The Body through the Lens: Anatomy and Medical Microscopy during the Enlightenment

Jed Rivera Foland
Wolfson College

Thesis submitted in fulfillment of the requirements for the degree of Doctor of Philosophy at the Faculty of History at the University of Oxford

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Short Abstract

This thesis examines the role of microscope technology in informing medical and anatomical knowledge during the Enlightenment. Past historians have claimed that microscopy generally stagnated until the popularisation of achromatic microscopes and cell theory in the middle of the nineteenth century. As evidence for this decline, historians have pointed to the poor quality and slow development of microscope designs until the popularisation of achromatic microscopes in the 1820s. In contrast, this thesis highlights the role of specific Enlightenment-era microscopes in answering medical and anatomical questions. It suggests that medical microscopy was far more advanced than previous scholarship has ascertained. Thus far, instrument historians have focused more attention on competing instrument *makers* as opposed to rival instrument *users*. This thesis presents several case studies which explore both makers and users. These concern the histories of Enlightenment-era epidemiology, reproduction theory, anatomy, and physiology as well as the different types of microscopes which influenced these fields.

In terms of methodology, this thesis neither follows nor casts doubt on any particular theory of historical development; rather, it attempts to shed further light on available primary sources and their contexts. Presenting key case studies illustrates the difficulties that early microscope users faced in acquiring and publishing new observations. To explore the practice of early microscopy further, this thesis presents re-enactments of these case studies using Enlightenment-era microscopes and modern tissue samples. Thus, this thesis is a call to broaden the scope of primary sources available to historians of science and medicine to include instruments and re-enactments.

This thesis finds that technological advances did not correlate to microscopical discovery in medicine or anatomy. Both simple and complex microscope designs aided anatomical and medical research. Broader advances in anatomy, physiology, and medical etiology dictated the utility of medical microscopy. Although various groups, such as the French clinicians, saw little need for microscopy towards the end of the eighteenth century, microscope-based evidence continued to play a diagnostic role among lesser-known practitioners despite its lack of visibility in medical literature.

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Abstract

This thesis investigates medical and anatomical microscopy during the Enlightenment. Historians have already focused much attention on the seventeenth-century use of the microscope: the so-called 'golden age' of microscopy when *virtuosi* such as Robert Hooke and Antoni von Leeuwenhoek first sought to investigate the invisible world. However, few historical studies of microscopy are concerned with the eighteenth century or the early nineteenth century before the development of cell theory and germ theory. Prominent historians of microscopy, notably Edward Ruestow, Gaston Bachelard, and Renato Mazzolini have argued that microscopic advances waned following Hooke's 1665 *Micrographia.* This argument likely stems from Hooke's own observation that improvements in lens technology had stalled. In 1691, Hooke dejectedly stated that 'many attempts have been made by divers ingenious men ... but all without success'. Hooke added that, aside from his contemporary Leeuwenhoek, 'I hear of none that make any use of that instrument, but for diversion and pastime'.

Was microscopy in decline during the eighteenth and early nineteenth centuries? This thesis will show that microscopy was less prominent, but not absent, from medicine and anatomy during this time. Case studies will illustrate the central role that microscopy played in informing various debates in anatomy, physiology, and disease etiology. Although experimenters used their lenses to make new empirical discoveries, they did not do so for the purpose of resolving controversies and uncertainties. Instead, the microscope offered another layer to on-going debates about the body's substance, form, and function. Therefore, this thesis is not a linear account of scientific discoveries. It is a portrait of how conceptual and methodological concerns influenced the use of the microscope and the interpretation of its images.

Some experimenters advertised their technological prowess, pointing to these instruments as a sign of their objectivity and diligence; others viewed microscopy as tangential to the concerns at hand. Moreover, I have found that technological limitations did not hinder microscope-based research. Early experimenters were not aware of the vast improvements in lens design that would characterize nineteenth and twentieth-century research. Although Enlightenment-era microscope makers invented numerous improvements and modifications, microscope users were typically satisfied that simple single-lens microscopes were sufficient for their investigations. Common single-lens designs also allowed for more uniform duplication. Despite the widespread use and availability of microscopes, microscopic evidence became less pertinent by the late eighteenth century. This is most apparent in the work of French physiologists and clinicians. Debates surrounding sanitation or the miasmatic origins of diseases also did not often incorporate microscopy. This is not to say that later

physiologists and disease theorists rejected the microscope as a detrimental influence in the face of new theories; rather, they understood that the microscope presented only optical evidence; it could not illustrate purpose, function, or causation. Despite its limitations, the microscope was not entirely absent from eighteenth and early nineteenth-century science, medicine, and anatomy.

There is some precedent for this line of research: recently, Marc Ratcliff has proposed that microscopy did not regress during the Enlightenment; rather, it became less visible. Ratcliff argues that eighteenth-century microscope users made important advances in natural history. My study of microscope investigations of the body lends weight to Ratcliff's argument. Furthermore, this thesis outlines the technological development of the microscope itself during this time period. My conclusions support Jan Deiman's thesis that chromatic and spherical aberration did not degrade images or limit the utility of the microscopy. To investigate this further, I have performed re-enactment experiments using antique microscopes preserved in Oxford's Museum for the History of Science. The images which I witnessed did not often appear like those in early printed illustrations. These re-enactments reveal the wide interpretation given to early microscope observations and the power of preconceptions in seeing and illustrating microscopic detail.

The introductory chapter of this thesis will outline the status of the microscope at the dawn of the eighteenth century. It reviews the most recent historiography concerning this period of microscopy and its supposed decline. Chapter 2 will investigate advances in microscope technology during the early and mid-eighteenth century as well as the social and economic conditions which affected manufacturing, patronage, and the marketing of the microscope. This chapter will devote special attention to the role of British microscope manufacturers who led the way in producing popular single lens and compound lens designs throughout the century. It examines previously unstudied primary sources which shed light on the complex relationship between opticians and the naturalist-patrons who promoted their work.

Chapter 3 will explore the contemporaneous use of the microscope to study disease etiology and the concept of 'animalcule contagion'. The notion that contagious invisible particles caused disease originated as far back as the sixteenth century, with Girolamo Fracastoro's *De contagione* (1546). In the seventeenth century, Athanasius Kircher (1658) argued that he could use a microscope to discern 'countless little worms' in putrefied tissue and the buboes of plague victims. He surmised that these microscopic worms were the signs of disease if not the cause of disease itself. Nevertheless, Fracastoro and Kircher's findings did not radically change disease treatment. Ventilation and avoidance of foul odours remained the primary precaution against disease whether one held to a miasmatic or animalcule-contagionist theory of disease causality. This has led some historians, such as Catherine Wilson, to argue that Enlightenment-era physicians remained ambivalent about contagion theory at best. The concept of contagious insects and microscopic animalcules engendered 'taxonomic awkwardness' thus keeping 'the theory of animate contagion just outside the realm of the incredible'.

In contrast to Wilson, I contend that contagion theory endured following the introduction of microscope-based medicine in the seventeenth century. In particular, I argue that physicians who used the microscope often remained amenable to the notion of contagious animalcules. Such medical microscope users were part physician, part naturalist; they included diverse experimenters such as Giovanni Bonomo, Edmund King, and Michael Ettmüller. Their texts also illustrated the influence of Baconian empiricism upon the medical field and the struggle to draw evidence from microscope observation. Of course, one must exercise caution when assessing early claims about disease etiology. One must not pigeonhole eighteenth-century physicians into 'miasmatic', 'animalculist', or 'contagionist' camps. Regardless of the many competing disease theories, the microscope remained an important instrument for the study of disease etiology and symptoms throughout the eighteenth century and beyond.

The core chapters of this thesis will include several case studies from medical and anatomical microscopy. Chapter 4 explores the role of the microscope in the investigation of human reproduction. By the mid-eighteenth century, the debate between spermists and ovists still dominated the discussion of reproduction. This thesis investigates how the microscope became central to that debate. Georges-Louis Leclerc, Comte de Buffon used a London John Cuff compound microscope to investigate spermatozoa in 1748. By this time, London had become the centre for microscope manufacturing and trade, and London-made microscopes prompted research across Europe. Buffon pointed to the magnificent clarity of his microscope and its supposed ability to magnify objects over 1000 times. With the microscope in hand, Buffon and others formulated a new theory of spermist generation: that both the male and female sex produced sperm which mingled in the womb to create a new organism. Buffon claimed that he could see this act of conception under his lens. The microscope also aided Buffon in formulating his well-known definition of species: a group which could produce fertile offspring through the commingling of their microscope sperm. I have used several Cuff microscopes to recreate Buffon's observations of spermatozoa and ovarian follicles.

Chapter 5 presents another case study in the history of physiology and its use of the microscope in the latter half of the eighteenth century. Blood corpuscles had been witnessed under the lens as far back as Leeuwenhoek's seventeenth-century observations; however, it was not until the 1770s that microscope user William Hewson confirmed their exact shape: concave disc-like corpuscles. To do so, he used a London-made screw-barrel microscope. Hewson's experiments were an attempt to correct the observations of a Naples physician, Giovanni Della Torre, who had argued that blood corpuscles were spherical. Interestingly, both Della Torre and Hewson used the same type of microscope; however, Hewson used less powerful lenses. Again, I have recreated these experiments using the microscope collections at the Museum for the History of Science which contain microscopes and lenses similar to Hewson and Della Torre's.

Chapter 6 examines the role of the microscope in answering further anatomical and physiological questions during the late eighteenth century, specifically the function and composition of the lymphatic system. Anatomists had long since mapped the lymphatic vessels throughout the body; however, there was no consensus on the purpose of this system, or whether or not the lymphatic vessels were a separate system from the circulatory and digestive systems. A number of microscope users were involved in this debate: Johann Lieberkühn, Albrecht von Haller, William Hunter, Alexander Monro *secundus*, William Hewson, John Sheldon, and William Cruikshank. Much of the controversy centred on the discovery of the lacteals: the micro-vessels in intestinal villi which are responsible for absorbing and transporting digestive nutrients. Their discovery by Monro *secundus* in 1757 prompted a bitter priority dispute with his rival William Hunter, who claimed to have made the discovery some years earlier. The feud prompted continued research by Monro, Hunter, and their followers on the lymphatics. This chapter thus explores how the microscope became central to such social disputes amongst leading anatomists and physiologists. Fortunately, the Hunterian Museum in London has Hunter's own set of microscope slides and tissue samples dating from 1750-70 which he and his students used in their observations of the lymphatic system. I have examined these under the microscope to learn more about the difficulty in studying villi and lacteals.

Chapter 7 will further explore the role of the microscope in the study of disease during the late eighteenth century. In the 1780s, Russian doctor Daniel Samoilowits claimed to have seen animalcules associated with bubonic plague beneath his Dellebarre microscope. Although, he was not certain that these animalcules caused the plague itself, he did speculate that they were the signs of the disease. Thus we see that the microscope remained a useful tool for diagnoses. To better understand the observation of disease animalcules, especially in the case of plague, I have examined *Yersinia* bacteria among other types of bacteria and tissue samples using microscopes in the museum. My results indicate that certain eighteenth-century microscopes could, in fact, capture images of bacteria. Nevertheless, without the framework of germ theory, such observations as those of Samoilowits did not radically alter disease etiology and treatment.

In addition to these case studies, this thesis will explore the supposed decline of the microscope in the late eighteenth century. Maurice Daumas has insisted that microscope-making experienced a 'revival' at least in France during the latter half of the eighteenth century. Indeed, the complexity and magnification of compound microscopes seem to have increased between 1770 and 1800. On the other hand, this same time period is well known for its eminent physicians and anatomists, many of whom, such as Xavier Bichat, deplored the use of the microscope. As one of the founders of clinical medicine in Paris, Bichat's reluctance to use the microscope obviously had long-standing implications. It has persuaded historians to doubt the influence of microscopy on medicine and anatomy at the end of the eighteenth century and into the nineteenth. This thesis will show that microscopical research continued in a less visible fashion among contemporaries of Bichat and his followers such as René Laennec.

Ultimately this thesis raises several questions for future scholars: if medical microscopy could focus on disease agents, such as bacteria, what other social and professional changes were necessary before the development of germ theory? Moreover, if the microscope was an integral part of research, why did physicians and anatomists so infrequently discuss the microscope in their treatises? Often, practitioners noted the fact that they used microscopes but failed to mention which model microscope they used. Finally, and perhaps most importantly, this thesis will make a case for re-enactment experiments. One cannot hope to recreate all the conditions of an early experiment in microscopical medicine and anatomy; nevertheless, attempting to recreate these experiments will shed light on the problems that faced early microscope users. Re-enactment experiments illustrate the method of discovery instead of discovery itself. In doing so, this thesis reveals that microscope methodologies varied greatly based on the type of instrument being used. These re-enactments illustrate the difficulties that microscope users faced when attempting to discover or focus upon particular anatomical details and relate them to the bodily whole. Thus, we find that simple single-lens microscopes, which were easier to use, contributed to key medical and anatomical discoveries and debates despite the availability of more complex models with higher magnification. This refutes the notion that medical and anatomical microscopy appeared because of the development of more advanced technology such as the achromatic microscope. Re-enactment experiments also reveal a wide range of competing microscope techniques. This thesis stresses the difficulty in operating Enlightenment-era microscopes and confirming microscopic evidence. In this way, the microscope itself serves as a primary source.

Acknowledgements

I have been fortunate to have two excellent supervisors, Professors Pietro Corsi and Jim Bennett. I would like to thank them for their exceptional insight, patience, and constant assistance. I am also grateful to Margaret Pelling who provided valuable feedback and superb suggestions during my confirmation of status interview. In addition, I am indebted to Mark Harrison and Stephen Johnston for their thoughtful advice during my transfer of status interview into the doctoral programme.

This thesis would not have been possible without the aid of many curators, archivists, and librarians. In particular, I would like to thank Lucy Blaxland, Collections Manager at the Museum for the History of Science, Oxford. Lucy graciously allowed me to conduct the majority of my experiments using the microscope collections at the Museum. I am also grateful to Steven Ruzin, curator of the U.C. Berkeley Golub Microscope Collection. Not only did Dr. Ruzin aid me in my experiments, but he has dedicated considerable effort towards the preservation and study of the optical instruments in his collection for the benefit of all historians. I am also grateful to the curators at the Museum Boerhaave, the Glasgow Hunterian Museum, and the Hunterian Museum at the Royal College of Surgeons. This work was made possible thanks to staff members at the John Rylands Library in Manchester, Magdalen College Library, Christ Church College library, the Wellcome Library, the British Library, and the Bibliothèque nationale. I am especially grateful to my colleagues at the Bodleian, particularly Valerie Lawrence and Isabel Holowaty, for constantly supporting me as a student and an employee at the History Faculty Library.

I am also indebted to Helen Curtis at the Department of Physiology, Anatomy, and Genetics in Oxford for the use of her laboratory microscopes. Judith Anderson, a bacteriologist and postgraduate at the Wellcome Unit for the History of Medicine, supplied me with outstanding slide samples and stains for my microscope experiments, as did the Department of Molecular and Cell Biology at U.C. Berkeley.

I would also like to thank my early undergraduate mentor, Sean M. Quinlan, whose lectures on the history of medicine and biology were so inspiring that they compelled me on the dangerous path towards postgraduate education. In addition, John Servos of Amherst College read early drafts of my research on Buffon and offered many insights into the importance of the broader context in the history of science and medicine. My fellow postgraduate and historian Fiona E. Whelan has invested far too much time in editing and commenting upon my research. I am truly grateful for her years of help.

Finally, I must dedicate this work to the late Roger Hahn, whose kind advice and intelligence bolstered my love of the history of science and medicine.

Abbreviations

Add MSS	Additional Manuscripts
BL	British Library
BNF	Bibliothèque Nationale de France
CNAM	Conservatoire National des Arts et Métiers
GMC	Golub Microscope Collection, University of California, Berkeley
JRL	John Rylands Library, Manchester
MHS	Museum for the History of Science, Oxford
RCSHC	Royal College of Surgeons Hunterian Collection

All translations are my own unless otherwise indicated.

