COMPOUND INDIVIDUALITY
IN
VICTORIAN BIOLOGY, 1830-1872

by

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Dedicated with love to my parents, John and Penny,
patient investors of sundry forms of capital

ABSTRACT:

COMPOUND INDIVIDUALITY IN VICTORIAN BIOLOGY, 1830-1872

by James M. Elwick, IHPST, University of Toronto Ph.D. 2004

Herbert Spencer (1820-1903) was an influential Victorian polymath, a popularizer of the term "evolution" who likened societies to organisms, and organisms to societies. His reputation declined even before his death, partly due to the judgment that he was a "bad scientist."

I propose that Spencer's science was far more respected and influential than has been hitherto suspected, but that around 1872 its primary assumption became alien to us. The mid-century biology informing Spencer assumed that organisms were not only compounds of smaller units, but that these units were nominally independent. Like Gogol's fable about the nose that left its owner one morning, each unit – nervous ganglia, cells, body parts - had an amount of "agency" and even "interests." So these constituent units were proposed to be *individuals* themselves. Spencer's work synthesized this.

The dissertation examines the questions which past researchers tried to solve at mid-century, linking different fields where the question of *compound individuality* and, conversely, the *disunity of the organism* was a real one. It discusses four specific research questions all assuming compound individuality. First, whether cells were the true "seats" of life. Second, vivisections and comparative anatomy that investigated how body parts and physiological systems communicated. Third, embryologists' assumption that certain organisms were truly individuals because of their centralized nervous systems. Fourth, investigations of the link between regeneration and reproduction, exemplified in Richard Owen's 1849 "parthenogenesis". The dissertation concludes with Spencer's answers to these questions in his *System of Synthetic Philosophy*.

The view about the disunity of the organism was supplanted by an historicist definition of an individual; questions about body parts' nominal independence and agency were supplanted by this temporal definition. Spencer's libertarian biology – cited by both Gilded Age robber barons and Kropotkinesque anarchists – lost ground to new arguments that spoke of unitary biological individuals governed by a single "brain" or "will." I suggest that these notions were welcomed and used by emerging Weberian status groups of professionals and experts who near the end of the nineteenth century deemed themselves well placed to take on the role of governing the "social organism."

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My trips to London archives were made far easier by Tina Craig at the Royal College of Surgeons; Paul Cooper at the Natural History Museum; Anne Barrett and Hilary McEwan at Imperial College, London; Alun Ford at the University of London Library, and Gina Broad at the Linnean Society Library. This London archival research was made possible by a grant from the Joint Initiative in German and European Studies, University of Toronto, for which I am grateful. I would also like to thank the staff at the University of Toronto's Fisher Rare Books Room and its Interlibrary Loans Service.

Some of my arguments have been previously rehearsed in conference papers, and were reinforced by questions and discussions; I especially thank the participants at the 2002 Joint Atlantic Seminar in the History of Biology, Harvard University, and members of the Toronto-York Science Studies Samizdat (TOYSSS). Part of Chapter 5 has previously appeared in the March 2003 number of *History of Science*.

My greatest thanks are to three people: to Mary P. Winsor, whose enthusiasm for this project first attracted me to IHPST, whose technical knowledge of 19th century invertebrate biology kept the project on track, and whose desire for rigor made this work far more precise than it would otherwise have been. To my wife, Susan Heddle, for her love, unflagging moral support and encouragement: the next work will be dedicated to you. And to Spencer John Ethan Elwick, born 17 January 2003, for making life far more interesting. Regarding our son's first name: it is *not* an expression of fealty to Herbert Spencer. We just thought it was a great name upon which mother and father could both agree.

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INTRODUCTION

Robin George Collingwood famously describes how he became obsessed with the "misshapen, corrupt, crawling, verminous" Albert Memorial as he walked past it each morning on the way to work. How could a community build and approve of such an awful structure? Nicholas Jardine's *Scenes of Inquiry* begins similarly, considering the "apparently deranged" writings of Lorenz Oken's "grotesque" *Elements of Physiophilosophy.* Both writers use these episodes to point out that it is only by understanding the specific questions which past historical figures attempted to answer that we can truly understand these figures. Alien works like Oken's were formulated as answers to these larger questions.[1]

The works of Herbert Spencer have been my own Albert Memorial. Both compelling and grotesque, they are the product of a person whom William James so aptly called a strange mix of genius and pettiness.[2] This dissertation investigates Spencer's work as Collingwood and Jardine have examined their own subjects: in order to understand his alien work it is depicted as an answer to larger research questions surrounding him.

It is well known how Spencer's evolutionary syntheses allowed him to become one of the most well-known and influential intellectuals of Victorian Britain, using science as a way to make pronouncements upon social relations. He became famous for his emphasis on voluntarism, competition and an opposition to any form of support for

[1]R. G. Collingwood, *An Autobiography* (Oxford: Oxford University Press, 1939), pp. 29-30; N. Jardine, *The Scenes of Inquiry : on the Reality of Questions in the Sciences*, 2nd ed. (Oxford: Clarendon Press, 2000), pp. 65, 68-76.
[2]W. James, *Memories and Studies* (London: Longmans, Green, 1911), p. 126.

the "undeserving"; after all he was the product of an evangelical background where free-trade economics was fused with the improvement of public morality[3] But after his death Spencer was reinterpreted as the paradigm 'social Darwinist.' His phrase "survival of the fittest" overshadowed his initial evangelical context, making him unpalatable as a source of political inspiration for later readers.[4] Making Spencer's work additionally strange is the reluctance of historians to deal with his work in its cultural and scientific context.[5] This is particularly evident in Spencer's place in the history of Victorian biology, a place where Charles Darwin's light has shone so brightly, obscuring other researchers.[6]

Like Collingwood's and Jardine's meditations upon their fantastic objects, I begin by assuming that Spencer's work was seen as worthwhile by contemporaries. More specifically I treat Spencer's biological discussions as a reasonable synthesis of many currents then extant in mid-century biomedical research. His *Principles of Biology* (1864-1867) can especially be seen as an answer to various questions then deemed relevant. The tactics used to study Spencer's writings on biology are then deployed against more familiar episodes in the history of biology and medicine that are not usually connected by historians. Thus the dissertation will deal with the reflex and cell theory, the uses of vivisection, regeneration research, views upon sexual and asexual reproduction, and embryology. The Spencerian evolutionary formula more generally – a

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[3]B. Hilton, *The Age of Atonement : the Influence of Evangelicalism on Social and Economic Thought, 1795-1865* (Oxford: Clarendon Press, 1988), pp. viii, 69-70.
[4]M. Hawkins, *Social Darwinism in European and American Thought, 1860-1945 : Nature as Model and Nature as Threat* (Cambridge: Cambridge University Press, 1997), p. 94; R. Hofstadter, *Social Darwinism in American Thought* (New York: Braziller, 1965).
[5]Exceptions include R. J. Richards, *Darwin and the Emergence of Evolutionary Theories of Mind and Behavior* (Chicago: University of Chicago Press, 1987) and M. Ruse, *Monad to Man : the Concept of Progress in Evolutionary Biology* (Cambridge, MA: Harvard University Press, 1996).
[6]J. Endersby, "Escaping Darwin's Shadow," *Journal of the Hist. of Biol.*36, 385-403 (2003): pp. 385-387.

cosmic move from homogeneity to heterogeneity through *differentiation* and *integration* – can be seen as his solution to certain problems then seen as fascinating.

COMPOUND INDIVIDUALITY AS A NEW COMMON CONTEXT

In order to better understand Spencer's strange works, then, it is necessary to reexamine what was taken for granted in Victorian biology. Because of this reexamination I propose a new common context of Victorian biology: the problem of *compound individuality* and its converse, the *disunity of the organism.* Like Robert M. Young's 'common context' of natural theology - which he saw as animating much of Victorian science and culture[7] - I claim that this different common context also informed much of the work done in mid-century British natural history, biology and medical research. By momentarily setting the great issue of evolution aside, one can see that compound individuality was a popular research topic uniting these different fields, as researchers assumed that organisms were compounds of *simpler* elements (compoundness), and that these units were nominally *independent* (disunity). Like Gogol's absurdist fable *The Nose,* in which a civil servant's nose frees itself from his body and wanders around St. Petersburg, each biological unit – be it a nervous ganglion, a cell, or a body part – was said to have an amount of 'agency' and even 'interests.'

The following chapters look at the different ways in which this assumption shaped mid-century Victorian research: questions about whether cells were truly 'individuals' themselves; vivisections exploring the interaction of body parts through sympathy and

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[7]R. M. Young, *Darwin's Metaphor : Nature's Place in Victorian Culture* (Cambridge: Cambridge University Press, 1985), pp. 127-128.

reflex arcs; the linkage of individuality with centralized and concentrated nervous systems; and queries about the link between regeneration and reproduction. What brought these different fields together was an interest in compound individuality, regardless of whether a researcher was Radical, Whig, or Ultra. By looking at their researches as part of a larger investigation into the disunity of the organism, we are forced to look at the broader picture of mid-century biology in a new way. Near the end it is even shown how Darwin's work was shaped by - then changed - this common context of compound individuality.

METHODOLOGICAL NOTES

To repeat, this dissertation uses the question-based approach, shifting from the examination of doctrines and theories (for instance the appearance and acceptance of reflex arcs) to the questions that researchers of the time tried to answer with their theories. By shifting to questions we can begin to understand some of the considerations that they deemed relevant in answering these "locally real" questions, thereby integrating theory and practice.[8]

Instead of tracing changes of a doctrine or of practices over time this dissertation tries to link different contemporaneous fields; it is set in an extremely local geographical and temporal context (London and occasionally Edinburgh between 1830 and 1872). This localization allows the fusion of the cultural context and technical content of Victorian biological and medical research. This dissertation might be seen as an

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[8] Collingwood, *An Autobiography*, pp. 29-30; Jardine, *The Scenes of Inquiry*, pp. 3-4.

examination of a mid-century biomedical *mentalité,* of the 'commonplace wisdom' and habits that drove Victorian investigations into natural history, anatomy and physiology.

To ensure the portrayal of this commonplace wisdom, the sources are as representative and wide-ranging as possible. Textbooks and handbooks not only include the well regarded - such as William Baly's translation of Johannes Müller's *Handbuch der Physiologie* - but also works such as Peter Mark Roget's *Bridgewater Treatise* on physiology, where the technical content has often been passed over by historians of biology. Encyclopaedia articles summarizing the then-most current knowledge about a particular subject are used.[9] Also cited are the four major British medical journals used in Adrian Desmond's *Politics of Evolution*: the *London Medical Gazette,* the *British and Foreign Medical Review,*[10] the *Medico-Chirurgical Review,* and the *Lancet.* In this fashion we can cover the ideological spectrum from Tory to Radical, respectively. Of particular importance as sources are reviews of various articles and textbooks, usually written by authors emboldened by their anonymity. Reviewers are used for two reasons: first, to take into account the historiographic precept that for a message to have meaning, it cannot simply be uttered by an author, but has to be taken up and interpreted by others.[11] Second, because these anonymous reviewers often monitored when a claim was acceptable or not. This dissertation pays special attention to how certain accounts were organized, and how different people used statements as resources.[12]

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[9]For the way in which encyclopaedias shaped knowledge into disciplines see R. R. Yeo, "Reading Encyclopedias: Science and the Organization of Knowledge in British Dictionaries of Arts and Sciences, 1730-1850," *Isis* 82, 24-49 (1991).

[10]Later renamed the *British and Foreign Medico-Chirurgical Review.*

[11]Jardine, *The Scenes of Inquiry*, pp. 246-247; A. Winter, "The Construction of Orthodoxies and Heterodoxies in the Early Victorian Life Sciences," in *Victorian Science in Context*, ed. B. Lightman (Chicago: Univ. of Chicago Press, 1997), 24-50, p. 32.

[12]G. N. Gilbert and M. Mulkay, *Opening Pandora's Box: A Sociological Analysis of Scientists' Discourse* (Cambridge: Cambridge Univ. Press, 1984), pp. 14-15; J. A. Secord, *Victorian Sensation : the*

Next, throughout the dissertation the various people – all men, sadly – are called 'researchers'. This generic terminology is an attempt at defamiliarization – it is not important for this dissertation to recover the "proper" discipline of these investigations, whether they were properly in the group of anatomists or physiologists, surgeons or physicians, zoologists or naturalists.[13] Though these historical figures fit themselves, or were later fit by historians, into particular disciplines – William B. Carpenter into physiology; George Newport into entomology; Robert Grant into comparative anatomy – the dissertation examines what these people did rather than how they described themselves. Disciplinary boundaries were not so rigid as to prevent a researcher from working in different areas: George Newport, for instance, ranged from insect comparative anatomy to millipede vivisections, from examinations of arachnid nervous systems to studying how frog eggs were fertilized. By setting aside whether a researcher was properly in anatomy, physiology, natural history or another field of biomedical research we can better focus upon the questions about the disunity of the organism that these researchers tried to answer.

HABITS OF REASONING, EXEMPLAR ORGANISMS AND SUBVERSIVES

The material is presented in three interacting ways. The first examines the grand habits of inquiry which united or divided various researchers. The second looks at the very particular and concrete organisms that recurred in investigations and textbooks.

Extraordinary Publication, Reception, and Secret Authorship of Vestiges of the Natural History of Creation (Chicago: University of Chicago Press, 2000), p. 19n28.

[13]For the opposite view see A. Cunningham, "The Pen and the Sword: Recovering the Disciplinary Identity of Physiology and Anatomy before 1800. Part 1, Old Physiology - the Pen," *Studies in History and Philosophy of Biological and Biomedical Sciences* 33, 631-665 (2002).

And the third explains why individual researchers chose one habit of reasoning over another by relating their institutional and cultural context to particular choices they made to reinforce or subvert a particular habit of inquiry.

The first perspective builds upon the insight that the historian should focus upon relevant questions. It was the question-based approach which led Alistair Crombie to devise his historiography of "styles of reasoning," where methods and objects of inquiry went hand-in-hand with certain questions deemed important. I have adopted and built upon this big-picture historiography in various ways, borrowing work done by Crombie, Ian Hacking and John Pickstone (who calls this historiography "ways of knowing").[14] Common to all of these approaches is the belief that a particular style of reasoning made certain types of science possible: it strengthened, and was strengthened by, certain institutions, career trajectories, objects, and types of evidence.

Against the charge of oversimplification, one can point out that a researcher used different styles of reasoning at the same time. Certain investigations use two or more styles: thus in historically-oriented investigations such as palaeontology and the evolutionary modern synthesis, they often use the different style of statistics.[15] Rather than call the two emphases "styles" or "ways of knowing", however, upon Hacking's suggestion I have called each one a *habit* of reasoning, as many of the people documented here didn't actively consider alternative research methods; instead they seem to have followed unexamined, customary, perspectives and practices that offered the easiest route.

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[14]The best overview of this work can be found in R. Iliffe, "Rational Artistry," Review of *Styles of Scientific Thinking in the European Tradition* by Alistair Crombie, *History of Science* 36, 329-357 (1998): p. 331. Another discussion can be found in M. Kusch, *Foucault's Strata and Fields : an Investigation into Archaeological and Genealogical Science Studies* (Dordrecht: Kluwer, 1991), p. 94.
[15]I. Hacking, *Historical Ontology* (Cambridge MA: Harvard University Press, 2002), p. 182.

The first chapters examine what Pickstone has called "analysis", the belief that an object or system could be known best by separating it into its basic parts or elements. I add to Pickstone's scheme by dealing with the converse of analysis, or "synthesis" (in which objects were seen as compounds of coalesced simpler elements). This term, synthesis, does not seem to have been used by other historians as a counterpart to how Pickstone defines analysis. But they were strongly related: we will see that analysis and synthesis went hand-in-hand, respectively contributing to the view of the disunity of the organism and compound individuality. The later chapters examine the deployment of a second habit, which John Beatty suggested be known as "palaetiology" (after William Whewell's phrase). Someone using palaetiology believed that temporal knowledge of something (its historical cause, the sequence that brought it about) was the best way to know about it.

These different emphases were noticed long ago. Theodore Merz in the first decade of the twentieth century differentiated between the "morphological" and "genetic" views of nature and how the latter view came to predominate around 1860. Later, Georges Canguilhem distinguished between two opposing views: either an organism was seen to be formed out of a primary plastic substance sprouting continuously out of a centre, or it was seen to be a composite of discrete parts.[16] And so these distinguished predecessors are followed by showing how people habituated to different habits of reasoning saw developmental patterns in different ways.

Borrowing Canguilhem's dichotomy, I present two different views of how development proceeded. They replace the familiar opposition of preformationism and

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[16]G. Canguilhem, *A Vital Rationalist : Selected Writings from Georges Canguilhem*, trans. F. Delaporte (New York: Zone Books, 1994), p. 163; J. T. Merz, *A History of European Thought in the Nineteenth Century*, 4 vols. (New York: Dover Reprints, 1965), pp. 2:279-280.

epigenesis. Those habituated to analysis and synthesis thought that development proceeded through "synthesis" – that is, they emphasized that organisms formed *centripetally*, as a composite of simpler units that coalesced into a compound (Canguilhem's composite of discrete parts). On the other hand the view of development depicted by palaetiologists was that of the *centrifugal* differentiation of a simple unit into one or more specialized ones (Canguilhem's plastic substance continuously sprouting from a centre). Someone habituated to palaetiology held that knowing the developmental process itself was more important than knowing the entities involved. Chapter four discusses an incident in which different researchers were habituated to different habits of inquiry and thus held these very different pictures of development. As a result they talked past each other, placing different emphases upon different types of evidence.

The dissertation's second perspective is more concrete. It looks at humble creatures such as starfish, sertularian polyps and planarian flatworms. The close focus upon local instances of concrete animals balances the vast scope of habits of reasoning. Focusing on the organisms themselves and their relation to theory helps expand upon Mary P. Winsor's notion of science as a dialogue between researcher and organism, the organism "teasing, cajoling and educating its examiner."[17] Some organisms were used because they were widely available, or because their anatomy or physiology facilitated particular investigations. Thus amphibians were favourites of vivisectors because as cold-blooded animals with lower rates of respiration they could survive far greater damage than warm-blooded ones.

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[17]M. P. Winsor, *Starfish, Jellyfish, and the Order of Life : Issues in Nineteenth-Century Science* (New Haven: Yale University Press, 1976), p. 6.

Certain organisms also recurred in this local geographical and temporal context. At a very general level, plants recurred in researchers' accounts in order to form analogies. At a more specific level, recurring organisms included

Tapeworms
Planarians
Leeches
Centipedes
Millipedes
Starfish
Naids
Tunicates, including ascidians (sea squirts) and salps (pelagic tunicates)
Hydra (Freshwater polyps)
Aphids
Sertularian polyps

Researchers often chose these simple organisms to depict basic life structures and processes. For the historian these recurring creatures and plants are additionally important because their sheer structural and functional variety defamiliarizes us with the easy conception of an organism, helping to challenge our preconceptions about Victorian biomedical research about development, about sex, and about regeneration. Most significantly they problematized individuality, blurring the distinction between individual and group.

The specific animals shown above - from tapeworms to naids, from centipedes to *Hydra* – shall be depicted as "exemplar organisms." Organisms shall be referred to as "exemplars" if they recurred and were used didactically, conveying concepts by ostension.[18] In research and in teaching, different exemplars were used to illustrate different problems and solutions, instances were linked to other instances, and a problem

[18]B. Barnes, *T.S. Kuhn and Social Science* (New York: Columbia University Press, 1982), p. 17; T. S. Kuhn, *The Essential Tension: Selected Studies in Scientific Tradition and Change* (Chicago: University of Chicago Press, 1977), p. 307fn.

was frequently reformulated in terms of a known exemplar-solution.[19] As Andrew Mendelsohn has noted, exemplar organisms took on an increasingly important role in nineteenth century biology – as a unified science of life arose in the early 19th century, there came to be various debates over which particular materials best illustrated its principles. Debates over principles and theories could not be separated from the status and "exemplarity" of particular organisms as these exemplar organisms took on a "life of their own."[20]

In many cases these specific organisms acted as 'virtual' illustrations, their examples being passed from researcher to researcher like the citation of precedents in common law. In these cases it was not so much the concrete organisms that mattered as the *accounts* of these organisms: exemplar organisms would appear in encyclopaedias, textbooks and other teaching material to illustrate instructors' points. Even the general cases of plants were often cited in teaching and research documents, and so they too are depicted as a special form of exemplar organism.

Intrepid researchers could therefore set out to reinterpret well-known exemplars; chapter four contains the most substantive discussion of this process. Sometimes they would set the familiar accounts aside and formulate new likenesses because of newly-found 'paradoxes'[21] (is an infusorian a structurally simple or structurally complex organism?) and sometimes these accounts would be used by ambitious newcomers to inform new researches upon these familiar organisms (is a plant a compound organism, with its buds as its 'true' individuals, or is it instead a temporally delimited individual that begins with the act of sexual generation?). Just as common law changed through the

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[19]Barnes, *T.S. Kuhn and Social Science*, pp. 52-53.
[20]J. A. Mendelsohn, "Lives of the Cell," *Journal of the History of Biology* 36, 1-37 (2003): p. 34.
[21]Barnes, *T.S. Kuhn and Social Science*, p. 29.

reinterpretation of precedents or the introduction of new cases, research changed over time as exemplar organisms were reinterpreted, or as new ones emerged to illustrate different points.[22]

Looking at exemplar organisms is also historiographically beneficial as the teachers' use of the same exemplar organisms indicates the existence of a community. People frequently copied others' exemplars: they became a sort of recurring shorthand for various problems and theories. Researchers often mentioned Abraham Trembley's name along with the freshwater polyp, *Hydra,* not only to refer to his famous 1744 mutilations of *Hydra,* but in order to point at larger issues about regeneration and reproduction in animals. Exemplars illustrated the meaning of the concepts and terms that any competent researcher had to know. In turn the training of competent researchers meant they had to learn about certain organisms which conveyed concepts deemed important.[23]

Treating exemplars as embodied concepts may be too intellectualist: the information gained about a particular organism also acted as a resource for future researchers. Giving up a particular organism meant giving up much of the knowledge gained with the study of that organism, explaining why older researchers were often uncomfortable discussing newly-popular exemplar organisms.[24] Certain organisms were popular because they were easy to obtain or easy to work with, too. But there is an even simpler explanation why particular organisms were copied so frequently: researchers and teachers often followed the path of least resistance. As always, time and energy were

[22]Barnes, *T.S. Kuhn and Social Science*, p. 49. Rose-Mary Sargent has previously examined the relationship of legal reasoning to science, particularly on the similarity of the search for practical solutions. R.-M. Sargent, "Scientific Experiment and Legal Expertise: the way of Experience in 17th-century England," *Studies in History and Philosophy of Science* 20, 19-45 (1989): p. 25.
[23]Barnes, *T.S. Kuhn and Social Science*, pp. 35-36.
[24]R. Burian, "How the Choice of Experimental Organism Matters: Epistemological Reflections on an Aspect of Biological Practice," *Journal of the History of Biology* 26, 351-367 (1993): p. 360.

limited. It was frequently easiest to copy others without giving the matter too much thought. Like habits of reasoning, then, many exemplar organisms might just as well be called 'creatures of habit.'

But sometimes habits can be broken. The dissertation's third perspective is to show certain researchers as 'brokers', copying findings, diagrams and methods from other people and even other cultures to further their research and their other interests. Emphasizing brokerage helps avoid the view that individual researchers were merely the creatures of grand habits of reasoning. Though the people depicted here certainly had a culture within which they acted, and though they had group interests which motivated them, they most certainly were agents, not uncritical "dopes and suckers" whose actions were entirely determined by that culture and by these group interests.[25] Though constrained within a particular context and habit of reasoning, they were nonetheless aware of other contexts and they imported what they thought useful.

Pierre Bourdieu's notion of scientific 'investment' strategies, and how practice conforms to symbolic calculation,[26] is therefore applied to understanding how habits of reasoning sometimes changed. It shows how social advancement and changes in research strategy accompanied one another – it nicely explains why certain people thought it worthwhile to import foreign innovations into Britain, and build or "reform" institutions along new lines, or even reinterpret exemplar organisms in a new way.

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[25]Jardine, *The Scenes of Inquiry*, pp. 150-151.

[26]P. Bourdieu, *Outline of a Theory of Practice* (Cambridge: Cambridge University Press, 1977), pp. 177-179; idem, "The Specificity of the Scientific Field and the Social Conditions of the Progress of Reason," in *The Science Studies Reader*, ed. M. Biagioli (New York: Routledge, 1999), 31-50, p. 33.

SUPPORTING AND CHALLENGING PREVIOUS SCHOLARSHIP

This dissertation challenges previous historical scholarship in certain areas. It no longer situates research in a "romantic" current or as a part of *naturphilosophie;* that work has been done well enough by others, and these themes ought to be built on. It also avoids many of the standard oppositions in the history of biology: materialism versus idealism; epigenesis versus preformationism; form versus function; and philosophical anatomy versus natural theology. Instead different oppositions are set out, including *integration* versus *disintegration*; *centrifugal* versus *centripetal* patterns of development; and the habit of *analysis and synthesis* versus the habit of *palaetiology*. These different oppositions reveal different biological, political and social problems that have previously gone unexamined.

In turn this work supports the new study of museums as important power bases and research centres in their own right. It also takes up historians' new emphasis upon how particular organisms shaped research. Its recovery of the context of compound individuality further supports other histories of the period. For instance it fits the standard picture of T.H. Huxley fiercely challenging Richard Owen, with the same figures lined up on the same opposing sides in a drive for cultural authority. By 1849 Owen - with the other members of the Anglican cultural elite - can be seen as habituated to an analytical and synthetical view of society and nature. The scientific naturalist challengers of the 1850s - exemplified by Huxley – in turn can be seen as palaetiologists.

Finally, the rise of palaetiology can be connected with the rise of evolutionary thinking, of which Darwin was its most famous exponent. I introduce Darwin late in the dissertation to show how his work worked within certain habits of reasoning; an earlier discussion of Darwin might overshadow these common habits. But even Darwinian descent with modification can be seen as part of a larger habit of palaetiology, sharing its centrifugal vision of a ramifying pattern of development. By understanding Darwinian evolution as part of a wider context of palaetiology, the rise of Darwinian evolution can be related to contemporaneous fields like comparative philology.

Finally it is in the relationship between nature and culture where this dissertation supports previous histories the most. This dissertation takes up Robert M. Young's complaint about Spencer scholarship: over 30 years ago he complained that two well-received books on social evolutionism were problematic because they failed to appreciate the narrowly scientific issues of the time. But then he also noted that most historians of science failed to consider Spencer at all.

> Their books are symptomatic of the mutual isolation between the study of the history of science and the study of social theory, while their subject is someone who never made that distinction.[27]

Young was quite right to note that Spencer made no distinction between social theory and biological theory – shown best when he likened a society to an organism.

I therefore set out to understand how investigations into compound individuality shaped and were shaped by views about society. This 'dialectical' relationship between society and organism was done not only by Spencer but also by previous and contemporary researchers. Rather than focus on how nature was used by Spencer and

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[27]Robert M. Young, "The Historiographic and Ideological Contexts of the Nineteenth-Century Debate on Man's Place in Nature," in *Changing Perspectives in the History of Science: Essays in Honour of Joseph Needham*, ed. by M. Teich and R.M. Young (London, 1973); reprinted in Young, *Darwin's Metaphor*, pp. 184-185. The books were J. W. Burrow, *Evolution and Society : a Study in Victorian Social Theory* (Cambridge: Cambridge University Press, 1966), which related Spencer's anthropological work to the Scottish Enlightenment, and J. D. Y. Peel, *Herbert Spencer: the Evolution of a Sociologist* (London: Heinemann, 1971), which discussed Spencer's social evolutionism as it related to sociological theory.

other researchers to legitimate particular understandings of society in a post-hoc way, this work will instead examine how models of nature provided Spencer and other researchers with resources to understand society, and vice versa, and how models of nature also constrained possible understandings of society and vice versa.[28] Accounts from nature have always been used to depict the relationship of the individual to the group, and Spencer was no exception. But the interesting story about Spencer is not how his work had particular "implications" – it is instead how different audiences appropriated his organicist views. His writings could be taken to support a harmonious order, formed spontaneously through the cooperation or competition of its parts; but they could also be taken to mean a vision of anarchy that had to be overcome by the guidance of experts.

Finally, this dissertation attempts to understand how certain habits of reasoning may have entailed certain social ideals. I propose that the habit of analysis and synthesis entailed what historian Harold Perkin has called the *entrepreneurial* and *capitalist* ideal; likewise, the habit of palaetiology offered the possibility of the *professional* ideal of expertise that Perkin claims gradually supplanted this norm of populist entrepreneurialism. In other words, certain forms of cognitive order strengthened and were strengthened by certain ideals of social order. By understanding this context we can better depict – and understand - Spencer's decline.

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[28]This is done in J. V. Pickstone, "How Might We Map the Cultural Fields of Science? Politics and Organisms in Restoration France," *History of Science* 37, 347-364 (1999): pp. 361-362. John Dupré has sought to rescue the word "dialectical" from its past baggage in (what I see as) an attempt to refer in a shorthand way to reciprocal causation. J. Dupré, *Human Nature and the Limits of Science* (Oxford: Clarendon, 2001).

CHAPTER 1
ANALYSIS: DISINTEGRATING THE ORGANISM AND THE MIND

A centipede was happy! Till
One day a toad in fun
said, 'Pray which leg
Moves after which?'
This raised her doubts to such a pitch,
She fell exhausted in the ditch,
Not knowing how to run.

-E. Ray Lankester, "The Structure and Classification of the Arthropoda," *Quarterly Journal of the Microscopic Sciences* 47 (1912): 523-82, p. 582.[29]

Lankester's poem introduces some seemingly-odd questions about nervous 'control' or 'co-ordination.' Do we indeed will our legs to move forward? If this question is too easy to answer, what about lower animals with many legs or appendages, like a centipede? If volition alone is responsible for the movements of appendages, then why, after they are "mechanically" separated from the rest of the body, do they often retain the ability of independent movement? Does this mean that they themselves are guided by some form of volition? What about body parts more generally with this power of independent movement? Conversely: if appendages are moved by some sort of "irritability" (which does help to explain their independent movement after separation) then how can they collectively move in a harmonious way – does this occur because they are coordinated by volition and consciousness? Is there a "seat" where this volition or harmonizing power is located? If so, where is it? And if we find it and it is surgically removed, what happens to the rest of the body?

[29]Lankester is seeking the poem's author, for he feels that the poem nicely illustrates the arthropod "laws of metamerism" which he has just discussed.

All of these questions revolve around the issue of compound individuality and the disunity of the organism. The researchers discussed in this chapter asked questions like these. To understand compound individuality and the disunity of the organism, we must first survey the habit of *analysis* and *synthesis*, the method of inquiry that gave simpler body parts or organic units their own agency.

Analysis decomposes a system into its simplest elements, isolating these elements; *synthesis* puts those components back together again, reconstructing how a system works as a whole. Following the Cartesian method, Étienne Bonnot de Condillac stated that a machine could be known by decomposing it and studying each part separately; when put back the way they were before, he thought that the machine would then be known perfectly. Analysis was important in Lavoisier's chemistry and Cuvier's functional anatomy, and Lavoisier credited Condillac for the method of analysis.[30] Since analysis disintegrated a system – a chemical, a machine, or a geological formation - into its constituent elements, this system could conversely be seen as synthesized from, or compounded out of, these basic elements.

In a stimulating recent work[31] John Pickstone has noted that at the beginning of the nineteenth century many different scientific investigations shared this method of analysis. Pickstone lists various different analytically-oriented sciences that existed around 1800, mostly French:

Analytical Chemistry (Lavoisier)
Mineralogy (Haüy)

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[30] W. Bechtel and R. C. Richardson, *Discovering Complexity : Decomposition and Localization as Strategies in Scientific Research* (Princeton: Princeton University Press, 1993), pp. 18-21; J. Simon, "Analysis and the Hierarchy of Nature in Eighteenth-Century Chemistry," *British Journal for the History of Science* 35, 1-16 (2002): pp. 3-4.
[31] J. V. Pickstone, *Ways of Knowing : a New History of Science, Technology and Medicine* (Chicago: University of Chicago Press, 2001).

Geology (Stratigraphy)
Clinical Medicine (Laennec)
Public Health (Chadwick)
Analytical Technology (Betancourt)
Aspects of Geography / Geodesy (Humboldt)
Heat (Fourier)
Light (Fresnel)
Mathematical Analysis (Fourier)
Statistics (Quetelet)

The following areas of biology and medical research used analysis too:

Botany (De Candolle)
Zoology (Cuvier)
Morphology (Geoffroy Saint-Hilaire)
General Anatomy / Pathological Anatomy (Bichat's followers)
Experimental Physiology (Magendie)
Organology (later known as Phrenology/Craniology) (Gall)

Pickstone's proposal may meet the fate of all wide-ranging syntheses, being pecked to death by specialists (some of the people are not French, and there will be disagreement with the dates). This dissertation tweaks Pickstone's work slightly by referring to the habit of analysis and *synthesis,* because analytic decomposition was accompanied by the belief that elements were then associated into compounds.

Pickstone demonstrates that different researchers shared a common method that was called different things by different people. In biology and medicine, organisms were decomposed into various elements (determined according to the discipline). Franz-Josef Gall localized mental operations into discrete units (mental faculties) in his "Organology", a study of the brain and mind that came to be known in Britain as phrenology. Xavier Bichat and his followers were interested in tissues as anatomical elements, while François Magendie was concerned with the single unit of organic function.[32]

There were several reasons for the growing popularity of analysis and synthesis. One was institutional. Just after the French revolution (Pickstone proposes the years 1793-95) the new political authorities took control of various collections that had formerly belonged to the King and Church or were being looted. They laid them out as public displays for all of the people of France, usually following the schemes of the *Encyclopédie*. By deliberately presenting certain parts of the old society that the revolutionaries claimed that they had moved past, they reinforced their claim to be building a new society.

In these collections, money, specimens, specialized practitioners and specialized sites came together, as in the reorganized Muséum d'Histoire Naturelle. Pickstone extends the name of "museums" to include other "museological institutions" where this confluence took place. Medicine, anatomy and physiology gathered around large teaching hospitals; field geology, geography, and public health was studied by government-sponsored surveys. The people "reforming" these institutions upon analysis and synthesis could point to the utility of these reforms, as this centralization cut costs and made investigations more efficient.[33]

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[32]Elements were not reducible into different domains – thus mental faculties would not be reduced further into chemical compounds. W. R. Albury, "Experiment and Explanation in the Physiology of Bichat and Magendie," *Studies in History of Biology* 1, 47-131 (1977): pp. 90-91; J. V. Pickstone, "Museological Science: The Place of the Analytical/Comparative in Nineteenth-Century Science, Technology and Medicine," *History of Science* 32, 111-138 (1994): p. 117.

[33]T. A. Appel, *The Cuvier-Geoffroy Debate : French Biology in the Decades before Darwin* (Oxford: Oxford University Press, 1987), pp. 11, 18; Pickstone, "Museological Science," pp. 117-119; idem, *Ways of Knowing*, p. 132. Surveys are included as museological institutions because the specimens were not always so easy to pick up and put in a specialized site; they collected data instead. Hospitals can be thought of as "collecting" patients for research – thus the growth of Paris teaching hospitals. This is discussed in greater

In describing the relationship between analysis and museums, Pickstone is careful to point out that museums were obviously extant before the French Revolution. He notes instead that museums and associated museological institutions suddenly became far more important as places to undertake analysis and synthesis. The habit of analysis and synthesis reshaped old museums just as much as it influenced the creation of new ones.[34] Sophie Forgan also states how in the first half of the nineteenth century, the word "museum" had a different meaning and association than we have today. They were not merely collections of dead or historical objects, but were also displays of modern objects, knowledge and the most up-to-date instruments.[35]

Another reason for the growing popularity of analysis and synthesis was specialization. The layout of these museum collections was to be set out by "Professor-Curators" who did not merely see themselves as beholden to patrons or the State, but to knowledge itself. Thus in June 1793 the Muséum d'Histoire Naturelle had its post of intendant abolished, and the twelve chairs of the Muséum were governed by a code of regulations. The patron-client system by which savants had formerly made a living was disrupted; at reoriented institutions like the Muséum, there were now salaried positions offering the prospect of stable funding and increased status. So savants began to compete for them.[36]

Holding posts at the Muséum also gave a person access to a major material resource – the Muséum's massive collections. By 1822, Cuvier's "Cabinet of

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detail in E. C. Spary, *Utopia's Garden: French Natural History from Old Regime to Revolution* (Chicago: University of Chicago Press, 2000), pp. 221-227.
[34]Pickstone, "Museological Science," pp. 123-124.
[35]S. Forgan, "The Architecture of Display: Museums, Universities and Objects in Nineteenth Century Britain," *History of Science* 32, 139-162 (1994): p. 140.
[36]Appel, *The Cuvier-Geoffroy Debate*, p. 18; Pickstone, "Museological Science," p. 119; Spary, *Utopia's Garden*, pp. 158-159.

Comparative Anatomy" contained 11,486 preparations; these included 881 mollusc specimens and 1097 other invertebrate specimens, all dissected.[37] The funding of the museums also meant that French researchers could examine strange and exotic new specimens. Though it was easier to preserve certain animal parts like bones and shells, it was far more difficult with squishier bits, like tissues or internal organs or invertebrate body parts. They had to be preserved in alcohol, in jars that didn't always seal properly, causing rot or the inconvenience of bleached and toughened specimens. Since the 1780s French naturalists were able to store greater numbers of specimens like starfish, polyps and zoophytes, as well as other soft specimens which included things like vertebrate internal organs. It was therefore easier to research a wider range of these organisms in French institutions;[38] conversely funding differences help explain the 'vertebrate-centric' nature of the research and collections in places other than France, such as Britain (where only in 1845 were the glass excise duties removed, making various glass items like specimen jars cheaper).[39]

The new specialized French Professor-Curators used analysis to determine the simplest parts common to the various specimens in their enlarged collections. Etienne Geoffroy Saint-Hilaire emphasized the unitary composition of all animals, reducing an extraordinary variety of specimens to common components. Likewise, Cuvier saw comparative anatomy as a form of analysis too - since a person could not isolate the simplest parts of an animal, comparative anatomy could replace experiment. By knowing all of the possible combinations of organs in animals, and all the various organs that

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[37]Appel, *The Cuvier-Geoffroy Debate*, p. 36.
[38]A. Larsen, "Equipment for the Field," in *The Cultures of Natural History*, ed. N. Jardine, J. A. Secord, and E. C. Spary (Cambridge: Cambridge University Press, 1996), 358-377, pp. 358-360.
[39]D. E. Allen, *The Naturalist in Britain : a Social History* (London: A. Lane, 1976), p. 137.

carried out a certain task, that function could be reduced to its essentials. Likewise, one could draw conclusions about the type and function of an organ viewed in isolation (as a specimen).[40]

One of Geoffroy's admirers, the embryologist Etienne Serres, also discussed the embryo's development in terms of analysis and synthesis. He has become best known for his part in the infamous Meckel-Serres law, a recapitulationist doctrine where the development of the embryo ran parallel to the adults of different species situated on a linear chain of being.[41] Its recapitulationism is not in dispute. What needs to be remembered is that Serres's view of development was one emphasizing discrete entities, in which organic elements associated centripetally and became incorporated into a compound. For Serres, development was thus synthesis. The techniques of comparative anatomy (be it through dissection, maceration with alcohol, or the use of acids and alkalis) reversed this synthesis and worked by analysis instead. Serres stated that "Association has united and as it were confounded the elements entering into their composition; disassociation isolates and separates them anew; art acts in an inverse sense to nature."[42] Serres shall be examined in more detail in the next chapter.

Analysis also granted body parts a certain amount of independence. Cuvier's successor at the Muséum, Henri Milne Edwards,[43] used analysis when he first set out the physiological division of labour in the late 1820s. Milne Edwards began his career investigating marine invertebrates on the French coast, and like other researchers wanted to understand why simpler organisms which lacked specialized organs could still carry

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[40]Appel, *The Cuvier-Geoffroy Debate*, p. 47; Canguilhem, *A Vital Rationalist*, p. 270.
[41]S. J. Gould, *Ontogeny and Phylogeny* (Cambridge, MA: Belknap Press, 1977), p. 483.
[42]E. R. A. Serres, "On the Laws of the Development of Organs (Part 4)," *Medical Times* 7, 115-116 (1842): pp. 115-116.
[43]Milne Edwards signed his name without a hyphen.

out essential functions like reproduction, respiration, sensation or nutrition.[44] For his example of the simplest possible organism Milne Edwards gave the freshwater polyp, or *Hydra,* made famous by Abraham Trembley's 1744 researches upon it. Trembley had mutilated *Hydra,* cutting them into various pieces, even turning them inside out, and showed that each fragment could regenerate back into a complete organism. Every part of the *Hydra*, then, was able to live independently, carrying out all of the essential physiological functions required for life. Milne Edwards cited Trembley's work for his insight – that any animal body could be compared to a workshop or series of workshops in which workers carried out tasks, all cooperatively churning out life.[45]

Referring to the "organic elements" that made up the "animal economy", Milne Edwards linked the workings of the animal body to economic principles. The amount of "life" produced could be increased in two ways. One was quantitatively - new workshops could be added to the organism. Milne Edwards used earthworms as an example, as they possessed quasi-independent physiological systems which repeated in each segment. This repetition was why Charles Bonnet could cut them transversely, with each moiety surviving and even forming new worms: because each moiety had the physiological systems necessary to maintain an independent existence.[46] So the "organic element" for Milne Edwards was the simplest possible organism or organism-part which could survive independently and even reproduce; organisms like the *Hydra* polyp, or even polyp-fragments, represented one such organic "workshop"-element.

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[44]Winsor, *Starfish, Jellyfish, and the Order of Life*, pp. 31, 23.

[45]Thus "Le corps de ces Animaux peut être comparé à un atelier où chaque ouvrier serait employé à l'exécution de travaux semblables, et où, par conséquent, leur nombre influerait sur la somme, mais non sur la nature du résultat." H. Milne Edwards, "Organisation," in *Dictionnaire Classique d'Histoire Naturelle* (Paris: Rey et Gravier, 1827), 332-344, p. 340.

[46]Milne Edwards, "Organisation," pp. 340-341.

The other way to increase life was qualitatively, through the physiological division of labour. Milne Edwards noted the specialization of workshops in an individual animal by "moving up the scale of being"[47] – a phrase referring not to evolution, but instead evoking an image of the researcher passing over a progressively arranged series of animals in a museum, comparing them with one another. The mere nutritive sac of the *Hydra* became a distinct cavity in annelids (worms); *Hydra's* single opening, through which food and waste passed, was replaced by two openings - one for ingesting food and the other for expelling waste. For Milne Edwards the highest animals were the ones with the most specialized and localized physiological systems. But these highest animals depended upon the integrity of each system: since each faculty of the highest organisms was dependent upon a localized organ, the destruction of these organs – as shown by Pierre Flourens, François Magendie and Charles Bell – destroyed the faculty too.[48]

The specialization of functions therefore came at a price – each specialized and localized system depended upon every other system for its own survival. The highest organisms could not regenerate their parts, as earthworms or *Hydra* could. A specialized physiological system could not exist independently if separated –separated sex organs could not exist independently, for example, because they could not feed themselves. The connection - between the simplicity of units and their independence, and the specialization of units and their dependence upon one another - was a theme continually repeated in mid-century medical and biological work in Britain, as French work was

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[47]Thus "En s'élevant d'avantage dans la série des êtres". Milne Edwards, "Organisation," p. 341.
[48]Milne Edwards, "Organisation," pp. 340-341. See also C. Limoges, "Milne-Edwards, Darwin, Durkheim and the Division of Labour: a Case Study in Reciprocal Conceptual Exchanges between the Social and the Natural Sciences," in *Natural Sciences and the Social Sciences*, ed. I. B. Cohen (Dordrecht: Kluwer Academic, 1994), 317-343, pp. 319-321; H. Milne Edwards, *Élémens de Zoologie* (Paris: Crochar, 1834), pp. 8-11.

taken up by a new group of British researchers. The link between specialization and mutual dependence was seen as a necessary implication of the physiological division of labour.

BRITISH IMPORTERS: EXPLOITING NEW RESOURCES

The analytical and synthetical habit of reasoning in biology was given new importance in Britain as various French researchers like Cuvier and Milne Edwards visited Britain in the 1830s and 1840s. The importance of marine invertebrates as "*analytic models* for the solution of the major questions of form and function" in higher animals was emphasized in Britain too, as British researchers followed Milne Edwards and his example of *Hydra* as the simplest organism. They too insisted that knowing the simplest organisms helped answer important physiological questions, because these creatures had the simplest structures able to manifest animal life.[49]

Just as important in strengthening the habit of analysis and synthesis, however, was a pervasive feeling of British cultural and scientific inferiority to the French. Throughout the early nineteenth century many British researchers pointed to French analytic and synthetic science as an example of what good science consisted of. In order to speak properly of an improvement or reform we must know the ideal against which existing arrangements were compared. Discontented Britons held up how science was practiced in France not only to highlight British failings but to also show what it ought to

[49]P. R. Sloan, "Darwin's Invertebrate Program, 1826-1836: Preconditions for Transformism," in *The Darwinian Heritage*, ed. D. Kohn (Princeton: Princeton University Press, 1985), 71-120, pp. 76-77. Emphasis in original. G. Johnston, *A History of the British Zoophytes*, 1st ed. (Edinburgh: W.H. Lizars, 1838), pp. viii-ix; G. Allman, "On the Anatomy and Physiology of *Cordylophora*, a Contribution to our Knowledge of the Tubularian Zoophytes," *Phil. Trans.* 143, 367-384 (1853): p. 367.

become (be it wealthier institutions, better instruments, different methodologies, different pedagogical methods, access to larger collections, or a larger number of salaried positions). Though French science was frequently resisted as materialist, atheist and radical, we shall see how it was also 'domesticated' by conservative Britons too also seeking to reform certain research fields. Richard Owen was one conservative.

In controversies about the "Decline of Science," perceptions of the state of Continental science were a useful tool with which subversives could compare and contrast the failings of various British research endeavours. It is already well known how mathematicians such as John Herschel, Charles Babbage and George Peacock called themselves the "Analytical Society", independently studying Lagrange and Laplace at Cambridge University though this French mathematics was unimportant for passing their examinations. Afterwards they decided to import these new French methods into Cambridge, publishing their translations of a French calculus text in 1818 in order to reform the Tripos. The Analytical Society tried to convey how Continental practices overcame failures of British math, frequently glossing over differences of opinion among British mathematicians to make their point about its problems.[50]

The icon of French science thus acted as a cultural 'exemplar,' an embodiment of research principles or practices that was paradoxically an idealization, a norm to be followed. In the case of institutions, British researchers always pointed to the Jardin des Plantes and the Conservatoire des Arts et Métiers. They not only enviously mentioned their architectural features and display layouts, but also the details of lectures given in

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[50]A. Ackerberg-Hastings, "Analysis and Synthesis in John Playfair's *Elements of Geometry*," *British Journal for the History of Science* 35, 43-72 (2002): pp. 54-56; S. F. Cannon, *Science in Culture : the Early Victorian Period* (New York: Science History Publications, 1978), pp. 33-34; J. L. Richards, *Mathematical Visions : the Pursuit of Geometry in Victorian England* (Boston: Academic Press, 1988), pp. 13-18.

these institutions, as well as the astonishing point that their Curators were also Professors! These French institutions highlighted the supposed decline or absence of British ones: William Whewell complained about the poor facilities at Cambridge, noting that because Cambridge had no room for them, Cuvier gave osteological casts to Oxford University instead.[51] The use of analysis and synthesis helps explain why British researchers thought it worthwhile to expend massive amounts of effort and money to change their institutions and practices.

Appeals to idealized Continental achievements continually accompanied calls for larger changes - better known, of course, as "reforms" - along those lines. One 1830 utilitarian's diatribe portrayed the British education system as inferior because it did not teach analysis. Southwood Smith - social reformer, Benthamite and co-founder of the *Westminster Review*[52] - thought that analysis was socially useful for Britons to learn because it was a technique of inquiry leading to certain and precise knowledge. He wrote that it was exemplified in different sciences: as all material objects were aggregates of simpler objects, analysis determined the number of elements in these material objects, showing them separately. Chemistry was one excellent example of the proper use of analysis, for it showed how seemingly-simple substances (such as water) were really compound. Mental science ought to follow the glowing example of chemistry by determining the elements making up thought. Smith added that analysis should be used

[51]Forgan, "The Architecture of Display," pp. 152-153, 141; C. Yanni, *Nature's Museums : Victorian Science and the Architecture of Display* (Baltimore: Johns Hopkins University Press, 1999), p. 36.
[52]Thomas Southwood Smith was a minister who studied medicine at Edinburgh and who became famous with his 1816 *Divine Government.* Besides helping to found the *Westminster Review,* he was active in the public health movement and became linked with the Utilitarians, becoming Jeremy Bentham's physician and carrying out the infamous 1832 dissection of his corpse. Smith is extensively discussed in A. Desmond, *The Politics of Evolution : Morphology, Medicine, and Reform in Radical London* (Chicago: University of Chicago Press, 1989), pp. 200-207. G. H. Brown and W. Munk, *The Roll of the Royal College of Physicians of London*, 2nd ed., 3 vols. (London: 1878), pp. 3:235-238.

in politics too: British legislators didn't analyze properly and thus could not think clearly and comprehensively. But analysis would make their job easier, reforming British laws by making them more precise.[53]

Analysis and synthesis, reform, and even political Radicalism accompanied one another.. Social advancement, institutional changes and reformations of British biomedical research went hand in hand. Adrian Desmond has already shown just how strongly Grant emphasized the French and Radical aspects of his comparative anatomy, particularly the use of Geoffroy's anatomy and Jean-Baptiste Lamarck's transmutationism. He has also demonstrated how Grant frequently held up the organization of French science as a model for British science to copy. Earning less money than he projected from admission tickets for his lectures, Grant thought that Britain should imitate the centralized and bureaucratic structure of French scientific institutions in order to guarantee researchers' salaries.[54]

To tell his story, Desmond uses the opposition of monarchy versus democracy, of idealistic laws 'from above' versus self-fulfilling, even materialist, laws 'from below,' seeing early fights over Lamarckian evolutionism in this light. Desmond depicts Tories and Conservatives as supporting transcendent laws handed down from on high, against Radicals, who derived support from immanent and materialist laws which manifested themselves autonomously.[55] Even if evolution is set aside, there is a strong association between philosophical anatomy and the Radical democratic reforms which so motivated

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[53]S. Smith, "Phenomena of the Human Mind," *Westminster Review* 13, 265-292 (1830): p. 275.
[54]Desmond, *The Politics of Evolution*, pp. 30, 84-85.
[55]Desmond, *The Politics of Evolution*, p. 19; L. S. Jacyna, "Immanence or Transcendence: Theories of Life and Organization in Britain, 1790-1835," *Isis* 74, 311-329 (1983): pp. 311-314. See also B. Hilton, "Politics of Anatomy and an Anatomy of Politics, c. 1825-50," in *History, Religion, and Culture: British Intellectual History 1750-1950*, ed. S. Collini, R. Whatmore, and B. Young (Cambridge: Cambridge University Press, 2000), 179-197, pp. 185-186, 189-191. Hilton elaborates further upon Desmond's groups, dividing them into monists and dualists, and optimists and pessimists.

Grant as he and other Radicals fought the Medical Corporations. Abstract models could be compared with existing arrangements, enabling the criticism of these arrangements as "corrupt." Conversely, there is an association between the use of natural theology, teleological explanations, a focus upon biological functions, and a political conservatism supporting existing institutions, because they all deployed *post hoc* explanations (even theodicies or apologies) for existing arrangements.[56] Subversive works like Grant's and Smith's can be seen as attempts at importing – at holding up the example of one field or institution (cultural or scientific) and its successes as something for others to copy. Their criticisms all pointed out how particular 'lagging' (or even 'corrupt') institutions or fields ought to "reform": by imitating other, exemplar, institutions or fields.

One way to see analysis is the method by which these subversive *a priori* models were determined. After all, in 1838 John Stuart Mill depicted the utilitarian Jeremy Bentham – intellectual fountainhead of much of the British Reform movement – and his attempts to comprehend and reform the English legal system as situated in the habit of analysis and synthesis. Mill defined Bentham's system as the "method of detail," understanding wholes by separating them into their parts. Meanwhile he also characterized Bentham's mind as "eminently synthetical," moving to reconstruct all philosophy upon what he took to be the elements of human nature.[57]

In turn subversives like Grant and Smith can be seen as *brokers*: as people trying to import techniques from one exemplar field or culture into another. Another example

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[56]Thus this is what Bob Young means when he discusses the common context of natural theology and its "functionalism". Young, *Darwin's Metaphor*, pp. 149, 240. I am unsure if Young's link between biological functionalism and sociological functionalism (as in the work of Talcott Parsons, generally deemed to be conservative) is deliberate.

[57] This was Bentham's problem, concluded Mill, for if one left out a crucial "element" of human nature then one's conclusions would fail – "Nobody's synthesis can be more complete than his analysis." J. S. Mill, *Mill on Bentham and Coleridge*, ed. F. R. Leavis (Cambridge: Cambridge University Press, 1980), pp. 48-50, 57-58.

of a broker was the translator, who not only advanced the field but also his own reputation – something best expressed by Goethe, who stated that the translator was a "broker in the great intellectual traffic of the world...promot[ing] the barter of the produce of mind". At least two young British translators cast themselves in the role of broker, for Goethe's statement formed the epigraph to a British translation of various Continental biomedical researches.[58] Continental teachings using analysis and synthesis were brought to Britain in this way, and became resources for subversives to exploit.

COMPARATIVE ANATOMY AS ANALYSIS

From such a broad perspective we can now focus more tightly upon a smaller British research community. For this group, analysis and synthesis became a fashionable way of doing natural history, surgery and medicine. It was trumpeted as an innovation by young British researchers who in the 1820s and 1830s sought to emphasize the "scientific" nature of their work, and the certainty of their Continental principles over what they deemed to be the merely parochial and lower-status craft knowledge of older surgeons and naturalists.[59] For instance William Sharpey was awarded the Chair of Physiology at University College London in 1836 on the grounds that he had spent time in Paris, Germany and Italy; his nearest rival for the post, Richard Grainger, was well-regarded, but had not studied on the continent.[60]

[58] *Scientific Memoirs, selected from the Transactions of Foreign Academies of Science and from Foreign Journals*, trans. A. Henfrey and T. H. Huxley (London: Taylor and Francis, 1853).
[59] Pickstone, *Ways of Knowing*, pp. 103, 112.
[60] P. M. H. Mazumdar, "Anatomical Physiology and the Reform of Medical Education, London 1825-1835," *Bulletin of the History of Medicine* 57, 230-246 (1983): pp. 243-244.

Robert Grant was considered by one student to be the "found[er of] the modern English school of Comparative Anatomy…who mould[ed] the study of Comparative Anatomy in this country into form".[61] For Grant, the fundamental task of the anatomist was to analyze the organism into its parts. The good student, guided by the demonstrator, took up the scalpel and through dissection printed in his mind

> the form, structure, appearance and relations of every part. In the dissecting-rooms he studies not merely the parts prepared by himself, but those of all his companions. The anatomical, physiological, and surgical relations of every part are practically demonstrated, and are repeated in a continuous course...[62]

Grant also advertised his French education and associations. After graduating from the University of Edinburgh in 1814, he studied medicine and natural history in Paris for five years, until 1820. After his return to Edinburgh he continued a correspondence with Cuvier and Geoffroy. In June of 1827 he moved south to become professor of Comparative Anatomy and Zoology at the new London University.[63]

Like the French Professor-Curators that he emulated, Grant began his own 'virtual' museological venture. In 1832 he projected the *Cyclopaedia of Anatomy and Physiology* with fellow London anatomist Robert B. Todd; Todd went to Paris for a while to solicit contributions from foreign researchers.[64] Though not built out of bricks and mortar, the *Cyclopaedia* – eventually numbering five volumes, about six thousand pages, with illustrations, spanning 23 years (1836-1859) – can also be considered as a sort of museum, for it too provided a resource both for elite researchers and for those who were still learning. The resemblance between museum and encyclopedia was recognized by

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[61]Letter to Sir James Clarke on behalf of R. E. Grant, Newport Correspondence, Linnean Society of London, 41.
[62]R. E. Grant, "Address on the Study of Medicine," *L* 1, 41-50 (1833): p. 46.
[63]G. T. Bettany, "Robert Edmond Grant (1793-1874)," in *DNB* (1890).
[64]E. M. Todd and L. S. Beale, "Robert Bentley Todd (1809-1860)," in *DNB* (1898).

other nineteenth-century British researchers - one calling museums a "general index of science, and, as it were, Encyclopedia of reference." In turn, Sophie Forgan has noted the similarity of museum layouts to another sort of institution: both the Hunterian Museum at the Royal College of Surgeons and the Museum of Practical Geology were arranged like libraries.[65]

Like many too-ambitious builders, Robert B. Todd finished the *Cyclopaedia* just before his death, despite having proposed the project when he was only twenty-three; there were muted complaints about the uncertainty of its "continuous publication."[66] Perhaps this delay resulted because he was too busy creating institutions. Todd had come to London in 1831 from Trinity College Dublin, his Aldersgate Street anatomy lectures gaining the favour of eminent medical figures such as Astley Cooper and Benjamin Brodie; from there he lectured at Westminster Hospital and by age twenty-seven he was the first Chair of Physiology and General and Morbid Anatomy at King's College London. By 1838 (at age 29) he helped to convert the poorhouse of St. Clement Danes into King's College Hospital, where he acted as one of its physicians until his death. In 1847 he helped establish St. John's House for the training of nurses,[67] a museological initiative where "collections" of patients were brought together with specializing groups, pedagogical aims, and money.

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[65]E.S. Forbes, "On the Educational Uses of Museums," London, 1853, p. 12; cited in Forgan, "The Architecture of Display," p. 148. These two museums were rectangular rooms dotted with small projecting bays, allowing more books (or materials in the collection) to be displayed; each bay was to contain a subdivision of knowledge. See also S. Forgan, "Bricks and Bones: Architecture and Science in Victorian Britain," in *The Architecture of Science*, ed. P. Galison and E. Thompson (Cambridge MA: MIT Press, 1999), 181-212.
[66]Review of *The Cyclopaedia of Anatomy and Physiology*, edited by Robert B. Todd, *M-CR* 28 (n.s.), 378-387 (1838): p. 379.
[67]Todd and Beale, "R.B. Todd."

Founding or reforming a museum seems to have been an excellent way to announce one's arrival or one's "improving" tendencies. When the anatomist John Goodsir - a gifted Scot who studied at the University of Edinburgh - became curator of the entire University anatomy and pathology museum in 1845, he announced his intention to create a teaching museum above all other British institutions; he held that a person would be able to learn anatomy simply by looking at the structures on display.[68] E.S. Forbes, Goodsir's friend and fellow-member of the Universal Brotherhood of the Friends of Truth,[69] was a museum "improver" too. Born on the Isle of Man, Forbes also studied at Edinburgh and then visited France in 1835; he enjoyed the Jardin des Plantes so much that he stayed in Paris over the 1836-1837 winter, studying at the Jardin and attending Geoffroy's lectures. By 1842 he had been elected to a professorship at King's College; to supplement his meagre (£100) salary he also became curator of the Geological Society's museum (which paid £150). Forbes became involved in arranging the collections of the Museum of Practical Geology, located on Jermyn Street (which was inspired by the example of the renovated Hunterian Museum). With his 1854 election as Professor of Natural History in Edinburgh University, he marked his new appointment by beginning to renovate the museum of his predecessor, Robert Jameson.[70] Forbes's celebratory "On the Educational uses of Museums" pronounced that museums were first and foremost useful for education, teaching the learned and making the ignorant curious.[71] Nor was the push to reform British museums merely the province of younger

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[68]C. Creighton, "John Goodsir," in *DNB* (1890); D. Heppell, "John Goodsir," in *Dictionary of Scientific Biography*, ed. C. C. Gillispie (New York: Scribner's, 1972), 469-471, pp. 469-471.
[69]An Edinburgh student group which counted as members Goodsir, Forbes, Samuel Brown, G.E. Day, George Wilson and John Hughes Bennett.
[70]G. T. Bettany, "Edward Forbes (1815-1854)," in *DNB* (1889); Yanni, *Nature's Museums*, pp. 51-53.
[71]Forgan, "The Architecture of Display," pp. 144-145.

scholars eager to make their mark. At Cambridge, William Clark – Trinity College scholar and friend of Lord Byron – had become Professor of Anatomy in 1817. In 1832 he too began a Cambridge museum of comparative anatomy. Upon the establishment of the Natural Sciences Tripos in 1848, Clark thought that a good museum was so important for the new program that he paid out of his own pocket to increase the museum collections.[72]

Note, however, that with Goodsir, Forbes and Clark we have moved onto researchers that Desmond classifies as conservative. It appears that analysis and the reformation of institutions upon analytical lines were not limited to radical democrats! Indeed, moving back to comparative anatomy, one can note how similar taxonomies were proposed by ideological enemies. Desmond's exemplar Radical comparative anatomist, Robert Grant, can be compared with his exemplar Conservative comparative anatomist, Richard Owen. Owen was the Hunterian Professor of Comparative Anatomy, who, against Grant, Desmond shows to be a Peelite conservative trying to uphold the privileges of the medical corporations like his own Royal College of Surgeons.[73] Grant renamed the four different Cuvierian *embranchements* by using their nervous structure as an index: he renamed Vertebrata the "Spini-Cerebrata"; Mollusca became "Cyclogangliata"; Articulata was transformed into "Diplo-Neura"; and Radiata turned into "Cyclo-Neura." In portraying Grant as a Lamarckian transmutationist, Desmond argues that Grant renamed the *embranchements* to unify the four embranchements into a single series, rejecting Cuvier and supporting Lamarckian progressive evolutionary principles.[74]

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[72]J. W. Clark, "William Clark (1788-1869)," in *DNB* (1887).
[73]Desmond, *The Politics of Evolution*, pp. 354-358.
[74]Desmond, *The Politics of Evolution*, pp. 86-87.

And at first glance Grant's scheme does seem to support a Lamarckian uniserial arrangement overturning the four separate Cuvierian *embranchements*. But the anti-Lamarckian Owen *also* used the nervous system as a taxonomic index, and he also coined new terms using nervous structure. Thus in the mid-1830s Cuvier's Articulata became Owen's "Homogangliata" to denote the phenomenon of the repeating ganglion in each one of an animal's segments; Cuvier's Mollusca became Owen's "Heterogangliata" to denote the irregular dispersion of ganglia in an animal's body.[75] So they may have had goals in addition to - or other than - the support or dismissal of transmutation.

Indeed, arranging the animal kingdom by the development of their nervous system was "generally admitted by comparative anatomists" to be the best taxonomic method, stated one of Grant's reviewers – though he grumbled that his new names weren't really needed.[76] Grant himself stated that the development of the nervous system was "more of an index of the general development of animals than any other system of their economy."[77] Even the anti-transmutationist Cuvier was interested in the phenomenon of the increasingly-compact nervous system in all *embranchements.* He associated the higher faculties and degree of sentience, even self-awareness, with an increasing development and concentration of an animal's nervous system.[78] Renaming taxonomic groups because of the shape of their nervous system wasn't even thought to be

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[75]R. Owen, "Cephalopoda," in *Cyclopaedia* (1836), 517-561, p. 547.
[76]Review of *Outlines of Comparative Anatomy* by Robert E. Grant, *M-CR* 23 (n.s.), 376-388 (1835): p. 381.
[77]R. E. Grant, "Lectures on Comparative Anatomy and Animal Physiology," *L* 1-2, 1833-1834): pp. 1:158-159. There were 60 lectures.
[78]K. Figlio, "The Metaphor of Organization: An Historiographical Perspective on the Bio-Medical Sciences of the Early Nineteenth Century," *History of Science* 14, 17-53 (1976): pp. 23-24.

original to Cuvier – one Briton thought that the French researcher Jules Joseph Virey was the first to have used the nervous system as a taxonomic index.[79]

The *ganglion* to which Grant and Owen's taxonomies both referred was a nervous "knot" made of grey "neurine," a tissue thought to be a unique source of nervous power. There were disagreements about how far the term "ganglion" ought to be extended. One researcher used it to refer only to the grey nervous masses of the sympathetic nervous system, not the encephalon and spinal column, but increasingly researchers saw the entire nervous system, including the brain and spinal column, as compounded from these simplest nervous centres. One reason why the view about compounded ganglia became popular in the 1830s and 1840s was because it helped explain why the separated body parts of simpler invertebrates moved independently.[80] The ganglion became the primary nervous anatomical element in the analysis of the organism.

RICHARD OWEN

This similarity offers a puzzle: how to reconcile Desmond's ideological continuum with the deeper research commitment to analysis. This section will attempt to reconcile the two and in the process understand Desmond's convincing picture of Owen

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[79]J. Anderson, "Sketch of the Comparative Anatomy of the Nervous System; with Remarks on its Development in the Human Embryo," *LMG* 18, 863-869; 906-912 (1836): pp. 864-865.

[80]R. D. Grainger, "Ganglion," in *Cyclopaedia* (1839), 371-377, p. 371; L. S. Jacyna, "Principles of General Physiology: The Comparative Dimension to British Neuroscience in the 1830s and 1840s," *Studies in History of Biology* 7, 47-92 (1984): pp. 70-72; R. Owen, "Articulata," in *Cyclopaedia*, (1836), 244-246, pp. 244-245; S. Solly, *The Human Brain, its Configuration, Structure, Development, and Physiology* (London: Longman Rees, 1836), pp. 15-16. Owen quotes Cuvier as partly founding the group Articulata upon the "divisibility of the body, and the power which the fragments possess of retaining a kind of independent vitality corresponds to the distribution of the nervous system into as many centres as there are corporeal fragments." Owen gives as his reference "Sur un nouveau rapprochement à etablir entre les classes qui composent le règne animale," *Annales du Muséum d'Histoire Naturelle*, 4to, tom. xix. p 73.

as a 'conservative reformer' of the medical corporations. We can depict both Owen and ideological rival Grant as inhabiting a deeper common context about the disunity of the organism founded upon the habit of analysis and synthesis. The differences presented by Desmond - between Radical out-and-outer and Conservative accommodationist – might be seen instead as disagreements within a similar idiom: Desmond's views can be supplemented by stating that at a deeper level, different ideological factions were committed to similar habits of analysis and synthesis. Though they may have had different commitments to social and political order, they shared views about how research ought to be done; they carried out their political and biological disputes in this shared idiom or metaphorical structure.

Though Owen and Grant moved in different circles, they both insisted upon importing new French methods because they were both subversives of the existing scientific order. Grant and Owen sought to use their French analytical assumptions and cultural connections not only to reform British comparative anatomy and physiology and its institutions, but to advance their own careers as well, albeit using different tactics. Pierre Bourdieu's discussion of scientific 'investment' strategies helps reconcile the career paths of the two men, explaining how social advancement and changes in British research accompanied one another.

Bourdieu proposed that all individual scientists make intellectual investments in anticipation of reaping some sort of "symbolic profit" – which includes such things as credibility and reputation, which can in turn be exchanged for worldly goods such as academic positions or higher salaries. Technical ability and the perception of scientific competence are intertwined as scientists compete for scientific authority (mostly done unconsciously, through habit, rather than in a deliberative way).[81] In their race to amass symbolic profit, individual researchers tend to concentrate on problems deemed important by their scientific community, because helping to answer them brings greater symbolic profit.[82]

Complicating matters are power differences - some researchers are more powerful, some less so. The power difference is often determined by how long a researcher has been in a field: dominant researchers have often been there the longest, and they usually want to keep doing science the same way they've always done it, as this research method has brought them good symbolic profits thus far. Scientific institutions can be seen as the sum of previous investment strategies – as their "crystallization", states Bourdieu.[83] We can see different habits of reasoning as different research and investment strategies.

A less-powerful researcher - often a newcomer - can take one of two investment options. One option is that of "succession": doing the same sort of science pioneered by the dominant researchers, meaning that the less powerful researchers innovate in a limited way, still conserving the existing research habits. The succession route is safer, assuring one's greater acceptance in a field, but its drawback is that any adherent will only be able to solve the problems raised by the dominant course of research. In short, she will have a very hard time beating the dominant researchers at the game that they themselves invented.

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[81]An example of this profit seeking might be the race to publish one's findings first, beating another researcher or research team to publication.

[82]Bourdieu, *Outline of a Theory of Practice*, pp. 177-179; Bourdieu, "The Specificity of the Scientific Field," p. 33; J. B. Thompson and P. Bourdieu, "Introduction," in *Language and Symbolic Power* (Cambridge MA: Harvard University Press, 1999), 1-31, pp. 15-17.

[83]Bourdieu, "The Specificity of the Scientific Field," pp. 31, 32, 36.

The other option is "subversion," where a researcher or group of researchers try to redefine the very principles by which good science is identified, changing the rules about what is deemed to constitute good research. The subversion option is riskier as they recognize no other form of "good scientific research" than what they themselves practice: they themselves can be rejected. Like Grant they can noisily accentuate their differences with the dominant research method / investment strategy by actively criticizing research done that way: this is what it means to criticize research done in the dominant fashion as 'outdated', for it is now compared to the new method. Or - more moderately - subversives can quietly ignore the old way of doing research, refusing to recognize its validity. Bourdieu states that by breaking with the old order, the subversive researchers try to accumulate symbolic capital to themselves. This break often occurs between old and new generations of researchers, and is the process described by the cliché of a "generation gap": the "new generation" of researchers is a group (of people who may not actually be particularly young) following a subversion strategy.[84]

This theoretical interlude allows us to better understand the quieter Owen and his career of reforming British museological institutions and comparative anatomy upon analytic lines. Like Grant, he was also a product of the University of Edinburgh, beginning his medical studies there but then leaving for London in 1825 before taking his degree. His mentor gave him a letter of introduction to the powerful John Abernethy,[85] who immediately appointed him prosector for his surgical lectures at St. Bartholomew's Hospital. An exceptionally hard worker and perceptive researcher, Owen was

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[84]Bourdieu, "The Specificity of the Scientific Field," pp. 38-39.
[85]Abernethy was full surgeon at St. Bartholomew's Hospital, the founder of its medical school and a very popular lecturer; he was also an occasional lecturer on anatomy and physiology at the College of Surgeons. J. F. Payne, "John Abernethy," in *DNB* (1885).

nonetheless mindful of the importance of patronage as a way to start his career – in a letter, his mother pointed out that mere skill and perseverance was not sufficient for success. It was equally important for an ambitious young man to become pupil to someone eminent, for this way the person would not only learn much, but also become "known" to his patron's friends. One of Abernethy's friends was William Clift, who was charged with preparing the Hunterian Museum's catalogue. In 1827 Abernethy asked the overworked Clift to give Owen the position of Assistant Conservator of the Museum. Clift gave him the position, and Owen's task was to prepare the catalogue of the Physiological Series.[86]

As he worked on the catalogue Owen followed his mother's advice, continuing to become known to other eminent figures. When Cuvier visited the Hunterian Museum in 1830 he invited Owen to visit him in Paris. In 1831 he did visit, attending Cuvier and Geoffroy's lectures and working in the galleries and dissecting-rooms of the Jardin des Plantes. He cultivated other French researchers including Milne Edwards, whom he first met in Paris in 1830. They exchanged articles, Milne Edwards had Owen's first major work (on the Pearly Nautilus) translated for a French audience, and he brought his students to London to meet Owen in the early 1840s.[87]

In 1833 Owen sought to renovate the existing Hunterian Museum, and so a new competition for the museum design was held, won by the architect Charles Barry. The museum was reopened four years later, in February 1837.[88] Owen's move to reform the

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[86]J. Dobson, Manuscript, "An Account of the Life and Achievements of Richard Owen", 1981, OCOLL, NHM 86, p. 4; W. H. Flower, "Richard Owen (1804-1892)," in *DNB* (1894).
[87]Dobson, "Achievements of Richard Owen", p. 14; Flower, "R. Owen"; H. Milne Edwards to R. Owen, 7 Jun 1841, OCORR, NHM, 10/294; R. S. Owen, *The Life of Richard Owen* (London: J. Murray, 1894), p. 1:200.
[88]Yanni, *Nature's Museums*, pp. 47-50.

Hunterian museum coincided with the beginning of the British "age of museums," when nearly all of its famous museums were built: Nicholaas Rupke notes that about 200 were built in the U.K. during Owen's career (1827-1883).[89] Building or remodelling a museum required a tremendous amount of effort and money. By one (hostile) estimate over £200,000 had been spent upon the Hunterian Collection between 1793 and the mid-19th century.[90]

A more specific example is Clift's own 1835 estimate that part of the collection, numbering 222 specimens, was worth £7000. In a partial breakdown of the costs, he calculated that the materials and labour used in the preparation of the specimens cost £500. The 222 bottles cost 10/- each; the spirit cost £55; and the labour (servants and hospital people, drawing up the histories, dissection and other materials) averaged 1:10:0 a specimen, totalling £166.[91] To put these amounts into some sort of perspective, in 1834 a person could rent an eight-room house in London for £35 a year (done by Thomas Carlyle, who together with his wife earned £300 a year when they first moved there). For his part, Owen was paid just £120 a year to prepare the catalogue, a modest income which his prospective mother-in-law didn't think very highly of.[92]

The person who estimated that spending upon the Hunterian Museum had reached £200,000 was critical of the amount; he thought it was done to give the Royal College of

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[89]Pickstone, *Ways of Knowing*, pp. 130-131; N. A. Rupke, *Richard Owen : Victorian Naturalist* (New Haven: Yale University Press, 1994), pp. 13-14.

[90]Robert Knox, *Great Artists and Great Anatomists: a Biographical and Philosophical Study* (London, 1852), p. 21, cited in L. S. Jacyna, "Images of John Hunter in the 19th century," *History of Science* 21, 85-108 (1983): p. 99.

[91]W.H. Clift to R. Owen and C. Clift, 22 Jul 1835, OCORR, NHM, 7/136.

[92]J. Burnett, *A History of the Cost of Living* (Harmondsworth: Penguin, 1969), p. 235. For example, Mrs. Clift wouldn't let Owen marry her daughter Caroline until he made more than £120 a year! Though engaged in 1828, Owen only married Caroline in 1835. With such a powerful incentive for financial advancement, Owen must have obtained some sort of a raise by 1835. We do know that in 1842 he accepted a civil list pension of £200, and that by 1856 he was making £800 a year when he took over the natural history departments at the British Museum. Flower, "R. Owen"; J. Dobson, Manuscript, "Achievements of Richard Owen (ref. 89)" p. 4

Surgeons a *frisson* of scientific authority, and opined that the money could have been better spent elsewhere.[93] Though partly accurate, this cynical view can be supplemented. These vast amounts of money were also spent upon the museum because it was supposed to enable people to do better research. It provided new resources for British biomedical research, and so Owen's work ought to also be seen in that context.

But complaints about museum spending show that museum-growth was in no way inevitable; outlays as large as £200,000 required justification. So on the one hand there were complaints about the state of the Hunterian Museum - in 1833 one Professor to the College of Surgeons told an audience of medical students that Clift and Owen were frequently irritated by foreign announcements of "discoveries" already present in the museum collection or in John Hunter's writings. He announced that "we" were to blame for this problem, for the Museum's treasures hadn't been sufficiently explored, or properly arranged, for it to be used effectively. On the other hand history was re-told to emphasize the utility of museums, thereby legitimating the money spent upon them. The very same lecturer who deplored the decrepit state of the collections then implied that John Hunter owed his famous reputation because he had access to a vast range of specimens. He related how Hunter had become a great surgeon because of his extraordinary access to his brother William's dissection-room and massive collection, something supplemented by his conversations with the eminent men who came to examine those specimens.[94]

Eventually the citation of French exemplars changed the management of the British Museum too – a Parliamentary Select Committee of 1835-1836 investigating the

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[93]Robert Knox, *Great Artists and Great Anatomists*, p. 21, cited in Jacyna, "Images of John Hunter," p. 99.
[94]C. Bell, "Lecture 1 on the Hunterian Preparations," *L* 1, 279-285 (1833): pp. 280-281.

"Condition, Management and Affairs of the British Museum" heard testimony from figures such as Owen and Grant. Grant was highly critical of the British Museum, comparing it to Continental counterparts. As a result of the Committee's findings, more money was spent on that institution: in 1837 the Natural History Department at the British Museum was divided into three branches (Mineralogy/Geology, Zoology, and Botany) and each branch given £800 a year for buying specimens, and £800 for their preservation. The heads of the branches were paid £450 a year and assistants £250.[95]

As British museums emerged or changed - their growth justified by appeals to French exemplars and the redefinition of good research practices as analytical - a virtuous circle appeared. The growth of a group of museums or other museological institutions made it easier for everyone's collections to grow, as one institution's collections could strengthen another's. For example, as Owen began cataloguing the Hunterian Museum he noted that many of Hunter's specimens had deteriorated – but he was able to fill some of these gaps by obtaining dead animals from the gardens of the Zoological Society (they offered Clift a dead orang-utan for ten pounds).[96]

By the end of the 1830s Owen was recognized as the leading comparative anatomist in Britain. In April 1836 he was made the very first Hunterian Professor of Comparative Anatomy and Physiology at the Royal College of Surgeons. In 1837 he was unanimously elected Fullerian Professor at the Royal Institution; he won over other London researchers such as Samuel Solly (lecturer at St. Thomas's Hospital and brain

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[95]A. E. Gunther, *The Founders of Science at the British Museum 1753-1900* (Suffolk: Halesworth Press, 1981), pp. 75-85.
[96]W. Clift to R.Owen and C.Clift, 22 Jul 1835; Flower, "R. Owen."

specialist), Herbert Mayo (author of successful physiology textbooks), Robert B. Todd, Gideon Mantell (surgeon and palaeontologist), and Robert Grant.[97]

As Owen rose his testimonials were increasingly sought too; his letters could ensure that a new researcher would win out in the starkly competitive job hunt for medical research and natural history posts. People such as William Baly (translator of Müller's successful physiology textbook), W.B. Carpenter (the physiologist), John Goodsir (the Edinburgh microscopist and anatomist), George Allman (Professor of Botany at Trinity College Dublin in the 1840s) and T.H. Huxley all sought and obtained his recommendations.[98] As Owen rose he in turn strengthened a nascent group of analytically-minded biomedical researchers. He had protégés too: as a lecturer on comparative anatomy at St. Bartholomew's hospital (acquiring the post through Abernethy in 1829), he taught Arthur Farre, White Cooper, and Thomas Rymer Jones.[99]

Owen's advice on how to best arrange museum collections was sought too. William Clark at Cambridge asked him to recommend someone who could help him organize the Cambridge museum, as "your recommendation will ensure us a fit person." The entomologist J.O. Westwood sought Owen's advice about rearranging the museum at Oxford, and then for a testimonial about his fitness to run the Zoology Department at the British Museum. The Sydney Museum board even sent someone all the way from

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[97]Flower, "R. Owen." As the Royal College of Surgeons prevented concurrent appointments, Owen turned down the offer and Grant was appointed instead. Rupke, *Richard Owen*, p. 19.

[98]G. Allman to R. Owen, 7 Jan 1849, OCORR, NHM, 1/123-4; W. Baly to R. Owen, 1 March ND, OCORR, NHM, 2/102; J. Goodsir to R. Owen, 6 Jun 1845, OCORR, NHM, 13/179-183; J. Goodsir to R. Owen, 23 Jan 1846, OCORR, NHM, 13/184-185. George Allman followed his father William in this post (who had held it from 1809-1844). See F.C. Sawyer, *A Short History Of The Libraries* (London: Natural History Museum, 1971), 89.

[99]R. Owen, Manuscript, "Memoirs", 1868-1889, OCOLL, NHM, p. 8.

Australia to consult Owen in the selection of a curator for that institution.[100] Following the French example, he had in turn become the paradigm British Professor-Curator.

JOHN HUNTER AND THE DOMESTICATION OF ANALYSIS

During his time at the Hunterian Museum, Owen saw himself as following John Hunter's path. He seems to have been genuinely moved by Hunter, but as we shall see he was just as good at enlisting particular aspects of Hunter's research as support for his British analytical programme. Moreover, Hunter's work helped strengthen the context of compound individuality.

As a student, Owen founded a students' society in Edinburgh called the "Hunterian Society"; in promoting Hunter's example he followed his patrons John Abernethy (whose lectures championed Hunter's theory of life) and Clift (Hunter's apprentice until Hunter's death, a man who viewed Hunter with "enthusiastic reverence").[101] Owen helped bring out two different volumes of Hunter's work, one in 1837 and one in 1861.

In being so well acquainted with Hunter and his work, Owen cannot have failed to notice Hunter's view of the anatomically localized vital property. Hunter proposed that each animal part had its own independent principle of action. When seen in terms of its internal economy, an individual animal could be seen as a grouping of independent parts

[100]W. Clark to R. Owen, 28 Jan 1838, OCORR, NHM, 6/181. This letter is a counterexample of the popularity of the Hunterian system: in 1833 Professor Mosley, the new Oxford Professor of Comparative Anatomy, considered the Hunterian system outdated and wanted to group by species instead, displaying the different organ-systems together. OCORR, NHM, 17/335; J.O. Westwood to R. Owen, 30 May 1833, OCORR, NHM, 26/277-278. J.O. Westwood to R. Owen, 1 Jan 1850, OCORR, NHM, 26/282; W.S. MacLeay to R. Owen, 28 Feb 1859.
[101]Flower, "R. Owen"; Payne, "J. Abernethy."

- "however an animal body may seen to be compounded, all its particular parts having a dependence upon one another; yet in an anatomical sense they are more or less distinct, so as to admit of distinct examination."[102]

In other words, Hunter presented each part as a causal agent. Stephen Cross has shown the similarity between the internal economy of the Hunterian animal, and classical political economy – that self-interested "individuals" interacted in the market just like Hunter's animal parts did, with each economic and physiological unit displaying agency. For Hunter the interaction of economic and physiological agents explained the seeming harmony of society and of the animal body – there was no need for a guiding sensorium or ruler in guiding either the processes of the market or the body, which meant that the importance of the will was diminished.[103]

Notice – just as in Milne Edwards's discussion of "workshops" and the physiological division of labour - the association of an organism's physiology with a market. And just as in Milne Edwards, the problem of compound individuality occurred in Hunter's work too. Hunter thought that the individual humble-bee body was an assemblage of quasi-independent parts: it showed "the union of the different parts of nature with each other, each part acting immediately for itself, yet collecting for others, and each depending on another, making in the whole one uniform machine, although made up of many and various parts."[104]

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[102]J. Hunter, *Essays and Observations on Natural History, Anatomy, Physiology, Psychology, and Geology*, ed. R. Owen, 2 vols. (London: J. Van Voorst, 1861), p. 371.
[103]S. J. Cross, "John Hunter, the Animal Oeconomy, and Late Eighteenth-Century Physiological Discourse," *Studies in the History of Biology* 5, 1-110 (1981): pp. 41-42, 74-75.
[104]Hunter, *Essays and Observations*, p. 60.

