the other, and this is duly noted. In still other instances a researcher would appropriate what he liked from another's work and reinterpret it according to his own style of reasoning. A less charitable reading is that others' work could be deliberately misinterpreted. Those still concerned with matters of agency and structure can turn to the conclusion of this work for an extended discussion.

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Another reason for organizing this work at the level of styles of reasoning is instrumental. Depicting different kinds of life research as instances of two different styles helps us draw a map, simplifying the terrain in order to discover new connections and recover forgotten problems that mattered to the people being studied. Technical issues are clarified and links made between realms generally studied by one historical discipline but ignored by another. Hence *Styles of Reasoning* shows how a London life researcher like Richard Owen saw reproduction and neuroanatomy as intimately connected issues, not as the respective and dis-

tinct provinces of, say, the history of biology on the one hand and the history of medicine on the other.

Indeed seeing various instances of life research as parts of a larger style of reasoning also sheds light on the oft-repeated insistence that scientific investigations are situated in a larger cultural pattern and historical context. Yes, the sceptic might say, but exactly *which* of the past researcher's myriad contexts was truly relevant to the scientific knowledge being produced? Some historians of science have worried that, without such clarifications, the sciences' cultural and social context becomes an inexplicable 'miasma' unconnected with the technical work performed,[3] turning histories of science into histories of just any other kind of culture. Such sceptics can be shown that a style of reasoning can be seen as a more specific context of scientific work – that a style of reasoning made one kind of scientific research *possible* rather than another kind.

This book presents two different styles of reasoning. The first style is the doublet of *analysis:synthesis.* An *analyst*[4] believed that the best way to learn about systems was to break them up into their simplest unit 'elements' and study them (analysis), then imagine that system to be an aggregation of those units (synthesis). A 'system' could be anything from an organism to an economy. Analysis: synthesis subtly differed from reductionism, for it was not only a doublet of disintegration and reintegration – it also granted the simpler constituent elements degrees of agency and life. Elements were not chemicals, but smaller organized systems in their own right. Thomas Henry Huxley would later complain that this view was little more than a form of decentralized animism.[5]

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The second style was *palaetiology*. The term was coined by William Whewell. A *palaetiologist* believed that the best way of learning about things was to understand their origin and change over time. Yet palaetiology was not quite the same thing as 'developmentalism', for analysis:synthesis also offered its own version of development. An analyst saw the developmental process as one of coalescence: an embryo formed by fusing, hence 'synthesizing', its parts. Some analysts claimed that an embryo developed by first appearing at several different points which then expanded inwards, met and compounded in a centre. While other analysts disagreed, they also saw development as a *centripetal* process. They related an embryo's development to the example of insect metamorphosis, where the serially repetitive segments of the larva coalesced and concentrated during the pupal and imago stages. Meanwhile the palaetiologist held a view with which we are more familiar – an embryo developed *centrifugally*, emanating outwards from a single and simple starting point. For instance Karl Ernst von Baer's embryology depicted development as the emergence of a heterogeneous set of specialized functions and structures from a homogeneous and unspecialized mass.

Adherents of the two different styles used different forms of evidence to justify their conclusions. Analysts deployed the structure and activities of sim-

pler parts to advance their views about a system. Palaetiologists pointed to the changes and developments of a system over time to make their point. Like an argument over two different things, clashes between the two styles tended to mean that the combatants ignored or mocked the validity and meaningfulness of the other side's evidence. This was because the relevance of the opponents' underlying style was denied.

Analysts generally used static evidence. For example they played up the point that since all reproductive bodies were histologically *indistinguishable* from one another, they were for all intents and purposes *identical*. Their focus on static evidence meant that sex was seen as just another form of reproduction, like budding. Meanwhile palaetiologists generally favoured dynamic evidence. In the case of reproductive bodies, they acknowledged that although all reproductive bodies were indeed histologically identical, this similarity was irrelevant. What instead mattered to them was how sexual fertilization was the only kind of activity causing the reproductive bodies called 'ova' to develop into complex organisms. Sex was therefore of the utmost importance to palaetiologists because it was the activity that distinguished one kind of reproductive body from all others.