If they are known, the dates of birth and death for each historical figure have been recorded after the first use of their name in the text. Whenever possible, these have been taken from the *Dictionary of National Biography* or the *Biographie universelle, ancienne et moderne.*

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Chapter 1. Introduction

This thesis is about perceptions. Did Enlightenment-era physicians and anatomists perceive the microscope in favourable or unfavourable terms? When observers did peer through their lenses, how did they observe the body and its functions? Finally, did microscope observations inform medical theories and practices? In addressing such questions, I will delve into the history of science, medicine and technology. Although, these disciplines of historical study seem to be diverging today, the boundaries between 'science' and medical 'physik' are interwoven in the context of Enlightenment microscopy. Furthermore, I do not intend to re-examine primary sources in light of modern or post-modern theories and jargon; rather, I will attempt to convey the history of microscopy in its own context. In doing so, I do not intend to construct a survey of microscope technology or an archaeology of antique microscopes; rather, this is an account of microscope use, microscope users, and the problems and philosophies that influenced microscope observations of the human body.

The role of the microscope in Enlightenment-era medicine and anatomy is debatable. Early historians of the microscope frequently ignored eighteenth-century contributions to microscopic research. In contrast, they dedicated much attention to the late seventeenth-century observations of Marcello Malpighi (1628-1694), Antoni van Leeuwenhoek (1632-1723), Jan Swammerdam (1637-1680), and Robert Hooke (1635-1703): during the so-called 'golden age of microscopy'.[1] Early historians also dedicated considerable attention to the invention of 'modern' microscopes in the nineteenth century, especially the achromatic microscopes of Joseph J. Lister (1786-1869) in the 1820s and the high-powered oil-immersed lenses of Giovanni Battista Amici (1786-1863) in the 1840s. Most historians have assumed that the development

[1] L.L. Woodruff, 'Microscopy before the Nineteenth Century', *American Naturalist,* 77, no. 749 (1939), p.499.

of cell theory and germ theory followed from these technological advances.[2] Such studies are not without merit, but they have overlooked the history of the microscope during the eighteenth century. This study will fill the gap in the history of medical and anatomical microscopy. I will answer the question: what was the influence of the microscope between the 'golden age' and the 'modern'?

Concerning microscope designs, historians initially posited a slow, but linear progression from the simple lenses of Leeuwenhoek to the complex achromatic scopes of over a century later.[3] In addition, both traditional and modern historians have argued that the microscope had a negligible impact upon medicine and anatomy owing to technological shortcomings.[4] I refute both claims. Microscope models did not necessarily become more intricate and effective. Simple lenses often exceeded the quality of complex compound models. As we shall see in subsequent chapters, early eighteenth-century anatomists and physicians often preferred simple single-lens microscopes to later models. Indeed, lens-makers were unable to resolve the problems of chromatic and spherical aberration; however, it would be anachronistic to assume that early microscope users were aware that such problems could be solved. Among academic circles, some scholars doubted that the magnification of lenses could be further increased or that lens-makers could overcome the problems of aberration; the most famous of these sceptics being Isaac Newton (1642–1727). These limitations aside, microscopy benefited from a radical transformation in manufacturing culture and new systems of commerce during the eighteenth century. As we shall see in the following chapter, new innovations rose amidst the burgeoning of manufacturing

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[2] W.T. Sedgwick, 'The Origin, Scope and Significance of Bacteriology', *Science*, 13, no. 317 (1901), pp.121-2.

[3] See for example, M.E. Barrow, ‘The Development of the Microscope’, *Bios*, 5, no. 4 (1934), pp.157-64; G.E. Blackham, ‘The Evolution of the Modern Microscope’, *Proceedings of the American Society of Microscopists*, 4 (1882), pp.25-47.

[4] J.W. Cousins, 'President's Address Delivered At The Sixty-Seventh Annual Meeting Of The British Medical Association. A Sketch Of The Century's Progress In Medicine And Surgery', *British Medical Journal*, 2, no. 214 (1899), p.326. S.J. Reiser, *Medicine and the Reign of Technology* (Cambridge, 1981), pp.73-5.

networks and the expansion of patronage and trade. In short, pointing to new microscope models will provide merely a superficial glance into the complicated practice of eighteenth-century microscopy. We must also delve into the social and intellectual aspects of eighteenth-century use, patronage, and manufacturing. Of course, a full analysis of these areas would require a separate thesis; this thesis will address these developments in the context of medicine and anatomy.

Perhaps the negative view of eighteenth-century microscopy stems from the rapid advances of the discipline during the nineteenth century. Later histologists disparaged the technology of the eighteenth century. John Quekett (1815-61) wrote of 'rapid advances which have been made in modern times towards a correct knowledge of the intimate structure of animate and inanimate beings'.[5] He grounded this opinion on the belief that 'the discoveries at this time were few and comparatively unimportant, and little or nothing more was exhibited by them'.[6] Similarly, the naturalist Cutherbert Collingwood (1826-1908) perceived the eighteenth century as a fallow field.

> The imperfect apparatus in the hands of Leuwenhoeck [sic] and Swammerdam afforded revelations which astonished the scientific world two centuries ago ... It is only, however, within the last ten years, that that perfection of definition and illumination have been attained, which render the microscope the most powerful auxiliary in anatomic analysis.[7]

Let us abandon these assumptions and instead discern Enlightenment-era microscopy in its own terms. Which microscope models influenced anatomy and medicine during this period? Enlightenment physicians and anatomists benefited from a vast network of microscope manufacturing and trade. Although historians have tended to view this period (particularly the eighteenth century) as encompassing the decline of

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[5] J.T. Quekett, *A Practical Treatise on the Use of the Microscope*, 2nd edn. (London, 1852), p.viii.
[6] Ibid., p.25.
[7] C. Collingwood, *The Influence of the Microscope Upon the Advance of Medicine An Address Delivered at the Opening of the Session 1859-60 at the Liverpool Royal Infirmary School of Medicine* (Liverpool, 1859), p.12.

microscopic research, as we shall see, the number of instruments and models increased as the century progressed. Unfortunately, many medical practitioners failed to record the specific type of microscopes in their research; however, primary sources do reveal a few specific models that were at the forefront of anatomical and physiological debates. These include the screw-barrel or 'pocket' microscope attributed to James Wilson (c.1665-1730), the John Cuff (1708-1772) 'universal double microscope' and its improvements by George Adams Sr. (1750-1795), and Benjamin Martin (c.1705-1784). Also, the compound microscope of Louis Françoise Dellebarre (c.1726–1805) supposedly limited chromatic aberration over forty years before Lister's invention. Anatomists and physicians used all of these designs well into the nineteenth century. This is not to say that a revolution in microscope technology induced new paradigms in medicine and anatomy. If historians have questioned that serious microscope research occurred during this century, they cannot deny that certain microscope models had become more complex in design and far more widespread in use. When eighteenth-century physicians and anatomists employed microscopes, they relied on a wide range of models: some being avant-garde and others widely produced copies of time-honoured designs. More important, for our purposes, is that when physicians and anatomists did use the microscope, they were often confident that the images presented through their lenses were true to reality.

How did these new microscope models arise? Historians such as Reginald Clay, Thomas Court, Gerard L'E Turner, and John R. Millburn have shown that the competitive market for instruments led to a number of new designs and modifications especially in London during the latter half of the eighteenth century.[8] To further

[8] R. Clay and T. Court, *The History of the Microscope Compiled from Original Instruments and Documents, up to the Introduction of the Achromatic Microscope* (London, 1932); G. L'E Turner, *The Great Age of the Microscope: the Collection of the Royal Microscopical Society through 150 Years*

address this question one must take into account not only developments in the instrument craft, manufacturing, and consumerism but also advances in optics, medicine, and natural philosophy. Of course, a broad analysis of such immense topics is far too demanding for this thesis on anatomical and medical microscopy. Moreover, a single summary of the rise of instrument manufacturing will ultimately be unsatisfying: the technical and social variables that influenced microscope design during the Enlightenment are innumerable and difficult to discern. Nevertheless, recent scholars have made progress in this field. Marc J. Ratcliff and Jutta Schickore have dedicated considerable parts of their studies to the state of microscope manufacturing and trade, mostly as it relates to the late eighteenth century and beyond.[9] Catherine Wilson's book, *The Invisible World*, also delves into the realm of manufacturing as it concerned the late seventeenth century. Alison Morrison-Low's *Making Scientific Instruments in the Industrial Revolution* considers microscope manufacturing among broader social aspects of the instrument craft.[10]

Unfortunately, historians have seldom focused on microscope use and manufacturing during the critical decades of the early eighteenth century. There is a surprising lacuna in scholarship regarding the role of instruments in science in the early eighteenth century. The period from 1700-1740 is particularly important as it witnessed a number of new instruments and changing practices. Wilson's study ends in the 1720s during the so-called 'decline' of microscopy (coinciding with the death of Leeuwenhoek in 1723). Conversely, Schickore and Morrison-Law have researched the latter half of the eighteenth century and the early nineteenth century. Most of

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(Bristol, 1989). J.R. Millburn, *Benjamin Martin: Author, Instrument-maker, and 'Country Showman'* (Leyden, 1976), and *Adams of Fleet Street: Instrument Makers to King George III* (Aldershot, 2000).
[9] M. Ratcliff, *The Quest for the Invisible: Microscopy in the Enlightenment* (Aldershot, 2009); J. Schickore, *The Microscope and the Eye: A History of Reflections: 1740-1870* (Chicago, 2007).
[10] C. Wilson, *The Invisible World: Early Modern Philosophy and the Invention of the Microscope* (Princeton, 1995); A. Morrison-Low, *Making Scientific Instruments in the Industrial Revolution* (Aldershot, 2007).

Ratcliff's research also concerns developments later in the eighteenth century. Thus, there is some difficulty in tracing the evolution of microscopy across the entirety of this century. Nevertheless, these recent histories have broken ground in several major areas: they have shown that serious microscopy did not stagnate during the eighteenth century and that new microscope models did in fact alter the course of scientific research. Ratcliff has drawn attention to several major advances occurring in natural history, particularly the study of the polyp and other aquatic animalcules. Schickore has similarly written about debates concerning the usefulness of the microscope and its contribution to the study of nerve fibres after 1740.

This thesis thus encourages an emerging trend in recent historiography. Marc Ratcliff writes, 'For 150 years, historians have studied Leeuwenhoek and *alia,* and for 150 years eighteenth-century microscopy has been neglected'.[11] As Ruestow put it, the early pioneers in microscopy lacked 'immediate heirs of significance' and 'the eighteenth-century failed to develop the study of microscopic life into a sustained and integrated field of research'.[12] Ratcliff and more recent scholars dispute this claim; unfortunately, this is no easy task. Ratcliff states, 'it is not so easy to break down so rooted a prejudice'.[13] Ratcliff, Wilson, Morrison-Low, and Schickore have provided critical reconsiderations to the historiography of Enlightenment science. They have cast new light on the prevalence of advanced microscopic research during the eighteenth century. However, I would argue that the impact of their work is more revolutionary than these authors have anticipated. Historians must now re-evaluate all aspects of eighteenth-century microscopic science. Modern scholars have typically focused more on fields such as microscopic naturalism and the frequently deliberated spontaneous generation debates, rather than the medical facets of eighteenth-century

[11] M. Ratcliff, 'Europe and The Microscope In the Enlightenment' (University College London, Ph.D. thesis, 2001), p.44.
[12] See also, E.G. Ruestow, *The Microscope in the Dutch Republic*, pp.276-7.
[13] M. Ratcliff, 'Europe and The Microscope In the Enlightenment', p.45.

microscopy. It is my contention that there needs to be increased attention drawn to the importance of microscopy in the field of medical theory. Furthermore, scholars of microscopy have neglected a number of Enlightenment texts. Much of our understanding of the history of medical microscopy is based upon major treatises and leading journals. In addition to these, I will call attention to the lesser-studied medical dictionaries, pamphlets, and popular textbooks.

Chapters 2 and 3 will highlight significant influences upon medical and anatomical microscopy during the early and mid eighteenth century, including early obstacles to microscopic research, new modes of marketing, new aspects of patronage, and, of course, the invention of new designs. Ultimately, these developments occasioned lasting changes to the construction and use of the microscope among physicians and anatomists during the eighteenth century. Chapters 4, 5, 6, and 7 will present case study episodes from the history of medical and anatomical microscopy. These more narrow case studies will help us to understand the methods, challenges, and doubts that microscope users faced when interpreting the body through the lens.

The rush of discovery and discussion that characterized late seventeenth-century microscopy took place in a radically altered social sphere as funded societies of science began networking with solitary practitioners. There is a wealth of recent historiography on this subject, and my aim is to draw attention to subsequent uses of the microscope further ahead. However, it is vital to note that late seventeenth-century microscopy was no longer the preserve of private *virtuosi*. State institutes such as the Académie des Sciences in Paris, and the Royal Society in London had embraced instruments and provided a context for spreading microscope discovery through their publications. Under the influence of Hooke and corresponding members such as Leeuwenhoek, the early Royal Society highlighted improvements in instrument design; in fact, the first article published in the *Philosophical Transactions* was, 'An Account of the Improvement of Optick Glasses' in 1665.[14] In the second issue of the journal, Henry Oldenburg included advertisements and reviews of Hooke's *Micrographia,* which were published that same year by the society.[15] Evidently, microscopy was off to a bold start under the aegis of these new societies.

It did not last. Prominent historians of microscopy, notably Marian Fournier, Edward Ruestow, and Renato Mazzolini have argued that microscope advances diminished following the *Micrographia.*[16] However, it would be more factual to claim that microscopy simply became less visible and well publicized. As Lorraine Daston explains:

> The peculiar economy of attention cultivated by the Enlightenment naturalists was pointillist, magnifying, and therefore deliberately repetitive ... The use of microscopes and, especially the more portable and versatile magnifying glass also tended literally to focus and circumscribe the observers attention.[17]

How then do we exlain this supposed decline of microscopy if the microscope itself embodied the Enlightenment ideal of close observation? The presumption probably arises from Hooke's personal observation that advancements in concave lens technology had come to a standstill. In 1697, Hooke dejectedly stated that 'many attempts have been made by divers ingenious men ... to make object-glasses and eye-

[14] Royal Society of London, 'An Account of the Improvement of Optick Glasses*', Philosophical Transactions*, 1 (1665-6), pp.2-3. Multiple reports followed concerning the grinding of lenses for both microscope and telescope users. See for example: Smethwick, F, 'An Account of the Invention of Grinding Optick and Burning-Glasses, of a Figure Not-Spherical...' *Philosophical Transactions*, 3 (1668), pp.631-2.

[15] Royal Society of London, 'An Account of Micrographia, or the Physiological Descriptions of Minute Bodies, Made by Magnifying Glasses', *Philosophical Transactions*, 1 (1665-6), pp.27-32.

[16] M. Fournier, *The Fabric of Life: Microscopy in the Seventeenth Century* (Baltimore, 1996); also, Fournier, 'Huygen's Microscopical Researches', *Janus,* 68 (1981), pp.199-209; E. Ruestow, *The Microscope in the Dutch Republic: The Shaping of Discovery* (Cambridge, 1996); R. Mazzolini, 'L'illusione incomunicabile. Il declino della microscopia tra sei e settecento 1670-1710', in H.K. Schmutz (ed.), *Phantastische Lebensräume, Phantome und Phantasmen* (Marburg, 1997), pp. 197-219. See also, G. Bachelard, *La formation de l'esprit scientifique* (Paris, 1972).

[17] L. Daston, 'Empire of Observation', in L. Daston and E. Lunbeck (eds.), *Histories of Scientific Observation* (Chicago, 2011), pp.99-100.

glasses of elliptical figures, but all without success'.[18] Although he extolled the work of Leeuwenhoek (who used a secret method for producing bead-like spherical lenses) Hooke was disconsolate that so few of his colleagues had chosen to follow his and Leeuwenhoek's vocation. Now nearing the end of his life, Hooke reflected that microscope users had only scratched the surface in answering a fraction of the questions that remained in natural philosophy.

> Much the same has been the fate of the microscope, as to their invention, improvements, use, neglect and sighting, which are now reduced almost to a single votary, Mr. Leeuwenhoek; besides whom, I hear of none that make any use of that instrument, but for diversion, and pastime.[19]

This theory of decline is not limited to British microscopy, but can be expanded to the continent. Jacques Roger found that the Paris Académie Royale des Sciences became reluctant to sponsor enterprises into microscopy by the end of the seventeenth century; furthermore, the society became indifferent to Leeuwenhoek's pioneering research and subsequent microscopic studies.[20] So we must ask: what precipitated this decline if indeed there was one?