Hunter was not talking about a hive of bees, but about an individual bee's body. And his approach tried to explain reciprocal dependence and specialization in other individual bodies by citing other familiar organisms:

> The more complicated a machine is, the more nice its operations are, and, of course, the greater dependence each part has upon the other; and, therefore, there is a more intimate connexion through the whole. This holds good in society. It also holds good in the animal economy. The most perfect animals cannot be hurt in part without the whole suffering, while the more imperfect may be considerably hurt without the other parts suffering much. Thus we find that a man cannot lose a leg without the whole frame sympathizing with the injured parts, as if conscious of a loss; while a frog appears to be but little hurt. A snail, lobster, lizard, &c., can lose many parts which will be restored again. A polypus is still less hurt by amputation; for a new animal arises out of the wound or cut. So far we find a gradation from the animal to the vegetable.[105]

T.H. Huxley later recognized Hunter's emphasis upon the interaction of physiological agents, noting that the "logical end" of his physiology was that the individual lives of the higher animals were the sum of each of its vessels, each vessel a sort of physiological unit "like a polype."[106] In explaining John Hunter, Huxley cited the organism which inspired Milne Edwards's insight in the first place – the polyp recurred once again as an example of an organic element.

These recurring exemplars and repeated mentions of "internal economies" - evoking the image of a hall of mirrors, with Hunter's work difficult to distinguish from Owen's own work and Huxley recalling Hunter's work in turn – are all the more noteworthy because it is sometimes difficult to separate Hunter's work from Owen's. Many of Hunter's unpublished papers, after all, were taken away from the College in 1800 and then burned in 1823 by Sir Everard Home under mysterious circumstances. Clift estimated that about 90% of Hunter's manuscripts had been destroyed, and so the

[105]Hunter, *Essays and Observations*, p. 119.
[106]T. H. Huxley, *Collected Essays*, 9 vols. (London: Macmillan, 1893-1894), pp. 3:366-367.

task of cataloguing the Hunterian Museum's specimens was very difficult. Owen had to redescribe almost all of Hunter's specimens – by 1836 he had compiled seven catalogues listing 8000 of them.[107]

As one of the posthumous editors and interpreters of Hunter's writings, Owen was in a good position to emphasize what he felt was important about Hunter's work. I strongly suspect that Owen distilled out of the ruin of Hunter's manuscripts and his often-cryptic statements a selection of points favourable to his own position, then used these points to conclude that Hunter was truly guided by the method of analysis and synthesis.

> Having thus laboriously obtained his knowledge and his material evidences of the modifications of particular organs analytically, Hunter strove to impart the higher conclusions deducible from those evidences by presenting them in the synthetical order requisite for such generalizations; as in the arrangement which governs the disposition of the Physiological Collection in the Museum.[108]

This is not to deny that Hunter used analysis and synthesis in his work; simply that Owen accentuated this aspect of it. L.S. Jacyna has noted how other various British surgeons, in an *"a posteriori* conscription of great names", invoked the spirit of John Hunter in order to legitimate some aspect of a favoured surgical practice,[109] perhaps just as 20th century biologists and psychologists invoked the name of Darwin to rhetorically strengthen their own work (for example the modern synthesis or evolutionary psychology).

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[107]Home had taken the papers away from the College, claiming that he wanted to catalogue them, but it was thought that over 30 years he instead appropriated much of Hunter's unpublished research into his own papers for the Royal Society. He burned Hunter's work to cover this up. D. Ottley, "Life of John Hunter," in *The Works of John Hunter, F.R.S.*, ed. D. Ottley, et al. (London: Longman Rees Orme Brown Green and Longman, 1835), 1-198, pp. 152-153. Desmond, *The Politics of Evolution*, pp. 246-248. also discusses this matter, along with lurid tales about the affair (the manuscripts were apparently used for toilet paper before being burned).

[108]R. Owen, "Observations on Palaeontology," in *Essays and Observations on Natural History, Anatomy, Physiology, Psychology, and Geology* (London: John Van Voorst, 1861), 281-340, pp. 282-283.

[109]Jacyna, "Images of John Hunter," p. 105.

In celebrating Hunter's legacy it may be that Owen was no different, but more subtle: as the most prominent editor of Hunter's works, he could reinterpret Hunter's story as a new founding myth for British comparative anatomy. Hunter thus became a historical exemplar of the use of analysis and synthesis, becoming a norm, a fountainhead of good principles that other researchers ought to follow.[110] The British Hunter had even anticipated certain French discoveries – Owen pointed out how "our great physiologist" had discovered a property of the developing embryo some fifty years before another researcher presented the same property to the Académie as a new finding.[111] In other words, Owen could use Hunter to assimilate French principles of analysis and synthesis to a different national context – by making it seem as though they were the same principles guiding a previous eminent British researcher. In other words Owen used Hunter to 'domesticate' and legitimate analysis and synthesis, cleansing them of the taint of French materialism and atheism, allowing the ideological conservative to use them to in his own projects to quietly reform how British comparative anatomy was to be carried out.

The assimilation of analysis and synthesis to a British context is also evident in the arrangement of the Hunterian Museum. An earlier editor of Hunter's work (Drewry Ottley) noted how Owen had reinterpreted Hunter's work by using new French innovations: he had adopted Cuvier's and Lamarck's classes (one "articulated", the other "inarticulated") in his arrangement and catalogue of 614 of Hunter's invertebrate specimens. But even the category of "invertebrates" did not exist in Hunter's time. For

[110]For thoughts on the use of histories as legitimations of scientific disciplines see Jardine, *The Scenes of Inquiry*, pp. 129-131.
[111]R. Owen, "Preface," in *The Works of John Hunter, F.R.S. with Notes*, ed. D. Ottley, et al. (London: Longman Rees, 1837), i-xl, p. xiii.

his part, Owen likened the Hunterian method of arrangement, with its organs moving from the simple to the complex, as very similar to Cuvier's and the "best modern works" (because they allowed the researcher to compare grades of organization).[112]

The Hunterian Museum's collections were organized in two divisions of a Physiological Series. The first looked at a particular organ in a mature human and then how it appeared in other mature animals; the other division examined an organ as it developed in the embryo from conception to maturity. In 1835, Ottley described how the subdivision "digestion" was laid out: teeth were shown, then their analogy with bird beaks; their varieties were shown; and then how teeth grew. Then the stomach was shown: the collection began with the simplest radiate "stomachs", moved through insects, molluscs, fish and birds, and ended with mammalian stomachs, the most complex ones. Then various glands (salivary glands, the pancreas, the liver and the gall bladder); the organs of circulation; organs of respiration; intestines; and brain were shown.[113] In 1835 Ottley did not mention any guiding rationale behind the display of certain collections, for instance the teeth. I thus cautiously note that only in certain cases did Ottley portray the Hunterian Collection as a series moving from the simple to the complex. This point will need more research.

Yet we do see that Owen - writing in 1837 and thereafter - interpreted and presumably catalogued the entire Hunterian collection strictly in terms of their simplicity and complexity: in other words making it appear as though Hunter was guided solely by the rationale of analysis and synthesis. He related how Hunter followed organ after organ

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[112]Ottley, "Life of John Hunter," pp. 183-184; R. Owen, *Lectures on the Comparative Anatomy and Physiology of the Invertebrate animals* (London: Longman Brown, 1843), pp. 3-4; Owen, *The Life of Richard Owen*, pp. 1:213-215.
[113]Ottley, "Life of John Hunter," pp. 158-161.

until he had seen the entire organ-scheme in the most complicated animal (usually humans) and then "traced down the simplifying modifications" shown in other animals and how they were "reduced to a more and more simple condition." Owen then discussed how Hunter reassembled his collection by reversing that order, starting with the simplest form of the mature organ, placing organs of increasing complexity after it, and ending with the most complex organ (again, usually a human one).[114]

Owen interpreted Hunter as seeking in the simpler versions of the organs of the lowest animals the real functions of the organs combined in humans.[115] Yet at the same time he left out other important points guiding Hunter's work: that the brain was not a part of the nervous system, it was part of the digestive system (something noted by Ottley). Nor did Owen seem to use Hunter's principle of sympathy between vital parts that explained their interaction, either the "universal sympathy" between the stomach and other body parts, or "peculiar sympathies" between different body parts such as the heart and lungs.[116]

To sum up, Owen cultivated his French contacts; he used French terminology to recatalogue invertebrate specimens (itself a French term) in a larger renovation of what was deemed to be a fatally disorganized Hunterian Museum; and he emphasized how Hunter obtained his anatomical and physiological knowledge analytically and synthetically. In turn Owen became a Professor-Curator himself, recommending certain candidates for new posts and giving testimonials for others. Owen matched his career ambitions with a redefinition of what constituted good anatomical practices.

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[114]Owen, "Observations on Palaeontology," pp. 281-284.
[115]Owen, "Preface," p. 4:xxiv.
[116]J. Hunter, *Observations on Certain Parts of the Animal Oeconomy* (London: 1786), pp. 116-117.

PHYSIOLOGY AS ANALYSIS

At around the same time as the reformation of British museums and comparative anatomy, another type of analytic investigation grew in prominence: the mutilation study. Mutilations studies were thought to disclose the workings of the animal oeconomy by revealing the parts associated with particular functions. The association of a part with a function was in turn revealed by damaging that part.

Though these investigations were often characterized as 'experimental,' I will still portray mutilation studies as following the habit of analysis: this is partly because these investigations did not occur in the institutional setting of laboratories, partly because the researchers were so frequently devoted to finding the simplest elements of an organism's physiology centring upon a ganglion, and partly because these mutilations were used to illustrate findings of comparative anatomy (for instance, the link between individual invertebrate organisms and the internal organs of higher vertebrates). By doing this we can situate mutilation studies in part of a far larger analytica project and discover similarities with other investigations.

One sort of mutilation research examined animals whose mutilation did not cause their death (either immediately or at all). The "tenacity of life" of an organism and its separated parts helped determine its place on the zoological scale: the lower its place, the greater the ability of each part to independently survive after separation. Lower organisms lacked "that bond of union" which meant that damage to one physiological system affected all other physiological systems; in lower animals that union was "less close and decided - so that particular organs may be insulated", allowing the study, and damage, of each organ and physiological system in isolation. Lower animals resisted injuries fatal to higher animals because their organs were often "but repetitions of one another". Cutting articulates into segments caused each segment to writhe individually because each segment had at its centre a ganglion.[117] For instance, Milne Edwards thought that low crustaceans were excellent specimens for vivisection because of their repetitive segments: each one acted the same way as its neighbour.[118]

Contemporaries noted the link between the diffusion of an organism's ganglia and its "tenacity of life". William Baly's 1838-1843 translation of Johannes Müller's *Elements of Physiology* pointed out that lower animals, for example planarian flatworms, could be cut into pieces, and these fragments would turn into new planarians. The production of new individuals occurred because the nervous power was spread throughout its nervous system and not limited to a central organ. Müller therefore asked how many animals immediately above these planarians had a diffused nervous system – posing a question for other researchers to answer. In that same year an anonymous reviewer for the *Medico-Chirurgical Review* noted the general assumption that ganglia were independent seats of nervous energy, allowing organs such as the heart to act independently.[119]

Another type of mutilation research was informed by the belief that organisms had different interior "lives", in the same manner of Aristotle's hierarchy of vegetable, animal and rational soul. How many of the three lives an organism possessed depended

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[117]W. B. Carpenter, *Principles of General and Comparative Physiology*, 1st ed. (London: John Churchill, 1839), pp. 5, 139-140; R. B. Todd, "Physiology of the Nervous System," in *Cyclopaedia* (1847), 720g-723g, pp. 720z-721a.
[118]H. Milne Edwards, "Crustacea," in *Cyclopaedia* (1836), 750-787, p. 753.
[119]"Publications on Anatomy," *M-CR* 28 (n.s.), 361-378 (1838): pp. 365-66; J. Müller, *Elements of Physiology*, trans. W. Baly, 2 vols. (London: Taylor and Walton, 1838-1843), pp. 1:792-793.

upon its complexity, and vivisection revealed their existence. If a researcher removed the anterior brain-lobes of a pigeon, for instance, he destroyed its organs of perception and consciousness, but left its "sentient" and "ganglionic" systems unharmed. The aim of vivisection was to remove an animal's "superstructures", reducing it to the simplest organization. Development was contrasted with mutilation – one researcher commenting that higher animals were built up of three independent forms of existence, as shown by the "actual analysis" by Magendie and Flourens. Therefore "as an animal is built up, so may we contrive carefully to unbuild him again."[120]

Thus vivisectors were often guided by the image of reverse recapitulation. Since higher animals were composed of three distinct forms of existence, properly-conducted vivisections followed the natural process of death, with each type of inner life dying in the reverse order of their appearance during the embryo's development. Vivisection presented a physiological mirror to comparative anatomists' dissections, as mutilation also moved an animal down the scale of being as its higher functions were removed. Reverse recapitulation contextualizes Owen's statement that physiology's primary goal was to know which parts of a physiological apparatus were essential and which ones were "superadded."[121]

The insights gleaned from vivisection and comparative anatomy reinforced one another: one could even draw parallels between internal organs and individual animals. Just as there were "insentient" animals with only ganglionic nerves, so too could their existence be said to be on the same level as the human heart or bowels; the resemblance between body part and individual organism allowed investigators to note that when they

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[120]W. Griffin, "Physiological Problem," *LMG* 24, 74-81; 108-114; 188-196 (1839): p. 190.
[121]R. Owen, Manuscript, "Lectures on the Sympathetic and Nervous System - Rough Outline", 1846, OPAP, RCS.

removed the cerebrum of a higher vertebrate, it was "reduced to the level" of an invertebrate.[122] Randall Albury notes that the work of French researchers such as Xavier Bichat and François Magendie was motivated by the habit of analysis and synthesis: that the organism's life was the sum of its independent elementary parts. Magendie used mutilations to determine how much each organ participated in a physiological function,[123] and his studies can also be seen as trying to determine the relative independence of body parts.

In the 1820s and 1830s French analytical vivisection came to Britain as Magendie held various public lectures and demonstrations in London. There, he mutilated dogs and rabbits in various horrible ways to demonstrate physiological functions. In one visit to the Windmill-Street School of Medicine he showed just how one "laid bare" the roots of the spinal nerves – by cutting an incision in an immobilized dog's back from head to tail, exposing its vertebral arches. The "most deservedly notorious" man then began to pinch, prick and pull its nerve-roots, and the dog began to howl and struggle. As its struggling made his irritations more difficult, Magendie made an attempt to play to the foreign crowd: suddenly looking up at the audience, "as if a bright thought had just struck him," he pointed to the howling dog and exclaimed "Ah! Mon Dieu, il n'entend pas Français."[124] He preferred young puppies for his vivisections – the vertebral column had not yet fully hardened around their spinal cords, facilitating his access to it[125] – a specimen-choice that cannot have endeared him to a considerable portion of the British

[122]W. B. Carpenter, "Noble on the Brain and its Physiology," *BFMR* 22, 488-544 (1846): p. 508; Griffin, "Physiological Problem," pp. 189, 190, 192-193.
[123]Albury, "Experiment and Explanation," pp. 90-91.
[124]"On Experiments on Living Animals," *LMG* 20, 804-808 (1837): pp. 805-806; R. French, *Antivivisection and Medical Science in Victorian Society* (Princeton: Princeton University Press, 1975), p. 20.
[125]In that the vertebral bones did not have to be broken open. P. Cranefield, *The Way In and the Way Out : François Magendie, Charles Bell, and the Roots of the Spinal Nerves* (Mt. Kisco, NY: Futura, 1974), p. 11.

public. Various commentators were outraged by Magendie's methods: William B. Carpenter called Magendie's vivisections "barbarous", and Richard Grainger believed that Magendie subjected his specimen animals to pain with little goal or reason. Thus it was not always useful to be French: one medical review, the conservative *London Medical Gazette*, held up Magendie's work as an example of Gallic cruelty.[126]

In turn the most prominent British vivisectionist of the time was Charles Bell. He was yet another Edinburgh graduate who moved to London in 1804, lecturing at the Great Windmill Street School of Anatomy and the Middlesex Hospital.[127] Like Magendie, Bell also sought to determine the basic elements of an animal. One example was his differentiation of the separate properties of nerve fibres. In his *New Anatomy of the Brain* - privately circulated in 1811 to various acquaintances - he attacked the "prevailing doctrine...that the whole brain is a common sensorium", and instead he proposed that nerves were bundles of smaller nerves. Some were nerves of motion, some of sense, and some were "vital". Bell determined these properties through vivisections (usually of dogs or rabbits), in which he laid bare the "roots" of the spinal nerves and cut various ones to see whether this cutting convulsed the back muscles of the subject animal.[128]

Sometimes vivisection could be avoided by citing virtual exemplars instead. Bell cited findings from comparative anatomy, stating that it was always best to begin with the simplest animals and then move to more complex ones, and he specifically cited polyps

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[126]"On Experiments on Living Animals," pp. 805-806; D. E. Manuel, "Marshall Hall (1790-1857): Vivisection and the Development of Experimental Physiology," in *Vivisection in Historical Perspective*, ed. N. A. Rupke (London: Croom Helm, 1987), 78-104, pp. 94-95.
[127]P. Amacher, "Charles Bell," in *Dictionary of Scientific Biography*, ed. C.Gillispie (New York: Scribner's, 1970), 583-584, pp. 583-584; N. Moore, "Charles Bell," in *DNB* (1885); Rupke, *Richard Owen*, p. 177.
[128]C. Bell, *Idea of a New Anatomy of the Brain* (London: Dawson's Reprints, 1966), p. 4.

and starfish as examples of simple animals.[129] Another popular virtual exemplar – relevant to understanding the human brain – was the "acephalous" infant, a child missing its cerebrum. One researcher both in 1833 and 1842 gave William Lawrence's example of an acephalous infant who lived for a while after birth, suckling, urinating and breathing naturally; for him the case of the acephalous infant was directly analogous to Magendie's vivisections in which animals with removed cerebral parts also lived for a time. In 1833 Bell also cited the acephalous child, showing how the medulla oblongata was the foundational mental organ: for though an infant could live without parts of the cerebrum or cerebellum, it would die without the medulla oblongata.[130] An 1839 physiological textbook reported on the "interesting example" of infants born without brains: they had only the functions of organic life, allowing the parallel to be drawn between the acephalous infant and the undirected movements of the articulates. And one of Robert B. Todd's own articles in his *Cyclopaedia* mentioned the simplest functions of an "acephalous monster" too.[131]

Though Bell performed some vivisections, he tried to distinguish himself from cruel Frenchmen like Magendie by advertising his reluctance to mutilate living animals. When mutilation had to be done in order to determine a nerve's function, Bell announced how he cut the spinal cord just below the medulla oblongata so that the animal felt no pain below that cut. When Magendie claimed that he had first shown (in 1821) that the anterior roots of the spinal nerve were motor nerves and the posterior roots were sensory,

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[129]C. Bell, "Lectures on the Physiology of the Brain and Nervous System," *London Medical and Surgical Journal* 1, 682-685, 752-757 (1832): pp. 684, 753.
[130]He added that in order for the analogy to hold, the brain would have to be removed from front to back, from beginning with the cerebrum and then moving backwards, to the cerebellum. H. Mayo, *Outlines of Human Physiology*, 3rd ed. (London: Burgess and Hill, 1833), pp. 228-230; idem, *The Nervous System and its Functions* (London: John W. Parker, 1842), pp. 21-24. C. Bell, "Lecture 14 on the Hunterian Preparations," *L* 1, 878-881 (1833): p. 880.
[131]Carpenter, *Principles of Physiology*, p. 457; Todd, "Physiology of the Nervous System," p. 720y.

Bell resorted to his privately circulated *New Anatomy of the Brain* (1811) to establish his priority. And he and his supporters further promoted Bell's hostility to cruelty to animals to further his claim, capitalizing upon prevailing British sentiments.[132]

The 1821 finding over which Bell and Magendie fought (which became known as the "Bell-Magendie law") proposed that nerves were functionally different. The motor nerve excited action, and the sensory nerve conveyed "the sense of that action". Because of the functional difference between motor and sensory nerve, it was thought that a "nervous circle" had to exist between a part of the brain and a muscle in order for there to be muscular activity.[133] Regardless of who first made the discovery, much of the period's research into the nervous system – exceptionally complex and loaded with arcane points - can be better understood by seeing it in the context of compound individuality. Between 1820 and 1850 the nervous system came to be seen as structurally differentiated and complex, a mass of coalesced nerves and ganglia. Likewise, researchers came to see that nervous activity was not controlled by the will as they extended the definition of a "ganglion" to include the grey nervous masses in vertebrates.[134]

Devices such as the Bell-Magendie law emphasized the primary neuroanatomical element of the ganglion. Meanwhile the notion of a unitary nervous centre – the *sensorium commune* – was increasingly discredited. It was held to be the centre where volition was located, or "seated," to command the rest of the body. One critic caricatured

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[132]"On Experiments on Living Animals," pp. 806-807; French, *Antivivisection*, p. 19.While antivivisectionist sentiments were supposed to increase the study of anatomy instead, with comparative studies of organs and their histories replacing vivisection, it was still held to be too important to give up. Despite his criticisms of Magendie's methods, Carpenter was later to admit that not a single point of physiology had been made without it. Carpenter, "Noble on the Brain," pp. 492-493; Desmond, *The Politics of Evolution*, pp. 189-191.
[133]Amacher, "C. Bell," pp. 583-4; Moore, "Charles Bell"; G. Rice, "The Bell-Magendie-Walker Controversy," *Medical History* 31, 190-200 (1987): pp. 190-191.
[134]Jacyna, "Principles of General Physiology," pp. 50-51.

the image of the *sensorium commune* as a "central apartment for the superintendent of the human panoptican [sic]".[135] Bell, in hindsight, saw that the notion of a unitary and central *sensorium commune* was perfectly "natural" to earlier researchers. But he concluded that anyone still holding the view of a unitary centre in 1832 had only a superficial grasp of anatomy: instead the spinal marrow should now be seen as "rather a prolonged brain" as it was a regular succession of ganglia.[136]

Comparative anatomy, especially the examination of the dispersed ganglia in lower organisms, enabled researchers' questions about a unitary *sensorium commune.* By the 1840s the relative importance of parts of the nervous system was actually reversed. Where at first the spinal cord was seen to be a mere extension of the brain, relaying its commands to the rest of the body, the brain came to be seen instead as an extension of the spinal cord. In turn researchers came to see the spinal cord, and the brain that it extruded, as made out of so many semi-autonomous ganglia.[137]

The traditional nervous hierarchy was disrupted and nervous agency decentralized. One could further bring up an analogy between nervous and political *authority* in order to make connections with the larger British cultural context. Nervous authority was becoming decentred - analyzed into its elements and distributed throughout the body – in a process strangely imitating the contemporaneous perception of those Reforming times that political authority was devolving, or should be devolving, to a larger and larger portion of the British population. Social order can be seen as an

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[135]W. Lawrence, *Lectures on Physiology, Zoology, and the Natural History of Man* (London: J. Callow, 1819), p. 87.
[136]Bell, "Physiology of the Brain," pp. 682-683, 753.
[137]E. Clarke and L. S. Jacyna, *Nineteenth-Century Origins of Neuroscientific Concepts* (Berkeley: University of California Press, 1987), pp. 30-1; L. S. Jacyna, "Somatic Theories of Mind and the Interests of Medicine in Britain, 1850-1879," *Medical History* 26, 233-258 (1982): p. 235.

expression of a deeper cognitive order:[138] the habit of analysis and synthesis might be seen as the shared code in which calls for increasing representation were made or rejected. In politics the struggle was over what constituted the basic element of political authority – on one side there were radical democrats calling for the enfranchisement of its (male) citizens; in the middle stood those who agreed with this devolution, though with various qualifications; on the other side those who supported the status quo. Nonetheless the idiom of analysis and synthesis structured the possibilities that this debate took.

In the study of the nervous system there was a similar process of devolution, supported by some, resisted by others. Struggles over what constituted the basic unit of nervous authority resulted. As the notion of a central point of the brain issuing commands to the rest of the body was discredited, the diffused and quasi-independent ganglia gradually gained more power and independence. Ganglia were even called brains themselves: Cambridge University's William Clark announced in his 1835 British Association Report that "[T]he ganglia are to be considered as so many subsidiary brains, which continually supply the parts to which they distribute their nerves with new impulses and fresh activity, without immediate dependence upon the brain".[139]

One of Bell's former pupils, Herbert Mayo, was also animated by questions about the disunity of the organism. He had studied under Bell between 1812 and 1815 at his Great Windmill School of Anatomy, eventually buying out Bell's position in the school. In 1814 he followed Bell to Middlesex Hospital, where he became one of its "brightest intellects"; in 1818 he became its house surgeon.[140] Mayo compared the single brain of

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[138]B. Barnes, *The Elements of Social Theory* (Princeton: Princeton University Press, 1995), pp. 97-98.
[139]W. Clark, "Report on Animal Physiology," *BAAS*, 95-142 (1835): pp. 102-103.
[140]J. F. Clarke, *Autobiographical Recollections of the Medical Profession* (London: J. & A. Churchill, 1874), p. 18; Z. Cope, *The Royal College of Surgeons of England: a History* (London: A. Blond, 1959), p.

vertebrates to the numerous 'brains' of the invertebrate nervous system; like Magendie and Bell, he often arrived at these comparisons through vivisection or dissection. In his *Outlines of Human Physiology* - a book composed of lectures he had given at his Windmill-Street school in 1827, and which had a good run as a student textbook until the late 1830s[141] - Mayo frequently remarked on the independence of body parts separated from the rest of the body. These parts were occasionally seen to be "sentient". Usually all nervous parts had to be connected with a particular part of the medulla oblongata, but sometimes nervous energy still existed after a part was separated from the medulla oblongata.

For example, when Mayo killed a pigeon and removed its head, he separated the cerebrum, cerebellum and medulla oblongata from the optic nerve, and pricked the optic nerve, causing the iris to react. He concluded that in vertebrates each segment of the spinal cord, and the nerves sent from it, was comparable to the fused ganglia-pair of each invertebrate segment. Radiates (several segments around a circle) and articulates (a successive jointed series) were therefore animals which could live after "mechanical division", suggesting that one mutilated Radiate or Articulate could become two "sentient beings". A starfish and centipede were a "composite type of organization" because their

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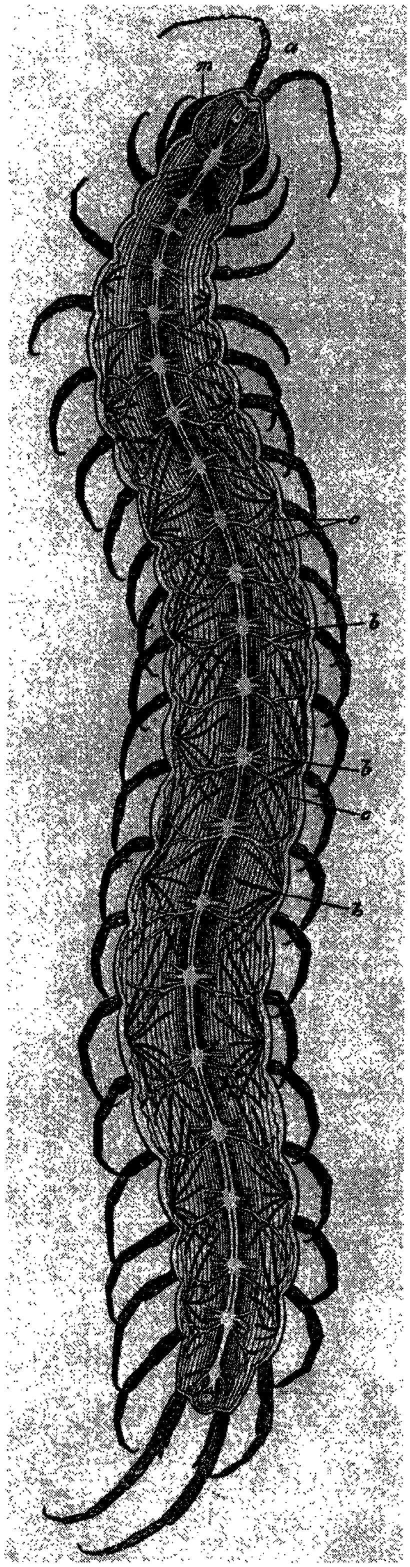
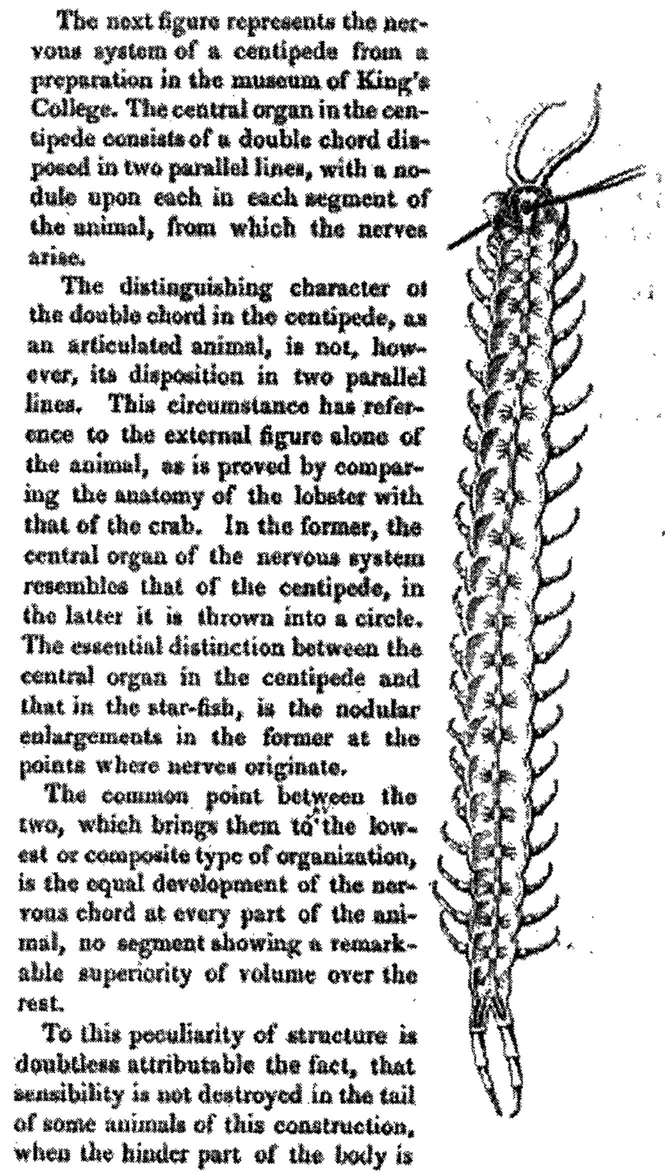
47; G. Gordon-Taylor and E. W. Walls, *Sir Charles Bell, his Life and Times* (Edinburgh: E. & S. Livingstone, 1958), pp. 129-130; C. H. Mayo, "Herbert Mayo (1796-1852)," in *DNB* (1894).

[141] Jacyna, "Principles of General Physiology," p. 76; Mayo, "H. Mayo." When released, Carpenter's *Principles of Physiology* had no rival in the London medical schools, quickly supplanting Mayo's *Physiology;* it was also seen as an easier book than William Baly's 1838 translation of Johannes Müller's *Elements of Physiology*. W. B. Carpenter and J. E. Carpenter, *Nature and Man : Essays Scientific and Philosophical* (New York: Appleton, 1889), p. 65.

nervous cords were equally developed along the entire length of the animal.[142]





The next figure represents the nervous system of a centipede from a preparation in the museum of King's College. The central organ in the centipede consists of a double chord disposed in two parallel lines, with a nodule upon each in each segment of the animal, from which the nerves arise.

The distinguishing character of the double chord in the centipede, as an articulated animal, is not, however, its disposition in two parallel lines. This circumstance has reference to the external figure alone of the animal, as is proved by comparing the anatomy of the lobster with that of the crab. In the former, the central organ of the nervous system resembles that of the centipede, in the latter it is thrown into a circle. The essential distinction between the central organ in the centipede and that in the star-fish, is the nodular enlargements in the former at the points where nerves originate.

The common point between the two, which brings them to the lowest or composite type of organization, is the equal development of the nervous chord at every part of the animal, no segment showing a remarkable superiority of volume over the rest.

To this peculiarity of structure is doubtless attributable the fact, that sensibility is not destroyed in the tail of some animals of this construction, when the hinder part of the body is

T.R. Jones, "Myriapoda," in *The Cyclopaedia of Anatomy and Physiology*, edited by R.B. Todd (London, 1837), p. 550. H. Mayo, *Outlines of Human Physiology*, (4th edition), 1837, p. 222.

Notice the similarity of the depictions – this is probably because Jones, who with Mayo taught at King's College, used the same museum specimen. It is also possible that one copied the other's picture.

[142]Mayo, *Human Physiology*, pp. 230-231, 220-222.

Mayo became a researcher of some repute, being promoted to surgeon of the Middlesex Hospital in 1827; he became Professor of Surgery at the Royal College of Surgeons in 1828, and Professor of Anatomy at King's College in 1830, where he lectured on anatomy and physiology five days a week during the school year.[143] In later work on the nervous system he continued to insist that there was no centre of a nervous system: there was no *sensorium commune*, not even the segment of the medulla oblongata with its roots in the fifth pair of nerves, as Magendie had said (because the separate segments of the spinal cord possessed independent powers).[144]

Since the nervous system was a collection of quasi-independent ganglia, the researcher could profitably study the lower invertebrates because they best displayed these diffused nervous centres. For instance, the nervous system of millipedes (a member of the group Articulata) or a starfish (a member of the group Radiata) could be portrayed with a simple model, using a bead to stand for a ganglion and silk thread to stand for the nerves emanating from that ganglion. Each segment had one bead, so a five-rayed starfish had five beads.

> Then pass a thick silk through the five beads, and having so strung them, fasten them at regular intervals, and tie together the ends of the string. The compound central nerve-organ would be represented by the silk and beads: the beads would represent the sensorial nuclei; the lateral tufts of threads would represent the nerves. It is evident that such a chaplet of beads might be laid either in a circle or drawn out in two parallel lines.

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[143]"Account of the Metropolitan Hospitals, Medical Schools, and Lectures for the Session Commencing October 1832," *L* 1, 3-11 (1832-1833): p. 5. Unfortunately Mayo tried to move from the Anglican King's College to the "godless" University College in 1836, and didn't succeed, so he also had to withdraw from King's too. Mayo, "H. Mayo."

[144]Mayo, *The Nervous System*, pp. 69, 71.

When the beads were arrayed in a circle, the model represented radiates. When laid out in parallel, it stood for articulates.[145] Because of their decentralized nervous structure, both articulates and radiates were compound animals, each one a "multiple animal" that had become "individualized" with each segment still capable of independent survival. This idealized model of the nervous system shows Mayo's experiences conveying the structure of the nervous system to a class – such a simple model could also be applied to other animals quite easily.

Mayo appears to have applied these models to other animals as he extended his notion of compound individuality to vertebrates. He even mused about whether consciousness itself was surgically divisible.

> Each lateral half of a vertebral animal is separately vitalized. Or the preservation of consciousness in one half is independent of its preservation in the other...Is it then possible, that by exactly severing in the median plane the two halves of the vitalizing segment, a vertebral animal might be made, temporarily, two separately conscious beings?[146]

To his dreamy proposal a reviewer invoked the Judgement of Solomon, tartly hoping that "[p]erhaps Mr. Mayo will try." He denied the possibility of two separate volitions in the same person. For

> Volition is either absolute or it is not. If it is, there can be no subdivision of it - if it is not, it is no longer volition. Two volitions in the frame, a central and a departmental one, would be very likely to fall out. Indeed we do not see how they should agree. The mental volition would be a kind of abstraction, willing that we should go to bed - while the cranio-spinal volition would be the means of putting on our night cap. So it would come to pass that the cranio-spinal segment would enjoy too opposite properties excited at the same time and in the same way - it would feel and it would not feel - it would will and it would not will, in short it would be an intelligent and a reflex centre. This is more than we can believe.[147]

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[145]Mayo, *The Nervous System*, pp. 15-16.
[146]Mayo, *The Nervous System*, pp. 14-16, 28-29, 57.
[147]Review of *The Nervous System and its Functions* by H. Mayo, *M-CR* 37 (n.s.), 16-40 (1842): pp. 20-24.

For the reviewer, the divisibility of volition was impossible because it divided internal authority.

Sadly, Mayo would lose interest in the nervous system. A year after the appearance of his *Nervous System and its Functions* he was incapacitated by "rheumatic gout" and he could no longer lecture at the Middlesex Hospital. He moved to Germany where he became a physician in a spa. An otherwise favourable biographer sniffed that near the end of his life Mayo threw "himself into the hands of the mesmerists", any prospect of a relief from his pain outweighing the threat of heterodoxy.[148]

THE REFLEX ARC AS NEUROPHYSIOLOGICAL ELEMENT

It is also in the context of compound individuality that one can understand anew the theory of the reflex arc. It can be seen as formulated in response to various questions: why could separated parts move independently? Was sensation and volition confined to a particular nervous centre, or was it distributed throughout the body? If certain nervous centres were removed, which functions would be removed? Instead of seeing the rise of the reflex arc in well-worn oppositions between materialism and vitalism, mechanism and idealism, the arc can be seen in the light of how nervous power was distributed throughout the body. It emerged amidst questions about which nervous centre was truly the brain, which nervous centres in turn were commanded by that brain, and what was the true "seat" of the soul, or volition, or consciousness.

In 1830, Marshall Hall began to formulate his notion of "diacentric" action (the word entailing a relationship between a centre and its periphery) when he cut a newt into

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[148]Mayo, "H. Mayo."

four pieces and then irritated the skin of these different parts with a needle. The separated tail "moved as in a living animal" when irritated, seemingly independent of volition.[149] Another product of the University of Edinburgh medical school (graduating in 1812), upon graduation Hall had also toured medical schools in Göttingen, Berlin and Paris. Nottingham, where he moved in 1817, was too small for someone of his ambition: in 1826 he moved to London, and over the next five years his income rose from £800 to £2,200.[150] Hall's goals were not simply monetary, however: as his pecuniary fortunes increased he also sought a great scientific reputation. Cutting doomed newts into pieces was one way to enhance his standing.

Hall began with the newt because his earlier researches into the circulatory system showed him that they had a low rate of respiration, meaning that they lived longer during vivisection.[151] He was also copying continental physiologists such as C.J.J.C. Legallois and Johannes Müller: they favoured cold-blooded animals because they were easier to work with, lived longer during vivisection, and showed "remarkable irritability." Their parts and organs also had a strange life of their own after being cut from the rest of the body - heads of turtles and snakes could bite even days after being severed, for example, and a frog's heart could move for hours after being removed from its body.[152]

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[149]C. Hall, *Memoirs of Marshall Hall* (London: R. Bentley, 1861), pp. 85-86; M. Hall, "A Brief Account of a Particular Function of the Nervous System," *Proceedings of the Committee of Science and Correspondence of the Zoological Society of London* 1, 1830-31): p. 190; D. E. Manuel, *Marshall Hall (1790-1857): Science and Medicine in early Victorian Society* (Amsterdam: Rodopi, 1996), pp. 243-244.
[150]G. T. Bettany, "Marshall Hall (1790-1857)," in *DNB* (1890).
[151]Manuel, *Marshall Hall*, p. 189.
[152]In frogs the spinal canal was wider, allowing the researcher to more easily get at the roots of the nerves. "On Experiments on Living Animals," p. 807; Manuel, *Marshall Hall*, p. 189; Todd, "Physiology of the Nervous System," p. 720u. Cold-blooded animals were less affected by blood loss and could live through injuries that would kill warm-blooded animals: frogs, for instance, could live for a time after their lungs were removed because they could also breathe through their skin. "Physiology," in *Oxford Encyclopaedia* (Oxford: Thomas Kelly, 1828), pp. 5:442-443; Carpenter, *Principles of Physiology*, p. 139.

Hall investigated the strange irritability of separated parts and organs. When he divided the spinal marrow of a snake between its second and third vertebrae, it "lay tranquil and motionless" – but when he irritated a segment it moved for a time, as each of its movements was equivalent to a new stimulation, which caused it to move again, and that movement meant a new stimulus, and so on. Hall concluded that the will ceased to act when the head, or brain, was removed[153] – and that conversely there was a part of the nervous system that had nothing to do with the brain, and therefore nothing to do with the will or consciousness. This section of the nervous system lacking a brain, that Hall called the "excito-motory system", was centred upon the "true spinal marrow"; the motions of the separated body segments didn't need to be connected to a central brain, because they were reflex arcs. Hall was to define the reflex arc as a sort of muscular excitement mediated by the medulla (the "marrow") of the spine. Upon the excitement of one area of the body, usually by outside forces, this excitement would 'move' "in a reflex course", always passing through the medulla, and stimulate the movement of a different area of the body.[154]

Despite insisting upon the reflex arc's independence from volition and irritability, Hall nonetheless set the reflex arc firmly within a hierarchy of vital systems, proposing five of these arranged from the most basic to the "highest". At the lowest point he placed the sympathetic system, responsible for functions that included secretion; then the nervo-muscular fibre, responsible for irritability; then the "medulla generally", which mediated

[153]M. Hall, *On the Reflex Function of the Medulla Oblongata and Medulla Spinalis* (London: Joseph Mallett, 1833), pp. 14-17.

[154]E. Clarke, "Marshall Hall," in *Dictionary of Scientific Biography*, ed. C. C. Gillispie (New York: Scribner's, 1972), 58-61, pp. 59-60; Hall, *On the Reflex Function*, pp. 39-40; M. Hall, "On the Reflex Function of the Medulla Oblongata and Spinalis, or the principle of Tone in the Muscular System," *Abstracts of the Papers printed in the Philosophical Transactions*, 210 (1837): p. 210.

the reflex arc; then the medulla oblongata as the source of respiratory motions; and at the highest point the cerebrum, the source of voluntary motions. Over time he continually revised the number of his systems, but he retained them in a hierarchy, for they allowed him to explain reverse recapitulation, showing how the proper functioning of the higher systems depended upon the lower systems. Thus Hall noted that in the dying hedgehog, the voluntary motions stopped first, then the respiratory motions, and finally the sympathetic functions:

> Such is the order, then, in which this series of functions disappear in death; an order which is inverted when the same functions and their appropriate organs gradually come into existence, in the foetal and natal states, and in the progressive series of the animal kingdom.[155]

Though charged with materialism, Hall continued to insist upon the existence of an immaterial "soul" and actually considered his work on the reflex to be a vindication of this anti-materialism.

Despite the ability of Hall's reflex arc to colligate other researchers' observations and answer their questions about the disunity of the organism, it took a while for his device to be fully accepted in Britain. One reasonable explanation for the delayed acceptance of the reflex arc was his personality, which continues to divide historians today. While L.S. Jacyna and Edwin Clarke call Hall a man of immense conceit - "evincing a paranoia" - both quarrelsome and possessive about his claims to priority, Diana Manuel's portrait is more sympathetic: she portrays instead a scientific pugilist railing against the corruption of the Royal Society and medical and surgical colleges.[156]

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[155]Hall, *On the Reflex Function*, pp. 45-47.
[156]Clarke and Jacyna, *Neuroscientific Concepts*, pp. 114-115; Manuel, *Marshall Hall*, pp. 156-158. I favour Clarke and Jacyna's view, particularly in the light of the wealthy Hall's persecution of the penniless and slightly pathetic George Newport.

We do know that Hall had a flair for self-promotion that fed into the period's spirit of disintermediation. "On the Reflex Function of the Medulla Oblongata and Medulla Spinalis" appeared in the *Philosophical Transactions* (being read on 20 June 1833), and he published it independently that same year, where it proudly announced its origin "From the Philosophical Transactions".[157] Hall, already friends with the rabble-rousing Thomas Wakley, editor of the *Lancet*, copied the *Lancet's* successful publishing example. Like Wakley's attempts to go around the teachers at the medical schools, publishing their lectures to a vast and curious audience made newly possible by cheaper and more widespread medical journals, Hall also seems to have tried an end-run of his own. He denied the Royal Society's ability to adjudicate and validate his findings, going to the 'people' instead to have his reflex arc accepted. And just like Wakley,[158] he infuriated the members of the spurned institution.

What happened next has been frequently repeated. By Manuel's account a subsequent 1837 paper of Hall's[159] was refereed by Herbert Mayo and William Lawrence, who rejected it for publication because it had too many details that had already been published. (Mayo had earlier supported Hall's application to become a member of the Royal Society).[160] They also suggested it contained basic mistakes that they thought should not be given credibility by their publication in the *Transactions.* Hall - further insulted by what he perceived to be a slight by Peter Mark Roget, then-secretary of the Society - instead published his paper himself as before, and proceeded to give what he purported to be all of his correspondence to Wakley. Wakley - never missing an

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[157]Hall, *On the Reflex Function.*
[158]*Report of the trial, Cooper versus Wakley, for Libel* (London: S. Highley, 1829), pp. 9, 76-80.
[159]M. Hall, "On the Function of the Medulla Oblongata and Medulla Spinalis, and on the Excito-motory System of Nerves," *Abstracts of the Papers printed in the Philosophical Transactions*, 463-464 (1837).
[160]Manuel, *Marshall Hall*, pp. 224, 190-191.

opportunity to attack a "corrupt" institution while stoking a controversy likely to raise the *Lancet's* sales - published the letters amidst various fire-breathing editorials about corruption.[161] Hall's independently published pamphlet, *Memoirs of the Nervous System,*[162] prompted critics and enemies to attack him for plagiarizing from earlier researchers. Supported by the *Lancet,* he was in turn mocked by the editors of the conservative *London Medical Gazette* (which had been founded directly in response to the *Lancet* and Wakley in 1827). And his supporters in the Radical community - particularly the *Lancet* and the new reforming British Medical Association - championed his reflex arc.[163]

The ideological picture about reflex arc acceptance is not monolithic, however. The *London Medical Gazette* published one of Hall's 1836 lectures on the reflex arc,[164] and the next year that journal published Michael Faraday's exposition of the reflex theory to a fascinated audience at the Royal Institution. (Faraday's interpretation paid a great deal of attention to how the reflex theory explained the independence of various body parts: for instance, while the brain sometimes slept, the spinal marrow never slept).[165] Hall stated and re-stated the utility of the reflex arc, and his claim to have discovered it, in various private letters to figures such as Owen,[166] to journals, and in pamphlets[167] for the next decade. According to one's perspective on Hall, then, he either heroically (or

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[161]Manuel, *Marshall Hall,* pp. 192-193.
[162]Hall, *Memoirs on the Nervous System* (London: Sherwood, Gilbert and Piper, 1837).
[163]"Marshall Hall Again," *LMG* 21, 985-986 (1837-1838); "Complete Anticipation of Dr. Marshall Hall's Doctrine of 'The Reflex Function,' by Prochaska," *BFMR* 5, 623-625 (1838); Clarke and Jacyna, *Neuroscientific Concepts,* pp. 118-119; Desmond, *The Politics of Evolution,* p. 129.
[164]M. Hall, "Dr. Marshall Hall on the Nervous System," *LMG* 17, 632-641 (1836).
[165]"Faraday's Exposition of Marshall Hall's Reflex Action of the Spinal Marrow," *LMG* 19, 828-829 (1837).
[166]"...the conduct of the Council towards me...[has been] absolutely *brutal*" M. Hall to R. Owen, 12 July 1837, OCORR, NHM, 14/220-221. Emphasis in original.
[167]M. Hall, *A Letter addressed to the Earl of Rosse, President-Elect of the Royal Society,* 2nd ed. (London, 1848).

paranoically) fought off various attacks until he defeated his opponents (or simply exhausted them through sheer bloody-minded ranting persistence).

One supporter, the anatomist Richard Grainger, defended Hall's reflex doctrine upon anatomical grounds.[168] Having run medical schools from the age of 22, teaching at such places as St. Thomas's Hospital, Grainger noted the existence of two different types of nervous matter. One was grey, a source of nervous power; the other was white, a fibrous substance transmitting that nervous power.[169] Grey nervous matter was found mainly in the ganglia of the sympathetic nervous system, the ganglia of the spinal / nervous cord, and in the cerebrum. Meanwhile the white "transmitting" nervous fibres that didn't end in the grey matter of the sympathetic ganglia or spinal / nervous cord instead went directly to the grey matter of the cerebrum.[170]

For Grainger the termination of white nervous fibres in the cerebrum explained voluntary control over body motions: the highest animals had a greater proportion of white transmitting fibres going straight into the cerebrum. In the case of simpler animals with no cerebrum, their fibres ended only in the sympathetic ganglia or nervous cord, explaining why their body parts acted especially independently (in other words, they showed only reflex activity). Thus a higher level of nervous development meant a higher level of voluntary control.[171] Though Grainger used findings from comparative anatomy to support a doctrine that was rooted in the disunity of the organism, he also cited

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[168]Review of *Observations on the Structure and Functions of the Spinal Chord* by R.D. Grainger, *L* 2, 127-128 (1838).

[169]"Account of the Metropolitan Hospitals," p. 4; "Some Recent Publications on Anatomy," *M-CR* 28 (n.s.), 116-136 (1838): p. 122; G. T. Bettany, "Richard Dugard Grainger (1801-1865)," in *DNB* (1890); Desmond, *The Politics of Evolution*, p. 422; R. D. Grainger, "Illustrations of the Medical Uses of Comparative Anatomy," *L* 1, 93 (1842-1843): p. 93.

[170]R. Leys, *From Sympathy to Reflex: Marshall Hall and his Opponents* (New York: Garland, 1990), pp. 301-302.

[171]R. D. Grainger, *Observations on the Structure and Functions of the Spinal Cord* (London: S. Highley, 1837), pp. 42-44.

anatomical findings which firmly placed this disunity in a hierarchical scheme. It portrayed a concentrated group of ganglia in the cerebrum as best able to control the rest of the body, showing the peripheral parts to be dependent upon a central and directing part.

REFLEXES, INDEPENDENT GANGLIA, AND INVERTEBRATE VIVISECTIONS

Also strengthening Hall's case was the establishment of a correspondence between the serial ganglia of the vertebrate spinal cord and the serial ganglia of the invertebrate nervous system. It was claimed that insect nervous cords were composed of two columns, one possessing ganglia (making them sensory) and the other not possessing them (making them motor). This distinction between the two nervous columns followed Bell's work, and so the presence or absence of ganglia on nervous cords was deemed analogous to vertebrates' cords.[172]

Though George Newport was frequently given credit for the discovery of the two separate columns,[173] it resulted in some ugly priority disputes. Newport's life was poignant: apprenticed as a wheelwright, Newport used the Canterbury Philosophical and Literary Institution to study entomology and elevate his own social standing. After being apprenticed to a Sandwich surgeon, he came to London in 1832 and was introduced to Robert Grant. Grant was interested in Newport's previous entomological work, asking him questions about the insect nervous system. Because of his "humble origins and

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[172]Leys, *From Sympathy to Reflex*, p. 307; G. Newport, "On the Respiration of Insects," *Phil. Trans.* 126, 529-566 (1836): p. 541.

[173]"Obituary Notice of George Newport," *Proceedings of the Royal Society of London* 7, 278-285 (1855): p. 283; W. F. Erichson, "Insecta," in *Reports on Zoology for 1843-1844* (London: Ray Society, 1847), 116-194, pp. 117-118; Leys, *From Sympathy to Reflex*, p. 307.

adverse circumstances of his upbringing", Grant sought to help Newport, giving him free tickets to two of his University College lecture courses, and he pushed other professors to give him free admission as well. Grant even encouraged Newport to write a paper on insect anatomy, and so his "first juvenile essay" appeared - in the *Philosophical Transactions.*[174] (The content of this celebrated early work - on the development of the insect nervous system during metamorphosis – is examined in the next chapter).

Grant would later ruefully declare that Newport's difficulties and misfortunes were not impediments to his advancement – but had actually accelerated his career,[175] for after having accepted Grant's help, Newport moved on to cultivate more important patrons. The "wily personage now shunned my sight", complained Grant, as Newport defected from his circle and became client to more powerful mentors such as Sir Charles Bell and Peter Mark Roget.[176] When in 1836 Newport announced his findings on the sensory and motory nervous system in invertebrates, Grant and Marshall Hall complained fiercely, with Grant claiming this discovery too. Their increasingly aggressive attacks on Newport's character led to his public retorts, and they fought for six months on the pages of the *Lancet* and *Medical Gazette.*[177] Though Newport would eventually receive two

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[174]"Obituary of George Newport," pp. 278-280; R. E. Grant, "Further observations on Dr. Hall's Statement regarding the Motor Nerves of Articulata," *L* 1, 897-900 (1837-1838): p. 898; M. Hall, "Letter from Dr. Marshall Hall to Mr. Newport," *L* 1, 748-749 (1837-1838): p. 749; G. Newport, "Mr. Newport's reply to Prof. Grant and Dr. Marshall Hall," *L* 1, 812-817 (1837-1838): pp. 812-813; idem, "Mr. Newport's second reply to Marshall Hall," *L* 1, 950-952 (1837-1838): p. 951.

[175]Grant, "Further Observations on Articulata," p. 898. (Newport retorted that Grant's own financial woes should have taught him not to scoff at the difficulties of others). G. Newport, "Mr. Newport's Second Reply to Professor Grant," *L* 2, 118-120 (1837-1838): p. 120.

[176]R. E. Grant, "Reply to Mr. Newport's Insinuations respecting the writings of Dr. Marshall Hall and Dr. Grant," *L* 1, 746-748 (1837-1838): p. 747.

[177]"The Rival Discoverers," *LMG* 21, 903-906 (1837-1838); G. Newport, "On the Anatomy of Certain Structures in Myriapoda and Arachnida which have been Thought to have Belonged to the Nervous System," *LMG* 21, 970-973 (1837-1838). Janet Browne has noted that Grant had a history of taking young protégés under his wing and then appropriating their findings. When a young Charles Darwin told Grant he was going to announce some observations on eggs, Grant warned him not to do this and then publicly announced Darwin's findings himself with only a minor acknowlegement. He also did this with John

medals from the Royal Society and be "esteemed in all lands" by entomologists, physiologists and anatomists, in 1846 he still lived in a single tiny bedchamber, deeply impoverished and described by a shocked foreign visitor as "in want of all things".[178]

Like the fights between Magendie, Bell and Mayo, however, one can move past these vicious priority disputes to reveal a deeper assumption about the disunity of the organism. One reviewer was to state in 1842 that the repetition of parts and segments made it important to understand reflex actions. And the view of a similarity between vertebrate and invertebrate nervous systems allowed researchers to deploy the reflex arc in order to explain the independent movement of body parts in either animal.[179]

The use of reflex arcs to explain independent body part movement was shown in the case of William B. Carpenter, one of Newport's allies. A Unitarian hailing from Bristol, Carpenter went to London and became an apprentice at Middlesex Hospital in 1834. With Newport as a classmate, Carpenter took a "peculiar interest" in Robert Grant's comparative anatomy classes; in 1835 he was awarded Grant's certificate of merit.[180] Privately he consoled Newport after various attacks by "the clique of detractors in the pay of our friend Marshall Hall" (which must have included Grant). Carpenter then went onto medical school at Edinburgh[181] where he wrote his 1839 "Dissertation on

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Coldstream, using Coldstream's own findings in a paper. J. Browne, *Charles Darwin: A Biography*, 2 vols., vol. 1: Voyaging (Princeton: Princeton University Press, 1996), pp. 86-87.

[178]A. Retzius to J. Forbes, 4 Aug 1846, Newport Correspondence, Linnean Society of London, 48. The next year, supported by testimonials from people like Richard Owen, James Scott Bowerbank, W.B. Carpenter, J.E. Gray, Hermann Burmeister and John Forbes, Newport received a £100 a year pension from the Queen. He became ill after collecting specimens in a London swamp, and died in 1854.

[179]"Review of *The Nervous System and its Functions*," p. 18; Erichson, "Insecta," pp. 117-118.

[180]Testimonial for George Newport, Newport Correspondence, Linnean Society of London, 11; Carpenter and Carpenter, *Nature and Man*, p. 10. Desmond, *The Politics of Evolution*, pp. 212-216.

[181]W.B. Carpenter to G. Newport, 1 Jul 1847, Newport Correspondence, Linnean Society of London, 12; Carpenter and Carpenter, *Nature and Man*, pp. 15, 18, 26.

the Physiological Inferences to be Deduced from the Structure of the Nervous System in the Invertebrated Classes of Animals".

But Carpenter's dissertation was not simply an application of Hall's reflex theory to the invertebrates,[182] it was also an answer to the question of the seeming-autonomy of body parts. Grainger had already remarked two years before on the odd finding that cephalopod suckers moved "independently of volition"; likewise, an articulate like a decapitated housefly remained in its natural position while its limbs twitched, and a decapitated mole cricket walked a considerable distance. Carpenter's dissertation was presented as an answer to why motion could be excited in a single, separated, articulate segment: because both vertebrate and invertebrate nervous systems were segmented.[183]

Contemporary interpreters of the dissertation noted how Carpenter ascribed increasing importance to "the will" as one "ascend[ed] the scale" of the animal kingdom. In proposing two separate nervous systems, the "sensori-volitional" system and the "reflex" nervous system, what Carpenter was seen to have done was point out how these two separate nervous systems gradually fused in higher and higher animals until they coalesced in a continuous ganglionic mass in the vertebrate spinal cord.[184]

While at Edinburgh, Carpenter was recruited to become a contributor for the *British and Foreign Medical Review*. His anonymous 1840 article in this journal also helped the British uptake of the reflex arc and the notion of a "spinal" nervous system operating separately from the cerebral system.[185] Privately Carpenter used his increasing

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[182]Desmond, *The Politics of Evolution*, p. 214; Leys, *From Sympathy to Reflex*, pp. 306-307.
[183]Grainger, *Observations on the Spinal Cord*, pp. 61-64; Leys, *From Sympathy to Reflex*, pp. 307-308.
[184]"Notices of some New Works: *Inaugural Dissertation on the Physiological Inferences to be Deduced from the Structure of the Nervous System in the Invertebrated Classes of Animals*, by William B. Carpenter," *M-CR* 11, 497-498 (1839): pp. 497-498.
[185]Carpenter and Carpenter, *Nature and Man*, p. 18; Leys, *From Sympathy to Reflex*, pp. 310-11.

pull with Owen to convince the leading British anatomist about the validity of the reflex arc; Owen's lectures on the nervous system in the *Medical Times* and his later *Lectures on Invertebrates* "completely adopted" it. Carpenter graciously thanked him for the acknowledgement, but also worried about the touchy Dr. Hall's possible reaction.

> I feel much obliged to you for giving me so completely the credit of the idea; as *my friend* Dr. M. Hall cannot see anything in it but what he knew before. I am just now upon very good terms with him; but I know not how long it may last.[186]

Perhaps Hall had a difficult personality, perhaps he was paranoid about others stealing his credit; but just because he was paranoid didn't mean that other researchers weren't out to steal his work. Various researchers gradually adopted Hall's reflex arc in the early 1840s. Despite Hall's earlier attacks on him, Mayo began to publicly compliment Hall and his reflex theory, rhetoric one reviewer saw as a preliminary manoeuvre to obtain credit for himself.[187] And Owen's acknowledgement of Carpenter's account of the reflex theory was prophetic: Carpenter - less scandalous than the mercurial Hall – and his writings were increasingly relied upon for an account of the reflex arc, not Hall's.[188]

Eventually the existence of two anatomically separate nervous systems was overturned. Carpenter was to point out that the most complex nervous functions were compounded from simpler reflexes, just as the most complex nervous structures were compounded from simpler ganglia. The reflex arc became increasingly accepted as the functional counterpart of the ganglion, and purposive behaviour could be disintegrated into units instead of being explained by a central directing agency in the mind.[189] Hence

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[186]W.B. Carpenter to R. Owen, 26 Jun 1842, OCORR, NHM, 6/302-303. Emphasis in original.
[187]"Review of *The Nervous System and its Functions*," pp. 21-22.
[188]Leys, *From Sympathy to Reflex*, p. 228. Carpenter and Carpenter, *Nature and Man*, p. 22.
[189]Clarke and Jacyna, *Neuroscientific Concepts*, p. 140; Desmond, *The Politics of Evolution*, p. 214; Jacyna, "Principles of General Physiology," p. 78; R. Smith, "The Background of Physiological Psychology in Natural Philosophy," *History of Science* 11, 75-123 (1973): pp. 83-84.

the neurophysiological element of the reflex reinforced the role of the neuroanatomical element of the ganglion.

A specific example - Newport's 1843 researches upon millipedes - shows how certain vivisections were undertaken to learn how agency and nervous 'authority' were distributed through the body. Newport, like previous researchers, saw the reflex as occurring most strongly in parts of the body that were deprived of the cerebrum's supervision, or "volition." He set out certain questions directing his investigation.

> 1st. Whether sensation and volition are confined to the supra-oesophageal ganglia, the brain, or whether they exist also in the first suboesophageal ganglion, or in the other ganglia of the cord? 2nd. Whether these functions are destroyed by partial destruction of the brain? 3rd. Whether there is any direct evidence of sensation in a portion of the cord that is insulated from the brain? 4. Whether the movements in these animals, when deprived of the brain, are identical with those of the Crustacea and Vertebrata?[190]

The particular organism Newport chose was a myriapod: *Iulus terrestris,* the white-legged snake millipede, a brownish-black millipede with a waxy sheen.

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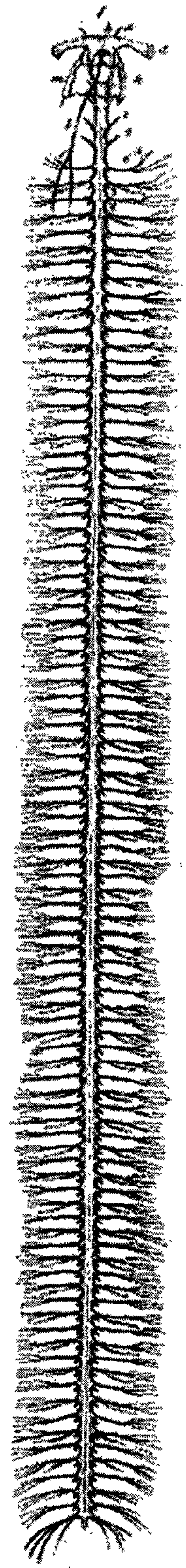
[190]G. Newport, "On the Structure, Relations, and Development of the Nervous and Circulatory Systems, and on the Existence of a Complete Circulation of the Blood in Vessels, in Myriopoda and Macrourous Arachnida," *Phil. Trans.* 133, 243-302 (1843): p. 265.



Julus terrestris

Nervous system of *Iulus terrestris,* from G. Newport, "On the Structure, Relations, and Development of the Nervous and Circulatory Systems, and on the Existence of a Complete Circulation of the Blood in Vessels, in Myriopoda and Macrourous Arachnida," *Philosophical Transactions of the Royal Society* 133 (1843): 243-302, plate 11.

To understand why Newport chose *I. terrestris* for his investigations, consider the millipede's structure and behaviour. It has a linear, diffused, nervous system, shown above: inside each segment of its body is a pair of fused ganglia, each pair roughly the same size as its neighbours. Its "brain" in the anterior forms only a small portion of the entire nervous system.[191] Further, *I. terrestris* is composed of over forty identically-shaped and -sized segments; most of these segments (from the fourth to the penultimate segment) have two pairs of legs, giving it at least 166 translucent yellow legs.

Because of the large number of legs and their similarity to one another, it is easier for one's eye to settle on the larger pattern of leg-movement rather than on the individual legs themselves. So as the millipede moves across the table it appears to *flow* across it; the word 'flow' suits the character of its movement better than 'walks,' for its legs rise and fall in undulating "metachronal" waves as each tiny leg touches the ground, makes a slight push and then rises again. Because each leg is slightly out of phase with its neighbours before and after it, the motion appears to be transferred from one leg to the next, and then to the next, and so on, in a series of rolling motions from the anterior (front) of the millipede to its posterior (hindmost) part.[192]

Newport mutilated these creatures in different ways to answer his questions. He first cut off the millipede's "head," or its first segment, removing with it the antennae, eyes, and mandibles. Then he placed it back on the table to see how it moved; it did so, in a straight line. Though it could move over a low obstacle, it did not move around a higher one. Instead the front, wounded, part – now oozing fluid - pressed against the

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[191]Newport, "Structure of Nervous System in Myriapoda," p. 247.

[192]*I. terrestris* is now known as *Tachypodoiulus niger*. J. G. Blower, *Millipedes : Keys and Notes for the Identification of the Species* (London: Linnean Society of London, 1985), pp. 135-138; S. P. Hopkin and H. J. Read, *The Biology of Millipedes* (Oxford: Oxford University Press, 1992), p. 229.

obstacle as though trying to push it out of the way. The legs' movement sped up and then slowed, with the movement finally ceasing after half an hour; when Newport prodded the millipede its legs started to move again, but eventually they stopped.[193]

On another occasion Newport cut a millipede into three pieces. First he cut away an anterior part, with eyes and antennae and some legs; he then cut the remaining posterior section into two. The anterior section could touch, avoid and seek objects, but it moved more slowly and had trouble maintaining its balance. The posterior sections, meanwhile, had no balance at all but their legs moved when he prodded them. In the most interesting episode of these gruesome investigations, Newport plunged a needle into segments fourteen to twenty, destroying each ganglion in the ventral nervous column. The millipede was otherwise left intact. The front half of the millipede repeatedly turned, "showing perfect volition": its antennae tapped the frontmost part of the wound (in the fourteenth segment) while ignoring the rest of the wound; but when it started to move, the leg-waves no longer flowed uniformly from anterior to posterior. Instead the undulations moved from the anterior legs to those of the fourteenth segment; then the wave "stopped" as the legs in the wounded section stopped moving; and in the posterior section the legs moved constantly, but not in the same rhythm as the front legs. When the front half of the millipede tried to stop moving in order to examine an object on the table, its legs stopped; but the legs below the wound kept moving. Newport inferred that these legs were propelled by reflex activity alone.[194]

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[193]Newport, "Structure of Nervous System in Myriapoda," p. 265.
[194]Newport, "Structure of Nervous System in Myriapoda," pp. 266-267.

EARTHWORM SEGMENTS AS INDEPENDENT AGENTS

Where Newport was careful not to overly anthropomorphize the quasi-independent segments and ganglia, similar and contemporaneous vivisections were by no means so subtle. One example was the mutilation of earthworms, which like millipedes have almost-equally distributed similar ganglia in their repeating segments. This ganglionic equality was pointed out by Henri Milne Edwards in Todd's *Cyclopaedia,* where (copying his earlier work in the *Dictionnaire Classique d'Histoire Naturelle*) he pointed out that Bonnet, Dugès and other observers had demonstrated that an earthworm could be cut into two moieties, with each half growing into a new organism.[195] Tales of earthworm vivisections are a good example of how researchers perceived ganglia to be related to one another in terms of their agency and power. They also strengthen my claim that the reflex theory emerged only within this larger context of compound individuality.

In 1832 Charles Bell remarked offhand that when an earthworm was cut in two, only the anterior half moved away. The posterior part writhed but did not move, stated Bell, and he concluded that only the anterior half moved because only it had ganglia.[196] Bell's claim provoked a response. In 1835 Charles David Badham - product of Eton and Oxford, clergyman and Oxford Travelling Fellow - criticized Bell's interpretation, seeing ganglia throughout the earthworm. He wanted to deny that certain ganglia in the anterior were more powerful than the rest, forming even a "small brain."

Badham thus carried out his own mutilations, dividing earthworms into two or more sections. He found that the posterior moiety writhed only temporarily, and that its

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[195]H. Milne Edwards, "Annelida," in *Cyclopaedia* (1836), 164-173, pp. 172-173.
[196]Bell, "Physiology of the Brain," p. 684.

motion eventually became "*progressive, associated and similar.*" He used terminology associated with a workshop in concluding that the anterior ganglion was therefore not the sole "officina" of sensation. But Badham actually drew the opposite conclusion than what might be expected from his research: he inferred that the ability of the separated halves to move on their own proved that an earthworm was an individual, "that he is not many, but one."[197]

Badham sent these 1835 accounts to the conservative *London Medical Gazette*. In 1838 he took his campaign to the more widely circulating and conservative *Blackwood's Magazine,* where he anonymously wrote "On the Supposed Sensibility and Intelligence of Insects." Badham began by listing a curious problem facing researchers on invertebrate nervous systems (specifically insects) – while some thought that their dispersed ganglia could be seen "as so many equal brains", other researchers, such as Bell, only looked upon the first (anterior) ganglion as the true "brain."

Badham again brandished the results of his earthworm mutilations to a more general audience. Both posterior and anterior worm-halves moved progressively: "If for a few seconds, the headless portion of the two seemed least lively, as soon as it had made up *its mind* it moved off, much after the fashion of the entire worm, or the piece, if divided, to which the head belonged." But the movement of both worm halves proved deeply problematic for Badham, as it meant a "*conclave* or *council* of brains in *one being*, and signalize the prodigious inconvenience of a plurality of brains to a *single* possessor." Wouldn't these many brains "*disintegrate* that creature, and make many individualities

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[197]D. Badham, "On the Nervous Circle of Sir Charles Bell," *LMG* 15, 71-74 (1835): p. 73.

out of or within one organization? Are the *pieces* of a worm, then, just *so many worms*...and *yet* capable of consolidation *into one existence?*"[198]

Badham continued to relate compound individuality, a hierarchy of nervous centres, and internal harmony by explicitly political imagery.

> [There must be instead]...an exact harmony and understanding between these different individualities, else his actions would have no unity, no rythm [sic] no steadiness of purpose or uniformity of character. In short, has a worm a *will*, or a *chorus of wills?* To *will* is one of the first attributes of *mind,* (and mind is *unity*, is indivisible.) When I walk, I indeed will to walk. I have but *one* brain. When a worm crawls with his *twenty brains*, is it *his* will or *their* wills that govern him? Were every ganglion a *separate brain,* there might come to be an insurrection of the wills! the balance of power in the *ganglionic republic* might be perpetually disturbed! and not only every motion be very difficult to be executed, but even the vital principle be often in exceeding doubt how to distribute itself, and do justice to all parties![199]

Perhaps earthworms were a favourite exemplar animal for displaying serially repetitive ganglia because they were easy to obtain. Their availability benefited the museum-less researcher. Writing from Limerick, Ireland to the *London Medical Gazette*, William Griffin also sought to understand problems of individuality by mutilating earthworms. Like Badham, he cut them into various pieces to investigate consciousness, wondering if the posterior earthworm-pieces would show "apparent consciousness and volition".[200] W.B. Carpenter's example of the mutilated earthworm also appeared in the *Gazette* (and he agreed with Badham's statement that the anterior and posterior moieties of a bisected earthworm would continue moving forward; unlike Bonnet, Carpenter thought that only the anterior moiety would permanently survive).[201]

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198 D. Badham, "On the Supposed Sensibility and Intelligence of Insects," *Blackwood's* 43, 589-606 (1838): pp. 590-591. Emphases in original.
199 Badham, "Supposed Sensibility of Insects," p. 591.
200 Griffin, "Physiological Problem," pp. 111-112.
201 W. B. Carpenter, "Lecture 3 on the Nervous System," *LMG* 27, 938-945 (1840-1841): pp. 939-940.

But unlike Newport's work, Badham's research was not celebrated at all. The *British and Foreign Medical Review* savaged one of his later books (on insects) as full of errors.[202] Like other researchers, Badham tried to interest eminent researchers in his work but with less success: he sent unsolicited copies of one of his books to Owen, plaintively reminding Owen of a past promise to send him a tract of his latest talk. (Owen does not appear to have replied to his request). Meanwhile, Griffin was located too far away from the London hub of research; the *Lancet* patronizingly congratulated his later book as a worthwhile effort from someone so remotely placed.[203]

Yet it would be a mistake to dismiss both vivisectors because of their anthropomorphization of the earthworms, or their linkage of the nervous system with a system of political authority. To point out that this political language was simply 'metaphorical' is to take an overly literary view of the role of metaphor. It ignores that metaphors structure our concepts and the way that we act upon them: they allow us to understand one unfamiliar realm in terms of another, more familiar, realm of experience. It has been pointed out that the metaphor 'argument is a war' is not simply a literary device with which to artistically portray an argument – the participants in an argument can actually win or lose that argument.[204] We might understand metaphors instead as

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[202]"Dr. Badham on Insect Life," *BFMR* 11 (1846): 494-495.
[203]D. Badham to R. Owen, 10 July 1851, OCORR, NHM, 2/9-10; R. Garnett, "Charles David Badham (1806-1857)," in *DNB* (1885). It is striking how important a *symbolic* role the gift played in the creation and maintenance of patron-client relations among these researchers. They usually took the form of a specimen, offprint or book. While the sender may have tried to influence Owen's views with the gift on some scientific point, they were often sent merely as a way for the sender to stay uppermost in Owen's mind. Owen, likewise, sent off his offprints to 'remember himself' to others.
[204]G. Lakoff and M. Johnson, *Metaphors We Live By*, 2nd ed. (Chicago: University of Chicago Press, 2003), pp. 4-6.

forming the similarity relations that then help researchers determine whether a term is properly used or applied;[205] in other words they shape the researchers' experiences.

In fact the metaphor of the nervous system being a system of distributing authority was more widespread; it was not merely limited to dilettantes or writers for large audiences. Specialists used these words too: as early as 1811, Charles Bell spoke of body parts being excluded from the "government of the will." Marshall Hall spoke of, and was then subsequently quoted, as speaking of the soul as "enthroned" upon the cerebrum, "receiving the ambassadors, as it were, *from* without, along the *sentient nerves*; deliberating and willing; and sending forth its emissaries and plenipotentiaries, which convey its sovereign mandates, along the *voluntary nerves, to* muscles subdued to volition."[206] And Herbert Mayo called for fellow researchers to determine the number of "departments" that made up the "public office" of the brain.[207] Indeed, the discussion of nervous centres in terms of their authority and power over subordinate centres or parts allowed these parts to be anthropomorphized and described as political agents.

Where Edwin Clarke notes the "mediation" of reflex arcs by the ganglia in the segments of the spinal cord rather than the cerebrum,[208] we might replace mediated with 'centred' (after all, recall Hall's own early term for reflex action, "diacentric.") L.S. Jacyna writes that between 1820 and 1840 the nervous system was transformed into a complex and differentiated one, where nervous power was distributed amongst centres that operated partly independently. This devolution might be interpreted as the

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[205]Barnes, *T.S. Kuhn and Social Science*, pp. 27-29.
[206]Review of *On the Diseases and Derangements of the Nervous System* by Marshall Hall, *M-CR* 35 (n.s.), 306-337 (1841): p. 306; Bell, *New Anatomy of the Brain*, p. 4; M. Hall, "Lectures on the Theory and Practice of Medicine," *L* 1, 649-657 (1837-1837): p. 649. Emphasis in original (from review).
[207]Mayo, *The Nervous System*, p. 73.
[208]Clarke, "Marshall Hall," pp. 59-60.

delegation of nervous power to more local authorities. The encephalon (as the seat of the unitary *sensorium commune)* lost some of its authority to control the rest of the body, handing over some of its power first to the spinal cord, and then increasingly to the nervous ganglia that could also act independently. These ganglionic nervous elements which collectively contributed to an organism's mental life each became the seat of its own microcosmic nervous system; each one formed the centre to a small area (either being depicted as the source of nervous power, or the point where reflexes were mediated), just as the *sensorium commune* was formerly seen to be the centre of the entire body, issuing commands to that entire body.[209]

FAILED ANALYSES: PHRENOLOGY, WIGANISM AND AUTHORITY

Establishing a larger cultural context for analytical anatomy and physiology is the field of phrenology, another form of analytical research occurring around the same time. Phrenology was a 'failed' analytical investigation whose practitioners tried to understand how the mind worked by describing its workings as the interaction of simpler mental elements. In turn we can see phrenology as inhabiting a common context of compound individuality.

As the child of J.F. Gall's "doctrine of the skull" or "Organologie",[210] phrenology held that the brain, as the organ of mind, was not a unity but an aggregate, a collection of organs with each one serving a specific mental function. The size of each organ determined each function's relative power. Since the skull hardened over the various

[209] Jacyna, "Principles of General Physiology," pp. 50-53.
[210] J. van Wyhe, "The Authority of Human Nature: the Schädellehre of Franz Joseph Gall," *British Journal for the History of Science* 35, 17-42 (2002): p. 22.

brain-organs in infancy, an external observer could use the resulting bumps to determine a person's mental abilities and character; thus its caricature as "bumpology."[211] Gall was devoted to the principle that different organs not only exercised a particular function, but that they also tended to work in harmony with the other mental organs, some of which were stronger than others; it was his attempt to determine which organs were responsible for particular functions that was the most controversial part of his work. The phrenologist's view of the head as a hierarchy of spaces with specialized functions has been likened to the factory in Andrew Ure's *Philosophy of Manufactures.*[212]

In Richard Owen's papers at the Royal College of Surgeons there is a critical review of phrenology. It notes how phrenology broke the mind into

> so many distinct *individual* and separate intelligences...The whole philosophy of Gall, [Pierre Flourens] concludes, consists in creating *multiplicity*, in the place of *unity*; dividing the intelligence, which is one and general, into twenty-seven petty and individual intelligences, and breaking up the brain into twenty-seven small brains; thus substituting an unintelligible chaos in place of the mutual relation and admirable order of the human understanding.[213]

The reviewer and Flourens were both correct. Gall sought to abandon the notion of a single consciousness – "the me" – for it was only imaginary. Gall instead thought that the various nominally independent nervous systems were connected with each other, and

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[211]R. Cooter, *The Cultural Meaning of Popular Science : Phrenology and the Organization of Consent in Nineteenth-Century Britain* (Cambridge: Cambridge University Press, 1984), p. 3; D. De Giustino, *Conquest of Mind : Phrenology and Victorian Social Thought* (London: Croon Helm, 1975), p. 18. John van Wyhe has corrected the view that Gall was "banished" from Vienna and thus went to Paris – this myth actually was disseminated by British phrenologists like George Combe to support their self-image of persecution. van Wyhe, "The Authority of Human Nature," p. 26.

[212]Cooter, *The Cultural Meaning of Popular Science*, pp. 111-112; B. Hollander, "Herbert Spencer as a Phrenologist," *Westminster Review* 139, 142-154 (1893): p. 144.

[213]"Phrenology examined, translated from the Gaz. Med. De Paris," *Medical Times* 7, 104-105 (1842). Emphasis in original. Flourens was opposed to this multiplicity. He was deemed to have "decisively" refuted phrenology by his investigations where he removed or destroyed portions of a pigeon's cerebrum thought to correspond with different phrenological mental faculties; Flourens concluded that the entire brain was necessary for its proper operation.

the result was mental activity.[214] Thus one was not in control of one's mind; instead, mental activity emanated from the interaction of one's mental organs.

In Britain Thomas I.M. Forster gave it the name "phrenology" in 1815, and it became popular in the 1820s.[215] Gall's assistant, Johann Gaspar Spurzheim, helped it become popular in Britain through his lectures and demonstrations. Spurzheim, too, held a similar view of the compound nature of the brain – desire was not a primary mental power, but instead resulted from the combination of individual faculties. Pain and pleasure also depended on these interactions.[216]

Like other analysts, phrenologists also laid claim to their field's scientific status by forming institutions. One of the most entertaining anecdotes in Roger Cooter's exploration of British phrenological culture is the phrenologists' attempt to set up their own section at the BAAS to demonstrate their scientific status. When denied this section, they set up their own phrenological group and occasionally met in the same town as the British Association, the meeting of the phrenologists receiving greater press coverage than the BAAS meeting. Meanwhile itinerant phrenological lecturers set themselves up as "Professors" of phrenology; various museums of phrenology also sprang up, like the one at the Liverpool Literary, Scientific and Commercial Institution, allowing these phrenological specialists to compare skulls.[217]

The observation that Gall and Spurzheim saw the entire human nervous system as built out of a number of independent units was made over fifty years ago by Oswei

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[214]J. C. L. Carson, *The Fundamental Principles of Phrenology* (London: Houlston & Wright, 1868), p. 309. Gall, or at least his popularizer, was conflicted on this point – earlier, in the same book, Gall's anonymous interpreter noted that the plurality of mental organs did not destroy the "unity of consciousness." *Manual of Phrenology : being an Analytical Summary of the System of Doctor Gall* (Philadelphia: Carey Lea and Blanchard, 1835), pp. 95-96, 85-86.
[215]van Wyhe, "The Authority of Human Nature," p. 22.
[216]J. G. Spurzheim, *Outlines of Phrenology*, 3rd ed. (Boston: Marsh Capen & Lyon, 1834), pp. 82-83.
[217]Cooter, *The Cultural Meaning of Popular Science*, pp. 90-94.

Temkin. Temkin then called this observation "dogma".[218] But in the late 1830s and 1840s medical reviewers paid a great deal of attention to phrenology. One article, which reviewed 23 different phrenological works, noted that its second major premise (its first was that the brain was the organ of the mind) was that the brain was not a single unit but instead a "plurality" of organs, each organ "serving for the manifestation of an individual faculty of the mind."[219] Likewise, though critical of phrenology, W.B. Carpenter did grant that comparative anatomy - by establishing that nervous centres were formed of distinct parts each with a different function - had helped phrenology. People who dismissed phrenology outright were simply ignorant: those "who *now* sneer at phrenology in toto, are neither anatomists nor physiologists" because the mind was obviously not a simple unity but was a "composite thing, built up of various and even clashing qualities".[220] At least phrenology offered a plausible explanation of the functions of particular brain-parts.[221] In a lecture directed against Flourens's ablation of pigeon brains to refute phrenology, Owen noted that while its goal - to know which parts of the nervous system were essential and which "superadded" – was laudable, in practice ablation was not always very revealing, particularly in mammals.[222]

Meanwhile phrenologists in turn used the findings of comparative anatomy and vivisection to support their work. They held up the Bell-Magendie distinction between motor and sensory nerves to show how the nervous system was compounded out of

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[218]O. Temkin, "Remarks on the Neurology of Gall and Spurzheim," in *Science, Medicine and History*, ed. E. A. Underwood (Oxford: Oxford University Press, 1953), 282-289, p. 284.
[219]"Notes on Phrenology," *BFMR* 9, 190-215 (1840): p. 197.
[220]Carpenter, "Noble on the Brain," pp. 518-519.
[221]"Dr. Combe on Phrenology," *BFMR* 12, 230-231 (1846).
[222]Owen, "Lectures on the Nervous System". He did not say why this was problematic, but it was likely because of the concentration of the mammalian nervous system, making it difficult to remove a particular mental faculty or organ without damaging nearby faculties or organs.

simpler parts.[223] Phrenologists quoted from various respected textbooks such as Müller's *Physiology* to support their work. And the same exemplar organisms were cited: where the lowest animals possessed no nervous system, as in "hydra vividis" [sic], asterias (starfish) had a circle of ganglia, and centipedes had "several brains", one to each leg. As we "ascend the scale of organic intelligence" the brain became double, united by commissures; as we moved up still further these commissures grew still larger.[224]

Like other researchers, not only did phrenologists reject the notion of the mind as a unified entity, but they also granted a limited amount of agency to each mental faculty. Mental faculties each possessed interests, desires and intentions. Phrenology's most popular book, George Combe's *Constitution of Man,* stated that each faculty stood in a relation with particular "external objects; - when it is internally active it desires these objects; - when they are presented to it they excite it to activity, and delight it with agreeable sensations." The lower faculties especially sought their own gratification, even when this gratification was opposed to the higher intellectual faculties.[225] By the insistence that each person's mind was not a unity but a plurality of mental faculties, phrenology literally devolved power to a broader internal constituency, a community of often-warring elements within the person.