Styles of reasoning were also related to more concrete matters like practices and physical locations. Different kinds of evidence had to be generated, stored and examined. This book especially highlights the role of invertebrate specimens as evidence: in the 1830s and 1840s it was believed that understanding the structure and function of these simple creatures would in turn allow more complicated ones to be better understood. Unfortunately many invertebrate specimens, particularly marine ones, were difficult to get and tricky to work with. Once one was obtained, where and how could it be kept? Following Pickstone, *Styles of Reasoning* claims that analysts who disintegrated organisms had to store their collection of specimens and various body parts somewhere; large museums dedicated to life research grew out of this requirement. Such museum collections gave an analyst access to a wide range of examples that he could compare; larger collections enabled more comparisons and therefore facilitated what was collectively acknowledged to be better science.

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On the other hand, since palaetiologists favoured changing evidence, their attention turned to living organisms. In this book's case of marine invertebrates, they needed to keep animals alive, in controlled conditions, so they could be watched closely and over a long time. Such observations were not really available until the early 1850s, when the aquarium became widespread. With the growing popularity of aquaria it became possible for many life researchers to view complex phenomena like reproduction and development. This book claims that one reason for the success of palaetiology in the British life sciences was the emergence and proliferation of aquaria.

Problematics

A style of reasoning made certain questions possible and even interesting to life researchers. Some of the questions asked between 1820 and 1858 included:

1. Why did simpler organisms have greater regenerative powers than more complex ones? Why, for example, did lobster limbs grow back at some points while human ones could not? What was the difference between the reproduction of a new *individual* and the regeneration of *part* of an individual? Was there any difference at all?
2. What were the similarities between metamorphosis and the development of embryos? Why did higher organisms tend to develop a head and centralized nervous system? Why did this process of 'cephalization' occur in both individual developing embryos and in the 'animal series' as a whole, running 'parallel' to each other? Why did simpler animals tend to have more repeating parts and more complicated ones tend to have fewer? Why did monsters so often appear as double individuals?
3. Decapitated animals such as millipedes moved around, but in a different, less coordinated, way than whole animals. Why was this movement less coordinated and how else did it differ from the movement of whole animals? Conversely, separated parts could move independently, shown by the ability of turtle and snake heads to bite after being severed – why? What did such phenomena say about the nature of volition? About the unity of consciousness?

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By articulating such points this book has tried to follow Collingwood, who claimed that one could most completely and sympathetically articulate past alien beliefs by seeing them as responses to deeper questions.[6]

Styles of Reasoning groups such questions under the general category of *problematics*. This term denotes that we are not simply interested in the various answers that researchers gave to these puzzles (metagenesis in the first; recapitulating cephalization and anchylosis in the second; consentaneity in the third), but also in the more general historical matter of why certain questions were asked at all. To use Hacking's language, this book is interested in the 'positivity' of certain research puzzles – whether a person could answer these questions in ways that were meaningful to his contemporaries.

The above questions belonged to three general problematics, each made possible by the style of analysis:synthesis. They were closely related because they all dealt with the relationship of part to whole. The first was the problematic of *compound individuality*: how did one best define a biological individual and what were its limits? The second was the problematic of how *spontaneous order* emerged when elements were put together: how did independent parts combine to form part of a larger system? The third was the problematic of *collective action*:

how did separate and independent parts combine their activities so as to work in a harmonious way? These three questions were particularly relevant for people who dealt with invertebrates, in which the relationship of part to whole was often a difficult one to resolve – for instance, when could one tell if a polyp was an individual or part of a larger colony?

While analysis:synthesis made it possible to answer certain questions pertaining to a part's relationship to the whole, palaetiology made these matters less important. Sometimes palaetiology even made them meaningless. To use Nicholas Jardine's words, certain problematics became 'unreal' because different styles of reasoning had different presuppositions.[7] Sometimes the very assumptions making a problematic sensible were misunderstood or rejected as unimportant. Hence for William Benjamin Carpenter and Huxley, their predecessors' views on the compound individuality of many invertebrates were meaningless because they rejected outright their predecessors' underlying assumptions. The work of Carpenter and Huxley between 1848 and 1858 can be seen as a mission to gradually restrict the explanatory and investigative possibilities of the then-dominant community of analytic:synthetic life researchers. It is asserted here that Carpenter and Huxley largely succeeded.