The revolutions in microscopy engendered a philosophical impasse: in one respect microscopy offered tantalizing empirical evidence, yet it relied on fallible senses. The followers of Bacon were divided on this issue. Although Boyle and Hooke welcomed the new instruments as triumphs of empiricism, their contemporaries, such as John Locke and John Ray, argued that the microscope could deceive the senses, rendering Cartesian deductivism as the last resort for the user. Moreover, Locke and other physician-empiricists deemed microscopes as unreliable machines which warped the true essence of particles. Locke wrote,

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[18] R.Hooke, 'Discourse Concerning Telescopes and Microscopes; With a Short Account of Their Inventors, Read in February 1691–2', *Philosophical Experiments of the Late Eminent Dr. Robert Hooke and Other Eminent Virtuoso's in His Time* (London, 1726), p.261.

[19] Ibid.

[20] J. Roger, *Les sciences de la vie dans la pensée Française du XVIIIè siècle* (Paris, 1993), p.183.

> [W]ere our senses altered, and made much quicker and acuter, the appearance and outward scheme of things would have quite another face to us; and, I am apt to think, would be inconsistent with our being, or at least well-being.[21]

The harshest rebukes were pointed at medically-minded microscope users who searched for the animalcules of contagion. More publications seem to have been refutations of this idea than evidence for it. One petulant critic, Gideon Harvey, excoriated his fellow anatomists in 1686, chiding them for having supplanted the study of diseases with errant theories about corpuscles and animal contagion. They 'gaze on little particles through a microscope, and whatever false appearances are glanced into their eyes, these to obtrude to the World in Print'.[22]

Historian Ruestow, in his investigation of seventeenth-century microscopy, argues that society members actually disregarded microscopic research for more pragmatic reasons. The Royal Society, for example, could have endowed further time and money to improve upon Hooke micrographic research or Nehemiah Grew's (1641-1712) pioneering work in microscopic botany; however, the advantage of this was difficult to ascertain, and, as Ruestow claims, funding for their projects slowly dwindled.

> Like the growing number of new scientific societies that sprang up elsewhere ... the Royal Society also lacked the commitment and training needed to sustain a research tradition.[23]

More recent historians have theorized that the decline of microscopy was due to technical inadequacies. The compound microscope could not possibly have answered the questions it instigated. As Catherine Wilson writes:

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[21] J. Locke, *An Essay Concerning Human Understanding*, 30th ed. (London, 1849), p.193. Locke further noted the impracticality of the microscope in solving practical problems: '[I]f by the help of such microscopical eyes (if I may so call them,) a man could penetrate farther than ordinary into the secret composition and radical texture of bodies, he would not make any great advantage by the change ... such an acute sight would not serve to conduct him to the market of exchange'. Ibid.
[22] G. Harvey, *The Conclave of Physicians, Part I* (London, 1686), p.25.
[23] E. Ruestow, 291.

> What is certainly true is that, shortly after its introduction, the microscope became a vehicle of hopes which it could not conceivably fulfil and that therein lay some of the potential for disappointment which Hooke was later to record.[24]

None of the simple and compound microscopes that I have used in my re-enactment experiments were able to focus on a specimen with the clarity that Hooke had illustrated in his 1665 *Micrographia.* Hooke's beautiful illustrations emphasized the detail of microscopic life and the uniformity found in certain natural patterns such as mineral crystals or cork cells. Whether or not Hooke actually saw the details that he claimed to have seen will never be known, but my research suggests that his wondrous images were unlikely to be duplicated. The *Micrographia* may have set a standard that was far too difficult to surpass without taking liberties in one's illustrations. Moreover, as we will see in the following chapter, major philosophical doubts plagued microscopy by the beginning of the eighteenth century: one simply could not see the uniformity in nature that Hooke had endeavoured to illustrate.

What impact did this have on medical and anatomical microscopy? My research suggests that microscopy might have generally waned in the midst of the Enlightenment, but this did not preclude the study of anatomical microscopy or even the study of microscopic causes of disease. As we will see, eighteenth-century microscope research still informed medicine and anatomy albeit with questionable success.

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[24] C. Wilson, 'Visual Surface and Visual Symbol: The Microscope and the Occult in Early Modern Science', *Journal of the History of Ideas,* 49, no. 1 (1988), p.86.

Chapter 2. Microscope Manufacturing and Patronage, 1700-1750

While Hooke lamented in 1691 that few of his contemporary naturalists were diligently pursuing microscopical investigations, the same could hardly be said for the generations of microscope users who followed him. Ratcliff contends that the 'thesis of the "absence of microscopy" virtually explodes on contact with supplementary sources'.[1] According to Ratcliff, the microscope became a primary tool of natural history and classification during the eighteenth century. Naturalists across Western Europe continued to publish texts relating to microscopy, although not always with the same alacrity as Leeuwenhoek and Hooke. In particular, eighteenth-century microscope users fostered a growing trend in the study of infusoria, worms, and rotifers. Schickore has also highlighted early anatomical investigations into microscopic nerve fibres, especially the works of Alexander Monro *secundus* (1733-1817) and Felice Fontana (1730-1805).[2] However, both Ratcliff and Schickore rely heavily on case studies from the middle to the late eighteenth century. This begs the question: although microscopy continued, did the rigor of microscopic research decline in the first half of the eighteenth century?

Although naturalists did not overlook the usefulness of the microscope in the early eighteeth century, the output of texts relating to microscopy in this period hardly measures up to the prolific work during the second half of the seventeenth century when Leeuwenhoek, Hooke, and Malpighi were most productive. However, the theory that early eighteenth-century naturalists and anatomists ignored microscopy remains debatable. For the purposes of this chapter, it is more helpful to question whether or not the microscope failed to evolve as an instrument. Did new designs emerge? If not, what limits to further development remained?

[1] M. Ratcliff, *The Quest for the Invisible*, p.3.
[2] J. Schickore, *The Microscope and the Eye*, ch.2.

On closer investigation we find that early and mid-eighteenth-century opticians diligently advanced the craft. New microscope designs continued to spring from artisan workshops. Several early models would eventually come to dominate the market. Opticians such as James Wilson, Edmund Culpepper, and John Cuff produced single and compound microscopes that other craftsmen widely copied. However, new challenges also arose. We cannot easily ignore the widespread doubts that natural philosophers entertained as they considered the potential for improving optical equipment. These concerns were both philosophical and practical. Late seventeenth-century microscopy could not free itself from epistemological concerns. The usefulness of the microscope in uncovering truths about the mechanical philosophy remained uncertain. As a means of discovering fundamental components of nature (such as corpuscles or perfect geometric forms) microscopy failed entirely. Furthermore, Newton's optical theories suggested that microscope lens shape was incapable of overcoming the aberrations associated with the deflection of light. As craftsmen began manufacturing more intricate designs (such as the triple or quadruple lens microscopes) the problems long associated with the simple microscope became magnified. Let us explore these issues in greater detail.

2.1. Early obstacles to microscopic research and microscope manufacturing

i. Philosophical doubts

Bolstering the theory that interest in microscopy had waned by the beginning of the eighteenth century, three recent studies have explored philosophical doubts impeding microscopy. Wilson, Ruestow, and Fournier have analysed the history of microscopy using different sources, but they have reached complementary conclusions that microscopy ultimately waned because it could not impart empirical evidence to sustain either the mechanical philosophy or the search for divine perfection. Seventeenth-century observers became incredulous, lamenting that the microscope could no longer serve as a valid tool for investigating nature. In short, microscope observations were easily refutable; therefore, early eighteenth-century microscope users could not participate in the inductive study of nature. They had arrived where they began half a century before: 'forced again to extract meaning from the optically indeterminate.'[3]

Not surprisingly, Ruestow's influential study of seventeenth-century Dutch microscopy is tied to the fate of Leeuwenhoek.[4] As an example of decline, Leeuwenhoek's early faith in the corpuscular hypothesis was just one of many suppositions that later proved untenable, or at least impossible to demonstrate. Similarly, Swammerdam's view of the microcosmos as divinely ordered proved unconvincing to contemporaries and eighteenth-century followers.[5] Social and cultural changes compounded these doubts. Meanwhile, hard-line Dutch Calvinists rejected the possibility of understanding the intricacy of God's complexity, thus wholly opposing the Cartesian search for experiential truths and fundamental particles of matter. Concerning anatomical uses of the microscope, Dutch physicians and anatomists had become less inclined to map particular microscopic components than they were in holistic discoveries. On the medical front, anatomical schools turned away from microscopic research as they embraced efforts to map connective bodily systems using venous wax injections and modelling.

There are nearly identically problems in Fournier's study of late seventeenth-century microscopic decline. Fournier emphasises epistemological debates concerning the demerits of the microscope. She does so largely in response to past claims that the decline of eighteenth-century microscopy arose from the technological limitations of the instruments themselves. Rather, her study of early microscope physiology posits that the mechanical philosophy, which had once prompted exhilarating and diverse observations, was no longer viable. The more Leeuwenhoek, Hooke, Malpighi, Grew and others searched

[3] C. Wilson, *The Invisible World*, p.256.
[4] E. Ruestow, chs.6 and 7.
[5] Ibid., ch.5.

for mechanical features of the microcosm or corpuscular foundations of life, the less they found.[6] Discontented, the next generation of microscope users turned to other means of studying physiology and the book of nature. In the case of anatomy, Fournier bolsters Ruestow's view that late seventeenth-century anatomy had rid itself of microscopy in favour of macroscopic investigation. In particular, hydrological studies of the body as a fluid-filled mechanism obviated the need for microscopic research. In short, the instruments were not to blame; rather, 'the conceptual foundations of physiology toward the end of the seventeenth century had no immediate need of the instrument.'[7]

Wilson's book, *The Invisible World,* provides an exceptionally detailed look at the reaction of seventeenth-century philosophers to microscopic observations. Like her colleagues, Wilson equates the decline in microscopy to the failure of natural philosophers to fulfil their quest to witness an order to nature: an underlying geometric property of matter or a universal component. At one time, seventeenth-century progress in microscopy contributed to the larger endeavour to understand the deity as 'a creator of a system that needs and can sustain no intervention after creation'.[8] The demand for ever increasing magnification and clarity was not only beneficial, it was fundamental, for 'the microcosm was as much a part of this expanding system as the macrocosmos.'[9] Leibniz, for example, had endorsed such a worldview, stating in 1702,

> Nothing better corroborates the incomparable wisdom of God than the structure of the works of nature, particularly the structure which appears when we study them closely with a microscope.[10]

[6] By the 1670s Spinoza and Christiaan Huygens also came to reject a wholly-mechanistic and mathematical world-view as a basis of natural history and natural philosophy. Although Huygens and Spinoza had an antagonistic relationship, both would eventually cast doubt on the relevance of the microscope and the merits of Descartes' deductivism. See Jonathan Israel's chapter, 'Spinoza, Science and the Scientists', in J. Israel, *Radical Enlightenment: Philosophy and the Making of Modernity 1650-1750* (Oxford, 2001), pp.242-57.
[7] M. Fournier, *The Fabric of Life,* p.194.
[8] C. Wilson, *The Invisible World,* p.181
[9] Ibid.
[10] G.W. Leibniz, 'Reflections on the Common Concept of Justice', in *Leibniz: Philosophical Papers and Letters,* ed. L. Loemker (Synthese Historical Library Ser., ii, 1969), p.566.

Similar statements abound among microscope users, from Swammerdam's observation of the elegant perfection of a butterfly's wings,[11] to Nehemiah Grew's view that God had set pores upon the human skin in wonderous patterns.[12] One cannot easily assume that the quest for God's handiwork through the instrument of the microscope suddenly terminated at the beginning of the eighteenth century.

The applause truly began to die once it became apparent that the microscope had failed to capture the 'alphabet of nature'.[13] It revealed few mechanical functions or physiological processes. In the words of Wilson, 'Microscopy was no replacement for the scholastic vegetative, motive, and nutritive faculties; it could show muscle fibres and vessels for sap, but it did not really show how much of anything could happen.'[14] Imperfect lenses then were but the final nail in the coffin. The 'fading significance at the microlevel is confirmed by the draining out of the quality of images.'[15]

Unlike fellow historians such as Fournier, Wilson argues that late seventeenth-century practitioners had not wholly abandoned their initial enthusiasm for microscopic anatomy. Rather, micro-anatomy had completed its practical function. 'Microscopy stalled because – the initial mapping of the major plant and animal organs and tissues as seen through the microscope completed – there seemed, as Hooke said, nothing more to be done.'[16] With such unsettling explanations in mind, Wilson ends her study at the dawn of the eighteenth century.

It now remains to determine the state of microscopy in the following decades. Past historians have been relatively mute on this subject with the exception of Ratcliff and Fournier. Not content to presume that these early doubts continued to plague microscopy,

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[11] J. Swammerdam, *The Book of Nature, Part II,* translated by T. Flloyd (London, 1758), p.19; also, C. Wilson, *The Invisible World,* p.185.
[12] N. Grew, 'The Description and Use of the Pores in the Skin of the Hands and Feet', *Philosophical Transactions of the Royal Society of London,* 14 (1684), pp.566-7.
[13] A term coined by Franciscus Mercurius van Helmont. F.M. van Helmont, *Alphabeti vere naturalis* (Sulsback, 1667).
[14] C. Wilson, *The Invisible World,* p.230.
[15] Ibid., p.231.
[16] Ibid., p.252.

Ratcliff has argued that 'the passage from the seventeenth to the eighteenth-century style of microscopical research is an example of the 'restructuring' and not the 'regression' of a whole research field.[17] Microscopic researchers began moving towards shared visions as they continued to search for ever increasing resolution and magnification. They stressed the importance of shared observation and samples. These required simplified experiments, not detailed investigations of ever-smaller particles of matter.

In his article 'The Birth of the Modern Scientific Instrument' Albert Van Helden declared that the legacy of late seventeenth-century science was to set in motion a constant effort to improve instruments over the next century. Truly 'scientific' instruments only developed after they 'had obtained an unquestioned role in science ... and when continuous improvement had become the quest of the community of scientists and instrument makers'.[18] In Van Helden’s view, this trend for constant innovation and improvement arrived in Europe towards the end of the seventeenth century, but this was only the beginning of a difficult path ahead. Clearly historians of microscopy such as Wilson and Fournier have taken issue with such claims. It may be the case that the microscope, as an instrument of observation, had reached the pinnacle of perfection. New improvements had developed rapidly in the seventeenth century as users such as Hooke and Grew scrambled to make sense of the images they observed. The lack of continued enhancements to the design suggests that early eighteenth-century microscopes cannot be classified as 'instruments of science' in the sense that Van Helden uses the term.

On the other hand, it would be unwise to categorize any facet of eighteenth-century microscopy as either scientific or non-scientific. This is not how users themselves perceived the application of the microscope. Eighteenth-century researchers, such as Felice Fontana, were well aware of the microscope’s limitations as a tool of accuracy. In his 1781

[17] M. Ratcliff, *Quest for the Invisible,* p.256.
[18] A. Helden, ‘The Birth of the Modern Scientific Instrument’, in J.G. Burke (ed.), *The Uses of Science in the Age of Newton* (Berkeley, 1983), p.69.

essay, 'Erreurs Microscopiques', Fontana separated microscopic research between two modes of judgement: observation and experience.[19] To avoid the errors of observation, technical abilities as well as the instruments needed improvement. Microscopy did not fail at this task. As Wilson, Fournier and others have described, the microscope excelled as a mode of observation and viewing on the condition that it was in the hands of a skilled and patient user. Additionally, microscopy prompted new improvements and served a didactic function, enhancing the user's technical prowess. This, as much as microscopic discovery, is the great achievement of microscopy according to Fontana; however, Fontana warned his readers that quandaries would arise if one used deduction to interpret the images beneath the lens.[20] In this way he broke with many of his predecessors. Fontana and his fellow users understood that the microscope was incapable of eliminating the cognitive fallacies of the viewer. Microscopy endured into the eighteenth century, but it had failed to fulfil its promise to improve sensory perception. Users inevitably risked falling back on the errors of assumption: what Francis Bacon had once called the 'idols of the human mind.'[21]

ii. Practical obstacles

While early eighteenth-century microscope users raised epistemological concerns, microscope makers encountered obstacles to improving lens design. Although we cannot definitively state that continuous improvements to the instrument emerged, the effort to improve microscope clarity and magnification was a constant task taken up by early eighteenth-century practitioners.