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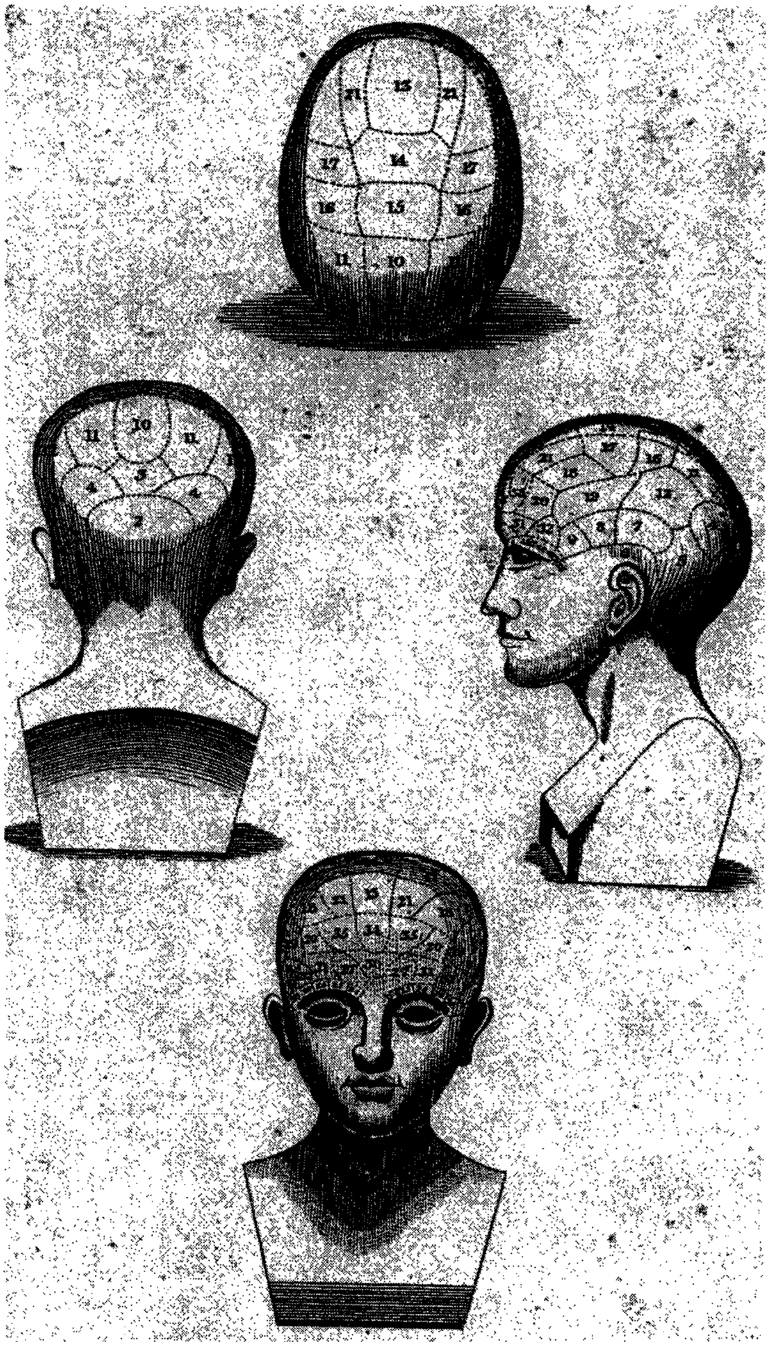
[223] "Report on Meeting of Westminster Medical Society, 15 Dec 1838," *LMG* 23, 486-489 (1839): pp. 487-489.

[224] Review of *A New View of Insanity* by A.L. Wigan, *Journal of Psychological Medicine and Mental Pathology* 1, 218-229 (1848): pp. 220, 224-226. This reviewer quotes from Baly's translation of Müller, pp. 1:813-24 and Solly's 1847 edition of his *Human Brain*, pp. 30-96.

[225] G. Combe, *The Constitution of Man Considered in Relation to External Objects*, 5th ed. (Boston: Marsh, 1835), pp. 55-56, 59.



Mental faculties, from *Manual of Phrenology* (Philadelphia: Carey Lea and Blanchard, 1835), plate 5.

The clearest example in which the mind was described as a collective institution appeared in Sidney Smith's 1838 *Principles of Phrenology*. Sometimes it acted harmoniously. But when one dreamt, "the faculties which, at other times, form a regular disciplined and combined army of operations, now disperse, and each acts for his own behoof, like sharp-shooters".[226] To portray internal disagreements Smith portrayed the brain as a parliament. If there was a faculty in a dreaming person which made him fearful, but another (weaker) faculty which caused him to fight,

> ...we can perfectly understand how he whose life is passed in days of prudence, should spend his nights in dreams of rash adventure. His organ of Cautiousness may then be asleep, and that of Combativeness, hitherto an incarcerated slave, may celebrate its jubilee of emancipation in awakened activity. And so, if there be an organ of Acquisitiveness, which prompts to the exercise of the appropriation *claws*, and another of Conscientiousness, which, in its upper-house, negatives every bill presented by such a party, it is plain how, when a man's entire faculties are awake, and both branches of his intellectual legislature sitting, he may be honest; while, when the Lords alone have adjourned their session, sleep may make him a thief or a rogue, when his organ of integrity slumbers, and his faculty of acquiring ranges uncontrolled through every scene of villainy.[227]

The strength of this image lay in its portrayal of the faculties in terms of their power and influence, as political agents. The picture of a parliament of homunculi - in which mental faculties acted as political agents, each one with a single idea continually put forward in the hopes of prevailing over the others - was later used in William James's 1890 *Principles of Psychology*. (James, however, used it to criticize phrenology).[228]

More evidence of the common context of the neurosciences and phrenology can be seen in the furore over the "double brain" proposed by Hewett C. Watson and Arthur L. Wigan. Watson attended medical classes at the University of Edinburgh in the early 1830s. Though he obtained no degree he became interested in phrenology there (meeting phrenological popularizers George and Andrew Combe); he also learned about ornithology, entomology and botany (winning a gold medal for an 1831 botanical essay). He became interested in the study of the distribution of plants, becoming a fellow of the

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[226]S. Smith, *The Principles of Phrenology* (Edinburgh: William Tait, 1838), p. 36.
[227]Smith, *The Principles of Phrenology*, p. 37.
[228]W. James, *Principles of Psychology*, 2 vols. (New York: Henry Holt, 1890), p. 1:29. James is quoting Friedrich Albert Lange, *Geschichte des Materialismus und Kritik seiner Bedeutung in der Gegenwart* (Leipzig: von J. Baedeker, 1887) 2nd ed, 2:345.

Linnean Society in 1834. Watson also bought the copyright of the *Phrenological Journal* in 1837, acting as its editor until 1840.[229]

In 1836 Watson used comparative anatomy to muse upon the existence of the double-lobed brain – after all, double lobes were found in all animals until one descended very low "in the scale" of organization and intelligence. It wasn't because of the threat of injury – the heart and stomach were important too, but they weren't double. Watson noted that the human frame was almost double, one side a counterpart to the other. Perhaps the double brain was required because many of the double parts acted "individually" as well as in concert: sometimes they moved in opposition to one another, for instance the legs alternately moving during walking, or the hands performing different motions at the same time. Since these movements were independent or antagonistic, Watson speculated that the brain might also have to act "singly or jointly."[230]

Eight years later in a series of letters to the *Lancet*, the physician A.L. Wigan independently suggested the mind's duality too. He suggested that both the right and left half of the brain were distinct wholes, and that each person had two separate thinking-processes occurring in each cerebrum. Each half was capable of distinct and separate volition, even in opposition to one other. A person's unity was nonetheless maintained through hierarchy: in the healthy brain one cerebrum was more powerful than the other, capable of controlling the volition of its "fellow." But if one cerebrum was damaged, the other, healthy, organ could only control the "morbid volitions" of the other to a certain extent – if this control was lost, however, then derangement, delusion or insanity resulted.

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[229]G. S. Boulger, "Hewett Cottrell Watson (1804-1881)," in *DNB* (1899).
[230]H. C. Watson, "What is the Use of the Double Brain?" *Phrenological Journal* 9, 608-611 (1834 -1836): p. 609.

Wigan thought that insanity was really the mixture of two "synchronous trains of thought" – one rational, the other irrational. Education was supposed to teach both brains to simultaneously focus on the same subject. Indeed, Wigan even spoke of the "intellectual antagonism" between the two brains as each brain watched over the other. Later that year, Wigan would publish these speculations as a book dedicated to Henry Holland, the Queen's physician, who had also speculated on the mind's duality.[231] Wigan drew four responses from *Lancet* readers. Two were critical or angry about its materialism; one was supportive, and one thought Wigan hadn't gone far enough.[232]

This fourth letter was from a phrenologist: James Davey, surgeon to the Hanwell Lunatic Asylum. He noted how others anticipated Wigan's speculations on duality – but Gall had surpassed them all. "If [Wigan] were to study the writings of phrenologists, and test their observations in the great Arcanum of nature, he would, I doubt not, directly see that the PLURALITY of the mind is indispensable to his conclusions." The conflicting volitions of which Wigan spoke took their character from the particular mental organs of the two brain hemispheres – to speak of the healthy brain strengthening its 'control' over the unsound brain wasn't precise enough. Instead Davey thought that the lunatic had

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[231] A. L. Wigan, "The Duality of the Mind, Proved by the Structure, Function, and Diseases of the Brain," *L* 1, 39-41 (1844): p. 40; idem, *A New View of Insanity : the Duality of the Mind proved by the Structure, Functions, and Diseases of the Brain* (Malibu, Calif.: Simon, 1985). For his part, Holland discussed cases of "mental derangement" where the mind was "double-dealing with itself", and gave examples from Mayo's *Physiology* for support. H. Holland, *Chapters on Mental Physiology*, 1st ed. (London: Longman, Brown, Green and Longmans, 1852), pp. 185-188. For more on Wigan see B. Clarke, *Arthur Wigan and The Duality of the Mind* (Cambridge: Cambridge University Press, 1987).

[232] Dublinensis, "The Duality of the Mind," *L* 1, 186 (1844); M. Ryan, "The Non-Duality of the Brain," *L* 1, 154 (1844); J. Sheppard, "The Duality of the Mind," *L* 1, 305-306 (1844). J. Davey, "The Duality of the Mind known to the Early Writers on Medicine," *L* 1, 377-378 (1844).

several mental organs excited at the same time, and each organ wanted different things - explaining why that lunatic often performed the very opposite of what he planned.[233]

Wigan responded later that year: Davey had misunderstood him, for his own argument was also founded on the plurality of mental organs. Wigan's only problem with phrenology was its "arbitrary division" of the brain where no anatomical differences could be found.[234] His later book even went to the trouble of acknowledging Watson's prior speculations on the double brain. Though sceptical of some of the phrenologist's musings, Wigan supported the notion that different body parts such as legs or eyes or ears exerted independent, antagonistic and simultaneous actions, making the double brain necessary for their guidance.[235]

Wigan's book was fairly well received by reviewers. Flattered by Wigan's dedication yet sceptical, Henry Holland thought that the cultivation of one's will might overcome the "doubleness" lurking in pathological cases such as hysteria or in "ill-regulated" motions and imperfect associations.[236] One exception was John Elliotson's review, which likened Wigan to Bichat, the contemporary neurosciences, and to phrenology. A popular surgical lecturer-turned phrenologist who then became a heterodox phreno-mesmerist, Elliotson was highly critical of Wigan. He ought to have been sympathetic; fifteen years before, Elliotson denied the existence of a single "seat of the soul, consider[ed]… as the throne before which the mind holds its court".[237]

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[233]Wigan's predecessors, according to Davey, were Hippocrates, Boerhaave, Haller and van Swieten. Davey, "The Duality of the Mind known to the Early Writers on Medicine," p. 377.
[234]A. L. Wigan, "Dr. Wigan on Duality of the Mind," *L* 1, 451 (1844): p. 451.
[235]Wigan, *A New View of Insanity*, pp. 300-304.
[236]Holland, *Chapters on Mental Physiology*, pp. 185-189.
[237]J. Elliotson, "Dr. Elliotson's Defence of Phrenology against the Attacks of Mr. Godwin," *L* 1, 360-363 (1831-1832): p. 360.

Reflecting his own declining fortunes as former phrenologist allies attacked him,[238] Elliotson was angry at the appropriation of the discovery that the brain acted doubly. Hadn't the respectable Charles Bell pointed out the nervous system's duality in 1826? Elliotson directly quoted one of Bell's texts:

> 'I speak of the lateral divisions of the brain being DISTINCT BRAINS combined in function...there is every provision for their acting with perfect sympathy...We are forced to admit that there are four brains' (the brain being divided into cerebrum and cerebellum, large and little brains; and each of these having two corresponding halves.)'[239]

Elliotson then quoted from one of his own textbooks, where each edition since the mid 1820s had interpreted Bichat for a new generation of British anatomists: Bichat inferred that a harmony of action in each organ-half was necessary. Elliotson got in a little dig at the successful Henry Holland too, saying that he had merely repeated Bichat as well. He concluded by pointing out that Wigan was ignorant of other continental researches, especially Gall's analytical anatomy of the brain (where Bichat had learned about the brain's duality, he noted).[240] To paraphrase Molière, Wigan had been practicing analysis all his life and hadn't known it.

BIOLOGICAL AND SOCIAL ORDER: A SOCIETY WRIT SMALL

Bringing us full circle, Robert B. Todd commented favourably on Wigan's work in his *Cyclopaedia* (by now struggling into 1847, some 15 years after its projection). He

[238] A. Winter, *Mesmerized : Powers of Mind in Victorian Britain* (Chicago: University of Chicago Press, 1998), pp. 153-155.
[239] Quoted from Charles Bell's *Anatomy and Physiology of the Human Body*, 2: 401. J. Elliotson, Review of *A New View of Insanity* by A.L. Wigan, *The Zoist* 5, 209-34 (1847-1848): pp. 211-212.
[240] Elliotson's *Human Physiology*, 4th ed, 1828, p. 55; 5th edition, p. 21, 1835; quoted in Elliotson, "Review of *A New View of Insanity*," pp. 221-223, 213-214.

thought Wigan's book was interesting, though he was sceptical about the "confusion" that would occur if a person had two separate and simultaneous mental processes.[241] Todd's comment was based upon a slight but telling misreading, explaining the continual insistence that various 'elements' – be they ganglia or reflex arcs or dual minds – had to be seen as acting within a hierarchical scheme. Recall that even the flamboyant 'dualist' Wigan could not accept two equally powerful consciousnesses in a single mind – one had to be more powerful than the other one. Or that Hall insisted that the reflex arc be seen as part of a hierarchy of separate nervous systems (he may have repeatedly changed the number of systems, but not the insistence of a hierarchy). Or that Grainger found that the highest organisms had the greatest proportion of white transmitting fibres moving to the cerebrum.

It was this continuous rejection of the equality of ganglia which is the most telling for the historian. Unity of thought - control of oneself as an individual - voluntary control - all rested upon the assumption of some sort of internal agreement. It was repeatedly assumed that some nervous centres had to be able to influence and act upon others more than they could be influenced and acted upon in turn. The unthinkability of the equality of all ganglia was rooted in a fear of confusion and internal anarchy; concerns about internal unity were just as much concerns about authority.

The situation of reflex arcs within a hierarchical scheme explains the subsequent emergence of the terms "consensus" and "consensual" and their association with reflex arcs. The recurring exemplar of the mutilated articulate shows the retention of a nervous hierarchy. Like Newport, William B. Carpenter explained the independence of myriapod body parts in terms of its reflexes. For a centipede to move in a directed, regular way,

[241]Todd, "Physiology of the Nervous System," pp. 722z-723b.

there had to be some general control or coordination of the various centipede parts. A centipede with its nervous cord cut in several places would also move, but not "with a combined object", so no progress toward a goal would occur. When its ganglia were isolated from one another and from the "presiding centre of the will", each ganglion was merely the centre of reflex actions that governed only a single segment. For instance, decapitating a centipede meant that only impressions made equally on all segments produced "consentaneous" activity - exposing the decapitated centipede to an acidic vapour caused the entire body to flex away from it. Yet doing different things to different segments of the body produced different reactions simultaneously in that same centipede.[242]

In other, later, words the "head never delegates to the tail the authority of leading, directing, or controlling the evolutions of the frame... [cephalic ganglia] are the controllers of all the *consensual* muscular actions of the body."[243] *Consentaneity*, as the name suggests, was just that – the "consent" of the various nominally independent ganglia moderated and guided by a higher regulatory agency – the will, the encephalon, the intellect. Without this higher agency there was disharmony, anarchy, confusion. The reflex arc was fit into a larger, hierarchical layout of the nervous system reminiscent of Aristotle's ancient distinction between the vegetable, animal, and rational soul.

"Consensus," "consentaneousness" and "consentaneity" referred to a new hierarchy of nervous centres in which local, "lower," nervous centres were co-ordinated by increasingly central, or "higher" ones. The criterion for being a higher nervous centre and function was how much it was able to influence the activities of other nervous

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[242]Carpenter, "Lecture 3 on the Nervous System," pp. 939-940.
[243]T. Williams, "Report on the British Annelida," *BAAS*, 159-272 (1852): p. 269.

centres and functions (which in turn were called 'lower'). Robert B. Todd noted that the cerebrum was able to issue "a general mandate for the execution of a certain action", but that the cerebellum was required to control the actual instruments used to carry out any bodily activity. Despite the discrediting of the *sensorium commune,* the cerebrum was still held to be the pre-eminent, highest, nervous centre by 1859.[244]

Because of the recognition of the quasi-independence of the ganglia and reflex arcs, the unity of an organism could only be maintained through the assertion of an organization recognizing the contribution of all of its members. Though the lower nervous "officers" drew their breath according to the "biddings of the higher powers", they were still necessary in a system oddly similar to a reforming constitutional monarchy, wrote an author in a fresh new Liberal publication, the *Cornhill Magazine*:

> Every part of the nervous system makes its influence felt by all the rest. A sort of constitutional monarchy exists within us; no power in this small state is absolute, or can escape the checks and limitations which the other powers impose. Doubtless the brain is King; but Lords and Commons have their seats below, and guard their privilege with jealous zeal. If the "constitution" of our personal realm is to be preserved intact, it must be by the efforts of each part, lawfully directed to a common end.[245]

Where volition, or the mind, was previously thought to be commanding the body to carry out certain actions, now activity was seen as guided by higher nervous centres, ensuring harmonious action in which the energy of each nominally independent and potentially unruly ganglion was moderated and channelled properly.

Terminology and examples of social and political order ("authority"; "presiding"; "delegates"; "insurrection"; "control"; "ill-regulated"; "higher"; "lower") were resources

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[244]Carpenter, "Noble on the Brain," pp. 494, 530-531; R. B. Todd, *The Physiological Anatomy and Physiology of Man*, vol. 2 (London: Parker, 1859), p. 1:205. Todd remarked that the nervous centres were the "laboratories in which the nervous force was generated."

[245]J. Hinton, "What are the Nerves?," *Cornhill Magazine* 5, 153-166 (1862): p. 166.

which researchers used to better convey their point. And in turn, examples were transferred the other direction - from the neurosciences to society - to help describe social and political order. Herbert Spencer copied terms such as "consensus" and "consensual" from the description of involuntary muscle movement and used them to describe social functions. Alison Winter has noted how other Victorian social commentators used the term "consensus" to depict the 'agreement' of different units in a single system – describing how a group became or stayed coherent while nonetheless preserving each member's individuality.[246] The term "consensus" allowed for the portrayal of a flexible hierarchy of authorities – a flexible hierarchy was the only way that a group could stay coherent while still retaining the individuality of its members.

This chapter has mentioned, but left undeveloped, how the immediate social interests of various factions promoted particular research agendas. There are obvious links between researchers' views on internal bodily order and their beliefs about social order, as shown in histories by L.S. Jacyna and Adrian Desmond.[247] Christopher Lawrence has outlined a case in the Scottish Enlightenment in which views about social order were congruent with views about nervous order.[248] Roger Cooter and Steven Shapin in a similar vein have explained phrenology as a vehicle of self-improvement for those (including merchants and professionals of the emerging shopocracy) who felt unjustly excluded from British institutions in the first half of the 19th century.[249]

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[246]R. Cooter, "The Power of the Body: the Early 19th Century," in *Natural Order: Historical studies of Scientific Culture*, ed. B. Barnes and S. Shapin (Beverly Hills, Calif.: Sage, 1979), 73-92, p. 87; Winter, *Mesmerized*, pp. 308, 341-343.

[247]Desmond, *The Politics of Evolution*, pp. 5-7; Jacyna, "Immanence or Transcendence," pp. 327-328.

[248]C. Lawrence, "The Nervous System and Society in the Scottish Enlightenment," in *Natural Order: Historical Studies of Scientific Culture*, ed. B. Barnes and S. Shapin (London: Sage, 1979), 19-40, pp. 27-32, 35.

[249]Cooter, *The Cultural Meaning of Popular Science*, pp. 113, 47, 84-86, 6-7, 10-11, 32; M. Douglas, *How Institutions Think* (Syracuse, N.Y.: Syracuse University Press, 1986), pp. 48-49. Shapin points out that

These histories can be built upon by using the common context of the disunity of the organism. We can set out a continuum of researchers who talked about nervous centres as political agents, and who in turn discussed centralized versus decentralized authority-schemes, unitary against devolved sources of power. People who tended to favour authority and a clear-cut hierarchy in politics also tended to rule out any division of consciousness precisely because any division of consciousness meant a division of internal authority. Without a unitary consciousness, who or what controlled the myriad body parts?

Thus different researchers might tend to support different versions: a researcher favouring Tory politics might believe in a single unitary seat of the nervous system, or a single supreme seat of nervous power to which all of the other nervous centres deferred. He might tend to believe in a *sensorium commune*, the power of one's soul or will to issue commands to the rest of one's body. Recall Badham's statement in the Tory-leaning *Blackwood's:* it was inconceivable for him to suppose that an earthworm could be anything but a unity, for then we would have to suppose a "*conclave* or *council* of brains in *one being*".[250] Yet Badham's belief in the unity of earthworms was entirely at odds with the 'implications' of his earlier mutilations of them.

On the other hand there are the phrenologists. Strongly linked with democrats and reformers, the common context of the disunity of the organism allows us to see how phrenologists devolved the mental life to faculties, just as they called for the devolution

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phrenologists were also challenging the existing order by trying to change the criteria by which one judged the credibility of a claim. For them the validity of their field lay in its openness - that it was based upon facts that any person could clearly judge for herself - and in its utility, that knowledge ought to be sought for practical ends. S. Shapin, "Phrenological Knowledge and the Social Structure of Early 19th-Century Edinburgh," *Annals of Science* 32, 219-243 (1975): pp. 235-238; S. Shapin, "Homo Phrenologicus: Anthropological Perspectives on an Historical Problem," in *Natural Order: Historical Studies of Scientific Culture*, ed. B. Barnes and S. Shapin (Beverly Hills, Calif.: Sage, 1979), 41-71, p. 61.

[250] Badham, "Supposed Sensibility of Insects," p. 590. Emphasis in original.

of political power to a larger political constituency than the British aristocracy. These 'devolutionists' – like political radicals – would deny outright the problem of authority, as it wasn't always clear that organizations required a single unitary authority from above. Because they rejected even the necessity for a clear chain of authority, they were far more willing to entertain the possibility of a disunified organism or other disunified organization.

The symmetry between political outlooks and one's stance on the plurality of the mental organs was strengthened by the intriguing image of the brain and nervous system as a political system. That various political disputes were rooted in the common assumption of compound individuality is unsurprising, because they were shaped by whether one accepted or rejected whether the individual was a unity or a collective.

This chapter has shown that in the first half of the 19th century, the source of thought and consciousness in a person was devolved into a community of ganglia and other mental elements. It provides an interesting, strangely literal, twist on the sociologist Ludwig Gumplowicz's dictum that "What actually thinks within a person is not the individual himself but his social community," or the similar Durkheimian point that the individual mind is often best portrayed as a society writ small.[251]

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[251]Ludwig Gumplowicz, *Grundriss der Soziologie*, Vienna, 1905, 268; quoted in Jerusalem, "Die soziologische Bedingtheit des Denkens und der Denkformen," in *Versuche zu einer Soziologie des Wissens*, pp. 182-207, ed. Max Scheler, Leipzig: Duncker und Humbolt, 1924, p. 182, then cited in L. Fleck, *Genesis and Development of a Scientific Fact*, ed. T. J. Trenn and R. K. Merton, trans. F. Bradley and T. J. Trenn (Chicago: University of Chicago Press, 1981), pp. 46-47. The history of different sociologists taking up this phrase might reveal some interesting links. "…writ small": Douglas, *How Institutions Think*, p. 45.

CHAPTER 2
SYNTHESIS: DEVELOPMENT BY THE FUSION OF PARTS

> Now it is well known that during development there is a tendency in nerves to approach and unite with each other, the lateral cords and ganglia are more closely approximated, and the ganglia in the anterior part of the body approach and coalesce into one mass.
>
> -George Newport, "On the Nervous System of the *Sphinx ligustri* (Part 2) during the Latter Stages of its Pupa and its Imago State; and on the Means by which its Development is Effected," *Philosophical Transactions of the Royal Society* 124 (1834): 389-423, 404.

> The reduction of many identical parts to fewer, more specialized, and more coordinated organs is an ancient truth of biology that has often been rediscovered. It is generally known to evolutionary biologists today under a later incarnation as 'Williston's Law.'
>
> -Stephen Jay Gould, *Ontogeny and Phylogeny* (Cambridge, MA: Belknap Press, 1977), 46.

When we use the term "development" today – for example, in the embryological context - it is difficult not to think of it as a move from a less-specialized to a more-specialized state, as a process of differentiation. But the term "development" was flexible enough to permit its adaptability: different people using this word over time meant different things. This chapter thus presents a different picture of development: a process whereby development occurred through fusion, compounding, and coalescence.

Using habits of reasoning to organize this dissertation allows me to depict this developmental process as coalescence as a corollary to what John Pickstone has called "analysis." Against analysis, I present *synthesis* – by which I mean the compounding of simple elements to form more complex systems, structures and organizations. When certain biologists and medical researchers discussed "development" in terms of discrete elements fusing to form composites they can be seen as discussing *synthetic development.*

This synthetic development was described using various words – as “anchylosis”; “approximation”; to “coalesce”; to “compound”; to develop “centripetally”.

We shall see that one important analogy used by these researchers was the process of metamorphosis, whereby the larva developed into a pupa and the pupa into an imago (the ‘perfect insect’) – through the visible fusion of its segments. As suggested by Newport’s statement in the epigraph, between 1830 and 1845 competent researchers thought that development through synthesis was obvious, and there was therefore less emphasis upon development as *specialization* and *differentiation.* In chapter four and five I show how it was only with the establishment of palaetiological dominance that “development” (for instance in terms of Darwinian descent with modification) was associated with centrifugal differentiation, not centripetal compounding, or coalescence, or “integration.”

This chapter focuses upon one important aspect of development as synthesis: the compounding of nervous elements (“ganglia”) into concentrated nervous organs. To repeat, researchers including Georges Cuvier, Robert E. Grant, and Richard Owen already proposed taxonomic groups using the nervous system as an index. Implicit in these schemes was the location of animals in a hierarchy by virtue of the concentration of their ganglia in a cerebrum or nervous / spinal cord. A nineteenth century *scala naturae* can therefore be said to have existed because of the assumption that an individual developed through the fusion of its simple, nominally independent parts.

The belief in development through fusion also reinforced recapitulationists’ beliefs. Ganglia were thought to coalesce in the individual animal as it developed (not just as an embryo, but also where the larva metamorphosed into a pupa and then a mature insect); metamorphosis resembled the fusion of ganglia as the researcher 'ascended' the animal series (for example, comparing the dispersed ganglia of an earthworm with the more concentrated ganglia of an insect). Development as synthesis gave the recapitulationist scheme more credibility that in turn strengthened researchers' willingness to use it and interpret their evidence accordingly. For brevity this chapter will use the slight anachronism, "cephalisation", to refer to the process.[252]

FOREIGN EXEMPLARS OF DEVELOPMENT THROUGH FUSION

In 1824 Etienne Serres used embryology to complement comparative anatomy. Embryological findings could be interpreted through comparative anatomy and comparative anatomy through embryology: using one finding to understand the other was known as embryological repetition. But this interpretation referred to organs, not entire organisms – the most advanced organs repeated the simpler versions of the plan, and animal shapes were compounds of variations of these simpler organ plans.[253]

Serres used the nervous system as a taxonomic index, associating simpler organisms with younger ones. In his view the nervous system of mature molluscs and insect larvae both consisted of two separate strands of ganglia and fibres; as the insect metamorphosed, its strands fused and concentrated around the oesophagus and then behind it too. But the mollusc's nervous system remained dispersed, allowing a parallel

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[252]"Cephalisation" was coined in 1862 by the American James D. Dana, according to the OED; it first appeared in his *Manual of Geology*.

[253]E. Richards, "The German Romantic Concept of Embryonic Repetition and its Role in Evolutionary Theory in England up to 1859" (Ph.D. Thesis, University of New South Wales, 1977), pp. 94-96, 155.

to be drawn with insect larvae – that the nervous system of the insect larvae was often portrayed as moving through the dispersed mollusc nervous system.[254]

Serres was committed to epigenesis against preformationism. But he was committed to a distinctive vision of epigenesis, noting that the embryo's development took place from different centres. Instead of the pattern of development moving from a central point and outwards to a circumference, or centrifugally, the embryo was seen as developing centripetally, from the circumference to the centre. Organs placed along an organism's central axis were initially double or multiple, but then grew together into a single unit during development; nerves appeared in the organs first and then fused into a nervous cord. Serres called these processes the "law of affinity" (as each organ-half attracted one another), and the "law of conjugation" (as they united). In a series of Paris speeches picked up by the *Medical Times* in 1842, Serres proclaimed that the law of centrifugal development, which supported pre-existence, had "completely fallen to the ground" and epigenesis (supporting centripetal development instead) had "again risen in its place."[255] Again, notice the emphasis upon development as synthesis, in which the parts formed through coalescence. This was not the same thing as development through differentiation and specialization.

Researchers who spoke of recapitulation thus not only spoke about organ-types "passing through" earlier and simpler organs; just as important was the recapitulationist emphasis upon the greater concentration and coalescence of parts. (Note the resemblance of the word "coalescence" to the word "coalition," which originally meant the growing

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[254]E. R. A. Serres, "Explication du Système Nerveux des animaux invertébrés," *Annales des Sciences Naturelles* 3, 377-380 (1824): pp. 379-380.

[255]Appel, *The Cuvier-Geoffroy Debate*, pp. 122-123; E. S. Russell, *Form and Function : a Contribution to the History of Animal Morphology* (Chicago: University of Chicago Press, 1982), p. 81; E. R. A. Serres, "On the Laws of the Development of Organs (Part 1)," *Medical Times* 7, 19-20 (1842): p. 19.

together of parts; they both come from the Latin word *coalescere,* meaning to grow together.) The emphasis upon divergent differentiation and specialization that we associate with embryology wasn't as strong in this type of development as it was to be in later developmental schemes like von Baerian embryology; earlier layouts like Etienne Serres's depicted the development of the embryo as recapitulating a linear taxonomic series. Moreover in these earlier schemes there seemed to be less interest in where the parts initially came from. Serres noted that very young embryos were "composed of various fragments, divided and sub-divided *ad infinitum*," and development united and associated these disjointed parts, like the 24 separate vertebrae, into a corporate whole.[256]

Serres's teachings were imported into British research with precisely this message of coalescence, through translated articles such as the one in the *Medical Times* and through reviewers. Using the language of analysis and synthesis, Serres stated that young embryos began as "various fragments" which developed through association (the joining together of "elements of organisms") or by incorporation (where the elements became "blended together").[257] British reviewers also interpreted him to be teaching centripetal development. One noted Serres's statement that in all embryos the nerves were formed before being "put in communication with the brain and spinal cord"; this initial scattering pointing to a likeness between vertebrate embryos and the dispersed nervous systems of mature invertebrates.[258]

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[256]Desmond, *The Politics of Evolution*, pp. 52-53; E. Richards, "A Question of Property Rights: Richard Owen's Evolutionism Reassessed," *British Journal for the History of Science* 20, 129-171 (1987): p. 133; Serres, "Development of Organs (pt. 4)," pp. 115-116; R. Williams, *Keywords: a Vocabulary of Culture and Society* (Oxford: Oxford University Press, 1976), p. 67.
[257]Serres, "Development of Organs (pt. 4)," pp. 115-116.
[258]S. Smith, "Nervous System (Part 2)," *Westminster Review* 9, 451-480 (1828): p. 459.

Likewise, an anonymous reviewer for the *Medico-Chirurgical Review* pointed out Serres's great contribution to embryology – to settle a great debate about how development proceeded. The reviewer distinguished between two directions: one was the "'Centripetal or Eccentric Theory of Development,' in opposition to the more ancient one, to which the appellation of the 'Centrifugal Theory' has been given." Before Serres it was thought that the great divisions of the body formed *centrifugally*, from a centre and moving outwards, with the heart, brain and spinal cord appearing first and then sending blood vessels and nerves outward. There were disagreements about precisely which body part appeared first, but researchers had been in agreement about development's centrifugal direction. Serres's contribution was to reverse the direction of development, proposing that the embryo developed in the opposite way: *centripetally* or eccentrically, from two lateral halves towards a central ("mesial") line, with blood-vessels and nerves developing in each lateral half of the embryo and then gradually coalescing in the centre.[259]

Because of Serres's reversal, the reviewer announced how recapitulation through centripetal coalescence explained a number of morphological questions. It explained anatomical symmetry - sex organs were symmetrical because the young foetus had begun with two halves which then coalesced centripetally. The phenomenon of hermaphrodism could be seen as an "embryonic" condition of the sex organs. The different *embranchements* also supported centripetal development because of their relationship with nervous structure. Radiata showed the first sign of a distinct nervous system; Mollusca had two ganglia. Articulata had animals with bodies divided into distinct,

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[259]Review of *Philosophie Anatomique* by Geoffroy Saint-Hilaire; *Histoire des Anomalies de l'Organization* by Geoffroy Saint-Hilaire; and *Sketch of the Comparative Anatomy of the Nervous System* by John Anderson, *M-CR* 27 (n.s.), 83-128 (1837): pp. 87-88.

repetitive segments and divisions of the nervous system, and those animals with more concentrated ganglia and a "supra-oesophageal" ganglion like the brain were deemed to be higher than other articulates.[260]

By 1830 British researchers' views had been influenced by the appearance of two works from another culture: Germany. Friedrich Tiedemann's *Anatomy of the Foetal Brain,* translated and published in Edinburgh in 1826, praised Serres's research, particularly the doctrine of centripetal development, for it was also obvious to Tiedemann that the nervous system developed from the circumference to the centre. For Tiedemann, Serres had answered the question of whether the spinal marrow was a continuation of the brain or the brain a continuation of the spinal marrow. Centripetal development showed the latter – that the brain was produced by the "superior part" of the spinal marrow, the medulla oblongata.[261]

To further investigate development's direction in embryos, Tiedemann had to work with a substantial collection of specimen-brains (preserving them in spirits of wine, then hardening the brain in alcohol, then in an alkali solution for two days). Because of his work, Tiedemann declared that comparative anatomy showed the development of the nervous system from the simplest animals to the most complex (humans), and the nervous system best showed the direction from simple to complex through fusion: "There is no set of organs, in the formation of which, we find so perfect a gradation from the simple to the compound, as in the cerebral and nervous system".[262]

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[260]"Review of *Philosophie Anatomique,*" pp. 114, 101-105.
[261]F. Tiedemann, *The Anatomy of the Foetal Brain, with a Comparative Exposition of its Structure in Animals*, trans. L. Jourdan Antoine-Jacques and W. Bennett (Edinburgh: John Carfrae, 1826), pp. 23-24, 149-150.
[262]Tiedemann, *The Anatomy of the Foetal Brain*, pp. 8-9, 2-3.

A year after Tiedemann's work appeared in Britain, Carl Gustav Carus's *Introduction to the Comparative Anatomy of Animals* was published in London. A reviewer for the *Edinburgh New Philosophical Journal* lauded it.[263] Later translations of Carus's work showed his depiction of the simplest nervous system as a "ray" – the nerve itself – and a "centre" – a nervous ganglion. A later British article showed how Carus thought the vertebrate nervous system developed.

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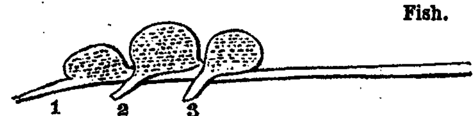
ing spirit is never obtained, the structure or formation of the brain divides itself throughout into a trinity, and every where are we enabled to distinguish very clearly three different masses of the brain, to which we apply the names of anterior, middle, and posterior cerebrum.

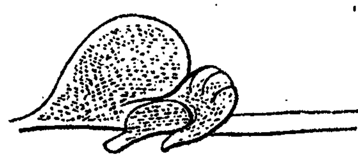
Fish.



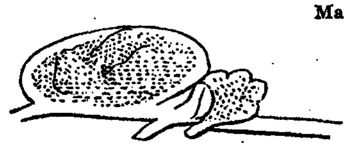
1, Olfactory, 2, Optic, 3, Acoustic nerves.

Amphibian.

Bird.

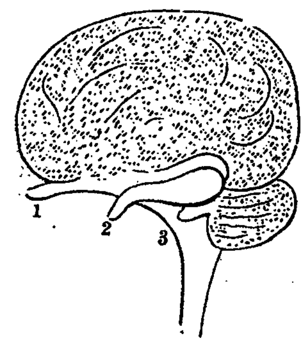


Mammal.



Of these three cerebral substances the middle part most reminds us of the simple cerebral knot which we meet the brain, being likewise of considerably greater size, and of finer structure; while the middle portion, on the contrary, remains limited both as to place and development.

Man.



1, Olfactory, 2, Optic, 3, Acoustic nerves.

4. The human brain, in its gradual development, is a true repetition of the forms met with in the lower orders of animals; in the embryo of two months it resembles that of fishes, and it acquires its full size at the period of seven years, but continues, as regards the inward structure of its fibres, in a progressive state towards perfection. On the circumstances above mentioned, namely, on the gradual increase of the brain's tripartite development, we base

Gradual coalescence of brain-masses in vertebrates, from C.G. Carus, "On a New Cranioscopy Upon a Scientific Foundation," *London Medical Gazette* 2 (n.s.) (1844): 680-684, p. 683.

[263]Richards, "Embryonic Repetition", pp. 190-192; *Edinburgh New Philosophical Journal* 4, 1827-1828, pp. 206-208.

Another German anatomy book appearing in translations (appearing first in French and then in English) was J.F. Meckel's *Traité Générale d'Anatomie Comparée / Manual of General, Descriptive and Pathological Anatomy.* As early as 1811 Meckel had argued that simpler animals had similar but poorly co-ordinated parts, while more developed ones had increasingly distinct and specialized organs: development was thus a progressive co-ordination and specialization of these similar parts.[264]

Meckel almost came to work in Britain: in 1827 he was offered the chair of comparative anatomy at the new London University to increase that new institution's reputation. But he asked for too much money (including £1000 to move his museum to London), so the chair was given to Robert E. Grant instead.[265] In his *Traité / Manual,* Meckel set out two grand classes of body parts, one the class of simple organs ("parts similares") which repeated in the body. The other class was the compound organs, or dissimilar parts. The simple organs often united to attain a special function, bodies could thus be seen as an "assemblage of compound systems". The unification of these different simple organs occurred through what Meckel (and his translators) called "anastomosis", and an example of this unification could be seen in the nervous system, which was really a double system consisting of two lateral corresponding portions.[266]

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[264]Gould, *Ontogeny and Phylogeny*, p. 46.
[265]Desmond, *The Politics of Evolution*, pp. 82-83; Mazumdar, "Anatomical Physiology and the Reform of Medical Education," p. 236.
[266]J. F. Meckel, *Manual of General, Descriptive, and Pathological Anatomy*, trans. A. J. L. Jourdan, 3 vols. (Philadelphia: Carey and Lea, 1832), pp. 1:26, 1:153, 1:31-32.

DISSEMINATION OF THE FOREIGN WORK THROUGH TEXTBOOKS

An early British introduction to these foreigners' works appeared in the *Westminster Review,* penned by Thomas Southwood Smith. It not only reviewed works by Serres and by Carus, but also by the phrenologist Johann Gaspar Spurzheim.[267] Smith repeated the continental researchers' claim that the concentration of the nervous system provided a taxonomic key: "The more the volume of the brain exceeds that of the spinal cord, the higher the animal is placed in the scale of being. In general, as we descend, the spinal cord is large, and the brain small."[268]

Five years later, Robert Grant's lectures on comparative anatomy appeared in the *Lancet;* any pecuniary loss for Grant (who depended upon student tickets for income) would probably have been offset by the ensuing publicity. In these lectures, he repeated that the nervous system became more complex as other organs became more complex. His other comparative anatomy lectures at London University / University College London used recapitulation too, as he stated that his task was to trace the "human organs coming successively into being, and rising in complexness", in order to determine the resemblances between the transient forms presented by men's organs during their development and their permanent forms in inferior animals. By focusing on organs, the comparative anatomist could find that the animal kingdom had gradually developed "from simple to compound."[269]

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[267]S. Smith, "Nervous System (Part 1)," *Westminster Review* 9, 172-198 (1828): p. 172. While Jonathan Topham notes Charles Bell's jaded view of Southwood Smith's materialistic view of life, note their common, analytical, focus upon the disunity of the organism. J. R. Topham, "Science and Popular Education in the 1830s: the role of the *Bridgewater Treatises,*" *British Journal for the History of Science* 25, 397-430 (1992): pp. 416-418.
[268]Smith, "Nervous System (Part 1)," p. 186.
[269]Grant, "The Study of Medicine," pp. 44-45.

To repeat, Grant also classified the animal kingdom by their nervous structure but introduced terms such as *cyclo-neura, diplo-neura, cyclo-gangliata* and *spini-cerebrata.* Grant's classification scheme was later taken up by at least two other British researchers in the 1830s, Samuel Solly and Thomas Rymer Jones.[270] Grant's scheme seems to have referred to four different arrangements of the nervous system. But leaving aside the question of whether Grant thought that there were animals bridging the gap between *embranchements,* the concentration of an animal's nervous system established its place on the scale, showing how strongly it was an integrated individual. Metamorphosis saw the "drawing together and uniting" of separate segments, nerves and blood vessels.[271] The words "anastomose" (to connect or join in together in a network) and "anchylosis" (the fusion of separate parts) frequently appeared in his lectures on development as Grant discussed the coalescence of simpler and repetitive distinct parts into a larger and more complex and harmoniously-functioning whole.

Grant reinforced the message of cephalisation and recapitulation by having his students give back concrete examples of cephalisation and recapitulation. Rather than having his students parrot methodological principles back to him in exams, they were to apply their understanding by solving concrete problems embodying these principles, showing they had learned specific anatomical terms. They were to therefore display their knowledge of a repertoire of paradigmatic examples.[272]

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[270]R. E. Grant, "Animal Kingdom," in *Cyclopaedia* (1836), 107-118, pp. 107-108; T. R. Jones, "Gasteropoda," in *Cyclopaedia* (1839), 377-403, p. 392; Solly, *The Human Brain*, p. 4.
[271]Grant, "Lectures on Comparative Anatomy," pp. 1:399-400.
[272]Barnes, *T.S. Kuhn and Social Science*, p. 18.

On one of Grant's exams, recapitulationist questions included:

12.	In what animals do you find the diaphragm retain permanently its rudimentary state, having only its lateral and external parts formed?	V: 3
16.	State the changes which are observed to take place in the Nervous System of Insects during their metamorphosis to the Pupa and the insect state.	V: 7
19.	Enumerate the parts of their internal structure in which the Cephalopodous Animals resemble Birds.	V: 7
20.	To what extent does the metamorphosis of the Amphibia affect their osseous, nervous, circulating, and digestive systems?	V: 13
21.	Describe the internal structure of amorphous animals, and state the differences which exist between their organization and that of zoophytes.	V: 9
25.	Enumerate the classes of the inferior animals, in which the permanent forms of the Circulating System are analogous to each of the stages of the development of that system in the Mammalia.	V: 15
28.	Where do you find the Nervous System begin to manifest itself in ascending through the animal kingdom; and what are the principal forms it assumes in the different classes, before you arrive at animals possessing a Brain?	V: 17
Total: 28 Questions	*Total value:*	*188*

Questions from an exam given Saturday, 8 July 1831, from R.B. Freeman, *Notes on Robert E. Grant, M.D* (London: Department of Zoology and Comparative Anatomy, University College London, 1964), pp. 9-11.

Grant obviously wanted comparative anatomists to be familiar with recapitulation; the two most valuable exam questions required the student to be familiar with the overall arrangement of the animal kingdom, which Grant set out by the shape of the nervous system. And the most valuable question dealt with the acquisition of a Brain as the surveying anatomist ascended a scale of being.

Successive waves of new anatomists would not only have sat in on his lectures at the London University / University College London (he lectured five times a week and did not miss a single lecture from 1827 until his retirement in 1873),[273] but would have taken tests with questions much like the ones shown above. And through their attendance

[273]Bettany, "R.E. Grant."

at his lectures they would have learned one of the key precepts of compound individuality: that the lower organisms are those having parts with a greater degree of independence, and the higher organisms were those with more specialized and integrated parts. Among the researchers studying under Grant were William B. Carpenter, Charles Darwin, Thomas Laycock, George Newport and Peter Mark Roget.

Pedagogically, cephalisation must have made Grant's work far easier. The use of cephalisation allowed organisms to be arranged in a clear-cut way in textbooks, providing a context that focused students' expectations and allowed them to fit other organisms into the context of cephalisation.[274] It was easy to use: a coherent scheme like cephalisation – just like a numerical series or pattern of declension (*amo, amass, amat, amamus, amatis, amant*) – could be extended to other examples, allowing teachers and students to concentrate their lessons. Once a student grew used to the rationale behind a scheme or series (and this could be done merely through using the series, not by consciously knowing that rationale), she could set aside laborious memorization and repetitive sequences of trial and error. Recapitulation-schemes like cephalisation allowed students to master their lessons with far more ease, allowing them to better retain their "arsenal of exemplars". And they could demonstrate their competence to their teacher on examinations requiring them to use that recapitulation-scheme, showing that they could indeed relate exemplar to the larger law correctly. In turn these schemes helped students to anticipate recapitulation actually occurring in nature: if they became researchers, recapitulation-schemes not only gave them a framework within which to place their

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[274]Barnes, *T.S. Kuhn and Social Science*, pp. 18-20.

individual findings, but they also allowed the researchers to choose from an otherwise-overwhelming number of exemplars.[275]

Thus in 1836 John Anderson not only used Grant's work but also Carus's to discuss how the nervous system formed: the more developed the system, the more "perfect" the connection between individual ganglia.[276] The following year, Anderson's *Sketch on the Comparative Anatomy of the Nervous System* repeated the statement about perfect connections, and reviewers approvingly commented upon his use of recapitulation. One lumped Anderson's work in with Geoffroy's, noting how they both thought that a comparative anatomist could use the nervous system to establish the stages through which an animal passed, just as the embryo's heart also passed through the stages of inferior animals. Since primitive forms of the spinal marrow and encephalon were similar, a reptile embryo had at one stage an "icthymorphous," (fish-like) nervous system. Another reviewer was less than enchanted with the language - Anderson's descriptions "would be intelligible if disembarrassed of the transcendental doctrines by which they are frequently obscured" – but he largely agreed with the conclusion that comparative anatomy traced the gradual development of the nervous system as we moved up the animal scale.[277] Meanwhile, Samuel Solly also discussed the nervous system in

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[275]D. Bloor, "Durkheim and Mauss Revisited: Classification and the Sociology of Knowledge," *Studies in History and Philosophy of Science* 13, 267-297 (1982): pp. 270-271; Bourdieu, *Outline of a Theory of Practice*, pp. 87-88; Douglas, *How Institutions Think*, p. 90; Kuhn, *The Essential Tension*, p. xix. Andrew Warwick's rich work suggests that Kuhn's claim for the importance of exemplary problems in textbooks is insufficient – that while solving these problems are important for students to acquire new skills or competencies, it is left unexplained how students learn exactly how principles are related to these correct problems. Warwick suggests that the final judge of whether students are engaged in proper practice is a competent teacher (thus my note about solving test problems *correctly*). A. Warwick, *Masters of Theory: Cambridge and the Rise of Mathematical Physics* (Chicago: University of Chicago Press, 2003), p. 230.

[276]Anderson, "Sketch of the Comparative Anatomy of the Nervous System," pp. 867-869, 864, 906; Desmond, *The Politics of Evolution*, p. 417.

[277]"Review of *Philosophie Anatomique*," p. 100; Review of *The Human Brain, its Configuration, Structure, Development and Physiology* by Samuel Solly; *The Practical Anatomy and Elementary Physiology of the*

the light of comparative anatomy. His 1836 textbook on the human brain observed the nervous system as it moved from a simple, diffused, set of nervous centres to the complex and concentrated system found in humans. He concluded that the encephalon was simply a mass of fused ganglia.[278]

Richard Owen also lectured upon the "natural groups" into which one could resolve the animal kingdom by examining the concentration of their nervous structure. First, there was a "tribe of beings" with a main centre to their nervous system and a great trunk forming a body axis – the Vertebrata. Second was the Annulosa – a group with a nervous system consisting of two long strings of "medullary matter", united to each other in several or ganglia, these ganglia performing for the surrounding parts "the function of so many brains and for a certain period even to be sufficient for nervous sensibility after the animal has been cut in pieces". Third, animals with a nervous system consisting of unevenly dispersed ganglia joined by nervous threads - Mollusca. Fourth, animals with simple sense-organs arrayed round a centre, giving a "radiant appearance" to the body – Radiata. Fifth, animals that were only masses of transparent, homogeneous, mobile and sensible pulp – the Acrita.[279] In Owen's taxonomy emphasizing compound individuality, he moved from Vertebrates, with the most concentrated nervous systems, downwards to the animals lacking any nervous system.

Nervous System by F. Le Gros Clark; *Sketch of the Comparative Anatomy of the Nervous System* by John Anderson, *Edinburgh Medical and Surgical Journal* 47, 477-485 (1837): p. 479.

[278]Solly performed daily anatomical demonstrations at St. Thomas's Hospital and was one of the last London hospital surgeons to acquire his hospital post by the rather non-meritocratic method of paying a large "apprenticeship" fee. "Account of the Metropolitan Hospitals," p. 3; "Mr. Solly on the Nervous System: Configuration and Development of the Nervous System," *L* 2, 199-200 (1836-1837); Solly, *The Human Brain*, pp. xiii-xvi, 15-16.

[279]R. Owen, Manuscript, "On the Animal Kingdom Generally - Museum Lectures", ND, OPAP, RCS.

Likewise, 1833 and 1842 textbooks by Herbert Mayo both pointed out that comparative anatomy showed the progressive development of the "enkephalon" [sic].[280] Supplanting Mayo's *Human Physiology* came Carpenter's 1839 *Principles of General and Comparative Physiology.* It, and its subsequent editions, became the pre-eminent text of the London medical schools, acting as a more accessible introduction to physiology than William Baly's 1842 translation of Müller's *Physiology.*[281] As a mainly synthetic work Carpenter's text took up not only Continental works on comparative anatomy and physiology, but also new British ones: he cited Carus and Tiedemann, but also Grant, Solly, and Todd's *Cyclopaedia.*

Carpenter also noted that development meant the fusion of simple parts into compound parts. This coalescence allowed similarities to be drawn between different animal groups through recapitulation or cephalisation more specifically. An individual's development could be likened to a move up the animal scale - insect larvae were "on a level" with annelids since both animals' segments were equal, and the ganglia distributed uniformity. For Carpenter, metamorphosis therefore meant that the insect moved past annelids.[282]

Another reason for the popularity of recapitulation-schemes was the necessity for authors to organize their material in a linear way – the use of the textbook itself shaped how facts were to be presented. In an enormously complex world of nature where organisms are linked to others in manifold ways, the requirement that authors present their material in a single serial 'argument' provided an additional need for coherence,

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[280]Mayo, *Human Physiology*, pp. 220-222; Mayo, *The Nervous System*, pp. 84-87.
[281]Carpenter and Carpenter, *Nature and Man*, p. 65.
[282]Carpenter, *Principles of Physiology*, pp. vi, 442-444.

therefore strengthening the utility of schemes like recapitulation. This linearity is obvious from a glance at a table of contents from one textbook.

NERVOUS SYSTEM.

Organization of textbook, from T.R. Jones, *A General Outline of the Animal Kingdom* (London: J. Van Voorst, 1841), ix.

Thus a fourth track might be added to Louis Agassiz's "threefold parallelism" (noted by Dov Ospovat).[283] Where Agassiz had noted a similar progressive developmental sequence linking taxonomy, the embryonic stages of developing animals, and the increasing complexity of the fossil record, the layout of certain textbooks offers another glimpse into - and perhaps shaping - recapitulationism.

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[283]D. Ospovat, "The Influence of Karl Ernst von Baer's Embryology, 1828-1859: A Reappraisal in Light of Richard Owen's and William B. Carpenter's "Palaeontological Application of Von Baer's Law"," *Journal of the History of Biology* 9, 1-28 (1976): p. 14.

Shown above is part of the table of contents of the *General Outline of the Animal Kingdom and Manual of Comparative Anatomy* (1841), written by Thomas Rymer Jones, student of Richard Owen and Professor of Zoology at King's College London. A Welshman, Rymer Jones studied at Guy's Hospital and then in Paris, qualifying in 1833. He joined Robert B. Todd at King's College as the first Professor of Comparative Anatomy in 1836; he contributed a number of articles to Todd's *Cyclopaedia* and later became Fullerian Professor of Physiology at the Royal Institution (1840-1842). But Rymer Jones became best known for his *General Outline*; dedicated to Owen, it was long the "chief book" used by English comparative anatomy students.[284] While T.H. Huxley was to later remark in private that Rymer Jones (along with Grant) had "mistaken their vocation", he grudgingly noted in public that his *General Outline* had "acquired a reputation amongst students."[285] Huxley's quip was nasty but accurate - Rymer Jones was a competent but hack naturalist, content to produce little new research but eager to use his position as a place to make comparative anatomy accessible. Rymer Jones's research followed Owen's work quite closely, and it is because of his lack of novelty that he can be used to depict the utterly conventional, representative state of British biomedical research in the 1840s.

As shown above, the *General Outline* was organized around cephalisation; its very layout depicted the animal kingdom according to the concentration of its nervous centres, from simplest animal element to the most complex compound of these elements. Thus polyps were presented first, and humans last. The development of an individual organism meant that its nervous system coalesced and concentrated, something

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[284]G. T. Bettany, "Thomas Rymer Jones (1810-1880)," in *DNB* (1891).
[285]T.H. Huxley to W.S. Macleay (Draft), 9 Nov 1851, HP, IC, 30.3-30.8; T. H. Huxley, "Owen and Rymer Jones on Comparative Anatomy," *British and Foreign Medico-Chirurgical Review* 18, 1-21 (1856): p. 3.

recapitulated in the higher classes. The "worm-like" insect larva had a series of equal and repetitive ganglia, one in each segment, which meant that it represented the condition of an annelid; but as the larva metamorphosed, the whole nervous chain grew shorter, with the ganglia nearest the anterior (its "head") growing larger and more powerful.[286]

Note the similarity of these assumptions to Grant's exam question #28. Indeed, Rymer Jones saw common links between Cuvier, Owen and Grant: they all used the nervous system as a key to classification, as an attempt to reach a "more natural method of classification". He repeated Owen's assertions that Owen was continuing Cuvier's project, and introduced each animal group by the nomenclature of specific naturalists. Cuvier's *Articulata* was very similar to Owen's *Homogangliata* and Grant's *Diploneura;* Grant's *Cyclogangliata* was the same as Owen's *Heterogangliata* and Cuvier's *Mollusca.*[287]

Jones was ambiguous about whether he was discussing the development of an individual organism, or the place of the organism on the *scala naturae* in comparison with others. But the language of compound individuality is unmistakeable. In articulates he thought it unsurprising that in the "humblest forms" of annelids, every ring "contained a complete nervous apparatus, consisting of a pair of ganglia and a set of nerves destined to supply the particular segment in which they are lodged. All these different brains...communicate with each other by nervous filaments".[288]

The dispersal of nervous centres meant a great deal to whether an organism was a true individual or a group of smaller, simpler, individuals meshed together. Not only

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[286]T. R. Jones, *A General Outline of the Animal Kingdom, and Manual of Comparative Anatomy* (London: J. Van Voorst, 1841), pp. 301-304.
[287]Jones, *Outline of the Animal Kingdom*, pp. 3-12. Jones noted exceptions: Grant's *Diploneura* included *Entozoa* and *Rotifera*, but Owen and Cuvier didn't; Cuvier didn't include cirripedes in *Articulata*.
[288]Jones, *Outline of the Animal Kingdom*, pp. 138-139, 186-187.

were these nervous masses the source of nerves, they were "in fact so many brains presiding over the functions attributable to the individual nerves".[289] The lower the organism, then, the more dispersed its ganglia; and therefore the more "brains" it had dispersed throughout its body, making a single lower organism a compound of potentially independent individuals.

Rymer Jones's later work continued to take up problems about compound individuality. In Royal Institution lectures in the mid-to-late 1840s, he noted that it was a common error to look upon an animal as being the same creature throughout its entire existence – "and yet no dogma was ever laid down so contrary to every-day experience."[290] Just as Shakespeare stated that every man played many parts, his acts being seven ages, this disunity was also the case for animals.

> Every animal, during the progress of its life, plays the parts of many different animals; and that under such diversified forms, that at successive periods of its existence it cannot in strictness be regarded as the same creature...every living being is, in fact, a succession of perfectly distinct animals growing one out of the other.

To illustrate disunity, Rymer Jones pointed out that the frog was formerly a distinct fish, a distinct tadpole with gills, and even a distinct egg – just like a caterpillar, which was first a larva, then a chrysalis, and finally a butterfly.[291]

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[289]Jones, *Outline of the Animal Kingdom*, p. 692.
[290]T. R. Jones, *The Natural History of Animals, being the Substance of Three Courses of Lectures Delivered before the Royal Institution of Great Britain*, 2 vols. (London: John Van Voorst, 1845-1852), p. 2:41.
[291]Jones, *The Natural History of Animals*, pp. 1:296-298.

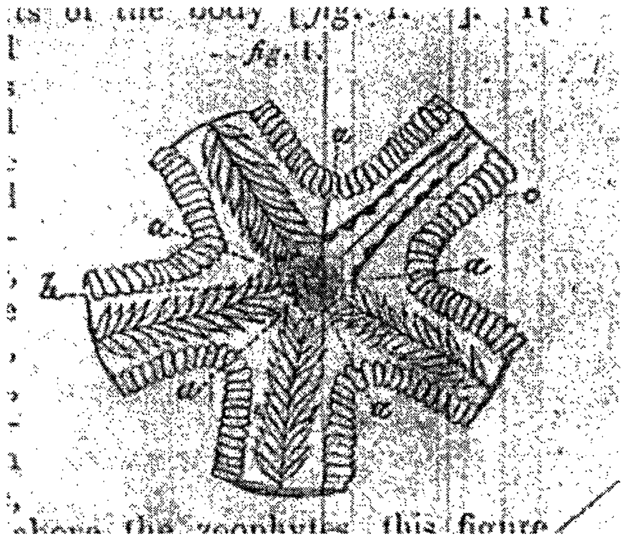
HUMBLE STARFISH

Rymer Jones associated the repetition of any organ in an individual with its simplicity and lowness on the scale of being. Lower creatures had a continuous repetition of their physiological "apparatus" – thus parasites such as tapeworms had generative organs repeating in every segment. Rymer Jones noted that an earthworm was able to reproduce small portions of its body: hadn't Antoine Dugès - who cut up to eight of its anterior rings away only to witness them grow back - proven this regenerative power? In radiates, the lowest crinoids, *Comatula*, had a central part containing its central organs, and around this central disk's margins grew five arms which then divided into long radiating branches themselves. Other members of this group (echinoderms) progressively concentrated upon the primary structure, the central part becoming proportionately larger than the arms, gradually obliterating the radiating form.[292]

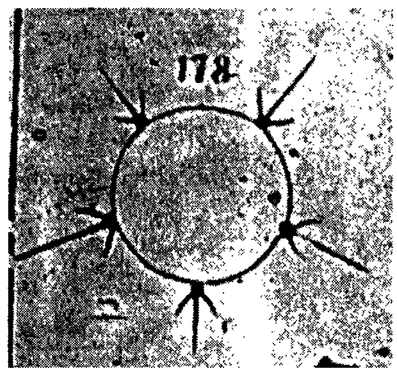
Indeed, it was the crinoid's relative, the starfish, which was one of the most repeated exemplar organisms. Its humility was misleading, for it was frequently used to illustrate grand concepts. The example of the starfish – using the common genus *Asteria* - was transmitted in a fairly clear way from Germany through successive British textbooks. Solly claimed that Tiedemann's beautiful drawing of a starfish nervous system first appeared in 1815, a ring surrounding the oesophagus and a nervous filament running off to each ray.[293] Southwood Smith's dual 1828 articles on the nervous system both showed

[292]Jones, *Outline of the Animal Kingdom*, pp. 186-187, 211. Dugès discussed compound organisms as coalesced from simple ones: "Un organisme est comme un animal élémentaire ou simple; plusieurs organismes réunis forment un animal composé." A. Dugès, "Mémoire sur la Conformité Organique dans l'Échelle Animale, lu à l'Académie des Sciences, Séance du 18 Octobre 1831," *Annales des Sciences Naturelles* 14, 254-260 (1831): p. 255.
[293]Solly, *The Human Brain*, pp. 13-15.

the same drawing of a starfish, noting that although Cuvier was the first to suspect that the whitish ring surrounding the starfish mouth was a nervous system, Tiedemann described it best.



Starfish / general nervous system, from S. Smith, "Nervous System (Part 2)," *Westminster Review* 9 (1828): 451-80, p. 451.



Starfish nervous system in which the simple "knots" represent ganglia and lines represent nerves, from W.B. Carpenter, *Principles of General and Comparative Physiology,* 1st edition (London: John Churchill, 1839), fig. 178.

Though the drawing was exactly the same in both articles, the caption accompanying it changed: the first (January) article's drawing was given as Tiedemann's diagram of a starfish nervous system,[294] while the second (April) article no longer referred to the specific animal portrayed by the diagram. In a subtle move to generalize his example, Smith stated that the starfish nervous system was the "fundamental type".[295]

Other mentions of the starfish nervous system followed Smith's article. In 1832 Charles Bell stated that a circle of nervous threads surrounded the starfish mouth.[296] In 1833 Robert Grant showed his class a diagram of the "lowest form" of the nervous

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[294]Smith, "Nervous System (Part 1)," p. 179.
[295]Smith, "Nervous System (Part 2)," p. 451. Other exemplars given by Smith include the earthworm and the leech, in which each segment possessed separate intestinal expansions, circulatory vessels, sex organs and nervous circles: "each segment of the body may almost be regarded as a separate individual." Smith, "Nervous System (Part 1)," pp. 180-181.
[296]Bell, "Physiology of the Brain," p. 684.

system in the animal kingdom, the white and opaque circular nervous chord surrounding the starfish mouth. That same year Herbert Mayo gave as an example of a radiated animal the starfish, with several segments arranged around a centre and the nervous cord developed equally around it.[297] Likewise, John Anderson noted the "primary nervous ring", which according to the "laws of philosophical anatomy" was the essential base of all of the varied forms of the nervous system. Anderson gave as its exemplar the starfish, noting Tiedemann's description. In 1836 Samuel Solly cited the starfish as an example of an animal with its power equally diffused amongst its ganglia, and that its nervous ring constituted the most uniform portion of the nervous systems of higher animals. Solly didn't copy Tiedemann's diagram but included his own rendition of a starfish nervous system instead, drawn from a specimen in the King's College London Museum.[298]

In 1838 Rymer Jones's *General Outline* cited Tiedemann's findings that *Asteridae*'s nervous system was a simple circular cord running around its mouth.[299] The next year W.B. Carpenter noted Tiedemann's findings too, though noting that Tiedemann must have been mistaken when he did not notice any ganglia distributed around the circle. He also included a more abstract diagram of the starfish nervous system, shown above. In 1840 Carpenter again gave the starfish as an example of an organism in which the ganglia were all alike and none of which had a "*presiding*" character. In 1842 Mayo once again used the starfish as the example of the simplest nervous system.[300]

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[297]This diagram is explicitly referred to but not published in the *Lancet.* Grant, "Lectures on Comparative Anatomy," p. 2:483; Mayo, *Human Physiology*, pp. 220-222.
[298]Anderson, "Sketch of the Comparative Anatomy of the Nervous System," p. 867; Solly, *The Human Brain*, pp. 33, 13-15, 173.
[299]Jones, *Outline of the Animal Kingdom*, pp. 158-159.
[300]Carpenter, *Principles of Physiology*, p. 435; idem, "Lecture 3 on the Nervous System," p. 861. Emphasis in original. Mayo, *The Nervous System*, pp. 14-15.

The recurrence of the starfish example in pedagogical texts shows its utility as a concrete representation of the simplest nervous system. It also shows that many researchers may have been content to merely parrot what was stated before them: they were merely following a habit. Where previous histories emphasized the role of grand abstractions in British biomedical research, like *naturphilosophie*, "archetypes" or "romanticism," it is now instructive to understand exactly which examples were used to embody these concepts. Indeed, these instances may have existed before the laws of philosophical anatomy were enunciated, in which case it would be instructive to note how the concrete exemplars became abstractions, as in the starfish's passage from the depiction of a very particular organism by Smith to its abstraction by Carpenter. In Britain the students may not have been taught to see the nervous system in terms of abstract rules or ideal shapes, but instead as an aggregation of concrete instances. In that concrete sense, then, the starfish was accepted as a 'paradigm' of a nervous system by competent researchers, taught to students, and then extended to other problems of the nervous system.

Once those students went onto more specialized research, cephalisation helped them once again. As part of a larger recapitulationist scheme, cephalisation enabled researchers to relate exemplars in a web of similarity, allowing both the researcher and student to see new problems as akin to problems previously encountered and mastered. Once "that likeness or analogy has been seen, only manipulative difficulties remain."[301]

Cephalisation and other forms of recapitulation made coherent the various analogies drawn and resemblances made between concrete instances of animals and embryos. It was a kind of mental routine helping researchers decide how to group

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[301]Kuhn, *The Essential Tension*, pp. 305-307.

different exemplars together; by conferring 'sameness', cephalisation covered over gaps in complex chains of reasoning and strengthened what might otherwise be seen as leaps of logic or evidentiary problems.[302]

RECAPITULATION IN RESEARCH

Various types of recapitulation allowed researchers to group embryos and groups of animals into similar classes called "stages", enabling fruitful comparisons to be made. Three examples show how these speculations had to be satisfying, even 'thinkable', by following the habit of analysis and synthesis.

Richard Grainger's *Observations on the Structure and Functions of the Spinal Cord,* which to repeat supported the reflex arc anatomically, subtly assumed cephalisation by understanding the lowest organisms in terms of the highest. By noting that higher organisms had more concentrated and integrated nervous systems, Grainger descended the scale from the perfect animal to the lowest. As he descended he noted how the body's motions became independent of the brain and more "reflex" in character; this growing disunity accompanied a change where the nerves contained an every-greater proportion of white fibres ending in the grey matter of the spinal cord and not the grey matter of the cerebrum.

In other words, the nerves of the lower organisms were increasingly centred upon the individual ganglia of each spinal segment; conversely the higher organisms could manifest higher levels of nervous activity centred upon the biggest and most concentrated nervous centre, the cerebrum. Nervous activity centred on the cerebrum could be called

[302]Douglas, *How Institutions Think*, pp. 63, 90.

voluntary control. Cephalisation motivated and guided Grainger's search for evidence, allowing him to associate the reflex arc (a new finding) to very fine distinctions of nervous anatomy, findings of comparative anatomy and the existence of higher levels of voluntary control in certain animals.[303]

Second, cephalisation worked the other way too, allowing the highest organisms to be understood in terms of the lowest. One example was the extension of the reflex arc to the activity of the human mind. Thomas Laycock, who also took Grant's courses, emphasized how J.F. Meckel, Tiedemann, Serres and Solly had all established that the brain and spinal cord were made out of compounded ganglia; because these concentrated nervous centres were compounded of smaller nervous centres he drew the analogy that all mental activity must be similarly compounded out of reflexes centring on each ganglion. He, with William B. Carpenter, both extended reflex activity to higher and higher portions of the central nervous system until even the highest forms of brain activity could be described as compounded reflexes.[304]

Laycock's work on the "reflex action of the brain", and his career arc, was quite similar to his rival Carpenter's:[305] besides taking Grant's classes, Laycock (after studying in Paris and Göttingen) also contributed articles to the *British and Foreign Medical Review* in the 1840s. In a slightly more heterodox fashion, however, Laycock also wrote articles for the *Phrenological Journal* and John Elliotson's *Zoist.*[306] His phrenological

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303Bettany, "R.D. Grainger"; Grainger, *Observations on the Spinal Cord*, pp. 42-44; Leys, *From Sympathy to Reflex*, pp. 301-302.
304Clarke and Jacyna, *Neuroscientific Concepts*, pp. 143, 140; Leys, *From Sympathy to Reflex*, p. 316.
305L. S. Jacyna, "The Physiology of Mind, The Unity of Nature and the Moral Order in Victorian Thought," *British Journal for the History of Science* 14, 109-132 (1981): pp. 112-113.
306T. Laycock, "The Lecture Mania," *The Zoist* 1, 5-25 (1843); idem, "Lectures on Phrenology," *Phrenological Journal* 20, 1847); T. Seccombe, "Thomas Laycock (1812-1876)," in *DNB* (1892). *The Zoist* was considered heretical by mainstream phrenologists because it allowed for mesmerism. Cooter

leanings may have motivated his extension of the reflex to mental activity - like other phrenologists, he denied the existence of a unitary *sensorium commune.* Instead Laycock decentred the sensorium, seeing it as a "common circle" made out of several central nervous points,[307] a structure resembling a starfish's nervous system.

Finally, recapitulation (this time as the concentration of parts) could be deployed to refute rivals' findings. In an 1843 letter on the Pearly Nautilus, Richard Owen noted a rival proposal. The French researcher Achille Valenciennes had proposed that the sheaths of Nautilus tentacles could be compared to outgrowths of the fleshy suckers of all Dibranchiate cephalopods –Valenciennes thought his comparison meant that naturalists should use suckers instead of tentacles to compare cephalopods. But Owen responded that the cephalic tentacles of a Pearly Nautilus were numerous and comparatively small, indicating Nautilus's lower place on the scale. Because development meant a reduction in number, an increase in size and "perfection", Valenciennes's proposal wasn't "conformable with the general law of development."

Valenciennes had proposed a reversal of what might be expected: a lowly-organized cephalopod like the Nautilus could not have two large and highly concentrated suckers, while a more highly organized species had two hundred smaller and simpler ones.[308] Owen felt confident enough to publicly reject Valenciennes's proposal based simply upon the assumption that higher organisms had more coalesced parts.

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considers its rejection an attempt at demarcation by members of a field that was itself rejected by other respectable researchers. Cooter, *The Cultural Meaning of Popular Science*, pp. 79-80, 90.

[307]T. Laycock, "Analytical Essay on Irregular and Aggravated Forms of Hysteria," *Edinburgh Medical and Surgical Journal* 52, 43-86 (1839): p. 53. See also F. E. James, "The Life and Work of Thomas Laycock 1812-1876" (Ph.D. Thesis, London University, 1996).

[308]R. Owen, "On the Structure and Homology of the Cephalic Tentacles in the Pearly Nautilus," *Annals and Magazine of Natural History* 12, 305-311 (1843): pp. 306, 310-311.

GEORGE NEWPORT AND THE CREATION OF A NEW EXEMPLAR

Versions of recapitulation, like cephalisation, focused a researcher's speculations. When George Newport studied lobster anatomy in 1834, he bought a large, living specimen that seemed healthy, though it was light blue instead of the normal blackish-blue. When he pinched and pricked it in various places, he thought it suffered little pain. When he killed it and removed its shell to look at its nervous system, Newport saw that the ganglia of its sensitive columns were far smaller than usual. "May we not infer from this fact," he asked rhetorically, "that the degree of sensation in the nerves belonging to the spinal column very much depends upon the size of the ganglia and the quantity of grey matter they contain?"[309]

In dissecting other articulates – insects this time – Newport was again informed by cephalisation. He concluded that as an insect metamorphosed, its nervous system gradually concentrated, its discrete and equally-dispersed ganglia coalescing. Newport chose *Sphinx ligustri* (the privet hawk-moth) for his investigations because it metamorphosed over a long period, giving him time to examine its changes. The larva changed its skin six times before pupation, growing larger with each change, and as it grew its nervous system changed too. Lying above the oesophagus sat a "nodulated mass in the head which is supposed to represent the brain", and arrayed along two longitudinal cords were eleven spherical ganglia, originally formed of two lobes.[310]

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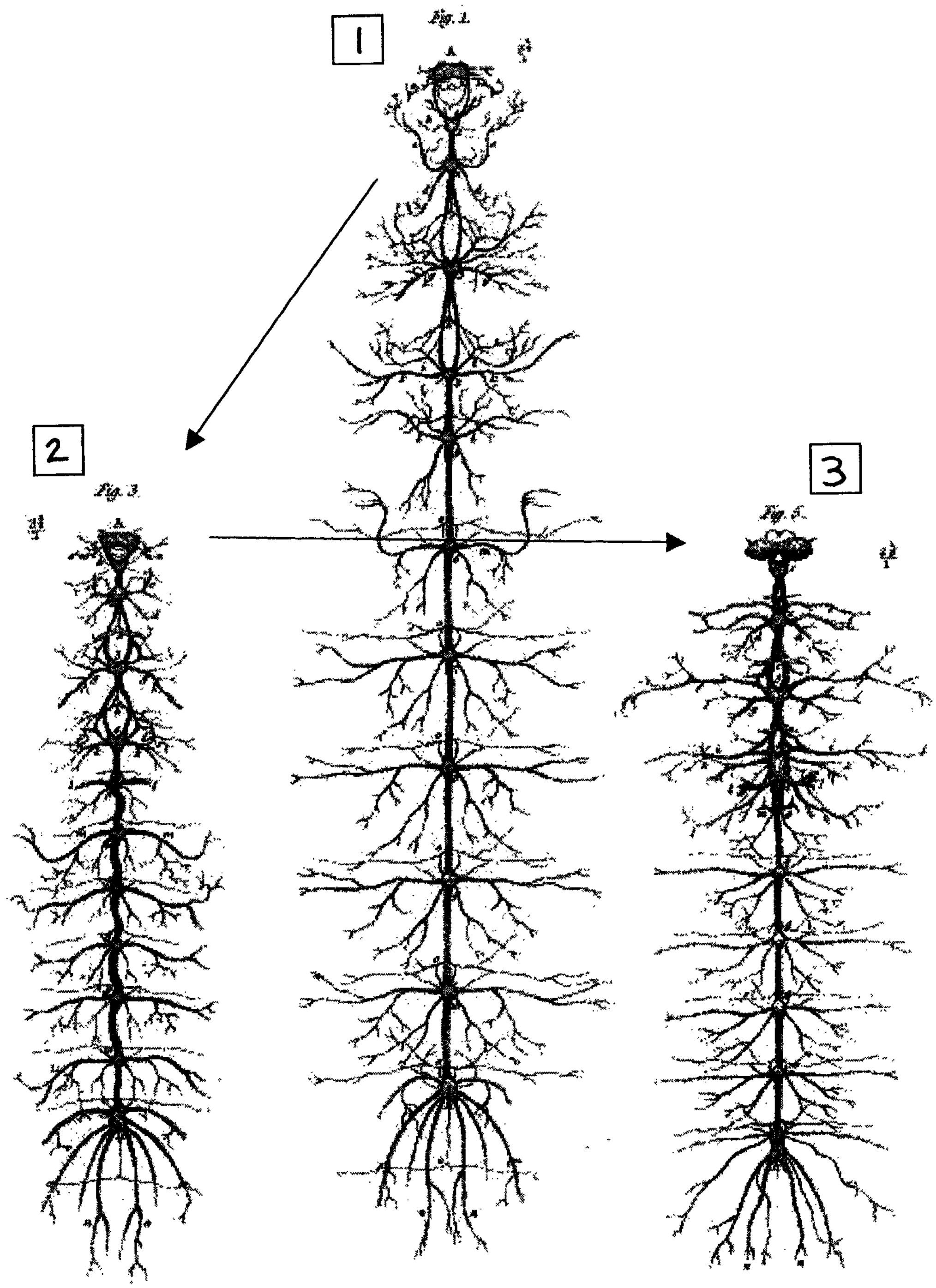
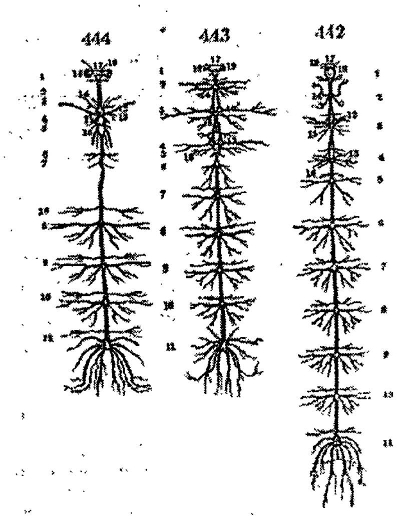
[309]G. Newport, "On the Nervous System of the *Sphinx ligustri* (Part 2) during the Latter Stages of Its Pupa and Its Imago State," *Phil. Trans.* 124, 389-423 (1834): p. 407fn.
[310]G. Newport, "On the Nervous System of the *Sphinx ligustri* and on the Changes which it undergoes during a part of the Metamorphoses of the Insect," *Phil. Trans.* 122, 383-398 (1832): pp. 383-384.

Between mid-August and early September the larva stopped eating and formed an oval chamber in the earth in which to change. Its cerebral lobes increased in size and its eleven ganglia moved towards each other, some segments lengthening and others shortening so they were no longer uniform. Four days after pupation, the fifth ganglion approached the fourth; thirty days after pupation the “cerebral lobes” above the oesophagus had grown far larger as the first ganglion had moved forward and fused with them. The fourth, fifth and sixth ganglia had fused. Though the cerebral ganglia were still distinct from one another, they too had enlarged forward; in later pupae the cerebral ganglia extended transversely too, forming (with the subesophageal ganglion) a continuous mass around the oesophagus.[311]

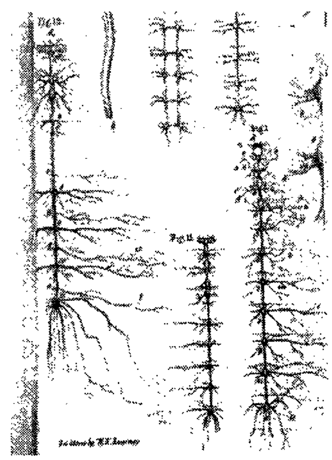
[311]Newport, "Nervous System of the *Sphinx ligustri*," pp. 389-393. Supporting the centripetal image of development, Newport observed that the development of the nerves to the wings took place from the periphery to the centre, thickening towards the middle line of the body. Newport, "Nervous System of the *Sphinx ligustri* (Part 2)," pp. 400, 411.



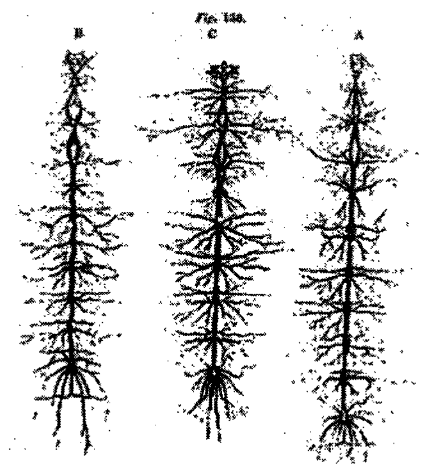
Coalescence of the privet hawk-moth nervous system. <u>1:</u> The nervous system of a fully-grown larval privet hawk-moth; <u>2:</u> The nervous system of the larva two hours before pupation; <u>3:</u> The nervous system of a privet hawk-moth 30 days after pupation. From G. Newport, "On the Nervous System of the *Sphinx ligustri* and on the Changes Which It Undergoes During a Part of the Metamorphoses of the Insect," *Philosophical Transactions of the Royal Society* 122 (1832): 383-98, plate 12.



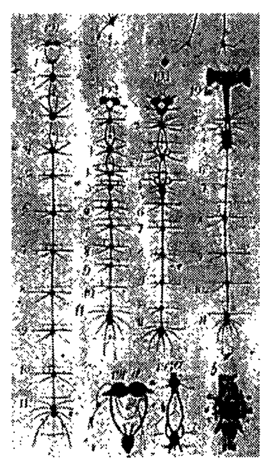
P.M. Roget, *Animal and Vegetable Physiology* (London: W. Pickering, 1834), p. 2:547.