A London Community of Life Researchers and other Historiographic Notes

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Because *Styles of Reasoning* discusses shared assumptions, it investigates second- and even third-rank researchers as well as elite ones. Charles David Badham – savaged by one journal as incompetent – is thus mentioned alongside Owen. This book's search for common beliefs from the 1820s to the late 1850s means that it bundles student textbooks together with influential articles in the *Philosophical Transactions of the Royal Society*. Anonymous and pseudonymous reviews of works – favourable, cautious, scathing, obtuse – are discussed to emphasize that a meaning of a statement should be found in its reception, not its utterance. Private manuscripts and correspondence provide a comprehensive glimpse of certain researchers' lives. *Styles of Reasoning* deliberately restricts its focus to English-language works for two reasons – because most life researchers of the period were conversant only in this language, and because this highlights the greater importance of multilingual people with advanced access to foreign life sciences. Their skills made them intermediaries. Men such as Martin Barry, Robert Grant, Owen and Huxley quickly made their scientific reputations because they imported French – or German – language research.

To emphasize permeable disciplinary boundaries, the people in this book are called life researchers. Calling someone like George Newport an entomologist obscures his work in other areas such as the reflex arc and the sexual fertilization

of frogs' eggs. Such people also described their own work as ranging across different fields. Thus, while Grant was Professor of Comparative Anatomy at London University, at the end of the 1820s his lectures for medical students explicitly covered not only comparative anatomy (also called zootomy) but also comparative physiology and zoology.[8]

Styles of Reasoning differs from a number of other histories of biology of the mid-nineteenth century by paying less attention to certain canonical points – it does not dwell on well-known categories such as materialism versus anti-materialism, for instance, or form versus function. This omission is not because such points are unimportant; it is merely an acknowledgment that other historians have already extensively covered these realms. For this same reason this book makes little mention of Darwin's 1859 *Origin of Species* and the ways in which it changed biology. Not only has Darwin been well examined; the *Origin of Species* managed to reshape the research of many life researchers and obscure the concerns of others, 'like the secretion of a cuttle fish'.[9] De-emphasizing Darwin brings forward obscure areas of life research and how past investigators defined such topics on their own terms.

By portraying Darwin as merely one of many other members of a larger community of life researchers, two important points are emphasized. First, the importance of the neurosciences to life research is made clear. Second, the close links between life researchers and the contemporary concerns of the medical community are highlighted. On the latter point this book strongly agrees with the histories of Adrian Desmond. In this way *Styles of Reasoning* charts a shift: the first part of the book looks at how life researchers cultivated an audience of naturalists, surgeons and physicians. The second part of the book shows how life researchers became increasingly aware of a new audience of 'biologists' less interested in medical issues. The story of how one style of reasoning gradually supplanted another in London life research is thus also a tale about how biology emerged as an independent discipline.

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The Argument and Structure

The argument of this work proceeds from analysis:synthesis to palaetiology. Its first four chapters are synchronic. To post-1815 British life science came the style of analysis:synthesis, largely associated with French research. This style led investigators to focus on the simplest living units, and analysts often depicted *relationships* between parts of a complex living system as *properties* of each part. It was possible to discuss life as the aggregated output of the activity of each cell or tissue, or mental activity as the aggregated product of each ganglion or phrenological mental unit. Some embraced these views and others rejected them, but problematics like these were seen as important and worth discussing and investi-

gating, or were at least considered meaningful. In the same way, an embryo was often portrayed as a synthesis of various elements (cells, ganglia, tissues) into an integrated whole. Again, it was thought that embryonic parts fused together in a process resembling metamorphosis, and although there were disputes over exactly *how* development resembled metamorphosis, the similarity itself was taken for granted.

By 1840 the style of analysis:synthesis dominated London life research and its adherents dwelt on the three above problematics of compound individuality, spontaneous order and collective action. The style's dominant figure was Owen and by recognizing this we can sympathetically recreate parts of Owen's larger research programme, like his principle of 'vegetative repetition' and his *On Parthenogenesis* (1849). Both were answers to the problematic of compound individuality. Indeed this work claims that Owen's programme, and his specific beliefs in such apparently alien causes as 'spermatic force', was considered reasonable by many contemporaries in 1849 because it wrestled with the problematic of compound individuality by using the underlying style of analysis:synthesis.

The final two chapters are diachronic. In the 1850s, analysis:synthesis as a coherent style started to be questioned as palaetiology grew in importance and provided alternative explanations and new problematics for life researchers. Not only had the term been introduced in 1837 by Whewell to denote the study of historical causation and how simple entities grew into more complicated ones – fields including geology, ethnology and comparative philology. In that same year Barry introduced von Baerian embryological principles to an English-speaking audience, describing an embryo's development from a homogeneous and sexually fertilized mass into differentiated and specialized tissues. In 1848 and 1849 – informed by comparative philology and the ethnology of James C. Prichard, and spurred by a growing conflict of interest with Owen – Carpenter challenged what he saw as the 'confusion' of the analytic:synthetic perspective and the 'paradox' of compound individuality. But for his troubles he was publicly rejected by Owen's allies, such as Edward Forbes.