By the eighteenth century, spherical and chromatic aberrations were evident, and seemingly omnipresent whether one used spherical lenses, bi-convex lenses, or plano-

[19] F. Fontana, 'Erreurs microscopoques et consequences deduites des observations microscopiques', in *Traité sur le venin de la vipère, sur les poisons Américains, sur le laurier-cerise et sur quelques autres poisons végétaux* (2 vols., Florence, 1781), ii, pp.245-52.
[20] Jutta Schickore dedicates considerable parts of her book, *The Microscope and the Eye* to a discussion of Fontana and his 1781 essay. See J. Schickore, pp.39-67.
[21] F. Bacon, 'Aphorismus', in *Novum organon* (London, 1620), xxxviii.

convex lenses. Difficulties multiplied as makers developed more elaborate microscope designs and incorporated multiple lenses. Although lens makers had long understood spherical aberration, its presence continued to trouble opticians. Seventeenth-century *virtuosi* had dedicated considerable study to the improvement of aspheric lenses since Descartes' *Dioptrique* of 1637, in particular, Spinoza and the Huygens brothers. Much of the emphasis here was on reducing spherical aberration through compound layers of lenses. To do so required new engines for grinding lenses based on Descartes' theory that one could carve a lens that was flat on one side and convex on the other, specifically, with a curvature that could fit within a pre-determined hyperbola. Such a hyperboloid lens would supposedly direct light towards a smaller focal point, thereby eliminating aberration. Following the example of Descartes, Robert Hooke illustrated lathe engines in his 1665 *Micrographia* that could grind such lenses with greater ease and uniformity.[22] Christopher Wren also proposed a promising hyperboloid grinding machine in the *Philosophical Transactions* in 1669, although it never came to fruition, and his illustrations of the device served more of a mathematical than a functional purpose.[23] Of course, most of these lens-grinding techniques aided telescopy, not microscopy. In 1667, the Royal Society extolled Francis Smethwick for his development of a semi-aspheric telescope.[24] The same accolades were not given for microscope designs. Microscope lenses were simply too minute: any slight adjustment to a grinding engine would cause major errors when manufacturing tiny microscope lenses.

[22] R. Hooke, *Micrographia*, plate 1.
[23] C. Wren, 'A Description of Dr. Christopher Wren's Engine Designed for Grinding Hyperbolical Glasses', *Philosophical Transactions of the Royal Society of London*, 4 (1669), pp.1059-60.
[24] Smethwick's telescope comprised three plano-convex lenses and one biconvex lens. F. Smethwick, pp.631-2.

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FIGURE 1. Light rays entering a bi-convex lens with aperture stop to correct for aberration. From J.B. McCormick, *Eighteenth-Century Microscopes: A Synopsis of History and Workbook* (Lincolnwood, 1987), p.34.

Both simple and compound microscopes exhibited varying degrees of spherical and chromatic aberration depending on the shape of their lenses (Fig. 1). An increased lens convexity aggravated aberration.[1] We see this problem especially in spherical lenses. One might assume that Leeuwenhoek was not concerned with the problems of aberration as his tiny spherical lenses exhibited such high degrees of spherical and chromatic aberration. Leeuwenhoek likely attempted to counter this: he simply set the lens in a slightly smaller aperture, effectively blocking off the outermost edges of lens while maintaining high magnification through the centre of the sphere. This technique minimised, but did not correct, aberration. Miniscule apertures also greatly reduced the light. Nevertheless, single and compound lens users employed the aperture blocking method throughout the eighteenth century to reduce spherical and chromatic aberration.

Multi-lens instruments compounded the problem of spherical aberration. This is clearly evident in three-lens microscopes, which gained popularity towards the end of the seventeenth century. Despite their obvious flaws, the problem of aberration did not keep Hooke from endorsing the compound microscope in his *Micrographia* (not surprisingly, historians widely attribute the invention of the three-lens microscope to Hooke and his

[1] For a more informative discussion of all types of lens aberration and their known solutions, see E. Hecht, *Optics* (San Francisco, 2002).

instrument maker Christopher Cock,[2] although Galileo developed a three bi-convex lens design as early as 1624[3]). Hooke's microscope widened the field of view through the addition of a third field lens between the eyepiece and the objective lens; however, doing so resulted in 'a very great company of radiating Pencils' of light that obscured the edges of the image. Hooke wrote that he often removed this third lens 'whenever I had occasion to examine the small parts of a body more accurately.'[4]

Examining seventeenth and early eighteenth-century treatises on optics, we find that spherical aberration remained problematic. In 1738, Cambridge mathematician Robert Smith dedicated an entire chapter of his *Compleat System of Opticks* to the problem of spherical aberration. Finding that instrument makers had done little since the days of Hooke and Huygens to address aberration, Smith endeavoured to improve compound devices. He experimented with apertures of various sizes, and encountered the same difficulties of aberration following increased magnification. Rather than employ new-lens shapes, Smith experimented with the distance between lenses in order to achieve maximum magnification with minimum aberration. He calculated the most precise imaging point based on the diameter of his lenses and their focal distances. Unfortunately Smith's convex lenses could not refract light into a single focal point. Most of Smith's techniques involved repositioning multiple plano-convex lenses of differing sizes to find the best solution. The result was a near limitless combination of various-sized lenses at differing focal lengths. Like his

[2] Hooke did not exclusively patronize Christopher Cock. Among his instrument makers was a young John Marshall, who also constructed microscopes for Boyle. Hooke records a meeting with him in a December, 1688 diary entry. Thereafter, Hooke's three-lens design heavily influenced the Marshall style scope. Hooke's reputation and Marshall's success as a business man may have popularized Hooke's compound design. See R. S. Whipple, 'Some Scientific Instrument Makers of the Eighteenth Century', *Science*, 72, no. 1861 (1930), p.209. Christopher Cock meanwhile also supplied telescopes to Newton and Hevelius. A.D. Andrews, 'Cyclopedia of Telescope Makers, Part 1', *Irish Astronomical Journal*, 20, no. 3 (1992), p.148.

[3] C.S. Ball, 'The Early History of the Compound Microscope', *Bios*, 37 (1966), pp53-4.

[4] R. Hooke, *Micrographia*, preface (no page number).

seventeenth-century predecessors, Smith found that these experiments could enhance magnification; however, high magnification still incurred high spherical aberration.[5]

The problems of aberrations have been the crux of recent scholarship concerning the question of decline. Attempts to assess the resolving power of eighteenth-century microscopes have been inconclusive.[6] Moreover, early opticians themselves seemed fatalistically disposed to accept aberration. The Newtonian revolution in optics during the first half of the century did not immediately foster new discoveries and designs, quite the contrary: Newton had argued as far back as 1671 that it was impossible to suppress chromatic aberration as all material possessed some range of refrangibility.

Much of Newton's doubt arose from difficulties he had in improving the refracting telescope. Although Newton's refracting telescope with its adjoined concave mirror minimised aberration, a similar refracting system was not easily applied to the microscope. Newton laboured to improve lenses for his microscope and telescope, but could not overcome the problem of spherical aberration.

> So that, were a glass so exactly figured, as to collect any one sort of rays into one point, it could not collect those also into the same point . . .Nay, I wondered, that seeing the difference of refrangibility was so great, as I found it, Telescopes should arrive to that perfection they are now.[7]

Newton abandoned work on the lenses themselves. He did not doubt that microscopes could improve, but to do so required additional design elements, as in his reflecting telescope, not improved lenses. In order to align refracted rays into a common focal point,

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[5] R. Smith, *Compleat System of Opticks in Four Books, viz. A Popular, a Mathematical, a Mechanical, and a Philosophical Treatise. To Which are Added Remarks upon the Whole* (London, 1738), book 2, ch.13.
[6] P.H. Cittert attempted to index resolution against magnification for late eighteenth and mostly nineteenth-century microscopes, finding a steady improvement in magnification, but little by way of eighteenth-century improvement in resolutions. *Descriptive Catalogue of the Collection of Microscopes in Charge of the Utrecht University Museum with an Introductory Historical Survey of the Resolving Power of the Microscope* (Groningen, 1934). More recently, Jan Deiman has contrasted chromatic and achromatic lenses from the late eighteenth and early nineteenth centuries. J.C. Deiman, 'Microscope Optics and J.J. Lister's Influence on the Development of the Achromatic Objective 1750-1850' (Imperial College London, Ph.D. thesis, 1992).
[7] I. Newton, 'A Letter of Mr. Isaac Newton Containing his New Theory about Light and Colours', *Philosophical Transactions of the Royal Society*, 6, no. 80 (1671), p.3079.

he theorised that a polished metal parabola could be fitted opposite the lens and focal point (Fig. 2).

> But there seemed very great difficulties, and I have almost thought them insuperable, when I further considered, that every irregularity in a reflecting superficies makes the rays stray 5 or 6 times more out of their due course.[8]

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FIGURE 2

Newton's hypothetical microscope with plano-convex lens (C-D), metal parabolic reflector (A-B), and focal point (F). From 'A Letter of Mr. Isaac Newton Containing his New Theory about Light and Colours', *Philosophical Transactions of the Royal Society*, 6, no. 80 (1671), p.3080.

Of course, we must be careful not to assume that Newton's optical theory cast a pall over microscopy. Newton himself endured immediate criticism from Hooke in a letter read before the Royal Society. The substance of Hooke's critique related to Newton's corpuscular theory of light and Newton's assertion that white light was a mixture of coloured corpuscles. 'Methinks, all the coloured bodies in the world compounded together should not make a white body' Hooke added, 'light is nothing but a simple and uniform motion or pulse of a homogeneous and adopted (that is a transparent) medium.'[9] In the same letter, Hooke decried Newton's claim that refracting lenses could not overcome the problems of chromatic aberration.

> And I am a little troubled, that this supposition should make Mr. Newton wholly lay aside the thoughts of improving telescopes and microscopes by refractions; since it is not improbable, but that he, that hath made so very good an improvement of telescopes by his own trials upon reflection.[10]

[8] Ibid.
[9] R. Hooke, 'Robert Hooke's Critique of Newton's Theory of Light and Colours (delivered 1672)', in *The History of the Royal Society* edited by T. Birch, (4 vols., London, 1757), iii, p.14.
[10] Ibid., p.12.

Newton's doubt might have dissuaded further innovation had anatomical investigation not provided evidence against his theory. Eighteenth-century dissectors were keenly interested in the mechanical function of the eye. They believed that the structure of the human eye itself evinced achromaticity. Leonhard Euler was the most well known of these. Although Euler was best known for his mathematical calculations, he was also interested in the refraction capabilities of the eye. Euler explained the lens system of the eye in a 1747 letter to the Royal Academy in Berlin. (Perhaps not coincidentally, by this time, Euler's own eyesight was failing on account of an unknown degenerative condition). Euler theorised, albeit incorrectly, that the human eye was a perfect achromatic system because fluid surrounds the lens of the eyeball. According to Euler, the 'contrary refractions' of the two materials minimised light dispersion and therefore eliminated chromatic aberration. Euler hinted that the same could be said for a glass lens soaked in water or possibly a fluid-filled prism. It was John Dollond (1706-1761) who first criticized Euler's reasoning in a letter to the instrument maker James Short, published in the *Philosophical Transactions* of 1753.[11] Like Newton, Dollond lamented that, 'according to the laws discovered by experiment, the aberration arising from the different refrangibility of light at the object-glass cannot be correct by any number of refractions whatsoever.'[12] Euler quickly responded that his algebraic formulations did indeed contradict Newton; nevertheless, Euler was adamant that practical techniques would eventually eliminate all problems of aberration although Euler likely realised that a water-soaked glass lens was not the solution. He pointed to Lieberkühn, who had just developed lenses with lessening curvatures on their outer rims. These effectively reduced spherical aberration, although not chromatic aberration.[13]

In 1755, Samuel Klingenstjerna (1698-1765), a fellow mathematician in Uppsala, supposed that the different angles of refraction could indeed combine without chromatic

[11] J. Dollond, J. Short, and L. Euler, 'Letters Relating to a Theorem of Mr. Euler', *Philosophical Transactions of the Royal Society of London,* 48 (1753), pp.287-96.
[12] Ibid., p.291.
[13] Ibid., p.293

aberration. Unlike Newton, he supposed that a similar system of immersed lenses would juxtapose the angles of refraction. The issue merely needed 'to be more accurately examined by experiments'[14] Klingenstjerna wrote to Dollond, who set about to readdress the problem, not with water-soaked lenses, but with achromatic doublets of flint glass and crown glass.[15]

Clearly, by the middle of the eighteenth century, mathematicians and opticians freely called into question Newton's scepticism regarding the achromatic lens. Moreover, instrument makers had already pre-empted Dollond's discovery of the achromatic doublet lens. Chester Moor Hall (1703-1771) already created achromatic doublets as early as 1830 for his telescopes. Unfortunately, neither Hall's nor Dollond's achromatic lenses immediately improved the minute lenses of the microscope.

Eighteenth-century authorities in microscopy continued to doubt the resolving quality of most lenses. This includes Henry Baker (1698-1774), patron to famed optician John Cuff. Baker, however, cast these problems in a hopeful light. While certain natural philosophers argued that microscopy had reached its zenith and could never resolve the known problems of aberration, Baker asserted that these were mere technical issues. Optical aberrations had indeed limited the capability of the microscope, but the skilled craftsman could obviate some, though not all, of these concerns. The wealthy Baker was in an optimal position to influence and advertise new microscope designs. Baker was a successful bookseller and respected naturalist. Among his broad contributions were observations on the regeneration of the polyp, the first instruction manuals for people with speech impediments, and observations on salt crystallisation. Baker's legacy continues today in the Bakerian Lecture he funded for the Royal Society. At the forefront of eighteenth-century microscopy, Baker published two popular guidebooks that illustrated the various instruments available: *The Microscope made Easy* (1742) and *Employment for*

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[14] H. C. King, *The History of the Telescope* (London, 1955), p.146.
[15] Ibid., p.145.

the Microscope (1753). For Baker, the goal of microscopy was to provide a clear image, and, equally important, to solicit witnesses who could confirm and repeat one's observations. High magnification was useful, but not vital to this task. More pressing was the need for repetition, clarity and ease of use. On the other hand, manufacturers and their patrons could easily point to higher magnification and 'new' compound lens arrangements as a selling point. Despite Baker's own reservations concerning the use of high magnifications for scientific pursuits, he and Cuff sold 'double' microscopes which provided greater magnification at the cost of clarity. One colleague, the naturalist Rev. Henry Miles (1698-1763), wrote to Baker that he could not recreate his experiments on the polyp using Baker's compound design.

> I wish you would make use of the single microscope more in some of your observations, as I am finding it impossible to obtain a distinct view of the minuter objects by that double.[16]

Perhaps because of these difficulties, the single-lens microscope remained popular among eighteenth-century observers as we shall see in later chapters. Although Baker and his instrument maker John Cuff hoped to profit from other compound microscopes, they were quite familiar with alternatives. Baker promoted several single-lens models, such as Lieberkühn's compass microscope and the Wilson screw-barrel design. He himself undertook a study of Leeuwenhoek's set of spherical-lens microscopes that the Dutch savant had willed to the Royal Society. Although an old design, the spherical lens (or 'globule') microscope posed a serious threat to the development of the compound scope. Leeuwenhoek was famously secretive about the construction of his lenses, but they were reported to possess astounding magnification (by some accounts, over 1000x), far beyond the capacity of any single bi-convex lens or double microscope. To test these claims, Baker borrowed Leeuwenhoek's spherical lenses from Martin Folkes at the Royal Society. Baker found that their magnification power was superb (although he estimated a conservative

[16] JRL, GB 133 MS 19 (Henry Baker Correspondence), vol 5, p.349: Henry Miles to Henry Baker, 13 Oct. 1763.

magnification of 160x). These smaller lenses made specimen observation difficult, especially as the short focal length required the lens to be pressed as close as possible to the specimen on one side and the observer's eye on the other. Baker quickly recognised that spherical aberration also grew as magnification increased.[17] To Baker, this was unavoidable in any microscope design, but it was a fault that his craftsman John Cuff could easily remedy owing to his skill in grinding and positioning lenses. According to Baker, not only did Cuff's microscopes reduce spherical aberration, even Cuff's single biconvex lens screw-barrel could outperform Leeuwenhoek's famous lenses as Cuff's could magnify over 400x.[18]

Unlike Leeuwenhoek, Baker was reluctant to advocate high magnification at the price of clarity. This led Baker to reject Leeuwenhoek's small spherical lenses. We will discuss these spherical lenses in greater detail chapter 5. Baker settled for a more profitable business solution: he advised clients to purchase both a simple lens for clear viewing and a compound microscope for high magnification. Turning to simple lenses, Baker, like most of his contemporaries, advocated the use of a biconvex lens with an aperture stop that could provide clear images but at the cost of high magnification. This included the Wilson screw-barrel design and later, the aquatic microscope. If one sought greater magnification, the compound microscope could do the job; however, the merits of the compound design remained a point of contention among observers. Of course, according to Baker, no craftsman could surpass his own John Cuff in either magnification or clarity. Cuff produced both simple and compound designs. Still, the aberration problems plagued these models as well.