S. Solly, *The Human Brain* (London: Longman Rees, 1836), plate 4.



T.R. Jones, *A General Outline of the Animal Kingdom,* figure 138 p. 304.



W. B. Carpenter, *Principles of Physiology*, plate 6, figures 191-194.

Depiction of Newport's observations of the coalescence of the nervous cord in the privet hawk-moth from larva to pupa (and imago in Carpenter's case). In all of these pictures except for Carpenter's, the pattern of coalescence is not depicted as moving from left to right.

Newport then repeated these dissections on the nettle butterfly (a more plentiful species than the privet hawk-moth and which conveniently had a nervous system with the same number of ganglia). Two hours after the larvae suspended themselves from the ceiling of the breeding-cage, getting ready to pupate, he dissected them. He noted that each one's suboesophageal ganglion had expanded to twice the larval size, and that other ganglia had moved toward each other. Dissecting other larvae that had continued metamorphosis seven hours past this point, he observed that other ganglia had expanded; and Newport carefully dissected other specimens who had attained periods of twelve, 24, 36, and 58 hours after pupation. In all cases the ganglia had moved closer together, "affording us a further proof of the adhesion of contiguous parts".[312]

Newport continued his developmental researches on *Iulus terrestris,* the white snake millipede whose vivisections were described in the previous chapter. Unlike the hawk-moth and nettle butterfly, this animal was a myriapod and didn't go through discontinuous metamorphoses. They only grew new segments: where insect larvae fused their uniform segments together during metamorphosis, myriapod larvae began with only a few uniform segments and added new ones between the penultimate and last segment in a "germinal space".[313]

When the millipede had developed 14 pairs of legs, Newport thought it analogous to the larval insect bursting from an ovum.[314] A myriapod's double legs were like a caterpillar's – thus the permanent state of *I. terrestris* was analogous to a larval insect on the one hand, and to annelids on the other, because annelids also developed by budding similar segments that did not fuse.[315] Newport later repeated his comparison between larva and annelid, noting how recapitulation explained myriapods' relationship to the rest

[312]Newport, "Nervous System of the *Sphinx ligustri* (Part 2)," pp. 412-416.
[313]G. Newport, "On the Organs of Reproduction, and the Development of the Myriapoda," *Phil. Trans.* 131, 99-130 (1841): pp. 99, 117, 122.
[314]They were analogous in that where the larval insect cast off its skin immediately upon leaving the shell, the myriapod didn't cast it off until some period after leaving the ovum.
[315]Newport, "On the Organs of Reproduction," p. 120.

of the articulate group: differently-developed individuals of different species could all be placed at the same developmental stage because of their repetition of simple segments.[316]

Newport distinguished between two types of development: "growth," the simple extension and enlargement of each individual element, which included the appearance of a new segment; and "anchylosis," the fusion of two or more elements to form particular body-regions. These explained not only the appearance and fusion of ganglia, but of any body part, like mandibles: any articulate movable body part was actually made of two different segments initially independent but then fused.[317]

Other British researchers took up Newport's work. Anderson noted that Newport clearly showed how metamorphosis contracted the nervous columns, shortening the space between the cerebral ganglion and first sub-oesophageal ring. Another of Newport's reviewers repeated that segmental independence was highest in the lowest articulate classes (though he disagreed with Newport's implicit linear scheme).[318] W.B. Carpenter thought Newport had confirmed Tiedemann's findings: that articulate heads developed through coalescence, with anterior ganglia "enfolding" posterior ones. The embryo's development confirmed the findings of comparative anatomy: the encephalon was added to the sensory ganglia and not the other way around, for if we descended the animal scale the cerebrum gradually disappeared, until in *Amphioxus* there was none. Carpenter added that lower animals were the best candidates for comparative anatomy because elementary units were easier to observe in these than in the human body: a fish encephalon was made up of at least four distinct masses in a line continuous with its spinal cord. Newport had

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[316]G. Newport, "Insecta," in *Cyclopaedia* (1839), pp. 942, 948; idem, "Monograph of the Class Myriapoda, Order Chilopoda," *Linnean Society Transactions* 19, 265-302, 349-440 (1845): pp. 268-269, 266.
[317]Newport, "Structure of Nervous System in Myriapoda," p. 244.
[318]"Mr. Newport's Researches in Natural History, &c.," *BFMR* 20, 487-508 (1845): p. 491; Anderson, "Sketch of the Comparative Anatomy of the Nervous System," pp. 910-911.

further discredited the notion of the *sensorium commune*: the presence of sensory ganglia in the lowest vertebrates, such as *Amphioxus,* meant there were different seats of different sensations (like the optic or auditory ganglia) instead of a single sensorium.[319]

Robert Grant's textbook on comparative anatomy had also presented research describing the fusion of ganglia-pairs in caterpillars, pupae and insects; though Grant cited Herold, an anonymous reviewer – perhaps Newport himself – was surprised that Grant's 1833 comparative anatomy lectures never mentioned Newport's researches.[320] Grant, meanwhile, claimed that Newport had plagiarized from him and from Continental researchers like Pierre Lyonet.[321] Another writer criticized Joseph Swan's work on the nervous system because it didn't mention Newport's recent researches, which the reviewer pointed out confirmed statements not only by Meckel and Herold, but also by Swammerdam, Lyonet, Steviranus, Müller, Dufour, Straus-Durckheim, Weber, Audoin and Milne Edwards.[322] Regardless of whether Newport was a plagiarist or not, this web of charges, criticisms and cross-citations helps strengthen the appearance of a larger community of researchers guided by cephalisation, recapitulation and compound individuality.

And in turn, Newport's work was taken up by a new group of Britons: his developmental diagrams became exemplars themselves as they were copied by other researchers.[323] Depictions of the coalescence of moth-ganglia during metamorphosis (shown above) appeared in Peter Mark Roget's *Bridgewater Treatise*, Solly's *Human*

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[319]Carpenter, "Noble on the Brain," pp. 503-504.
[320]"Review of *Outlines of Comparative Anatomy,*" p. 384; Grant, "Lectures on Comparative Anatomy," pp. 2:514-515.
[321]Newport, "Newport's Second Reply to Grant," p. 119.
[322]Review of *Illustrations of the Comparative Anatomy of the Nervous System* by Joseph Swan, *M-CR* 24, 435-439 (1836): p. 437.
[323]Solly, *The Human Brain,* pp. 54 and Plate I, fig. 12.

Brain, Rymer Jones's *General Outline* and W.B. Carpenter's *Principles of Physiology.* Tellingly, Carpenter's diagram was more abstract than Newport's, moving from left to right, showing how Newport's work could be turned into a more general exemplar applicable to the rest of the insect world.

MONSTERS AS COMPOUND ORGANISMS

Where Newport had proposed two developmental processes - growth and anchylosis - and shown these processes in different articulates, the problem of monsters was quite similar. Researchers applied growth and coalescence to teratology too: where ordinary organisms like the privet hawk-moth developed by first budding simple parts and then fusing these elements into compound parts, teratology saw monsters as very frequently formed through the fusion of entire individuals, literally forming a compound organism.

Anatomy museums were arranged according to the belief that monsters were compound. For example, the Hunterian Museum's collection of monsters was arranged in four groups. The first group featured the "preternatural" situation of parts in its specimens – for instance, foetuses found in men's bellies. The second group featured the addition of parts in specimens – thus double parts, like a woman's double uterus and vagina (with one uterus containing a seven-month old foetus), or a six-year-old's double skull, with witnesses relating that the superfluous head had undertaken "mental operations, distinct from those of the lower head." The third group had specimens with a deficiency of parts – thus one-eyed pigs or animals in which the entire face in front of the ears was missing. Finally, there was the group of hermaphrodites, like "free-martins," which Hunter had discussed in the *Philosophical Transactions*.[324] A review of Geoffroy's and Serres's philosophical anatomy, appearing in 1837, explained that a free-martin was a cow appearing to be a female, but incapable of generation – thus it was an "imperfect animal," similar to cattle whose testes or ovaries had been removed. The reviewer thought that Geoffroy's and Serres's work might help to explain Hunter's monstrosity by directing our attention to hypertrophy or atrophy of the sex organs as a cause of hermaphrodism.[325]

In that same year, Owen set John Hunter's teratological beliefs against Geoffroy's, pointing out that where Geoffroy attributed the production of monsters to external causes that could be induced experimentally,[326] Hunter instead argued that monsters had a "disposition" to deviate from Nature in a manner specific only to its species. Owen favoured Hunter's view of the endogenous, specific causes of monsters, noting that Hunter's opinion was the most "generally adopted."[327]

The difference between Owen's endogenous explanation of monsters and Geoffroy's exogenous explanation has been interpreted in the standard opposition of form versus function, with associated political beliefs: thus with form one gets *a priori* and rationalist principles with which one can criticize society, and problems can be

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[324]Ottley, "Life of John Hunter," pp. 181-183.
[325]"Review of *Philosophie Anatomique*," pp. 117-118.
[326]Richards, "Embryonic Repetition", p. 98. Geoffroy was interested in human monstrosities - linking them to accidents during a normal pregnancy. In 1826 he experimented on hen's eggs - interrupted their incubation at various times by shaking, perforating them, and heating/cooling them.
[327]It is unclear if Owen meant Etienne Geoffroy St-Hilaire or Isidore Geoffroy; in the preface to the Hunterian Catalogue he refers to Isidore's 1832 *Histoire des Anomalies de l'Organization chez l'Homme et les Animaux, un Traité de Teratologie*), but in the 1837 Hunterian lectures in Comparative Anatomy Owen simply refers to "Geoffroy St. Hilaire." Owen, "Preface," p. 4:xxv; R. Owen, *The Hunterian Lectures in Comparative Anatomy, May and June 1837*, ed. P. R. Sloan (Chicago: University of Chicago Press, 1992), p. 185.

caused or ameliorated through environmental changes (the Radical stance); with function one gets *post hoc* 'functionalist' rationalizations for society, and problems cannot be changed as easily or at all (the conservative stance). Evelleen Richards, for instance, has seized upon Owen's preference for Hunter's endogenous explanation, thoroughly showing how Owen fused it with his own conservative ideological interests. Because in Hunter's view all monsters were caused by a force specific to each species (and perhaps by Divine intervention), all organic change was conservative and lawful. Monsters were formed as monsters from the very beginning, and an external force or person couldn't cause the formation of a monster. By posing the endogenous explanation of monsters against Geoffroy's materialist and environmentalist explanations, Owen in turn reinforced his own commitment to natural law, natural theology, and functionalist explanations.[328]

But regardless of how monsters were caused, it is also important to look at the type of monsters that interested Hunter, Geoffroy and Owen - people and animals with additional parts fused together or certain parts missing. (Also recall that explanations could cross from one camp to another – that at least one British reviewer used Geoffroyan philosophical anatomy to explain Hunterian findings). In the case of the production of double monsters, Geoffroy explained their union as caused by the attraction of homologous parts, what he called the law of "soi pour soi."[329] Owen was also interested in these "double monsters", an interest shown upon Carus's visit to Owen's skull collection:

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[328]Richards, "A Question of Property Rights," pp. 148-149, 157; E. Richards, "A Political Anatomy of Monsters, Hopeful and Otherwise: Teratogeny, Transcendentalism, and Evolutionary Theorizing," *Isis* 85, 377-411 (1994): pp. 394-395.
[329]Richards, "A Political Anatomy of Monsters," pp. 398-399.

> The most remarkable was a monstrous formation from India, in which another skull was joined to the head of a child in such a manner, that the two crowns were united...The bony parts of the two united skulls were in Owen's hands, and we considered attentively this extraordinary malformation.[330]

Richards even describes how Owen became delighted upon encountering a child with a double head; she pays particular attention to how the skulls were not joined by homologous parts but were instead turned in opposite directions, frustrating Geoffroy's law of soi pour soi. Nonetheless Owen and Carus agreed with Hunter's law that "supernumerary" (extra) parts are joined to similar parts.[331] In 1837 Owen stated that one of Hunter's "most remarkable laws of aberrant formations" was "that monsters are formed monsters from their very first formation, for this reason, that all supernumerary parts are joined to their similar parts, as a head to a head, &c, &c." In the Hunterian lectures of that same year Owen again noted that the law of teratology meant that extra parts were joined to similar parts.[332]

Owen's interest in monsters as compound organisms was shared by other researchers: Johannes Müller was also interested in double monsters, explaining them either as the "concretion" of two germs, or as the production of two embryos which then grew together.[333] L.S. Jacyna notes that the curator of the Museum of Pathology at Guy's Hospital, Thomas Hodgkin, was interested in a conjoined twin and suggested that "these plural births, seem...to be analogous to those animals which possess a sort of community of life...in animals of a still lower grade, such as the zoophytes, which produce coral,

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[330]C.G. Carus, *The King of Saxony's Journey through England and Scotland in the Year 1844* (London: Chapman & Hall, 1846), pp. 93-94, cited in Richards, "A Political Anatomy of Monsters," p. 396.
[331]Richards, "A Political Anatomy of Monsters," pp. 398-399.
[332]Owen, "Preface," pp. 4:xxv-xxvi; idem, *Hunterian Lectures in Comparative Anatomy, 1837*, p. 185.
[333]Müller, *Elements of Physiology*, p. 1:403.

asidia [sic], and sertularia."[334] Though Jacyna is pointing out Hodgkin's belief in parallelism or recapitulation, this quote can be further interpreted as implying that lower grades of development or monsters meant compound individuality, the existence of a "sort of community of life" in a single organism. And Hodgkin used the same set of exemplar organisms – coral polyps, sea squirts, and sertularian polyps - to convey a sense of compound individuality.

All of these examples show that even biological anomalies were characterized as abnormal because of their over-compoundedness or their lack of compoundedness. This compound individuality was the deeper system upon which researchers argued over different causes.

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[334]Thomas Hodgkin, "The History of an Unusually-Formed Placenta, and Imperfect Foetus, and of Similar Examples of Monstrous Productions," *Guy's Hospital Reports* 1, 218-226 (1836), pp. 224-225, cited in L. S. Jacyna, "The Romantic Programme and the Reception of Cell Theory in Britain," *Journal of the History of Biology* 17, 13-48 (1984): p. 37.

CONCLUSION TO CHAPTERS 1 AND 2

The habit of analysis and synthesis in biomedical research had a self-reinforcing, even a self-vindicating, appeal. It formed a holistic and harmonious system: a finding arrived at through the habit of analysis and synthesis could be deployed to validate other findings in the habit. My point can be illustrated by posing a few rhetorical questions.

Why did certain organisms survive bilateral sectioning while other organisms died quickly? Because the surviving organisms were aggregations of simpler quasi-independent elements. If a vivisector removed certain organs in which certain physiological systems were seated, the higher organism died immediately – thus showing these systems to be 'foundational.' Organisms died in the reverse order of their development, losing the "highest" functions first and finally these foundational ones.

Why did an embryo's development resemble its species' place in the *scala naturae?* Because it developed centripetally, through the coalescence of its quasi-independent parts into a concentrated, "perfect," result. Vivisection showed recapitulationism to be valid by revealing a hierarchy of different physiological systems within a single higher organism. Meanwhile, monsters were organisms with too many elements, could even be double; or had too few elements, missing certain important parts.

Why were some organisms "higher" than other organisms on the *scala naturae?* because they had a higher physiological division of labour, meaning their parts and physiological systems were more coalesced; but their higher division of labour also meant their increased vulnerability to a quick death by injuries that wouldn't kill other organisms. Because higher organisms had a more concentrated nervous system which better communicated between their different parts and systems, this concentrated nervous system in turn explained their increased vulnerability and increased self-hood. The development of the nervous system was even used as a taxonomic index: because it was a communication system it was qualitatively different from any other physiological system.

Researchers' repeated deployment of the same exemplar organisms – starfish, *Hydra, Sphinx ligustri* – also helped ensure the holism of the habit of analysis and synthesis, for they were illustrations of how well the habit explained different phenomena. The habit of analysis and synthesis worked, in other words. And it was strengthened further by its ability to convey complex facts – recapitulation-schemes could be followed both in nature and in the textbook. Further reinforcing this habit were the techniques used to display these specimens: dissected, or presented in alcohol inside glass containers. In other words most specimens were dead, unchanging (or in the case of vivisection, dying). Live specimens were often inaccessible except by elite or very determined researchers, so students in turn had to depend upon dead specimens for dissection or textbook accounts of these organisms, which acted as virtual exemplars.

The holism of analysis and synthesis entailed other things. First, (zoological) taxonomies where the nervous system was an index tended to be anthropocentric as humans would be situated on the highest point of the scale. They would also tend to be linear – a *scala naturae* - if they emphasized the proportion of nervous matter to the rest of an animal's body parts. Second, analysis led to the disunity of the organism as one common research context because this habit meant an emphasis upon the abilities and characteristics of discrete body elements (be they body parts, phrenological faculties, physiological systems or ganglia). Synthesis led to compound individuality as an accompanying common research context because this habit emphasized development as the gradual association of these discrete body elements.

Third, there is a tantalizing link between the habit of analysis and synthesis and certain ideals of social order. It is noteworthy that the heyday of analysis and synthesis in Britain occurred during the rise of political Radicalism. This was exemplified in events before the 1832 Great Reform Bill to the repeal of the Corn Laws in 1846. While the usual caveats about crudely proposed symmetries must apply, it is nonetheless rewarding to explore this in slightly greater detail.

The habit of analysis and synthesis was extremely popular during the emergence of what Harold Perkin has called the "entrepreneurial" ideal in Britain. According to this ideal the movement of capital was the economic engine of society and obstacles to its movement (like government regulations) ought to be overthrown. The ideal representative of entrepreneurialism was the 'self-made man.'[335] Contemporary economists saw the economy as composed of interacting individuals.

We can find suggestions of other links in James Cross's fascinating 1981 essay on John Hunter's physiology. There, Cross links the "natural harmony of interests" in both bodily unit and economically self-motivated individual, and notes how Hunter accordingly diminished the role of the will in regulating animal functions – it simply wasn't necessary.[336] In Hunter's physiology, the organism – like the economy - was constituted merely by the interaction of its units.

Cross's thesis can be pushed ahead about forty years and used to understand how analysis and synthesis interacted with the entrepreneurial ideal. Analysis and synthesis

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[335]H. J. Perkin, *The Rise of Professional Society: England since 1880* (London: Routledge, 1990), pp. xiii-xiv.
[336]Cross, "John Hunter and the Animal Oeconomy," pp. 74-75.

entailed a closed system. Analysis studied the discrete units of a system, disunifying that system, and synthesis saw the system as nothing more than the sum – a compound – of those units. Thus both in economics and in biology, systems resulted from their units' interaction. This meant that any system would be a closed one. But in turn this offered a possibility of *spontaneous order* arrived at simply by the interaction of the units in that system. People voluntarily interacting (competing or cooperating) in the market, combined with the abolition of government regulations that restrained this interaction, was supposed to liberate 'market forces' or the 'invisible hand', other words for this spontaneous order.

These notions likely appeared in biological and social research because of a shared vocabulary and imagery of interacting and interested units. Two different scholarly currents might be used to understand this further. One would build upon previous works that discuss struggles over language: an example of this is the historiography of the "linguistic turn." By using this method one might show that analysis and synthesis provided a deeper idiom upon which struggles occurred; in this sense the idiom of analysis and synthesis both facilitated certain ways of speaking and made difficult other ways of speaking.[337] At a deeper level we might use research into how metaphors themselves structure concepts. If we shed the notion of metaphors as merely figurative, this research can provide fascinating insights into why Victorian social and biological researches were linked at the level of metaphor. They would hint at why researchers often understood and described one less-coherent domain in terms of a more-

[337]Cooter, *The Cultural Meaning of Popular Science*, pp. 167-168, 190; J. Epstein, *Radical Expression : Political Language, Ritual, and Symbol in England, 1790-1850* (Oxford: Oxford University Press, 1994), p. 28; J. Golinski, *Making Natural Knowledge : Constructivism and the History of Science* (Cambridge: Cambridge University Press, 1998), pp. 128-129; G. Stedman Jones, *Languages of Class : Studies in English Working Class History* (Cambridge: Cambridge University Press, 1983), p. 96.

coherent, "structured" domain.[338] In this way we might see the habit of analysis and synthesis as a 'structural' metaphor that was used to define other domains like biology and social theory. By using these two methods one could further understand how analysis and synthesis helped constitute the entrepreneurial ideal.

[338]Lakoff and Johnson, *Metaphors We Live By*, pp. 61-68.

CHAPTER 3
ELEMENTARY AGENTS: CELL THEORY AND MICROSCOPY IN LONDON AND EDINBURGH, 1830-1852

> A number of persons – anatomists, botanists, geologists, chemists – are...combined [in the Microscopic Society], as if to form one large and complex Leeuwenhoeck [sic], with nothing more in common in their disorderly pursuits than that they might all study little things with the same apparatus. With as much propriety might a society be formed of those who will examine none but large things...the microscopic society exactly fits the foolish spirit of these times, in which men seem to think that a society and transactions are essential to the progress of every artificial division of science; and to compass the formation of a society, will take anything for a bond of union.
>
> -"History and Mode of Study of Minute Anatomy." *British and Foreign Medical Review* 14 (1842): 478-92, on 489.

This chapter is divided into three parts. The first part examines early Continental researches upon the 'wee beasties' known variously as infusoria and protozoa, and how these researches were interpreted in Britain as supporting or denying compound individuality. In other words, some asserted that infusoria were "monads," the simplest 'organic elements', while others denied this. This problem became more important in the light of Matthias Schleiden's and Theodor Schwann's "cell theory," with its proposal of autonomous cells that were somehow harmoniously incorporated into a larger body. Researches into cells and infusorians can be seen as part of the common context of compound individuality and the habit of analysis and synthesis.

The second part shows how a different view about development was imported into Britain, not from France but from Germany. In the late 1830s Martin Barry introduced a different view of development, mainly from the embryologist Karl Ernst von Baer. Against the predominant belief that development was centripetal and synthetic, Barry introduced a perspective in which development occurred through the differentiation of parts from a single mass. This new view - that development proceeded *centrifugally* –

was one aspect of a different habit of reasoning. This part foreshadows chapter four's depiction of a struggle between the supporters of two habits.

To flesh out the themes set out in the first two parts of this chapter, the third part investigates the specific case of John Goodsir's investigations upon cellular secretion (noteworthy for their influence upon Rudolf Virchow's more famous researches). Infusorians appear to have provided analogies that informed Goodsir's work upon cells. Of particular interest is how units at one level of organization could be made to stand for units at other levels of organization – thus a patient with geometrically replicating spores in his stomach which caused him to periodically vomit fluid could be analogized to cells filling up with secretions and then bursting. In short, compound individuality provided Goodsir with an interpretative resource.

1. MONADS OR COMPLEX ORGANISMS?

BEFORE 1838 - INFUSORIA

For microscopical investigations to be deemed meaningful, it was assumed that glimpses into the world of the minute led to interesting findings. In medical research before the cell theory, Pickstone notes how the "coagulation" view of the world justified microscopical investigations on medical subjects. From the mid-eighteenth century with John Hunter, to about a century later with Henri Milne Edwards, it was presumed that tissues were formed out of the coagulation of fluid "globules." Coagulation and microscopy thus supported one another, the theory providing an incentive to hunt for tiny globules in the human body.[339]

But the "infusoria", like animalcules, provided another important justification for microscopy before the cell theory. But for an intrepid microscopist, Infusoria held out the possibility of a vast world to be explored.

> When we consider the infinity of animals with which the material world is filled, their multitude is truly amazing. Every green leaf swarms with inhabitants. The bodies of other animals are in a manner filled with intestine life. The seas, lakes, marshes and rivers of our planet teem with numberless living creatures...we might almost conclude that the strength of creative energy has been more lavishly expended upon the animate than upon the inanimate world...[340]

In his article on nineteenth-century research into infusoria, Frederick Churchill has quipped that if Huxley referred to the Radiate class as the "lumber-room of zoology," then the Infusoria might be designated as the "refuse bin at the far corner of the same mental construction."[341] But as we shall see, the investigation of infusoria provided researchers with immense resources for understanding how cells - even human ones - worked. And in turn these examinations both justified microscopy and reinforced the view of compound individuality, that bodies were made out of smaller, simpler and quasi-independent units.

In 1830s Britain a prominent climate of opinion on infusorians followed the work of French researchers such as J.B. Lamarck and Georges Cuvier: though some

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[339]W. Coleman, *Biology in the Nineteenth Century : Problems of Form, Function, and Transformation* (Cambridge: Cambridge University Press, 1977), p. 144; J. V. Pickstone, "Globules and Coagula: Concepts of Tissue Formation in the Early 19th Century," *Journal of the History of Medicine* 28, 336-356 (1973): pp. 336-339.
[340]M. Gairdner, "Analysis of Professor Ehrenberg's Researches on the Infusoria," *Edinburgh New Philosophical Journal*, 11:201-225; 12:78-102 (1831): pp. 12:98-99.
[341]F. B. Churchill, "The Guts of the Matter - Infusoria from Ehrenberg to Butschli - 1838-1876," *Journal of the History of Biology* 22, 189-213 (1989): p. 189.

animalcules were complex (the Rotiferae[342]), most were simple. These simple infusorians were structureless, like bits of animated jelly. Their very simplicity led some researchers to propose that they were like, or even were, the units out of which more complex organisms were formed. As is well known, Lorenz Oken was one such believer. W.S. Kent's 1880 *Manual of the Infusoria* noted that Oken's 1805 announcement - that infusorians were simple vesicles out of which higher organisms were fashioned - was the first instance of this sentiment. Kent solemnly announced "'The divine fiat, 'Dust thou art, and unto dust thou shalt return,' thus received unconsciously at the hands of Oken a practical and truly remarkable illustration."[343]

But the Comte de Buffon had announced that infusorians were organic elements before Oken, and at least two British researchers in the 1830s and 1840s cited Buffon's view about infusorians as organic elements. There may have been predecessors to Buffon, too. The work of Leeuwenhoek, Hartsoeker and Needham were also mentioned in an 1831 summary of the view that every animal was an

> aggregation of these creatures, and that the body of man himself was, as it were, only an accumulation of such monads; - as if the aggregation of myriads of these could explain the principle of life itself, - the active moving agent in each individual monad.[344]

In an 1834 discussion about infusorians as "monads" - reminiscent of Leibniz's elementary units - they took the role of elementary beings. They were the "smallest of

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342"Wheel-animalcules".
343W. S. Kent, *A Manual of the Infusoria* (London: D. Bogue, 1880), p. 2.
344Gairdner, "Ehrenberg's Researches," pp. 11:201-203.

visible animalcules", forming "the ultimate term of animality" by retaining the power of spontaneous but irregular motion.[345]

The belief in monads as the constituent elements of organisms was partly motivated by an analytical habit of reasoning, which drove researchers to look for the simplest possible organic elements. In turn it was assumed that these elements could be synthesized or compounded into higher organisms. The search for these elements formed part of a larger context of the disunity of the organism. In turn the analytic habit helped motivate the investigation of smaller organisms, driving both the increasing elaboration of new microscopes and associated equipment, and the growth of societies for microscopical research. In turn these instruments and institutions motivated work upon cells.

Support for my hypothesis can be seen in an interesting episode in which a person's research was rejected because it undermined assumptions about the existence of these elementary organisms. Rather than always citing positive statements about what behaviour was thought to be desirable, we can take a 'back-door' approach instead, studying rejected research – the work or assumptions thought impossible or too fantastic to believe.[346] My example is the work of the Prussian researcher C.G. Ehrenberg, who tried to demonstrate that all infusorians were just as complex as higher animals, and who was widely believed for a short time.[347]

Ehrenberg had extensively studied infusoria in the field, at one point being the only survivor of a disastrous expedition to the Nile Basin and the Red Sea. But despite

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[345]P. M. Roget, *Animal and Vegetable Physiology Considered with Reference to Natural Theology*, 2 vols., *Bridgewater Treatises on the Power, Wisdom and Goodness of God as Manifested in the Creation, Number 5* (London: W. Pickering, 1834), p. 1:184.
[346]Douglas, *How Institutions Think*, p. 76.
[347]Kent, *A Manual of the Infusoria*, p. 16.

the awful conditions of the trip he was able to send home specimens of nearly 4000 species, and by 1838 Ehrenberg had produced the well-illustrated classic *Die Infusionsthierchen als Vollkommene Organismen.*[348] Some of his views were uncontroversial – for instance he reinforced the view of life as a plenum, that infusorians made up the main group of living bodies on earth. He estimated that a cubic inch of infusorians contained more than 41,000 animals.[349] He copied previous microscopical techniques: previously, Wilhelm Friedrich von Gleichen had suspended carmine and indigo in water and watched infusorians devour these pigments. Ehrenberg used this tactic, colouring food with indigo, or rubbing carmine or green sap on a glass plate, to show off the infusorian's ciliary motion and trace the "lines" running from an infusorian's mouth to its "the various compartments of [its] alimentary canal."[350]

But some of Ehrenberg's observations were more controversial. He noticed how the coloured substances collected in certain small, spherical spaces inside infusorians, and concluded that these spaces were fixed. He then inferred they were "alimentary cavities", or stomachs. Building on his belief that they were stomachs, Ehrenberg divided Infusoria into two groups: the Polygastrica (those with several internal "alimentary cavities") and the Rotatoria (which had only a single internal cavity). But Ehrenberg did not merely see infusorian stomachs. Citing Spallanzani's observations of an infusorian's rhythmically contracting transparent space, he proclaimed it to be a seminal vesicle and that the "gland-like nucleus or endoplast" was the testis. Minute granular particles distributed throughout the infusoria were called "eggs". Coloured

[348]Churchill, "The Guts of the Matter," pp. 190-192; W. G. Siesser, "Christian Gottfried Ehrenberg: Founder of Micropaleontology," *Centaurus* 25, 166-188 (1981).
[349]A. Pritchard, *A History of Infusoria, Living and Fossil* (London: Whittaker and Co, 1841), pp. 4-6.
[350]Gairdner, "Ehrenberg's Researches," p. 11:210; J. T. Quekett, *A Practical Treatise on the Use of the Microscope* (London: Hippolyte Baillière, 1848), p. 363.

specks in infusorians such as *Euglena* were pronounced to be eyes - and from these eyes Ehrenberg inferred their possession of a nervous system.[351] The title of his book, after all, reads, *Infusorians as Perfectly Complete Organisms.*

It is hard - for even the most resolutely contextualist historian - not to smile at some of Ehrenberg's descriptions. For they were motivated by an assumption completely alien to us: that infusorians had complex physiological systems just like higher animals. In turn he complained about the "false" view motivating most researchers – their assumption of a *scala naturae,* that the animal kingdom was a series with simple creatures at the very end (or bottom). He protested that even Cuvier maintained a system where organization gradually simplified from humans to those creatures in which the organic system disappeared.[352] Churchill and Ilse Jahn have both remarked upon the significance of Ehrenberg's views on infusorians for his beliefs on transmutation: that he was part of a larger reaction against the *Naturphilosophen*, who argued that the first-generated organisms must be simple. And they note how Ehrenberg's belief in a single, physiologically complete, organic type reaching even the "monad" was part of an argument against the spontaneous generation of organic life from inorganic substances.[353]

But Ehrenberg's work on infusoria can also be seen in the context of views about the disunity of the organism, as a rejection of this disunity. For his observations of physiologically complex infusorians were seen to overturn a view taken by others besides the *Naturphilosophen*: the Leeuwenhoeckian/Buffonian view of higher organisms as syntheses of elementary organisms, or monads. By removing the Infusoria from their

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[351]Gairdner, "Ehrenberg's Researches," p. 11:80; Kent, *A Manual of the Infusoria*, pp. 17-18; Siesser, "C.G. Ehrenberg," pp. 172-173.
[352]Winsor, *Starfish, Jellyfish, and the Order of Life*, pp. 29, 32-33.
[353]Churchill, "The Guts of the Matter," p. 192; I. Jahn, "Ehrenberg, Christian Gottfried," in *Dictionary of Scientific Biography*, ed. C. C. Gillispie (New York: Scribner, 1981), 288-292, pp. 288-291.

traditional place at the bottom of the scale, Ehrenberg was deemed to have removed this view from consideration. In 1834 one writer thought he had refuted Buffon's "brilliantly imaginative" view that the human body was nothing more than vast congregations of monads capable of spontaneous motion[354] – for as Ehrenberg had presented "animalcules" as individuals themselves, there could be no scale of being.

Ehrenberg's views were initially quite respectable in Britain –the very first book purchase of the Royal Microscopical Society, in about 1841, was Ehrenberg's *Die Infusionsthierchen,* acquired for the substantial price of £13, 10 shillings[355] (or the rent of Thomas Carlyle's 8-room London house for about three months). Also extending Ehrenberg's British reach was the optician/microscopist Andrew Pritchard's *History of Infusoria,* which first appeared in 1834 and was enjoyed enough to have reached four editions by 1861. This work was regarded mainly as an "excellent transcript" of Ehrenberg's infusorial descriptions, useful to the general British microscopist.[356]

But Ehrenberg's bold theoretical move and striking observations were viewed with suspicion. (In addition to his opposition to the *scala naturae*, his work was hampered by the conditions in which he made his observations – his earliest work was done in the field, not in the more controllable environment of a laboratory or museum.)[357] Tellingly, the first criticisms were levelled against Ehrenberg's "polygastrica" because they were the easiest 'complex' infusoria to view closely. Since carmine and dyes could

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[354]Roget, *Animal and Vegetable Physiology*, pp. 1:184-186.
[355]G. L. E. Turner, "The Origins of the Royal Microscopical Society," *Journal of Microscopy* 155, 235-248 (1989): p. 242.
[356]Kent, *A Manual of the Infusoria*, pp. 26-27; B. B. Woodward, "Andrew Pritchard (1804-1882)," in *DNB* (1896).
[357]For the importance of fieldwork and local conditions on Ehrenberg's zoophyte researches (dealing with the difficulties of examining coral polyps when they are preserved as specimens), see Winsor, *Starfish, Jellyfish, and the Order of Life*, p. 29.

be easily used by other researchers to see if infusorians indeed had a "gastric system", the polygastric theory became - as Churchill succinctly states - the "totem of his system".[358]

Where Ehrenberg saw stomachs - as the dyed food passed through successive infusorian vacuoles until it was expelled - others thought that a simpler explanation was that the infusorian had simply turned upon its axis. But what drove them to require a 'simpler' alternative? Because Ehrenberg junked the scale of being, his critics speculated differently, rejecting Ehrenberg's naming the mysterious infusorian vacuoles "stomachs." It seems that attacks upon Ehrenberg were frequently motivated by the very unthinkability of his assertion that the smallest possible organisms were complex.

The rejection of Ehrenberg's presupposition made alternative explanations for his observations more credible and contributed to his swift downfall. Between 1835 and 1841 attacks on Ehrenberg came from different quarters, all of which returned to the view that the infusoria were the simplest possible organisms. On the continent, the chemist Félix Dujardin denied the existence of specialized infusorian stomachs. Instead he sought a materialist, chemical, explanation, interpreting the material inside the infusorian to be a kind of "living jelly". He called this jelly "sarcode" (which later became known as *protoplasm*).[359]

In Britain, Dujardin's criticisms of Ehrenberg appeared in later editions of Pritchard's *History of Infusoria.*[360] Another critic of Ehrenberg's polygastric thesis was Thomas Rymer Jones. At the 1838 BAAS meeting in Newcastle, he was the only

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[358]Churchill, "The Guts of the Matter," p. 192.

[359]J. R. Baker, "The Cell Theory : a Restatement, History, and Critique," *Quarterly Journal of Microscopical Science* 90, 87-108 (1949): p. 89; Churchill, "The Guts of the Matter," p. 193; Pritchard, *A History of Infusoria*, pp. 52-53.

[360]Thus the second edition of Pritchard deals with these problems in detail, and Dujardin's "sarcode": Kent, *A Manual of the Infusoria*, pp. 26-27; Pritchard, *A History of Infusoria*, pp. 52-61.

opponent of Ehrenberg's polygastric grouping, and his criticisms were turned into one of his few specialized papers.[361] This presentation seems to have been one of the only forays into focused research for Rymer Jones as he then went on to write textbooks.

For by taking a stand against Ehrenberg and the physiological completeness of infusorians, Rymer Jones was trying to clear the way to fly his disunifying colours. In his *General Outline of the Animal Kingdom*, and in lectures at the Royal Institution, he pronounced Buffon to be essentially correct, as "each atom of the substance of [the humblest] plants…[was] endowed with independent life."[362] Rymer Jones repeatedly and explicitly linked infusorians and the constituent units of larger organisms: Buffon's only failing, he noted, was that he was too advanced for his time. In the case of one organism, *Gaillonella*, (composed of a series of little cells placed end to end like a string of beads), who could deny that "each individual cell is a living creature; seeing that it can both nourish itself and reproduce itself, exhibiting thus the two great functions which characterize organized beings?"[363] Chapter Five will examine Rymer Jones's influence on Herbert Spencer, another person who wrote on the disunity of the organism.

Back on the continent, Theodor von Siebold continued his attack on Ehrenberg. In 1845 he reclassified Infusoria, placing many into the new group Protozoa.[364] Working in the framework of Matthias Schleiden's and Theodor Schwann's 1838-1839 cell-theory, von Siebold stated that the unicellularity of the Infusoria/Protozoa was their most distinguishing characteristic. Linking infusorians and the cells of higher organisms, he

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[361]Baker, "Cell Theory v.90," p. 91; Bettany, "T.Rymer Jones"; T. R. Jones, "On the Digestive Apparatus of Infusoria," *Annals and Magazine of Natural History* 3, 105-107 (1839); Pritchard, *A History of Infusoria*, pp. 52-53.
[362]Jones, *Outline of the Animal Kingdom*, p. 52; Jones, *The Natural History of Animals*, pp. 1:302-303.
[363]Jones, *The Natural History of Animals*, pp. 1:298-300.
[364]Siesser, "C.G. Ehrenberg," pp. 172-173.

emphasized the importance of the nucleus in both groups: no longer was the infusorian/protozoan nucleus an Ehrenbergian “gland”, like an egg or testes. Each separate animalcule was instead “reducible to the type of a cell”, their “stomachs” had some sort of circulatory function, and von Siebold agreed with Dujardin’s opinion that they were composed of sarcode.[365]

The “unicellular hypothesis” of Protozoa and Protophyta allowed the lowest plants and animals to be formally identified as cells themselves. Minute organisms were returned to the bottom of a restored scale of being as the simplest animals in existence: protozoans became what Marsha Richmond has called exemplars of cells living independently, allowing the comparison of simple protozoans with the nucleated cells of more complex organisms. Medical researchers and naturalists therefore saw themselves as observing the same sort of object by the early- to mid-1850s.[366]

SCHLEIDEN AND SCHWANN’S CELL-THEORY

Accompanying Ehrenberg’s rejection was the reappearance of organisms as compounds of monad-elements; this came in 1838-1839 with Schleiden and Schwann’s cell theory. A British reviewer welcomed the theory in 1840, and Schleiden and Schwann’s account was fully translated into English in 1847.[367] The nucleus was

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[365]Churchill, "The Guts of the Matter," pp. 193-194; Huxley, "Owen and Rymer Jones on Comparative Anatomy," pp. 2-3; Kent, *A Manual of the Infusoria*, pp. 19-20.
[366]T. H. Huxley, "Lecture 3 on General Natural History," *Medical Times & Gazette* 33:869 (n.s. #308), 507-511 (1856): p. 507; M. L. Richmond, "T.H. Huxley's Criticism of German Cell Theory: an Epigenetic and Physiological Interpretation of Cell Structure," *Journal of the History of Biology* 33, 247-289 (2000): p. 257.
[367]"Schwann on the Structure of Plants and Animals, A Review of *Mikroskopische Untersuchungen über die Uebereinstimmung in der Struktur und dem Wachsthum der Thiere und Pflanzen*, Berlin, 1839," *BFMR* 9, 495-528 (1840); T. Schwann, *Microscopical Researches into the Accordance in the Structure and*

proclaimed to be the most important element of the cell. Schleiden said it was the primary cell-organ – that there were no cells without nuclei (a statement helped by the fact that the nucleus was the most stainable object of the cell). Schwann followed him by calling the nucleus a "cytoblast," or cell-bud, which appeared in the cytoplasm of an already-formed cell; cell-formation was therefore seen to be like crystallization. It was then thought that the nucleus secreted a membrane about itself, forming what Timothy Lenoir has called a sequence of "eggs within eggs" (and which he also notes mirrored von Baer's description of the developing ovum).[368]

Schleiden and Schwann's cell-theory pointed to cell-individuality from the outset, supporting the disunity of the organism. Schwann recalled how he was influenced by Schleiden's vegetable-cell work: when he dined with Schleiden, Schleiden told him an hypothesis:

> Il s'en suivrait en effet, à cause de l'identité de phénomènes si caractéristiques, que la cause qui produit les cellules de la corde dorsale ne peut être différente de celle qui donne naissance aux cellules végétales. Il y aurait dès lors dans un animal un organe, la corde dorsale, composé de parties élémentaires qui ont leur vie propre, qui ne dépendent pas d'une force commune de l'organisme. Ce serait donc le contraire de la théorie généralement admise pour les animaux, d'après laquelle une force commune construit l'animal à la manière d'un architecte.[369]

Growth of Animals and Plants, trans. H. Smith (London: Sydenham Society, 1847). Carpenter is given as the author of this review by both L.S. Jacyna and Martha Richmond, but this article is not given in the list of writings appearing in his biography. They are probably correct but I am being cautious. See Carpenter and Carpenter, *Nature and Man*, p. 468; Richmond, "Huxley's Criticism of German Cell Theory," p. 254. Jacyna, "Reception of Cell Theory in Britain," p. 26.

368 J. R. Baker, "The Cell Theory : a Restatement, History, and Critique," *Quarterly Journal of Microscopical Science* 89, 103-125 (1948): p. 104; idem, "Cell Theory v.90," p. 106; T. Lenoir, *The Strategy of Life : Teleology and Mechanics in Nineteenth Century German Biology* (Dordrecht: D. Reidel, 1982), pp. 124-131.

369 F. Duchesneau, "Vitalism and Anti-Vitalism in Schwann's Program for the Cell Theory," in *Vitalisms from Haller to the Cell Theory*, ed. G. Cimino and F. Duchesneau (Firenze: L.S. Olschki, 1997), 225-252, p. 228. The "corde dorsale" refers to the notochord, a line of cells which develops into the spine in vertebrate embryos. M.J. Schleiden, « Discours, » in *Manifestation en l'Honneur de M. le Professeur Th. Schwann*, 23 Juin 1878. Düsseldorf, 1879, cited in M. Florkin, *Naissance et Déviation de la Théorie Cellulaire dans l'Oeuvre de Theodore Schwann*, Paris, 1960, p. 62.

Contradictory to the view of a top-down force shaping the animal, the parts composing both vegetable cells and the elementary parts of a vertebrate embryo's notochord instead had a life of their own. Schwann took this insight and expanded this into the view that the vegetable and animal cell had a life of their own too.

There has been a large amount of scholarly effort trying to understand the "contradictions" in Schwann's discussions about cell-formation.[370] The putative independence given to cells is no exception. E.S. Russell noted the implication of the "materialistic" cell-theory: that as cells were the centres of vital activity, the whole individual became only a cell-aggregate and individuality a teleological convenience for the researcher. Likewise, for L.J. Rather, Schwann said two different things, successfully "suppressing the major contradictions in his own thought" –in the *Microscopical Investigations*, Schwann stated that the basis of nutrition and growth lay in the individual elementary cells, and yet in that very same text Schwann argued that the cells were guided and influenced by the whole organism.[371]

But this search for "implications" and "contradictions" is problematic, for there is the danger of overlooking Schleiden and Schwann's proposal of two guiding forces, one localized in the cell, the other harmonizing and integrating it into the entire organism. Schleiden seems to have changed his mind from when he dined with Schwann and admitted the presence of two forces after all. While Schleiden saw plants as aggregates

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[370]For example: why did Schwann not recognize the inherent problems with exogeneous generation – where a cell formed about a nucleus in a process akin to crystallization – when he previously opposed spontaneous generation? L. J. Rather, "Johannes Müller, Theodor Schwann, Matthias Schleiden, Jacob Henle and the Nature of Plant and Animal Cells," in *Johannes Müller and the Nineteenth-Century Origins of Tumor Cell Theory*, ed. L. J. Rather, J. B. Frerichs, and P. Rather (Canton, MA: Science History Publications, 1986), 1-55, pp. 23-24. Or: was Schwann a mechanist or vitalist? R. C. Maulitz, "Schwann's Way: Cells and Crystals," *Journal of the History of Medicine* 26, 422-37 (1971).
[371]Russell, *Form and Function*, pp. 181-183. Nor did he see any similarity between the generation of cells in the cytoblastema with the spontaneous generation of animalcules in infusions (which Schwann had already denied). Rather, "Müller and the Nature of Cells," pp. 23-24.

of cells, “fully individualized, independent, separate beings,” each cell nonetheless led “a double life: an independent one, pertaining to its own development alone; and another incidental, in so far as it has become an integral part of a plant.”[372] And for his part, Schwann also saw the need for some sort of integrating force too. Schwann noted that each cell was

> within limits, an Individual, an independent Whole. The vital phenomena of one are repeated, entirely or in part, in all the rest. These Individuals, however, are not ranged side by side as a mere Aggregate, but so operate together, in a manner unknown to us, as to produce an harmonious Whole.

To illustrate the independence of cells within a larger harmonious framework, Schwann used the example of the social insects: the relationship of a cell separated from an organized body resembled the separation of a bee from its swarm.[373]

In Britain it was accepted that two forces influenced cells. The reviewer for the *British and Foreign Medical Review* approvingly noted the proposal of the cell’s "double life".

> It has been justly remarked by Schleiden, that every cell leads a double life; an entirely independent one, belonging to its own development alone; and an incidental one, so far as it has become the constituent part of a plant.

By “incidental” the reviewer felt that the cell’s life as part of a plant was less important than its own, independent, life. While Schwann’s researches on animal cells showed that organisms consisted of “similar elements…each endowed with an apparently independent power of growth and self-nutrition”, the idea that “naturally suggests itself” as a consequence was that “the organism was an aggregate of parts endowed with independent vitality.” Yet in pursuing his analysis of the body’s similar elements, Schwann had not gone too far, said the reviewer. While his belief in the “independent vitality of the

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[372]Schwann, *Microscopical Researches*, pp. 231-232.
[373]Schwann, *Microscopical Researches*, pp. 2, 192-193.

component elements" of the organism was like Mayer's hypothesis that organized bodies were composed of monads, or "biospheres" with consciousness, Schwann did not go as far as Mayer, for he hadn't given the individual cells higher functions.[374]

The "double life" of the cell is similar to the double movement of analysis and synthesis – on the one hand the organism is localized into its simplest elements, cells; on the other hand they are synthesized into a larger whole. Because of the interplay of analysis and synthesis, the cell's double life meant that its autonomy was balanced against the harmony of the larger organization to which it belonged. Pathologies could often be explained as violations of the organism's harmony, as anarchic phenomena.

An example of an anarchic pathology disrupting the organism's harmony is shown by cancer cells, units that were too autonomous.[375] For Johannes Müller, cancer cells caused problems precisely because they were growing too freely – as structures undergoing "anarchic vegetative phenomena," their extreme individuality posed a danger for the rest of the organism. Müller explained that the cancer cell was not harmonized by the entire organism's "living principle" – that it had somehow loosened its ties with the rest of the organism. The autonomy of cells had to be balanced by a creative force that guided their development, and Müller complained that researchers such as Schwann had ignored the problem of how cells formed tissues, tissues formed organs, and organs formed organisms.[376]

In Britain, Richard Owen levelled a similar critique against views of the organism as mere cell-aggregates – whether Buffon's organic molecules or Oken's monadic

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[374]"Schwann on the Structure of Plants and Animals," p. 525. I have unable to determine which Mayer the reviewer was referring to.
[375]I am grateful to Fredrik Jonsson for bringing up this example.
[376]Duchesneau, "Vitalism and Anti-Vitalism," pp. 245-247; Russell, *Form and Function*, p. 192.

cell/infusorian vesicles. In 1840, Owen noted that while these images were imaginative, they did not answer why these elements developed into distinct tissues. Merely looking at the autonomy of the cell didn't explain why a fish, bird, or mammal had attained its shape and had not remained a "mass of infusories." Structural harmony – the "subordination of interdependent parts to the well-being of the whole" – plainly showed a "ruling principle" guiding the organizing process.[377]

This interplay - between the autonomy of subunits and the harmony of the structure of which they were a part - was as much a problem for Owen as it was for Müller. Worries about the chaotic relationship of cancer cells with the larger body are similar to views of certain monsters as truly compound, or double, organisms, in that they were both instances in which the body's harmony was overwhelmed by anarchy. In the next chapter we will see how Owen's morphology after 1840 was increasingly articulated as a principle of two balanced forces –the force of anarchic "vegetative repetition" set against the "adaptive" teleological force which ensured the individual's harmony.[378] We could probably associate Owen's conservative politics with his belief that some sort of 'ruling principle' was necessary to ensure structural harmony.

BRITISH MICROSCOPY AND INVESTIGATIONS

Owen's comments bring us to the development of British microscopy in the 1830s and 1840s, which was intimately related to British researchers' discussion of cells.

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[377]R. Owen, Manuscript, "Hunterian Lectures on Generation", 1840, OCOLL, NHM 38.1, p. 38.1.
[378]François Duchesneau points out that Schwann divided somatic cells into five classes, ranging from the 1) isolated, independent cells to the ones which were more specialized, losing their morphological individuality. Duchesneau, "Vitalism and Anti-Vitalism," pp. 232-233.

Microscopes were changing, allowing their users to discriminate more and more between minute objects. A brighter field of vision was made possible by new compound microscopes, because the largest possible cone of light could be collected from the light reflected by the specimen; yet at the same time this field of vision became less blurry as achromatic object glass greatly improved the microscope's resolution. Higher levels of magnification were increasingly possible, and microscopists were further helped by the construction of ever-more stable microscope mountings that helped keep the field of vision from wavering.[379]

But just as important as technical advances was the emergence of a large and prestigious group of British microscopists who ensured a large enough supply of money and criticism to ensure the continued elaboration, or 'refinement,' of microscopes and microscopy. J.A. Bennett has noted that by the second quarter of the nineteenth century the above-mentioned technical advances were made possible because of a large group of microscopists, constituting a market: there had to be discerning people wealthy enough to buy the improving microscope. With the foundation of the Royal Microscopical Society of London in 1839 – the only society devoted to a single instrument – the major British microscope makers (Andrew Ross, James Smith, and Hugh Powell) not only had a central market for their wares, but also a discriminating group which collectively evaluated their instruments. Because of these different manufacturers and this group of judges, a competition emerged in which the microscope manufacturers produced

[379]J. A. Bennett, "The Social History of the Microscope," *Journal of Microscopy* 155, 267-280 (1989): pp. 275-276; Coleman, *Biology in the Nineteenth Century*, p. 22.

increasingly elaborate instruments, creating larger apertures, higher resolutions and higher magnifications.[380]

The members of the Society increasingly evaluated one another too, discriminating between committed microscopists and mere dilettantes. Representative of this social evaluation was James Scott Bowerbank's[381] "goodness gracious box", containing slides of various microscopical wonders that he carried about on his person. Bowerbank would show the box and its slides upon greeting newcomers to the Society. The box was actually a test of the newcomer's commitment – a person only superficially interested in microscopy tended to stay with the box all evening (showing, perhaps, that they were less interested in the Society as a place for research than as a social venue).[382]

At the same time, the pool of new microscopists increased in the 1830s and 1840s as changes to medical training increased microscopy's prestige. The microscope became a symbol of a medical practitioner's scientific status, distinguishing him as someone who obtained accurate and demonstrative knowledge. In the free market of British medical schools, the use of a microscope set certain teachers apart from others – and in a virtuous circle this expanding pool of new microscopists in turn set themselves apart from the "old-fashioned" researchers who did not acknowledge the microscope's authority.[383] One sign of this change was a complaint by an old-fashioned reviewer about too much reliance on the microscope. His complaint was reminiscent of Ehrenberg's – in their rush

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[380]Bennett, "Social History of the Microscope," pp. 275-277.
[381]A founder of the Society. Born into a wealthy family, Bowerbank continued the family distilling business until his retirement in 1847. In addition to the Microscopical Society, Bowerbank helped found the Palaeontographical Society to publish descriptions of British fossils, the geological "London Clay Club," and over a twenty-year period he met various geologists at his residence once a week. R. Hunt, "James Scott Bowerbank (1797-1877)," in *DNB* (1885).
[382]Turner, "Origins of the Microscopical Society," pp. 235-236.
[383]L. S. Jacyna, "'A Host of Experienced Microscopists': the Establishment of Histology in Nineteenth-Century Edinburgh," *Bulletin of the History of Medicine* 75, 225-253 (2001): pp. 230, 251.

to microscopy, men confounded "relative smallness with absolute simplicity", for there was no reason to believe that the processes in a single cell were any simpler than what occurred in a larger organ.[384]

Adding to microscopy's legitimacy as a research tool was a change in the same period from secretive or personal 'recipes' of practices and techniques to their more open dissemination. Representative of this move towards codification was the use of Canada Balsam. In the mid-to-late 1830s it was discovered that Canada Balsam could be used to permanently and transparently "glue" specimens between two glass slips, a development allowing the repeated observation of the same specimen. Initially its practitioners kept this substance quiet – its inventors were actually sworn to secrecy about it. But when Bowerbank and a friend managed to guess its secret after seeing a specimen mounted this way (by the distinctive smell), they "spread the knowledge far and wide" amongst other microscopists, and Canada Balsam soon became the favourite way of mounting objects.[385] By 1848 the move from a secretive to a more open system was formalized with books such as John Quekett's *Practical Treatise on the Use of the Microscope,* a handbook collecting and disseminating all of sorts of these recipes. Quekett described techniques like how to use Canada Balsam, how to obtain the best infusorians, and how micrometers worked. In a sign of microscopy's newfound popularity, the handbook was highly successful: going to three editions, it was even translated into German.[386]

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[384]"History and Mode of Study of Minute Anatomy," *BFMR* 14, 478-492 (1842): pp. 490-491.
[385]J. S. Bowerbank, "Reminiscences of the Early Times of the Achromatic Microscope," *Monthly Microscopical Journal* 3, 281-285 (1870): pp. 282-284.
[386]Quekett, *Treatise on the Microscope;* Turner, "Origins of the Microscopical Society," pp. 237-238. Quekett was Owen's underling as assistant conservator at the Royal College of Surgeons's Hunterian Museum.

Thus amidst these developments emerged two groups of elite British microscopists. In Edinburgh there was John Hughes Bennett, Allen Thomson, William Sharpey (to 1836, when he moved to London), Martin Barry and John Goodsir. Elite London microscopists included Bowman, William B. Carpenter (after 1844), George Busk and James Paget, as well as Bowerbank, Richard Owen and Quekett.[387] Some of these people obtained paid positions as microscopists. Quekett was hired in 1843 by the Royal College of Surgeons as Owen's assistant conservator; in 1846 it purchased his collection of 2500 microscopic preparations (of normal and pathological tissues) for the Museum, and asked him to prepare an illustrated catalogue of these and the rest of the Museum's microsopic preparations. Quekett was eventually rewarded with a higher status position when - after Owen left the Hunterian Museum - he was promoted from Demonstrator of Minute Anatomy to Professor of Histology.[388]

And these elite microscopists evaluated each other's work. Researchers such as Marshall Hall and George Newport visited the Royal Microscopical Society of London to use the Society's elaborate microscopes and to have their investigations confirmed or refuted by its experts. These expert microscopists also issued consensual diktats: when Owen spoke of the "globules" of the human blood, Bowerbank objected to the term. He stated that the term "globules" were not "descriptive of concave circular discs. Professor

[387]H. Lonsdale, "Biographical Memoir," in *The Anatomical Memoirs of John Goodsir*, ed. J. Goodsir, H. Lonsdale, and W. Turner (Edinburgh: A. and C. Black, 1868), 1-206, pp. 88-89. Confusingly there were three Quekett brothers, two of whom were surgeons and microscopists – John and also Edwin, who practiced out of his home.
[388]G. S. Boulger, "John Thomas Quekett (1815-1861)," in *DNB* (1896). When Quekett was only sixteen he somehow made a microscope out of a roasting-jack, a parasol and a few pieces of brass; he then went to King's College London and the London Hospital medical school. In 1840 he won the first three-year studentship in human and comparative anatomy offered by the Royal College of Surgeons.

Owen concurred with my observation, and exclaimed, 'From this time forth then they are discs of the human blood.'"[389]

An interesting episode which shows how it was important to use the correct specimen to see microscopical phenomena is shown in the case of Martin Barry's notorious "double fibre." In the early 1840s Barry had proposed that each cell was itself composed of a simpler element – a "double fibre" out of which muscle and nervous cells were made. Some microscopists attacked Barry, as shown by L.S. Jacyna, [390] but others welcomed the discovery. William B. Carpenter initially wrote to Richard Owen expressing "regret" that Owen endorsed Barry's double spirals – for it was obviously an optical illusion caused by imperfect illumination.[391] But soon after a visit from "our friend" Barry, Carpenter was less certain of his dismissal of the double spirals and told Owen this.[392]

Carpenter's newfound uncertainty was partly founded in his ambition to obtain Owen's patronage – that very same year he sought a testimonial from Owen for a Chair in Edinburgh, and in the very next year Carpenter had Barry present one of his papers to the Royal Society. Soon he would move to London (from Bristol) and try to make money only doing research rather than medicine, and so he would need allies.[393] But the particular specimen he used also caused his uncertainty. Carpenter retreated from his

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[389]Bowerbank, "Early Times of the Achromatic Microscope," pp. 281, 284.
[390]L. S. Jacyna, *A Tale of Three Cities: The Correspondence of William Sharpey and Allen Thomson* (London: Wellcome Institute, 1989), pp. xix-xx; idem, "Moral Fibre: the Negotiation of Microscopic Facts in Victorian Britain," *Journal of the History of Biology* 36, 39-85 (2003): pp. 39-41.
[391]W.B. Carpenter to R. Owen, 26 Jun 1842, OCORR, NHM, 6/302-303.
[392]W.B. Carpenter to R. Owen, 23 Aug 1842, OCORR, NHM, 6/304-305. Likewise, he thought that Bowerbank's 1843 paper purported to see more cells than really existed, but as Bowerbank was so experienced, he did not "wish presumptuously to arraign his correctness." W.B. Carpenter to R. Owen, 23 Jun 1843, OCORR, NHM, 6/321-324.
[393]W.B. Carpenter to R. Owen, 26 Jun 1842; W.B. Carpenter to R. Owen, 23 June 1843. On Carpenter's self-fashioning see also Winter, "The Construction of Orthodoxies."

criticism of Barry's spirals because he didn't think he had the specimen that showed the spirals properly. His initial inability to see Barry's spirals wasn't caused by the microscope, for as Barry stood next to him, Carpenter was able to see Barry's spirals better under his own microscope ("one of [Hugh] Powell's last") than under Barry's. Carpenter could only account for his inability to see these spirals because he had examined the fibre from a lobster ("which Barry says is a bad subject") instead of a shrimp's or turtle's heart; and so Carpenter resolved to obtain different specimens.[394]

Andrew Mendelsohn has already discussed how Schwann was able to see a similarity between Schleiden's vegetable cells and structures in the animal kingdom despite the vast range of morphological structures. It was because Schwann was looking at cartilage from a frog's notochord – and cartilage cells have a delicate membrane and an outer cell wall closely resembling a vegetable cell.[395] Because of the particular resemblance of the units comprising the cartilage with cells from other groups, Schwann was able to infer a larger similarity between plant and animal cells. Thus the shapes that one saw under the microscope frequently depended on the organisms that one used.

2. CENTRIFUGAL DEVELOPMENT

MARTIN BARRY AND A NEW TREE OF LIFE

Martin Barry is of further interest in our story about compound individuality. He signifies a change in British science from the late 1830s onward, as the cultural exemplar

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[394]W.B. Carpenter to R. Owen, 26 Jun 1842.
[395]Mendelsohn, "Lives of the Cell," p. 11.

of France as the land of state-of-the-art research was gradually replaced by Germany. One reason was for its insistence upon the use of microscopy. Germany was a different cultural model against which to compare and critique British research – like France, it also served as an idealization with which to criticize British shortcomings. Various translations of German textbooks appeared, exhorting readers to take up the microscope in their investigations. Albert Kölliker's text recast the history of histology in the light of the neglected microscope – without it, histology had made little progress in the 18th century. And von Siebold's text on invertebrates proclaimed that the anatomy of lower forms was not reliable unless it was based upon minute examination of tissues.[396]

Martin Barry serves as the best early example of a broker between German and British research. After studying in Paris and Berlin he received his MD at Edinburgh in 1833, and then went on to study with Friedrich Tiedemann in Heidelberg, moving between Scotland, England and Germany. In 1837 he decided to introduce the embryological principles of Karl Ernst von Baer to an English-language audience. Von Baerian embryological principles asserted that the developing embryo repeated the taxonomic layout of the animal kingdom, moving from the "general" to the "special" as it became increasingly differentiated, specialized and individuated.[397]

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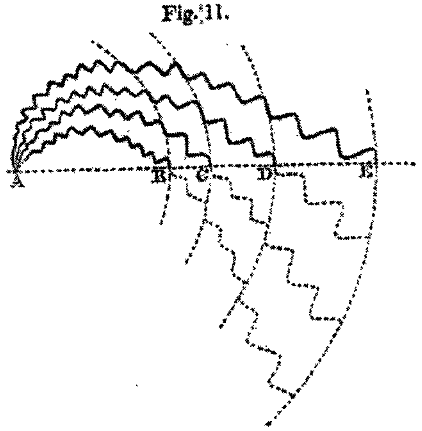
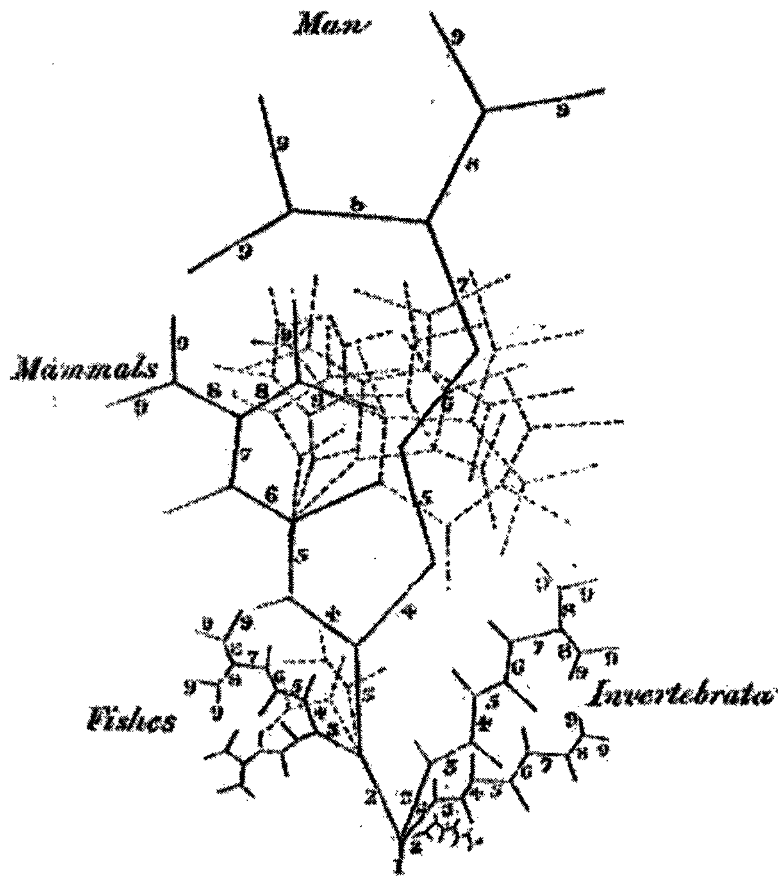
[396]Huxley, "Owen and Rymer Jones on Comparative Anatomy," p. 2; A. Kölliker, *Manual of Human Histology*, trans. G. Busk and T. H. Huxley, 2 vols. (London: Sydenham Society, 1853), pp. 1:1-2; C. T. E. v. Siebold, *Anatomy of the Invertebrata*, trans. W. I. Burnett (London: Trùbner and Co, 1854), pp. vii, x.
[397]Richards, "Embryonic Repetition", p. 214; W. C. Williams, "Martin Barry," in *Dictionary of Scientific Biography*, ed. C. C. Gillispie (New York: Scribner's, 1970), 476-478, pp. 476-478.

Barry depicted the sequence of changes from the general to the special in two tree diagrams.



Martin Barry, "On the Unity of Structure in the Animal Kingdom," *Edinburgh New Philosophical Journal* 22 (1837): 116-41, p. 134.



Martin Barry, "Further Observations on the Unity of Structure in the Animal Kingdom, and on Congenital Anomalies, Including 'Hermaphrodites'," *Edinburgh New Philosophical Journal* (1837): 345-64, 346.

Both portrayed a set of paths increasingly diverging from a single central point. In the diagram on the left Barry showed the initial structural unity of vertebrate germs and the subsequent variety that ensued. Point A represented the similarly-formed vertebrate germs; points B, C, D and E represented the developmental paths of fish, reptiles, birds and mammals respectively. The similarity of the curves were supposed to show that although resemblances did appear, they were not identity.[398] Barry's diagram on the right, appearing later that year, was more elaborate than the first, representing the divergences that a differentiating embryo could take. At point 1, before any ramifications had occurred, it was a homogeneous and undifferentiated germ. At the first ramification-point, point 2, it differentiated in the most general fashion, becoming a member of the group of invertebrates or vertebrates. Successive differentiations sent the embryo on increasingly specialized paths – thus one might follow the foreground path into the group mammals and then humans. This diagram showed the initial structural unity and subsequent diversity acquired through development.[399]

In using a tree diagram, Barry's depiction of von Baerian embryological principles conveyed a different sort of development than synthesis. Where synthetic development emphasized a centripetal process in which discrete elements fused to form concentrated, compounded units, and where the development of the nervous system was emphasized above all else as an index of an animal's development, Barry presented a different image. He instead portrayed a centrifugal process: the differentiation of a simple unit into a more specialized one. Moreover the diagram emphasized the *activity* and process of development, where the discrete entities were seen as less important than the activity itself.

In Barry's view, development moved in the opposite direction, from the general to the special, from a single central point and outwards, through the differentiation of existing parts. Barry noted how new parts were not *added* to the embryo, as Serres maintained; for Barry the key point about von Baerian embryology was its critique of

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398 M. Barry, "On the Unity of Structure in the Animal Kingdom," *Edinburgh New Philosophical Journal* 22, 116-141 (1837): pp. 134-135.
399 Barry, "Unity of Structure in the Animal Kingdom," pp. 134-135.

Serres's embryology. Von Baerian embryology didn't propose that parts appeared in isolation and then fused with others – instead, it showed how new parts appeared through the transformation of existing parts. And against Serres, who portrayed development as proceeding from the periphery towards the centre, Barry noted that von Baer convincingly showed how development moved centrifugally, "from the centre towards the periphery."[400]

So this distinction cannot be placed in the familiar opposition of preformation versus epigenesis, for we have already seen that Serres also thought of his work as supporting epigenesis. To repeat, Barry explicitly contrasted what he saw as two developmental directions: one a centripetal coalescence, or synthesis, of parts within a closed system, the other a centrifugal differentiation of parts emerging from a central point. And he not only depicted these developmental directions as being in opposition, but he also used the image to the tree to distinguish the cultural origins of these different directions. It was no longer the French example that British researchers ought to copy, but the *German*. When it came to classification,

> The fact is, that naturalists have begun, just where they should have ended. They have attended to details, but neglected general principles. Instead of analyzing, their process has been one of synthesis. Their attention has been directed to the groupings of the *twigs*, - as if thus they were to find their natural connections, without even looking for assistance towards the branches, or the trunk that gave them forth...It is only now that a way is beginning to be opened, by which it may by and by be possible to proceed in an opposite direction; viz. from trunk to branches and to twigs... [If this is accomplished it has to be through]... the *History of Development* or *Embryology,* both human and comparative; a science almost new, and regarding which, there prevails in this country the profoundest ignorance and indifference. The French are in advance of us; but it is to *German* enterprise, industry, and perseverance, that we are indebted for almost every fact known to us on this subject; at least of those brought to light in recent times.[401]

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[400]Barry, "Unity of Structure in the Animal Kingdom," p. 123.
[401]M. Barry, "Further Observations on the Unity of Structure in the Animal Kingdom, and on Congenital Anomalies, including 'Hermaphrodites'; with some remarks on Embryology, as Facilitating Animal

Researchers were looking at the wrong part of the tree: instead of starting from the twigs and moving inwards, researchers ought to be moving from the trunk and moving outwards. Despite the stridency of Barry's message, however, at least one of his colleagues, John Goodsir, was to read Barry as supporting the opposite view - that development proceeded centripetally.

3. JOHN GOODSIR: INFUSORIA, CELLS AND COMPOUND INDIVIDUALITY

Where Martin Barry introduced new abstract principles of development to guide British researchers, John Goodsir provides a case study of more concrete research and the use and misuse of these principles. He used infusoria as examples which could be extended to better understand processes of cell-birth and cellular secretion. Recall that at the time (the early to mid-1840s) the identity between cells and infusoria (later to become protozoa) was still contested. These researches show how Goodsir's work was informed by a belief in the disunity of the organism.

Goodsir has been discussed above as a reformer of museums. But he was also one of Britain's elite microscopists, and he took up the study of cells after Schleiden and Schwann's announcement, for they promised to become the "Archimedean lever to the biological world".[402] In 1840 Martin Barry noted that cells develop within a "parent" centre, with the germinal vesicles filling with cells, and these cells in turn being filled with other new cells. Goodsir followed Barry's findings, as well as previous Continental

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Nomenclature, Classification, and the Study of Comparative Anatomy," *Edinburgh New Philosophical Journal* 22, 345-364 (1837): pp. 362-4.

[402]P. F. Rehbock, *The Philosophical Naturalists : Themes in Early Nineteenth-Century British Biology* (Madison: University of Wisconsin Press, 1983), p. 93. Lonsdale, "Biographical Memoir," pp. 89-90.

work too; he mentioned as important forerunners Müller and Johannes Purkinye. Goodsir's work has therefore been seen as situated in an 'idealist' or 'transcendental' worldview, in which the cell existed as the fundamental unit of life.[403]

But in the late 1830s and early 1840s Goodsir still occasionally saw patients, in addition to his cellular researches. And the infusoria basking in one man's upset stomach seems to have given Goodsir an interesting example that in turn could be applied to his cellular researches. While causal links are still difficult to establish the affinities are exceptionally close and suggestive. On 10 February 1842 John Goodsir read a paper at the Botanical Society of Edinburgh, on a medical subject.[404] One of his patients, a man of age 19, had been complaining of an upset stomach for four months – in the morning he would awake with a feeling of a distended stomach. He could pass up a quantity of fluid from his stomach varying from two-thirds to an entire "wash-handbasinful", which relieved the distension. But throughout the day, the man and others around him could hear bubbling in his stomach, and he would awake the next morning with the same distended feeling to again pass up more stomach-liquid.

Goodsir examined some of the expelled stomach fluid. It smelled of fermenting worts, which to Goodsir indicated the possibility of fermentation. Upon examining some residue from the fluid, he noticed "numerous individuals" in each drop - infusorians that he related to related to O.F. Müller's genus *Gonium*.[405] Below is a picture of *Gonium*,

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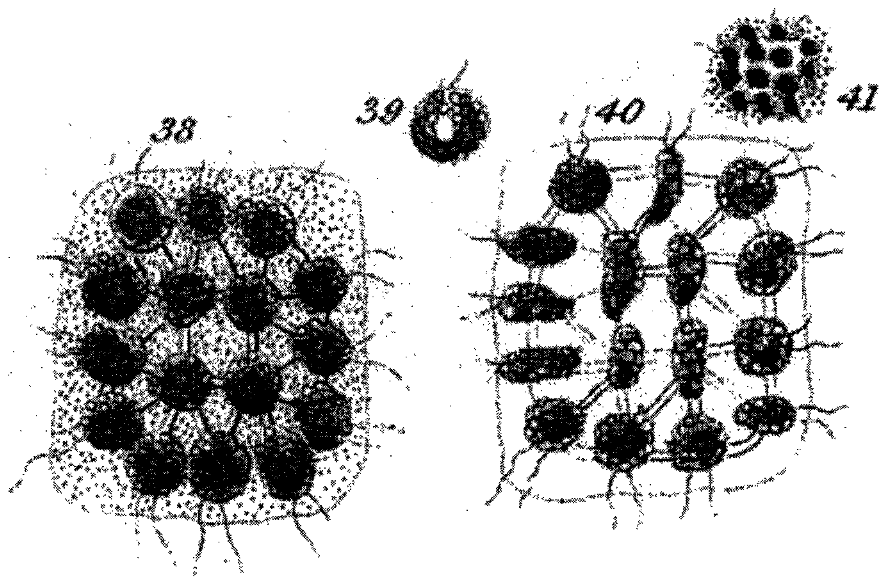
[403]L. S. Jacyna, "John Goodsir and the Making of Cellular Reality," *Journal of the History of Biology* 16, 75-99 (1983): pp. 89-90; idem, "Reception of Cell Theory in Britain"; Rehbock, *The Philosophical Naturalists*.

[404]"Papers read at the Botanical Society of Edinburgh," *Annals and Magazine of Natural History* 9, 153 (1842).

[405]J. Goodsir, H. Lonsdale, and W. Turner, *The Anatomical Memoirs of John Goodsir*, 2 vols. (Edinburgh: A. and C. Black, 1868), pp. 2:351-353. Goodsir prescribed a few drops of creosote a day for the man, and told him to eat more meat; this alleviated the problem, so the man only (!) had to pass up stomach fluid once or twice a week. *Gonium* are now thought to be spores.

taken from Pritchard's *History of Infusoria* – notice their arrangement into "squares" of 16.



Gonium, in A. Pritchard, *A History of Infusoria* (London: Whittaker and Co, 1841), plate 1.

Because of their peculiar form – like *Gonium*, appearing as a square with sixteen squares each containing four cells - Goodsir gave his particular specimens the name *Sarcina ventriuli*. The term "sarcina" was taken from the Latin word for "bundle." For these organisms were arranged into nested sets of squares. There were always 16 squares, and each square was itself divided into four "secondary squares." Goodsir faintly made out that these secondary squares were themselves composed of four tertiary squares. The principle of nested sets extended to the reproduction of these infusorians – they reproduced "fissiparously," with each "individual" infusorian (of 16 primary-level squares) dividing into four. Goodsir admired the symmetry of the organism, how their parts increased geometrically and how the species also propagated the same way. But he noted that it was because of the very similarity between part and individual that it was difficult to distinguish between specimens that were simple individuals and "composite ones."[406]

On 21 March, just over five weeks after reading the paper on his patient's unfortunate encounter with *Sarcina*, John Goodsir read a paper to the Royal Society of Edinburgh on cellular secretion. He noted that anatomists such as Purkinye had already hypothesized that the cells lining gland-ducts somehow secreted; other researchers such as Müller and Jacob Henle had built upon the notion of cellular secretion by noting that there were indeed "closed vesicles" at the tips of these gland-ducts. Goodsir proudly announced his solution to the mystery: that secretion took place inside the cell itself. The cell wall secreted the fluid (for instance, milk, or bile, or squid ink), the cell-cavity stored that fluid, and the nucleus was the cell's reproductive organ. While he admitted that his finding still didn't yet explain how secretion occurred, he suggested that the cell was the "ultimate secreting structure."[407]

Goodsir used the familiar language of ripening fruit to convey the process of cellular secretion. Because the secretions were enclosed in the "ripe" cells making up the duct wall's surface, it explained why secretions appeared only on the surface. Goodsir used the term "acinus" (the OED gives the original form of this word as a berry growing in a cluster) to describe a primary "germinal" cell with numerous "secondary" cells enclosed within it. Indeed, this ripe fruit would actually burst open: each secondary cell grew larger as fluid built up in its cell-cavity, the pressure eventually breaking open the primary and often the secondary cell, releasing the secretion (the secretion, like semen,

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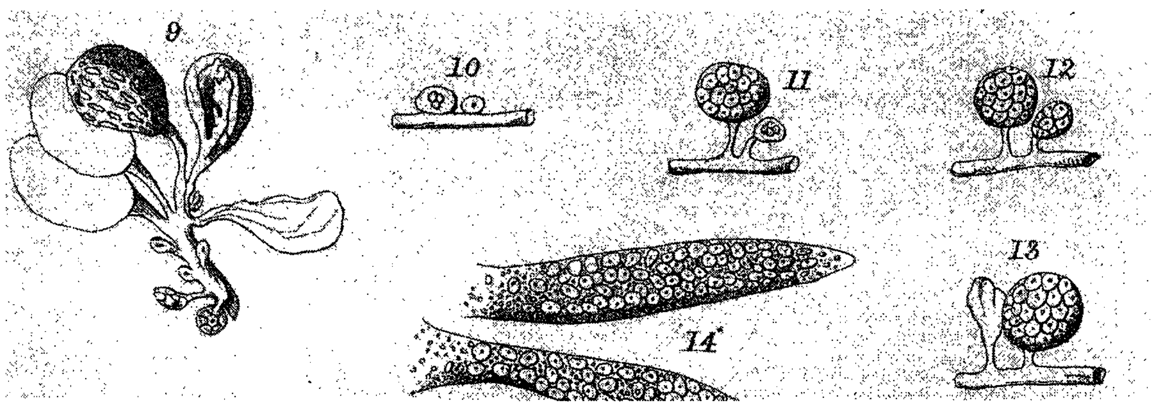
[406]Goodsir, Lonsdale, and Turner, *Anatomical Memoirs*, pp. 2:356-359.
[407]"Papers read at the Royal Society of Edinburgh," *Annals and Magazine of Natural History* 9, 254-256 (1842): pp. 254-255.

might contain secondary cells in it like spermatozoa). A neighbouring acinus, growing in the same fashion, took the place of the burst acinus.[408]

In an 1845 collection of his papers (reprinted in 1868), Goodsir included a diagram. Figures 10 through 13 sequentially represent the change of two "acini", with secondary cells swelling inside the primary; by figure 13 the left acinus has burst. Figure 9 illustrates the same sequence, this time at the tip of a secreting duct.



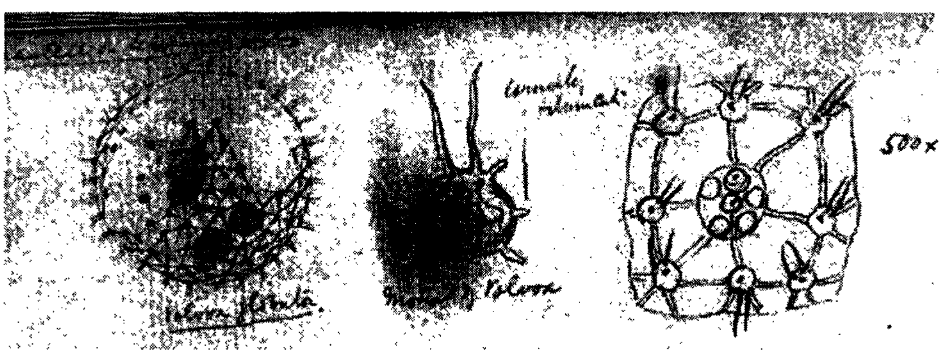
Sequence of bursting acini (figures 10 through 13, and in figure 9 from left to right), from J. Goodsir *et al*, *The Anatomical Memoirs of John Goodsir* (Edinburgh: A. and C. Black, 1868) pl. V, 2:412.

It cannot be entirely coincidental that Goodsir's proposal of cells that swelled and then ruptured, releasing the fluid inside, occurred just after his description of a patient who regularly passed up a mysterious stomach fluid caused by infusorians. The resemblance is also stronger than a vague analogy. It appears that Goodsir linked a familiar instance with a research problem, associating a patient - whose stomach became distended with fluid, having to push out that fluid to obtain some relief - with the view that cells likewise released fluid from an inner cavity by "bursting" after distension. To repeat, while the resemblance of the two cases does not mean a definite historical cause, the similarities are fascinating and indicate a direction for more research.

408 "Papers read at the Royal Society of Edinburgh," pp. 254-255.

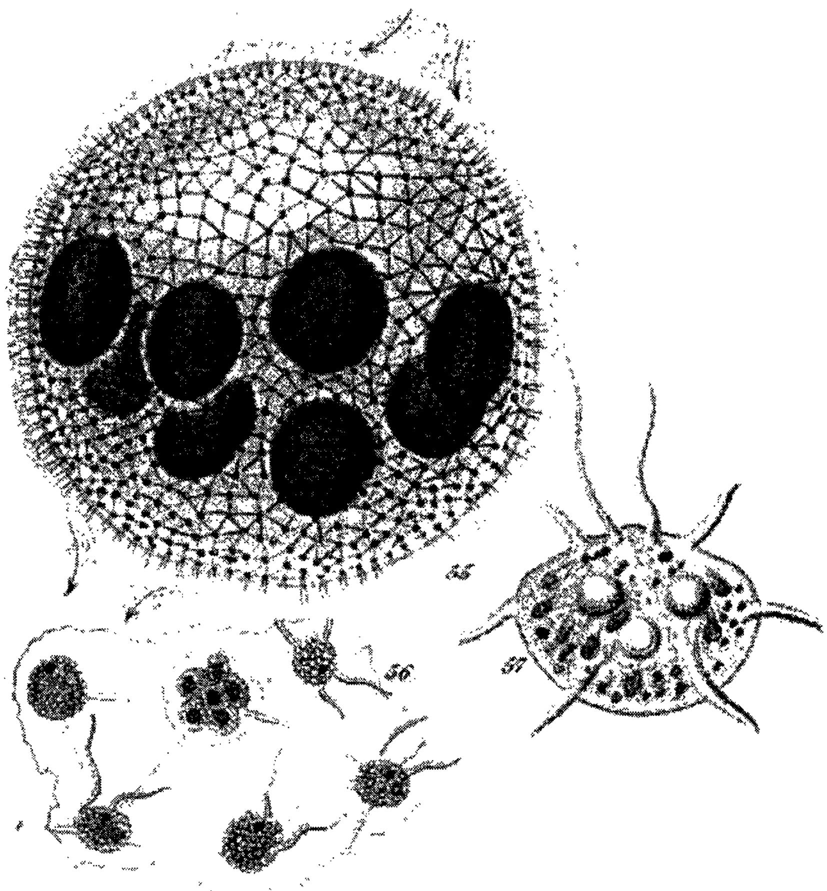
Strengthening this hypothesis is the fact that these infusorians, placed in the genus *Gonium*, were members of the volvocina family – a group including *Volvox globator*.[409] Goodsir explicitly noted that *Sarcina ventriuli,* as members of the genus *Gonium*, were members of the volvocina group.[410] Indeed, only three years before Goodsir's paper was read at the Royal Society of Edinburgh, Allen Thomson, also Edinburgh-based, had given both *Gonium pectorale* and *Volvox globator* as exemplars of fissiparous generation – in both cases, they were analogous to the multiplication of individuals by division.[411]

Volvox was an extremely popular specimen for British microscopists of the 1840s. The first picture, below, is from Andrew Pritchard's 1841 *History of Infusoria,* an imitation of Ehrenberg's work. Pritchard used terminology too, calling the eight smaller globes inside the main globe, "sisters". The second picture is a sketch from T.H. Huxley's notebook, in which Huxley faithfully copied the picture of *Volvox* from Gideon Mantell.[412] The first image is of an entire *Volvox;* the next is of one of the interior globes, which Huxley called a "monad"; and the third is of the surface of *Volvox.*



Volvox from T.H. Huxley, *Rattlesnake Notebook* 50.7, Huxley Papers, Imperial College, London

[409]Pritchard, *A History of Infusoria*, p. 74.
[410]Goodsir, Lonsdale, and Turner, *Anatomical Memoirs*, p. 2:353.
[411]A. Thomson, "Generation," in *Cyclopaedia* (1839), 424-480, p. 432.
[412]G. A. Mantell, *Thoughts on Animalcules; or, A Glimpse of the Invisible World Revealed by the Microscope* (London: J. Murray, 1846). Huxley's drawing (which has "Mantell on Animalcules" scrawled on the top, visible in this drawing) reproduces Mantell's even to the placement of the interior globes and depiction of the volvox's monad. Compare this with his later dislike of students who merely "crammed" from textbooks and did not see nature for themselves; this is discussed in G. Gooday, "'Nature' in the Laboratory - Domestication and Discipline with the Microscope in Victorian Life-Science," *British Journal for the History of Science* 24, 307-341 (1991).