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In the early 1850s the young Huxley appropriated Carpenter's insights for his own purposes. He therefore also used palaetiology – von Baerian embryology and insights from comparative philology – to challenge the status quo of life research while also enhancing his reputation. Like Carpenter, Huxley defined biological individuality temporally. But he had an even sharper conflict of interest with Owen and the gentlemanly patron-client structure of British life research. So he used palaetiology to attack Owen, compound individuality's strongest defender. They argued past one another, but Huxley introduced new terms and, with others, came to control how students in the life sciences were examined. He therefore successfully enforced the proper use of his new terms amongst the young men who would come to form the next generation of elite

London life researchers. This entrenched the palaetiological style among many of them.

To make such an argument, this book is arranged in the following way. The first three chapters break up the doublet of analysis:synthesis into its two components, analysis and synthesis. Hence Chapter 1 looks at the use of analysis in life research, particularly in museums, and studies Owen's use of analysis in his comparative anatomy and arrangement of the Hunterian Museum of the Royal College of Surgeons. Chapter 2 looks at the use of analysis in the orthodox and less-orthodox neurosciences, from the emergence of the reflex arc to the feuding mental faculties of phrenology. Chapter 3 looks at synthesis alone, noting how in the 1830s and 1840s embryos were seen as developing in a coalescing way that resembled metamorphosis. As recapitulation was a synthetic process, parallelism therefore made sense to most London researchers of the time and they fought only over details. Chapter 4 looks at attempts to portray reproduction and regeneration as part of a common process and shows how Owen's 1849 proposal of 'parthenogenesis', later 'metagenesis', was an attempt to reconcile reproduction and regeneration. He proposed metagenesis as an all-encompassing answer to solve several mysteries faced by contemporary life researchers. Chapter 5 looks at the style of palaetiology, how it grew stronger in Britain after 1837, and Carpenter's failed attempts to use it to change British life research in the late 1840s. Chapter 6 investigates how Huxley used palaetiology to overturn Owen's life research programme during the 1850s while trying to prevent its revival by those whom he increasingly defined as 'outsiders'. This group included the self-taught G. H. Lewes, who offered the threatening model of *populist* life science.

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Historians' Questions

In addition to problematics faced by historical actors, *Styles of Reasoning* also tries to answer certain questions asked by historians of science about technical, social and institutional matters. They include:

1. Why was there so much contention over the definition of the biological individual in the mid-Victorian period? Then why did it suddenly stop?[10]
2. How did the new historicist perspective change life research?[11]
3. Why was recapitulation so appealing to leading British researchers?[12]
4. Starting in the late 1820s and early 1830s, why did so many museums appear in Britain?[13]
5. Why was phrenology taken up so strongly by middle-class radicals? How was it connected with orthodox neuroscience of the time?[14]
6. Similarly, why were there strong links between these political radicals and philosophic anatomists? What did they have in common?[15]

7. What exactly was Owen's research programme and what was the importance of metagenesis and serial homology for that programme? Why did large portions of his work go into decline by the late 1850s?[16]
8. How did life research 'professionalize' into biology? How and why were non-elite life researchers forced into 'popular science?'[17]

In dealing with such queries this book most strongly engages with the work of such historians as Frederick Churchill, Desmond, John Farley, Stephen Jacyna, Bernard Lightman, Evelleen Richards, Nicolaas Rupke, Phillip Sloan and Paul White.

Styles of Reasoning also explores the different images and language used by analysts to describe the relationship of parts to whole. More specifically, because analysis was not quite the same thing as reductionism – again, the elements of a system themselves were deemed to possess agency – many life researchers likened body parts to members of a political or economic system. The will commanded other body parts; the body had an 'oeconomy'; the workings of the quasi-independent ganglia of the nervous system were deemed to be 'consentaneous'. One explanation for the use of such language and imagery is that people outside life research also used the same analytic: synthetic style of reasoning. This book notes how philosophic anatomists used similar methods as the philosophic radicals – Grant's tools strongly resembled those of the philosopher Jeremy Bentham. In this sense, although analysts often used micropolitical language and imagery in their life research, they were not simply projecting images of society into their work. They were not looking into mirrors when they looked at their specimens. Instead they used the same cognitive tools that others used to learn about, understand and discuss complicated systems. It just so happened that complicated systems included both organisms and societies.

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