To illustrate the aberration problems associated with eighteenth-century compound microscopes, Savile Bradbury of the Royal Microscopical Society observed modern test

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[17] H. Baker, 'An Account of Mr. Leeuwenhoek's Microscopes', *Philosophical Transactions,* 41 (1739), p.506.

[18] Ibid. p.513.

plates through several multi-lens microscopes.[19] His 1966 tests compared the magnification and aberration of a wide range of models, including a 1690 English Tripod Microscope, a 1705 Marshall microscope, a 1740 Culpeper, a 1750 Cuff Double microscope, a 1776 Martin Universal microscope and a similar George Adams Universal microscope of 1790. The earliest of these compound lenses (The English Tripod, Marshall, Culpeper and Cuff models) typically contained two lenses with a smaller convex condenser lens mounted beneath the stage mount and specimen. Later compound microscopes (such as the Martin and Adams designs) incorporated three and sometimes four magnifying lenses. As a practicing micro-anatomist, Bradbury endeavoured to show how such early compound scopes compared with his own modern standards. As Bradbury concluded, the quality of the images was diverse; nevertheless, he found that none of these eighteenth-century compound microscopes was comparable to the achromatic lenses developed in the early nineteenth century under J.J. Lister or Giovanni Amici.

A modern comprehensive study of period microscopes would prove daunting as Bradbury and others have found that microscope performance differed considerably even when one compares designs of the same model and maker. Nevertheless, when comparing several examples from the eighteenth century, Bradbury found that most, if not all, the compound scopes exhibited conspicuous aberration and focusing difficulty. It is tempting to compare these flaws with more modern microscopes; however, for our purposes, it is more useful to discern how these drawbacks affected the contemporaneous practice of early microscopic manufacturing and research. The intense aberrations associated with the compound designs indicate why the single-lens microscopes continued to gain popularity throughout the eighteenth century.

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[19] Published in S. Bradbury, ‘The Quality of the Image Produced by the Compound Microscope: 1700-1840’, in S. Bradbury and G. L’E. Turner (eds.), *Historical Aspects of Microscope: Papers Read at a One-Day Conference Held by the Royal Microscopical Society at Oxford, 18 March, 1966* (Cambridge, 1967), pp.151-74.

On the other hand Jan Deiman has more recently used modern micrometers and star tests to gauge the quality of similar eighteenth-century microscopes and has not found them significantly inferior to later achromatic models. Deiman finds that aberration was present in eighteenth-century microscopes but was 'relatively harmless' in single-lens microscopes; moreover, Deiman contends that aberration did not severely impede the resolution and magnification of pre-achromatic compound microscopes. As Deiman argues, 'The claim that the image of these microscopes was degraded by spherical and chromatic aberration is in my view an oversimplification'.[20] My own re-enactment experiments lend weight to Deiman's argument. Furthermore, one could argue that aberration was not a severe problem from an examination of early illustrations, such as those in Hooke's *Micrographia.* Hooke's illustrations of his views through the lens do not indicate aberration or any distortion: this is not because Hooke's microscope lacked spherical and chromatic aberration; rather, Hooke, and later microscope users recognised aberration and corrected it in their illustrations. In any case, whether or not spherical and chromatic aberration was an actual limiting factor to research remains debatable. More important is the fact that, by the mid-eighteenth century, instrument makers recognised aberration as a liability to selling their products. Master opticians such as Cuff, Martin, and Adams continuously flaunted their abilities to minimise aberration or decried the poor quality of their competitors' lenses; thus, aberration became an eighteenth-century problem whether it originally was or not.

Bradbury's optical tests provide a straightforward illustration of aberration and decreased resolution that eighteenth-century manufacturers faced as they laboured to improve the compound microscope. One of Bradbury's examples is the three-lens microscope of Edmund Culpeper (1660-1740). This was a common early eighteenth-century compound microscope for anatomical and naturalist work. This consisted of an eyepiece, field lens, and objective lens: all bi-convex. One of the major difficulties of this

[20] J.C. Deiman, 'Microscope Optics and J.J. Lister's Influence on the Development of the Achromatic Objective 1750-1850' (Imperial College London, Ph.D. thesis, 1992), p.155.

instrument was the fact that it possessed no true focusing mechanism. Pressure was applied to the cardboard tube to reduce the distance between the field and objective lenses. The friction of the tube alone maintained focus. Although a seventeenth-century design, these models remained popular well into the following century, as evident from the following model dated 1750. While popular, this design exhibited prominent spherical and chromatic aberration (Fig. 3, right test plate).

MICROSCOPE **LENS STRUCTURE** **MICROMETER TEST**

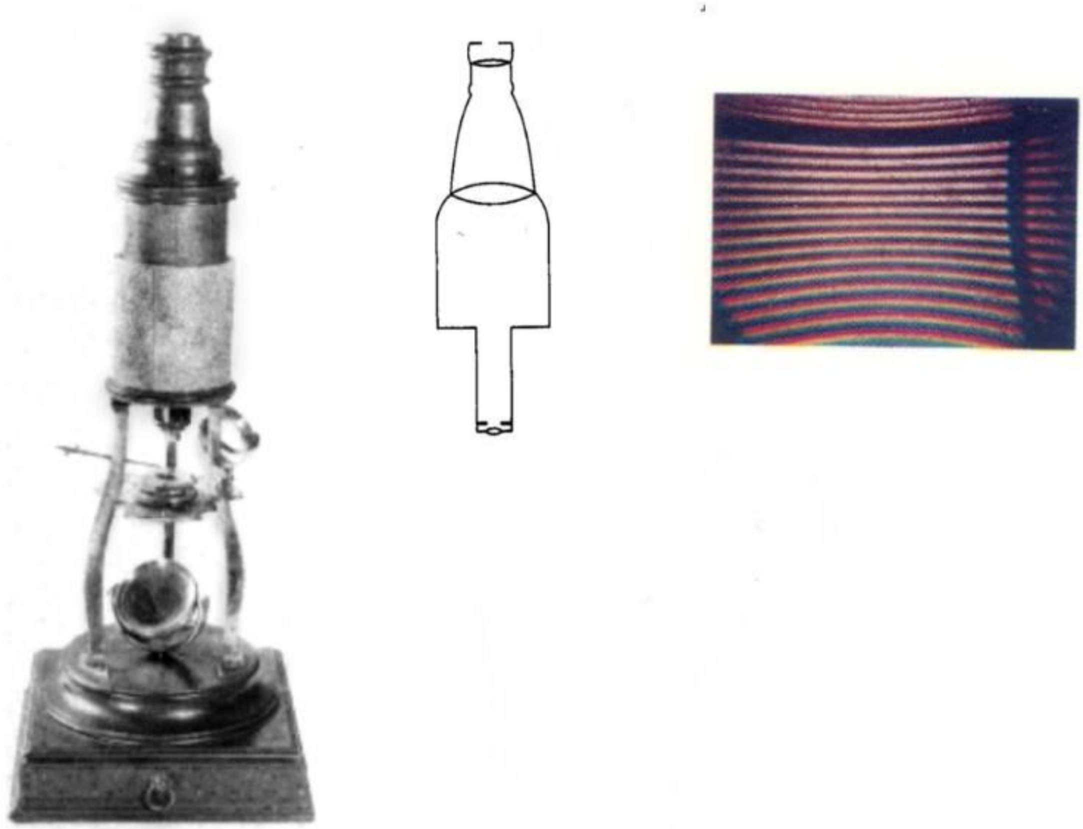

FIGURE 3. Culpeper type microscope, c.1750. From S. Bradbury, 'The Quality of the Image Produced by the Compound Microscope: 1700-1840', in S. Bradbury and G. L'E. Turner, eds., *Historical Aspects of Microscope: Papers Read at a One-Day Conference Held by the Royal Microscopical Society at Oxford, 18 March, 1966* (Cambridge, 1967), p.164. Permission to reprint images granted from the Royal Microscopical Society.

While aberration is severe in this model, less noticeable is the loss of peripheral lighting, a process now known as vignetting. This has many causes: it typically arises from physical obstructions within the focal length. One example of mechanical vignetting occurred in Leeuwenhoek's hand-punched apertures, which created an uneven circle around the field of view. In the case of compound microscopes, the edges of layered lenses noticeably blocked light. An optician who could not perfectly align the lenses within the optical tube ran the risk of aggravating this particular form of vignetting. Finally, there is the unavoidable fact that brightness decreases when light rays arrive at a higher angle of incidence.[21] This diminished light in all microscope lens systems whether they were chromatic or achromatic. Most subsequent compound models attempted to improve lighting quality through the use of a condenser lens just below the specimen sample as well as Culpeper's innovation of the reflecting base mirror.

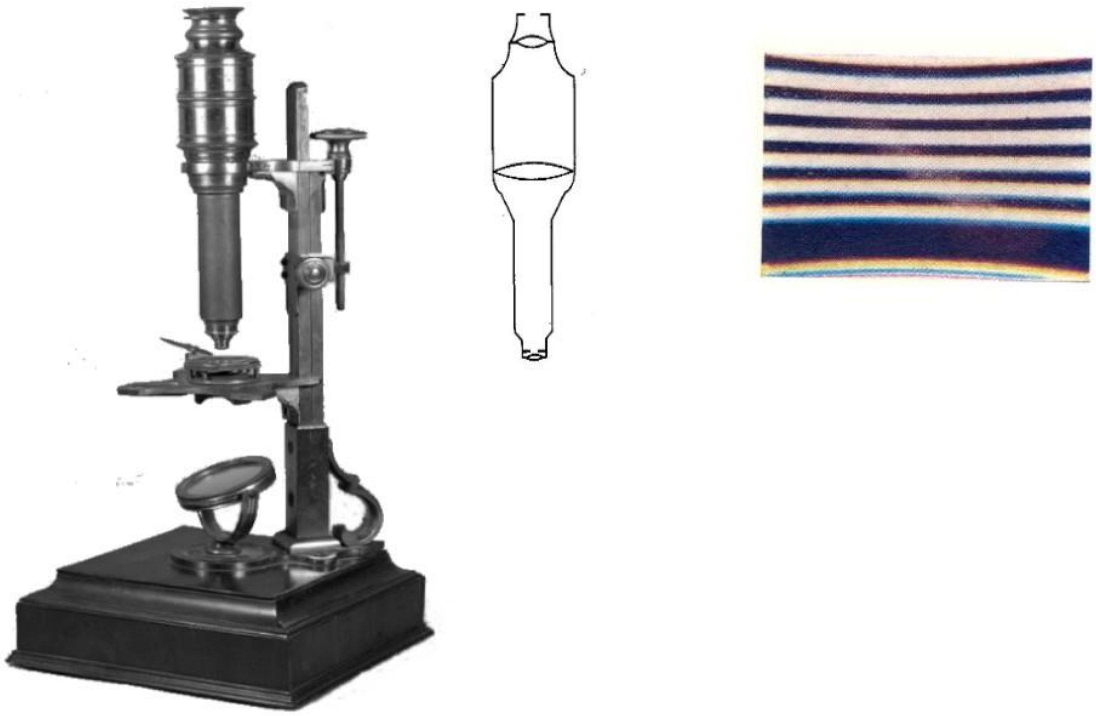

FIGURE 4. John Cuff Microscope, c.1750 From S. Bradbury, 'The Quality of the Image Produced by the Compound Microscope: 1700-1840', in S. Bradbury and G. L'E. Turner, eds., ***Historical Aspects of Microscope: Papers Read at a One-Day Conference Held by the Royal Microscopical Society at Oxford, 18 March, 1966*** **(Cambridge, 1967), p.161. Permission to reprint images granted from the Royal Microscopical Society.**

[21] This is a form of vignetting now labelled the 'cosine fourth law' because brightness is proportional to $\cos^4$ of the angle between the incident light ray and the optical axis.

John Cuff's famous 'Double' microscope (first introduced in 1743) was one of the most influential designs to emerge in the eighteenth century. Cuff, in collaboration with Henry Baker, eliminated the tripod in favour of a sliding side-pillar. This freed the space for the optical sample. Another exceptional innovation was the fine-focus screw-lens. Not surprisingly, Cuff's model found numerous imitators both in Britain and on the continent. Instrument makers Benjamin Martin, George Adams, Claude Passement, and others quickly created their own versions of the universal double microscope.

Spherical and chromatic aberration is drastically reduced through Cuff's microscope. However, if we compare his lens system to the Culpeper models, there is little variation. Both models used a system of three similar bi-convex lenses. We can attribute the success of Cuff's design to his skill as a craftsman and lens-grinder.

Turning to other microscope models, we see the development of the four-lens compound microscope growing in popularity, especially in the later half of the eighteenth century. Optician Benjamin Martin had promoted the four-lens microscope as early as 1759, although he recorded that they had been in existence since before Huygens's *Traité de la Lumière* of 1690.[22] Martin did not warn his clients against the four-lens design although he stipulated that instrument makers such as himself could construct such a device only if they had a firm understanding of Newton's theory of refraction and aberration. This continuous effort to incorporate Newton's *Opticks* and address the problems of aberration was, in Martin's estimation, the difference between eighteenth-century opticians and their earlier predecessors. According to Martin, most of these older models had been a product of trial and error and not theory.

Another four-lens design, George Adams' 1790 model, attempted to resolve chromatic aberration using multiple biconvex and plano-convex lenses (Fig. 5), Such a lens combination, as historian Edward Frison found, typically attained a magnification up to 30

[22] B. Martin, *New Elements of Optics; or, the Theory of the Aberrations, Dissipation, and Colours of light: of the General and Specific Refractive Powers and Densities of Mediums* (London, 1759), p.52.

times.[23] The finer images produced from this complex design did minimize chromatic aberration (Fig. 5, right); however, spherical aberration clearly still remains, and was oftentimes more obtrusive than in simpler models. Bradbury writes, '[T]he end result is a worse performance than in many earlier microscopes in which no attempt has been made to overcome their optical shortcomings.'[24] The unavoidable glare produced from these multiple lenses was also problematic as evident in the low-contrast of the following image from Adams' three-lens design.