Volvox globator with eight daughters inside it, from A. Pritchard, *A History of Infusoria*, plate 1, p. 128.

Unlike Goodsir's *Sarcina ventriuli,* which divided by splitting ("fissiparously"), *Volvox* provided the observer with an occurrence of 'Russian-doll' organization, in which "Each sphere or globe is a hollow cluster...of many hundreds or even thousands of these living occupants, and often contain within it other hollow spheres".[413]

In a slightly different way than Goodsir's *Sarcina,* the bursting of the "mother" *Volvox* after its young had fully developed also seems to be close to Goodsir's notion of secretion through bursting "acini". We do know that *Volvox* appeared repeatedly in British textbooks and articles between 1830 and 1850, usually as an example of this Russian-doll organization, and especially for their distinctive mode of reproduction, in which the young *Volvox* burst out of their parent. In 1830, John Dalyell argued that the

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[413]Pritchard, *A History of Infusoria*, pp. 127-128.

Volvox contained smaller, younger *Volvoxes* inside it, and within them smaller ones – he noted that some observers had found up to five generations in one *Volvox*. When the younger ones attained maturity, they detached from the parent's wall and swam about its inside; the mother then burst and disappeared.[414] In 1834 Peter Mark Roget gave *Volvox* as his example of germs forming inside an animal.[415] In 1840 Richard Owen gave the propagation of *Volvox* to illustrate preformationist doctrines, adding that Spallanzani, Leeuwenhoek and Roesel all used *Volvox* as the "true representation" of this type of generation: the parent in the form of a hollow sphere, young spheres developing inside it, and successively expanding to rupture the parent case.[416] Thus *Volvox* may have also provided Goodsir with his image of the endogenous replication of cells.

In fact infusorians may have been more important for Goodsir's work than abstract principles, foiling our view of Goodsir as an idealist or transcendentalist. This is shown by Goodsir's assimilation of Barry's principles to his own context, even his 'misreading' of Barry's work. Goodsir credited Barry with pointing out how all organic forms had a central origin. In his view, Barry had given the first consistent description of the development of "parent cells" from a "parent centre," and Goodsir also noted how he had borrowed the term "germinal spots" from embryology.[417] And yet Goodsir still

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[414]J. G. Dalyell, "Animalcule," in *The Edinburgh Encyclopaedia*, ed. D. Brewster (Edinburgh: Blackwood, 1830), 134-143, p. 138.

[415]Roget, *Animal and Vegetable Physiology*, pp. 2:590-591.

[416]Owen, "Hunterian Lectures on Generation". Owen mentions *Volvox* again in his 1849 *On Parthenogenesis,* in the context of an example of compound individuality: "The stomach of a man if detached would not live and reproduce its kind like the polype of the Sertularia or the monad of the *Volvox*; nor would the lung manifest its individuality like the leaf of the Bryophyllum under similar circumstances." R. Owen, *On Parthenogenesis, or, The Successive Production of Procreating Individuals from a Single Ovum : a Discourse Introductory to the Hunterian Lectures on Generation and Development* (London: J. Van Voorst, 1849), p. 61. *Volvox* is still used as an example in the mid-20th century illustrations of compound individuality: see J. T. Bonner, *Cells and Societies* (Princeton: Princeton University Press, 1955), pp. 102-104.

[417]J. Goodsir and H. Goodsir, *Anatomical and Pathological Observations* (Edinburgh: Myles MacPhail, 1845), pp. 2n, 1.

interpreted Barry's work through the lenses of analysis and synthesis. For against Barry's explicit, even didactic points, Goodsir still thought of development as a centripetal process, noting that "Instead of growth being a species of imbibing force, and secretion on the contrary a repulsive, the one centripetal, the other centrifugal, they are both centripetal."[418] The belief that development was a synthetic process, occurring centripetally through coalescence, explains Goodsir's conception of cell-growth as endogenous reproduction – for both secretion and growth saw the cavity of a cell (between the cell wall and the nucleus) fill with either "nutritive matter" or with other cells, eventually bursting and throwing the secretion or other cells out.[419]

In addition to the possible exemplar of *Volvox* for his notion of growth and secretion, Goodsir was also motivated by geometry, astronomy and Newtonian mechanics. In noting how every organism formed a "system in which every part was related to some common centre"[420] - much like a system of planets revolving around a sun, governed by the centripetal force of gravity - Goodsir sought to find the similar Newtonian principles. He noted how Newton had shown in the *Principia* that if attraction varied as the inverse cube instead of the inverse square of the distance, astronomical bodies wouldn't revolve in ellipses but would adopt logarithmic spirals instead, diffusing and rushing off into space. So Goodsir suggested that "if the law of the square were the law of attraction, the law of the cube might therefore prove to be the law of production" – explaining how cells expanded at a geometrical ratio. For his example

[418]Goodsir and Goodsir, *Anatomical and Pathological Observations*, pp. 25-26.
[419]Goodsir, Lonsdale, and Turner, *Anatomical Memoirs*, pp. 2:417-419, 2:426-427.
[420]Goodsir, Lonsdale, and Turner, *Anatomical Memoirs*, p. 2:207. This is from his 1849 lecture "On the Employment of Mathematical Modes of Investigation in the Determination of Organic Forms".

of organisms expanding at a geometric rate he gave the Goniums and *S. ventriuli.*[421] Goodsir's discussion of secreting acini – composed of "secondary cells" enclosed within "primary" ones – can further be compared with the "composite" *S. ventriuli* replicating geometrically in his patient's stomach. In both cases we are presented with a 'nested set' where each part can be seen as an individual too.[422]

A further paper of Goodsir's - "Centres of Nutrition" – provides another link between his cellular researches and the infusorian *Volvox globator.* At one point Goodsir showed a portion of an encysted tumour removed from a patient's tongue. The picture appears to move in magnification from number 1 to 3 to 2 in order to focus on one particular cell (2b is a magnification of 3b). Goodsir commented that the nuclei, "instead of remaining as single germinal spots for each cell, have broken up into numerous spots or centres of nutrition."

Goodsir used the language of compound individuality to explain how cells reproduced. The tumour had some cells with one nucleus, but others with many nuclei; the ones with multiple nuclei - depicted in 2b and 3b - were "reproductive individuals" that gave birth to new tumours. Some cells had specialized functions. To show this principle of the differentiation of reproductive functions in different cells, Goodsir tried using a different example. The morbid tumour indicated the principle that

> ...certain parts are set aside as reproducers, the remaining parts performing the functions of the whole mass, texture, or organ; just as in certain communities of animals certain individuals are set aside to reproduce the swarm, the others are devoted to the duties of the hive.[423]

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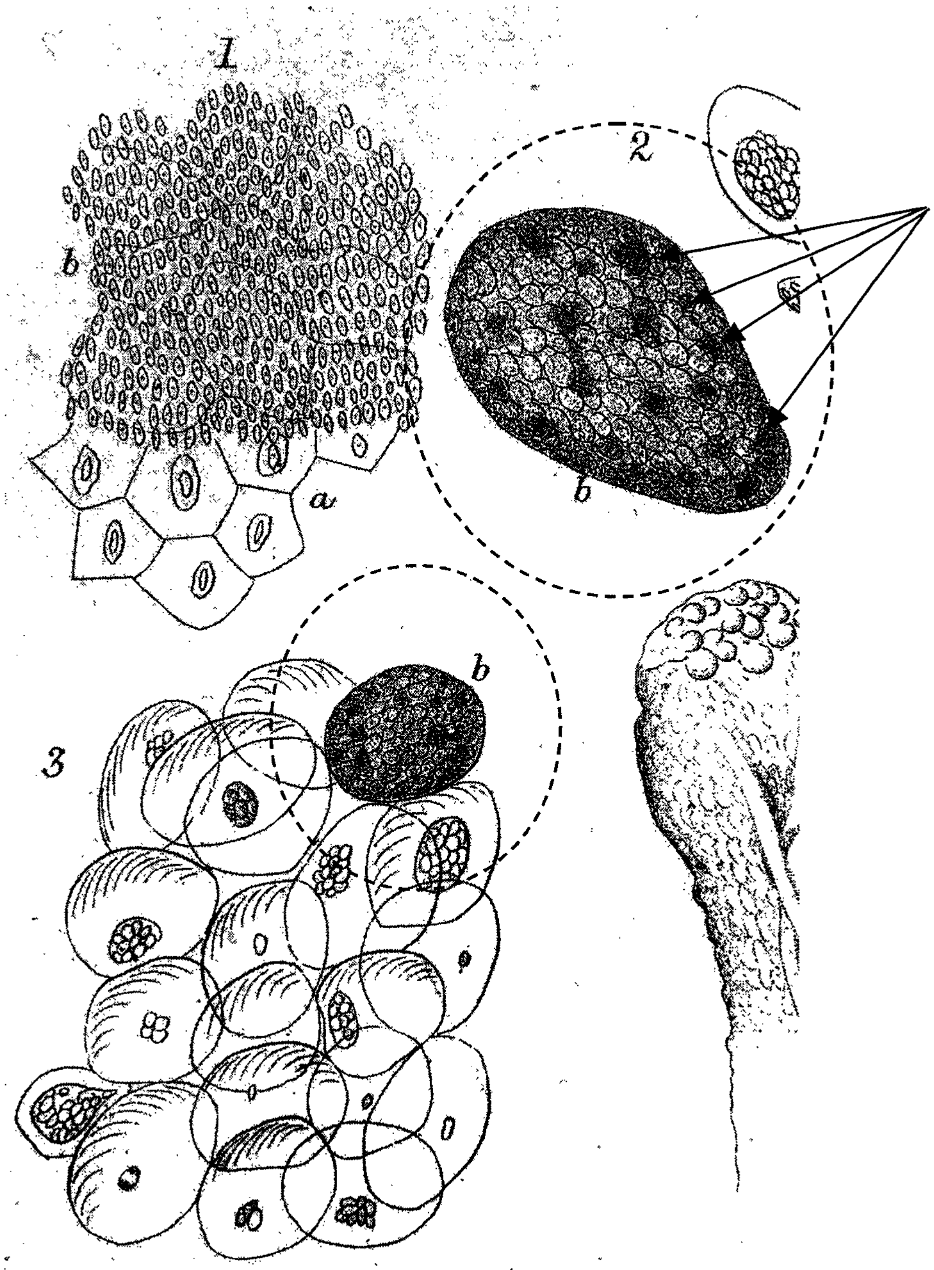
[421]Goodsir, Lonsdale, and Turner, *Anatomical Memoirs*, pp. 2:211-213; Rehbock, *The Philosophical Naturalists*, p. 94.
[422]Goodsir, Lonsdale, and Turner, *Anatomical Memoirs*, pp. 2:359-360.
[423]Goodsir, Lonsdale, and Turner, *Anatomical Memoirs*, p. 2:xv.



Germinal spots in tumour, from J. Goodsir *et al*, *Anatomical Memoirs*, pl. IV, 2:389

Note how similar 2b and 3b are to the depictions of *Volvox*.

As in Schwann, the example of the colonial insects appeared once again to illustrate how the cellular community was organized. Georges Canguilhem once wondered if the human mind, in consciously borrowing from the beehive the term "cell," did not also unconsciously borrow the notion of cooperative labour producing the honeycomb. He noted how strongly the social values of cooperation and association lurked in the background of the cell theory.[424] Goodsir's above statement makes Canguilhem's observation far more specific.

By subdividing the organism into separate regions, each arrayed around a centre, Goodsir thought he had improved upon Schleiden and Schwann's proposal that each simple developed cell had an independent vitality. To depict each cell's relationship to a centre he used terms from the social world: each of these subdivisions were "departments" centred upon a "capital" cell: "It would appear that from this central cell all the other cells of its department derive their origin. It is the mother of all those within its own territory."[425] Goodsir's cellular work would prove influential: his work on "centres of nutrition" - even his wording of "departments" with simple cells grouped around a "capital" cell - was taken up by Rudolf Virchow.[426]

Goodsir's example shows how infusoria were used to understand and depict how cells grew and secreted. He was habituated to analysis and synthesis, which constrained his view of infusoria and organisms as nested sets of elementary cell-individuals. Goodsir's cellular research contributed to a common context of compound individuality. Likewise, the rejection of Ehrenberg's theory of complex infusorians also seems to have been rooted in analysis and synthesis, for those rejecting Ehrenberg's notion were driven to reject him because they also saw organisms as nested sets of elementary units, or 'monads.' Ehrenberg's rejection serves as a negative example of how powerful a habit of reasoning could be.

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[424]Canguilhem, *A Vital Rationalist*, p. 162.
[425]Goodsir and Goodsir, *Anatomical and Pathological Observations*, pp. 1-2.
[426]Goodsir's biographer groused that Virchow took Goodsir's work without the proper acknowledgement – despite Virchow's dedication of *Die Cellularpathologie* to Goodsir. Jacyna, *A Tale of Three Cities*, p. xx; Lonsdale, "Biographical Memoir," pp. 92-94.

Goodsir's work serves as a positive example. His support of analysis and synthesis was shown in his rehabilitation of Martin Barry's message to his own ends. Though Barry explicitly noted that von Baerian embryological development meant the *centrifugal* differentiation of a homogeneous mass into heterogeneous specialized tissues, Goodsir read Barry in the opposite way. Despite explicitly acknowledging Barry's embryological findings as a support for his own cellular researches, and even though he adapted embryological terminology in his cellular investigations, Goodsir saw both cellular secretion and growth as governed by a process of *centripetal* accumulation, a process not unlike Newtonian gravity. In that sense, Goodsir's notion of cellular secretion and growth were both forms of synthesis. This 'misreading' by Goodsir shows how a seemingly intractable point could be reinterpreted to better fit a holistic habit of reasoning – showing how difficult it was to free oneself from that habit.

CHAPTER 4
ANALYSIS VERSUS PALAETIOLOGY

> But all these special matters of conflict are nothing more than tokens of a totally different way of thinking – of a certain incompatibility of scientific opinion…. We are not aware that either author has given in these works any formal definition of the exact meaning he wishes to attach to these words [like "higher" and "lower" animals]; but we venture to say that they may be regarded as centres of ideas totally different in the one case and in the other.
>
> -[Michael Foster], on the use of terms like "higher" and "lower" animals, in his review of Richard Owen's *On the Anatomy of Vertebrates,* 1868; T.H. Huxley's *Lectures on the Elements of Comparative Anatomy,* 1864. In "Higher and Lower Animals," *Quarterly Review* 127 (1869): 201-11, p. 201.[427]

This chapter examines one clash between supporters of two different habits of reasoning. On the one hand we have those habituated to analysis and synthesis, like E.S. Forbes, Thomas Rymer Jones and Richard Owen; on the other we have those habituated to palaetiology, like W.B. Carpenter, George Allman and T.H. Huxley. Understanding the two different groups as supporters of different habits of reasoning – and in turn as committed or opposed to compound individuality - allows the reinterpretation of the infamous clashes between Richard Owen and T.H. Huxley that began in the 1850s and intensified as the decade progressed.

This chapter begins by examining recurring exemplar organisms which were favoured as illustrations of compound individuality. It then looks at Richard Owen's doctrines of "vegetative repetition" and "parthenogenesis" / "metagenesis", showing that what appears to be a confusing set of principles can be simply explained as analytic and synthetic answers to the problem of compound individuality. These doctrines marked the high point of the analytic and synthetic habit's British career.

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[427]Foster's name was determined through the *Wellesley Index.*

Next, the the habit of palaetiology - a term coined by William Whewell to understand a field of researches that investigated historical changes - is introduced and examined in detail. Palaetiology was cultivated by two ambitious young researchers, W.B. Carpenter and T.H. Huxley, who used it to compare and criticize previous analytic and synthetic researches and the people who carried out these researches.

Both men used palaetiology to reinterpret familiar exemplar organisms and convert other researchers to this habit of reasoning. This meant that the exemplar organisms seen as compound individuals in the habit of analysis and synthesis were reinterpreted by palaetiologists as unitary individuals sprouting from a single central point: the act of sexual fertilization. Thus as palaetiology waxed, the common context of compound individuality waned.

EXEMPLAR ORGANISMS

Between the 1830s and 1860s certain organisms recurred in textbooks and research papers. Each 'exemplar organism' - in some cases its past interaction with a famous researcher - became a representation of a different problem. The barest mention of them became shorthand for a particular physiological or morphological problem. Accounts about exemplar organisms were told and re-told by different British researchers, sometimes out of habit, sometimes consciously; but the account itself became a resource, a case repeatedly cited to illustrate a biological point.[428]

Any competent researcher was supposed to know the implication of a particular exemplar organism. Thus in 1845 Steenstrup (via translator George Busk) pointed out

[428]Gilbert and Mulkay, *Opening Pandora's Box*, pp. 14-15.

that the alternation of generations in aphids "as a superfecundation is sufficiently well known". In 1846 George Newport noted that the story of Bonnet's investigation of aphid "virgin-births" was already so well known it would be an act of "supererogation" to repeat the story, though he sought to examine them again to see how they illustrated "some universal law of reproduction." William Benjamin Carpenter presumed that all of his readers were so familiar with Trembley's, and others', mutilations of *Hydra* that he didn't need to repeat them.[429]

This chapter will look at other familiar exemplar organisms of the period. In addition to aphids and *Hydra* there were tapeworms and earthworms, Salps, Planarians, Leeches, sertularian polyps and Naids. Moreover, certain naturalists were continually associated with these animals: in addition to Trembley's *Hydra* and Bonnet's aphids, we see Dugès and leeches and planarians, and Chamisso and his salps. These exemplar organisms were related to the common context of compound individuality and illustrated the problems of *regeneration / the reproduction of parts*; *virgin-births;* the mysterious origin of *parasites;* the problem of *young emerging through budding*; the process of *metamorphosis;* and *serial homology*. In 1854 the physiologist Allen Thomson compiled a list of these exemplar organisms (in Todd's *Cyclopaedia of Anatomy and Physiology,* by now some 22 years after its projection):

> The occurrence of non-sexual multiplication among some of the Invertebrated animals had long been known, as of Polypes, by budding, so admirably described by Trembley in his work published in 1744; and of the Aphides, by internal production, discovered by Reaumur and Bonnet; and of the Nais and Nereis, by external extension, described by Otto F. Müller, in 1800; the imperfect conditions

[429] W. B. Carpenter, "On the Development and Metamorphoses of Zoophytes," *British and Foreign Medico-Chirurgical Review* 1, 183-214 (1848): p. 195; G. Newport, "Note on the Generation of Aphides," *Transactions of the Linnean Society* 20, 281-283 (1851): p. 281; J. J. Steenstrup, *On the Alternation of Generations, or, The Propagation and Development of Animals through Alternate Generations* (London: Ray Society, 1845), p. 7.

> of some of the Entozoa had been detected by Nitsch and V. Baer in 1818: the two forms of the Salpae were known to Chamisso in 1819, who, more than any other observer, appears to have foreseen in these animals the discovery of alternate or dissimilar generations...[430]

"Entozoa" refer to animals living inside other organisms, such as parasites.

One frequently-appearing exemplar entozoan was the tapeworm (*Taenia* and its larval form, *Caenurus).* It was a familiar parasite, something sucking the life from a biological - or social – host. Edward Forbes made his own analogies here:

In Downing Street the tape worms thrive;
In Somerset House they are all alive;
And slimy tracks mark where they crawl
In and out along Whitehall.

When I'm dead and yield my ghost,
Mark not my grave by a government post;
Let mild earth worms with me play,
But keep vile tape worms far away.

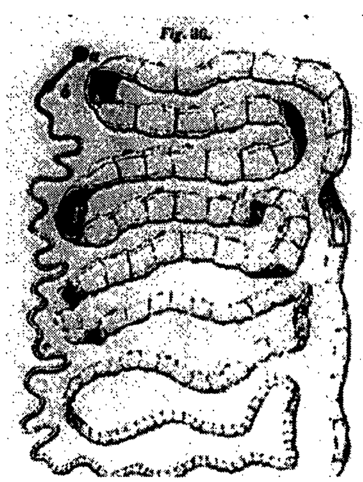
And if I deserve to rise
To a good place in Paradise,
May my soul kind angels guide,
And keep it from the official side![431]

But the tapeworm also had a distinguished career in textbooks and articles as an exemplar of repetitive, quasi-independent, physiological systems. Robert Grant's 1833 comparative anatomy lectures described each tapeworm segment as complete in itself – each segment could thus "be viewed as a separate being". Peter Mark Roget's 1834 *Bridgewater Treatise* on comparative anatomy and physiology described the tapeworm as an organism with an independent "nutritive apparatus" in each segment, though with

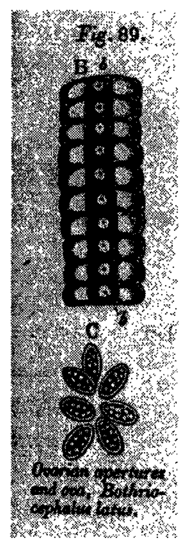
[430]A. Thomson, "Ovum," in *Cyclopaedia* (1852-1856), 1-142, p. 35.
[431]Bettany, "E. Forbes."

mouths only in the anterior.[432] W.B. Carpenter's 1839 physiology textbook noted that tapeworms had a separate ovary in each segment, so some regarded each segment as a separate animal. In 1852, Allen Thomson noted that if sexual completeness was the mark of individuality, then each tapeworm segment could be seen a distinct animal.[433]

One of the early discoveries which made Owen famous was of the parasite that caused trichinosis: he suggested that the *Trichina spiralis* lying in human muscular tissue was actually the embryo of a larger worm (overthrowing the view that these parasites were spontaneously generated, which Owen strongly opposed).[434] As an expert on parasitism, Owen wrote an 1839 article on "Entozoa" for the *Cyclopaedia of Anatomy and Physiology;* in the section dealing with "Organs of Generation", he portrayed each tapeworm segment as having both male and female generative organs.



Taenia solium, the tapeworm usually found in humans, in T.R. Jones, *A General Outline of the Animal Kingdom,* p. 84.

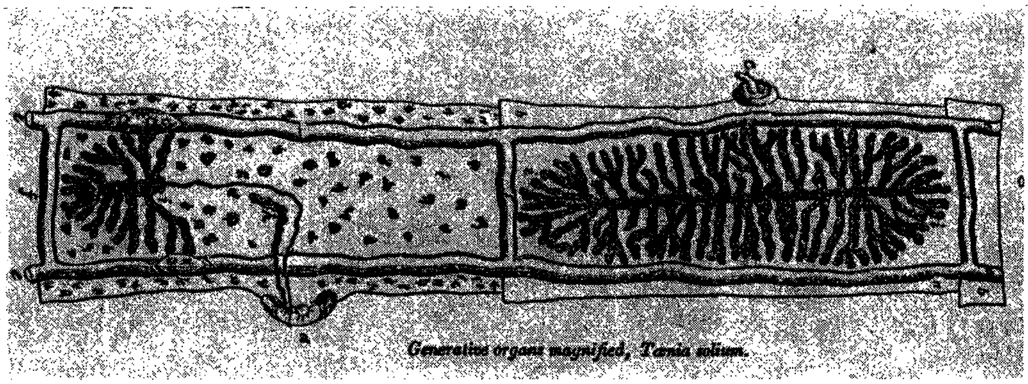


Ovaries of the simplest Cestoid Worms, located between each segment. R. Owen, "Entozoa," in *The Cyclopaedia of Anatomy and Physiology*, 111-143, p. 137.

[432]Grant, "Lectures on Comparative Anatomy," p. 2:487; Roget, *Animal and Vegetable Physiology*, pp. 2:82-83.

[433]Carpenter himself did not agree with this, as he believed the tapeworm head was essential for it to be an individual. Carpenter, *Principles of Physiology*, pp. 106, 109; Thomson, "Ovum," p. 29.

[434]R. Owen, "Presidential Address," *BAAS*, xlix-cx (1859): pp. lxx-lxxii. Interestingly, reputable researchers were in favour of spontaneous generation even in 1840: Owen singled out von Baer as one. Owen, "Hunterian Lectures on Generation".



Repeating generative organs of *Taenia solium*, in R. Owen, "Entozoa," p. 137.

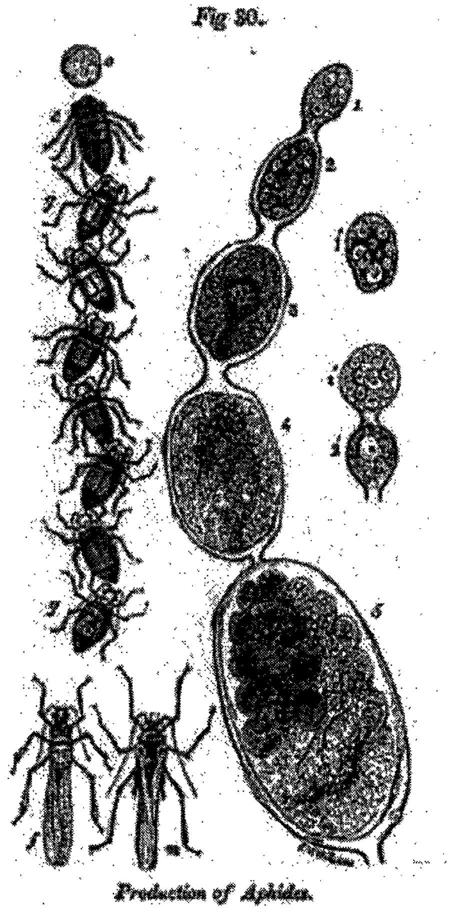
Owen also discussed planarians and Dugès's researches on them, particularly their "remarkable" ability for "spontaneous fissiparous generation" and the ability with which detached or mutilated parts took on the form of the perfect animal.[435]

In 1841 Owen's protégé Thomas Rymer Jones repeated the example of planarian mutilations, again citing Dugès, and he noted that every separated portion of a planarian eventually became complete and perfect again.[436] In his 1843 *Lecture on Invertebrates*, Owen - like any busy academic - recycled bits from his previous "Entozoa" article, noting that in animals with repeating sex organs, each segment could be considered "generating individuals… [with] various grades of development; some are infants in this respect; others adolescent, the hinder ones fully formed and pregnant."[437]

A second favourite case of British researchers of the 1840s was Bonnet's work with aphids, and their successive generations caused by "virgin births". For these Britons, the importance of Bonnet's findings was that a number of aphids (from seven to eleven waves of progeny) were produced from a single act of sexual fertilization. This succession of generations was a problem revolving around individuality, particularly as

[435]R. Owen, "Entozoa," in *Cyclopaedia* (1839), 111-143, pp. 137-139.
[436]Jones, *Outline of the Animal Kingdom*, pp. 89-91.
[437]R. Owen, Manuscript, "Lectures on the Comparative Anatomy and Physiology of the Invertebrate Animals (Annotated)", 1843, OCOLL, NHM.

each wave was produced from parents "which seem to possess the structure of females imperfectly developed".[438]



On the left: successive forms of aphids in metagenesis. On the right: the "fine nucleated cells" which descend from the ovarian oviducts of a viviparous (asexual) Aphis, and which were histologically identical to the sexually produced egg. A. Thomson, "Ovum" part 2 (1854), *The Cyclopaedia of Anatomy and Physiology*, edited by R.B. Todd, 1-142 (London: Sherwood, Gilbert and Piper, 1852-1856), p. 33.

In his 1840 lectures on reproduction, Owen pointed out that Bonnet's observations were an established fact, tested by Lyonnet and others. And the transition of the "fecundating influence" over numerous generations had already been shown in lower crustaceans.[439]

Aphids exemplified "intervening generations" between acts of sexual fertilization, notably in the work of J.J. Steenstrup (who will be examined in more detail below).[440] In the same year, George Newport thought it worthwhile to test Bonnet's observations – and

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[438]Thomson, "Ovum," p. 34. They were imperfectly developed in that they lacked wings.
[439]Owen tried to see the problem of pre-existence (which he denied) from Bonnet's view. What other option would he have - lacking the Victorians' sophisticated microscopes – than to conclude that the germs of the nine generations lay within each other? Owen, "Hunterian Lectures on Generation".
[440]Review of *On the Alternation of Generations* by J. J. Steenstrup, *M-CR* 45 (n.s.), 22-34 (1846): pp. 23-24.

though Bonnet had demonstrated the oddities of aphid-generation in the previous century the Linnean Society found this problem of sufficient interest to justify publishing Newport's findings. Newport repeated Bonnet's observations, carefully detailing the appearance of rose-aphids from parents, confirming that at certain points aphids produced true "ova", and at other points produced living, "viviparous," young.[441]

Another favourite case from the previous century was Trembley's mutilation of the *Hydra.* Even in 1858 the writer G.H. Lewes (and close friend of Herbert Spencer) recounted that Trembley's illustrations were so accurate, little could be added:

> He taught us that the Polype, which originally comes from an egg, produces a quantity of other Polypes, exactly similar to itself, by a process of "budding," after the manner of a plant. He taught us, moreover, that not only is this the normal mode of multiplication, but that if we lacerate the Polype, each lacerated fragment will become a new Polype, which in its turn may be cut into several pieces, every one of them developing into perfect Polypes. Several naturalists have repeated and confirmed his experiments.[442]

Hydra's multiplication was frequently likened to a plant's. Lewes was following a long line of previous Britons citing Trembley's work: in 1837 and again in 1840, Owen remarked that Trembley's "marvellous" experiments showed just how much the polyp's body could be divided into parts which then lived, grew into complete animals, and propagated.[443] Thomas Rymer Jones in turn noted that Trembley was the "enthusiastic discoverer" of the astonishing tenacity of life and "power of reproduction" of *Hydra viridis* and *Hydra fusca*, noting its power of multiplication by mechanical division.[444]

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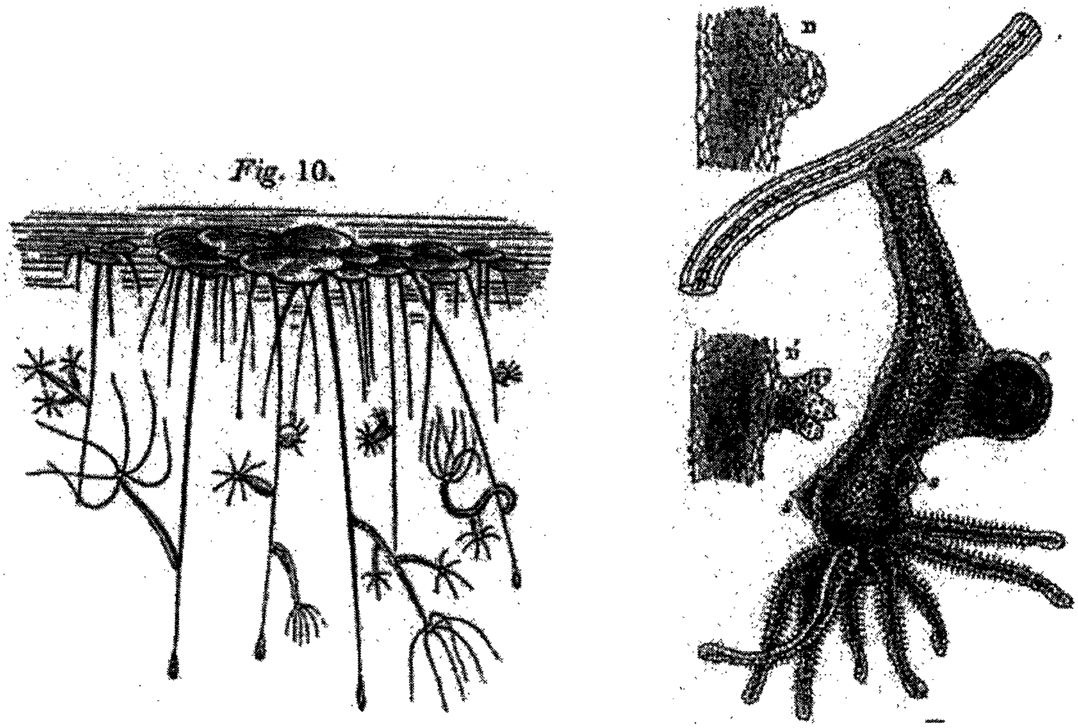
[441]Newport, "Generation of Aphides," pp. 281-283. The paper was read in 1846, but the Linnean Society – almost as slow as the Ray Society at publishing – only printed it in 1851.
[442]G. H. Lewes, *Sea-Side Studies at Ilfracombe, Tenby, the Scilly Isles, & Jersey*, 2nd ed. (Edinburgh: W. Blackwood and Sons, 1860), p. 299. This is a detailed study of the mysteries of invertebrate reproduction.
[443]Owen, "Hunterian Lectures on Generation"; idem, *Hunterian Lectures in Comparative Anatomy, 1837*, p. 214.
[444]Jones, *Outline of the Animal Kingdom*, pp. 22-26.



On the left: a group of *Hydra viridis*. On the right: a magnified *Hydra*. D and D1 are depictions of buds sprouting; C (on the right side of the side of the *Hydra*) is an ovum. A. Thomson, "Ovum" part 1 (1852), p. 17.

Scottish researcher Sir John Dalyell recounted how he obtained 22 *hydrae* in 550 days from a single specimen of *Tubularia indivisa*, and a reviewer duly noted the resemblance of Dalyell's work to Trembley's experiments on *Hydra*.[445]

Another exemplar organism was the earthworm, mutilated by Bonnet and Antoine Dugès. Rymer Jones mentioned Dugès's work on the earthworm; Henri Milne Edwards, in an English-language article for the *Cyclopaedia of Anatomy and Physiology*, used this same example (and cited Bonnet and Dugès's work too) to explain the principle of repetition of parts – earthworms could be cut into two and develop into two new organisms because each body segment was almost an exact repetition of the rest.[446]

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[445]Review of *Rare and Remarkable Animals of Scotland* by Sir John Graham Dalyell, *Annals and Magazine of Natural History* 1 (2nd series), 132-139, 311-315 (1848): p. 134; J. G. Dalyell, *Rare and Remarkable Animals of Scotland*, 2 vols. (London: J. Van Voorst, 1847), pp. 1:36-37.
[446]Milne Edwards, "Annelida," pp. 172-173.

Owen, in his 1843 *Lectures on Invertebrates,* interpreted similar decapitation experiments upon earthworms, performed by Bonnet, as a way to demonstrate the earthworm's "vegetative power." He pointed out that Bonnet only succeeded in wearing out its vegetative power after eight decapitations. This example was an important one to Owen, for in his own annotated copy of his lectures he bracketed the passage dealing with Bonnet's investigations, placing two exclamation marks next to it and noting that Dalyell had propagated a *Sabella* "just like Bonnet".[447]

William Baly's translation of Johannes Müller's *Principles of Physiology* also pointed out Dugès's mutilations (though this time on planarians) – with Müller using them to show the incredible regenerative power of simpler animals. He explained these mutilations in the context of compound individuality, concluding that the power to regenerate was greater wherever the animal was simpler and younger. Since simpler animals were merely the sum of similar parts, these separated parts contained the essential parts of any integral being. As further evidence for this regenerative power, Müller cited Bonnet's mutilation experiments on *Nais*, Moquin-Tandon's experiments upon leeches, Dugès's cutting of planarians into pieces, and – once again – Trembley's experiments upon *Hydra*.[448] In other words, Müller's similar parts were organic elements. Roget's *Bridgewater Treatise* also used these exemplar animals to discuss "regeneration", pointing out that *Hydra* and planarians had the power of full or partial regeneration by new growth, "which is very analogous to that of complete reproduction." He also said that starfish, sea anemones and annelids had the same power[449] – indeed,

[447]Owen, "Lectures on Invertebrates (Annotated)".
[448]Müller, *Elements of Physiology*, pp. 1:401-404, 1:18-19.
[449]Roget, *Animal and Vegetable Physiology*, pp. 2:586-587.

another example of an exemplar organism was *Actinia,* a sea anemone, which rose in popularity and familiarity with the marine aquarium craze of the 1850s.

"Reproduction" was frequently used interchangeably with "regeneration". Planarians were able to form new complete planarians after the bisection of one into two because they had high "reproductive" power. Tadpoles were better able than frogs at "reproducing" lost body parts. In his "On the Reproduction of Lost Parts in Myriapoda and Insecta," George Newport detailed how antennae and legs grew back after he cut them off in myriad ways. Newport wanted to relate this reproduction of parts to the "laws of nutrition and development,"[450] and the interchangeability of these terms suggests that regeneration and reproduction were seen to be coextensive, or part of nutrition and development.

Another group of animals exemplifying the problem of regeneration and reproduction were the "Naids", marine annelids such as *Nais* and *Nereis*. For various naturalists in the 1840s, they represented the strange ability of certain animals to reproduce through budding at a certain point near their tail. Allen Thomson wrote in 1839 that *Nais* and *Nereis* exhibited "fissiparous generation", the division of the animal body into subordinate bodies, each of which had independence. In *Nais* and *Nereis* especially, the small portion separated from its tail became a new animal, meaning that the *Nais* tail was "*gifted with perpetual life.*"[451] At around the same time (1838-1841) Thomas Rymer Jones repeated an extremely similar statement: "The tail of the original *Nereis* is still the tail of its offspring, and, however often the body may divide, still the

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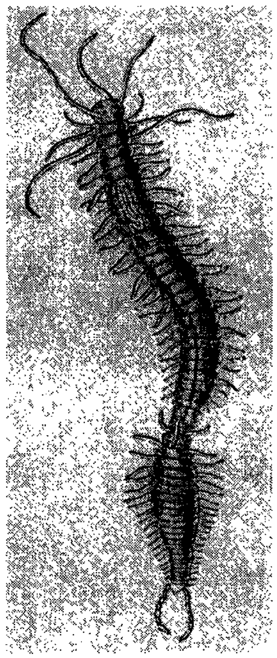
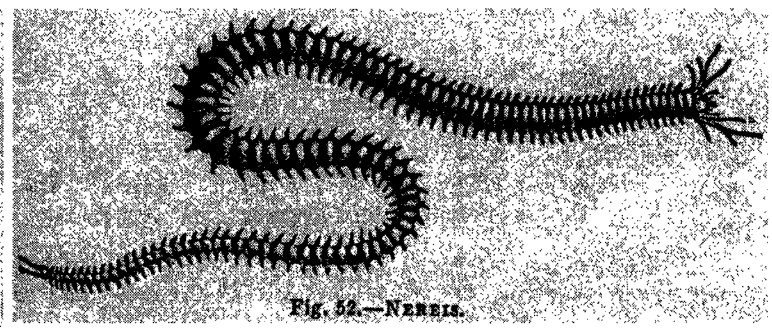
[450]G. Newport, "On the Reproduction of Lost Parts in Myriapoda and Insecta," *Phil. Trans.* 134, 283-294 (1844): p. 283.

[451]Thomson, "Generation," p. 432. My emphasis.

same tail remains attached to the hinder portion, so that this part of the animal may be said to enjoy a kind of *immunity from death.*"[452]



Nereis, in T.R. Jones, *A General Outline of the Animal Kingdom,* p. 221.



Nereis again, in W.B. Carpenter, *Animal Physiology*, p. 110.

The choice of the same animal, and the very similarity of these statements, shows the presence of a common habit.

In 1837 Richard Owen noted that in lower articulates such as the Naids - in which each joint contained the essential organs of the whole - a segment might be separated and still live as an individual. The separation of a Naid joint was an example of fissiparous generation, or budding. In 1843 Owen again mentioned the fissiparous generation of Naids, and how it allowed them to regenerate parts of their bodies that were bitten off.[453] Again, fissiparous generation was linked to regeneration - one of Newport's reviewers

[452]Jones, *Outline of the Animal Kingdom*, p. 221. My emphasis again. It is difficult to determine precisely which researcher wrote first, because Jones's work came out in a serialized format.
[453]Owen, "Lectures on Invertebrates (Annotated)"; idem, *Hunterian Lectures in Comparative Anatomy, 1837*, p. 214.

pointed out that regenerative power was greater in animals which were still budding new segments.[454]

Cases involving exemplar organisms had a life of their own too: when they were rejected, others came to their defence. Thomas Williams, in writing the 1852 BAAS summary on annelids, criticized Owen for repeating Bonnet's observation that *Nais* reproduced new individuals through fission and gemmation. He quoted Owen - because the subsequent generations did not separate, they could be thought of as "'forming one compound individual'". But Williams denied that this compound individuality could occur: in hundreds of his own observations, he had never seen them fail to separate.[455] Later, G.H. Lewes noted Williams's denial of Bonnet's "fables" about *Nais* regeneration, and offered his own experimental results in response. Lewes cut some Naids in two, threw their heads away, and placed the posterior portions in glass vessels, which re-grew the heads. Only after five decapitations did regeneration fail. Lewes noted that "reproduction" occurred in other annelids such as *Syllis* and *Myriana* too, and more than one individual could appear in the same body through decapitation. It was

> as if a head were suddenly to be developed out of your lumbar vertebrae, yet still remain attached to the column, and thus produce a double-headed monster, more fantastic than fable.

In some worms the process continued, and sometimes six worms could be seen forming a continuous line with only one tail; eventually separation would occur.[456] With such a display, how could one determine what was an individual and what was a group of individuals?

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[454]"Newport's Researches in Natural History," p. 505.
[455]Williams, "Report on the British Annelida," pp. 246-247.
[456]Lewes, *Sea-Side Studies*, pp. 62-66.

Another exemplar animal becoming prominent in the 1840s was the salp, an animal related to sea squirts but which stayed mobile through its entire existence. Salps were to become an exemplar animal embodying two different problems. On the one hand they represented the analysts' fascination with compound individuality. In 1839 salps were given as an example of "associated animals" – they had been seen joined up "for mutual support and protection from injury," forming a hollow cylinder up to 14 inches long with an opening in either end. In their association, they were deemed similar to plants. But a "smart shock" to the container in which they swam separated this compound organism into separate individual salps. These salps presented "the association of a number of single and independent beings to form a compound animal."[457]

But salps become more prominent in naturalists' literature because they illustrated the problem of the "alternation of generations." In 1819, the poet/naturalist Adelbert von Chamisso claimed that solitary salps produced chains of associated salps which then produced solitary salps again, giving this process the term "generationweschel".[458] In 1828, Henri Milne Edwards claimed that Ascidians (sea squirts) did the same thing; in 1835 Otto Sars, then Sven Lovén, Lister, and John Dalyell repeated the view that generations alternated in certain marine invertebrates. In 1842 salps figured prominently in the Danish naturalist J.J. Steenstrup's account, *Om forplantning og udvikling gjennem vexlende generationsraekker*. This book was quickly translated into German and then appeared in English in 1845 as *On the Alternation of Generations.* By contemporary

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[457]Carpenter, *Principles of Physiology*, pp. 100-101.
[458]Winsor, *Starfish, Jellyfish, and the Order of Life*, pp. 53-54.

accounts it caused a furore lasting until at least 1859; it was reported that in Britain, Owen, Huxley, Allman and Forbes all followed Steenstrup's work extremely closely.[459]

Steenstrup recounted Chamisso's observation that solitary salps produced associated ones, and that in turn each associated salp was the parent of a solitary one. His explanation of an "alternation of generations" meant that the entire species was not merely represented by a solitary, full-grown individual, but also by "supplementary individuals". These supplementary individuals, or "nurses", helped to bring about the perfection of the solitary individuals, in a teleological vision of the completion of a species. Against Frederick Churchill, who has carefully presented Steenstrup as accentuating the primacy of sexual reproduction, this dissertation instead presents Steenstrup's work in the context of the disunity of the organism. For the alternation of generations could be used against morphological problems too: Steenstrup wanted to convey something more than a simple metamorphosis, because metamorphosis depicted a change of form in a single individual only.[460] While Steenstrup focused on the process of salp-generation, he nonetheless took an analytical view of the matter, referring to a species as a composite of its different generations. He even used the word "species" to refer to this grouping of generations: in other words, these generations succeeded one another in a series for the "complete development of the species."[461]

But the ambiguity in Steenstrup's attempt to convey both 'pattern' and 'process' meant a strange reception of his work in Britain. Where salps were formerly depicted as

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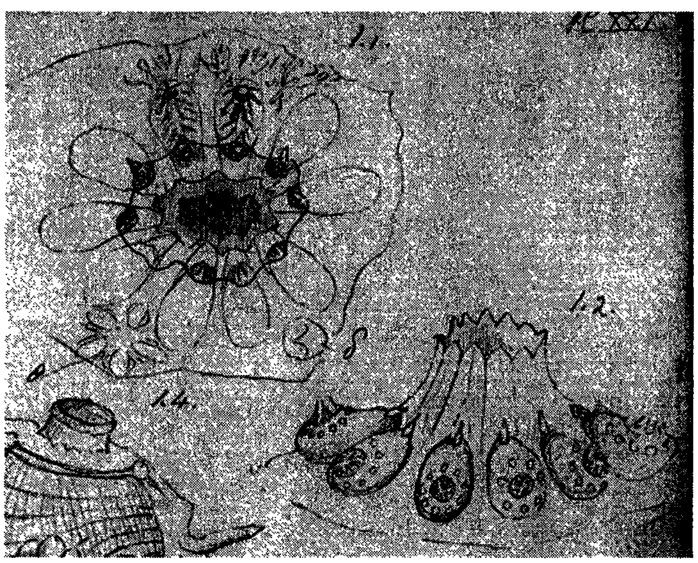
[459]John Lubbock, "On the Ova and Pseudova of Insects," *Phil. Trans.* 1859, p. 341-369, p. 341. Steenstrup has been given too much credit for beginning the debate over reproductive patterns in lower invertebrates. T.H. Huxley actually gave Chamisso priority over Steenstrup, which was noticed by contemporaries: Lewes, *Sea-Side Studies*, pp. 287-289.
[460]F. B. Churchill, "Sex and the Single Organism: Biological Theories of Sexuality in the mid-19th Century," *Studies in History of Biology* 3, 139-177 (1979): p. 145; Steenstrup, *On the Alternation of Generations*, pp. 38-39, 106-108, 1, 6.
[461]Steenstrup, *On the Alternation of Generations*, pp. 3, 36-37.

embodying a *morphological* problem - as the problem of associated organisms produced by solitary ones and vice versa - Steenstrup's use of the term "generations" was to be interpreted by British developmentalists such as William B. Carpenter as confused. They would interpret Steenstrup as discussing a problem not of analysis and synthesis, but of *historical development* instead. This different interpretation is examined in greater detail below.

Owen briefly discussed the "multiparous" and "uniparous" generation of salps. But the problem seems to have been a morphological one for him – he was mainly interested in why a solitary salp gave rise to a "social chain" of young salps, which in turn gave birth to a solitary one again. Following Milne Edwards, Owen remarked that the case was "strictly analogous" to the generation of compound ascidians (sea squirts) such as *Botryllus.*[462] Shown below, *Botryllus* were another recurring exemplar organism embodying the problem of compound individuality.



Botryllus sea squirts, from T.H. Huxley, *Rattlesnake Notebook*, Huxley Papers, Imperial College London 50.59

[462]Richard Owen, "Hunterian Lectures on the Generation and Development of the Invertebrated Animals - Lecture XIX, Generation of Mollusca. OPAP, RCS, p. 3.

VEGETATIVE REPETITION

Steenstrup suggested a parallel between the alternation of animal generation and plant metamorphosis – feeding the habit of some botanists to see plants as compound organisms.[463] By linking alternating invertebrate generations with plants, Steenstrup also supported the view that invertebrates, like plants, were compound organisms. John Farley has nicely summarized the reception of Steenstrup's work.

> An extraordinarily lengthy and complex discussion followed on the relationship between Steenstrup's alternating generations and animal and plant metamorphosis, all of which centered on the problem of individuality. Are a tree and a hydrozoan polyp individuals or a colony of individuals? If a plant is a colony, what is the nature of the individuals of which it is constructed? Is the plant individual the cell or the shoot? Are larval stages of insects true individuals and thus equivalent to Steenstrup's nurses and plant shoots?[464]

However, the comparison between invertebrate (plant, insect, hydroid) and plant seems to have occurred in Britain for some time before Steenstrup's work appeared. Perhaps this is a comparison going back to Aristotle, giving meaning to the term *zoophyte,* or 'animal-plant'. Researchers noted that compound individuality – associated with "gemmiparous reproduction" - was associated with lower animals. After all, Milne Edwards had moved Tunicates (which included sea squirts such as *Botryllus*) from the mollusc group down into the lower zoophyte group because certain tunicates propagated gemmiparously, just like plants.[465]

The sertularian polyp was frequently invoked to show the similarity between a plant and lower invertebrate. On its 'branches' were multiple polyps, and each was thought to be capable of carrying on an independent existence, like the branches of a tree.

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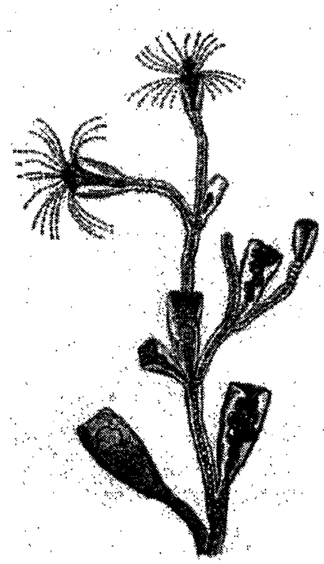
[463] J. Farley, *Gametes and Spores : Ideas about Sexual Reproduction, 1750-1914* (Baltimore: Johns Hopkins University Press, 1982), pp. 78-80; Steenstrup, *On the Alternation of Generations*, p. 115.
[464] Farley, *Gametes and Spores*, p. 80.
[465] Carpenter, "Development of Zoophytes," p. 195.



"Branch" of *Sertularia geniculata*, in A. Thomson, "Ovum" part 1 (1852), p. 17.

Thus in 1833 the botanist John Lindley announced that "a tree is more analogous to a Polype than to a simple animal; that it is a congeries of vital systems, acting indeed in concert, but to a great degree independent of each other, and that it has myriads of seats of life".[466] In 1834 Peter Mark Roget noted how Erasmus Darwin and other writers on vegetable physiology saw "each annual shoot as a collection of individual buds, each bud being a distinct individual plant, and the whole tree an aggregation of such individuals." In 1839 Allen Thomson compared the ability to propagate by division with trees' ability to reproduce by "slips" which stayed attached to the common stem and then separated.[467] He repeated the example when he asserted that the multiplication of individual polyps resembled the addition of leaves and branches to a tree. Dalyell spoke of polyp heads "vegetating" from the stem.[468] In 1837, Owen noted that John Hunter classified animals

[466]John Lindley, *On the Principal Questions at Present Debated in the Philosophy of Botany* (London: Richard Taylor, 1833), p. 32. I thank Sara Scharf for alerting me to this important passage.
[467]Roget, *Animal and Vegetable Physiology*, p. 1:89; Thomson, "Generation," pp. 424-425.
[468]Dalyell, *Rare and Remarkable Animals*, pp. 1:5-7; Thomson, "Ovum," p. 2.

by their modes of generation, with one group being animals that propagated by slips, small pieces either dropping or being cut off from the main body.[469]

Indeed, Richard Owen's projects in the 1840s make far more sense if situated in the context of the disunity of the organism. One example is his theory of two opposing morphological forces, summarized in his 1846 "Archetype of the Vertebrate Skeleton": one force was "adaptive," shaping living things to their functions – a teleological force. The other was the "vegetative" or "irrelative repetition" of structure, like the growth of crystals. Owen believed that the internal force of vegetative repetition explained all repeating structures in organisms. In the particular case of the vertebrate archetype, this repetition caused recurring vertebral segments. Owen could rank animals as higher or lower according to how strongly the teleological adaptive force overcame the power of vegetative repetition; as we descended the scale, vegetative repetition prevailed.[470]

But Owen didn't discuss the two opposing forces only in the case of vertebrate morphology: he used them earlier to solve why animal bodies were made of independent parts which nonetheless acted harmoniously. Only later does he seem to have applied the principle of two opposing forces to his vertebrate work. Owen first mentioned the "vegetative principle of development" to explain the repetitive segments of annelids (such as earthworm rings) in his 1843 *Lectures on Invertebrates.* In an 1845 paper on molluscs he referred to his law of "uncoordinated repetition",[471] which may explain why he occasionally called this developmental principle "irrelative" repetition – because each

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[469]Owen, "Preface," p. 4:xxviii. The other classes of animals included *Vivipara* (like Linnaeus's mammals), *Ovovivipara* (eggs hatched in the body), and *Ovipara* (eggs hatched outside the body).
[470]R. Owen, "Report on the Archetype and Homologies of the Vertebrate Skeleton," *BAAS*, 169-340 (1847): p. 339.
[471]Owen, *Lectures on Invertebrates*, pp. 129-130; idem, "Lettre sur l'Appareil de la Circulation chez les Molluscques de la Classe des Brachiopodes, Adressée à M. Milne Edwards," *Annales des Sciences Naturelles* 2 (3rd series), 315-320 (1845): p. 315. In French Owen calls it "la *loi des répétitions non coordonnées",* emphasis in original.

repeating component developed without regard to the entire organism of which it was a part. It was only by 1846 that he used the law of vegetative repetition ("which is so much more conspicuously manifested by the segments of the exoskeleton of the invertebrata") to explain vertebrate special, general and serial homology.[472] Owen's explanation also closely resembles George Newport's proposal of two sorts of development, growth through expansion (budding) and concentration through anchylosis (coalescence).

In an 1848 letter to his Oxford friend, the Reverend Daniel Conybeare, Owen again used vegetative repetition to explain invertebrate structure. He tellingly used the exemplar of a tapeworm and its repetitive joints: with its 500 segments, the worm could suffer immense mutilations without being the worse as an "individual worm". No "individual joint" was so important to the whole worm that its removal would harm the entire organism. But as the worm rose in the scale and the adaptive force became more prominent, these joints lost their individuality. As a further illustration Owen told "my dear Dean" to compare it to a musical instrument: vegetative repetition was like the keys of an organ, if each key struck the same note. As the adaptive force increased in power, however, each key gradually took on a different note.[473]

Owen used the tapeworm to depict his two competing forces because they were one solution to the problem of compound individuality. Also recall his 1840 question why an organism didn't simply form a "mass of infusories", a heap of autonomous cells instead of a harmonious structure. Like his thoughts on the cell-theory, the adaptive force was an integrating force that overcame the potential independence of each simpler element, be that element a segment, cell, physiological system or other component of the

[472]Owen, "Report on the Archetype," p. 339.
[473]R. Owen to W.D. Conybeare (Draft), 13 Mar 1848, OPAP, RCS.

entire organism. Owen is often portrayed as a transcendentalist, an adherent of German *naturphilosophie* who, by seeing in each plant a colony of individuals, followed Goethean plant morphology.[474] Situating Owen in this philosophical school may supplement my discussion of compound individuality, but it is unclear that thoughts about compound individuality were specific only to *naturphilosophie.* E.S. Forbes, for instance, declared that Linnaeus was the first to point out that a plant was a group of individuals, a statement taken up by Wolff and only then by Goethe.[475] Additionally we ought to move away from the historiographic tradition of subsuming Owen within a particular philosophical or 'romantic' tradition; this work has already been done well by others. It is now time to set Owen in a specific context of a particular community of researchers who were interested in concrete problems.

Owen's contemporaries were also interested in the problem of reproduction and regeneration. Roget had observed in his 1834 *Bridgewater Treatise* that agriculturalists were well aware that even the smallest fragments of a plant could multiply from separated parts: each fragment, or "slip," therefore possessed an inherent power of reproduction that could form a complete individual plant. In both 1844 and 1845 George Newport, as President of the Entomological Society, pointed to the problem of the "reproduction" of missing insect and myriapod limbs as a subject of great importance and interest, because the solution to this problem would reveal important laws of nature.[476] In the same period Richard Owen discussed the problem of the reproduction of articulate

[474]Churchill, "Sex and the Single Organism," pp. 145, 150.
[475]"Edward Forbes's Royal Institution Lecture, on some Important Analogies between the Animal and Vegetable Kingdom," *Annals and Magazine of Natural History* 15, 210-212 (1845): p. 210.
[476]G. Newport, *An Address delivered at the Anniversary Meeting of the Entomological Society of London 22 Jan 1844* (London: Richard and John E. Taylor, 1844), p. 5; idem, *An Address delivered at the Adjourned Anniversary Meeting of the Entomological Society of London 10 Feb 1845* (London: Richard and John E. Taylor, 1845), pp. 12-13; Roget, *Animal and Vegetable Physiology*, p. 2:585.

(spider) limbs with a Welsh naturalist; though Owen's correspondent saw the limbs grow back after he cut them off, he had no idea why they grew back.[477]

Indeed Owen himself pointed out that in certain cases - like polypes propagated by cuttings – the emergence of new individuals resembled the grafts of an apple tree.[478] Where Nicholaas Rupke has argued that Owen chose the phrase "vegetative repetition" because it seemed similar to the repetition of leaves on a branch,[479] we can push Rupke's observation even farther. Owen used the term "vegetative" because he saw it as the same sort of power that plants used to regenerate and propagate by "slips." Simpler animals exhibiting vegetative repetition of parts were like plants, and both were compound organisms.

E.S. Forbes noted that animals tended to concentrate their essential parts, while vegetables tended to extend and elongate these essential parts to reproduce their species – thus certain animals assumed "vegetable" characteristics. These vegetable characteristics could especially be found in Articulata. In another talk Forbes suggested a likeness between simple organisms and plants, likening the "composite beings" of sertularian zoophytes to plants, the reproductive vesicles of these animals like flowers. Other naturalists, including Owen (who was at Forbe's talk) spoke highly of Forbes's proposal

[477] J. Blackwall, "Researches having for their Object the Elucidation of Certain Phaenomena in the Physiology of the *Araneidea*," *Annals and Magazine of Natural History* 1 (2nd series), 173-180 (1848): pp. 173-174, 176-177. For Owen's link with Blackwall, see J. W. Gruber and J. C. Thackray, *Richard Owen Commemoration : Three Studies* (London: Natural History Museum Publications, 1992), pp. 36-37. Owen was also acquainted with Harry Goodsir, brother of John Goodsir, and Conservator of the Edinburgh Royal College of Surgeons, who did similar regeneration experiments on crustaceans. His work was enthusiastically recommended by Newport in his 1845 Address to the Entomological Society. Newport, *An Address delivered at the Adjourned Anniversary Meeting of the Entomological Society of London 10 Feb 1845*, pp. 12-13. For an example of Goodsir's regeneration research, see his "The mode of reproduction of lost parts in the crustacea," in John Goodsir and Harry Goodsir, *Anatomical and Pathological Observations* (Edinburgh, Myles MacPhail, 1845), pp. 74-78. Sadly, however, Goodsir was naturalist on the H.M.S. *Erebus,* which went north on the ill-fated Franklin Expedition, and as a result no further Goodsir articles would appear. There is a portrait of Harry Goodsir (sketched by E.S. Forbes) in the Owen Papers.
[478] R. Owen, Manuscript, "Definitions from Museum Lectures on the Animal Kingdom", ND, OPAP, RCS.
[479] Rupke, *Richard Owen*, pp. 172, 197.

and thought his analogy would help them understand the simpler polypes.[480] A later writer noted how the whole sertularian zoophyte seemed to be but "one compound creature, derived from the same source...In this respect they resemble trees; each branch is independent of all the others and may be cut off without injury to the whole...".[481] The constant comparison of lower invertebrates to trees shows that in the 1830s and 1840s, trees exemplified compound individuality.

PARTHENOGENESIS AND METAGENESIS

Owen's notion of vegetative repetition was thus strongly linked to his 1849 proposal of "parthenogenesis," in the eponymous book (subtitled *or, the Successive Production of Procreating Individuals from a Single Ovum).* Like vegetative repetition, Owen's parthenogenesis should be seen as a device that was proposed to solve problems of the disunity of the organism.

Owen arrived at parthenogenesis after discussing how aphids multiplied in his 1843 *Lectures on Invertebrates.*[482] He defined parthenogenesis as occurring

> In proportion to the number of generations of germ-cells, with the concomitant dilution of the spermatic force, and in the ratio of the degree and extent of the conversion of these cells into the tissues and organs of the animal is the perfection of the individual, and the diminution of its power of propagating without the reception of fresh spermatic force.[483]

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[480]"Edward Forbes's Royal Institution Lecture," pp. 211-212; E. Forbes, "On the Morphology of the Reproductive System of Sertularian Zoophytes, and its Analogy with that of Flowering Plants," *BAAS*, 68-69 (1845): pp. 68-69. For Owen's presence at that meeting see E. Forbes, "On the Morphology of the Reproductive System of Sertularian Zoophytes, and its Analogy with that of Flowering Plants," *Athenaeum* 978, 977-978 (1844).
[481]R. Q. Couch, "On the Morphology of the Different Organs of Zoophytes," *Annals and Magazine of Natural History* 15, 161-166 (1845): pp. 161-162.
[482]Thomson, "Ovum," p. 37.
[483]Owen, *On Parthenogenesis*, p. 69.

Owen proposed that a particular amount of "spermatic force" was bestowed upon an egg during sex. Spermatic force could be converted into tissues: but like any sum of money or energy it was used up, 'diluted,' in constructing them. Higher organisms had more complex tissues, using up greater spermatic force. Since they used more force, sexual reproduction had to occur each generation, as if to recharge it. Conversely, simpler organisms had fewer complex tissues, so spermatic force not only produced tissues in a single individual, but could actually be carried over into the next individual. It was like credit used up in each transaction, whether growth or the production of another individual.

Owen used spermatic force to explain vegetative repetition while reformulating an implicit *scala naturae*. He explicitly linked lower animals with vegetables, and through this association he also portrayed lower animals showing vegetative repetition as compound organisms. To support the view that a plant was an "associated colony of simple organized individuals", Owen quoted John Hunter, Steenstrup and Edward Forbes; he inveighed against the "common" notion that only the entire plant was a single individual. He discussed how composite plant or composite zoophyte could be seen as a tree – the leaves as parts of a whole, the polypes as digestive organs of a compound organism. But these parts were also themselves individuals, just as the "members of a regiment or a corporation constitute one organized whole."[484]

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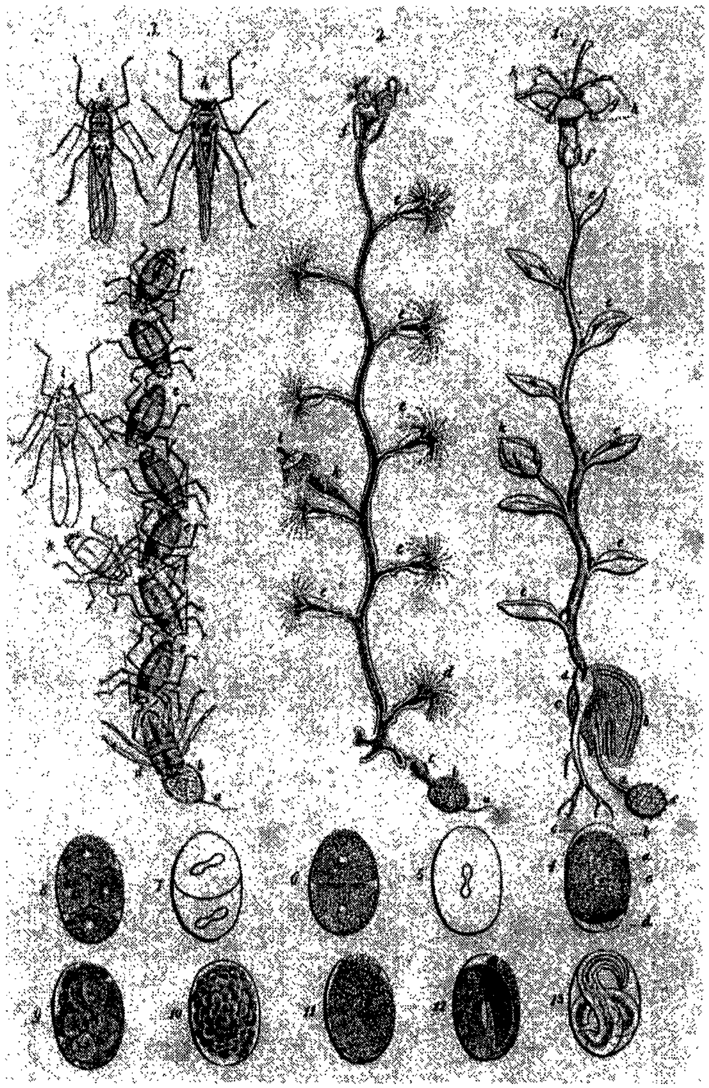
[484]Owen, *On Parthenogenesis*, pp. 56-57, 61-62.



"[In the Aphid] (fig. 3)...we have, in fact, at length 'male (h) and female (i) individuals', preceded by reproductive individuals (e, e) of a lower or arrested grade of organization, analogous to the gemmiparous polypes of the zoophyte (e, e, fig. 2) and the leaves (e, e, fig. 1) of the plant." Figures 4-13 depict the development of the germ-cell into secondary ones, forming the germ-mass. R. Owen, *On Parthenogenesis* (London: J. Van Voorst, 1849), frontispiece, pp. 59-60.

This picture shows the link, obvious to Owen, between aphid, polyp, and plant. He also drew attention to the common 'tree-like' shape of the three groups of individuals by arranging the independent aphids in a tree-like way.

Other exemplar organisms recurred in Owen's account. *Hydra*, the Naids, annelids and lobsters appeared, and just as these organisms exemplified particular research problems, he used these same exemplars to show how spermatic force solved them. In the case of regeneration, Owen noted that *Hydra* could be cut into many pieces which themselves turned into a number of new *Hydrae* because of the *Hydra's* simple structure, which was inversely proportional to the amount of spermatic force in their body. Lobsters, structurally more complex than *Hydra* but less so than other animals, couldn't regenerate as much as *Hydra* but could still grow a new claw. The fact that the new claw could only be grown from the basal end of the first joint of each leg implied that spermatic force existed only here. Likewise, spermatic force explained the "virgin births" of aphids, because a portion of the unchanged germ-mass was retained in the following generation. Parasites such as fluke-worms - which alternated between detaching alive from the parent and hatching from eggs - reproduced through parthenogenesis.[485] Budding and gemmation, like regeneration, could be seen as instances of parthenogenesis - because the spermatic force was retained in certain body parts. In Nais and other annelids, for example, Owen thought that the spermatic force was mostly located between the last and penultimate segments where the new animals budded.[486]

Various problems - including the reproduction of wounded parts, the correlation between structural simplicity and ability to regenerate, irrelative or vegetative repetition, serial homology, and the production of new animals without sex - could all be colligated through spermatic force. As a result Owen thought extremely highly of his doctrine and told others that he thought highly of it.[487] Moreover, not only did it link so many different aspects of compound individuality, but it also localized the spermatic force histologically in what he called "derivative germ cells". Other inquirers could therefore investigate parthenogenesis and the spermatic force with microscopes. Owen saw

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[485]Owen, *On Parthenogenesis*, pp. 72-73, 48-49, 15-16. Owen pointed out that Harry Goodsir had discovered how the regenerative power of lobsters was localized.
[486]Owen, *On Parthenogenesis*, p. 7.
[487]C. Darwin to T.H. Huxley [Apr?][1858?], HP, IC, 5.58-5.60.

himself as proposing more than a mere verbal force – after all, he criticized Steenstrup's own description of certain generations as "nurses" as lacking explanatory power.[488]

Derivative germ cells were in more places than in the reproductive system, and these "nucleated" cells could be found wherever parts reproduced or regenerated. Owen noted that Trembley's *Hydra* possessed three layers of tissue: on the outside was one layer of cells - the integument - and on the inside was another layer, forming its digestive system. But *Hydra's* middle layer had a large proportion of "retained and unaltered" nucleated cells and nuclei, identical with the "progeny of the primary impregnated germ-cell". These retained nucleated cells were capable of spontaneous fission, each one able to become a new polype. He explained that *Hydra* could reproduce after mutilations because it had "derivative germ-cells" identical to impregnated cells spread throughout its body.[489] The best way to think about Owen's vague proposal of "nucleated" cells is to think of these cells as a sort of organic element, another version of Milne Edwards's "workshop" which could live independently after separation. My suggestion that Owen saw "nucleated" cells as organic elements further suggests why he thought so highly of his explanation of parthenogenesis and spermatic force – because it solved all of the problems raised by the habit of analysis and synthesis.

How was Owen's explanation taken up? Owen appears to have sent copies of *On Parthenogenesis* to Continental researchers, including Julius Victor Carus, Henri de Blainville, Arnand de Quatrefages, Milne Edwards, Frederic Cuvier, C.G. Ehrenberg and Johannes Müller; he also sent copies to British institutions such as the College of Surgeons and the Athenaeum; and he sent copies to individual British researchers such as

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488 Churchill, "Sex and the Single Organism," pp. 147-149.
489 These derivative germ-cells weren't everywhere in *Hydra* – its tentacles couldn't reproduce anything but other tentacles. Owen, *On Parthenogenesis*, pp. 7, 48-49.

Rymer Jones, John Lindley, W.B. Carpenter, and Charles Darwin. W.S. MacLeay, now in Sydney, received a copy, and Owen even sent a copy to Prime Minister Peel.[490] E.S. Forbes "listened with delight" to Owen, and made various suggestions. Had he looked at the work of the "British Spallanzani", John Dalyell? At John Reid's work on Medusae development, or James D. Dana's work on zoophytes? Forbes also hinted that Owen should also look at his own recent book on gemmation in the naked-eyed medusae.[491]

Others responded favourably to Owen's insistence that the spermatic force could be found in "derivative germ-cells" which were "nucleated." Allen Thomson noted that the simplest animals, the infusoria, could divide easily because they were little more than "nucleated cells" – just like the middle layer of a *Hydra*'s wall. A few years later, G.H. Lewes proposed that Owen was the first to advance a solution to the "alternation of generations" that was not merely a verbalism, because of his claim that the spermatic force was situated in particular cells and could thus be found.[492]

But there were complaints too: Thomson nonetheless grumbled that the term "nucleated cells" to refer to the "derivative germ-cells" was not very clear, as no one yet had a clear definition of a nucleated cell. Thomson also refused to use the term "parthenogenesis" because it implied its occurrence only in females.[493] In response to statements such as Thomson's, Owen began shifting terminology over the next several years. As a way to shift the term's connotations, he began to call the process "metagenesis" instead of "parthenogenesis". He wrote that "partheno-" was Greek for

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[490]R. Owen, Manuscript, "On Parthenogenesis (Annotated)", OCOLL, NHM 18. I have inferred this because Owen's manuscript contains a list of people with check-marks next to their names.
[491]E. Forbes to R. Owen, [1848], OCORR, NHM, 12/320-323.
[492]Lewes, *Sea-Side Studies;* Thomson, "Ovum," p. 37.
[493]Thomson, "Ovum," p. 38.

"the virgin-state," depicting purity and chastity, and privately complained that other researchers were therefore applying the term only to females.[494]

Where "metamorphosis" described the change of different forms in a single individual, the term "metagenesis" portrayed the succession of different forms and individuals. Metagenesis applied to compound organisms too: if we cut a compound zoophyte into pieces, "setting each individual free," or detached one of the "monadiform individuals" of *Volvox globator,* or the leaf-bud, these individuals would often live and continue to reproduce gemmiparously. Metagenesis better conveyed the problem of compound individuality, and Owen again pointed out the importance of the problem facing them.

> The analogy between the procreating larvae of the *Aphis*, the *Medusa*, and the *Coralline* is so true and so close, that if the larval Aphis be a distinct individual and not a part, so must the strobila, the planula, and the gemmiparous leaf: if the succession of larval Aphides be truly described, as a succession of generations, so must that succession of planula, polype and strobila which leads to the oviparous Medusa: and that succession of planulae and nutritive polypes which precede the detachment of the free procreative medusoid polypes in the Coryne; and the like with the plant generations preceding the flower.[495]

By 1851 Owen was calling the cycle of morphological change in aphids *metagenesis* instead of *parthenogenesis.*

Owen's word-change was duly noted and appreciated. Julius Victor Carus thanked him as it "wonderfully" expressed Steenstrup's term "Generationwechsel." Thomson thought metagenesis was a far more precise term for a type of morphological change: since it depicted the change of form through the production of a new individual it

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[494]Owen, "On Parthenogenesis (Annotated)".
[495]R. Owen, "Professor Owen on Metamorphosis and Metagenesis," *Edinburgh New Philosophical Journal*, 269-278 (1851): pp. 271-272, 276-277.

could now be distinguished from metamorphosis, the change of form in the same individual by growth.[496]

As shown by his change from "parthenogenesis" to "metagenesis," Owen sought to emphasize morphology and the change of forms instead of the process by which these forms emerged. But it was over the process itself where Owen's problems were to come.

PALAETIOLOGY

In 1982, John Farley distinguished between morphologists - who accepted Owen and Steenstrup's definition of "generation" - and physiologically-researchers who did not. Farley noted that sexual and asexual reproduction was part of that physiologically-oriented mindset.[497] The morphologists saw "generations" as a sequence of forms and were unconcerned with whether reproduction was sexual or asexual. Meanwhile, physiologists interpreted "generations" temporally – as a process rooted in the actual act of generation. We can expand on Farley's point by noting that the interpretation of "generations" really depended upon which habit a researcher followed. The people whom Farley calls "morphologists" were habituated to analysis and synthesis; but the people he calls "physiologists" usually focused upon the process itself, taking a *palaetiological* perspective.

The term is William Whewell's: his *History of the Inductive Sciences,* and then his *Philosophy of the Inductive Sciences,* called a number of different researches "palaetiological" because they focused on origins, studying the principles by which

[496] J.V. Carus to R. Owen, 1 Oct 1851, OCORR, NHM, 6/365-366; Thomson, "Ovum," p. 38.
[497] Farley, *Gametes and Spores*, pp. 78-81.

various fields changed. Geology was one such palaetiological field, in which the geologist studied a number of successive changes which were somehow similar.[498] Whewell also mentioned the palaetiological field of comparative philology. Stephen Alter has already noted the links of comparative philology with Darwinian descent with modification; both propose genealogies, following the path of branching descent (or ascent) from a common ancestor.[499]

What differentiated palaetiology from other habits of investigation was its use of temporal explanation. Some sciences discussed causes in terms of force, which always occurred under all circumstances and which produced motion when unopposed. But the palaetiological sciences historically discussed causes in terms of force, setting them as part of a larger temporal sequence. Thus the Alps were produced by a sequence of particular forces occurring only once. Whewell noted that solar system could be interpreted in terms of mechanics – as the workings of different forces; but it could also be interpreted palaetiologically, like the formation of the solar system from a nebula. Palaetiology moved from the present to a "more ancient condition" – but to simply describe such research as a "History" was insufficient, as the goal of palaetiological investigation was to determine what caused these changes. Most importantly, Whewell believed that a temporal sequence meant a movement from a simple to a more complex

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[498]W. Whewell, *History of the Inductive Sciences*, 3rd ed. (London: Cass, 1967), p. 3:399. Whewell's use of geology is discussed extensively in M. J. S. Hodge, "The History of the Earth, Life, and Man: Whewell and Palaetiological Science," in *William Whewell: a Composite Portrait*, ed. M. Fisch and S. Schaffer (Oxford: Oxford University Press, 1991), 255-288.
[499]S. Alter, *Darwinism and the Linguistic Image: Language, Race, and Natural Theology in the Nineteenth Century* (Baltimore: Johns Hopkins University Press, 1999), pp. 2, 13-14.

state: "phenomena at each step become more and more complicated, by involving the results of all that has preceded".[500]

Theodore Merz, who between 1904 and 1912 wrote a comprehensive survey of the sciences, also devoted a chapter to the "genetic view of nature." He set out a number of different examples of the genetic view – Merz proposed that they emerged at the turn of the 19th century and then gradually gained strength.

Geological uniformitarianism (Hutton and Lyell)
Political Economy (Malthus)
Lamarckism
Embryology (von Baer)
Vestiges of the Natural History of Creation (Chambers)
Taxonomy (Darwin)

Merz thought that the publication of Darwin's *Origin of Species* signified the maturation of the genetic orientation.[501] In addition to Whewell's discussion of comparative philology another example of the historical/genetic view might be the appearance of historical biblical criticism, like Strauss's *Life of Jesus,* the immensely controversial book (anonymously translated from German by Herbert Spencer's friend George Eliot in 1846) that sought to examine the veracity of Biblical accounts by reconciling them with historical accounts.[502] Merz noted that different examples of this historical/genetic view reinforced one another: palaeontology and the geological record helped explain the

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[500]Whewell, *History of the Inductive Sciences,* p. 3:399; idem, *The Philosophy of the Inductive Sciences : Founded upon their History,* 2nd (1847) ed. (New York: Johnson Reprints, 1967), pp. 654-655, 637-638. On the importance of this "style," see Hodge, "Whewell and Palaetiological Science."
[501]Merz, *A History of European Thought,* pp. 2:292-337.
[502]Here, Strauss uses "historical" as a term of *praise.* Hence one of his rules is that for an account to be seen as "historically valid", it can't be inconsistent with itself or contradict other accounts; where a Biblical occurrence is presented as miraculous or fits a little too harmoniously with Messianic ideas then the entire occurrence must be seen as "unhistorical." D. F. Strauss, *The Life of Jesus Critically Examined,* ed. P. C. Hodgson, trans. G. Eliot, 4th ed. (Philadelphia: Fortress Press, 1972), pp. 88, 91.

development of species, while embryology and systematic zoology and botany supported each other.[503]

It might be rewarding to investigate the relationship of the palaetiological habit with institutions. If we take seriously the belief that habits of reasoning shaped and were in turn reinforced by new institutions, then it is not unreasonable to infer that in addition to museological institutions (for analysis and comparison) and laboratories (for experiment) there emerged another class of specialized institutions around the same time as Merz's genetic view of nature, either as new institutions or as institutions renovated in accordance with that habit. One interesting possibility is the museum's change of perspective between 1850 and 1900 as it shifted its focus from present to past, transforming from places which surveyed humanity's current culture and knowledge into repositories, "mausoleums" even, of past cultures and knowledge.[504] This historicist shift might have allowed Victorians to understand their current culture - or others' - by understanding how cultures changed.

But more appropriate instances of a palaetiological institutions that strengthened over the course of the 19th century are zoos, aquariums, and botanical gardens, in which living specimens could be observed as they changed over time. The aquarium is the most relevant example for this dissertation. Matthew Goodrum's interesting Ph.D. thesis[505] discusses a shift in British marine natural history from the study of "cabinet" specimens in museums to the study of living organisms. He examines how certain British researchers - mainly in Scotland and northern England - were able to observe living

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[503]Merz, *A History of European Thought*, pp. 2:363-366.

[504]The observation about this change in perspective, and the term "mausoleum", are both taken from Forgan, "The Architecture of Display," p. 140.

[505]M. Goodrum, "The British Sea-Side Studies, 1820-1860: Marine Invertebrates, the Practice of Natural History, and the Depiction of Life in the Sea" (Ph.D. Thesis, Indiana University, 1997).

marine invertebrate specimens by visiting nearby tidal pools. Likewise, Phillip Sloan has shown how much of the ability of Edinburgh researchers to view live invertebrates rested upon the proximity of the rich marine life in the Firth of Forth to the University of Edinburgh.[506] The observations made by these people and by other travelling naturalists became resources for a few researchers, such as W.B. Carpenter. But throughout the 1850s more and more researchers were able to see living specimens of marine invertebrates in a domestic environment because of the emergence of the "aqua-vivarium" (later shortened to "aquarium").

Goodrum aptly calls the aquarium a sort of tidal pool uprooted and brought indoors, facilitating the observation of living marine animals by more and more people. By 1853 a large aquarium opened in the Zoological Gardens in Regent's Park; that same year the British middle class became tremendously enthusiastic about aquaria, to the point that specialized aquarium stores were opening (implying a big enough market for at least some to be commercially viable). By 1855 much of the knowledge about aquaria was starting to be codified in textbooks and other popular works.[507]

Where some see the rise of the aquarium, or microscopy, as part of a way of "disciplining" the working classes into an orderly appreciation of nature, one can make a more prosaic observation. Certain institutions became 'more' palaetiological throughout the 19th century as the ability to keep organisms alive and under observation increased. In another sort of virtuous circle much like this facilitated researchers' ability to observe organisms throughout their entire life cycle, strengthening the prevalent view of animals as changing and dynamic. Thus tools and institutions helped reinforce the palaetiological

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506 Goodrum, "British Sea-Side Studies", pp. 2, 92-93; Sloan, "Darwin's Invertebrate Program," pp. 73-74.
507 Goodrum, "British Sea-Side Studies", pp. 252-256, 278-283. Allen, *The Naturalist in Britain*, pp. 135-137.

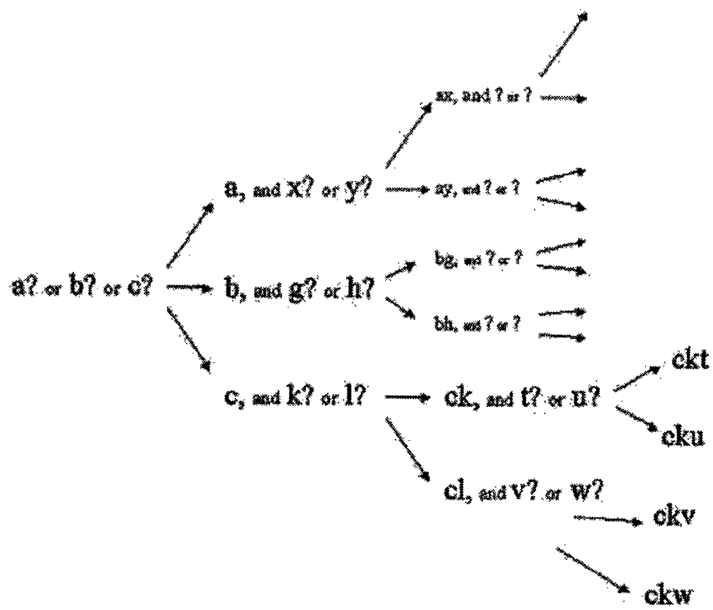
habit: eventually it would get to the point where Anton Dohrn would complain to Charles Darwin about how difficult it was to study embryology without an aquarium.[508] Future research will investigate the relationship between those working in aquaria and zoos and other palaetiological institutions, and what habit of reasoning they followed.

Palaetiological investigations and explanations can be set against analytic and synthetic investigations and explanations. One key difference between the two habits is that in the analytic:synthetic view a researcher decomposes an organization into its simpler elements and then sees the organization as a synthesis of these simpler units, but as nothing more than an association of these simpler units. The habit of analysis and synthesis might be depicted as the following pattern.

Owen's picture of the three similar parthenogenetic 'trees' - aphid-chain, sertularian polyp and plant – resembles my abstraction above.

Meanwhile someone with a palaetiological view sees a system or organization temporally, a process in terms of its history and its future possibilities. The palaetiological view might be depicted as:

[508]Gooday, "'Nature' in the Laboratory." Dohrn was seeking Darwin's *imprimatur* to bolster support for a new Zoological Station; von Baer had already approved and donated some money, he added. C. Groeben, ed., *Charles Darwin (1809-1882) - Anton Dohrn (1840-1909) Correspondence* (Naples: Macchiaroli, 1982), pp. 25-26.



In palaetiology, "by involving the results of all that has preceded", one possibility is taken, or differentiated, from a set of potential possibilities. As a result multiple paths ramify from this set of options. Thus the 'set' (a?)or(b?)or(c?) depicts a range of possibilities for a particular organism; one of these options is taken, which then sets out a new set of future, more specialized possibilities, and so on. The more differentiated and specific the options, the more "individuated" and particular the result. Notice the resemblance of this pattern not to Owen's trees, but instead to Martin Barry's trees, shown on p 171. Robert J. O'Hara has already pointed out the similarity of palaetiological depictions in Darwin's work, in linguistics, and in manuscript transmission – all produce treelike shapes showing ramifying sequences of ancestry and descent.[509]

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[509]R. J. O'Hara, "Mapping the Space of Time: Temporal Representation in the Historical Sciences," in *New Perspectives on the History of Life: Essays on Systematic Biology as Historical Narrative*, ed. M. T. Ghiselin and G. Pinna (Memoirs of the California Academy of Sciences, 1996), 7-17, pp. 7-9. O'Hara's website (http://rjohara.net) should also be noted as an good resource on palaetiological depictions.

This is not to say that the habit of palaetiology was placed entirely in opposition to the habit of analysis and synthesis. In presenting a new map of the period's complex researches in biology and medicine it has been necessary to make abstractions and oversimplify. The two habits could be usefully combined for fruitful work, but only insofar as one habit was placed in the service of the other. Owen, for instance, was not completely habituated to analysis and synthesis; he used palaetiology to clarify analytic points, like distinguishing between the identity or mere resemblance of two body parts. For instance, Owen used von Baer's embryology to distinguish between homology and mere analogy;[510] von Baerian principles meant that a person could find homologies only between adults of the same *embranchement.*[511] Indeed, it was his claim to be first interpreter and British owner of von Baer's embryology that was to land him in trouble with a researcher who also claimed "property rights" over these principles.

WILLIAM B. CARPENTER

Owen thought he had chosen well in sending a copy of *On Parthenogenesis* to W.B. Carpenter. We have seen how Carpenter sought Owen's attention as he sought to carve out his own London scientific career. Already seen as a researcher of the first rank, and as an author of a well-regarded physiology textbook, Carpenter patiently cultivated

[510]Though Evelleen Richards believes that Owen never really understood von Baerian embryology until 1853, with T.H. Huxley's translation of the fifth scholium of von Baer's *Entwickelungsgeschichte.*) Richards, "A Question of Property Rights," pp. 142-143.
[511]A. L. Panchen, "Richard Owen and the Concept of Homology," in *Homology : the Hierarchical Basis of Comparative Biology*, ed. B. K. Hall (Toronto: Academic Press, 1994), 21-62, p. 50; Richards, "Embryonic Repetition", p. 218; Rupke, *Richard Owen*, p. 170; K. E. von Baer, "Fragments relating to Philosophical Zoology," in *Scientific Memoirs, selected from the Transactions of Foreign Academies of Science and from Foreign Journals*, ed. A. Henfrey and T. H. Huxley (London: Taylor and Francis, 1853), 176-238, pp. 222-225.

Owen. He dropped names of Owen's Oxbridge associates in letters while trying mirror Owen's current research interests. He sought Owen's testimonials for research positions, and requested his presence at informal meetings of London naturalists. Carpenter also sent gifts, at one point offering *Botryllus* specimens, the common compound sea squirt depicted above. It is unlikely that Owen lacked for them, so Carpenter's proffered gift might be seen as an attempt to "remember himself" to Owen. When Owen sent a gift back to Carpenter, Carpenter was cheered by this "mark of approval from the 'facile princeps' in the vast science of Biology".[512] When Carpenter took over from John Forbes as the editor of the *British and Foreign Medical Review* in 1847 (renamed the *British and Foreign Medico-Chirurgical Review*), he wrote favourable reviews of Owen's researches, for instance praising his work on the vertebrate archetype.[513]

But one of Carpenter's other goals was to be thought of as an "original and independent" discoverer,[514] not merely as a compiler and synthesizer who lectured and wrote physiology textbooks. Though he busily examined the microscopic structure of shells, Carpenter thought he had the best chance of making his reputation as a discoverer by being the first to apply von Baer's law of embryological change to the vegetable kingdom. But Owen claimed priority in applying von Baer's law too, and so a simmering priority dispute would slowly poison the relation between Carpenter and the *facile princeps*[515] as social and epistemological changes mixed. The reception of Owen's parthenogenesis can thus be placed against the growing popularity of von Baerian

[512]W.B. Carpenter to R. Owen, 4 Jul 1845, OCORR, NHM, 6/329-330.
[513]W.B. Carpenter to R. Owen, 23 Aug 1842; W.B. Carpenter to R. Owen, 1 Oct 1843, OCORR, NHM, 6/325-326; Rupke, *Richard Owen*, p. 183.
[514]W.B. Carpenter to R. Owen, 23 June 1843.
[515]Review of *Principles of General and Comparative Physiology* by William B. Carpenter, *Annals and Magazine of Natural History* 4, 111-116 (1840): pp. 112-113; Rupke, *Richard Owen*, pp. 180, 153.

embryology in Britain. Carpenter indicates this change, a researcher initially habituated to analysis and synthesis who then gradually took up palaetiology in order to make innovative new findings while also boosting his reputation. In other words, he gradually became a subversive.

At the beginning of his career Carpenter was favourable to the link between the plants, lower animals, compound individuality, reproduction and regeneration. In his 1839 physiology textbook, he recounted the ability of simple animals to create new individuals either through cuttings or budding. He mentioned the exemplar of *Hydra* cut into small pieces, and lower articulates' power of budding new individuals from segments. Carpenter also noted that the power of regenerating lost parts was related to the general capability of reproduction. The power of reproduction was strongest in lower animals. He thought that since this constant regeneration occurred only in living bodies, it might be a process of nutrition. Or perhaps it was the other way around: "nutrition has been not unjustly spoken of as a *perpetual generation.*"[516]

But Carpenter shifted ground through the 1840s; he moved from seeing an equivalence between reproduction and nutrition to the stance that what mattered was how plants - like lower animals - reproduced sexually. He publicly showed this change in an anonymous review of Steenstrup's work in 1848. Carpenter attacked Steenstrup's "alternation of generations" from the habit of palaetiology, ignoring Steenstrup's analytical / morphological use of the term "generation." Instead he seized upon the ambiguity of the term by interpreting "generation" as an activity – as reproduction. Carpenter showed his move to von Baerian palaetiology by quoting much of Martin Barry's statement - shown above on p. 173 - which discussed how development was

[516]Carpenter, *Principles of Physiology*, pp. 391-393.

centrifugal, expanding from a single point.[517] By doing so he was rejecting the habit of analysis and synthesis and instead taking up the palaetiological notion of development as process.

Carpenter didn't think that Steenstrup's theory could be applied to plants because they reproduced in two ways. One way, "gemmiparous" reproduction, was the generation of new cells, either on their own or in compounded groups which formed tissues like leaf-buds. (Carpenter did admit that these cells and cell-complexes often possessed "independent vitality.") The other way, "oviparous" reproduction, occurred in cells forming distinct germs – these germs produced by special reproductive organs, like flowers. Oviparous reproduction only occurred after two sexual organs interacted.

As a result, Carpenter stated, the true similarity between plants and simple animals lay not in their morphological resemblance or compound individuality of their buds / units – it lay instead in how they reproduced. The detachment of buds from *Hydra* resembled the detachment of bulbs from plants.[518] Carpenter had shifted the association of simple organisms with plants from an analytical likeness to a palaetiological one. Where before a plant could be decomposed into nominally independent components, just like a simple organism could, he emphasized a different view. Regeneration was no longer to be interpreted in terms of the localization of vitality, but as evidence of an organism's "gemmiparous" power.[519]

From his new stance which emphasized sexual reproduction, Carpenter explained other lower organisms in novel ways. For example, he could reinterpret the two forms of marine invertebrate hydroids by how they reproduced:

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[517]Carpenter, "Development of Zoophytes," p. 187.
[518]Carpenter, "Development of Zoophytes," pp. 192-194, 204-205.
[519]Carpenter, "Development of Zoophytes," pp. 196-198.

Hydroids embodied two different types, the

Medusoid	and the	*Polypoid.*
The medusoid could be distinguished by its ability to *move*, as well as by its ability to *sexually reproduce.*	while	the polypoid could propagate by gemmation or mutilation but could not move.

While various kinds of hydroids emphasized either the medusoid-type or polypoid-type more strongly, all hydroids nonetheless possessed the capabilities of both types to some degree. *Hydra* united both the polypoid- and medusoid-type by reproducing by gemmation or ova-production the most evenly (it also moved and was capable of reproducing from mutilation). Meanwhile sertularian zoophytes (the tree-like organism pictured on p. 204) were the "most complete evolution" of the polypoid type – in that they could not move, but could reproduce from mutilations. But given his reinterpretation of hydroids, Carpenter had to assume that sertularian zoophytes must reproduce sexually at some point, no matter how small a role this type of reproduction played. Therefore what became the "chief question" concerning sertularian zoophytes was this sexual origin of its gemmules.[520] Note that he had no observational evidence of the presence of sexual / oviparous generation in sertularian zoophytes – but given his reinterpretation, it was entirely reasonable for him to exhort others to look for such evidence.

Carpenter newly interpreted medusoids as having their own sexual physiological systems. To demonstrate that they had sexual systems, he likened the ova in each of their sexual systems to the seeds of a plant – and the polypoids that grew from these ova in turn were like the first bud of a new leaf. Because of the similarity of growth with

[520]Carpenter, "Development of Zoophytes," pp. 211-213.

asexual generation, Carpenter concluded that the development of medusoids and polypoids constituted a single generation, not two. And if one stuck to his image, it could be seen that the only difference between lower and higher organisms was the increased ability of the lower organisms' parts to stay separate and independent.[521]

Carpenter took advantage of his anonymity's shelter to criticize his patron: even Owen had confused the distinction between "bud" and "ovum",[522] and Carpenter sought to correct him.[523] He distinguished between different types of reproduction and the germs in which they occurred: gemmiparous reproduction (asexual generation) took place in buds. Meanwhile oviparous reproduction (sexual generation) took place in ova. But Carpenter's distinction between buds and ova seems rather weak and circular: an ovum was different from a bud by being matter "not yet organized", becoming fertile and producing new tissues when impregnated; the bud, meanwhile, had its own spontaneous power of forming new tissues.[524]

Churchill has aptly commented upon Carpenter's criticism of Owen as being rooted in a theoretical presupposition (the primacy of sexual reproduction).[525] Churchill's statement can be built on by saying that Carpenter's distinction between the two types of germs was rooted in a palaetiological habit and could only be supported by different types of evidence. That is, his criticism of Owen and Steenstrup could be seen

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521 Carpenter, "Development of Zoophytes," pp. 204-205.

522 Carpenter, "Development of Zoophytes," p. 197. Churchill notes that although this essay review was published before Owen's *On Parthenogenesis*, it referred to Owen's 1848 Hunterian Lectures, which contained many of the same statements. Churchill, "Sex and the Single Organism," p. 151.

523 On the other hand Owen may have known that Carpenter was the author very quickly, as anonymity was often quickly pierced; at any rate it was public knowledge that Carpenter was editor of the journal. See note 544. For Carpenter's attempt to cultivate his scientific reputation while also earning a living writing anonymous reviews, see J. R. Topham, "Scientific Publishing and the Reading of Science in Nineteenth-Century Britain: a Historiographical Survey and Guide to Sources," *Studies in History and Philosophy of Science*, 559-612 (2000): pp. 596-597.

524 Carpenter, "Development of Zoophytes," p. 197.

525 Churchill, "Sex and the Single Organism," p. 151.

as valid only if the germs were seen changing over time (for example, interacting sexually).

Another way to illustrate this point is to distinguish between exemplars: perhaps a living specimen in an aquarium versus a dead specimen preserved in alcohol in a glass jar. Part of the difference between an analyst and a palaetiologist is affected by institutions and methods of specimen-preservation. For example, Carpenter criticized Steenstrup by deploying (Sir) John Dalyell's observations. Dalyell had observed the entire life cycles of various marine invertebrates such as hydroids as they changed from one form to another. But Dalyell was only able to make these observations because of his wealth: he could afford to have "capacious glass vessels" made for his creatures to live in, and the water in which they lived was kept fresh only through the expensive option of having a fresh batch of seawater brought round to his house every morning.[526]

But because he had these tanks, Dalyell had the rare option of watching his marine invertebrates change over time. He can be contrasted against researchers such as Owen who tended to look at dead, and thus quite unchanging, museum specimens preserved in alcohol in glass jars. The increasing ability of researchers to observe the entire lives of organisms (through either new inventions or observations in the field) meant that a new generation of palaetiological researchers could call upon new 'evidentiary resources,' meaning new and novel ways to see the standard exemplar organisms. In turn, people such as Carpenter cited these new perspectives in order to support their palaetiology.

To understand the role that different interpretations of standard exemplars play, there is an enlightening similarity in Barry Barnes's interpretation of Thomas Kuhn's

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[526]Allen, *The Naturalist in Britain*, pp. 132-133; Carpenter, "Development of Zoophytes," pp. 200-205.

work. Barnes notes that in one of Kuhn's papers on Aristotle's *Physics,* Kuhn discussed Aristotle's notion of "speed". In one place, there is the notion of speed in the sense of an object crossing a greater distance in the same time as another; crossing an equal distance in less time than another; or crossing a greater distance in less time than another. In the case of a race between the tortoise and the hare, the speed of the tortoise exceeded the hare's. We would now call the tortoise's speed "average speed."

But Kuhn noticed that in other passages of the *Physics,* Aristotle discussed objects gaining or losing speed very rapidly, at an instant, without any reference to the end-point of their motion. We would now call these cases examples of "instantaneous velocity," which to us is extremely different from average speed. But Aristotle did not make this distinction as both "average" and "instantaneous" speeds fell under his general term, "speed." Though Aristotle's exemplars differed from one another, he did not explicitly acknowledge (or notice) the difference between them.

Barnes notes how Galileo demonstrated why it was important to differentiate the term "speed" into both "average speed" and "instantaneous speed": he gave examples of problems that arose if the distinction wasn't made. Going back to the example of the race between the tortoise and the hare – which one showed more speed? Though the hare was faster at some points of the race, the tortoise got there first. An Aristotelian might say that the tortoise was faster but also slower than the hare – a paradox. Kuhn's achievement wasn't to say that Aristotle and his followers were confused about motion. Instead Kuhn noted how people such as Galileo decided to elaborate on the Aristotelian notion of speed by referring to concrete instances where using the original Aristotelian concept caused paradoxes or practical difficulties. But until such situations are pointed out by someone, Barnes notes, people will confidently use (retrospectively problematic) definitions and terminology.[527]

Kuhn's insight can in turn be applied to John Farley's interpretation of Steenstrup's alternation of generations. Farley was brilliantly correct in pointing out the distinction between "morphologists" who saw organisms statically and "physiologists" who saw organisms dynamically, and the differences this distinction caused over the alternation of generations. But Barnes's discussion of Galileo and Aristotle shows why it is problematic for Farley to portray Steenstrup's work as confused (which he claims occurred because Steenstrup defined the word "generation" morphologically and not physiologically).[528] One must distinguish between an author's work and its use by others - Steenstrup's notion of an alternation of generations only became confused when palaetiologists such as Carpenter, like Galileo, pointed out where problems arose in the confident use of terms such as "generation".

They pointed out problems by using exemplar organisms like hydroids. In a second article on the subject Carpenter pointed out that while Owen's comparison of simple plants to polyps was unproblematic if seen analytically, it was far more confusing when seen palaetiologically: "Now if we look to what a part can develope [sic] or *become*, instead of to what it *is*, as our test of individuality, we shall find ourselves reduced to a state of great perplexity."[529] By trying to see the individual polyps which made up a compound individual as each changing, Carpenter tried to push these exemplar organisms into absurdities, forcing others to reinterpret them.

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[527]Barnes, *T.S. Kuhn and Social Science*, pp. 36-38.
[528]Farley, *Gametes and Spores*, pp. 75, 78.
[529]W. B. Carpenter, "Owen and Paget on Reproduction and Repair," *British and Foreign Medico-Chirurgical Review* 4, 409-449 (1849): pp. 441-442. Emphasis in original.

This point - about how confusion is not simply found, but also *created* by later interpreters - is strengthened by noting that initially *Carpenter* was thought confused, not Owen. Other elite researchers saw Carpenter's belief - that the various animal bodies formed out of a single sexual act constituted a single individual - as very strange indeed. E.S. Forbes thought the view of compound individuality was far more reputable and conventional: if Carpenter meant that medusae produced by gemmation were simple metamorphoses, then he meant that they weren't distinct individuals but "parts of some one capable of maintaining a separate and independent existence." But Carpenter's definition didn't work, as good researchers knew better than to take this popular view: "popularly we look upon the whole plant as an individual. Yet every botanist knows that it is a combination of individuals, and if so, each series of buds must certainly be strictly regarded as generations."[530]

Another researcher, Allen Thomson, called Carpenter's opinion "arbitrary" because not enough was known at the time; he called for more research into the differences between a bud and an ovum to settle the question. Nonetheless he still thought that if various animal bodies moved about separately and independently, they should be seen as distinct individuals. Attempts such as Carpenter's to deprive Steenstrup's views of their importance had "entirely failed",[531] and it was still reputable to hold onto the definition of individuality as independence.

After Forbes's charge, Carpenter retreated a bit. In 1849 he still thought that the likeness between a tree, or "composite plant", and a polyp was valid, but that Owen's discussion - of even the simplest plant as an assemblage of individuals - was more

[530]E. Forbes, *A Monograph of the British Naked-Eyed Medusæ* (London: Ray Society, 1848), pp. 87-88.
[531]Thomson, "Ovum," pp. 39, 35-36. In the footnote Thomson refers to Carpenter as the author of Carpenter, "Development of Zoophytes", despite its anonymity.

problematic. Carpenter didn't think that Owen's example worked, as there were only a few cases of single leaf-parts, petals, or stamens maintaining an independent existence.[532] Carpenter continued to try to have it both ways: in an 1851 BAAS paper with James Dana, they palaetiologically reinterpreted the "law of alternating generations", stating that plants and zoophytes ought to be defined by how they reproduced. They further hoped that biological individual would be defined palaetiologically. But old analytical habits were hard to break: it was nonetheless an "admitted fact" that each leaf-bud was an individual, and each tree and zoophyte was therefore a

> compound group of individuals...among plants the seeds produce leaf individuals; and these produce seeds; precisely, as the egg produces polyps, the polyps bulbs, that develop into Medusae, and the Medusae eggs.[533]

The tree still remained as both an instance and a representation of compound individuality.

T.H. HUXLEY – SUBVERSIVE PALAETIOLOGIST

A person with more terminological consistency was T.H. Huxley. In the 1840s the young surgeon-naturalist read Matthias Schleiden while attending John Lindley's botany lectures; when he attended Charing Cross Hospital Medical School on a scholarship, he met the physiologist Thomas Wharton Jones (another Edinburgh product),

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[532]Carpenter, "Owen and Paget on Reproduction," pp. 441-442.

[533]W. B. Carpenter and J. D. Dana, "On the Analogy between the Mode of Reproduction in Plants and the "Alternation of Generations" observed in some Radiata," *Edinburgh New Philosophical Journal* 50, 266-268 (1851): pp. 266-267.

getting on very well with him. And when Huxley graduated in 1845 he won the gold medal for anatomy and physiology.[534]

Huxley's early (1846-1847) London medical notebooks contain several quotes explicitly citing Schleiden: that scientific morphology would only occur "whose leading principle can be *Development* alone"; "Without the study of Development there exists no Science of Botany". Huxley also reflected upon the use of palaetiology for taxonomy. Cirripedes (barnacles) had confused previous researchers who looked at the form alone, and they were only classified successfully (as molluscs) when their entire life cycle was examined. A human forearm was the same structure as a horse forearm, though they didn't resemble one another. Thus Huxley redefined "identity" in terms of palaetiology: he privately described affinity as the "resemblance of development", and any lesser form of resemblance as "analogy."[535]

Huxley cast about for examples in other fields to support his new view of affinity and analogy. He saw a similarity between zoology and comparative philology, which he defined as "the science of verbal forms". Like zoology, comparative philology distinguished between analogy and affinity – letters or sounds of a root were elements just like the "organic elements" of an animal. But then Huxley moved away from the terminology of analysis. Merely analyzing words into their component roots and finding similarities wasn't enough to establish an identity between two languages. Instead a palaetiological view had to be taken. Philologists didn't consider two different languages with many similar sounding words to be related; it was only when they showed how these words arose from similar roots that the languages could be said to be related.

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[534]W. F. R. Weldon, "Thomas Henry Huxley (1825-1895)," in *DNB* (1901).
[535]T. H. Huxley, Manuscript, "Some Considerations upon the Meaning of the Terms Analogy and Affinity", [1846-1847], HP, IC, 37.1-21, pp. 37.11, 37.13-15, 37.20.

Huxley speculated further: perhaps the law adding prefixes and suffixes to this root was just like an animal's development - and if one knew the way in which this development occurred (its "general laws"), in turn one could deductively form that language. Just like comparative philology, zoology might become a deductive science if one only knew exactly how its zoological elements developed.[536] Huxley had reinterpreted elements not to stand for the simplest possible independent forms of a language, but instead palaetiologically, as points from which further development occurred.

Whewell had already noted a similarity between the discipline of geology and comparative philology, for just as geologists studied causes of change,

> so, in like manner, the tendencies, instincts, faculties, principles, which direct man to architecture and sculpture, to civil government, to rational and grammatical speech, and which have determined the circumstances of his progress in these paths, must be in a great degree known to the Palaetiologist of Art, of Society, and of Language, respectively, in order that he may speculate soundly on his peculiar subject.[537]

Instead of seeing Whewell's work as pontifical, as a tract that issued directives on how the sciences ought to be carried out, one can see his work as offering a range of exemplar disciplines, such as comparative philology, whose methods could be copied by practitioners in other fields.[538] Huxley in turn saw comparative philology as an 'exemplar discipline' and imported its methods into his zoology. While it has not been established that Huxley read Whewell's *History*, it is hard to imagine other British writers who linked the field of comparative philology with other disciplines through the method

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[536]Huxley, "Considerations upon Analogy and Affinity", p. 37.21.
[537]Whewell, *History of the Inductive Sciences*, pp. 3:399, 3:401.
[538]Jardine, *The Scenes of Inquiry*, pp. 103-104.

that they shared. At any rate we do know that Huxley acquired a more general developmentalism from German researchers.

Huxley's use of palaetiology increased as he voyaged to Australasia on H.M.S. *Rattlesnake.* Like Galileo, Huxley pointed out examples which caused problems for familiar definitions – he indicated organisms where it was problematic to define an individual by its independence. The detached and free-swimming sexual parts of one type of marine invertebrate might have the right to distinct names, but what about the sexual parts of other animals that were "obviously mere organs?" Huxley did not want "*true polypes*" possessing "*medusiform* generative organs" to be confused with *"Polypiform larvae* of true Medusae."[539]

Huxley used a palaetiological perspective to make his first major innovation, one which gave him his initial reputation. He showed that many marine invertebrates, including polyps and acalephs (siphonophores such as the Portuguese Man-o'-War) were composed of two fundamentally different membranes, the "foundation layers" of the endo- and ectoderm. The key to properly classifying these confusingly-formed organisms, then, was to do it at the level of the tissues which they possessed in common.[540] Rather than examining the various polyp-types that could exist independently, Huxley grasped just how useful it was to group organisms by common tissues with the most potential to become differentiated into more specialized tissues: in other words, its organic elements that he gave a different palaetiological importance.

[539]T. H. Huxley, M. Foster, and E. R. Lankester, *The Scientific Memoirs of Thomas Henry Huxley*, 4 vols. (London: Macmillan, 1898), p. 1:29. This is a reprint of "On the Anatomy and the Affinities of the Family of the Medusae," *Phil. Trans.*, 1849, pt. 2, 413. Emphasis in original.
[540]Winsor, *Starfish, Jellyfish, and the Order of Life*, p. 61.

To describe what Huxley did using Martin Barry's terminology, he classified from the root of the ramifying tree of development rather than by grouping its twigs. But even using the image of a tree of development to describe Huxley's achievement is slightly anachronistic: it implies that other attempts which used only "mature" organisms in their classifications therefore failed, presuming what they were really doing was using palaetiological criteria. In order for Huxley's achievement to be seen as a useful contribution to research, others had to believe in a ramifying tree of development too.

Huxley read Owen's *On Parthenogenesis* on the decks of the *Rattlesnake* to understand generation better. Just as he had copied out Gideon Mantell's depiction of infusorians such as *Volvox globator,* so too did he copy out Owen's writings about different exemplar organisms. *Gregarina* was a single-celled parasite whose young appeared in their own interior. *Nais* possessing generative (sexual) organs were nonetheless developed from parents with no generative organs, emerging instead by budding. Their "budding" section, located between the last and penultimate segments, had this power of gemmation and regeneration because it retained derivative germ-cells there. Other annelids possessed derivative germ cells, a phenomenon also explaining regeneration in organisms such as the "compound hydriform polyps". Aphids could generate embryos without sex because part of the germ-mass from sex was retained through successive generations in the "branchial uterus" of the larvae. Huxley paid special attention to Owen's histological observations, copying *verbatim* the explanation that the retained "sperm-force" was kept in "nucleated" cells identical to the "progeny" of the fertilized germ cell, and that both types of cells were able to undergo spontaneous fission.[541]

And upon his return, Huxley bore a letter from his Australian mentor William Sharp MacLeay to Owen himself. MacLeay recommended Huxley's researches on the "lower pelagic animals", particularly his drawings, which bore in "some considerable degree on the subject of your 'Parthenogenesis.'" Patron spoke to patron: any favour that Owen could do for Huxley would be considered as conferring a favour upon MacLeay himself.[542] In turn Owen wrote to the Admiralty seeking a second appointment for Huxley on another ship so that Huxley could prepare his materials.[543]

HUXLEY IN LONDON: ZOÖIDS AND INDIVIDUALITY

In the two years after his return, Huxley busily set out to make a name for himself and earn a living doing science. He sent letters to famous medical researchers and naturalists. Huxley complimented "the course pursued" by John Goodsir, and set out Goodsir's memoirs as his own models. Huxley presented himself as a disinterested observer of facts confining himself to the re-examination of doubtful points about lower marine forms. Yet he then belied this purported empiricism by asking Goodsir about tunicate (like sea squirt) anatomy – could he confirm Huxley's belief that they all had internal shells?[544] Huxley formed contacts with other researchers too: one was W.B.

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[541] T. H. Huxley, Manuscript, "Notes on Owen's Parthenogenesis", [1849-1850], HP, IC, Rattlesnake Notebook, 50.13-50.19, p. 50.19.
[542] W.S. MacLeay to R. Owen, 28 Apr 1850, OCORR, NHM, 17/331-332.
[543] Draft Letter on behalf of Huxley to Sir Francis Baring, First Lord of the Admiralty, OPAP, RCS.
[544] T.H. Huxley to J. Goodsir (Draft), 20 Jan 1850, HP, IC, 17.72-73.

Carpenter, for whom he corrected the proofs of the 1852 edition of *Principles of Comparative Physiology*.[545]

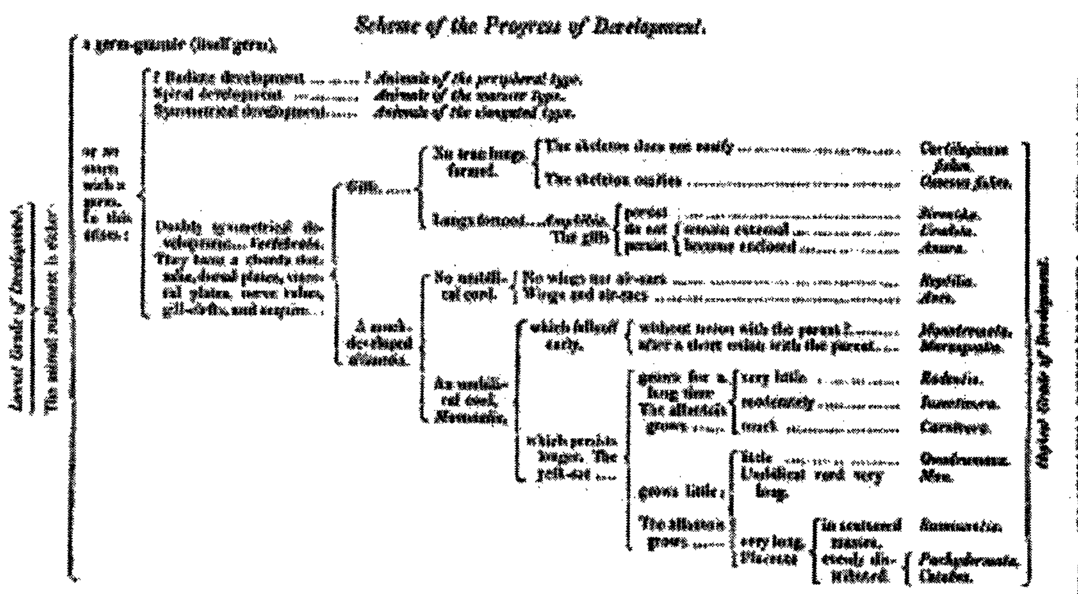
And like the earlier generation of analysts bringing fashionable Continental knowledge to Britain, Huxley's knowledge of German not only kept him up to date with the latest developments in German science. He could achieve two other goals simultaneously, earning money by translating scientific works from German to English while also bringing to a non-German-speaking audience the findings he deemed the highlights of German research. It was Huxley who cited Goethe's statement about every translator being "a broker in the great intellectual traffic of the world…promot[ing] the barter of the produce of mind."[546] And his brokerage-duty enabled him to make new and influential contacts. The well-known (but non-German-reading) Newcastle naturalist Albany Hancock asked Huxley to confirm some of Albert Kölliker's findings on cephalopod anatomy, and a long correspondence ensued between senior and junior naturalist.[547]

Some of Huxley's translations therefore revealed his leanings: a translation of Kölliker's histology (done with fellow Germanophile George Busk, also a naval surgeon), provided Huxley with both a palaetiological outlet and a chance to flatter Owen. Owen's nomenclature of the teeth was better than Kölliker's and more "scientific", they stated in an extensive footnote – because Owen's language had "recourse to development". Owen's terms for different teeth, such as "deciduous molars" and "premolars", could refer to both the first and second set of teeth that an animal had

[545] A. Desmond, *Huxley : from Devil's Disciple to Evolution's High Priest* (London: Penguin, 1997), p. 180.
[546] *Scientific Memoirs*. Epigraph.
[547] A. Hancock to T.H. Huxley, 16 May 1852, HP, IC, 17.252-256.

during its lifetime.[548] “Deciduous” brought to mind the image of leaves falling from a tree.

Huxley also translated a section of von Baer’s works in the early 1850s, and it also depicted palaetiological trees. A table portrayed the process of differentiation from an origin: development moved from a single, undifferentiated “animal rudiment” to the most differentiated, specialized, version of the mature animal. In one developmental path, the “animal rudiment”: was an ovum; then turned doubly symmetrical (showing it to be a vertebrate embryo); developed gills; didn’t develop true lungs; then had its skeleton ossify; it ended up as an “osseus fish” (instead of one with cartilage).[549] At every point the path could have taken a different direction, could have diverged, but the



Possible developmental options to be taken by a vertebrate embryo, in K.E. von Baer, "Fragments Relating to Philosophical Zoology," in *Scientific Memoirs*, p. 215.

[548]Huxley and George Busk, footnote, in Kölliker, *Manual of Human Histology*, pp. 2:60-61.
[549]von Baer, "Fragments," pp. 214-216.

range of available options grew smaller and more specialized as development proceeded. In bringing out von Baer's work, Huxley was again bringing up the image of the ramifying tree which Barry had done fifteen years before in his own presentation of von Baer.

The image of the ramifying tree shows why Huxley defined an individual as the entire product of a single sexually fertilized egg, regardless of the independence of its parts. Independence was no longer his criterion for individuality – only sexual reproduction was. The analyst defined an individual by criteria of independence, boundaries, the localization of vitality. When Trembley cut the polyp into many pieces, it raised questions like what happened to the 'soul' or organizing principle of the polyp – did it have one or many? Why could separated pieces form new polyps?[550] By contrast the palaetiologist defined the individual by its origins and history: it raised different questions, such as where one individual began and another ended. In this palaetiological habit one possible answer to these problems was to focus upon the most obvious starting-point of the individual. The sexual act was the act with the greatest potential to lead to the widest range of possible specific options, as well as the easiest-to-distinguish 'singularity' where differentiation began. The single sexual act can be seen as the base, or origin, or root, of a tree with diverging branches.

Huxley had been thinking about his new definition of individuality as early as 1850, and wanted to stay terminologically consistent. One possible term offered itself: while on HMS *Rattlesnake* he privately used the term "zoöid" to refer to the different salp-forms that he pulled from the ocean.[551] He chose it because it resembled "phytoid,"

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[550]T. L. Hankins, *Science and the Enlightenment* (Cambridge: Cambridge University Press, 1985), p. 133.
[551]T. H. Huxley, Manuscript, 5 September 1850, HMS, IC, 63.8.

the "partly analogous" word used by botanists: Huxley noted that they considered phytoids to be individuals and plants as compound structures. But he didn't consider a zoöid to be a true individual – the term was to be applied to forms that were really only animal parts that "simulate" complete animals.[552]

Huxley began to publicly use his new word after returning to London. The distinction between zoöid and individual was founded upon the surest zoological basis – that of development. His definition of the true zoological individual was "the sum of the phenomena successively manifested by, and proceeding from, a single ovum, whether these phenomena be invariably collocated in one point of space or distributed over many". For support, he pointed to Carpenter's work.[553]

As Huxley started publishing his *Rattlesnake* findings, he emphasized his new term as a solution to the problems of individuality raised by previous researchers such as Carpenter. Was the aphid a sequence of individuals? No, it was composed of nine to eleven zoöids: the aphid-individual was really the entire sequence of forms produced after sexual generation. Sertularian polypes were not compound organisms, made up of numerous independent individuals: instead they were also made up of zoöids. Huxley was applying the term, and the palaetiology associated with it, to exemplar organisms used by analysts to best depict compound individuality.[554]

Huxley applied his terminology and his habit of reasoning in anonymous reviews too. Steenstrup had made an error by using the criterion of independence to define an individual – for if we used this definition then standard sperm- and cancer-cells were also

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[552]T. H. Huxley, Manuscript, "On Animal Individuality", HP, IC, 38.2-38.52, p. 38.5.
[553]Huxley, Foster, and Lankester, *Scientific Memoirs*, pp. 1:52-53, 1:52fn. This is a reprint of Huxley's paper in *Phil. Trans.*, 1851, pt. 2, pp. 567-594.
[554]Huxley, Foster, and Lankester, *Scientific Memoirs*, pp. 1:52-53.

individuals. He invoked the example of mathematics to illustrate his point: it was a mistake to attribute biological individuality to an independent existence, for a fraction didn't become equal to the unit by standing alone; it was only the sum of these parts/fractions that was equivalent to the individual of higher animals.[555]

George Allman (who worked on freshwater hydroids in Dublin and who already valued Huxley's research) complimented Huxley for his article on Müller – he chortled that he didn't even have to confess authorship. Allman stated that Huxley had hit upon the true explanation of the matter, as the alternation of generations and parthenogenesis were no more than "specious fictions." He used Huxley's term "zoöid" in an 1853 paper on freshwater polyps, publicly complimenting both Huxley and Carpenter for avoiding ambiguous definitions of individuality, showing the emergence of a new, self-aware, community of palaetiological hydroid researchers. Allman thought that the term "zoöid" allowed the researcher to avoid the confusion accompanying the term "individual" when it served as "the logical element of a *species.*"[556]

Allman now rejected the habit of analysis and synthesis, which bestowed the term "individual" upon an independent organic element of a larger group, the species. Allman's conversion occurred despite his easy reference to polyps two years before as compound animals (citing, for instance, Trembley's famous discovery).[557] So the term "individual" and its standard of independence was not problematic - until it was pointed out as such.

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[555]Huxley, Foster, and Lankester, *Scientific Memoirs*, pp. 1:116-118. This is a reprint of T. H. Huxley, "Report upon the Researches of Prof. Müller into the Anatomy and Development of the Echinoderms," *Annals and Magazine of Natural History* 8, 1-19 (1851).
[556]G. Allman to T.H. Huxley 12 Apr 1852, HP, IC, 10.54; Allman, "On the Anatomy and Physiology of *Cordylophora*," p. 379. Emphasis in original.
[557]G. Allman, "On the Present State of our Knowledge of the Freshwater Polyzoa," *BAAS*, 305-327 (1851): pp. 305, 307.

Yet other researchers were sceptical of Huxley's innovation. Charles Darwin used the old standard of independence in a circular way, doubting whether "creatures having so plainly the stamp of individuality as have many of your zooids will ever cease to be called individuals."[558] Allen Thomson didn't think the term "Zoöid" was really necessary either. He misinterpreted Huxley too, probably because of the strangeness of his definition: thus Thomson noted that Huxley saw animals such as the Portuguese Man-o'-War as "compound organisms", despite Huxley's explicit statement that no such term could be applied to any organism which sexually reproduced.[559] Thomson did nonetheless correctly point out Huxley's distinction between "true ova" and mere "reproductive bodies": following Carpenter, Huxley argued that true ova were single cells undergoing special development, while "reproductive bodies" were aggregations of cells surrounded by a shell and *simulating* true ova. Thus Huxley was seen as using the act of sexual generation to distinguish certain bodies from others: by using sex as his criterion the "true" ova could be picked out of the confusing mass of buds, bud-germs, gemmae, spores, winter ova, ephippial ova, and statoblasts.[560]

In 1852 Huxley was invited to lecture at the Royal Institution, and he leapt at the opportunity. When meeting Faraday at a social gathering, he told "the little guy" that he would "introduce some peculiar speculations of his own which might coincidentally modify the theory of zoology". These Royal Institution lectures had both scientific men and fashionable ladies in the audience, requiring that one approach the topic both in a

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[558]C. Darwin to T.H. Huxley, 17 Jul [1851], HP, IC, 5.2-3. This can also be found in C. Darwin, *The Correspondence of Charles Darwin*, ed. F. Burkhardt and S. Smith (Cambridge: Cambridge University Press, 1989), p. 5:49.

[559]Thomson, "Ovum," pp. 22, 39. Huxley said that the only organism entitled to the term "compound animal" was thus the *Diplozoon paradoxum,* which von Siebold had shown to be the "fusion of two previously distinct individuals." Huxley, Foster, and Lankester, *Scientific Memoirs*.

[560]Thomson, "Ovum," pp. 119, 130.

specialized and "popular" way; successful lectures played "at battledore & shuttlecock with it – so as to nail both". Huxley therefore thought that a good topic for such a lecture was his palaetiological opinion about biological individuality. He knew his views were still new and unique - though Carpenter approved of them, Forbes and "some others" would have to come around.[561]

"On Animal Individuality" was a forceful repetition of many of Huxley's earlier statements. As he lectured, he drew various animals "commonly called Compound Animals". One was the salp, which had two different forms (one had horns and the other did not, but was pointed at each end, for example). There was *Salpa democratica,* and there was *Salpa mucronata. S. democratica* developed a tubular extension from which a chain of minute buds spontaneously grew, and the chain turned into a long association of "individual" *S. mucronata.* At first the *S. mucronata* were adhered together, but they separated from one another over time. Each *S. mucronata* in turn had an egg which grew inside its "respiratory" cavity and grew into a solitary *S. democratica* in turn; the egg detached from the parent when mature. Both salp forms were highly organized, Huxley noted, and no one hitherto would think of either as anything else than a distinct individual.[562] These sketches increasingly magnify the bud-tube which turned into future *S. mucronata.*

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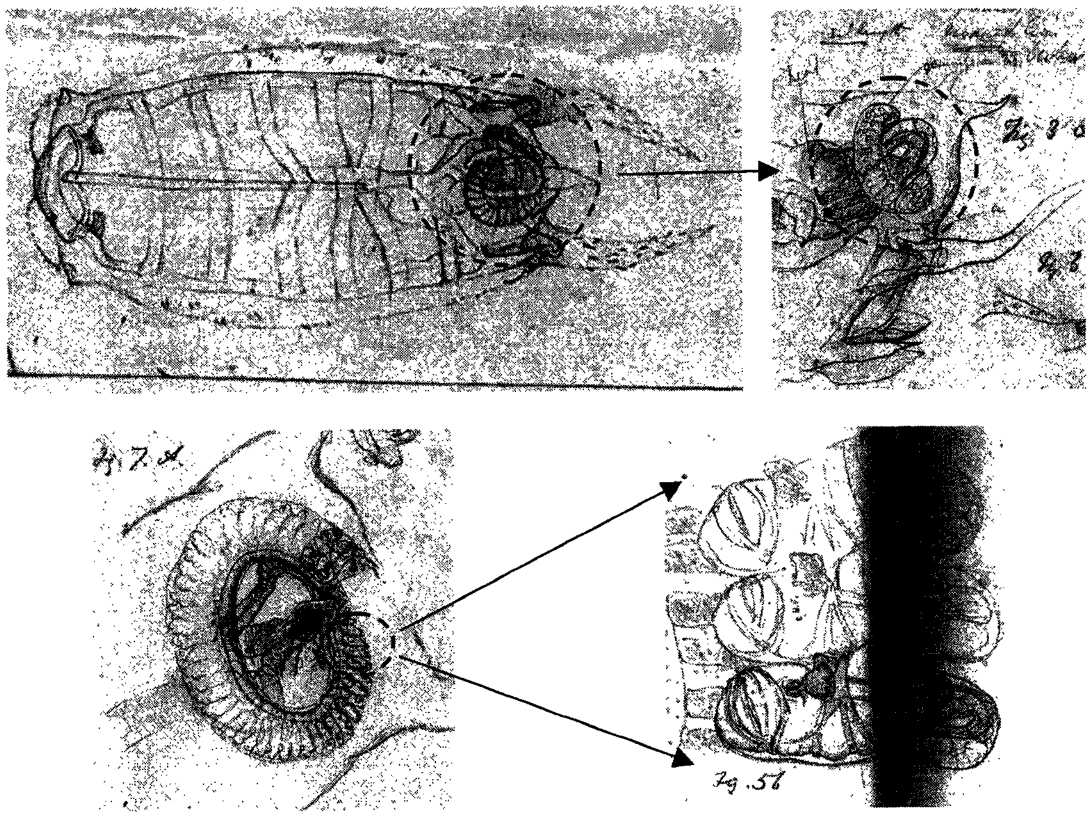
[561]T.H. Huxley to W.S. Macleay (Draft), 9 Nov 1851; T.H. Huxley to H. Heathorn, 27 Feb 1852, T.H. Huxley - Henrietta Heathorn Correspondence, Imperial College, London, Huxley Archives, HH 189.
[562]Huxley, "On Animal Individuality", pp. 38.16-18. I am not sure why the *S. democratica* had this name – contrary to what one might expect from this name, it was the solitary form of the salp. *S. mucronata* was the one forming the salp-association.



Buds of *Salpae* from T.H. Huxley, *Drawings*, Huxley Papers, Imperial College London, vol. 74, plate 4, 5 and 10. These drawings very closely follow Otto Sars's drawings of salps, and Huxley (or an archivist) refers to Sars in a scrawled note at the top of plate 4.

He stated that the free forms of salps were anything but individuals; that only the entire developmental cycle - starting with the single act of sexual fertilization - could be considered an "individual." Huxley acknowledged that his temporal view was a novel one: that he, with Carpenter, stood "almost alone" in this opinion, as other naturalists took independent existence to be the criterion of animal individuality. But they were mistaken, he proclaimed: they had only used independence because it was the standard of higher animal individuality, unthinkingly imposing this standard upon the lower animals.[563]

[563]Huxley, "On Animal Individuality", pp. 38.39, 38.42.

But his proclamation was disingenuous: instead salps had previously been seen in the light of the analytical problem of the simplest possible independent organic elements which at times coalesced together. Huxley accentuated a different viewpoint of this exemplar organism. His intervention might be even called 'iatrogenic': he reinterpreted salps as presenting a problem and then he presented his own solution to it. In his view the salp was not a compound animal, a congeries of nominally independent organisms representing the morphological problem of the succession of different forms. Instead Huxley used sexual and asexual reproduction as a clear but new distinction which solved the dilemma.

To show that salps weren't compound, Huxley turned to previous interpretations, using his new view to depict them as confused. Without naming Thomas Rymer Jones (an earlier popular lecturer at the same Royal Institution), he deliberately contradicted him. Huxley argued that it was the sum of caterpillar, chrysalis, and butterfly which constituted the individual insect. The moulting of a caterpillar's skin meant the separation of one part from another part, one of which died; moulting was thus "concentric fission." The separated skin, unlike the rest of the caterpillar, couldn't live independently. Huxley stated that this moulting occurred in salps too – except in their case, both portions lived independently after separation! *S. democratica*, which corresponded to the caterpillar, had an independent life even after *S. mucronata* (corresponding to the butterfly) budded from it.[564]

The difference between a caterpillar skin and *S. democratica* could be newly seen as one only of degree. And the difference between moulting, fission, and budding also were only matters of degree – the multiplication of an individual's forms, of zoöids, was

[564]Huxley, "On Animal Individuality", pp. 38.28-34.

simply a case of "irrelative repetition," like the budding of myriapod and annelid segments.[565] While the phrase "irrelative repetition" seemed to show similarities between Huxley's position and Owen's, in fact they used "repetition" differently: Owen saw it as an analytical pattern ("morphologically," as repeating identical segments) and Huxley seeing it as a palaetiological process (as the act of the asexual budding of new segments).

Huxley was further preparing a major attack on Steenstrup and Owen which never appeared. We know of this attack because the Huxley Papers at Imperial College not only contain the clean longhand account of his talk - with his own page numbering - but also the messier draft version of the lecture. Huxley likely wrote this first draft and then became cautious, wisely omitting any overt criticisms of Steenstrup and Owen as he did not feel that he was secure enough to challenge their authority.

In this private first draft, Huxley began by again examining the "common" notion of the lower invertebrates as "compound animals". For Huxley, who used the criterion of a "generation" as the progeny of a sexually fertilized ovum, Steenstrup's position made no sense – from fertilized ova emerged a generation of differently-formed animals, but then this second generation multiplied asexually, by gemmation. Because this second generation multiplied by gemmation, it could be seen as the "irrelative repetition" of the stages of metamorphosis: it was irrelevant whether the zooid-units of this generation were associated or independent, of the same or of different form. What really mattered was the matter of their asexual origin: since they were all zooids produced asexually, this group wasn't really a second "generation" at all. If he was correct, Huxley thought, the whole theory of the alternation of generations "must fall".[566]

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[565]Huxley, "On Animal Individuality", pp. 38.36-38.
[566]Huxley, "On Animal Individuality", pp. 38.7-38.8.

Huxley then realized that this emphasis upon origins would also overthrow Owen's notion of parthenogenesis – for in his view Owen had merely restated Steenstrup's conclusions. He cheekily noted that "spermatic virtue" was merely a verbalism. In highly regenerative animals like polyps, he could not distinguish any mass of cells which might contain this "virtue" from other cell-masses. He could not find proof that spermatic virtue was transmitted, moreover, as semen never came into contact with primary embryo cells. But someone with Huxley's palaetiological perspective – someone looking for origins - would be looking for different kinds of evidence than an analyst (for example, seeking out localized changes in cells rather than looking for broad comparisons between highly-regenerative and less-highly-regenerative animals).

Nonetheless Huxley privately acknowledged contradictions in his own definition of an individual. The distinction between zoöid and animal was not always of equal importance: in *Nereis,* there could be no distinction between a zoöid and animal because a *Nereis* produced by gemmation couldn't be distinguished from one emerging from a fertilized ovum. In this case, sexually and asexually-produced *Nereis* were equivalent. To assuage his doubts Huxley fell back upon an earlier resemblance. Just as a tree grown from a cutting or graft had different qualities than one formed from a seed, so too must *Nereis* zoöids formed by gemmation differ from those emerging from fertilized ova, however similar they might appear. Like Carpenter's point about sertularian polyps, Huxley's resemblance was founded upon a palaetiological presupposition: Huxley didn't point out how a tree grown from a cutting or graft differed from one grown from a seed.[567]

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[567]Huxley, "On Animal Individuality", p. 38.9.

Despite these criticisms, Huxley was not entirely against Steenstrup's work. As late as 1854 he would speak of the theory of the Alternation of Generations as one of the "best established and most notorious scientific generalizations of the day."[568] But his coming fight with Owen would push any of this ambivalence to the side, sharpening his opposition to the Alternation of Generations, to Owen's explanation of spermatic force, and to Owen himself.

HUXLEY ATTACKS

A year after Huxley's return to London, he recounted to MacLeay that although Owen had helped him in some cases - particularly with the navy - he was still guilty of some "ill natured tricks". Owen might have been a good comparative *osteologist*, but he got lost when it came to abstractions – Huxley pointed to *On Parthenogenesis* as evidence.[569] While Huxley initially made attempts to cultivate and flatter Owen as a patron, in private he began to reject Owen's authority.

Why did Huxley finally mount public attacks on Owen? Part of the reason lay in Huxley's fierce temperament – in one letter to his fiancée, Henrietta Heathorn, he wrote of the need to actively attack and destroy the old: "however painful for oneself this destruction of things that have been holy examples – it is the only hope for a new state of belief – that this destruction take place."[570] Accentuating Huxley's aggression must have been his dire finances in 1852. He had unsuccessfully tried for teaching posts in Toronto

[568]T. H. Huxley, Review of *The Vestiges of Creation*, *British and Foreign Medico-Chirurgical Review* 13, 332-343 (1854): p. 341.
[569]T.H. Huxley to W.S. Macleay (Draft), 9 Nov 1851.
[570]T.H. Huxley to H. Heathorn, 23 Sept 1851, T.H. Huxley - Henrietta Heathorn Correspondence, Imperial College, London, Huxley Archives, HH 166.

(1851) and Aberdeen (1852). He had not seen Henrietta for two years, and could not even think of bringing her to England from Australia until he had made enough money, a seemingly distant prospect.[571] Some of Huxley's friends were emigrating to Australia, and he actually considered this possibility. Making things even worse was the death of his mother in April 1852; following her death, his father became physically and mentally incapacitated.[572]

Meanwhile, Huxley was surrounded by researchers whom he regarded as his inferiors. Ready to attend the 1852 British Association meeting in Belfast, Huxley wrote Henrietta that he had to attend. For there were people there whose reputation exceeded them – "your Hal rather flatters himself that *per contra* – he is better than his reputation".[573] To be sure, Huxley's reputation was increasing, with successful papers, an FRS and even a Royal Medal in November 1852 – but these made little or no money. There is no doubt that Huxley was an extraordinarily hard worker, skilled writer and researcher, and a tactical wizard. But it is insufficient to point to these attributes as an explanation for his rise to fame. In the same vein, it is also insufficiently explanatory to ascribe Huxley's attacks upon Owen as caused by Owen's 'sneaky' personality, something strongly smacking of *post hoc* apologetics.[574]

Instead it was a combination of these facets which led to Huxley's attacks upon Owen: Huxley's social position, his love of fighting, and the technical aspects of this

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[571]Ironically the is the same situation that Richard Owen found himself in when Mrs. Clift refused to let him marry Caroline until he made more than £120 a year.
[572]A. Desmond, *Archetypes and Ancestors : Palaeontology in Victorian London, 1850-1875* (Chicago: University of Chicago Press, 1984), pp. 27-28.
[573]T.H. Huxley to H. Heathorn, 28 Aug 1852, T.H. Huxley - Henrietta Heathorn Correspondence, Imperial College, London, Huxley Archives, HH 221.
[574]For a view of Huxley's own "sneaky" personality, see his treatment of H.C. Bastian when he promoted spontaneous generation - J. Strick, "Darwinism and the Origin of Life: The Role of H.C. Bastian in the British Spontaneous Generation Debates, 1868-1873," *Journal of the History of Biology* 32, 51-92 (1999): pp. 80-83.

dispute fed upon each other. In fact Huxley's attacks were a noisier version of what Owen had himself carried out in the 1830s – Bourdieu's "subversion strategy." Owen made his own reputation by being a sort of broker of all French practices of comparative anatomy and physiology – and so Huxley copied Owen's strategy, but by using many German practices as his exemplar. By imitating Owen he similarly redefined what was considered good research while simultaneously advancing his own reputation: what constituted first-rate work was the Germanizing perspective which depicted analysis as antiquated. As a subversive, Huxley not only rejected Owen's version of research but additionally refused to recognize Owen's legitimacy. And he sought to convince others to reject Owen's version of research too: their struggle was thus a battle over the very definition of good comparative anatomy and physiology,[575] and so they would differ over the relevance of different types of evidence.

What Owen considered to be valid evidence, Huxley would frequently consider less relevant, and vice versa. This context of the palaetiologist Huxley attacking the analyst Owen adds a new dimension to previous work depicting Huxley as a challenger to Owen and the institutions of the Anglican establishment. The framework shown above builds upon Rupke's observation that Huxley attacked Owen because Owen represented the scientific "establishment;" he mentions that Owen's program, which appeared cutting-edge in the early 1850s, suddenly went into decline later in the decade, and mentions that this decline was caused by Owen's failure to cultivate a coherent group of followers. Different habits of reasoning coloured how different people viewed the nature of scientific institutions, contested cultural authority. Seeing the dispute between Huxley

[575]Bourdieu, "The Specificity of the Scientific Field," pp. 38-39.

and Owen in this light also supplements Theodore Merz's insight that around 1860 the "morphological period" was replaced by inquiries into how forms changed.[576]

Huxley's palaetiological definition of the individual, and Carpenter's similar reinterpretation of the likeness between a polyp and a plant, informed their criticisms of Owen as they attempted to redefine what passed as good research. By 1852, their writings already objected to aspects of British comparative anatomy. Huxley, strongly dissatisfied with his place in the scientific order, would use these objections over the next few years as levers, both to reinforce his social position and to change the criteria for what constituted good research.

One of Huxley's first converts was Carpenter. Carpenter was becoming alienated from Owen, ironically over who was the first to use von Baerian embryological principles. Huxley's introduction to his 1853 translation of von Baer's work noted that Carpenter was the only English physiologist to use von Baer's writings.[577] In that same year Carpenter informed Owen that in a forthcoming publication he would claim to have been the first to use von Baer's law (in 1845); hopefully the *Quarterly Review* would "*then* give me the credit for sometimes presenting myself as an original 'discoverer'". Carpenter prostrated himself, promising not only to publicly recognize Owen's priority in using von Baer's law, but also promising to show him his proofs, hoping that Owen would find them "unexceptionable".[578]

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[576]Merz, *A History of European Thought*, pp. 2:270-275; Rupke, *Richard Owen*, p. 211.
[577]von Baer, "Fragments," p. 176.
[578]W.B. Carpenter to R. Owen, 20 Oct 1851, OCORR, NHM, 6/333-334; W.B. Carpenter to R. Owen, 2 Aug 1853, OCORR, NHM, 6/335-336.

Yet by 1854 Carpenter was in full retreat about his claim to priority: he would no longer make any claim to von Baer's principle, and moreover he would not even try to be original. At least his textbook was popular, concluded a sad Carpenter:

> It is probable that the rest of my life will be so much occupied in Educational matters, that I must be content to see younger men taking the place that I had hoped to occupy as a *discoverer*, and satisfy myself with endeavouring to qualify them for a philosophical appreciation of what they may have the good fortune to find out.[579]

Huxley was one of these "younger men," and by June of 1854 his fortunes had finally begun to rise. He had finally attained a paying scientific position: Edward Forbes, who had just begun lecturing at the Royal School of Mines on Jermyn Street, became professor of Natural History in Edinburgh and had Huxley finish his course. By July Huxley was appointed lecturer on natural history at the Royal School of Mines.[580]

Carpenter began to strengthen his connections with Huxley while holding up Owen as a negative exemplar of how science should be practiced. He criticized British science against an idealized Continental (mostly Germanic) science, and privately mocked the second edition of Owen's *Lectures on Invertebrates*. Busk and Carpenter "*roared* over his absurdities" which were put forward as the state of British science circa 1855: "What *will* the Continentals think of us?" he asked Huxley in a letter.[581] Huxley began to call upon other supporters too: George Allman admitted that he hadn't seen Owen's book yet, but "what you tell me does not in the least surprise me". Allman's caution to publicly criticize Owen was rooted in his self-interest, for Owen was helping him obtain the Chair of Natural History at Edinburgh, recently vacated by E.S. Forbes's

[579]W.B. Carpenter to R. Owen, 11 Feb 1854, OCORR, NHM, 6/337-338. Emphasis in original.
[580]Weldon, "T.H. Huxley."
[581]W.B. Carpenter to T.H. Huxley 16 Jul 1855, HP, IC, 12.78-79.

untimely death.[582] Indeed, Forbes's death in 1854 meant that Owen had lost a prominent ally in the support for compound individuality.

A fascinating new outlet for this small crew of subversive palaetiologists was the specialist journal, which provided outlets for attacks on the old analytical habit while simultaneously strengthening the palaetiologists' own communal self-awareness. So while Owen wrote articles for large audiences in prestigious reviews such as the *Quarterly,* the palaetiologists clawed away at Owen in these medical and naturalist journals. W.B. Carpenter, as editor of the *British and Foreign Medico-Chirurgical Review,* now had Huxley writing for him. In this journal, Huxley attacked Schleiden and Schwann's granting of individuality to each cell, stating that "before histology can be said to be complete, we must have a histological *development* as well as a histological *anatomy.*"[583] Huxley had already sniped at Owen in his anonymous review of the *Vestiges of the Natural History of Creation,*[584] and now Carpenter had Huxley anonymously review Owen's despised new edition of *Lectures on Invertebrates.*

Huxley rejected his work on invertebrates as a poor attempt to colonize a foreign domain. It would be best if famous researchers confined themselves to one area, he sniffed: though the authority acquired in one field might give their forays into other fields some prestige, they could be dangerously misleading if done badly. This observation

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[582]G. Allman to R. Owen, 6 Dec. 1854, OCORR, NHM, 1/126-127; G. Allman to T.H. Huxley, 1855, HP, IC, 10.74-75. Allman was successful; by 1855 he was the Regius Professor of Natural History at Edinburgh. G. Allman, *Introductory Lecture delivered to the Students of the Natural History* (Edinburgh: A. & C. Black, 1855).

[583]Huxley, Foster, and Lankester, *Scientific Memoirs*, pp. 1:247-248; this is a reprint ofT. H. Huxley, "The Cell-Theory," *British and Foreign Medico-Chirurgical Review* 7, 285-314 (1853). Ironically, note that Huxley is paraphrasing Schleiden's own statements (which he himself copied out) about the need for morphology being guided by development alone. See also Richmond, "Huxley's Criticism of German Cell Theory." for a more detailed look at Huxley's criticism.

[584]This was in retaliation for Owen's "ill-natured" attack on Quekett (a "very inoffensive man") in his anonymous criticism of Lyell in the 1851 *Quarterly Review.* T.H. Huxley to W.S. Macleay (Draft), 9 Nov 1851. Desmond notes that this review probably marked the outbreak of hostilities between Huxley and Owen. Desmond, *Archetypes and Ancestors*, p. 38.

could be applied to Owen: while Owen was "second to none" in vertebrate anatomy, Huxley thought that his work in invertebrate zoology had not kept up with the rapid "advances" in it. But rather than credulously take Huxley at his word one must sceptically consider precisely what constituted an 'advance'. Huxley recommended Siebold's new (1854) textbook on invertebrates as the best available. Here, Siebold said that it was only through histology and embryology that one could correctly interpret invertebrate organs – as these organs often had no similar analogues in higher animals, development was the only way to determine whether an organ was really a kidney, ovary or liver.[585]

Second, Huxley took the opportunity to support his new crew. He referred to two previous articles in the *British and Foreign Medico-Chirurgical Review* - anonymously penned by Carpenter[586] – to support his assertion that Steenstrup's alternation of generations and Owen's views on parthenogenesis were both flawed. Huxley noted that any of Allman's discussions on compound hydrozoa went unmentioned in Owen's work. Was this omission because Allman dissented from Owen's parthenogenesis, Huxley wondered? Finally, he took the opportunity to bolster his own reputation a bit, asking if Owen had also neglected to mention any work by Huxley because of "Mr. Huxley's very pointed repudiation of the whole doctrine of Alternation of Generations, and of Professor Owen's parthenogenetic modification of it". The anonymous reviewer mentioned his own name seven times in a single paragraph.[587]

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585 Huxley, "Owen and Rymer Jones on Comparative Anatomy," pp. 2-3; Siebold, *Anatomy of the Invertebrata*, pp. ix-x.
586 These were the same articles where Carpenter reinterpreted the resemblance of lower organisms to trees, defining both as individuals that began and ended with sexual reproduction.
587 Huxley, "Owen and Rymer Jones on Comparative Anatomy," pp. 14, 18-19.

Third, Huxley attacked the meaninglessness of Owen's classifications. He complained that Owen ignored other naturalists by keeping intestinal worms out of the articulate group – in so doing he was like his "confused" protégé, Rymer Jones, who used a "now-exploded" principle of classification. Huxley conveniently neglected to mention the exploded principle – of nervous system complexity as a taxonomic index – let alone the rationale for its use.[588] He then used Carpenter's palaetiological view of a plant to attack Owen. Just as one couldn't regard a plant without its flowers as a complete individual, neither could a naturalist regard only a polyp as a complete individual without its medusan (sexual) form as well. And Owen was mistaken in trying to determine homologies merely of adult animal forms, not the entire developmental cycle.[589]

Huxley's attacks on Owen's doctrine of spermatic force were to continue. In 1858 Huxley attacked Owen's poverty of examples. He protested that Owen had generalized from only two exemplar organisms, *Hydra* and the aphid. But Huxley made this statement in bad faith; as shown above, he was indeed aware of Owen's extension of parthenogenesis to other organisms (such as *Nais*) when he took notes from *On Parthenogenesis* on HMS *Rattlesnake.*[590]

Huxley complained that Owen's histological observations were too vague. After all, structures resembling "unaltered germ cells" lay in places like our own sub-epidermal tissues - "nevertheless, no one feels any alarm lest a nascent wart should turn out to be an

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[588]Huxley, "Owen and Rymer Jones on Comparative Anatomy," pp. 14-15.Earlier, Huxley had made complaints like this in private – in 1851 he complained that Owen's solution to the confusion of radiates – their division into *Nematoneura* and *Acrita* – was not helpful, he didn't think that general propositions were implied in this division. So he may have been ignorant of Owen's use of nervous structure as a taxonomic index. T. H. Huxley, Manuscript, "Arrangement of Radiata", [1851], HP, IC, 37.43-52.
[589]Huxley, "Owen and Rymer Jones on Comparative Anatomy," pp. 16-18.
[590]T. H. Huxley, "On the Phenomena of Gemmation," *Proceedings of the Royal Institution* 2, 534-538 (1858): p. 537; Huxley, "Notes on Owen's Parthenogenesis".

heir."[591] And he asked why parthenogenesis didn't produce full-grown Minervas from our spermatic-force-laden scalp![592] These were superb, if misleading, rhetorical points – Huxley was associating all germ-cells with the production of new individuals, when in the Owenian scheme these "unaltered germ-cells" would probably be used only in regeneration. More importantly, Huxley had invoked a familiar example from everyday human experience to discredit the possibility of producing new individuals asexually. Huxley was engaging in the very sort of thought he had decried in 1852, playing to those "only aware of vertebrates"[593] and their reproductive patterns, using them to mould the boundaries of what was thought possible in the far stranger kingdom of invertebrates.

Huxley's other criticism was a familiar one to subversives unwilling to play by the rules of earlier researchers: he decried the "metaphysical" position and terminology of his opponent. Huxley complained that the "spermatic force" was merely a verbal flourish, an undetermined force with no scientific value.[594] Huxley's righteous indignation about the use of unseen forces was merely for public consumption, however. For privately, in that very same year, he himself proposed an unseen entity to J.D. Hooker, an invisible "plane of differentiation" from which cells metamorphosed into specialized tissue.[595]

The criticism about metaphysics and unseen entities can thus be seen as a ruse. Mario Biagioli's examination of Descartes's and Galileo's similar critique of Aristotelians – the complaint that their terms were meaningless - gives us a valuable

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[591]Huxley, "On the Phenomena of Gemmation," p. 537.
[592]T. H. Huxley, "Lecture 2 on General Natural History," *Medical Times & Gazette* 33:868 (n.s. #307), 481-484 (1856): p. 482.
[593]Huxley, "On Animal Individuality", p. 38.2.
[594]Huxley, "On the Phenomena of Gemmation," p. 538.
[595]This could be in either animals or plants. T.H. Huxley to J.D. Hooker (Draft), Jan 1858, HP, IC, 2.29-32.

insight into Huxley's attacks. Biagioli believes that the complaint about metaphysics is not merely a rhetorical strategy – it can also be taken as a statement that there is no possibility of communication or compromise.[596] Huxley's criticism of Owen's unseen spermatic force – as well as his strong moralizing tone – showed his unwillingness to negotiate with Owen, and it also acted as a vehicle to strengthen the cohesion of Huxley's own crew while reinforcing Owen's status as an enemy.

A poignant reminder that Owen paid very close attention to the subversive palaetiologists' criticisms is shown in his annotated copy of *On Parthenogenesis*. There, he cut out a passage from an unnamed text and pasted it on the immediate inside cover. The passage read:

> ALTERNATION OF GENERATIONS. - You are wrong. The idea involved in the term unhappily is still expressed in our "text-books." The researches of Huxley, Quatrefages, and a host of others showed that the sum of all the phases of an ovum is one animal.

Underneath this passage, Owen wrote the note: "So that one animal may consist of many individuals, or 'many individuals in one individual.'"[597] Owen did not recognize or understand Huxley's new definition of an individual – indeed, in all of Owen's private notes and publications, he never once used the term "zoöid".

Owen mounted few public defences of parthenogenesis and spermatic force. One interesting place where Owen did respond was in an 1857 translation of von Siebold's book on parthenogenesis. Noting that Siebold's topic - the phenomenon of reproduction

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[596]M. Biagioli, *Galileo, Courtier : the Practice of Science in the Culture of Absolutism* (Chicago: University of Chicago Press, 1993), pp. 211-212, 235-240.

[597]Owen, "On Parthenogenesis (Annotated)". It is unclear what journal this cutout was from, or when it was pasted into Owen's annotated copy. Quatrefages's *Métamorphoses de l'Homme et des Animaux* appeared in French in 1862 and English in 1864, so Owen's note may have been written after this.

by virgin females[598] - could also be described by Owen's term "parthenogenesis," the translator, W.S. Dallas, thoughtfully submitted his proof-sheets to Owen. Owen "enriched them with some valuable notes" and in turn related Siebold's text to the work of John Hunter.[599]

Owen's attempt to forge a link between his work, Siebold's, and Hunter's can be seen once again as a conservative tactic used by dominant members of a field preserving their research habits. It followed his earlier attempt to 'domesticate' the habits of analysis and synthesis to a British context by presenting Hunter as being informed by this method. As young subversives held up foreign, Continental, research as an example of what good research ought to become, conservatives like Owen pointed to past examples of glorious domestic research in order to link their own work with these Britons. This tactic showed that their habit of reasoning was acceptable because it worked successfully in the past. Owen's use of von Siebold - the very same exemplar Continental researchers used by the subversives – can still be seen as conservative in that Owen subsumed aspects of Siebold's work into part of a grand British tradition dating back to Hunter.[600]

Owen's other attempts to comment upon Siebold's text were mostly defences of his term. When Siebold objected to "parthenogenesis" as he could not understand what reproduction in asexual larval creatures meant, Owen enriched Siebold's text with an

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[598]Siebold claimed that a male insect's semen often remained for years in a "seminal receptacle" in female insects, capable of impregnating eggs. This new evidence explained why a queen, fertilized only once at coitus, could lay eggs for years. Viviparous aphids lacked these seminal receptacles, which oviparous aphids possessed. C. T. E. v. Siebold, *On True Parthenogenesis in Moths and Bees*, trans. W. S. Dallas (London: John Van Voorst, 1857), pp. 4-5.
[599]Siebold, *On True Parthenogenesis*, pp. v-vii.
[600]These strategies are in no way limited to Owen – for instance, Huxley's later work on David Hume might be seen in a similar way. Hume became a resource for Huxley, a way for him to portray himself as an heir to Hume's scepticism. This supported his agnosticism and rejection of various "metaphysical" fancies, while also showing him as fighting to preserve a distinguished British philosophical tradition. B. Lightman, "Huxley and scientific agnosticism: the strange history of a failed rhetorical strategy," *British Journal for the History of Science* 35, 271-289 (2002): p. 277.

enormous footnote. Where Siebold restricted reproduction to females with "true" ova only, Owen stated that he did distinguish between sexual and asexual organisms, but his focus was on a "spermatic virtue" bestowed by "ancestral coitus", which became exhausted in proportion to the complexity of the tissues produced. When Siebold charged that Owen had confused reproduction by fertilized ovum, and reproduction by germ-cell division (Siebold thereby emphasizing the distinction between sexual and asexual reproduction) – Owen again responded. The point of "Parthenogenesis" was that the "germ-masses" of "nucleated cells" (which became an organism) were histologically the same whether they came from a sexually fertilized ovum or the "spontaneous fission" of an asexual germ.[601] In other words if one did not look at the cells as they changed over time, then they appeared identical.

In private notes, Owen noted that parthenogenesis applied to either sex, minimizing the importance of whether one's reproductive germs were "true" ova or not. And he repeated the same exemplar animals. Tapeworm reproduction was parthenogenetic because when two or more buds emerged they could either adhere to the parent or detach. Parthenogenesis also explained the emergence of new annelid individuals which budded from the penultimate joint of their parents. Salps reproduced parthenogenetically too as they alternated between single and multiple salp-individuals, "a succession of little Salpae".[602]

In his annotations of the second edition of his *Lectures on Invertebrates*, Owen extended parthenogenesis to other exemplar animals which Huxley had reinterpreted. One was *Pyrosoma* (an animal related to salps and sea squirts) in which the closed end

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[601]Siebold, *On True Parthenogenesis*, pp. 10-11.
[602]Owen, "On Parthenogenesis (Annotated)".

"of the association / colony" was formed by four primitive individuals. And Owen mentioned salps once again, in which "agamous" (non-sexual) salps were held to be equivalent to sexual salps. He thought them equivalent, for although the sexual salps possessed a true ovum that could be fertilized, they lacked the "faculty of gemmation" which the asexual salps possessed[603] – meaning that both could produce the requisite cell-mass from which new organisms could be formed. Therefore Owen saw sexual reproduction as equivalent to asexual reproduction.

As these comments were private, they could do little to convert others. But in 1858 Owen did go public with some of his observations, using the venue of the Presidential Address to the BAAS to attack what he saw as the growing palaetiological hegemony in comparative anatomy. Embryology alone should not be a decisive test of homology – though important in determining the true position of lower invertebrates due to their larvae, like barnacles, he nonetheless thought it was over-valued and warned of cases where embryology might not work. And Owen publicly restated the tenets of his parthenogenetic / metagenetic programme. *Metagenesis* referred to the changes of form that the representative of a species went through in successively agamically-propagating individuals; *metamorphosis* instead referred to the changes of form that the representative of a species went through in a single individual. [604]

Owen fell back upon celebrated cases to emphasize his position: he had confirmed Bonnet's researches on aphids, and there was no histological distinction between buds

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[603]R. Owen, Manuscript, "Lectures on the Comparative Anatomy and Physiology of the Invertebrate Animals (Annotated)", 1855, OCOLL, NHM. Notes appear on interleaving facing p. 482 and on interleaving facing p. 484.

[604]He warned that embryology alone might not be a decisive judge of homologies: developing cephalopods, for example, resembled vertebrates, not the gastropods to which they properly belonged. Evelleen Richards also notes that Owen didn't use embryology as the sole criterion for homology. Owen, "Presidential Address," p. lxxvi; Richards, "A Question of Property Rights," p. 170fn.

and eggs. Parthenogenetic phenomena in *Hydrozoa* and *Entozoa* showed that animals differing in form were really two parts of a "metagenetic transformation," the young propagated by buds and seeds, just like plants. And Owen brought up Hunter again: hadn't he enunciated the principle that plants could propagate in two ways – by seeds and by division? Didn't Hunter show that every part of a vegetable was a whole, and could be multiplied as far as it could be divided; while at the same time certain plant parts could become reproductive organs, producing seeds? To drive home his case for metagenesis, Owen then cited research by Trembley on *Hydra* and Spallanzani on *Nais* to show that propagation by both fission and buds occurred in animals.[605]

HISTOLOGY: GERMS AS BUDS / OVA

But by 1858, Owen's use of these exemplars and the researchers associated with them had become rather dated. For during the 1850s new attempts were made to determine the difference between sexual and asexual reproductive germs. Since Huxley had introduced an historical, sexual, account of individuality as the product of a fertilized ovum, he became more interested in the specific location where sexual fertilization was supposed to occur and that single point where individuality began: the ovum.

In Huxley's 1858 discussion on aphids, he agreed with Owen on the difference between ova and buds – that there was no histological difference between them. But for Huxley this structural identity was no longer an important point – instead, he argued that researchers ought to pay attention to the physiological difference between ova and buds. The ova required fertilization to proceed, while the bud did not (it spontaneously turned

[605]Owen, "Presidential Address," pp. lxxiv-lxxv, lxx-lxxii.

into an embryo). "Physiological" can be read in Whewell's palaetiological sense – as a sequence of changes over time. Huxley also coined new terms to emphasize temporality and sexual reproduction, using the neologism "pseudova" to denote reproductive germs not requiring sexual fertilization.[606] In other words, Huxley denoted a type of reproductive germ by a feature that it lacked as a way to convey the importance of sex.

Around the same time, the young John Lubbock was examining the same question – in 1857 concluding that there was "sufficient evidence of the fundamental identity of eggs and buds...". But in his next words, Lubbock put them in a palaetiological context "…or rather of their forming parts of one and the same series, a point on which I would particularly insist".[607]

Lubbock's work had been privately shifted by Huxley from an Owenian stance to one closer to his own view. The "rounded bodies" that Lubbock observed at one point in reproduction he first called "mother cells"; like Owen's nucleated "cell-masses", they were receptacles of spermatic virtue. But Huxley intervened and Lubbock changed his mind, calling them "ovarian masses" instead, emphasizing the requirement of sexual fertilization.[608] Indeed, before Lubbock submitted his paper on the identity of eggs and buds to the Royal Society, he submitted a passage to Huxley for approval. He further promised to let Huxley see the exact words before they were printed, so Huxley had at

[606]Churchill, "Sex and the Single Organism," p. 151; Huxley, Foster, and Lankester, *Scientific Memoirs*, p. 1:323. This is a reprint of Huxley, "On the Phenomena of Gemmation," p. 536.
[607]J. Lubbock, "An Account of the Two Methods of Reproduction in Daphnia," *Phil. Trans.* 147, 79-100 (1857): p. 99.
[608]J. Lubbock to T.H. Huxley, 8 Dec 1856, HP, IC, 22.54.

least two chances to change Lubbock's wording. As we see below, he used them.

> LUBBOCK'S *PROPOSED* WORDING, 1856
> Prof Huxley also might at first be supposed to advocate the essential difference between eggs and buds, since he has proposed to designate the whole product of each egg or rather each act of coition, as the individual and to call the separate specimens obtained by budding or Parthenogenesis, zooids.... Huxley however proposes the nomenclature merely as being in his opinion the most convenient and not expressing any essential difference between eggs and buds, which he, like Prof. Owen, believes merge imperceptibly into one another.[609]

> LUBBOCK'S *PUBLISHED* WORDING, 1857
> Prof. HUXLEY might be supposed to agree with the naturalists as to the essential difference between eggs and buds above-mentioned, since he has proposed, developing the idea of which we owe the germ to DR. CARPENTER, to extend the use of the word "individual" to the whole product of one impregnation, and to designate as 'zooids' the independent forms of the individual. Prof. HUXLEY, however, proposes this system of nomenclature merely as convenient, and not as expressing any fundamental, structural, or potential difference between eggs and buds.[610]

Lubbock's published version differed from the original in several ways: W.B. Carpenter was now mentioned, a symbolic reward upon a member of Huxley's crew; by contrast, Owen's name disappeared, as had the term "parthenogenesis." And the difference between sexual and asexual reproduction was sharpened, while nonetheless acknowledging the caveat that there was not even a "potential difference between eggs and buds." Lubbock was not entirely Huxley's creature, complaining that Huxley's definition of "individual" as the product of a fertilized ovum was inconvenient and confusing. Yet by 1859 Lubbock was to also agree with Huxley's term "pseudova" as applicable to those "eggs which do not require impregnation."[611]

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[609]J. Lubbock to T.H. Huxley, 19 Dec 1856, HP, IC, 22.60-61.
[610]Lubbock, "Two Methods of Reproduction in Daphnia," p. 99.
[611]Lubbock, "Two Methods of Reproduction in Daphnia," p. 99; idem, "On the Ova and Pseudova of Insects," *Phil. Trans.* 149, 341-369 (1859): p. 341.

Frederick Churchill has shown that by 1858, researchers were aware of a stunningly wide variation of reproduction, of which sexual reproduction in sexually separate individuals through the fertilization of eggs made up only a small part.[612] Yet a new generation of elite British researchers nonetheless insisted upon the primacy of sexual reproduction, an insistence belying this broad range of reproductive models. Their insistence coloured even the definitions of individuality and how one designated localized reproductive germs.

Churchill's point has been placed in a larger framework. Things such as the re-emphasis upon sexual reproduction were part of a larger emphasis upon palaetiology and the struggle to define an individual. For the palaetiologists, the sexual act served as the single point by which one of nature's primary units, the individual, could be determined. The elements discussed in the habit of analysis and synthesis - "individuals" - were no longer to be defined statically but dynamically. Likenesses between different instances of "individuals", which included the resemblance of plants to lower invertebrates, were redrawn accordingly, an understanding of them shifting from pattern to process. Trees, for example, were no longer compounds of buds. In turn we can situate many of the familiar struggles between different researchers – between the establishment figure Owen and the scientific naturalist Huxley who challenged him, for instance – in this new context. The new emphasis upon sex and palaetiology furthered the career goals of a new 'generation' of researchers.

By the late 1850s, Huxley's challenge to Owen as a way to boost his reputation was starting to bear fruit. People to whom he had written flattering letters in the 1840s and early 1850s were now writing thankful letters to him. John Goodsir, receiving a copy

[612]Churchill, "Sex and the Single Organism," p. 166.

of Huxley's 1858 lecture "On the Phenomena of Gemmation," wrote that he "scarcely deserve[d] to be remembered by you in this way!" George Allman, who penned testimonials as Huxley searched for positions in the early 1850s, eventually found himself writing letters to Huxley justifying comments he had made (after an attack on him by E. Ray Lankester).[613] Huxley defended the interests of the young, receiving effusive letters of thanks (addressed to "Reverend Sir") from new clients such as Michael Foster. And he protected the old, obtaining a civil list pension for the impoverished Thomas Wharton Jones, his first physiology teacher.[614]

By the late 1850s Huxley was also beginning to move away from his specialization in humble marine invertebrates. Despite his earlier complaints about the vertebrate comparative anatomist Owen leveraging his reputation to write textbooks on invertebrate biology, Huxley himself now began to move the other way. After chasing Owen out of the field of invertebrate biology entirely, Huxley set out to colonize the field of vertebrate comparative anatomy and palaeontology. This move was partly because of his job duties at the School of Mines, where he taught about vertebrate fossils. But vertebrate anatomy was also more prestigious because of its greater association with medicine, giving its practitioners the right to discuss the medical, social, and cultural matters that came with it.[615] By the 1859 publication of his monograph on the *Oceanic Hydrozoa* – published at the stately pace of nine years after his return to London on the

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[613]J. Goodsir to T.H. Huxley, 17 Jul 1858, HP, IC, 17.80; George Allman to T.H. Huxley, 1880, Huxley Papers, Imperial College London, 10.80-81.
[614]W. F. Bynum, "Thomas Wharton Jones (1808-1891)," in *DNB* (1993); M. Foster to T.H. Huxley, April 1870, HP, IC, 4.173-175.
[615]Thus see T. H. Huxley, "On the Theory of the Vertebrate Skull," *Annals and Magazine of Natural History* 3, 414-439 (1859).