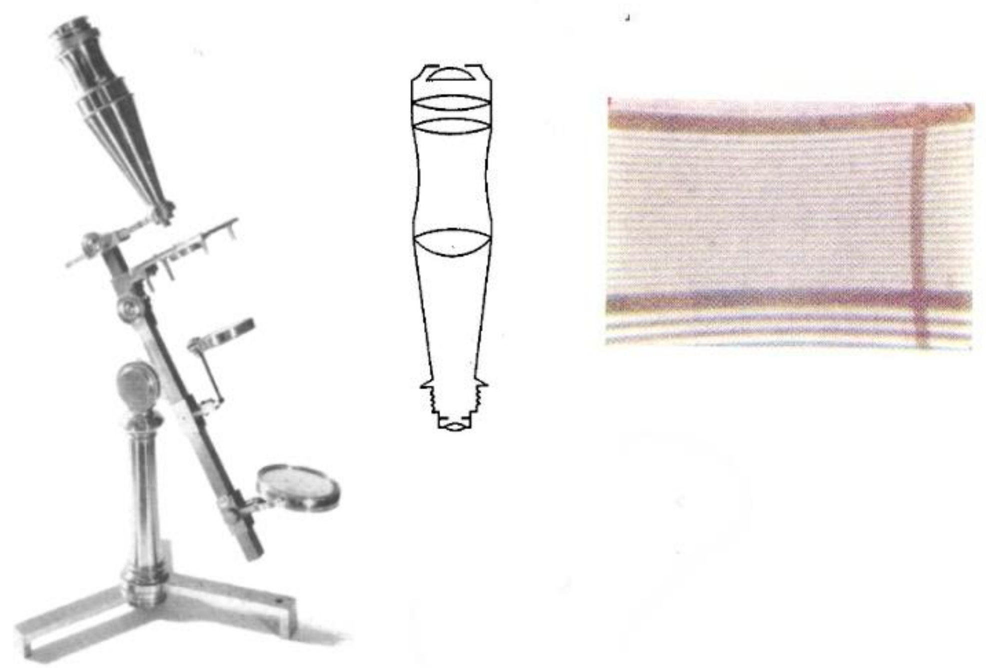

FIGURE 5. Adams the younger 'Universal' microscope, undated. From S. Bradbury, 'The Quality of the Image Produced by the Compound Microscope: 1700-1840', in S. Bradbury and G. L'E. Turner, eds., *Historical Aspects of Microscope: Papers Read at a One-Day Conference Held by the Royal Microscopical Society at Oxford, 18 March, 1966* (Cambridge, 1967), p.168. The box containing this microscope is dated 1830, but the design was introduced as early as 1790. Permission to reprint images granted from the Royal Microscopical Society.

Any multi-lens system increased the cloudiness of the optical image owing to impurities in the lens glass or improperly polished lenses. Additional plano-convex lenses could reduce, but not eliminate, chromatic aberration. Microscope makers, such as Adams, and designers, such as Baker, were quick to advertise new models that might enhance

[23] E. Frison. 'Adams' Microscopes and Microtomes 1', *The Microscope,* 8 (1951), p.204.
[24] S. Bradbury, p.169.

magnification and clarity. Nevertheless, even a well-aligned layer of bi-convex and plano-convex lenses could prove unsuitable because of the haziness and glare of low-quality lenses. To counter this, the maker of compound microscopes needed to be highly adept at lens grinding and polishing. Each hand-polished lens was therefore unique; just as each hand-crafted instrument was distinct from the last. One could not advantageously mass-produce such instruments, and most craftsman chose not to expose the process to error by dividing the manufacturing among many different hands. Most serious eighteenth-century users recognised these limitations and did not necessarily flock to the latest design trends or inexpensive quickly-made instruments.

According to Bradbury, these difficulties hindered further developments of the compound microscope.

> The result of these troublesome optical faults was that the scientist of the eighteenth and early nineteenth centuries was often forced to use the simple microscope.[25]

The expertise of established and methodical lens makers typically proved more attractive than novel designs. Although one might suspect that the defects of compound models inhibited eighteenth-century microscopy, compound designs remained in use, often as a supplement to simple-lens viewing. Furthermore, overcoming well-known optical difficulties could enhance the prestige of those opticians skilled enough to provide viable models. The difficulty in constructing clear lenses escalated the cost of labour. More importantly, it solidified the optician's role as more than a mere manufacturer. Craftsmen could attain prestigious positions associated with the court (such as George Adams the younger, who became optician to King George III).[26]

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[25] S. Bradbury, p.171.

[26] Dollond also became a Royal Society member in 1758. Cuff and Martin contended, albeit unsuccessfully, for such membership.

2.2. New modes of marketing and manufacturing

Eighteenth-century microscopy survived in the context of deepening consumerism and emerging manufacturing. Whether or not 'scientific' research declined or stagnated during this period is moot; microscope sales flourished, and production increased steadily.

Despite their difficulties in improving microscope designs, microscope makers, especially on the British Isles, benefited from a thriving commercial sector and an increasing demand for instruments. This is not to say that manufacturing scientific instruments was a stable profession: both Cuff and his rival Benjamin Martin eventually declared bankruptcy. Nevertheless, new techniques of manufacturing, mining, and trade cheapened supplies and fostered microscope production during the early eighteenth century. The opportunity for microscopic research had never been greater and competition among instrument makers was on the rise. Indeed, as Morrison-Low shows, the number of philosophical, mathematical and optical instrument makers operating in the British Isles greatly increased as the century progressed.

One must tread carefully here lest we assume that industrialisation provided the catalyst to scientific and technological advancement. As Lewis Mumford cautiously noted, technological endeavours are fundamentally 'the result of human choices and aptitudes and strivings, often irrational when apparently most objective and scientific.'[27] The growth of microscopy was hardly a *fait accompli*. Morrison-Low cautions that the industrialization of Europe by no means fostered the contemporaneous rise of microscopic manufacturing. Instrument manufacturing was, after all, an artisan occupation. Instrument production gradually expanded into larger-scale workshops in the provinces, but for most of the eighteenth century, it remained a small-scale business.

[27] L. Mumford, *Technics and Civilisation* (New York, 1936), p.6.

The image originally presented here cannot be made freely available via ORA because of copyright. The image was sourced from the book: A. Morrison-Low, *Making Scientific Instruments in the Industrial Revolution* (Aldershot, 2007), p.38.

FIGURE 6. Morrison-Low's bar chart concerning the number of instrument manufacturers operating in eighteenth-century Great Britain. A. Morrison-Low, *Making Scientific Instruments in the Industrial Revolution* (Aldershot, 2007), p.38.

How then do we explain new developments in microscope manufacturing? Morrison-Low’s argument that instrument production was a separate entity from industrialisation is a good starting point. She has found that the instrument-trade remained linked to urban centres, while provincial manufacturers seldom flourished, even well into the nineteenth century. This has little to do with the steady eighteenth-century industrial urbanisation of Europe. On the contrary, instrument makers remained imbedded in cities such as London and Paris, often because of deep-rooted social institutions. In the case of microscope manufacturing, proximity to the Royal Society or the Académie des Sciences greatly aided business. On the other hand, microscope makers were equally interested in keeping close ties to important patrons who lived and conducted business in the major urban centres.[28]

[28] Morrison-Low has also found that new markets for microscopes emerged in Bristol and Liverpool in the later half of the eighteenth-century (See chapter 2 'Bristol and Liverpool' in A. Morrison Low, pp.47-72). Port towns fared well in the sale of compasses, quadrants and other nautical instruments. However, at least one instrument maker, Henry Edgeworth of Bristol sold microscopes of various kinds. His 1779 trade card (A. Morrison-Low, p.55) advertises solar microscopes 'of the latest improvements with a truly parallel glass' along with Wilson's microscope and achromatic telescopes.

Further studies of eighteenth-century craft society have explored the new resources available to manufactures. In particular, John Rule's, *The Vital Century*[29] examines a number of diverse elements impacting the eighteenth-century artisan trades, from the price of metal, to transportation, to increasing per-capita income. For Rule, artisans and instrument makers were the core economic and creative forces behind the economy of eighteenth-century Britain. A 1772 portrait of John Cuff, painted by Zoffany, graces the cover of his book (Fig. 7).

The image originally presented here cannot be made freely available via ORA because of copyright. The image was sourced at the Royal Collection Picture Library: 'John Cuff and His Assistant', By Johann Zoffany, 1772.

FIGURE 7. 'John Cuff and His Assistant', By Johann Zoffany, 1772

Rule has found that instrument making benefited from a dramatic reduction in the price of metals and refining. Nehemiah Champion introduced granulated copper in 1723, just as a steep drop in the price of copper and zinc hit Britain. Together with the newly invented reverberatory furnaces, these innovations made smelted brass cheaper, stronger,

[29] J. Rule, *The Vital Century: England's Developing Economy* 1714-1815 (London, 1992).

and much more available for use in instruments and other functions.[30] Meanwhile, transportation improved following the expansion of the canal systems and carriageways. Instrument makers were able to extend their pool of suppliers. James Short, for example, was able to purchase brass for his microscopes and telescopes at a cheaper price by importing it some distance from London to his Edinburgh shop in 1736.[31]

Early eighteenth-century opticians also experimented with flint glass: a vital component in later achromatic doublets, but also useful in creating generally clearer lenses. Flint powder, added to regular silica or soda-lime powder creates highly transparent and refractive glass once set in a furnace.[32] Early attempts at this process exposed the resulting glass to added impurities within the flint stones. Although the technique was advantageous for seventeenth-century makers of decorative glassware, it created imperfect optical lenses. Glass makers learned to improve the process around 1730 with the addition of lead, which dramatically enhanced transparency and maintained the desirable refractivity. According to historian of science Edward Frison, 'pure pieces without defects ... were of very small size and often unobtainable'[33], but this soon changed. This type of glass became so successful, in fact, that Parliament imposed a weight tax on international shipments of lead-flint glass in 1746. Such glassware proved to be an enticing commodity among optical instrument makers. Thus, when Chester Moore Hall produced the first achromatic telescope using his lead flint/silica doublet innovation in 1730, he utilised previously unavailable resources. These doublets of Hall (and later Dollond) featured a bi-convex lens made of crown glass pressed into a concave piece of lead flint-glass. While this achromatic doublet greatly improved telescopes, most eighteenth-century microscopes still contained only silica-based

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[30] J. Day, 'Copper, Zinc and Brass Production' in J. Day and R.F. Tylecote (eds.), *The Industrial Revolution in Metals* (London, 1991), pp.135-44.
[31] A. Morrison-Low, p.140.
[32] According to most accounts, George Ravenscroft (1632-1683) first performed this technique in the early 1660s.
[33] E. Frison, 'Achromatism', *The Microscope: The British Journal of Microscopy and Photomicrography* 8, no. 6 (1951), p.158.

blown glass (also known as crown glass). Microscope lenses did not lend themselves well to doublets because minute adjustments between lenses could wildly alter refractive capabilities. Manufacturers also did not fully understand the benefits of using more than one doublet. A proper achromatic microscope requires several doublets arranged at various focal distances. Without these, chromatic and spherical aberration could actually worsen.

Given the availability of these resources and doubleting techniques, historians have often questioned why the achromatic microscope arrived decades after the achromatic telescope. According to Gerard L'E. Turner, 'there was an unwillingness to develop new lens-grinding techniques and machinery for an unproved market when a perfectly good living could be had by making the same old designs.'[34] One could use the half-century gap between the development of the achromatic telescope and the achromatic microscope to feed the polemic that eighteenth-century microscopy was a lagging discipline. However this argument distracts from a deeper analysis of the history of microscopy. In reality, microscope makers did attempt to recreate the achromatic doublet for their magnifiers not long after Dollond popularised achromatic telescopes. Frison records that Harmanus Van Deijl manufactured and sold the first achromatic microscope in 1770, just over a decade after Dollond introduced his telescope model.[35] Nonetheless, the technical limitations remained challenging. It was not until the early nineteenth century that Joseph Jackson Lister, Giovanni Amici, and Alexander Gilles[36] perfected this achromatic microscope design.

2.3. Social changes affecting microscope manufacturing

Social changes also affected the microscope craft. Opticians remained rooted in traditional modes of master/apprentice labour although guild restrictions had declined since the late

[34] G.L'E. Turner, 'Micrographia Historica', p.16.
[35] Frison, 'Achromatism', The Microscope: The British Journal of Microscopy and Photomicrography 8, no. 6 (1951), p.159.
[36] Alexander François Gilles published under the nom de plume 'Selligue'.

seventeenth century. Guilds and craft companies such as the Worshipful Company of Spectacle Makers had lost their powers of enforcement by the mid-eighteenth century even though some influential opticians, such as John Cuff, retained membership. It had formerly exercised complete control over the quality of merchandise. In one 1671 incident, the company found the lenses of a Canning Street optician's shop to be deficient. They acquired a warrant and brought the optician's stock to Guildhall, where a jury deemed the merchandise inadequate and ordered the lenses broken on the cobblestones of the street.[37] It is difficult to say how typical such an incident was during the seventeenth century, but the company clearly wielded less power by the mid-eighteenth century. In 1752, the Company of Spectacle Makers opened its membership to any labourer who cared to pay dues, effectively abrogating its limitations on shop ownership and craftsmanship. This lessened the pressure on the optician to meet guild standards; it also freed him to develop a wider range of other products. Perhaps the loss of guild authority fostered competition, which soon became severe among British instrument makers.

Leading instrument makers such as Benjamin Martin may not have been a member of any craft company at all. Illustrative of the changing nature of the profession, Martin did not work as an apprentice to an instrument maker before pursuing the trade; instead, he was an itinerant lecturer in natural philosophy. Other instrument makers, such as George Adams Sr. and his sons all had experience as apprentices and had joined all-inclusive companies. In Adams' case, this was the Grocer's Company, although Adams' real source of stability was his title as instrument maker to the Prince of Wales and later the King.[38]

Eighteenth-century opticians typically continued the traditional master-apprentice relationship. Optician apprentices remained close to and, in several instances, married into the family of their masters, if they were not the sons of the masters themselves. For

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[37] The Worshipful Company of Spectacle Makers, 'Company History' <http://www.spectaclemakers.com/company/history.htm> Accesssed 18 Jan 2010.

[38] J.R. Millburn, *Benjamin Martin: Author, Instrument-Maker, and Country Showman*, p.86.

example, the eminent optician, John Smith married John Marshall's daughter and also succeeded his father-in-law at his Ludgate Street shop.[39] An apprentice relationship created close bonds that could set a novice shop-owner on the path to success. John Cuff's master, James Mann the younger, likely aided Cuff in establishing his own Fleet Street business just a few doors down from his own.

Shop assistants and visiting assistants began taking on a more critical role. Whereas apprentices or journeymen would traditionally labour in the hopes of owning a shop themselves, by the middle of the eighteenth century assistant-visitors occasionally arrived to learn new techniques for their clientele abroad. This is relevant to the history of microscopy outside of the British Isles, as a number of microscope makers travelled to Great Britain to learn the latest manufacturing techniques. Morrison-Low relates one example where Russian instrument maker Nikolai Chizhov travelled to London to assist George Adams Sr. for eight months in 1759 before returning to St. Petersburg to foster the optical instrument trade there.[40]

In Britain, legislation prohibited masters and journeymen from taking their business abroad until 1824; however, that restriction did not apply to certain professions such as clock makers. Parliament was more restrictive on the attempts of colonials to lure established instrument makers away from Britain (for example the Artificers Act of 1718).[41] Later acts of Parliament (1785, 1786) also prohibited the exportation of iron tools and machinery in an effort to preserve production within Britain; however Parliament aimed this legislation at the large workshops, not the smaller instrument manufacturers or their goods.[42] I have found no instances where legislation hindered the movement of individual opticians.

[39] Circa 1723, The shop was called 'The Archimedes and Spectacles' in Ludgate, opposite the west end of Saint Paul's. Andrews, p.213.
[40] A, Morrison-Low, pp.149-50.
[41] See, for example, Act of Parliament 5 Geo. 1, c.27 (1718).
[42] Ibid., p.149.

Historian James A. Bennett has found that travellers frequently chose London over Paris as a centre of instrument craft. This appears to have been the dominant trend throughout the eighteenth century.[43] According to Maurice Daumas, this trend persisted until the French instrument trade became more competitive towards the end of the century.[44] The decline of the French instrument trade has puzzled many historians of science, especially Daumas who cited its brief commercial revival only towards the end of the eighteenth century. According to Anthony Turner, the poor shape of the French instrument trade may be due to the stalwart Paris guild system that catered to small groups of local clients, as opposed to large international markets.[45] There is much evidence to support this claim: the French *jurandes* tightly controlled entry into the artisan trades. When Turgot briefly disbanded the *jurandes* in 1776, the number of advertisements for new instruments and workshops soared. However, this was only temporary. Throughout most of the century, established opticians in Paris apparently benefited from an enclosed and more stable trade environment. As Turner and Ratcliff have found, Parisian opticians typically catered to the elite members of the Académie des Science.[46] This, in Ratcliff's estimation, does not mean that French manufacturing diminished. Contrary to Daumas, Ratcliff contends that French opticians simply conducted business with less visibility than their British counterparts. As evidence, he points to the influx of advertisements during the suspension of the *jurandes.* More convincing is Ratcliff's argument that French opticians conducted business inconspicuously in private institutions, such as abbeys belonging to the Church or under the authority of the Académie des Sciences and King, whose *Lettres patentes* could exempt opticians from the corporate restrictions on advertising and trade.

[43] J.A. Bennett, 'Shopping for Instruments in Paris and London', in P.H. Smith and P. Findlen (eds.), *Merchants and Marvels: Commerce, Science and Art in Early Modern Europe* (London, 2002), p.377.
[44] M. Daumas, *Les instruments scientifiques aux xvii et xvii siècles* (Paris, 1953), pp.353-60.
[45] A.Turner, 'Mathematical Instrument-Making in Early Modern Paris', in R. Fox and A. Turner (eds.), *Luxury Trades and Consumerism in Ancien Régime Paris: Studies in the History of the Skilled Workforce* (Aldershot, 1998), p.3
[46] M. Ratcliff, 'Europe and the Microscope in the Enlightenment', pp.162-8.

Nevertheless, there was not a dramatic influx of advertisements among opticians who had been fortunate enough to obtain such patent rights. This is possibly because their market and reputation had already been established among elite users.

Examining primary sources, such as Henry Baker's correspondences, I have found that a number of British microscope users actually envied the French system of instrument manufacturing and trade. Although it may have limited competition, support from the French Académies ensured a more stable working environment for their craftsmen. As one correspondent complained to Baker,

> This hath been wanting hitherto, I believe in all our worthy societies in England, tho' not in all others for if I be rightly informed, the Royal Académie at Paris frequently assists the inventor of any curious art to put his inventions into practice.[47]

The role of eighteenth-century French instrument makers and users will be further explored in Chapter 8.

Aside from state and corporate regulation, external events greatly impacted the instrument trade. Any account of early eighteenth-century trade networks must address the rapid political and social events that impacted commerce across Europe. The War of Spanish Succession (1702-1714) plunged the continent into chaos. The war disrupted trade and more than doubled the national debt of Great Britain. Not long after, in 1720, the Great Plague of Marseille halted travel between key Mediterranean ports (momentarily crippling the French economy) and provoked similar fears about pan-European commerce. However, these early eighteenth-century troubles did provide some benefit for the instrument trade. Under the threat of war, military expenditure on navigational and gunnery instruments surged, particularly in aid of the Duke of Marlborough's engineering corps that was fighting in the Netherlands.[48] As we will see in the next chapter, the outbreak of plague

[47] JRL, GB 133 MS 19 (Henry Baker Correspondence), vol. 7: William Arderon to Henry Baker, (no day recorded) Feb. 1760.

[48] J. Ostwald, *Vauban under Siege: Engineering Efficiency and Martial Vigor in the War of the Spanish Succession* (Boston, 2006).

prompted renewed interest in contagion theory and compelled some microscope-based studies related to contagious animalcules. On the whole, however, there is no conclusive evidence that these events either diminished or enhanced the production and use of scientific instruments. They did however cause great disruption within the network of letters and specimen samples sent between microscope users.

As John Millburn found, instrument makers (including microscope makers) began to cater to military interest with increasing vigour, particularly during the next major conflicts: the War of Austrian Succession (1739-1748) and the Seven Years War (1756-1763). The British Office of Ordnance continuously indebted itself though purchases of equipment, with Parliament absolving its quarter-million pound debt in 1747.[49] Craftsmen had much to gain if they catered to these military offices by advertising themselves as mathematical instrument makers as opposed to mere opticians. George Adams the elder certainly landed a lucrative position as official Mathematical Instrument Maker to the Office of Ordnance. He supplied over 1500 instruments between 1748-1772.[50] Of course, other instrument makers continued to compete with Adams for business as independent contractors, such as John Dollond, Thomas Wright, Jesse Ramsden, and others. Although some records of the Office's expenditure have been lost, Millburn records that George Adams earned at least £2,425 in total[51], which greatly supplemented his commercial business in the instrument trade. Of course, these military expenditures had no immediate impact on microscope manufacturing. British military hospitals did not regularly purchase microscopes, although army and navy surgeons infrequently received them later in the century. Mid-eighteenth-century military purchase requests do exist for sets of medical microscopes; instrument makers occasionally received these especially from military

[49] J.R. Millburn, 'The Office of Ordnance and the Instrument-Making Trade in the Mid-Eighteenth Century', *Annals of Science,* 45, no. 3 (1988), p.243.
[50] Ibid., p.221.
[51] Ibid.

hospitals in Russia and Prussia.[52] Regardless of the need for microscopes, military expenditures indirectly aided microscopic manufacturing. The income and financial stability earned from military instruments allowed makers to fund and advertise work on microscopes for their private clients. Adams, who was well known for his speed in meeting demand, could more easily afford to keep a stock of universal microscopes on hand for potential customers. Military needs also encouraged improvements to lens designs. Ordnance offices require various lens instruments: spy-glasses, periscopes, and telescopes, and telescopic theodolites. The Office of Ordnance paid quite well for these instruments, purchasing several achromatic telescopic theodolites from Adams at a price of £21 each in the 1760s.[53]

The obvious benefits aside, negotiating with government institutions such as the Office of Ordnance could be a double-edged sword: at any time, the Office could demand a large order of instruments or let months slip by without issuing any order warrants at all. Adams suffered some distress when the Office cancelled purchases before delivery or accidentally issued duplicate orders to competing instrument makers.[54] The business also required a sizeable overhead to fulfil large orders, craftsmen required a reserve of material and assistants at the ready. The Office's policy of payment *after* delivery surely complicated this business. Millburn explains that even after a supplier delivered his merchandise, his bill of sale would linger in bureaucratic chambers, eventually proceeding before a panel for consideration. Several months could elapse before it was 'allowed' to proceed into the Ordnance Bill Book. Further time passed before the Ordnance Treasurer made the actual payment.[55] Compare this with private instrument manufacturers, such as John Cuff, who frequently received payment weeks or months before supplying his

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[52] JRL, GB 133 MS 19 (Henry Baker Correspondence), vol 6, p.145: James Mounsey to Henry Baker, 10 Apr, 1755.
[53] J.R. Millburn, 'The Office of Ordnance and the Instrument-Making Trade in the Mid-Eighteenth Century', p.245.
[54] Ibid., pp.251-2.
[55] Millburn explains this with a useful flow chart. Ibid., p.230.

customers with their orders; this particularly applied to foreign clientele who needed their instruments manufactured and sent swiftly abroad. In summary, Adams' relationship with the Ordnance Office is just one example of the changing nature of personal and institutional patronage in the eighteenth century.

What can we conclude from this history of instrument manufacturing and trade? Certainly, the microscope trade in the eighteenth century was a complex and radically changing profession. Moreover, we find that the most desired microscope clients were not universally dilettantes as early scholars such as David J. Bryden have posited.[56] Ratcliff, Fournier, and others have paid more recent attention to the customer in addition to the instrument maker. I take a similar approach in this thesis, examining the client's correspondences with instrument makers, patrons, and fellow users. In doing so, we discover microscope users from a wide range of social ranks: dilettantes but also professors, and physicians as well as naturalists. New authorities on microscopy linked these customers together. These included naturalists such as Henry Baker and the instrument makers Benjamin Martin and George Adams. They authored textbooks on microscopy and entertained advice from correspondents about improving their microscopes. Above all, they used their networks of letter recipients to sell items for themselves or the craftsmen they sponsored. As we will explore in the next section, correspondences, not shop displays, had become the staple of eighteenth-century microscope advertising.

i. Advertising and marketing

Microscope makers who prospered as correspondents could survive even without a shop. John Cuff, for example, abandoned his business several times following his bankruptcy in 1750 and the loss of his shop in 1757; however, his patron, Henry Baker, kept his business

[56] D. Bryden, *Scottish Scientific Instrument-Makers 1600-1900* (Edinburgh, 1972).

thriving throughout these years through his postal network of friends and clients. Other instrument makers, such as James Short (1710-1768) in Edinburgh, could not afford to sell optical instruments at the lowered price of competing shop owners. Again, patronage and correspondences saved the day: Short was able to stay in business through the help of his patron, the mathematician Colin Maclaurin (1698-1746). Maclaurin, of course, aided Short financially, but equally important, he provided the upstart instrument maker with a network of professor colleagues and other learned gentlemen, many of whom never entered the shop. By the time Short moved to London in 1738, 'such was his reputation that he was able to command twice the price of his contemporaries.'[57]

Even without patronage, microscope-manufacturers relied on new modes of advertising. Advertising microscopes still required considerable effort in the eighteenth century. Instrument makers could outperform their rivals through reputation, shop location, or print. As we will see, the eighteenth century witnessed a changing print culture related to microscopy. In addition to trade cards and simple catalogues, microscope makers issued longer advertisements appealing to learned gentlemen. These stressed the quality and applicability of their instruments as much as their availability as mere show-pieces. Opticians frequently sold these pamphlets in conjunction with their microscopes as informative guidebooks.[58] Some calculating and opportunistic opticians sold their guidebooks separately in the knowledge that their customers required them in order to operate their microscopes.[59] Moreover, popular magazines that catered to gentlemen and naturalists highlighted the diversity of animalcules and their roles in nature, thus prompting further research.[60] By the middle of the century, leading microscope makers and their

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[57] A. Morrison-Low, p.141.
[58] J. Cuff, *The Description of a New-constructed Double Microscope: in which Some Useful Improvements are Introduced: as Made and Sold by the Inventor* (London, 1744); also, H. Baker, *Of Microscopes and the Discoveries Made Thereby: Illustrated with Many Copper Plates* (London, 1753).
[59] E. Nairne, *Description and Use of the Compound Microscope, as Made and Sold by Edward Nairne, at Number 20, in Cornhill, London* (London, 1780).
[60] One such journal, *The Gentleman's Magazine,* continuously published microscopic observations for the casual experimenter. One article concerning microscopic animalcules even contained a refutation of Buffon's

patrons produced detailed instructional manuals replete with discourses upon experiments alongside illustrated catalogues. The most well known of these are Benjamin Martin's 1742 *Micrographia Nova*, George Adams Sr.'s 1746 *Micrographia Illustrata* (Fig. 8), and Baker's *The Microscope Made Easy*.

FIGURE 8: The Variable Microscope of George Adams Sr. A 'universal' style of microscope intended to Serve a variety of purposes. From George Adams, *Micrographia Illustrata* 4th edn. (London, 1771). Also reprinted in J. Millburn, *Adams of Fleet Street*, p.141. The Adams firm would sell similar models into the 19th century. Compare to the later incarnation of the 'universal' microscope in Fig. 5.

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observation that spermatozoa were not living entities. W. Martin, 'Animalcules Microscopically Described From Nature', *Gentleman's Magazine,* 64 (1788), pp.761-2.

These works served a dual role as advertisements and also as serious scholarly texts. Martin and Adams obviously intended their *Micrographias* to convey a connection to Hooke's famed book. Their goal was to highlight the latest microscopic discoveries, while also drawing attention to their own manufactured 'universal' and pocket microscopes.[61] Baker similarly stressed the importance of his microscopes (as sold by Cuff); however, his textbook emphasised sample preparation and experimental sets. Each of these authors illustrated and sold 'variable' or 'universal' microscopes like those illustrated in Fig. 8. As my case studies will later show, these adaptable designs were well suited to the tasks of microscopic anatomy and medicine.

Such publications provided an important opportunity to address the practical and philosophical doubts that had plagued microscopy. Most successful eighteenth-century opticians did not shy from addressing the limitations of their lenses. Benjamin Martin discussed the problems of aberration candidly in his 1759 treatise, *New Elements of Optics*. He described the various types of aberration, dedicating considerably more attention to Newton's problem of chromatic aberration. Martin also suggested methods and designs that might amend these difficulties. Although Martin's ulterior motive may have been to promote microscopes from his own workshop, he nevertheless encouraged readers to asses their instruments with an objective eye.

> Many will conclude with myself that one method is too difficult for practice, and the other too imperfect to deserve much regard from the judicious part of the public.[62]

Therefore, the goal of such treatises was to foster knowledge and establish a rapport with readers as fellow-witnesses and naturalists, not immediately as potential consumers.

For Martin, publishing was a necessity. Although he dedicated most of his life to the instrument trade, his early living depended on textbook sales and his work as an itinerant lecturer. Unfortunately, Martin's textbook on microscopy sold poorly. Contrast

[61] J.R. Millburn, *Benjamin Martin: Author, Instrument-maker, and 'Country Showman'*, p.33.
[62] B. Martin, *New Elements of Optics*, p.viii.

this with Baker, whose text bore the sponsorship of the Royal Society. A first edition of Baker's book sold 1,000 copies within a year followed by a reprint in April, 1743.[63]

George Adams' 1746 book was somewhat more successful, but it could not compete with Baker's book either. Baker attempted to suppress Adams' *Micrographia,* claiming plagiarism, but his most effective weapon was price. As a publisher, Baker better understood the textbook market. He sold *The Microscope Made Easy* at a considerably low price of 5 shillings compared to Adams' 15.[64] This certainly benefited Baker's craftsman John Cuff because *The Microscope Made Easy* exclusively advertised Cuff's models.

There were other benefits of allying with a wealthy naturalist such as Baker. Baker's network promised repeat sales. Clients benefited from this relationship as well. Often the purchase of a microscope through an esteemed naturalist would establish a much-desired personal connection within the Royal Society. Customers could have easily applied to craftsmen directly for the latest microscope models (particularly Adams and Cuff as they heavily advertised their wares in pamphlets and local newspapers); however, a large number of naturalists and physicians instead wrote to their patrons. They did so, even though Baker explicitly mentioned in his book that potential microscope users should consult Cuff at his shop at Fleet Street. Baker's closest correspondents, William Arderon, Henry Miles, and the physician James Mounsey each began their long association with Baker through initial letters requesting microscopes. Baker further obliged them by reviewing and submitting numerous papers they had written for the *Philosophical Transactions.*

In contrast to these elites, the instrument maker's world was one of fierce competition. For those craftsmen who were dependent on their shop, location was key. Microscope makers generally flocked to specific areas where clientele were sure to thrive. As Morrison-Low suggests, London was the hotbed of instrument making in Great Britain

[63] J.R. Millburn, *Adams of Fleet Street,* p.63.
[64] Ibid., p.35.

throughout the eighteenth century, with only marginal manufacturers operating in the provinces. On the other hand, D.J. Bryden has identified key microscope manufacturing centres in Edinburgh.[65] John Millburn more precisely mapped optician shops clustered along Fleet Street and Ludgate just west of Saint Paul's. This would have been an ideal location in which to cater to naturalists; in order to enter the Royal Society on Crane Court, they would have alighted from their carriages on Fleet Street, just in front of the opticians' shops.

On the other hand, such dense competition could prove fatal: in one well-known anecdote, we hear of Benjamin Martin opening a successful shop in 1756 at 171 Fleet Street, next door to the recently bankrupted John Cuff (who owned much smaller premises at 172 Fleet Street). Cuff abandoned the shop within months.[66] However, this did not end his business as Cuff's patronage network continued to thrive.

Another strategy that proved particularly useful was to connect one's shop to the legacy of a great naturalist or other celebrity. John Cuff, for example, could trace his apprenticeship back to James Mann the younger, who claimed to have received instruction from Isaac Newton himself. Thus, when Mann sold his wares in 'The Oldest Shop at the Sign of Sir Isaac Newton'[67], he was projecting a personal bond to natural philosophy. The heads of Tycho Brahe, Archimedes, and other luminaries of science typically appeared on shop signs and trade cards. Fortunate instrument makers such as John Dollond, George Adams, and his sons could advertise their official positions as Instrument Makers to King George III. We also see on Adams' trade card that he advertised himself as 'Mathematical Instrument Maker to his Majesty' as opposed to labelling himself an Optician or Spectacle

[65] D. Bryden, 'Three Edinburgh Microscope Makers: John Filayson, Wiliam Robertson and John Clark' in *The Book of the Old Edinburgh Club*, 33 (Edinburgh, 1972), pp.165-176
[66] J. R. Millburn, *Benjamin Martin: Author, Instrument-Maker, and Country Showman*, pp.108-16.
[67] See H.R. Calvert, *Scientific Trade Cards in the Science Museum Collection* (London, 1971), trade card 'James Mann', catalogue no. 251.