Rattlesnake - he exclaimed that he felt "like the editor of someone else's posthumous work."[616]

Despite this move into vertebrate research and then grander meditations on politics, culture and the role of scientists in society, Huxley never forgot the bugbear of Owen's spermatic force. Churchill has noted a similarity between Owen's spermatic force and August Weismann's theory of the continuity of the germ plasm. Weismann himself acknowledged this similarity in 1892,[617] and it appears that Huxley saw the resemblance too. But he didn't see the resemblance in the new context of heredity: instead Huxley saw Weismann's germ plasm as a sort of persistent force like Owen's, where the germ plasm instead affected complexity and growth.

In his notes, Huxley directly matched Weismann's statements against similar quotations from Owen's *On Parthenogenesis*.[618] For Huxley, the difficulty for Weismann was to localize the cells in which the germ-plasm was located: just as Owen hypothesized that spermatic force was located in certain "nucleated cells" deployed about the body, Weismann had to demonstrate this too. To illustrate Weismann's flaws, Huxley cited examples of organisms lacking specialized generative organs. Most moss-cells could become independent moss plants on their own, new individuals; since these new plants contained germ cells when they matured, one might assume that all, or nearly all, of these cells contained Weismann's germ plasm. But Huxley's second exemplar organism was more telling: hydroids, such as the polyp, had no specialized generative organs either, and like the moss cells any part of a hydroid could become a new

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[616]T.H. Huxley to J.D. Hooker, 5 Sept 1858, HP, IC, 2.35-38; T. H. Huxley, *The Oceanic Hydrozoa: a Description of the Calycophoridae and Physophoridae Observed during the Voyage of H.M.S. "Rattlesnake", in the years 1846-1850* (London: Ray Society, 1859).
[617]Churchill, "Sex and the Single Organism," pp. 172-173n15.
[618]T. H. Huxley, Manuscript, "Weismann and Keimplasm", ND, HP, IC, 41.118-41.123, p. 41.118.

individual. He concluded that because the germ-plasm could be located anywhere, Weismann's hypothesis was fatally flawed, just like Owen's spermatic force.[619] Huxley's return to the context of compound individuality - and his choice of an old favourite exemplar organism - showed his habituation to old battles and old enemies.

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[619]Huxley, "Weismann and Keimplasm", pp. 41.122-123, 41.119.

CHAPTER 5
THE WORLD OF HERBERT SPENCER

At the beginning of the dissertation I set out the task of trying to understand Herbert Spencer's alien *System of Synthetic Philosophy* by setting it within the common context of compound individuality. After setting out this common context in the first four chapters, this chapter tries to make Spencer's writings less alien by relating them to this context. This part therefore can be read as a 'primer' to his work, especially displaying the interaction of his biology with his social theory.

In both biology and social theory, Spencer saw the interaction of the individual with the group as being of fundamental importance. Because of this problem and because of the problems of compound individuality faced by previous researchers, he also meditated upon the nature of individuality and offer a solution. Spencer was to define individuality as a perspective relative to the observer and as relative to an environment. Units at different levels of organization, like a cell, a person and a solar system, could be defined as individuals – thus even a group of units that met Spencer's criteria for an individual could itself be seen as a higher-order individual.

Spencer's meditations upon individuality were shaped by his early habituation to analysis and synthesis. This is exemplified by his infatuation with Euclidean geometry. His gigantic *System of Synthetic Philosophy,* which applied 'evolution' to psychology, philosophy, biology, education, sociology and ethics, can be seen as a sort of Euclidean system writ large. It can also be read as a gigantic tribute to the father and uncle who so influenced him, for his *System* revisited their attempts to simplify geometrical teaching, shorthand and political radicalism upon simpler analytical foundations. Spencer's work always began with simple principles and definitions and then synthesizing increasingly complex proposals from these simple principles.

Because of this early habituation to analysis and synthesis this chapter depicts the early Spencer as an 'Owenian.' Through his use of the analytic and synthetic habit of reasoning and his father's acquaintance with Thomas Rymer Jones, Spencer met and appreciated the research of Richard Owen as early as 1850 and probably before this. Spencer's early articles extended Owen's insights into metagenesis and were read by contemporaries as doing this.

However Spencer's deepening friendship with T.H. Huxley meant that he had to choose between two rapidly diverging social alternatives, and he chose Huxley over Owen. Cognitive change accompanied social change: at the same time a palaetiological emphasis crept into Spencer's work. Thus by the early 1860s he characterized 'evolution' as progressive "integration" (an 1855 Spencerian terminiological invention denoting synthesis) and differentiation (a palaetiological stance). Between the late 1850s and about 1870, then, Spencer tried to reconcile the analytic and synthetic habit and the palaetiological habit as both parts of the evolutionary process.

These two opposing developmental directions, one centripetal and one centrifugal, presented conflicting entailments. Even Spencer recognized this conflict. For these different entailments allowed different audiences to appropriate different messages from his evolutionary writings; different groups deployed different habits to support different social projects.

I claim that on the one hand the centripetal habit of analysis and synthesis – reformulated as the Spencerian process of 'integration' - was used to support spontaneous social order emerging from the voluntary interaction of constituent parts, thus justifying political doctrines like anarchism or *laissez-faire* market approaches. I will also claim that the centrifugal habit of palaetiology – reformulated as the Spencerian process of 'differentiation' – was used to legitimate professionals, 'professionalization' and specialization. In turn it emphasized the fragmentation of society, depicting social order as a form of anarchy requiring rational guidance.

Around 1870 Spencer noted these two different entailments and started to rethink the relationship of differentiation and integration: he believed that the habit of analysis and synthesis and the habit of palaetiology could not be reconciled. Spencer thus started to explicitly emphasize the importance of synthesis, of 'integration,' over differentiation in the evolutionary process. This change meant that he began to move back towards analysis and synthesis.

But Spencer's retreat meant a growing schism between cognate ideals of social order. From 1871 onwards a conflict grew between Spencer and his friend Huxley: this occurred because Spencer was moving back towards synthesis. Thus this cognitive schism led to a social schism. In 1871 Huxley moved against what he saw as Spencer's *laissez-faire* politics by attacking it at its weakest point: he began to undermine Spencer's advocacy of compound individuality. This weakened Spencer's proposal of a liberal "social organism" formed out of autonomous parts.

Huxley moved to make biological individuality self-evident by ensuring the uptake of several new biological terms. As a result this terminological innovation made Spencer's image of a social organism with autonomous parts more and more alien. Huxley's redefinition began to work: despite Spencer's characterization of individuality as a relative concept he was increasingly interpreted as an "individualist" whereby individuality was defined as an atomistic freedom from a group. The chapter ends by considering how the 20th century interpretation of Spencer reinforced this image of a "contradiction" of a liberal social organism: because historians unwittingly engaged with Spencer's work using the Huxleyian terminology of the self-evident biological individual.

PART I: ANALYSIS, SYNTHESIS AND SPONTANEOUS SOCIAL ORDER

GEOMETRY AND SHORTHAND

Herbert Spencer was an only child who lingered in the company of grown-ups rather than associating with other boys his own age. A member of a Derby family that was involved in political, religious and scientific questions, he watched family members debate earnestly over these matters.[620]

The most important figures in Spencer's early life were his uncle, Thomas Spencer, and his father, William George Spencer. Thomas Spencer was a rogue Anglican Minister who issued best-selling tracts calling for the right of local parishes to elect their minister and who said grace at the first and last Anti-Corn Law League banquests. Meanwhile Spencer's father William George Spencer was a failed lace manufacturer who made a living as a well-regarded schoolteacher. Hoping to get rich, he continued to

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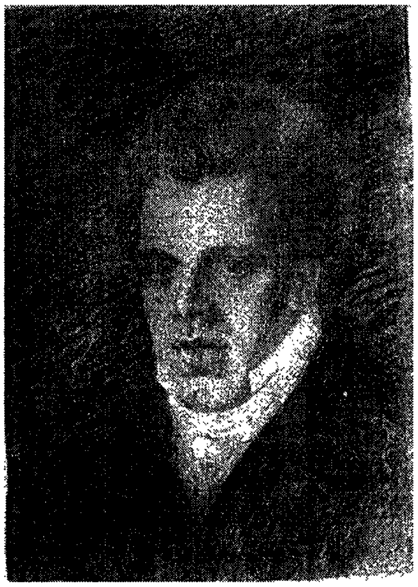
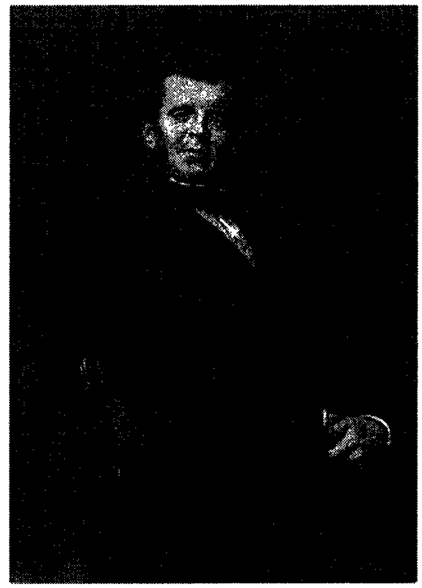
[620]D. Duncan and H. Spencer, *Life and Letters of Herbert Spencer* (London: Methuen, 1908), p. 11.

dabble in invention with Spencer.[621] Spencer appears to have been estranged from his long-suffering mother Harriet Spencer, something that he was to later regret.[622]



Reverend Thomas Spencer, n.d. Herbert Spencer Papers, University of London Library, MS 791/354 f.5.



William George Spencer, 1840. Herbert Spencer Papers, University of London Library, MS 791/354 f.2.

Thus as a boy Spencer was deeply involved in two of his father's inventions: the perfection of a simplified version of shorthand and a new way of teaching Euclidean geometry. It was hoped that money could be made from these systems as they were both deemed to be improvements on existing forms of shorthand and geometry education. They were deemed to be improvements because both of them followed analysis and synthesis, and it is from these inventions that Spencer became habituated to analysis and synthesis.

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[621]H. Spencer, *An Autobiography*, 2 vols. (London: Williams and Norgate, 1904), pp. i:65, Chapter 2; T. Spencer, *Practical Suggestions on Church Reform* (London: John Green, 1841), pp. 9-10.

[622]Spencer, *ABHS*. Thus the saddest letter in the Herbert Spencer collection is a letter from William George Spencer to Herbert Spencer, dated 1866 – the year of W.G. Spencer's death. He first discusses an injury to Harriet Spencer's finger, then his recovery from an apparent heart attack and then how the mucous around his larynx has diminished. This segue aside, the letter ends with the single sentence "Your mother wonders from day to day that you don't come to see her." SP, ULL, MS.791/69.

W.G. Spencer's geometrical teaching system was the more successful of the two projects, appearing in the journal *The Educator* and reissued in 1860 as *Inventional Geometry,* with a fourth section added and a penny stamp affixed for convenient delivery. Spencer republished the work in 1892.[623] The book noted that geometry strengthened one's intellectual powers, inducing a "habit of reasoning" far better than arithmetic could – but unfortunately the didactic way in which geometry was taught did not do it justice. *Inventional Geometry,* its author proudly proclaimed, would remedy this by teaching practical geometry. It set out increasingly complex problems for the student to solve on his or her own, while at the same time it was sprinkled with helpful geometric tips.[624] The habit of reasoning was something that everyone could naturally and easily do, because it moved from the simple to the complex. Indeed, this method of moving from simple steps to the most complex constructions would also become Spencer's principle of how education ought to generally be carried out.[625]

Thus while the book's first question dealt with a relatively simple question on cubes:

> 1. Place a cube with one face flat on a table, and with another face towards you, and say which dimension you consider to be the thickness, which the breadth, and which the length. 2. Show to what objects the word height is more appropriate, and to what objects the word depth, and to what the word thickness.

it moved quickly to conic sections and other complex geometrical manoeuvres. The last question asked:

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[623]W. G. Spencer, *Inventional Geometry: a Series of Questions, Problems, and Explanations, intended to Familiarize the Pupil with Geometrical Conceptions, to Exercise his Inventive Faculty, prepare him for Euclid and the Higher Mathematics* (London: J and C Mozley, 1860); E. I. Carlyle, "William George Spencer," DNB

[624]Spencer, *Inventional Geometry*, pp. 1-3.

[625]Spencer, *ABHS*, pp. 1:436-438.

> 350. Illustrate by geometry the respective values of .9, .99, .999, and .9999. A circle may be supposed to consist of an indefinite number of equal isosceles triangles, having their bases placed along the circumference of the circle, and their vertices all meeting in the centre of the circle. And as the areas of all these triangles added together would be equal to the area of a circle: To find the area of a circle - multiply the radius which is the perpendicular common to all these imaginary triangles, by the circumference which is the sum of all their bases, and divide the product by 2.[626]

This passion for geometry would continue throughout Spencer's life. In 1840 (then 20) he himself invented a method of geometry using similar triangles. Geometry even acted as a form of leisure for him: as a young railway engineer based in London for the first time, away from his Derby home, Spencer resisted the temptations of the modern Babylon by solving problems in Chamber's *Euclid* after work. When he returned to Derby, his father tested his engineering abilities by asking him to survey a family property; he used geometry to perform the survey. And one of Spencer's very first articles was his announcement of his "discovery" of a geometrical property.[627]

W.G. Spencer's second system, "lucid shorthand," became another dominant pastime in Spencer's early days. His father had invented it and had Spencer use it from age 13 to 20 to record his uncle's sermons; more importantly, Spencer used this system to correspond with his father and with his uncle. Unfortunately there only seems to have been these three people who ever used this system of shorthand, so it can really be seen

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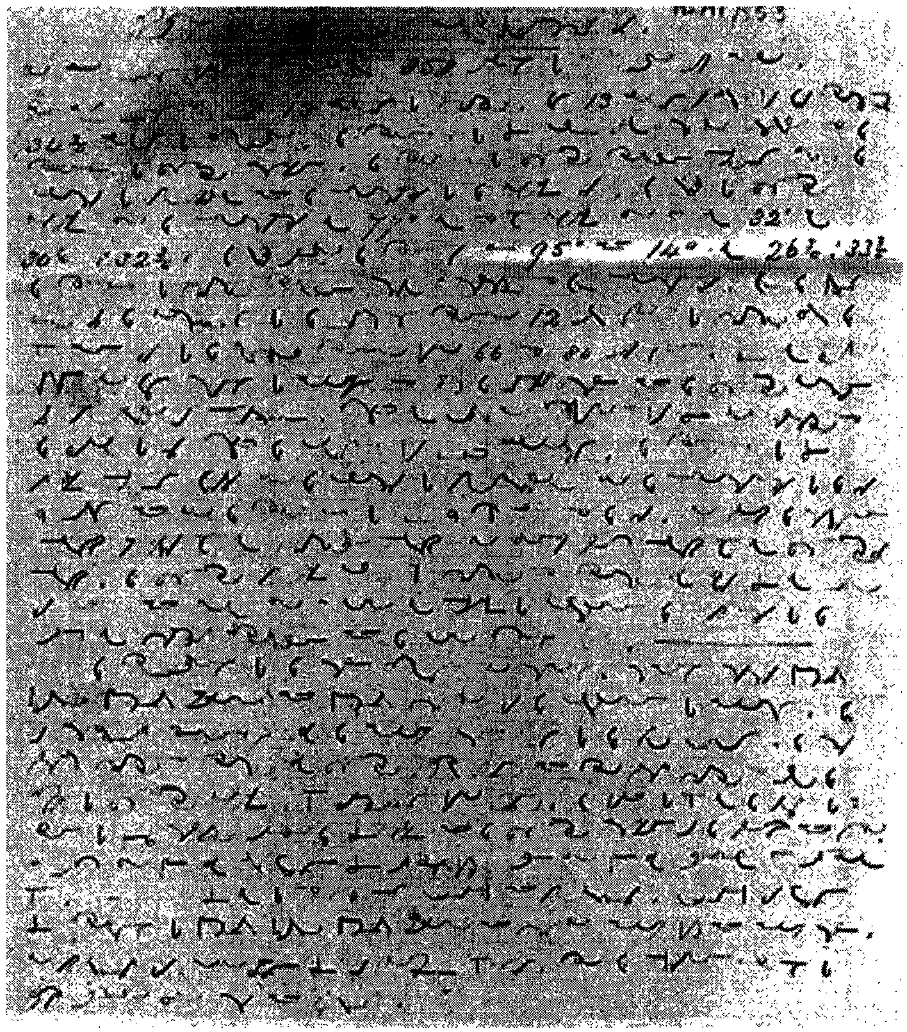
[626]Spencer, *Inventional Geometry*, pp. 3-4, 4-5, 26.
[627]Duncan and Spencer, *LLHS*, pp. 25-26; Spencer, *ABHS*, pp. 1:134, 149. It had been known since Plato.

as a private code.[628] One letter is shown below.



Letter written in W.G. Spencer's Lucid Shorthand, from *Correspondence*, n.d. Herbert Spencer Papers, University of London Library, MS 791/353.

When Spencer's father died in 1866, Spencer took it upon himself to publish the work on shorthand; it eventually appeared in 1894. He rescued the system because of his conviction, "*long since formed and still unshaken, that the Lucid Shorthand ought to replace ordinary writing*"; those using it would attain a "larger economy of life."[629]

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[628] W. G. Spencer, *A System of Lucid Shorthand. With a Prefatory Note by Herbert Spencer* (London: Williams and Norgate, 1894), pp. 4-5, 30.

[629] Spencer, *A System of Lucid Shorthand*, pp. 4-5. Emphasis in original.

The *System of Lucid Shorthand* noted that ease of use was most important. The worst possible language would be one in which a separate and arbitrary symbol was assigned to each word in a language - this technique would exhaust our memory. Better languages grouped similar word-elements together. So the inventors explicitly used the method of analysis and synthesis:

> Attempts have therefore been made to analyze words, and, as it were, to decompose them into their primitive elements, for the purpose of assigning to the several articulations of which they are composed, characters called "letters," such as, when arranged in a proper manner, might represent the various combinations of sound of which language is composed.[630]

It was additionally important for any system of communication to follow logical rules: the current regular alphabets were complicated, full of contradictions, arbitrariness and disorder. The book did allow that a rival form of shorthand, Pitman's Phonography, worked well in some ways – but it was still too complicated, as it was "not a perfectly analytical" system (for example, it gave a simple character to both "ch" and "j").[631]

By contrast, W.G. Spencer's system claimed to be far easier than Pitman's because it moved from the simplest elements to the most complex compounds of these elements in an exceptionally consistent way. The simplest form of the Lucid Shorthand's alphabet was the "aspirate," placed first because of its "elementary nature". Next came vowels: the simplest ones were capable of being combined with each other, forming double and triple vowels. Then the reader was shown signs representing "the compound sounds being formed by the union of the signs of the simple ones." It also distinguished

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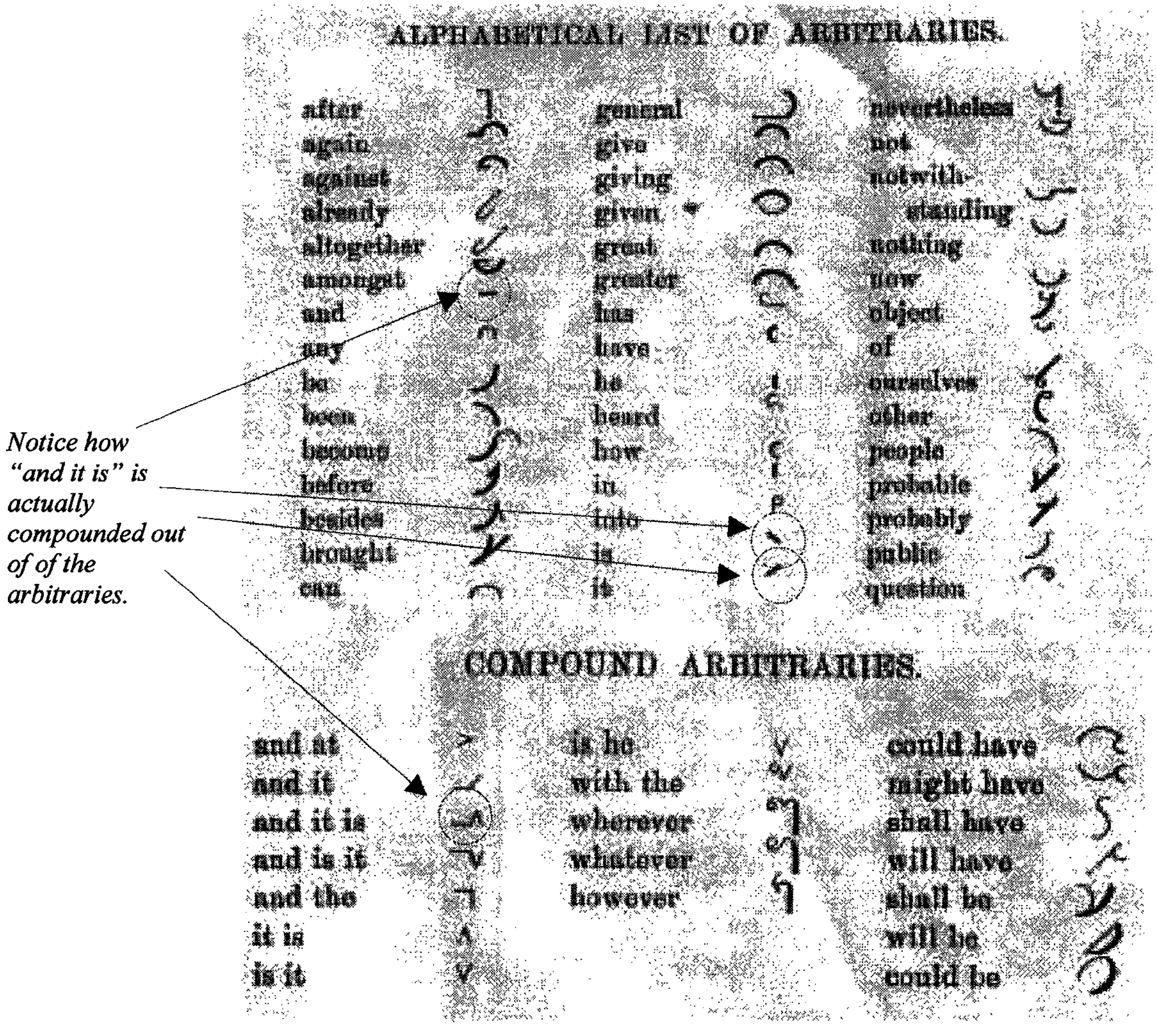
[630] Spencer, *A System of Lucid Shorthand*, p. 9.
[631] Spencer, *A System of Lucid Shorthand*, pp. 10-12.

between simple arbitraries ("after," "again," "against,") and compound ones ("and at," "with the," "shall be").[632]



Simple and compound arbitraries, from W.G. Spencer, *A System of Lucid Shorthand* (London: Williams and Norgate, 1894), pp. 26-27.[633]

This habit of analysis and synthesis would frequently appear in Spencer's works, often explicitly. For instance his second major monograph - the 1855 *Principles of Psychology* - had its first chapter entitled "Compound Quantitative Reasoning".

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[632]Spencer, *A System of Lucid Shorthand*, pp. 14-15, 20-21, 26-27.
[633]Please note that it does not appear this way in the book – the relevant parts of two sections have been pasted together for convenience, for they appear on separate pages.

Spencer was to also state that

> An analysis conducted in a truly systematic manner, must commence with the most complex phenomena of the series to be analysed: must seek to resolve these into the phenomena that stand next in order of complexity; must proceed after like fashion with the less complex phenomena thus disclosed: and so, by successive decompositions, must descend step by step to the simpler and more general phenomena; reaching at last the simplest and most general.[634]

Emily Grosholz has pointed out that understanding Descartes's mathematical method allows the rest of his philosophical method to be understood. She points out a similarity between Cartesian metaphysics and Cartesian geometry, even down to how his books are organized: the order of Descartes's *Meditations* is modelled on the construction of extended proportionalities in the *Geometry*.[635] Spencer's use of analysis and synthesis also means that Grosholz's insight can be also applied to Spencer's entire system.

METHODISM AND ANTICLERICALISM

At the same time, habits of reasoning helped define the possibilities that Spencer's politics could take. This is not the same as determining those possibilities – for equally important was the involvement of Spencer's family in religious secessions from the Wesleyan Methodist church. Nonetheless a link can be drawn between Spencer's use of analysis and synthesis, and his support for a voluntarist social order which came out of these religious secessions. His later political tracts reflect much of the image of small local religious groups spontaneously coalescing and forming larger religious groups; they

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[634]H. Spencer, *The Principles of Psychology*, 1st ed. (London: Longman, Brown, Green and Longman, 1855), p. 71.
[635]E. Grosholz, *Cartesian Method and the Problem of Reduction* (Oxford: Clarendon Press, 1991), pp. 3-5.

borrowed language from the anti-clericalism first articulated for him in these religious secessions.

In Spencer's childhood town of Derby the Tories were in the minority and the Radicals were dominant: its politics was not distinguished so much by class as by religion.[636] His own family history showed the importance of religion too - his grandparents were all Wesleyan Methodists, some of whom were were lay preachers; his grandmother Catherine Spencer (who lived until Spencer was 23) even knew John Wesley personally and met with him frequently. Spencer was brought up in a fairly religious Dissenting household. By age nine, for example, Spencer had memorized *Hymns for Infant Minds* and *Divine and Moral Songs.*[637]

What probably accentuated religion's importance in the Spencer household and extended family, however, were the involvement of family members in various secessions from the main Wesleyan Methodist movement. Spencer's own father deserted the discipline of the Derby Wesleyan Methodist chapel for the Quaker meeting house – for the Quakers had no priests, a sentiment so well described in Voltaire's *Letters concerning the English.*[638] This personal secession meant that Spencer directly experienced religious schisms, because he was caught in the middle of a parental split. To placate his mother Harriet Spencer, still a devout Wesleyan Methodist aghast at William George Spencer's desertion of the chapel, the boy worshipped at the Quaker meeting house on Sunday mornings with his father; in the evenings he went with his

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[636]The Strutts, the town's first family, were Unitarians, not Anglicans.
[637]Duncan and Spencer, *LLHS*, p. 9; Peel, *Herbert Spencer*, pp. 35-37; Spencer, *ABHS*, p. i:19.
[638]F. M. A. d. Voltaire, *Letters concerning the English Nation*, trans. N. Cronk (Oxford: Oxford University Press, 1994), pp. 14-15.

mother to the Wesleyan service and presumably a class-meeting. Spencer went to both houses of worship from age ten to thirteen.[639]

In going with his mother to the Wesleyan chapel, Spencer would have first experienced local and seemingly-spontaneous groups in the form of the *class-meeting*, where small groups of faithful would gather under the guidance of a class-leader. There, "full inquiry was made into the behaviour of every person…Advice or reproof was given as need required, quarrels made up, misunderstandings removed; and after an hour or two spent in this labour of love, they concluded with prayer and thanksgiving". The class meeting was the most important part of Wesleyan Methodism, "essential to the integrity, consistency, working order, and existence of the Methodist Connexion".[640] Wesleyanism's extraordinary growth between 1791 and 1850 (in England and Wales it went from 57,139 to 354,178 members) was in part driven by a perception of the local chapel as an association – as a form of community that served not only religious, but also other social functions. Their communal spirit distinguished many of the Methodist societies - John Wesley likened its society to a family – and the favoured language spoken at Methodist meetings was of brotherhood and fellowship.[641]

But Wesley himself recognized the tenuous nature of his charismatic leadership near the end of his life. Watching over the preachers assembled before him, he resorted to a bodily metaphor as he exclaimed, "'They obey me; but when I am gone, who shall govern this unwieldy body - so many wills? this unwieldy body!'" After Wesley died in

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[639]Spencer, *ABHS*, p. 1:83.
[640]S. W. Christophers, *Class-Meetings in Relation to the Design and Success of Methodism* (London: Wesleyan Conference Office, 1873), pp. 21-22. "Connexion" was the name for the entire Wesleyan Methodist community.
[641]D. A. Gowland, *Methodist Secessions : the Origins of Free Methodism in Three Lancashire Towns, Manchester, Rochdale, Liverpool* (Manchester: Manchester University Press, 1979), p. 165; Peel, *Herbert Spencer*, pp. 38-39; M. R. Watts, *The Dissenters*, 2 vols. (Oxford: Clarendon, 1978, 1995), p. 2:31.

1791, the leadership of Methodism passed onto 100 itinerant preachers who met in an annual Conference. Conference held supreme legislative power over Wesleyanism, and controlled the appointment and stationing of preachers to local districts.[642] But without Wesley's dominant personality, the movement encountered a growing disagreement common to many Christian movements. Some Methodists interpreted Christian life in authoritarian terms - about how to agree with, and make others agree with, common denominational and Christian principles. But others interpreted Christian life in terms of the autonomy of individual believers gathered in local communities, in which the members remained loyal to that immediate group.[643]

Wesleyan Methodism in the first half of the nineteenth century experienced constant secessions because of this familiar disagreement over organization. The first secessions followed spontaneous religious outbursts, in which groups of revivalists left Wesleyanism after censure by Conference.[644] In a popular religious movement emphasizing the feelings of the humble, the unity of the movement was seen to be in jeopardy.[645] By the 1820s, however, Jabez Bunting extended his control over the members of Conference, first as its Secretary and then its President. As his power extended he sought to transform Wesleyanism from a movement defined mostly by its class-meetings into a full-fledged denomination like the Church of England, in a way that suited Bunting's Tory politics and cultivate Wesleyanism's respectability in the eyes of

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[642]J. Everett, *'Methodism as it is,' with some of its Antecedents, its Branches and Disruptions*, 2 vols. (London: W. Reed, 1865), p. 1:21; Watts, *The Dissenters*, pp. 2:30-31.

[643]R. Currie, *Methodism Divided: a Study in the Sociology of Ecumenicalism* (London: Faber, 1968), p. 17.

[644]These various revivals in Wesleyan Methodism occurred in 1805-6, 1808 and 1815 – the seceding groups forming, respectively, the Independent Methodists, the Primitive Methodists, and the Bible Christians. Watts, *The Dissenters*, pp. 2:32-33.

[645]D. Hempton, *Methodism and Politics in British Society 1750-1850* (London: Hutchinson, 1984), p. 20.

Anglican Tories anxious to ensure loyalty to the Crown.[646] To Methodist democrats, however, Bunting was the "evil genius", the preacher seeking to establish his power over "all the body", destroying the local independence of the societies in order to establish a centralized hierarchy. Anticlericalism was the watchword as the struggle of Methodism was defined as being over "*Priestly* rule or *church* government, a modified Popery or Christianity".[647]

The extension of Bunting's authority prompted a second round of secessions as local chapel decisions were overturned by Conference. In 1827, for example, 900 Leeds members left Wesleyanism after their opposition to placing an organ in their chapel was overruled by Conference.[648] By the late 1820s the phrase "pastoral prerogative" had become fashionable amongst Bunting's men, whilst many of the lay members felt excluded from the operations of Wesleyanism. By 1844 this organizational dispute became uncomfortably public when anonymous *Flysheets* attacked Bunting's system of government as contrary to the provincial and communal nature of Methodism. They noted ominously that Bunting was replacing Wesleyanism's true character with a London bureaucracy.[649]

In 1831 Spencer's uncle John Spencer, a solicitor, played a leading role in a Derby secession. John Spencer was quite active in the Derby chapel, leading three Bible classes and preaching to rural congregations.[650] But in 1831, citing the "despotic acts of

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[646]To support this, Bunting argued that John Wesley was closer to the Anglican Church than to Dissent to emphasize its respectability. Watts, *The Dissenters*, p. 2:463.
[647]Everett, *'Methodism as it is'*, pp. 1:21, 1:23.
[648]J. Kent, "The Wesleyan Methodists to 1849," in *A History of the Methodist Church in Great Britain*, ed. E. G. Rupp, R. E. Davies, and A. R. George (London: Epworth Press, 1965), p. 269; Watts, *The Dissenters*, pp. 2:33-34. This group became the Protestant Methodists.
[649]Gowland, *Methodist Secessions*, p. 164; Kent, "The Wesleyan Methodists to 1849," pp. 214-215.These Flysheets were distributed until 1848. Gowland, *Methodist Secessions*, pp. 16-17, 167.
[650]Peel, *Herbert Spencer*, p. 7; Spencer, *ABHS*, pp. 1:24-25.

superintendents", he and a number of other community leaders announced their secession from the main Wesleyan Church. There are different accounts for why this split occurred. One historian argues that it was a theological dispute – the secessionists held that faith was not a gift from God but was simply the exercise of powers inherent in human nature. But it was also a political dispute emphasizing the power of local religious groups against centralizing clerics. When young women were encouraged to preach and the District Superintendent tried to stop this practice, he was vehemently opposed. In another display of rebellion, when the Reverend William Davis attempted to stop the "various gymnastic exercises" of the Derby Wesleyans in a Band Meeting, the members immediately told him "'this is the People's Meeting, this is our time for speaking.'"[651] The people were thus depicted as opposing clerics.

Early in 1832 the district authority, appointed by Conference, expelled 4 local preachers; to protest the explusion, 600 local members withdrew. By 6 February 1832, a large group of trustees, stewards, local preachers and class leaders met at John Spencer's house, forming the Arminian Methodist Society. While the Arminian Methodists used Wesley's form of worship, they distinguished themselves from Wesleyanism by their revivalism. More concretely they insisted upon majority rule, linking themselves with local political reformers.[652] Ultimately, thirteen of the fifty-three local preachers seceded from Wesleyan Methodism, and seven hundred out of 1900 members – with over half coming from John Spencer's bible classes - left the Derby Wesleyans. The Derby

[651]G. B. MacDonald, *Facts against Fiction: or a Statement of the Real Causes which Produced the Division among the Wesleyan Methodists in Derby; Forming a Reply to the Account Published by an Anonymous Writer* (Derby: W. Horsley, 1832), pp. 17-18; G. Smith, *History of Wesleyan Methodism*, 3rd ed., 3 vols. (London: Longman Green Longmans and Roberts, 1862), pp. 3:171-172.
[652]T. E. Brigden, "Notes and Queries - The Arminian Methodists or Derby Faith Folk," *Publications of the Wesley Historical Society* 4, 124 (1899): p. 124; A. W. Harrison, "The Arminian Methodists," *Publications of the Wesley Historical Society* 23, 25-26 (1941-2): pp. 25-26.

secession even made its way into literature in the form of Dinah Morris, the heroine of George Eliot's *Adam Bede* – for she was modelled on Eliot's aunt Elizabeth Evans, herself a Derby Arminian Methodist for several years. George Eliot was an excellent friend of Spencer's: there were rumours that they would marry in the early 1850s, but they did not.[653]

In 1833, at age 13, Spencer's immediate engagement with these Derby religious organizations stopped when he was sent to live with his uncle Thomas Spencer, parson of Hinton Charterhouse. This was because it was felt that the strong-willed uncle would help straighten out the equally strong-willed Spencer, helping his education. Yet Spencer's continued to send him letters of "religious exhortation", appealing to his religious feelings. Spencer gradually moved away from Methodism, claiming that going to twice the number of religious services - along with the hymn-memorization and his general repugnance to any form of priestly rule and ceremony – had made him unsympathetic to formal worship. Yet Spencer's religious upbringing affected his views of the temporal world. He was obviously part of the evangelical sentiment that supported the free market – at age 16 Spencer wrote a letter to *The Bath and West of England Magazine* defending the New Poor Law, for scripture noted that if a man did not work, then neither should he eat.[654] And his "Nonconformist instincts and training" also enhanced his distaste for authority: it gave him an instinctive revulsion for the Church of

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[653]Peel, *Herbert Spencer*, pp. 7, 37; Spencer, *ABHS*, pp. 1:398-399; Watts, *The Dissenters*, p. 2:34.
[654]Herbert Spencer, "Poor laws: reply to 'T.W.S.' " *The Bath and West of England Magazine* 3, 81-83; reprinted in idem, *Political Writings*, ed. J. Offer (Cambridge: Cambridge University Press, 1994).

England, for example, and he commented on the secession of the Free Church of Scotland from the established Church of Scotland in 1843.[655]

Much of the evidence presented here is circumstantial. In the case of the Derby secession led by John Spencer, Spencer would have only been twelve when the secession occurred. But it is the early years of a person's life which are often the most important in shaping his or her later outlook. More proof is difficult to obtain after the disappearance of much of Spencer's early correspondence with his relatives. But these pieces of evidence indicating his relations with Dissent suggest a larger pattern.[656] Religion and discussions about religion strongly shaped Spencer's later outlook. First, in the field of religion, anticlericalism was always present in Spencer's life: one rebellion took the form of a public split from Wesleyan authority by an uncle, another took the form of a private secession by his father, meaning that Spencer had to attend two different houses of worship. Second, the uncle responsible for Spencer's teenage education wrote numerous critiques of the Church of England. He was heavily involved in a social network of democratizing Radicals, and just as important his best-selling tracts gave Spencer an example of a successful author – making authorship and journalism real career possibilities for the young Spencer.

Finally and perhaps most importantly, Dissent moreover offered an organizational model of a hierarchy of local associations. The local class-meeting of 20 members sent their leader as a delegate to a district meeting, which in turn sent delegates to a larger district meeting, and so on; this pattern of sending delegates went all the way to a

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[655]Duncan and Spencer, *LLHS*, pp. 28-29; H. Spencer, "The Non-Intrusion Riots," *The Nonconformist*, 11 October 1843.

[656]J.D.Y. Peel thought that Spencer's Radical, Dissenting Derby background required an entire chapter, the marvellous "Enthusiasts and Lunaticks". Peel, *Herbert Spencer*, pp. xii, chapter 2.

national Conference. It has already been observed that Methodism's organization into ascending hierarchical levels was imitated by various working class political societies in the 1830s and 40s, from the Chartists to the Owenite National Union.[657] It was a way of preserving a group's community and autonomy while also facilitating collective action upon common principles. So it is not too far a leap to the suggestion that Spencer's own later views upon social and biological organization - like his views upon superorganisms, symbiosis and ascending "orders of individuality" - bore some debts to the Methodism with which he had such intimate childhood experience. Future research will not merely be limited to Spencer's religious formation, but to other researchers' religious experiences too; it will explore how their immersion in these forms of life made them aware of different organizational options and whether these researchers might have applied these organizational possibilities to their own biological investigations.

PHRENOLOGY AND POLITICAL RADICALISM

As Spencer grew older he began to fuse analysis and synthesis with his political outlooks in support of spontaneous order. One field exemplifying this was phrenology. Historians of phrenology have noted its congruence with Spencer's *laissez-faire* views, and its ability to act as a vehicle for social advancement for ambitious early Victorians.[658] But here, we are concerned simply with phrenology's view of organization, which shared many of the assumptions of Spencer's politics.

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[657]H. J. Perkin, *The Origins of Modern English Society, 1780-1880* (London: Routledge & K. Paul, 1969), pp. 358-359. Another similarity which Perkin points out is that the open-air mass meetings of the various working-class reform groups imitated Wesley's earlier outdoor "camp-meetings".

[658]Cooter, *The Cultural Meaning of Popular Science;* Richards, *Darwin and Evolutionary Theories of Mind*, p. 251.

Spencer first experienced phrenology at age eleven, when Spurzheim lectured in Derby. Eleven years later another phrenologist examined Spencer's head, and he became curious about the practice. Its promise of moving from the complex to the simple and then back again to the complex must have been alluring for him: it even suggested future research. One problem was the artificially complex nature of the mental faculty of conscientiousness, which offered the possibility of further analysis - Spencer wrote that "like many of the chemical bodies that were at one time believed to be simple elements, [conscientiousness] is fated to undergo decomposition". It was not caused by one mental faculty, but was, like Benevolence, a "compound feeling" composed of simpler feelings like sympathy.[659] In January, April and June of 1844, Spencer's interest in phrenology was itself freely exercised, shown by the appearance of his three phrenological articles in *The Zoist*. One article spoke of the "common phrenological principle" that "organs are prone to action in proportion to their size", meaning that imaginative people had large mental organs of "reviviscence". In another, Spencer mentioned how an active part of the brain "tends to arouse the organs located in its neighbourhood"; the organ of Amativeness, (what we would now call sexual attraction) awoke "the adjacent propensities" when activated.[660]

Spencer's period of phrenologizing coincided with his political writing and activism. Just as the analyst sought to decompose systems to their elements, Spencer the political writer sought to devolve authority to society's true elements – the (male) individuals that constituted the "people". In 1843, in a series of letters to *The Nonconformist* later reissued as *The Proper Sphere Of Government,* Spencer mentioned

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[659]Spencer, *ABHS*, pp. 1:200, 228-229.
[660]H. Spencer, "The Situation of the Organ of Amativeness," *The Zoist* 2, 186-189 (1844-1845): p. 186; idem, "A Theory Concerning the Organ of Wonder," *The Zoist* 2, 318-325 (1844-1845): pp. 321, 324.

the "social organism" for the first time. His mention of a social organism was likely brought on with Spencer's involvement with phrenology – for Spencer cited a phrenological text alongside Adam Smith's *Wealth of Nations* in these letters, Sidney Smith's *Principles of Phrenology.*[661]

Smith's book has been already been discussed in chapter one: it was the very same book that likened the brain to an "intellectual legislature". It contained other notes on mental plurality. In the section entitled "Plurality of Organs and Faculties," Smith used the brain's disunity to explain states of consciousness, again equating each mental faculty with an individual. The sleeping person had a brain that formerly acted corporately, a "combined army of operations". But upon slumber the mental faculties started to act like guests in the travellers' room at an inn, "one reading, another writing, a third eating, while a fourth, having just arrived after a long journey, is snoring, with his legs across a chair, unconscious of the presence of the rest, who are hurrying in and out, each upon his own peculiar business."[662]

By substituting the phrase "mental faculty" for "person," one can quickly see how Spencer's phrenological arguments were transferred so easily from politics, and why he referred to a phrenology book while writing his first political tract. The structures of both mental and political systems were similar. His early view of natural rights, for example, was described as the ability to work freely and not be impinged upon by any other person. Injustice was a violation of this freedom,[663] a doctrine of rights imitating the

[661]H. Spencer, *The Proper Sphere of Government* (London: W. Brittain, 1843), pp. 17, 5. Smith, *The Principles of Phrenology*. This is the only phrenological text that Spencer cites in any publication.
[662]Smith, *The Principles of Phrenology*, pp. 31, 36-37. On page 33 Smith had an evolutionary message, noting that Vimont, Serres, Tiedemann and Vicq d'Azyr all observed that every rise of intelligence in the animals was accompanied by more cerebral parts, both as adults and in the fetal brain. This may have influenced Spencer.
[663]Spencer, *The Proper Sphere of Government*, p. 6.

phrenologist George Combe's belief that all of the mental faculties should be permitted free exercise. If we persist in speaking about the role of rights in Spencer's political thought,[664] then mental faculties can be said to have certain rights too. Spencer's insistence that society was self-adjusting, with all of its elements in equilibrium,[665] is exactly like phrenology's view of the equilibrium maintained by the different mental faculties.

Spencer's next anti-authoritarian encounter occurred between 1842 and 1844 when he participated in the Complete Suffrage Union (CSU). Led by the Birmingham Quaker, Joseph Sturge, who had previously pushed to abolish slavery in Britain, Spencer followed his uncle Thomas Spencer into this group. The CSU was formed at a March 1842 conference of middle- and working-class Chartist reformers in Bath; Thomas Spencer and others argued that all advocates of suffrage should unite into a national association (while rejecting the Chartist label so frightening to the middle classes). They noted that every committed Briton "must deplore the estrangement of feeling between the middle and working classes".[666] Middle-class radicalism and working-class Chartism can be seen as a common movement to overcome political exclusion; the CSU was a group identifying with the "people" and the "nation," believing that they held real political

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[664]David Stack denies that Spencer was as deeply influenced by Thomas Hodgskin (colleague during Spencer's four year term at *The Economist*) as previous historians, beginning with Elie Halévy, have implied. Spencer, therefore, cannot be seen as standing in a long, individualist, natural rights tradition stretching to Godwin. Instead Stack notes that Spencer mainly shared the common Combean technique of drawing analogies between societies and organisms. D. Stack, *Nature and Artifice: the Life and Thought of Thomas Hodgskin* (London: Royal Historical Society, 1998), pp. 189-193.
[665]Spencer, *The Proper Sphere of Government*, pp. 5-6.
[666]*The Nonconformist,* 23 March 1842, 186-187.

power.[667] In this polarized account of society, the natural source of order was the people, a group unified by a common feeling of exclusion from political power.

This group then met in Birmingham with "a view to union" with the moral force (non-violent) faction of the Chartists; its main argument was that "the people" were the true source of political power.[668] At this meeting, Thomas Spencer showed his rationalist leanings by calling for the destruction of unnecessary laws, arguing that every Englishman ought to have a plain, simple code of laws about the size of the New Testament. After Sturge visited the Spencer household in Derby, Spencer – then 22 - became Honorary Derby Secretary of the CSU. At first it grew rapidly, helping to elect some middle class councillors, clergy and Chartists.[669] When a popular CSU lecturer came to speak in Derby, the local magistrates – thought to be working on orders from the Home Secretary – made several attempts to stop the speech, prompting Spencer to pen a letter of protest. He even took to wearing a cloth cap about Derby, a fashion copied by other young men and secretly thrilling Spencer with the prospect of being mistaken for a Chartist leader.[670]

Though the fortunes of the CSU declined thereafter,[671] Sturge and the two Spencers did their best to revive it. The energetic Thomas Spencer lectured in twenty

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[667]On this point see Jones, *Languages of Class*, p. 104. For the articulation of different visions of British society in the nineteenth century see D. Cannadine, *The Rise and Fall of Class in Britain* (New York: Columbia University Press, 1999), pp. 19-21, 68-69.
[668]A. Miall, *Life of Edward Miall, formerly Member of Parliament for Rochdale and Bradford* (London: Macmillan, 1884), pp. 75-76, 78; Spencer, *ABHS*, p. 1:219.
[669]*Report of the Proceedings at the Conference of Delegates of the Middle and Working Classes, held at Birmingham, April 5, 1842, and three following days* (London: Davis and Hasler, 1842), pp. 58-60; Spencer, *ABHS*, pp. 1:218-219; A. Wilson, "The Suffrage Movement," in *Pressure from without in Early Victorian England*, ed. P. Hollis (London: E. Arnold, 1974), p. 85.
[670]Herbert Spencer, "An Address from the Municipal Electors of the Borough of Derby, to the Authorities of the Town", *The Morning Chronicle*, 6 Sept. 1842, p. 3. Another indignant account can be found in "The Derby Magistrates", *The Nonconformist*, 14 Sept. 1842, pp. 625-6. Duncan and Spencer, *LLHS*, p. 35.
[671]This was mostly because the members of the CSU refused to call their proposals a "Charter" because they feared it would alienate their middle-class supporters. But many Chartists, particularly Feargus

towns.[672] Spencer, for his part, wrote articles in Edward Miall's *Nonconformist*, a journal mixing Radical politics with Dissent and the occasional organic example to justify its Christianity.[673] Spencer's strategy, like the others, was to emphasize common interests between middle and working class democrats, in the hopes that these people would coalesce and form a larger group. This rhetoric emphasized the gap between the people and aristocracy. His "Effervescence – Rebecca and her Daughters", examining the Welsh Rebeccaite rebellions, was one example. Spencer noted that not only had formerly contented and quiet agriculturalists started to riot, but that these disturbances were quickly spreading into other groups in "adjacent ranks" of society. As in the image of small Dissenting groups coming together to oppose the priests, he commented that rioting had occurred because the two great antagonistic elements of social existence, the democratic and aristocratic spirits, were in contact. Spencer used examples from magnetism and chemistry to illustrate how social coalescence worked – that like "antipodean masses", one pole or the other would soon influence every "neutral particle".[674] Just as iron filings were drawn to a magnet, all the disparate groups with democratic interests were gradually joining, making common cause with each other in their rioting.

Spencer not only used scientific rhetoric to articulate his political vision, but he also shared the assumptions of Thomas Spencer and others in the CSU. On the one hand there was a hierarchical vision of society, with vertical lines of affiliation, of patronage

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O'Connor's physical-force faction, refused to abandon this name. Peel, *Herbert Spencer*, p. 12; Spencer, *ABHS*, pp. 1:218-221; Wilson, "The Suffrage Movement," p. 88.

[672]Watts, *The Dissenters*, p. 2:564; Wilson, "The Suffrage Movement," p. 91.

[673]Miall, *Life of Edward Miall*, p. 86; Spencer, *ABHS*, p. 1:218. One article likened the Christian religion to an organism, an "elephant" compared with a steam engine. The elephant was capable of growth, having interior energy vastly superior to any apparatus created by "external authority." This does not seem to have been written by Spencer. "Mechanism – Natural and Artificial," *The Nonconformist*, 21 Sept 1842, 633.

[674]Herbert Spencer, "Effervescence - Rebecca and her Daughters", *The Nonconformist*, 28 June 1843, 457.

and deference, subscribed to by the purported anti-democrats. On the other, there was a vision of two classes, where an interest in democracy united members of the unrepresented class. Peel correctly notes that this vision saw society as horizontally stratified, governed by contract and voluntary association; for Spencer this latter vision was the norm that ought to guide political action. This sentiment later developed into his dichotomy between the "militant" form of organization and the "industrial" one, in which regimentation and coercion characterized the militant society; contract and free association, the industrial society.[675] The young Spencer's organizational visions and the old Spencer's remained quite consistent - he was to later use similar fears of hierarchy and vertical affiliation to oppose the rise of professional society.

ANALYZING THE ORGANISM

In December 1850 Spencer's first book, *Social Statics,* was released. In it, he drew parallels between a society formed of individuals and an animal formed of living cells or units.[676] Borrowing from phrenology and using the notion of the division of labour, Spencer argued that "Man...consists of a congeries of faculties, qualifying him for surrounding conditions. Each of these faculties, if normally developed, yields to him, when exercised, a gratification constituting part of his happiness..." Congeries was opposed to a conscious will, and any organism was not a unity. The body could be seen instead "as a commonwealth of monads, each of which has independent powers of life,

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[675]Duncan and Spencer, *LLHS*, p. 573; Peel, *Herbert Spencer*, p. 61.
[676]Duncan and Spencer, *LLHS*, p. 540.

growth and reproduction; each of which unites with a number of others to perform some function needful for supporting itself and all the rest".[677]

Many of Spencer's examples were drawn from invertebrate zoology, from Thomas Rymer Jones's *General Order of the Animal Kingdom* and Richard Owen's "Hunterian Lectures" (likely his 1849 *On Parthenogenesis).* He noted how organisms were organized by the concentration of their elements – annelids were an extended series of rings; myriapods had less numerous and denser rings; in insects this condensation was even more pronounced.[678] In turn Spencer noted how both simple creatures and simple societies were aggregations of like parts, citing the polyp as an exemplar: "Every portion of the community performs the same duties with every other portion; much as each portion of the polyp's body is alike stomach, skin, and lungs". Each person in a simple society, like each segment, was warrior, toolmaker, fisherman, builder.

> Between creatures of the lowest type, and creatures of the highest type, we similarly find the essential difference to be, that in the one the vital actions are carried on by a few simple agents, whist in the other the vital actions are severally decomposed into their component parts, and each of these parts has an agent to itself.... [679]

Agency and the division of labour were distributed throughout the body.

Spencer also had social contact with his naturalist sources, knowing Rymer Jones as early as 1834. For William George Spencer was Rymer Jones's Derby tutor. When Spencer and his father visited London in 1834, Rymer Jones took them to the Zoological Gardens, which was open only to fellows of the Zoological Society and their guests.[680] Previous chapters have discussed Rymer Jones's belief in the organism's compound

[677]H. Spencer, *Social Statics : or, The Conditions Essential to Human Happiness Specified and the first of them Developed* (London: J. Chapman, 1851), p. 280.
[678]Spencer, *Social Statics*, pp. 451-453.
[679]Spencer, *Social Statics*, pp. 274-275.
[680]Spencer, *ABHS*, p. 1:106.

individuality: Spencer probably obtained the term "monads" from Rymer Jones. And both Spencers – father and son – had links with Owen too, perhaps through Rymer Jones. When the two Spencers went on a holiday to the Isle of Wight in the summer of 1841, William George Spencer saw some fishermen haul in a sun-fish, and, "Knowing Prof. Owen...wrote to him telling of the fact", thinking that the fish could be dissected. When Spencer finished *Social Statics* he moved from merely citing Owen's work to attending his courses, attending Owen's comparative osteology lectures in the spring of 1851.[681]

Owen wrote at least three times to Spencer: one letter thanked Spencer for his appreciation of the *Archetype*, notifying him that it would be republished as an elementary introduction to comparative osteology.[682] In another, Owen, "your well-wisher," stated that Spencer's work agreed very closely "with some of the trains of thought that have been suggested to me by the comparisons of dry bones" but doubted that the adaptive force would ever completely overcome vegetative repetition in the present "sphere of our existence."[683] In the third, Owen was grateful for "the last product of your pen" (probably either Spencer's "Development Hypothesis" or the "Theory of Population,") and declared that he had "watched with much interest your progress as an independent thinker and writer".[684] They were in contact even to the mid-1850s: in February 1855 Spencer had dinner with some friends "in company with Professor Owen and his wife", after which the group proceeded to the Royal Institution to hear Owen lecture.[685]

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[681]Spencer, *ABHS*, pp. 1:194, 368.
[682]Abstracts of letter to HS from Richard Owen, SP, ULL, MS.791/355/1 ff1-2.
[683]R. Owen to H. Spencer, 28 Apr 1851, SP, ULL, MS.791/27.
[684]R. Owen to H. Spencer, 30 Jun 1852, SP, ULL, MS.791/33. In early 1851 Spencer collected materials for 'A Theory of Population deduced from the General Law of Animal Fertility'; he wrote it in January, February and March 1852. Duncan and Spencer, *LLHS*, pp. 64-5; Spencer, *ABHS*, p. 1:388.
[685]Spencer, *ABHS*, p. 1:462.

Owen was interested in Spencer's writing because Spencer was bringing his work to a wider audience. After publishing *Social Statics,* Spencer continued his research into anatomy and physiology. One resulting article seems to have been his 1852 *Westminster Review* piece, "The Theory of Population, deduced from the General Law of Animal Fertility." Dealing with the problems of sexual reproduction, it reviewed Owen's 1849 *On Parthenogenesis* as well as Robert Bentley Todd's *Cyclopaedia of Anatomy and Physiology* and Johannes Japetus von Steenstrup's *Alternation of Generations.*[686]

Spencer even took up Owen's classification scheme word for word, with all of its assumptions of compound individuality; he dutifully placed invertebrates into groups such as *Nematoneura, Homogangliata* and *Heterogangliata,* because of the development "of the internuncial or co-ordinating apparatus" allowing "intercommunication between parts". He saw Owen's method as a continuation of John Hunter's analysis of the nervous system: in order for the separate parts of an organism to act in concert, they had to communicate.[687] Nerves were thus intermediaries between nominally independent systems, carrying messages between them.

Indeed, nerves were qualitatively different from any other body part – since organic life was the "co-ordination of actions," then we could divide an organism into the parts co-ordinated and the parts co-ordinating them. For Spencer an organism therefore consisted of the muscular, digestive, vascular, and other systems on the one hand, and the nervous system on the other. An organism's proportion of nervous tissue and nervous activity determined its individuality, consciousness and purposive action. Since the nervous system was different from the rest of the bodily systems, vital energy was

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[686]H. Spencer, "A Theory of Population, Deduced from the General Law of Animal Fertility," *Westminster Review* 57, 468-501 (1852): p. 468.
[687]Spencer, "Theory of Population," pp. 474-475.

directed either to nervous tissue or to any other type of system. The qualitative difference between nervous system and the other physiological systems further illustrated why parthenogenesis / metagenesis was a helpful explanation: for "Individuation and Reproduction are antagonistic".[688]

Spencer was becoming Owen's metagenetic standard-bearer, for his review made a large audience aware of Owen's theories of parthenogenesis / metagenesis. Owen's spermatic force, responsible for the formation of simple individuals or complex parts, showed that reproduction was merely another form of growth. It could either take place in an integrated way, leading to more complexity, or it could lead to the separation of similar parts. Growth could therefore be seen as a continuum, where at one end sat the procreation of simple individuals, and at the other sat the growth of parts of more complex organisms. Yeast was little more than a collection of cells that could reproduce in enormous numbers because it was so simple; at the other end of the scale, the far more complex oak tree used up its vital energy developing its parts, so it did not reproduce nearly as much. "This combination of parts that are tending to separate and become distinct beings - this union of many incipient minor individualities into one large individuality - is an arrest of reproduction - a diminution in the number produced".[689]

In turn, Spencer's work brought him increasing recognition amongst leading British naturalists – E.S. Forbes, wanting to meet and discuss it, told Spencer he had read it twice and was about to read it a third time.[690] And Spencer sent an offprint to T.H. Huxley after hearing Huxley's 1852 talk on colonial ascidians. In an accompanying letter, Spencer noted that the enclosed pamphlet explained why he was interested in

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[688]Spencer, "Theory of Population," pp. 497, 492, 487, 479.
[689]Spencer, "Theory of Population," pp. 476-477, 485.
[690]Duncan and Spencer, *LLHS*, p. 64.

colonial ascidians: he had recently read Carpenter's *Physiology,* and "Being much interested in all facts elucidating the production of composite animals by the union of simpler ones Mr. Spencer would feel further obliged if Mr. Huxley would put him in the way of seeing all that he has written on this subject."[691]

Spencer's peculiar habit of referring to himself in the third person ended once they became friends. Huxley used the grim biological image that when he refuted many of Spencer's speculations, they were left "choking in an embryonic state".[692] One of these disagreements was likely over the issue over which they first met, that of animal individuality, for when they met Huxley was privately beginning to attack Owen's parthenogenesis. He was already publicly denying that animals were collections of partly independent parts: studies of marine invertebrates should not chase after metaphysical problems of "psychical individuality",[693] and the problem of polymorphism would not lead to fruitful research. Twenty years later he would publicly air this difference of opinion with Spencer.

DEVELOPMENT AS SYNTHESIS

Throughout this dissertation two different directions of development have been set against one another – the centripetal, coalescing, type of development against the centrifugal, differentiating, type. It is my point that the philosopher that became famous

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[691]H. Spencer to T.H. Huxley, 25 Sept 1852, HP, IC, 7.94-95. Just as Spencer sought to learn about biology, Huxley may have seen in Spencer his own childhood dream: in the late 1830s he wanted to become a mechanical engineer. Weldon, "T.H. Huxley."
[692]T. H. Huxley, L. Huxley, and J. Strachey, *Life and Letters of Thomas Henry Huxley*, 2 vols. (London: Macmillan, 1900), p. 1:358.
[693]Huxley, Foster, and Lankester, *Scientific Memoirs*, pp. 1:116-118.

for deploying 'evolution' as a universal explanation first thought of development moving centripetally: for Spencer, development proceeded by the synthesis of elements into compounds, like the process of metamorphosis.

Thus as a boy of about eleven Spencer drew a picture of the butterfly life-cycle, with a caterpillar, chrysalis and then butterfly.

Butterfly metamorphosis, H. Spencer, 1830-1833. Herbert Spencer Papers, University of London Library, MS 791/354 f.10.

It shows how strongly his childhood influenced his later life, for it does not seem coincidental that an extremely similar drawing of the same process appears on the front covers of the various *System of Synthetic Philosophy* books, which started to appear 22 years after Spencer drew this picture.

When Spencer was a 20-year-old engineer on the Birmingham and Gloucester railroad, he bought Charles Lyell's *Principles of Geology* to better understand the fossil shells and skeletons his navvies uncovered as the railway cut through blue clay hills. The second volume of *Principles* devoted thirty pages to a criticism of Jean-Baptiste de

Lamarck's vision of evolution, and it is here that Spencer allowed that he was converted to Lamarck's doctrine and thus to evolutionism.[694] By 'conversion' Spencer didn't mean simply learning about the mechanism of the inheritance of acquired characteristics, but also the view that there was a scale of being up which organisms moved over time – what became known as the "development hypothesis" with Robert Chambers's enormously successful *Vestiges of the Natural History of Creation.*

Lyell's account was notable because as he attacked Lamarck he continuously emphasized his analytic and synthetic view of nature, referring to Lamarck's notion that animal bodies were at first simple associations that became more complex by compounding over time. For instance, Lyell noted the belief that if we arranged the entire series of animals by their natural relations, we would pass progressively

> ...from beings of more simple to those of a more compound structure; and in proportion as the complexity of their organization increases, the number and dignity of their faculties increase also...plants and animals of more simple organization existed on the globe before the appearance of those of more compound structure, and the latter were successively formed at later periods: each race being more fully developed than the most perfect of the preceding era.[695]

The word "compound" appeared four times in as many pages to refer to increasing complexity of structure.[696] When we see Spencer as a "Lamarckian", then, we should not just emphasize his use of the inheritance of acquired characteristics as an evolutionary mechanism, but also his similar discussion of development through compounding and coalescence of simple structures.

By 1844 Spencer was using changes in time to understand the world: in a bumptious article for the *Philosophical Magazine* about how fossils became increasingly

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694 Spencer, *ABHS*, pp. 1:175-177.
695 C. Lyell, *Principles of Geology*, 1st ed., 3 vols. (London: J. Murray, 1830-1833), p. 2:11.
696 Specifically, from page 11 to page 14.

complex, he tried to use chemistry to show why this change had occurred over time.[697] But it was with his move to London that he became more aware of the development question. In the spring of 1850, newly situated in London as subeditor for the *Economist,* Spencer met G.H. Lewes, another London intellectual in John Chapman's avant-garde circle. Their first discussion was about the development hypothesis, and Spencer rejected the interpretation set out in the *Vestiges.*[698]

PALAETIOLOGY, OWEN AND HUXLEY

Over the next year, Spencer - contributing articles to Chapman's *Westminster Review* - was to review a new edition of W.B. Carpenter's *Principles of General and Comparative Physiology,* and he learned Karl Ernst von Baer's principle of embryological development[699] - the "change from homogeneity to heterogeneity". With Spencer's introduction to von Baer also came an introduction to the habit of palaetiology, of differentiation, and a ramifying tree of developmental possibilities.

As a budding author who sought to make a living through writing, Spencer cannot have failed to notice that the development hypothesis was an exceptionally fashionable topic, as shown by the popularity of the *Vestiges*. It would be worthwhile to write about

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[697]J.-B.-A. Dumas, "On the Chemical Statics of Organized Beings," *Philosophical Magazine* 19 (3rd series), 337-347, 456-469 (1841); H. Spencer, "Remarks Upon the Theory of Reciprocal Dependence in the Animal and Vegetable Creations, as Regards its Bearing on Palaeontology," *Philosophical Magazine* 24 (3rd series), 90-94 (1844). Spencer wrote this in response to Dumas, proposing that the fossil record became more complex because the proportion of oxygen was increasing in the earth's atmosphere. It may be because of Dumas's title that Spencer chose the word "statics" for his first book in 1851, not because he followed Auguste Comte (Spencer was particularly insistent that he bore no intellectual debts to Comte). Sadly, to keep the dissertation manageable I have emphasized the life sciences and neglected the influence of other fields - like chemistry and engineering – upon Spencer's vision of the social sciences.
[698]Spencer, *ABHS*, p. 1:348.
[699]Spencer, *ABHS*, p. 1:384.

such topics, as the prospect of larger audiences would mean more money for his articles and a greater reputation. Lewes, now a friend, had started his own journal (*The Leader)* and asked Spencer to contribute some anonymous articles. He agreed, and his second *Leader* article, "The Development Hypothesis," appeared on 20 March 1852.[700] In it, Spencer proposed the view that the organic world was governed by a process of development by successive modifications, much like embryological development; all organisms, in this view, were connected by "insensible gradations."[701]

Spencer wrote that most people habitually looked at things in their "statical", not their "dynamical" aspect, and so they didn't realize that "by small increments of modification, any amount of modification may in time be generated." Following works such as *Vestiges,* his piece was indeed a success - it "created a sensation," he exclaimed, and the journal *The Reasoner* asked to reprint it. Spencer sent complementary offprints to Owen, Lyell and Adam Sedgwick. Even Robert Chambers expressed his admiration of the piece to G.H. Lewes.[702]

As discussed at the beginning of chapter two, however, there were different views about how 'development' proceeded. Though this phrase "homogeneity to heterogeneity" drearily peppered his writings thereafter, we must be careful not to overemphasize Spencer's embrace of von Baerian principles. For his newfound habit of palaetiology always sat extremely uneasily with his previous use of analysis and synthesis. After all when Spencer met Huxley he was already 32 years old and a strong

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[700]Duncan and Spencer, *LLHS*, pp. 64-66.
[701]Spencer, *ABHS*, p. 1:387.
[702]Duncan and Spencer, *LLHS*, pp. 65-66; H. Spencer, "The Development Hypothesis," *The Leader* 3:104, 280-281 (1852): p. 280.

supporter of Owen's programme, even socially acquainted with Owen and his protégé Rymer Jones.

Throughout this dissertation, two types of development have been contrasted, which were largely shaped by one's habit of reasoning. If one thought of development in terms of synthesis then one thought of development as the centripetal coalescence of discrete units into a composite whole; one further tended to emphasize the *unit* itself. If one thought about development in terms of palaetiology then one thought of the centrifugal differentiation of a simple (homogeneous) unit into a more specialized (heterogeneous) one; one further tended to emphasize the *process*.

Because these developmental directions were opposed it seems to have been a difficult matter reconciling the two directions. Nonetheless Spencer would try to have it both ways (even mooting the term "involution" instead of "evolution" at one point, as we shall see below). Without speaking about 'implications' or 'logical contradictions' in his thought, we shall instead look at how these different emphases could be appropriated by different audiences, and see how these different emphases were to create problems for him in the long run.

PRINCIPLES OF PSYCHOLOGY

Spencer was to press on with his authorship in the early 1850s. His second monograph appeared in 1855 and used evolutionary principles to understand psychology. Yet its version of evolution was strongly synthetic and linear – Spencer set out initial axioms and built increasingly complex deductions from these statements, resembling work in geometry or more generally a Cartesian thought process; its chapters were divided into chapters bearing titles like "Compound Quantitative Reasoning," "Special Analysis," and "Special Synthesis." He set out various methodological principles following the habit of analysis and synthesis too, thus:

> An analysis conducted in a truly systematic manner, must commence with the most complex phenomena of the series to be analysed: must seek to resolve these into the phenomena that stand next in order of complexity; must proceed after like fashion with the less complex phenomena thus disclosed: and so, by successive decompositions, must descend step by step to the simpler and more general phenomena; reaching at last the simplest and most general.[703]

Around 1855, then, Spencerian "evolution" was still the process whereby simpler elements coalesced into more complex compounds of elements. The OED even credits Spencer with coining a new term for this coalescence: in his second book, *Principles of Psychology* (1855), the word "integration" denoted a process where diverse parts synthesized into an harmonious whole.

And just as the phrenological feeling of "conscientiousness" could be analyzed into simpler units of sympathy, Spencer argued that all nervous activity could also be broken down into simpler elements. The element of consciousness was a nervous shock, or reflex – and different types of consciousness could be produced through synthesis, "by the compounding of this element with itself and the recompounding of its compounds with one another in higher and higher degrees...."[704]

Borrowing Carpenter's discussion of reflexes in *Principles of Human Physiology*, Spencer saw reflexes as serial reactions to external stimuli. The pressure of complex environments meant the increasing complication of these reflexes, and memory and instinct were therefore compounded reflexes. When he looked back upon his psychology

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[703]Spencer, *The Principles of Psychology*, p. 71.
[704]Spencer, *The Principles of Psychology*, pp. 539, 120.

in 1870, Spencer explained to his correspondent that one part - his “Special Synthesis” - traced the evolution of reflex action through instinct, memory, reason, the feelings and the will. Conversely his “Special Analysis” resolved intelligence from its most complex to its simplest elements.[705]

This shared habit explains why Spencer, like the phrenologists, did not believe in a unified ego: it was an illusion to think of the ego as “something more than the composite state of consciousness which then exists.”[706] His insight applied to basic nervous functions too – there occurred a sort of secondary, independent, set of activities of which we were not aware. Thus after learning to walk, one became unaware of it: the muscular tension and the combinations of sensation and contraction that it took to balance formed an independent series of changes occurring alongside our consciousness. Spencer applied analysis to a scale of being, in which the simplest animals had the least integrated nervous functions: that is, in the lowest animals each part of the organism performed all of the vital functions independently.[707]

Robert M. Young maintained that Spencer gradually moved away from this “faculty psychology” derived from phrenology to associationism, in the process taking away the independent status of the mental faculties.[708] I am unsure about this assessment – though it is clear that Spencer made the reflex the dominant element in his system, he still thought that certain parts of the brain were quite important as places where reflexes were synthesized into compound mental states. In Spencer’s own words, these parts of

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[705]Richards, *Darwin and Evolutionary Theories of Mind*, p. 283; H. Spencer, Manuscript, "An Intellectual Autobiography by Spencer written for Youmans", 1870, SP, ULL, MS.791/355/6.
[706]Spencer, *The Principles of Psychology*, pp. 617-618.
[707]Spencer, *The Principles of Psychology*, pp. 495-496.
[708]R. M. Young, *Mind, Brain, and Adaptation in the Nineteenth Century : Cerebral Localization and its Biological Context from Gall to Ferrier* (Oxford: Oxford University Press, 1990), pp. 161-162, 181.

the brain became "agencies".[709] His use of the word *agency* was used in a way quite strange to us, for by this term Spencer did not imply choice, or activity, or even the control of the surrounding body-part; instead "agency" meant a localized office where manifold environmental impressions were reacted to through simple or compound reflex activity.

Indeed, though Spencer repudiated phrenology, a later writer - stung by this rejection - noted just how much he had borrowed from it, showing how Spencer spoke constantly about the independence of each mental faculty. The writer concluded that the discussions about faculty independence could have only come from phrenology.[710] But the phrenologist was likely overstating his case, as similar analytical assumptions were held in the more respectable neurosciences: recall the widespread contemporary belief that the encephalon and spinal cord were coalesced nervous centres, each nervous centre, or ganglion, partly independent.[711]

To support his psychology, Spencer used recurring exemplar organisms like certain invertebrates and acephalous infants. He quoted directly from the fourth edition of W.B. Carpenter's *Principles of Comparative Physiology,* thereby also borrowing from Marshall Hall and George Newport. Since ganglia were only responsible for governing their own part of the body, argued Spencer, many of the lower invertebrates weren't really unitary organisms at all but showed a "dispersion of the psychical life": a decapitated praying mantis would stay balanced, even recovering when pushed over, because the remaining ganglia could retain control. A centipede could continue to walk

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[709]Spencer, *The Principles of Psychology*, p. 610.
[710]Hollander, "Herbert Spencer as a Phrenologist," p. 149.
[711]Clarke and Jacyna, *Neuroscientific Concepts*, p. 31; Jacyna, "Principles of General Physiology," pp. 50-51.

forward if decapitated; if the nervous cord linking each segment's ganglia was severed, the body below the injury would still be governed by its own set of ganglia, and move in opposition to the part above the injury. Acephalous infants showed the independence of the parts of the nervous system, noted Spencer: despite their lack of a cerebrum, they could still perform reflex activities such as crying, breathing or sucking.[712] These examples were instances of the simplest extant nervous systems; Spencer therefore depicted them as the analytical elements out of which more complex nervous systems were synthesized. Spencer's psychology related increasingly-complex nervous phenomena not only to a linear scale of being, but also temporally. He had enunciated an evolutionary psychology. Spencer noted that as an individual's nervous system appeared, its psychical changes became increasingly co-ordinated, "their various strands connected."[713] The synthesis of elements therefore occurred over time.

In the second half of the 1850s, however, Spencer began to emphasize differentiation in development so that evolution became *both* a process of differentiation and integration. This movement to include palaetiology in his work seems to have been partly caused by his support of Huxley over Owen, as the battle between the two intensified; in this case different habits of reasoning were emphasized according to shifting social relationships. Nicolaas Rupke has noted how Spencer's "Ultimate Laws of Physiology" approvingly used Owen's theory on the vertebrate skeleton to discuss the "longitudinal integration" of organisms; but after Huxley's attack upon Owen's work in his Croonian lecture, Spencer also started to attack Owen's work, rejecting his explanation for serial homology. Though in his autobiography, Spencer claimed to have

[712]Spencer, *The Principles of Psychology*, pp. 492-494.
[713]Spencer, *The Principles of Psychology*, p. 496.

doubted Owen's work from the beginning, Rupke notes that Spencer's 1857 article showed no such scepticism.[714]

One reason for this change was because of Spencer's increasing insistence upon using adaptation to the environment as a cause – by 1867 he argued that instead of vertebrate segmentation being caused by an internal "vegetative" force, it was caused – "superinduced" - by "mechanical intercourse with the environment."[715] The demands of Spencer's philosophical system demanded that he present a somewhat consistent explanation. But another reason for Spencer's change was his switch of allies – he was changing allegiances to Huxley. By 1858 Huxley was writing testimonials for Spencer,[716] and so it probably wouldn't have been judicious for Spencer to trumpet rival theories to Huxley's while trying to make a name as an author.

Spencer chose well: eventually in the mid-1860s joined the X-Club, a sort of mutual support network of scientific naturalists. Including Huxley, Busk, Hooker and John Tyndall, they shared a naturalistic understanding of the universe and wanted to promote this outlook. Together they would help shape British science behind the scenes.[717] Spencer had fulfilled his dream of moving in culturally authoritative circles. Unlike the self-effacing person depicted in the *Autobiography*, Spencer seems to have been ambitious and exceptionally aware of the people with whom he was in contact.

His quiet ambition was probably formed from childhood influences. Upon his hiring at the *Economist,* uncle Thomas Spencer thought that Spencer would eventually be

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[714]Rupke, *Richard Owen*, pp. 206-207.
[715]H. Spencer, *The Principles of Biology*, 2 vols. (London: Williams and Norgate, 1867), p. 2:110. It was Spencer's charge about serial homology that was later attacked by Owen's student St. George Mivart.
[716]H. Spencer to T.H. Huxley, 31 December 1858, HP, IC, 7.104-105.
[717]R. Barton, "'Huxley, Lubbock, and Half a Dozen Others': Professionals and Gentlemen in the Formation of the X Club, 1851-1864," *Isis* 89, 410-444 (1998): pp. 416-417.

"identified with the good sense of sound principles of England" and "be able to speak with considerable power upon the political movements of the day."[718] In turn Spencer sent his uncle a collection of autograph letters from politicians like Peel, Cobden, Bright, Landor and Crabbe.[719] Two years later, the now-42-year-old man made another demonstration of his growing influence to his father: on the back of a letter that John Lubbock sent him, Spencer proudly announced that he had "made some new friends" and noted how John Lubbock was the son of the famous Sir John Lubbock.[720] His need for approval shows why Spencer resisted change: he never stopped trying to please his father and uncle.

PRINCIPLES OF BIOLOGY: OVERVIEW

Spencer's 1864-1867 *Principles of Biology* marked his strongest attempt to combine the analytic and synthetic habit and the palaetiological habit. It presented evolution as made up of two different processes – of differentiation and of "integration" (Spencer's new word). It was primarily a philosophical work attempting to unify various recent biological findings – thus he first presented the various "inductions" of biology, then a philosophical view of them and how deductions from his premises held up nicely with various observations made by biologists. Here, *Principles of Biology* will be investigated as a philosophical answer to various questions about individuality noted in the previous chapters.

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[718]T. Spencer to H. Spencer, 18 Nov 1848, SP, ULL, MS.791/24.
[719]Autograph Letters to my Uncle Thomas…Peel, Cobden, Bright, Landor + Crabbe [ND], SP, ULL, MS.791/11.
[720]J. Lubbock to H. Spencer, 16 Mar 1862, SP, ULL, MS.791/64.

Principles of Biology attempted to solve biological "data" and various biological "inductions" by proposing that they could be explained through *evolution,* the movement from homogeneity to heterogeneity through differentiation and integration. Three factors were at play in evolution: first, the opposition between the "individuation" of an organism and the forces of "genesis" (the various ways in which reproduction of new individuals occurred either sexually or asexually). Second, the survival of the fittest, or Darwin's mechanism of natural selection. Third, the inheritance of functionally acquired characteristics (what was to become known as 'Lamarckian' evolution) – in which the play of "incident forces" upon organisms shaped their structure and function, and these adaptations were in turn passed on to following generations.[721] It was this play of incident forces which Spencer saw as the most important factor of evolution (thus explaining things like serial homology).

Spencer would apply these principles over a three-year period, in successive instalments to subscribers. There he tried to explain biological phenomena like Growth, Development, Function, Morphology, Waste and Repair, Genesis, Variation, Herdity, Classification and Distribution and the Multiplication of the Human Race.[722] It was quite a comprehensive list.

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[721]Thus a later disciple of Spencer's was to define the organism as partly the expression of the environmental forces acting upon it. Spencer define himself as a "Lamarckian" in 1867, however – indeed he read Lamarck as stating that adaptive modification was not the cause of *progression* but of *irregularities* in what was otherwise a natural progression of forms. H. S. R. Elliot, "Herbert Spencer (1820-1903)," in *DNB* (1912); Spencer, *The Principles of Biology*, pp. 2:88-89, 1:408.
[722]Spencer, *The Principles of Biology*, pp. i:vii-viii.

PRINCIPLES OF BIOLOGY: LEVELS OF INDIVIDUALITY

Biological individuals were Spencer's elementary units which, through evolution, both differentiated and integrated into higher and higher "orders" of individuals, like "superorganisms." He disagreed with Huxley's tactic of calling a single individual the entire product of a single ferilized germ. Instead, following his definition of agency as a localized office where manifold environmental impressions were compounded and reacted upon, Spencer defined the biological individual as any organization continuously maintaining equilibrium with its surroundings.[723]

But a glance at his manuscript indicates that Spencer had trouble with such an open-ended use of the term "individual." In one section he noted that individuals were variously "gemmiparously produced"; there were formed "strings of...individuals" which then separated into "individuated groups of segments"; formed groups of "united individuals of successive segments". He crossed out this part, probably because of its terminological confusion.[724]

As a response to this uncertainty it seems that Spencer postulated different levels, or orders, of individuality. He favourably quoted Matthias Schleiden's belief that the individual was the "subjective comprehension of an actual object" – meaning that individuality was relative to the observer. The solar system could be seen as an

[723]Spencer, *The Principles of Biology*, pp. i:204 -207. This definition does not quite match Bernard's later notion of *homeostasis* in that the Spencerian notion of a dynamic equilibrium proposes the inner individual's "environment" matching its external environment, not maintaining independence from it.
[724]H. Spencer, Manuscript, "Principles of Biology", Herbert Spencer Collection, British Library, London, Additional Manuscripts. Page 120, crossed out section.

individual, or it could be seen as an aggregation of planets;[725] and, likewise, the human body could be seen as a biological individual, or it could be seen as an aggregation of cells. It depended upon what the observer was trying to describe. As noted above, these orders of individuality bear some interesting resemblances to the Wesleyan Methodist system of organization in which delegates were sent from local organizations to district meetings and delegates were in turn sent from these meetings to increasingly 'higher' meetings.

It is noteworthy that in Germany Ernst Haeckel was doing the same thing at just about the same time, setting out his theory of "tectology" in 1866. Haeckel proposed six levels of individuals in an ascending hierarchy: "plastids," or unicellular organisms and cells; "organs," meaning anything from groups of fused cells to organ-systems; "antimeres," meaning halves of bilaterally symmetrical bodies; "metameres," or segments; "persons," like conventionally understood individuals; and "corms," like colonial animals or populations of "individuals."[726] This similiarity of hierarchies of individuality may have occurred through mutual debts to Schleiden. At any rate Huxley told Haeckel about Spencer's *Principles of Biology,* and Haeckel seems to have read it by 1868; that year he wrote to Spencer noting the similarity of their work and urging him to

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[725]Spencer, *The Principles of Biology*, p. i:202.

[726]Indeed it appears as though Spencer's account of hierarchies of individuality appeared just before Haeckel's - always sensitive to his property rights, Spencer noted the exact appearances of the instalments comprising *Principles of Biology,* and if we use these, then his discussion about "orders of individuality" appeared between January of 1863 and June of 1865, at least six months before Haeckel. Spencer, *The Principles of Biology*, pp. 1,2:v. Nonetheless this search for priorities is probably not a very interesting one as earlier "resemblances" can always be found – as I shall note below, Charles Darwin informally and Louis Agassiz more formally deployed notions of "orders of individuality" before Spencer and Haeckel. On Haeckel's work on Siphonophores (marine invertebrates like Portuguese Man-o'-War) and compound individuality see M. Winsor, "A Historical Consideration of the Siphonophores," *Proceedings of the Royal Society of Edinburgh* 73, 315-323 (1971-1972).

look at his own *Generelle Morphologie,* especially chapters 5, 9, and 10 of the first volume, all of which dealt with tectology and individuality.[727]

In turn to help his readers understand levels of individuality Spencer used the familiar example of the marketplace. Thus though a consumer might only see a single "unit," the retailer kept dozens of them; the wholesaler had a gross, and the manufacturer supplied in groups of 100 gross. This view applied to any organism too, since it could be an individual of the first, second, third or even fourth order. Even though the ultimate units were the same, we could group these simplest elements together through compounding, and by grouping these compounds into groups themselves.[728]

By setting out different orders of individuality, Spencer set out a hierarchy of individuality. And by using individuals as his simplest biological 'elements' Spencer noted how the evolutionary process synthesized and integrated these individuals into compound individuals, marking the ascent of the hierarchy of individuality. If life began long ago with simple and small forms out of which all individual organisms arose, and if these smaller forms budded but then did not separate, then it would be "impossible to say where the lower individualities ceased, and the higher individualities commenced".[729]

One example was a familiar one cited above. Spencer noted the repeating segments of annelids - in some of the lower annelids, each successive segment not only had its own legs and internal organs, but also its own eyes and reproductive organs. Following Milne Edwards and Bonnet, he noted how each segment was therefore a "physiological whole", an individual. Displaying Huxley's lecture diagrams, quoting him on how the insect head and body were quite obviously fashioned out of repeating

[727]SP, ULL, MS.791/73. (in German).
[728]Spencer, *The Principles of Biology*, pp. 2:5-6.
[729]Spencer, *The Principles of Biology*, p. 1:204.

segments, Spencer proposed that these segments were independent individuals themselves. This was actually the case for all articulates, but we could not see this very easily because these segments had gradually integrated and hidden this primordial individuality.[730]

Segmentation occurred because newly-budded individuals didn't separate but instead stayed attached to the parent, forming a chain or cluster of individuals, a true compound organism. It was often useful for these individuals to stay united because of the mechanism of natural selection (the "survival of the fittest"). Spencer also used the differentiation through the division of labour to explain their greater success: a physiologically differentiated organism had additional benefits such as greater strength or higher reproductive abilities. At this point Spencer thought that this increased differentiation *necessarily* meant the increasing dependence of each individual-component upon all of the other individual-components, meaning the greater integration of the whole.[731]

Following Huxley's lead, Spencer reinterpreted various organisms cited by previous researchers in the light of this process of evolutionary differentiation and integration. The union of protoplasm produced organisms such as sponges. A *Hydra* could be divided into *Amoeba*-like portions and each could move about independently – something caused by the "half-independent co-operation" of the *Amoeba*-like organic elements composing the *Hydra*. This union occurred if the separation of individuals from a parent was postponed. *Hydrozoa* showed this delayed separation when "gemmiparous individuals" didn't separate from the parent, forming a "permanent aggregate of the third

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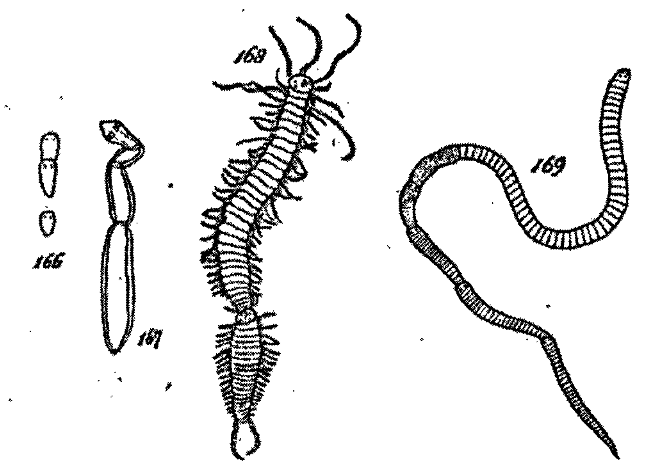
[730]Spencer, *The Principles of Biology*, pp. 2:91, 101-102, 98. Despite the similarity of the example, Spencer did not cite these two prior researchers (though he cited Milne Edwards in other cases).
[731]Spencer, *The Principles of Biology*, pp. 2:84-87.

order." And *Botryllus* (compound sea squirts depicted in the previous chapter) revealed "a number of individuals so completely combined as to simulate a single individual".

To further illustrate the hierarchy of individuality, Spencer showed a diagram of evolution in four familiar specimens – *Hydra* (fig. 166), *Taenia* (fig. 167), and *Syllis* (related to Naids, fig. 168 and 169), which displayed how "primordially, the segments were independent individuals" but had delayed budding:



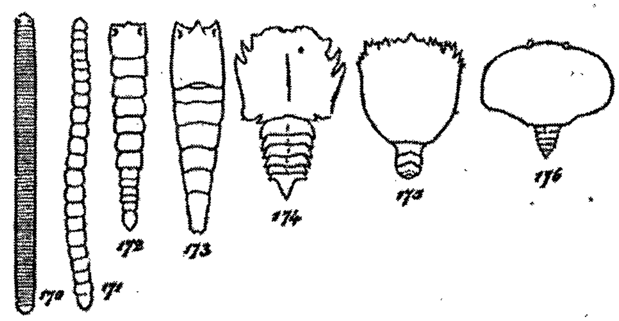
H.Spencer, *Principles of Biology*, 2 vols (London: Williams and Norgate, 1867), p. 2:95

Thus in figure 169, the "terminal" individual *Syllis* (bringing up the rear) was the eldest of the seven individuals. Higher-level organisms were distinguished from lower-level ones by how strongly the individuality of the larger organism (the "tertiary aggregate") dominated the individuality of the segments (the "secondary aggregate").[732]

To show how higher orders were more concentrated, or "integrated," than lower orders, Spencer laid out six more specimens to establish a further contrast.

[732]Spencer, *The Principles of Biology*, pp. 2:83-84, 2:86-87, 2:95-96. Figure 169 omitted the appendages of *Syllis*.

H. Spencer, *Principles of Biology*, p. 2:100



Working in his familiar tradition, Spencer had again cited familiar exemplar organisms: a millipede, a centipede, an "isopodous" crustacean and four types of "decapodous" crustaceans, ending with the crab. Spencer was merely following the path of least resistance, copying previous depictions of exemplar organisms.[733] He was was working in a long tradition of questions about individuality, his citation of these exact specimens provides us with more evidence that he felt these questions were pressing ones that were worth answering.

By depicting evolution as a general process of integration over time, Spencer - like his recapitulationist predecessors before him – had set out yet another similarity-scheme that focused the readers' expectations and allowed them to fit other examples in his new evolutionary context. Like a numerical series or pattern of declension, it allowed his readers to set memorization aside, making their learning easier by making an extraordinary range of phenomena appear related.[734] And in addition to psychology and biology Spencer applied this in a fairly consistent way to sociology, education and ethics too, creating a sort of crib-sheet which one could use to understand the world.

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[733]He even acknowledged that figure 169 above was copied from Milne Edwards. Spencer, *The Principles of Biology*, p. 2:95.
[734]Barnes, *T.S. Kuhn and Social Science*, pp. 18-20.

Spencer's ability to relate a wide range of phenomena through evolution is best conveyed in Jack London's 1913 novel, *Martin Eden.* London shows the thrill that Spencer's work had on the curious, dramatically shown when the eponymous working-class character (based loosely upon London's own experiences) first read the *System*:

> That, in the fabric of knowledge, there should be any connection whatever between a woman with hysterics and a schooner carrying a weather-helm or heaving to in a gale, would have struck him as ridiculous and impossible. But Herbert Spencer had shown him not only that it was not ridiculous, but that it was impossible for there to be no connection. All things were related to all other things from the farthermost star in the wastes of space to the myriads of atoms in the grain of sand under one's foot. This new concept was a perpetual amazement to Martin, and he found himself engaged continually in tracing the relationship between all things under the sun and on the other side of the sun. He drew up lists of the most incongruous things and was unhappy until he succeeded in establishing kinship between them all - kinship between love, poetry, earthquake, fire, rattlesnakes, rainbows, precious gems, monstrosities, sunsets, the roaring of lions, illuminating gas, cannibalism, beauty, murder, lovers, fulcrums, and tobacco. Thus, he unified the universe and held it up and looked at it, or wandered through its byways and alleys and jungles, not as a terrified traveller in the thick of mysteries seeking an unknown goal, but observing and charting and becoming familiar with all there was to know.[735]

By allowing his readers – many of them newly educated, like Jack London – to unify the universe, Spencer's writings grew increasingly popular. In this fashion he "set free" the minds of "countless" contemporaries, to use William James's expression. His essay on *Education* did very well but his 1873 *Study of Sociology* was his single greatest success, reaching eight editions by 1880, marking him as the greatest philosopher of the age.[736]

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[735]J. London, *Martin Eden* (New York: Penguin, 1984), pp. 150-151.

[736]H. S. R. Elliot, *Herbert Spencer* (London: Constable, 1917), p. 305; James, *Memories and Studies*, p. 126; Peel, *Herbert Spencer*, p. 1.

SOCIAL ORGANISMS

One of Spencer's innovations was to return to a comparison of society with an organism. He extended this similarity in an extensive way but tellingly used lower organisms like annelids, sea squirts and *Hydra* to illustrate the similarities between societies and organisms. He criticized previous social theorists like Hobbes who associated society with the human body: Spencer noted that this restricted example made the comparison fail.[737]

Instead Spencer compared societies to simpler organisms like articulates and polyps because of their segmental organization – their elements could be compared to self-sustaining societal elements like a tribe. Society's evolution from simple to complex occurred through the differentiation and integration of these previously-independent and uniform elements into higher-order individuals. Spencer returned to his early religious experiences of local chapter-meetings coalescing to form churches, and turned Milne Edwards's discussion of organisms as aggregations of workshops full circle.

For instance Spencer used animal reproduction and regeneration to illustrate how new societies appeared. Young *Hydra* emerged from the parent body to become independent, but in higher members of this class (like the Portuguese Man-o'-War) – while the young also emerged, they remained attached to the parent, forming a compound animal. Spencer likened the *Hydra* to simple tribes that multiplied by detaching parts of

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[737]H. Spencer, "The Social Organism," in *Essays: Scientific, Political, and Speculative* (London: Williams and Norgate, 1863), 265-307, pp. 269-271. This is a republication of his 1860 *Westminster Review* article.

themselves to live separately from the parent – meanwhile, complex hydroids lived together as a series of more differentiated, more integrated, tribes.[738]

Spencer used the image of the social organism to show how evolutionary integration accompanied evolutionary differentiation. Examples culled from Owen's work conveyed how simpler societies, like simpler organisms, had lower divisions of labour, while more complex societies and organisms had higher divisions of labour. In the simpler segmented animals, like annelids, the body had numerous, almost-identical segments each with equal portions of the digestive and circulatory system; each segment had its own ganglion that combined impressions, meaning that each segment showed agency. More complex segmented animals had more integrated segments, and in these organisms the internal organs no longer serially repeated. Spencer noted that this process occurred in societies too: in the feudal era various small communities gradually lost their independence as they integrated into a larger organization. But they still retained traces of their initial primitive segmentation through old divisions like counties and parishes, just as the highest segmented animals showed traces of segmentation in their muscles and exoskeletons. And just as the "sustaining" internal organs integrated through evolution, so too did the sustaining institutions of society gradually integrate: English cotton-manufacturing had spread into North Derbyshire from its original location in Lancashire, and the stocking-trade had spread into the segments of Leicestershire and Nottinghamshire.[739]

Spencer used his understanding of ganglia and the reflex arc to depict the communication centres of both sorts of organisms. In doing so he was following a long

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[738]Spencer, "The Social Organism," pp. 280-281.
[739]Spencer, "The Social Organism," pp. 287-289.

line of research into nervous systems. He noted that in segmented animals, each segment's ganglion was nominally independent, akin to societies with a number of small and independent kingdoms. As evolution proceeded, however, a king or other figure collected advisors around him who communicated information; thus in place of a solitary governing unit there grew up a group of governing units. In nature one saw more complex segmented animals with a chief ganglion assisted by minor ganglia, and local independence was given up. Yet the advising ministers/ganglia exercised their own control over the ruler, and over time the ruler became only an automatic centre, content to reflect only impressions.[740]

Spencer granted that differences existed between an organism and a society, but sought to minimize them. Though societies didn't have external forms like organisms, simpler organisms were also indefinitely shaped. Though societies weren't continuous masses, organisms weren't always continuous masses either – for instance the living parts of a hydrozoon were distributed through a gelatinous inert substance – and didn't people in a society live in places covered by simpler life forms, like vegetation? "Hence the members of the body-politic are not to be regarded as separated by intervals of dead space, but as diffused through a space occupied by life of a lower order". Another criticism was that all members of a society had feelings – and did not an animal body only feel in its nervous tissue? Spencer pointed out that in the simplest animals, all parts had an equal degree of sensitivity.[741]

The political conclusions that Spencer drew from his comparison of a society to an organism were anarchic or *laissez-faire*. The context of the disunity of the organism

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[740]Spencer, "The Social Organism," pp. 300-304.
[741]Spencer, "The Social Organism," pp. 273-276.

must be recovered in order to understand why Spencer thought this was the case. Following previous researchers in the analytic and synthetic habit, like Milne Edwards, Spencer thought that differentiation *had* to be accompanied by integration: as parts grew more specialized this tradition held that they had to become more dependent upon one another. This is what Spencer meant when he spoke of the increasing division of labour requiring the increasing reciprocal dependence of each part upon every other part. Because of this 'necessary' implication Spencer felt that there was no requirement for a coordinator or controller of these parts: social order, like biological order, emerged spontaneously through the interaction of its elements, instead of through planning or commands issued downwards through a hierarchy.[742]

Spencer's contemporaries recognized the anti-authoritarian emphasis in his biology and his politics. T.H. Huxley's statement - shown at the beginning of this dissertation as an epigraph - about biological and political philosophers supporting one another is likely directed against Spencer. Another commentator on Spencer's biology noted that if the sociologist looked into his own body he would find that the

> great physiological problem of the living body is really one of Sociology, namely, what is the greatest increase of power and substance which may accrue to any tissue with least expenditure of effort on its own part; and Nature has so worked out this problem in her organized forms that we find that *somehow* every tissue is protected and cared for in proportion as it, in its own life, assists its neighbors in satisfying their individual needs.

The body was made up of "distinct communities of untold myriads of living individuals", the cells: each one lived for itself but at the same time relied upon other cells for its own health.[743]

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[742]M. W. Taylor, *Men versus the State : Herbert Spencer and late Victorian Individualism* (Oxford: Oxford University Press, 1992), p. 146.
[743]H. Sewall, *Herbert Spencer as a Biologist* (Ann Arbor: Andrews & Witherby, 1886), p. 2.

It was this binary relationship that Spencer was trying to explain in his attempt to reconcile the palaetiological habit with its evolutionary correlate of differentiation, with the analytic and synthetic habit with its evolutionary correlate of integration. But at some point – it appears that he began re-thinking their relationship around 1870 – he began to emphasize that integration was far more important in the evolutionary process than differentiation.[744] To understand why Spencer began to increasingly value integration over differentiation we must turn to the person who was most intertwined with Spencer in the public mind, Charles Darwin.

CHARLES DARWIN AND CHANGING TREES

Spencer's evolutionary approach was seen as exceptionally novel by his contemporaries. His use of evolution as an explanatory device for different realms was his mark of distinction, which he had begun in the early 1850s. The rise of Charles Darwin's explanation of taxonomy as descent with modification must have been a mixed blessing for the ambitious Spencer, both useful and galling as Darwin's accounts began to mingle with his own evolutionary accounts. Thus Alexander Bain, one of Spencer's psychologist competitors, told him in 1863 that he had read the 1855 *Principles of Psychology* with attention and admiration: "you have certainly constituted yourself *the* philosopher of the doctrine of Development...notwithstanding that Darwin has supplied a most important link in the chain".[745] Spencer started to emphasize integration over differentiation in order to distinguish his own evolutionary accounts from Darwin's, to

[744]Spencer, "An Intellectual Autobiography by Spencer written for Youmans".
[745]A. Bain to H. Spencer, 17 Nov 1863, SP, ULL, MS.791/67.

counter what he saw as the overly disintegrating tone of Darwin's work. Moreover Spencer saw Darwin as merely explaining how evolution worked in biology and natural history, one of the 'links in the chain' – *his* explanation, meanwhile, was supposed to be far more wide-ranging.

Darwin's later work can be seen as an exemplar of the palaetiological habit. Yet his work had not always emphasized differentiation – recovering the context of compound individuality not only transforms our picture of 19th century biology, but of Darwin's work more specifically. He initially inhabited the common context of compound individuality and then broke from it, moving from the habit of analysis and synthesis to the new habit of palaetiology.

We know that in 1838 Darwin still thought of trees as compounds of "bud-elements." He privately noted "...I am sure I remember my pleasure in Kensington Gardens has often been greatly excited by looking at trees [as] great compound animals united by wonderful & mysterious manner.-".[746] And Darwin applied this view of compound individuality to his invertebrate research too. M.J.S. Hodge has already shown how Darwin linked invertebrates with trees. Studying with Robert Grant in Edinburgh, he linked the "associated life" of zoophytes (of *Flustra,* what we'd now call Bryozoans) with a tree considered as a colony of buds. After reading his grandfather Erasmus Darwin's *Phytologia,* a work beginning with a meditation on the "Individuality of the Buds of Vegetables,"

[746]C. Darwin, *Notebooks, 1836-1844 : Geology, Transmutation of Species, Metaphysical Enquiries*, ed. P. H. Barrett, et al. (Ithaca, N.Y.: Cornell University Press, 1987), p. M41:529. This entry is dated about autumn 1838. [As] was indicated [at] in the notebook; interpreted by the editors to be "as". See also that page's footnote 41-1, on Erasmus Darwin's note on the resemblance between the tree - a "congeries of living buds," and the coralline (a marine invertebrate) - a "congeries of a multitude of animals."

Darwin privately noted in 1839 how a tree, seen as a union of buds, made *Flustra's* compound individuality easier to understand.[747]

Darwin carried out other researches into compound individuality. On the *Beagle,* for instance, he bisected planarians, a type of animal which had "always interested" him; they produced two perfect individuals after two weeks. Darwin was following earlier planarian research by John Dalyell, who cut planarian bodies in various places, making them two heads that actually duelled over which direction the body was to take. And following statements by researchers such as Charles Bell, Darwin also noted that individuals divided into many fragments each crawled in the "proper" direction, as if each fragment had a true "anterior extremity," or head.[748] That the cautious Darwin published these observations shows that he presumed the problem of their individuality to be an interesting one and that others would share this interest.

Common research interests into compound individuality gave Darwin the insights allowing him to reinterpret entities across levels of organization. In other words, like Spencer and Haeckel, Darwin also thought of individuality in a hierarchical fashion, as relative to the observer. The common context of compound individuality builds upon Hodge's observation that Darwin understood entities like species as analogous to individuals, and entities like buds or cells as analogous to individuals too. Phillip Sloan

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[747]M. J. S. Hodge, "Darwin as a Lifelong Generation Theorist," in *The Darwinian Heritage*, ed. D. Kohn (Princeton: Princeton University Press, 1985), 207-243, pp. 210, 212-213.

[748]C. Darwin, "Brief Descriptions of Several Terrestrial *Planariae,* and of some Remarkable Marine Species, with an Account of their Habits," *Annals and Magazine of Natural History* 14, 240-251 (1844): p. 244; idem, *Journal of Researches into the Natural History and Geology of the Countries visited during the Voyage of H. M. S. Beagle round the World, under the Command of Capt. Fitz Roy*, new ed. (New York: Appleton, 1871), pp. 26-27. In the planarian with the duelling heads, Dalyell observed that "the inclinations of the superfluous head were not always in unison with those of the remainder of the animal; that it preferred quiescence, when they chose motion; and that it would willingly have traversed its element [slithering along the bottom of the jar] could its strength have predominated over the inactivity of the major parts". J. G. Dalyell, *Observations on Some Interesting Phenomena in Animal Physiology, Exhibited by Several Species of Planariae* (Edinburgh: Archibald Constable, 1814), p. 64..

has similarly argued that colonial invertebrates, which confounded conventional views about biological individuality, allowed Darwin to liken an individual organism to a species.[749] The common context of the disunity of the organism shows how Darwin allowed the different parts of colonial marine invertebrates or plants to stand for different units at different levels of organization: they could stand for constituent units; for individuals; for colonies; for species. This willingness to allow different parts to stand for different units at different levels of organization also appears to resemble John Goodsir's cellular researches.

At the same time Darwin emphasized differentiation. For example, Darwin applied the physiological division of labour to groups of constituent units at different levels:

> The advantage of diversification in the inhabitants of the same region is, in fact, the same as that of the physiological division of labour in the organs of the same individual body – a subject so well elucidated by Milne Edwards.[750]

In this way, Darwin transformed Milne Edwards's principle of the physiological division of labour into what Camille Limoges has called the "ecological division of labour". Not only was the division of labour applied to the animal body but to the population of a region. As with body parts, specialization favoured organisms adapting to new niches in the economy of nature.[751]

But Milne Edwards's principle of specialization was now deployed to understand temporal divergence: the Darwin of the 1859 *Origin of Species* differed from the author of the earlier *Notebooks* in that he was less interested in compound individuality. Darwin

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[749]Hodge, "Darwin as a Lifelong Generation Theorist," p. 209; P. R. Sloan, Manuscript, "Breaking the Circle: the Historical Interpretation of Quinarian Classification", 1991, pp. 19, 21-22.
[750]C. Darwin, *On the Origin of Species. A Facsimile of the First Edition* (Cambridge, MA: Harvard University Press, 1964), pp. 115-116.
[751]Limoges, "Milne-Edwards and the Division of Labour," pp. 328-330.

the palaetiologist left analysis and synthesis behind, becoming more interested in differentiation as divergence. As a result, certain important questions started to be forgotten or ignored. One example is serial homology, the repetition of segments or vertebrae. In the *Origin,* Darwin appropriated Owen's homological work, translating special and general homology into ancestral, historical terms – an archetype could now be seen as a parent. But Rupke notes how serial homology was far more problematic for Darwin. One could not speak of a serially repetitive body part as being an 'ancestor' of another similar body part, especially in the same organism. An arm could not be seen as ancestral to a leg; one vertebra or segment could not be seen as descended from another.[752]

Darwin did try to explain serial homology: "Why should one crustacean, which has an extremely complex mouth formed of many parts, consequently always have fewer legs; or conversely, those with many legs have simpler mouths?" he rhetorically asked. His answer: because the jaws of a crab were originally true legs or some more primordial appendage. This historically explained why the crab's jaws retained "leg-like" characters, and thus visible serial homology implied low organization. But then how did this serial homology originate? Darwin cited Owen's principle of "vegetative repetition", weakly tried to reinterpret it in historical terms, and then changed the subject.[753]

Darwin's dance away from polymorphism was immediately noticed in the United States by esteemed invertebrate researcher Louis Agassiz; Agassiz's attack recalled the fading common context of compound individuality. He pointed out that individuality was obvious in the higher animals and maintained by "genetic transmission" between

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[752]Rupke, *Richard Owen*, pp. 209-210, 218.
[753]Darwin, *Origin of Species*, pp. 149, 437-439.

generations. But in radiates (like Acalephs), this obvious individuality was rarer. Like Spencer and Haeckel and Darwin, Agassiz *also* made distinctions between different types of individuality – "derivative individuality"; "hereditary individuality"; "secondary individuality"; even "complex individuality." So how could one even determine the existence of species variation if one couldn't even distinguish between the different individuals comprising that larger organism? He sniffed that *real* naturalists looked at tougher exemplars than Darwin had mustered to make his case. In the case of lower invertebrates such as radiates,

> Surely, we have here a much greater diversity of individuals, born one from the other, than is exhibited by the most diversified breeds of our domesticated animals; and yet all these heterogeneous animals remain true to their species, in one case as in the other, and do not afford the slightest evidence of a transmutation of species. Would the supporters of the fanciful theories lately propounded, only extend their studies a little beyond the range of domesticated animals, - would they investigate the alternate generations of the Acalephs, the extraordinary modes of development of the Helminths, the reproduction of the Salpae, etc., etc., - they would soon learn that there are in the world far more astonishing phenomena, strictly circumscribed between the natural limits of unvarying species, than the slight differences produced by men, among domesticated animals; and, perhaps, cease to be so confident, as they seem to be, that these differences are trustworthy indications of the variability of species. For my own part, I must emphatically declare that I do not know a single fact tending to show that species do vary in any way, while it is true that the individuals of one and the same species are more or less polymorphous.[754]

It is telling that Agassiz returned to the problem of alternate generations and exemplar organisms like salps to criticize Darwin's theory of natural selection. In Agassiz's view, Darwin had limited himself only to the animals with the most obvious individuality – vertebrates - and then the most familiar, domesticated, ones at that. By bringing up compound individuality, researchers such as Agassiz thought they had respectable

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[754]L. Agassiz, *Contributions to the Natural History of the United States of America*, 4 vols. (Boston: Little Brown and Co., 1857-1862), pp. 3:98-99. This is part of a larger section, "Individuality and Specific Differences among Acalephs", pp. 2:88-90. This third volume was issued in 1860, one year after Darwin's *Origin of Species.*

intellectual grounds for opposing Darwinian natural selection, because the problem allowed them to question Darwin's very ability to distinguish between different species, a problem which had "nothing to do with the question of their origin."[755]

But Darwin had already moved to obscure the problem of compound individuality in a more basic way. Like Carpenter before him, Darwin moved away from the interpretation of plants as compound individuals to plants as historical entities. He did this with trees: Darwin moved away from his grandfather Erasmus's view of trees as compound individuals and now deployed them as diagrams of historical changes. Trees were transformed from familiar depictions of compound individuals to familiar depictions of palaetiological differentiation.

Darwin's change from a spatial to an historical interpretation of trees has already been noted by historians like Sloan. He has argued how colonial marine invertebrates, like coral, can be interpreted either spatially or historically: certain types of coral even resemble trees, and yet unlike trees individual coral polyps are linked to one another only by the bones of their ancestors. Thus Darwin was to write that "The tree of life should perhaps be called the coral of life, base of branches dead; so that passages cannot be seen."[756] In this fashion the polyp – a leading organic element in the habit of analysis and synthesis, was depicted differently.

In 1837 Darwin linked species with another familiar organic element of analysis, that of the plant-bud.

> All animals <are> of same species are bound together just like buds of plants, which die at one time, though produced either sooner or later.- Prove animal like

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[755] Agassiz, *Natural History of the United States*, p. 3:99.
[756] Darwin, *Notebooks, 1836-1844*, p. B25:177; Sloan, "Breaking the Circle", pp. 19-21.

> plants: trace gradation between associated & non associated animals.- & the story will be complete.-[757]

In this case the plant was a compound individual, an association of plant-buds. But by 1859 the plant had become his palaetiological "tree of life" - buds were now important because they gave origin to new species-branches.

> The affinities of all the beings of the same class have sometimes been represented by a great tree. I believe this simile largely speaks the truth...As buds give rise by growth to fresh buds, and these, if vigorous, branch out and overtop on all sides many a feebler branch, so by generation I believe it has been with the great Tree of Life...[758]

The only graphic in the *Origin* was his famous depiction of this tree of life, the "Diagram of Divergence of Taxa." The tree had become Darwin's exemplar organism of evolution, a way of depicting evolution as a ramifying, open system of possibilities.

Darwin had opted for a picture of evolution as emphasizing development as centrifugal, showing differentiation and divergence. Against his previous use of the analytic and synthetic habit he now used palaetiology. A key difference between the two modes of development is that one habit offers a closed system while the other habit offers an open one. In analysis and synthesis a system is closed because it is nothing more than the sum of its synthesized elements. In palaetiology a system is an open one because units are characterized in terms of their history and in terms of their future possibilities – it is not so much their nature as entities that is important to the palaetiologist, but their 'trajectory.' In the palaetiological tree of life the ensuing branches become something larger and greater than the starting point, as novelties continuously appear and two or more ramifications emerge from any single point.

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[757]Darwin, *Notebooks, 1836-1844*, p. B73:189. <are> is deleted.
[758]Darwin, *Origin of Species*, pp. 129-130.

This long Darwinian digression has depicted an increasingly powerful account of evolution that Spencer saw as rivalling his and which increasingly shaped Spencer's reception by audiences. In other words Spencer started to be interpreted in a Darwinian light.

As Spencer made a living from writing, he was extremely interested in how his audience interpreted his work. And thus he began to fear that the word "evolution" was slipping out of his control. Three years after Darwin's *Origin* appeared Spencer openly mused over whether he ought to use the word "involution" instead of "evolution" to convey his notion of development, for it would more strongly convey not only differentiation, but also a process of integration whereby organic elements concentrated over time. But he decided to retain the word "evolution" because he thought that this word would be more easily understood by his readers.[759]

Perhaps "involution" would have been better understood as conveying the developmental process as one of integration. But Spencer had to consider his audience if he was to continue making money as an author – and so he continued to write about "evolution." In the end this contributed to his interpretation in a solely palaetiological light. His work would increasingly become associated with Darwin's and in the end judged entirely against its palaetiological, differentiating, evolutionary process. For instance where Spencer's 1852 essay "A Theory of Population, Deduced from the General Law of Animal Fertility" was written as a way of extending Richard Owen's analytic insights about metagenesis, it was eventually to be interpreted by one sympathetic historian as an essay that was mainly noteworthy as an "anticipation of

[759]H. Spencer, *First Principles* (London: Williams and Norgate, 1862), pp. 285-286.

natural selection."[760] With the common context of compound individuality forgotten, of course, any contest between Spencer and Darwin would show Spencer's work as lacking.

HUXLEY AND THE "CONTRADICTION" OF A LIBERAL SOCIAL ORGANISM

We ended by wondering what might have happened if Spencer had become known not as the philosopher of "evolution" but instead as the philosopher of "involution." This section examines how his loss of control over the term "evolution" led to different audiences drawing different conclusions from Spencer's work and how the disappearance of the problem of compound individuality helped shape their interpretations, leading to his decline.

It is a matter of considerable historical interest how Spencer, who was shaped by questions about the disunity of the organism, and who defined individuality both as a relative concept (depending upon the stance of the observer) and as the adjustment of internal relations to external ones (thus always bearing in mind the relationship of the individual with his environment) came to be interpreted far differently. Increasingly Spencer was read as stating that individuality was an atomistic concept; he was read as the exemplar political 'individualist' and portrayed as the paradigm Social Darwinist who proposed that society was simply a collection of individuals driven by competitive *laissez-faire* concerns and nothing more.

Spencer's anti-authoritarian themes were thus traditionally interpreted to have Social Darwinist 'implications' that supported capitalist robber barons like Andrew

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[760]M. Ruse, *The Darwinian Revolution : Science Red in Tooth and Claw* (Chicago: University of Chicago Press, 1979), pp. 153-154.

Carnegie. To be sure, this was one faction that derived support from Spencer's work. But other anti-authoritarian social groups also obtained support from Spencerian writings. Anarchists, for instance, took heart from Spencer's note that the most desirable form of life was a cooperative one in which people voluntarily created or joined mutual assistance societies to help one another. Mathematician and scientific naturalist William Clifford compared Spencer to anarchist philosopher Pierre-Joseph Proudhon.[761] The anarchist Petr Kropotkin - who wrote *Mutual Aid* against the individualist view of nature and against T.H. Huxley's view of nature as a "gladiator show" - also saw associations of animal units as the key to the evolutionary process. Kropotkin even explicitly noted a kinship between his own views, Spencer's, and the French zoologist Edmond Perrier (author of *Les Colonies Animales).* In turn Perrier provided many of the biological examples for Émile Durkheim in his 1893 *De la Division du Travail Social;* Durkheim's discussion of simple "segmental societies" seems to have literally referred to invertebrate segments that could survive independently if separated, a pattern showing a lineage to Perrier, thence to Milne Edwards and perhaps even Bonnet.[762]

To begin the story of Spencer's downfall we begin with one of his career high points. The winter of 1871-1872 marked a highlight for Spencerian explanations of compound individuality and levels of individuality. In his 1872 Presidential Address to the Entomological Society of London, Alfred Russel Wallace - co-discoverer of the

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[761]W.K. Clifford to H. Huxley, 19 April 1876, HP, IC, 12.242-243; Taylor, *Men versus the State*, pp. 94-95.
[762]P. A. Kropotkin, *Mutual aid : a Factor of Evolution* (Montreal: Black Rose Books, 1989), p. 53. See also J. Sapp, *Evolution by Association : a History of Symbiosis* (Oxford: Oxford University Press, 1994).for a discussion of the Kropotkin - Spencer affinity. E. Perrier, *Les Colonies Animales et la Formation des Organismes* (Paris: Masson, 1881). Durkheim refers to Perrier *seven times* over pp. 208-210, citing Perrier's argument that all organisms are structurally colonial. E. Durkheim, *De la Division du Travail Social* (Paris: Alcan, 1893). Strangely, however, Limoges - the noted historian of biology - defers to Lukes's ahistorical explanation that Durkheim's "metaphorical style" of linking societies with organisms led him into trouble, that Durkheim thereby "misrepresented his own ideas"! Limoges, "Milne-Edwards and the Division of Labour," pp. 333-334.

principle of natural selection - saw Spencer's theories about compound individuality as solving the problem of why insects had repeating segments. This repetition was known as the problem of serial homology. Praising *Principles of Biology,* Wallace noted that an insect might be "a compound, representing as many individuals as there are true segments in the body, these individuals having become severally differentiated and specialized to perform certain definite functions for the good of the whole compound animal". Proposing that each segment was originally a separate individual that had integrated into a larger "individual" over time might solve the origin of insects, and by extension serial homology. Wallace even noted the similarity between Owen's 1843 *Lectures on Invertebrates* and Spencer's views.[763] Wallace's own past helps explain his receptiveness to Spencer's work: *Social Statics* had a "permanent effect" on his ideas and beliefs on political and social matters. Earlier, Wallace had been a committed phrenologist who read Combe's *Constitution of Man* in 1844, and had become so interested in phrenology and phreno-mesmerism that he conducted his own phrenological research.[764]

Despite this high praise for Spencer from such an eminent Victorian, the only written response to Wallace's speech was a letter one month later denying the compound individuality of insects, arguing that "the conception of segmentation is erroneous which leads to ascribing to insects peculiar physiological or physical properties on account of their being composed of 'a number of individualities fused into one.'" Insects were not a number of individualities fused into one, but instead one individuality partly broken into many. The letter steadfastly denied the centripetal integration of independent units and

[763]A. R. Wallace, "Wallace on the Origin of Insects," *Nature* 4, 1872): pp. 350-351. Wallace's speech was given on 22 January. I learned about this speech through R. G. Perrin, *Herbert Spencer : a Primary and Secondary Bibliography* (New York: Garland, 1993).
[764]A.R. Wallace to H. Spencer 13 Nov 1873, SP, ULL, MS.791/89; A. R. Wallace, *My Life : a Record of Events and Opinions*, 2 vols. (London: Chapman & Hall, 1905), pp. 1:234-236.

spoke instead of the centrifugal emergence of a unitary individual from a central source.[765] This palaetiological emphasis upon differentiation was understandable because the author of the letter was the young Edwin Ray Lankester, protégé of T.H. Huxley.

It was especially Lankester's terminology that differed from Wallace's. Where words like "individual" and "compound" appeared liberally in Wallace's address, they did not appear in Lankester's letter at all. Instead he used terms such as "zooids" and "somites" to denote the various constituent units of the insect. To explain their similarity, Lankester referred to the "theory of somites" or to "zooid-production."[766] In other papers he used words such as "prostomium" and "peristomium" (explicitly adopted to refer to the "somites" comprising the mouth and its appendages), which he thought would help improve their identification. Lankester was introducing nomenclature changes from Huxley's *Lectures on General Natural History.*[767]

Since Huxley had used the word "zoöid" in the early 1850s to avoid referring to independent marine invertebrate parts as individuals, he had steadfastly retained this terminology in later presentations on colonial marine invertebrates. "A whole tree of Sertularia, a Pennatula, a Pyrosoma, a mass of Botrylli, must no longer be considered as an aggregation of individuals, but as an individual developed into many zoöids".[768] Other researchers, like George Allman, also started using this word, and the term "compound" began to disappear from other biologists' vocabularies too.[769] As people

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[765]E. R. Lankester, "The Segmentation of Annulosa," *Nature*, 443-444 (1872): p. 443.
[766]Lankester, "The Segmentation of Annulosa," p. 443.
[767]E. R. Lankester, "On some New British Polynoïna," *Transactions of the Linnean Society of London* 25, 373-388 (1867): p. 373.
[768]Huxley, Foster, and Lankester, *Scientific Memoirs*, pp. 1:117-118.
[769]See, for example, Allman, "On the Anatomy and Physiology of *Cordylophora*," pp. 367-384, particularly the footnote on p. 379; G. Allman, *A Monograph of the Gymnoblastic or Tubularian Hydroids*, 2 vols. (London: Ray Society, 1871), p. 1:22. See also E. R. Lankester, "A Contribution to the Knowledge of the Lower Annelids," *Transactions of the Linnean Society of London* 26, 631-646 (1870): pp. 632-633. where

such as Lankester and Allman adopted Huxley's terminology, they also seem to have adopted the rejection of compound individuality that went with it. In their work, specimens that were formerly described as *compound organisms* instead became a collection of *zoöids*. In order for Huxley to refer to these troublesome, seemingly-independent parts without granting them undue independence, he had coined new words for them. After all, Huxley invented the word "agnosticism" to respectfully refer to unbelief. By inventing a new word as an option to the term "atheism" he tried to control debate by defining not only his own position, but also his opponents'.[770] In this same way, "zooid" may have helped close off the very possibility of expressing certain organisms as compound – terms like these can be thought of as palaetiological words.

Intellectually, this change was a sort of "dynamic nominalism", for as new descriptions for these organisms appeared, new possible interactions with them appeared too.[771] New names emphasized different aspects of organisms than had previous names, and new names therefore emphasized different research possibilities and ways of interacting with those organisms. But this change also closed off certain possible interactions with these humble invertebrates as Huxley's terms were adopted and became popular. For example, when faced with a *Nereis*, contrast the implicit research possibilities when one sees it as a *compound organism*, as a member of the repeating-brained group *homogangliata* that buds off new *individuals;* or, conversely, when one sees it as a collection of *zoöids* showing the phenomenon of *metamerism.* In this way

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the term "compound" is never used to describe *Chaetogaster limnaei,* a tiny parasitic worm which reproduced asexually. Lankester instead stated each "single individual" had an "attendant train of zooids."

[770]B. Lightman, "Fighting even with Death: Balfour, Scientific Naturalism and Thomas Henry Huxley's Final Battle," in *Thomas Henry Huxley's Place in Science and Letters: Centenary Essays*, ed. A. Barr (Athens: University of Georgia Press, 1997), 323-350, p. 325.

[771]I. Hacking, "Making up People," in *The Science Studies Reader*, ed. M. Biagioli (New York: Routledge, 1999), 161-171, p. 166. This critical point was raised by Darrin Durant, for which I am grateful.

Huxley and the zoologists whom he trained gradually rendered the problem of biological individuality - to borrow Nicholas Jardine's language - an "unreal" question; more concretely it seems to have become an irrelevant problem that ambitious young scientists such as Lankester no longer troubled themselves with.[772]

Socially, the decline of the terminology of compound individuality accompanied and reinforced biologists' growing professional and epistemological exclusivity. Lankester's public rejection of Wallace by his display of new terms is one example of this growing exclusivity.[773] Indeed, rather than rehearsing notions of biologists "professionalizing" by following ideals of how the biologist ought to act, it is also helpful to think of biologists as a Weberian "status group": a group of people that, within a larger group, claim special respect and begin to isolate themselves from that larger group by setting up boundaries. This is done in order to monopolize a particular field, frequently to ensure that the members of the group will each obtain a steady reimbursement. People can be socially excluded through credentials such as certificates or degrees: in addition to signifying competence these credentials allow group membership to be restricted. And status groups linguistically exclude others by inventing new words which must be learned and used in order to signify one's status group membership. Status groups frequently turn to the state to protect their monopolies (for instance think of the ultimate legal way in which a person practicing law without a licence can be prevented from doing this), and

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[772]Jardine, *The Scenes of Inquiry*, p. 51.

[773]Additional support for my description of one habit of thought replacing another is shown in Lankester's attempt to rename Owen's terms, like "homology", with something more in keeping with a genealogical tree: like "homogeny." See E. R. Lankester, "On the use of the Term Homology in Modern Zoology, and the Distinction between Homogenetic and Homoplastic Agreements," *Annals and Magazine of Natural History* 4, 35-43 (1870). and the rejection of this in S. G. J. Mivart, "On the Use of the Term 'Homology'," *Annals and Magazine of Natural History* 6, 113-21 (1870). The discussion in Merz, *A History of European Thought*, pp. 2:258-259, predates E.S. Russell's.

there are suggestions that this process of forming groups through exclusion is quite widespread, maybe even the norm.[774]

I suggest that what is generally known as the "professionalization" of biology might be called the 'status-grouping' of biology instead – that we see it as a process of monopolization and exclusion that occurred to ensure that biologists were financially rewarded by the state or by institutions regulated by the state. Local evidence supports this new perspective. One way that membership as a biologist could be enforced was through the examination, which determined how credentials were granted. Susan Cannon notes how the second half of the nineteenth century in Britain was marked by scientific societies' push for examinations and certification, and by Germanophile reformers who emphasized training.[775] As early as the mid-1850s J.D. Hooker tried to change how natural history was taught by ensuring that the textbooks upon which examinations were written were appropriate, and Huxley's group made other attempts to become assessors. By 1860, for instance, Huxley, Hooker, Carpenter and Busk were the examiners for the first B.Sc. degree at the University of London.[776] In this way they could increasingly enforce the uptake of new words such as "zooid", in that students who didn't use these words wouldn't be granted credentials and wouldn't become biologists.

This ensured the eclipse of words that could convey compound individuality. One can thus observe Huxley's terminology passing to a third generation of researchers: in 1877 Oxford, students copied out from their teacher, Lankester, that the sea squirt

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[774]Barnes, *Elements of Social Theory*, pp. 130-133, 145-146; R. Collins, *The Credential Society: an Historical Sociology of Education and Stratification* (New York: Academic Press, 1979), p. 72; idem, *Weberian Sociological Theory* (Cambridge: Cambridge University Press, 1986), pp. 128-132. For a survey of this problematic term by historians of biology see A. Desmond, "Redefining the X Axis: 'Professionals', 'Amateurs' and the Making of mid-Victorian Biology : a Progress Report," *Journal of the History of Biology* 34, 3-50 (2001).
[775]Cannon, *Science in Culture*, pp. 148-149.
[776]Barton, "'Huxley, Lubbock, and Half a Dozen Others'," pp. 426-427.

Botryllus was no longer a group of individuals forming a colony, but a collection of "ascidiozooids" united into an "ascidarium."[777] This builds upon David Allen's observation that in the last quarter of the century various biological disciplines became increasingly jargon-heavy, its practitioners speaking private and dense codes to maintain a boundary between insiders and outsiders. Outsiders trying to practice natural history became demoralized as a result.[778] Words such as "somite" and "metamere" and "zooid" were markers segregating the status group of elite, "professional," research biologists from outsiders – initially from mere field naturalists, and thence from the group that became the mere "amateurs."

Ironically Huxley then deployed his own work to make the unity of the individual self-evident against Spencerian compound individuality – he did this in order to undermine Spencerian anti-authoritarian political visions. Huxley's paper "Administrative Nihilism" - appearing in the winter of 1871 – asked, "upon what foundation does the authority of the State rest"? By undermining the notion of compound individuality, Huxley's paper in turn moved to undermine the scientific credibility of Spencerian *laissez-faire* political doctrines. Instead of seeing the nervous system as it had been interpreted by the contemporary neurosciences – as a system of quasi-independent ganglia coordinating external sense impressions and mediating reflex arcs - Huxley now characterized it as the "governing power of the body". Like Spencer, he used political imagery too, but of a darker type.

> The fact is that the sovereign power of the body thinks for the physiological organism, acts for it, and rules the individual components with a rod of iron. Even the blood-corpuscles can't hold a public meeting without being accused of

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[777]D. A. Power, Manuscript, "Lectures on Zoology by E. Ray Lankester, Oxford", 1876-1877, Royal College of Surgeons of England.
[778]Allen, *The Naturalist in Britain*, pp. 182, 192.

> 'congestion' - and the brain, like other despots whom we have known, calls out at once for the use of sharp steel against them. As in Hobbes's 'Leviathan,' the representative of the sovereign authority in the living organism, through he derives all his powers from the mass which he rules, is above the law.

Huxley noted that the image of the social organism suggested a greater level of government interference, working against Spencer's own libertarian or anarchist political interests.[779] Huxley cited Hobbes, asserting not only that central control was obvious, but that it was also necessary because anarchy would occur in the absence of central control. He used Hobbes as an example even though Spencer had explicitly rejected any reference to him because Hobbes had used the the human body as his exemplar.

Huxley used Hobbesian principles because of his support for hierarchical organizations. Jestingly but tellingly he once wrote to Hooker that they must constitute themselves into a "permanent 'Committee of Public Safety'" to keep an eye on Owen and the Natural History Museum; Ruth Barton has noted Huxley's belief that he and Hooker represented the true interests of the scientific community, defending them against personal politics and short-term interests.[780] Like Spencer, Huxley tried to appropriate the messages of evolutionary biology for his own political views; but unlike Spencer, Huxley supported elite scientists. Amused to hear that Huxley was to criticize the *Quarterly Review* for not admiring Darwin enough, St. George Mivart wrote to Owen that Huxley was now contradicting himself: Huxley had earlier vehemently repudiated any "*reverence for authority* in matters of science" while proclaiming his own lack of this reverence.[781]

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[779]T. H. Huxley, "Administrative Nihilism," *Fortnightly Review* 10, 525-543 (1871): pp. 530, 534-535.
[780]Barton, "'Huxley, Lubbock, and Half a Dozen Others'," p. 432.
[781]M. Fichman, *Alfred Russel Wallace* (Boston: Twayne, 1981), p. 104; St. George J. Mivart to R. Owen, 26 Oct 1871, OCORR, NHM, 19/267-268. Emphasis in original.

After all, Huxley was a Carlylean. In a telling image, Huxley denied the value of absolute democratic equality:

> I should be very sorry to find myself on board a ship in which the voices of the cook and the loblolly boys counted for as much as those of the officers, upon a question of steering, or reefing topsails; or where the "great heart" of the crew was called upon to settle the ship's course. And there is no sea more dangerous than the ocean of practical politics–none in which there is more need of good pilotage and of a single, unfaltering purpose when the waves rise high.[782]

Huxley's above statement imitated Carlyle's similar statement about sailing, that no ship could round Cape Horn by a vote of the crew. Frank Turner has pointed out a link between many scientific naturalists such as Huxley and the work of Thomas Carlyle. As they bid for cultural authority, the scientific naturalists derived support from Carlyle's call for a new social and intellectual elite.[783]

But Spencer utterly loathed Carlyle from the very first, when he met him at one of John Chapman's parties in the early 1850s. He thought that Carlyle's theory of society "took its colour from his own nature, which was despotic; and he sympathized with the unrestrained exercise of power which he liked and longed for himself.... his political view was naturally a reflex of his own nature."[784] Where Carlyle saw the feelings as part of "an assembly under the autocratic control of the 'will'"; Spencer instead thought that the feelings

> constitute an assembly over which there reigns no established autocrat, but of which now one member and now another gets possession of the presidential chair (then temporarily acquiring the title of 'the will') and rules the rest for a time: being frequently, if not strong, ejected by combinations of others, and occasionally, if strong, effectually resisting their efforts.[785]

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[782]Huxley, *Collected Essays*, p. 1:313.
[783]F. M. Turner, *Contesting Cultural Authority : Essays in Victorian Intellectual Life* (Cambridge: Cambridge University Press, 1993), p. 137.
[784]H. Spencer, Manuscript, "About Carlyle", [1882-1883], SP, ULL, MS.791/355/4.
[785]Carlyle proposed the "forcible deposition of the tyrant emotion" by the conscious will. Spencer thought this impossible – while a strong emotion like sorrow reigned, as in the mind of a mother who had lost a child, all consciousness was entirely occupied by it. A strong emotion could not be consciously

In Spencer's view, order resulted from the combination of the units that composed it. Compare how close the image of a political assembly is to the one used by phrenologist Sidney Smith in 1838. The example of the legislature has recurred once again: to repeat, the mind can be depicted as a society writ small.

Thus Spencer defended himself against Huxley's charge of a "contradiction" in the image of a liberal social organism. He did this in a fashion befitting an opposition between Carlyleans and anti-Carlyleans. In the very next issue of the *Fortnightly Review* Spencer distinguished between two forms of order in the social organism: an *external* one in which the different external organs were directed by a government "capable of directing their combined actions", like a strong nervous system; and an *internal* form of "visceral co-operation" in which local ganglia, through connection with other ganglia, regulated the automatic functions like nutrition or respiration. Both systems influenced one another. But Spencer moved quickly to emphasize the independence of the parts making up the organism, noting that this internal, visceral cooperation was far more important to the life of the organism: "Digestion and circulation go on very well in lunatics and idiots, though the higher nervous centres are either deranged or partly absent."[786]

To illustrate his point that societies didn't need to be controlled, he once again brought up lower invertebrates as his exemplar organisms. Spencer especially favoured the *Hydrozoa,* probably using this particular exemplar organism to make a private point

overthrown: it could only be exhausted, and a change in mental state "spontaneously effected". Spencer, *ABHS*, pp. 1:279-280.

[786]H. Spencer, "Specialized Administration," *Fortnightly Review* 10, 627-654 (1871): pp. 632-633, 640.

with Huxley, as Huxley had written an early treatise on them.[787] Spencer noted that *Hydrozoa* lacked any nervous centre at all, yet they seemed to flourish. Indeed, each one of these animals was composed of many different parts, and each part was made up of very dissimilar cells, like thread- or ciliated-cells. But each group of units pursued their "individual" lives (which Spencer called their "respective 'interests'") without any direction by a nervous system or nervous centre, and they nonetheless tended to cooperate for the good of the whole. Surely this was also the case for the vital parts of a higher animal, like its digestive or circulatory system – this was why lunatics and idiots (and acephalous infants) could still live.[788] Thus he saw no reason to abandon his link between society and an organism, as the parts of both organizations showed spontaneous order.

THE REINTERPRETATION OF SPENCER

It was after this exchange that Huxley and Spencer began to openly disagree about how society should function. When Huxley and Spencer were both invited to join the London Liberty League, Huxley turned them down because he concluded that either he or Spencer would end up in a "false position" because of their political differences.[789] In the 1870s and onwards, Spencer's work became increasingly famous, and more people began to attack it. Spencer's voluntaristic politics of competition but also mutual assistance began to be reinterpreted in the light of Darwinian self-preservation and Spencer's own phrase, "survival of the fittest."

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[787]Huxley, *The Oceanic Hydrozoa.*
[788]Spencer, "Specialized Administration," pp. 640-642.
[789]Huxley, Huxley, and Strachey, *Life and Letters of T.H. Huxley*, p. 2:195.

As the political and economic situation changed, Spencer was forced to change with it as best he could in order to appear consistent with his earlier principles; after all, he was a philosopher stubbornly engaged in a forty-year project explicitly founded upon those early principles. The changing political atmosphere meant that he had to choose new friends: he was forced to gradually associate with the hated Tories in support of property rights as the rise of the New Liberalism supported government intervention in new areas of society like the provision of education, gas and water.[790] A recession in the early 1880s showed the possibility of economic decline as well as progress, dampening Spencer's cosmic optimism. This optimism was further harmed by Weismann's attempt to demonstrate the all-sufficiency of natural selection and the germ-soma distinction – a development which was seen to imply that acquired functions could not be transferred from parent to child. Weismann's work further meant that ethical improvement might not be inevitable either. Finally, Spencer he began to despair at the growth of British imperialism and militarism.

In late 1889 Huxley used the *Times* to publicly attack Spencer's *a priori* approach to science and politics (specifically over the issue of land socialism). Infuriated by this public criticism, Spencer resigned from the X-Club. Adrian Desmond concludes that this division was rooted in the opposition between Spencer - the anachronistic Nonconformist supporter of free competition - and a growing social democracy supported by Huxley, the government scientist who sought State money for science, libraries and schools.[791]

Indeed, Huxley's reputation waxed as Spencer's waned. His interests were different, particularly in the role that the State ought to play in funding science and

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[790]Taylor, *Men versus the State*.
[791]Desmond, *Huxley*, pp. 573-574.

education. Huxley was one of the first members of the London School Board, joining in 1870. It is worth noting that Huxley ended his speech on "Administrative Nihilism" by calling for the British government to give more funds to museums, or research monies to the Royal Society to distribute.[792] Indeed, Huxley's "Administrative Nihilism" appeared before a number of changes in Victorian science. In 1872 the Liberals appointed the Devonshire commission, raising the expectations of many Victorian scientific naturalists that state funding for science would increase. The perceived failure of this commission to give more money to British scientists was irritating to these scientists, and they found themselves with little influence despite their claims to hold socially useful knowledge.[793]

The scientists' continuing lack of influence may have brought on a rhetorical shift: they began to emphasize their utility in a more national and competitive context. Frank Turner noted that after 1875 the spokesmen for British science shifted their language from peace, cosmopolitanism, self-improvement and progress to nationalism, patriotism and political elitism. Science was no longer portrayed as a way to improve the moral condition of the student and of humanity in general, but instead as a way to create better British citizens and a more productive economy. In the late 1870s various attacks were made on the competence and scientific illiteracy of British political leaders. It was charged that the British political system lacked scientific procedures and that science was the only way to rescue the national interest from partisan politics. Huxley claimed that he sought merely to reinforce British liberalism while at the same time calling for citizens to avoid partisan prejudice. In judging politics against science and finding politics wanting,

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[792] Huxley, "Administrative Nihilism," p. 542; Weldon, "T.H. Huxley."
[793] Turner, *Contesting Cultural Authority*, pp. 204-205.

Huxley, like other scientists, proposed that politics should be seen not as a power battle, but instead as a mode of rational administration.[794]

Indeed in his reinterpretation of the social organism one might see Huxley as a forerunner of the group which James Scott calls "high modernists" – small elites of the late 19th and early 20th century, confident to an often-utopian degree in the ability of science and scientific progress to fundamentally change the lot of humanity. This confidence meant their dismissal of other sources of judgement;[795] from 1880 and onwards, after all, the common assumption was that science would make social arrangements rational and controlled by people, as ignorance and scarcity were to be worked out "like a long-division sum."[796]

An excellent vehicle to advance these claims would be Huxley's image of a centralized social organism, with scientists and other experts presumably sitting in its cortex. Power is thereby associated with knowledge. The use of this picture by those who sit, or more importantly who want to sit, in society's brain not only validates those elites and would-be elites. It also strengthens their will to act in ways that they deem to be society's interests, even if they have to compel people to act in those ways; for these new experts claim to know others' interests better than the non-experts themselves know.

James Scott has already noted the appearance of the image of 'society's brain' in two twentieth-century contexts, that of revolutionary politics and architecture. In both, a small group saw itself as sitting in a centre and issuing directives to those on the peripheries; both used the human body for their example of natural order. Lenin spoke

[794]Turner, *Contesting Cultural Authority*, pp. 205, 208-209.
[795]J. C. Scott, *Seeing like a State: how Certain Schemes to Improve the Human Condition have Failed* (New Haven, Conn.: Yale University Press, 1998), pp. 4-8, 93-94.
[796]J. Harris, *Private Lives, Public Spirit : Britain, 1870-1914* (London: Penguin, 1994), p. 33.

of the Bolsheviks as the leaders of a revolutionary army, with the Party as the "brain" that motivated and directed the unthinking "masses" towards revolution. Le Corbusier spoke of "monocephalic" cities where skyscrapers in the core of his utopian city issued commands to the rest of the peripheral country. Scott notes that this image justified the claims of this small "central" group to give these orders, allowing them to ignore or overrule "peripheral" outsiders' objections.[797]

Scott observes that another image advancing the claims of a experts to become an elite is that of the garden. In one of Huxley's most famous works, *Evolution and Ethics,* which appeared near the end of his life, he noted that the ethical progress of society depended not on imitating the cosmic process, but combating it. For his new example of what society ought to become Huxley cited the garden – "it was obvious that only in the garden of an orderly polity can the finest fruits humanity is capable of bearing be produced".[798] But the image of society as a garden is a favourite of someone requiring that society be ordered along rational and aesthetic (or scientific) criteria: the gardener imposes her own principles of order and beauty on a nature perceived to be anarchic, a potential jungle which must be cultivated.[799] Though this may have indicated Huxley's rejection of Darwinism as a prescription for how we ought to live, the image of society as a garden also means that someone has to do the gardening. In short, using the example of a garden again strengthened Huxley's professional ideal, of scientifically-trained experts as a Weberian status group prescribing for - and even designing and re-designing - societies. All of this went against Spencer's populism and anti-clericalism.

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[797]Scott, *Seeing like a State*, pp. 254, 111-12, 149.
[798]Huxley, *Collected Essays*, pp. 9:83, 9:55.
[799]Scott, *Seeing like a State*, pp. 92-93.

A WARNING TO GENERALISTS

This section examines Spencer's reinterpretation as an extreme individualist in histories up to the present day by looking at how historians and other academics took up Huxley's charge that Spencer had contradicted himself. In the mid-20th century one historian of political thought summarized this "contradiction":

> An organism is a unity with a nerve-centre; that nerve centre regulates the whole body; and thus of a sudden the 'growing' organism which should not be regulated becomes a bureaucratic or socialistic state under control of the central brain. Starting with a conception of organic growth intended to justify individualism, Spencer ends with a conception of organic unity which tends to justify socialism. Huxley, with his keen eye, fixed on this inward contradiction in his essay on *Administrative Nihilism* (1870)....[800]

By this reading, Spencer's organic image 'logically implied' a strong nerve centre, which was equated with more central control and thus a greater degree of State intervention.

Huxley's critique provided later commentators with an appealing story. Robert C. Bannister notes that Huxley's criticism of the Spencerian social organism became a "beacon for Spencer's critics for several decades", reinforcing calls for state activism. Thus in 1898 Lester Frank Ward echoed that centralization and a strong state was the "logical" outcome of the social organism analogy (a sentiment uncritically echoed by Richard Hofstadter in at least four editions of his *Social Darwinism in American thought,* ranging from 1944 to 1965). In 1904 this accusation was repeated by John Dewey; in 1929 by Ivor Brown; in 1933 by F.J.C. Hearnshaw; in 1957 by J.W. Gough; in 1961 by Werner Stark; in 1964 by Stanislav Andreski; in 1978 David Wiltshire concluded that the political and scientific components of Spencerism were not merely incompatible, but

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[800] E. Barker, *Political Thought in England, 1848 to 1914*, 2nd ed. (Oxford: Oxford University Press, 1959), pp. 96-97.

helped legitimize Fascism, for any social organism would have to be firmly controlled. In 1983 and 1989 Ellen Frankel Paul noted this contradiction.[801] In 2000 John Burrow argued that the Spencerian social organism implied *dirigisme.*[802] Mike Hawkins's 1997 account on Social Darwinism correctly linked Spencer's social organism with the dispersed nervous system of invertebrates, but mentioned that this association was "arbitrary", noting Huxley's role in pointing out this incoherence.[803] In another discipline (political philosophy), Tim S. Gray has performed the most comprehensive work on solving this 'problem,' devoting a 1985 article and 1996 monograph[804] to reconciling individualism and organicism in Spencer's thought. Other analysts who pointed out this contradiction in Spencer's thought are noted in the footnotes.[805]

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[801]S. Andreski, *Elements of Comparative Sociology* (London: Weidenfeld and Nicolson, 1964), pp. 173; R. C. Bannister, *Social Darwinism: Science and Myth in Anglo-American Social Thought* (Philadelphia: Temple Univ. Press, 1979), pp. 144-145, 31-32; I. J. C. Brown, *English Political Theory* (London: Methuen, 1920), pp. 129-135; J. Dewey, "The Philosophical Work of Herbert Spencer," *Philosophical Review* 13, 159-175 (1904): p. 166; J. W. Gough, *The Social Contract, a Critical Study of its Development*, 2nd ed. (Oxford: Clarendon, 1957), pp. 213-216; F. J. C. Hearnshaw, *The Social and Political Ideas of some Representative Thinkers of the Victorian Age* (New York: Barnes & Noble, 1933), pp. 72-73; Hofstadter, *Social Darwinism in American Thought*, p. 80; E. F. Paul, "Herbert Spencer - the Historicist as a Failed Prophet," *Journal of the History of Ideas* 44, 619-638 (1983): p. 628; idem, "Herbert Spencer-Second Thoughts - a Response to Michael Taylor," *Political Studies* 37, 443-448 (1989): pp. 443-448; W. Stark, "Herbert Spencer's Three Sociologies," *American Sociological Review* 26, 515-521 (1961): pp. 517-519; L. F. Ward, *Outlines of Sociology* (New York: MacMillan, 1898), p. 61; D. Wiltshire, *The Social and Political Thought of Herbert Spencer* (Oxford: Oxford University Press, 1978), pp. 255-256, 235.

[802]Burrow notes that "as the brain is a highly peremptory organ and the motor nerves, and the limbs they control, on the whole carry out orders; the model seems to be one for a strong, centralized government, with discussion allowed only in a kind of cerebral cabinet". Spencer's "...marriage of evolution with laissez-faire principles...was accomplished in the teeth of the most obvious implications of his organic analogy. Highly complex organisms, as he recognized, are, after all, highly *dirigiste.* The 'lower' functions...are spontaneous and self-regulating". Burrow argues that Spencer solved this problem by resorting to Lamarckian evolutionary explanation. J. W. Burrow, *The Crisis of Reason : European Thought, 1848-1914* (New Haven: Yale University Press, 2000), p. 74.

[803]Hawkins, *Social Darwinism*, p. 94.

[804]T. S. Gray, "Herbert Spencer - Individualist or Organicist?," *Political Studies* 33, 236-253 (1985); idem, *The Political Philosophy of Herbert Spencer : Individualism and Organicism* (Aldershot: Avebury, 1996).

[805]M. Francis, "Herbert Spencer and the Myth of Laissez-Faire," *Journal of the History of Ideas* 39, 317-328 (1978): p. 327; R. P. Hiskes, "Spencer and the Liberal Idea of Community," *Review of Politics* 45, 595-609 (1983): pp. 597-602; R. H. Murray, *Studies in English Social and Political Thinkers of the Nineteenth Century*, 2 vols. (Cambridge: W. Heffer, 1929), pp. 2:25-26, 2:37-39; W. M. Simon, "Herbert Spencer and the Social Organism," *Journal of the History of Ideas* 21, 294-299 (1960).

One reason why this 'contradiction' may have emerged was because of a near-contemporaneous fascination with a transition in British society from "individualism" to "collectivism". This dichotomy originated with works like A.V. Dicey's 1906 *Lectures on Law and Opinion* – there, Dicey emphasized a shift from private and local services provided by the community to the gradual expansion of the central state exemplified in the "new Liberal" laws of the first decade of the 20th century. Thus features like the freedom of contract was overtaken by administrative authority. José Harris notes how interested historians like Beatrice and Sidney Webb took up this dichotomy between individualism and collectivism, and Dicey's explanation thus became a way to *justify* the transition to collectivism. As a result this dichotomy still "haunts" analyses of the period.[806] My point is that it may have been this dichotomy between individualism and collectivism which also shaped later historians' opinion of Spencer as an atomistic individualist. Huxley's observation - that Spencer had contradicted himself - fed this dichotomy.

But Huxley's apparent "refutation" of Spencer's "contradiction" - repeated habitually and unreflectively by generations of academics - tells another interesting story. From the late 19th to the early 21st centuries, Huxley's reinterpretation of Spencer's social organism acted as a new societal myth. On one level it portrayed a heroic sceptic overturning a pompous dogmatist, much like the famous tale of Huxley's 1860 Oxford triumph over Bishop Samuel Wilberforce when Wilberforce tried to publicly oppose Darwinian descent with modification. But more subtly the telling and re-telling of Huxley's reinterpretation of Spencer's social organism became an example of a

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[806]Harris, *Private Lives, Public Spirit*, pp. 11-12; M. W. Taylor, *Herbert Spencer and the Limits of the State: The late Nineteenth-Century Debate between Individualism and Collectivism* (Bristol: Thoemmes, 1996), p. xi.

professional specialist, an expert confidently at home in his field, correcting and triumphing over the generalist, a mere philosopher who dared to trespass in a scientist's area of specialization. This account recurred, becoming an exemplar itself which quietly warned other generalists not to do the same thing.

Throughout this dissertation I have shown how the question about compound individuality was seen as a pressing problem by numerous elite London biomedical researchers; much of Spencer's work was written in response to this problem. He explicitly differed from other social theorists by choosing organisms that problematized biological individuality in order to illustrate certain anti-authoritarian principles of social order. But when Huxley returned to the human body as his example of an organism against which society should be compared, he disingenuously ignored the vast range of alternative organisms that could be used, even ignoring his own early invertebrate researches.

In using the human body for his example, Huxley made individuality and thus authority self-evident. The historians, philosophers and sociologists listed above who all rehearsed Huxley's "contradiction" trusted Huxley's word as an expert. But this trust came about because their very exclusion from biology – which he had helped bring about – made it difficult for them to come up with credible alternatives to his examples. The historiography surrounding the "contradiction" of a Spencerian anti-authoritarian social organism is itself an example of Spencer being judged by newly-created outsiders to biology, whose exclusion meant their ignorance of the common context of compound individuality.

CONCLUSION

> Herein lies the strength of the scientist. [While o]ther men in other pursuits have to dread and encounter the rivalry, enmity, and slander of their fellow workers; he is sure of the solidarity with him of his fellows; like all gangs, all guilds, its members are sworn to mutual support. They assume all the airs and use all the cant phrases of a priesthood; they speak of the rest of mankind as the 'lay public'; they deny the right or power of that public to penetrate their temples or to judge their creed; they assert the superiority and expect the deference which every other priesthood has asserted and expected, and during the season of its supremacy has obtained, from the superstition or the cowardice of the nations.
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> -"Ouida," *The New Priesthood* (London: E.W. Allen, 1893), 29-30.

In the previous chapter I suggested that we see "professionalizing" biologists as a Weberian status group. "Ouida's" quote forming the epigraph builds upon this in a more concrete way: at the time many saw Huxley and other scientific naturalists as a reformulation of the priesthood, the very group that Spencer grew up opposing. Indeed, the model profession for the scientist and scientific naturalists as they struggled for cultural leadership was not medicine, or the law, but the Church of England.[807] Huxley the scientist had issued the book of essays *Lay Sermons,* a title implying sermons made from a "scientific altar" supplanting those made by the Church. And George Holyoake called Huxley's group a "priesthood of science" in 1868.[808] By seeing Huxley as a new sort of cleric we better understand not only Spencer's opposition, but also Huxley's drive to acquire greater prestige, cultural leadership, and government funding for science, and various strategies that he and others took to convince non-scientists of the utility of science.

This conclusion puts these insights in a larger framework by noting how different habits of reasoning might have contributed to this view of scientists as a new priesthood.

[807] Barton, "'Huxley, Lubbock, and Half a Dozen Others'," p. 414.
[808] Desmond, *Huxley*, pp. 626-628.

It links link the habit of palaetiology with the rise of what Harold Perkin has called the "professional" ideal of how society ought to be organized, and the habit of analyis and synthesis with what he calls the "entrepreneurial" ideal. To understand the dispute between Spencer and Huxley over the State's role in science in society, we can depict each man as representing one of these two conflicting norms guiding late Victorian and early Edwardian society.

COGNITIVE ORDER AND SOCIAL ORDER

The clash of Huxley and Spencer was not simply over politics but over alternative visions about what counted as legitimate and certain knowledge. For illumination one can revisit Steven Shapin and Simon Schaffer's *Leviathan and the Air-Pump* and the conflict between two other iconic figures, Thomas Hobbes and Robert Boyle; from their clash we can infer similarities with Spencer and Huxley. Though the analogy can be taken only so far the parallels are instructive. Spencer can be depicted as a Victorian Hobbes, an anticlerical systematizing philosopher whose scientific writings are now mainly forgotten and who is read mainly as a social and political theorist.[809] In turn Huxley can be seen as a 19th century Boyle, a man who like Boyle busily created a network of specializing research workers. Though not a clash between natural philosophy versus the production of experimental matters of fact, we might see what I have called "habits of reasoning" as Wittgensteinian forms of life, and in the process try

[809]The analogy does fall down in one key area: Spencer drew libertarian conclusions where Hobbes drew authoritarian ones.

to understand how ideals about social order were expressions of a deeper cognitive order.[810]

In other words, it is rewarding to discover how the habit of analysis and synthesis supported certain visions of how society ought to be shaped, and how palaetiology in turn supported different visions of social order. In the conclusion to chapters one and two I proposed that the habit of analysis and synthesis reinforced, or even gave rise to, what Harold Perkin has called the entrepreneurial ideal. In turn the habit of palaetiology reinforced or gave rise to what Perkin calls the professional ideal. The entrepreneurial ideal spoke of the self-made man, of the beneficial way in which competition removed blockages to the free movement of capital. It was a portrayal of society as organized along horizontal lines, supporting the view of society as organized by class: indeed, accompanying this entrepreneurial ideal as a dark twin was an ideal of working-class solidarity and the labour theory of value. But against entrepreneurialism stood the professional ideal, which prized expertise, meritocracy and skilled labour. In this view, education and planning was valued and the 'wasting' of human talent was to be avoided. The route for social mobility, for bettering one's prospects, was through the acquisition of specialized knowledge, or expertise, and in turn professional groups sought to exclude those without that particular specialized knowledge. Perkin believes that professional society was organized in vertical career hierarchies instead of horizontal classes, and he insists that this professional ideal began to supplant the entrepreneurial ideal in the late Victorian era.[811]

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[810]Barnes, *Elements of Social Theory*, pp. 96-98; S. Shapin and S. Schaffer, *Leviathan and the Air-Pump : Hobbes, Boyle, and the Experimental Life* (Princeton: Princeton University Press, 1985), pp. 342-343.
[811]Perkin, *The Rise of Professional Society*, pp. 2-4.

This difference is exemplified in the X-Club split and sequence of battles between Spencer and Huxley. Indeed, Perkin's dichotomy has already been used by another historian of science, J.F.M. Clark, to understand the orientation of another member of the X-Club, John Lubbock. Instead of portraying Lubbock as yet another scientific naturalist opposing the clergy, Clark instead depicts Lubbock as a capitalist, and hints at a fault line running through the X-Club itself over a dispute between the entrepreneurial and the professional ideal.[812] To better understand this fault line, Clark's use of Perkin's scheme can be expanded upon by reviewing the different habits of reasoning and the different social options that they entailed.

The habit of analysis and synthesis emphasized elements that interacted and gave rise to compounds; it focused upon entities instead of processes; it portrayed any system (an organism, a society, a system of knowledge) as a closed one because it was merely the sum of its interacting elements and nothing more. Because of synthesis it also held out the possibility that development was centripetal: a form of spontaneous order could thus emerge from the interaction of its parts. The habit of analysis and synthesis further offered the possibility that philosophical knowledge accompanied or could even replace empirical inquiry. One can point out the similarity between Spencer and Hobbes in that both philosophers held up the example of geometry as what it meant to reason rightly – analysis and synthesis provided a person with a path to certain knowledge.

To build upon this, recall that studying Euclidean geometry was seen by the early Victorians as important in any person's education, because it instilled mental discipline and trained them in a most important habit of reasoning. Euclidean geometrical axioms

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[812]J. F. M. Clark, "'The Ants were Duly Visited': Making Sense of John Lubbock, Scientific Naturalism and the Senses of Social Insects," *British Journal for the History of Science* 30, 151-176 (1997): pp. 154-155; Perkin, *The Rise of Professional Society*, pp. xiii-xiv.

were used to describe spatial reality, setting out what was obviously true. Its definitions and the geometrical findings deduced from them could not only be reconciled with empirical studies of geometrical figures: they could also compel assent. That is, if it was known that a triangle's angles added up to 180° both in geometry and in the observable world, then one couldn't assert or even imagine something to the contrary. Even John Stuart Mill had to agree with his antagonist William Whewell on the truth of mathematical ideas, regardless of whether these ideas were inductive or innate.[813] This meant that evidence could be judged both empirically and philosophically.

Because of this equivalence of philosophical and empirical knowledge offered in an analytic and synthetic habit, certain social possibilities can be said to follow. One is that of *populism.* Any person - even a newly-educated one - could learn the definitions and rules of Euclidean geometry and use them to generate increasingly complex theories which he could then see as matching the world around him. Likewise someone like Jack London could read Spencer, apply Spencer's rules to his observations and thereby 'understand' the structure, function and connections of all phenomena in the universe. It meant that everything could still be done in the head by anyone dogged enough to learn a Euclidean or Spencerian *System.* This helps to explain why Spencer's work was so popular with the newly-educated. Moreover the populism of analysis and synthesis also meant that exclusively empirical forms of research – with its emphasis upon laborious training techniques, institutions, and thus credentials and other exclusionary entry rituals – weren't as important, meaning experts were also less important.

Another possibility indicated by the habit of analysis and synthesis was that of a 'naturalistic' ethics. 'Naturalistic' appears in scare quotes because in this habit the term

813 Richards, *Mathematical Visions*, pp. 1-2, 29, 38.

was unnecessary: there wasn't really any alternative form of ethics than the ones that could also be found in nature by using that habit. If analysis and synthesis acted as royal roads to knowledge then they could establish that there were only certain ways in which a person ought to act, and any system following the habit of analysis and synthesis could find these ways. It was because of his use of analysis and synthesis that Spencer thought his system was capable of prescribing a form of evolutionary, naturalistic ethics.

Finally, by pointing to a form of spontaneous order created by the interaction of a system's elements, habit of analysis and synthesis held out the possibility of capitalism, anarchism and other voluntarist social patterns. Thus it was believed that markets, or competition, or cooperation, or other forms of voluntary interaction necessarily led to bodily and social harmony. No higher controller of this resulting order was required, as that order was nothing more than the synthesis of simpler elements. Understanding this habit helps contextualize Spencer's anti-authoritarian biology and social theory.

Meanwhile the palaetiological habit, to repeat, emphasized a centrifugal developmental direction, of differentiation emerging from a single central point. Where the converse of synthesis is analysis, the converse of palaetiology seems to be genealogy, where one moves back in time to find an origin, the initial simple act or substance where differentiation began. Darwin's tree of life – itself reinterpreted from compound organism into historical process – was one such genealogical depiction, a move to find the common ancestors of diverging branches. Another palaetiological/ genealogical tree was used in comparative philology, a field that sought the ancestors of modern languages and the ways in which modern languages became increasingly differentiated from those ancestral languages.

Conversely the centrifugal differentation of palaetiology pointed at fragmentation, of systems growing growing less and less unified as the branching-process of differentiation and specialization continued. Recall that Perkin's insistence that the professional ideal began to replace the entrepreneurial ideal. It is amidst this replacement of one ideal by another that we can set Spencer's rapid decline beginning in the early 1870s. This contextualizes Huxley's increasing complaints about Spencer's *a priorism:* Spencer can be seen as a sort of declining Euclidean, perhaps even 'the last Euclidean,' his attempt to synthesize all knowledge seen not merely as unfashionable, but as impossible.

For support one might point to Susan Cannon's point about the eventual disintegration of the Victorian "truth-complex" where people were forced to live with many possible truths; she states that what was held to constitute scientific proof even began to differ.[814] A specific case of this disintegration can be seen in the appearance of non-Euclidean geometry in Britain. It was found that one of Euclid's postulates couldn't be proven from the rest of his system, and that contradictory views could be put forward instead. The timing alone supports my belief that Spencer's decline began in the early 1870s: in the late 1860s this Continental non-Euclidean geometry suddenly appeared in Britain, and led to questions about whether Euclidean geometry truly described spatial reality or whether it was only one possible representation of it. By this destruction of geometry's claim to certainty, Victorians were forced to reassess the very nature of truth: Joan Richards has detailed how people like Hermann von Helmholtz and William

[814]Cannon, *Science in Culture*, pp. 3-4.

Clifford offered alternative geometric axioms, and then even alternative non-Euclidean geometric systems which were equally possible depending upon one's psychology.[815]

Indeed this 'new geometry' can be depicted as another ramifying tree of knowledge like Darwin's tree of life, one also emphasizing different and equally possible systems diverging from one another. Richards notes that it was over issues such as the nature of mathematical truth that the Metaphysical Society was formed in 1869, with the idealists arrayed against the empiricists. Clifford, interpreter of Riemann and also a scientific naturalist, spoke strongly for empiricism, believing that one can only know what one has experienced, not by grasping some transcendental point. Also in 1869, Huxley openly questioned the value of mathematics in education because he thought it was purely deductive and not really needed to train the mind. Instead Huxley advocated learning how to observe as far more important in a scientist's education.[816]

Contrast Huxley's criticisms with earlier Victorians' insistences that Euclidean geometry instilled mental discipline in those who learned it: Huxley the palaetiologist and scientific naturalist was now insisting solely upon empiricism, not deduction from *a priori* postulates. In so doing he directly attacked the utility of analysis and synthesis: no longer would philosophical knowledge be able to accompany or improve upon observation. The appearance of non-Euclidean geometry was part of a challenge to Spencer's claim to be presenting a certain and universal picture of the universe simply because he arrived at this picture by deducing it from simple *a priori* truths. After all, Huxley's criticisms of mathematical education and its overly deductive nature were made only two years before he began to publicly criticize Spencer.

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[815]Richards, *Mathematical Visions*, pp. 75, 67, 112, 96.
[816]Richards, *Mathematical Visions*, pp. 107-109, 133-134.

Like the habit of analysis and synthesis, the centrifugal habit of palaetiology also indicated certain social possibilities. In her point about the disintegration of the "truth-complex" in the late Victorian era Susan Cannon notes how scientists were no longer seen as possessors of truth, as what was seen as constituting scientific proof began to differ (thus a biologist could be a Darwinian or anti-Darwinian, regardless of what physicists claimed about the age of the earth). Instead they were increasingly portrayed as explorers into new realms.[817] The universe was deemed unknowable by any single person – a person had to be specialized, taking up only a single field of research. It meant that she had to undergo difficult rites of passage to acquire the credentials and training necessary to become and be seen as an expert, a process that in turn meant the increasing exclusion of outsiders and non-experts as status groups of scientists began to take shape. It meant that a non-expert could not possibly know a particular area as well as an expert, forcing that non-expert to trust the expert. Palaetiology also reinforced, and was in turn augmented, by changes in publication, where large octavo volumes were being replaced by increasingly-specialized articles submitted for "high journals" of science such as *Nature* or *Science*.[818]

Palaetiology entailed fragmentation too, as in the separation of different fields of investigation into nature as well as the separation of nature from ethics. Though Victorians initially looked to natural science for its norms (as in natural theology) a multi-normative world eventually emerged. Cannon notes that not only were there divided and warring scientific disciplines – like Darwin's evolutionary claims pitted against Lord Kelvin's physics over the age of the earth – but also in the separation of the

[817]Cannon, *Science in Culture*, p. 280.
[818]I thank Nicholas Jardine for pointing out the significance of this change from books to articles and for his stimulating discussion on this issue.

"moral sciences" from the "natural sciences." She believes that this separation of morality from nature was the result of the *Origin of Species,* which Huxley realized in *Evolution and Ethics*.[819] But the *Origin of Species* can instead be seen as part of a larger palaetiological habit of reasoning appearing in other fields like non-Euclidean geometry.

Palaetiology therefore also indicated the possibility of *anarchy*. If the study of nature and society prescribed multiple forms of social order, morality and ethics, then it raised the question about which of these manifold paths ought to be taken and in turn who ought to prescribe this path. In the same vein one could not have 'perfection' (of morality or culture, for instance) if the very standards of what constituted perfection were up for grabs.

Perkin has proposed that in Britain after 1880 the professional ideal gradually replaced the entrepreneurial one, particularly through critiques of industrial society. Spencer and other supporters of the capitalist/entrepreneurial ideal sought to prevent what they saw as government "interference" in society. But professionals sought to persuade non-professionals that their services were vital and ought to be adequately compensated: because these rewards were not always guaranteed by a free market, this required state interference. Though the professional ideal was initially limited only to people of leisure, like gentlemen, the industrialization of Victorian society - with its massive creation of new wealth, specialized tasks and new possibilities - made this professional ideal an option for increasingly large numbers of people. Anyone could be an professional if they were provided with the opportunities for an adequate education. Perkin even suggests that the rise of the welfare state - justified as a way to avoid wasting human talent - can

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[819]Cannon, *Science in Culture*, pp. 2-4, 276.

be seen as a way of extending the option of becoming a "professional" to the heretofore-excluded.[820] Huxley's famous *Evolution and Ethics* can thus be seen in this light.

Perkin's view calibrates nicely both with the view of the scientific naturalists as a new priesthood, and with the Weberian view of professions as status groups that recruited talent while simultaneously excluding outsiders. To become a scientist was to become a member of a new status group. This enriches our perspective on Huxley's push for the cultural leadership of elite research scientists. It also explains his support for standardized education (because it enabled the recruitment of new scientists), Spencer's opposition to it (because it entailed a form of government interference, which meant a move away from self-help and an interference with personal freedom and voluntarism), and their subsequent split.

Spencer's reputation declined because of the emergence of this professional ideal. His populist appeal can be set against experts; his claim to universality placed against growing fragmentation; his views about spontaneous order posed against increasing fears of anarchy; his evolutionary ethics refuted by what was to become formally known in 1903 as G.E. Moore's "naturalistic fallacy." Even the very media in which Spencer expressed his thoughts can be seen as conspiring against him as it shattered into tiny specialized fragments. While it was possible to apply an evolutionary formula to different disciplines in a *System of Synthetic Philosophy* presented in lengthy volumes over a forty year period, this would not be possible in a three-page missive to *Nature*.

Specialization helped to overcome Spencer's *System*. Francis Galton, for instance, noted how specialists thought Spencer's writings were wonderful in all other branches of knowledge except their own. Indeed the best symbol of Spencer's decline

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[820]Perkin, *The Rise of Professional Society*, pp. xiii-xiv.

occurred one year after his 1903 death when some of his admirers sought to commemorate his life by placing a tablet in Westminster Abbey. Hannah Gay has pointed out the strange failure of any of Spencer's admirers to articulate exactly *why* he deserved such a memorial, and points to professionalization in the sciences as one reason for this failure.[821] Their admiration for his work might be likened to people lost in a trance – for, like most people awakening from a dream, they too failed to remember why his work was special. Spencer's followers had become inhabitants of a new, fragmented, palaetiological world.

[821]Francis Galton, "Reminiscences of Spencer," SP, ULL, MS.791/355/8 (mislabelled as 10.i.x); H. Gay, "No `Heathen's Corner' here: the Failed Campaign to Memorialize Herbert Spencer in Westminster Abbey," *British Journal for the History of Science* 31, 41-54 (1998): pp. 41-42, 53-54.

ABBREVIATIONS

MANUSCRIPT SOURCES

HP, IC	Thomas Henry Huxley Papers, Imperial College, London
HMS, IC	Thomas Henry Huxley Manuscripts, Imperial College, London
OCORR, NHM	Richard Owen Correspondence, Natural History Museum, London
OCOLL, NHM	Richard Owen Collection, Natural History Museum, London
OPAP, RCS	Richard Owen Papers, Royal College of Surgeons, London
SP, ULL	Herbert Spencer Papers, University of London Library

PRINTED SOURCES

ABHS	H. Spencer, *An Autobiography*, 2 vols. (London: Williams and Norgate, 1904)
BAAS	British Association for the Advancement of Science *Reports*
BFMR	*British and Foreign Medical Review*
Cyclopaedia	*The Cyclopaedia of Anatomy and Physiology*, ed. R. B. Todd (London: Sherwood, Gilbert and Piper, 1836-1859)
DNB	*Dictionary of National Biography,* ed. L. Stephen and S. Lee (London: Smith, Elder and Co, 1885-1912, 1993).
L	*Lancet*
LLHS	D. Duncan and H. Spencer, *Life and Letters of Herbert Spencer* (London: Methuen, 1908)
LMG	*London Medical Gazette*
M-CR	*Medico-Chirurgical Review*
Phil. Trans.	*Philosophical Transactions of the Royal Society*

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