Maker, although spectacles certainly provided much of his income.[68] Less scrupulous instrument makers preyed upon the reputation of their competitors. Thus, after John Dollond began selling his highly profitable achromatic telescopes in 1758, we find the names Doland, Dolland, and Dolound inscribed on instruments not made by Dollond.[69]

Microscope manufacturing was only one aspect of the instrument maker's profession. Makers drew attention to their ability to produce diverse philosophical, mathematical, and optical devices, including barometers, sextants, quadrants, sundials, telescopes, air-pumps, and compasses, and a wide range of other products. Opticians were more focused on lens instruments than mathematical instrument makers, although their products were equally diverse: visual-glasses, spectacles, camera-obscuras, mirrors, and of course, telescopes. In his 1972 article, 'Micrographia Historica,' Gerard L'E. Turner stressed the importance of these other devices that required so much of the instrument maker's time and finance.[70] Clay and Court's influential book *The History of the Microscope* makes no such account of alternative instruments. In doing so, the long history of eighteenth-century microscope manufacturing seems lacklustre. However, in Turner's view, microscope manufacturers were only a few of the competitors in a race to produce new and more affordable instruments of science. Such devices solidified the instrument maker's role as an authority figure in mathematics and natural philosophy. Microscopes were just one of many new 'ideas made brass.'[71]

Turner's critical approach to microscopy centred on the volume of instruments produced and their slight variations. In conducting studies on a particular instrument, such as James Short's reflecting telescopes, Turner uncovered 112 such instruments; however,

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[68] Ibid, 'George Adams' catalogue no. 6.
[69] A.D. Andrews, pp.148-63.
[70] G.L'E. Turner, *Micrographia Historica*. Also, G.L'E. Turner, *Scientific Instruments and Experimental Philosophy 1550-1850* (Aldershot, 1990).
[71] G. L'E. Turner, *Micrographia Historica*, p.5.

he also found numerous copies, non-functional showpiece models, and many variations in the design. All of these exemplify the complexities of the microscope makers trade.

As we can see, eighteenth-century instrument-making flourished in a competitive market. However, rivalries necessitated enormous effort among makers to maintain their patent rights or keep trade secrets hidden. H.C. King narrates the interesting story about the secret development of the achromatic lens. [72] When Chester Moore Hall first developed his doublet around 1730, he contracted two leading opticians, Edward Scarlett and James Mann, to grind each of the two doublet components separately: the flint glass and crown glass lenses. Unbeknownst to one another, each of these opticians subcontracted their work to another craftsman, George Bass, who soon enough concluded that the lenses were for the same customer and that they would fit together into an achromatic doublet. Although Bass completed the project for Hall, it appears that he told Dollond about the invention much later in the early 1750s, just as Dollond was trying to resolve Newton's problem of achromaticity. Clearly, Dollond's design was not unique. Although Hall did not contest his priority, the Dollond business suffered constant patent infringements and counter priority claims.[73]

John Dollond's son Peter ultimately succeeded in upholding his family's patent; however this was the exception not the rule for eighteenth-century disputes among opticians. Even the highly-successful Dollonds could not deter foreign manufacturers from copying the achromatic lens design. Claude Passement, the opportunistic optician to the

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[72] H.C. King, *History of the Telescope* (London, 1955), ch.8.

[73] This priority debate would outlive any of the actors involved. John Dollond's son Peter continuosly defended his patent right to the achromatic lens after his fathers death in 1761. He also submitted a report (read before the Royal Society in May 1789) to defend John Dollond's intellectual contribution to the discovery of achromats. One opponent of Dollond's came came from Peter Dollond's own brother in-law Jesse Ramsden, who quickly issued a counterclaim to the Royal Society (June, 1789) and one published in the popular *Gentleman's Magazine* of 30 September 1790 (890-891). Recently, Jim Bennett has drawn attention to Peter Dollond's rebuttal of November 1789 (the original copy now held in the Museum of the History of Science at Oxford), in which Peter acknowledged a cordial meeting between his father and Chester Moor Hall in 1758. Peter Dollond also reaffirmed his father's intellectual as well as technical contribution. See J.A. Bennett, 'Peter Dollond Answers Jesse Ramsden', *Sphaera: The Newsletter of the Museum of the History of Science, Oxford,* 8 (Autumn, 1998). For more on the Dollond family see R. Sorrenson, 'Dollond & Son's Pursuit of Achromaticity 1758-1789', *History of Science,* 39 (2001), pp.31-55.

court of Louis XV, reduced Dollond's invention to a more portable hand-held design that he presented to the king as his own discovery.[74]

For our purpose, this tumultuous history of innovation provides a wonderful glimpse into the complexity of eighteenth-century instrument manufacturing. It was precisely because of the web of out-putting and sub-contracting (combined with the constant struggle for secrecy) that the achromatic lens developed as it did. The obvious gain prompted John Dollond to perfect his design; whereas, the need for opticians to output their workload led to Hall's failure to maintain secrecy and ultimately Dollond's success.

ii. Naturalists as patrons

Naturalists such as the Duc de Chaulness or Henry Baker evaded much of this secrecy by acting as patrons to instrument makers. As patrons, they would be among the first to know about a new design or manufacturing technique. Conversely, an instrument maker might have some advantage in withholding information from his patron. He may seek more funding before revealing progress or he might be motivated to preserve trade secrets. Increasingly, it was the patron who designed new features. The craftsmen would duly implement new designs with some modification. In this way, the creative control of microscope designs vacillated between makers and patrons.

Naturalists who acted as craft patrons encountered serious risks as well. Such a patron might lose the race to patent a design or claim priority. The patent-owner was often the craftsman who first constructed the instrument, not the user who first dreamt of the idea. It was the craftsman, after all, who gained the most from controlling the sale of the

[74] M. von Rohr, 'Contributions to the History of English Opticians in the First Half of the Nineteenth Century (With Special Reference to Spectacle History)', *Transactions of the Optical Society*, 28, no. 3 (1927), p.124. Von Rohr has also found that Swiss optician Pierre Louis Guinand (1748-1824) of Neuchâtel quickly copied the design that same year, as did Viennese opticians later in the eighteenth-century. Historian Moritz von Rohr himself was a brilliant optician who contributed to aspheric lens designs in the early twentieth century. See M. von Rohr, *Geometrical Investigation of the Formation of Images in Optical Instruments* (London, 1920).

microscope. The craftsman's claims were bolstered in 1711 when the law offices of the Crown began requiring detailed and operational schematics to be incorporated in patent applications, rather than simply written notions. However, the claims of priority remained much in dispute. Patent claims were a notorious point of difficulty in Britain throughout the eighteenth century.

In contrast to Britain, courts on the continent tended to favour designers over craftsmen. Such was the case in France. The Maison du Roi officially issued all *lettres patentes* until the Republic; however, the Académie des Sciences retained a monopoly on the inspection of patent applications and the issuing of patents for scientific instruments. Furthermore, patent applications were expensive, often exceeding 1,000 francs: far beyond the expenditure of the common artisan.

On the whole, patron naturalists had the advantage over craftsmen in claiming patent rights and priority. One instrument illustrates this point well: the dividing engines of the Duc de Chaulnes (1714-1769) and Jesse Ramsden (1735-1800). The Duc de Chaulnes designed a number of scientific instruments, including two enhancements to the dividing engine in the 1760s. The machine in question was a vital tool for accurately dividing and marking units of measurement on navigational, astronomical and other geometric-based equipment. The Duc de Chaulnes was the first designer to add a compound microscope to the point of measurement, thus ensuring microscopic precision. Meanwhile the Englishman Jesse Ramsden began designing a more accurate engine for the Board of Longitude. Ramsden's device was capable of achieving gradations up to one two-thousandths part of an inch. However, Ramsden had to invest £250 of his own money into the project and the Board spent a year deliberating before paying Ramsden £615 for his work.[75]

In France, leading opticians benefited from somewhat more stable alliances with the Académie des Sciences. Alexis Magny (1712-1777) was a court optician and frequently

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[75] See Anita McConnell's chapter, 'The Dividing Engines' in A. McConnell, *Jesse Ramsden (1735-1800): London's Leading Scientific Instrument Maker* (Aldershot, 2007), pp.39-52.

collaborated with Académie members including the Duc de Chaulnes and René Réaumur. His first patron was a physician: Joseph Bonnier de la Mosson (1702-1744). After Mosson's death, Magny solicited the backing of de Chaulnes, Mosson's brother-in-law. This was a patron-manufacturing relationship that had enormous benefits for de Chaulnes and his colleagues in the Académie as well as Magny. While we have seen that British instrument makers such as Adams and Cuff were constantly engaged in advertising, Magny, like most French instrument makers, had less need of public adverts. Magny did however engage in self-promotion through academic channels. Announcements of his microscope inventions appear in the *Journal Économique* and the *Memoires pour l'Histoire des Sciences et des Beaux Arts.*[76] Ratcliff has spent considerable effort comparing the British and French practices of microscope-advertising. He concludes that, in France, 'a tacit rule urged scholars to avoid quoting makers and created an invisible constraint for microscope makers.'[77]

However, with great benefit also came great responsibility. The craftsman's business conduct reflected on the character of his patron. This was problematic for Henry Baker, who frequently needed to defend John Cuff's skills. Cuff, meanwhile, had difficulty fulfilling orders, much to his patron's consternation. In one incident, a customer, Stephen Demainbray (1710-1782), required a set of microscopes to conduct some medical experiments, but Cuff failed to send them to Demainbray in Dublin in a timely manner. Demainbray wrote an angry letter to Baker months later; he was furious that Cuff had left him to wait 'a whole winter which hindered the doctor and myself from pursuing our trials as we expected he would send them at every return of the stage coach ... and therefore, we

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[76] 'Article 'LXVIL', *Memoires pour l'histoire des sciences et des beaux arts*, 3 (June, 1752).
[77] M. Ratcliff, 'Testing Microscopes Between Market and Scientific Societies', in Generali, D. and M. Ratcliff, (eds.), *From Makers to Users: Microscopes, Markets, and Scientific Practices in the Seventeenth and Eighteenth Centuries* (Milan, 2007), p.138. This is not to say that microscope makers were less prolific on the continent. As we shall see in later chapters, French anatomists and physicians made great advances using microscopes manufactured in Paris in addition to those purchased from London. On the other hand, competition was less severe among the French instrument makers.

would not write to any other workman.'[78] To make matters worse, Cuff did eventually send a letter to Demaibray, cancelling the order, but Demaibray was required to pay the postage from London in order to receive his refund. The interesting thing about this incident is that Demaibray let the burden fall on Baker, not Cuff, for the mishap. 'I should be very unhappy were you to harbour the least doubt of my behaving with severity to any tradesman,' he told Baker.[79]

Patrons also witnessed their craftsmen engaged in bitter disputes which they were powerless to resolve. Baker's craftsman John Cuff bitterly detested one of Colin Maclaurin's microscope makers, Robert Finlayson. In a 1743 letter to Baker, Maclaurin describes how Finlayson came down from Edinburgh, ostensibly to study under Cuff and learn new methods of designing microscopes.

> [Finlayson] entered into an Agreement with Mr Cuff, for selling his microscopes at London, and got Instructions here how to improve and fit them up in a useful Manner. He left 3 or 4 Unfinish'd with Mr. Cuff and was to send the Apparatus for them on his return to Edinburgh, upon the promise of which he borrowed Money and took up goods of Mr Cuff, but has never perfected the Microscopes nor returned him any Money. [80]

In his reply Baker hinted that his Edinburgh colleague should not reward such machinations, but should instead purchase better quality microscopes from Cuff in London. As Cuff's patron, Baker knew that Cuff's instruments were more expensive.

> I should be glad to know what reputation [Finlayson] bears at Edinburgh, if you have any opportunity of enquiring, and if he goes on to sell his Microscopes there and for what Price.[81]

Baker goes on to stress the importance of quality over price: the major reason why Cuff remained in business as long as he did despite threats from Finlayson and other opticians.

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[78] JRL, GB 133 MS 19 (Henry Baker Correspondence), vol. 5, p.99: Stephen Demaibray to Henry Baker, 12 Nov. 1751.
[79] Ibid.
[80] JRL, GB 133 MS 19 (Henry Baker Correspondence), vol. 1: Henry Baker to Robert Blair, 27 Oct. 1743.
[81] Ibid.

As a patron naturalist, Henry Baker often found himself in a two-front war with his critics: he needed to promote and sustain his craftsman's instruments while also defending his own microscopic techniques and observations. For example, in 1750, Baker defended the effectiveness of the solar microscope with his frequent correspondent, Miles. Miles complained that he could not see the anatomical features of the rotifers that Baker had famously described; and, moreover, Miles had great difficulty observing the animals through the unwieldy solar microscope that Cuff and Baker had sold him.

> Mr Cuff has made me a slider of two plates of glass kept asunder with thin pieces of wood, but these leave so much room for the animals to play about that you may wait halfaday and never see it. Upon the whole (for I perceive I am got into conversation) I must ingenuously confess, that I am not without my suspicion of a deception in this matter.[82]

Baker defended his own observations, but could say little about the microscope in question. He had noted similar observational difficulties in the *Microscope Made Easy,* and could only plead that Cuff's 'universal' instruments provided an equitable service for the price. To Miles' suggestions that Baker could improve upon Cuff's microscope, Baker responded, 'The machine you propose would be of singular serve was it profitable to make it small enough, which I am afraid no workman can do.'[83]

Inventers such as Cuff and Baker were not above altering their designs. Cuff, like many of his contemporaries, made a habit of procuring advice about his instruments, then implementing changes without crediting the source. Fournier and Ratcliff uncovered such an incident involving Cuff and Abraham Trembley's microscope for observing aquatic animals. Following his celebrated research on the polyp, Trembley had been in London in 1745 to acquire instruments for himself, his colleagues on the continent, and his own patron William Bentinck. He engaged Cuff to build a microscope (which Trembley designed) featuring a moveable eyepiece (known as a Porte-loupe) for viewing aquatic animals.

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[82] JRL, GB 133 MS 19 (Henry Baker Correspondence), vol. 3, p.133: Henry Miles to Henry Baker, 31 Aug, 1747.

[83] JRL, GB 133 MS 19 (Henry Baker Correspondence), vol. 3, p.134: Henry Baker to Henry Miles, 3 Sep. 1747.

Unfortunately, Trembley did not acquire exclusive rights to his Porte-loupe. Around the same time as Miles' letter to Baker, Cuff began selling his own version of the microscope now called the aquatic microscope. Other craftsmen quickly copied the design without acknowledging Trembley including George Adams II and Jan Paauw in Holland.[84]

The story does not end there. During Abraham's investigations on the regeneration of the polyp early in the 1740s, he sent freshwater polyp samples to Baker, Martin Folkes, and other naturalists across Western Europe. According to Ratcliff, Baker 'effectively plagiarized' Trembley's research on the Polyp.[85] Baker had attended Folkes' demonstrations of the Polyps before the Royal Society and set out to repeat Trembley's experiments on their regenerative capabilities. Baker then scrambled to publish his own account, 'An Attempts towards a Natural History of the Polype' (1743) before Trembley's study appeared the following year: *Mémoires pour Servir à l'Histoire d'un Genre de Polypes d'Eau Douce.* Not surprisingly, both Baker and Cuff come across as rather mischievous, if we examine their careers through their interactions with Trembley.

Baker's personal papers reveals that he was fervently defensive of his own achievements, frequently accusing rivals, such as George Adams with plagiarism. Baker was also quick to divert threats away from his own craftsmen. In June 1745, a watchmaker from Norwich named Thomas Page invented a new focus mechanism for the simple pocket microscope. This single-lens microscope was similar to a design produced by George Lindsay, a supplier of Baker. Page's design featured a screw-pinion to focus the lens, rather than a sliding focus bar. The obvious benefit of this design was that the lens would be kept in place by the pinion, whereas a screwing or sliding focus mechanism (such as those produced by Lindsay and also Cuff) frequently slips out of focus. Page wrote, 'I have contrived a different method for bringing the object to focus than any that have yet been in

[84] M. Ratcliff and Marian Fournier have dedicated a chapter to this incident: 'Abraham Trembley's Impact on the Construction of Microscopes' in D. Generali and M. Ratcliff, (eds.), pp.91-112.
[85] Ratcliff, *Quest for the Invisible*, 109. See also, Ratcliff, 'Abraham Trembley's Strategy of Generosity and the Scope of Celebrity in the Mid-Eighteenth Century', *Isis*, 95, no. 4 (2004), pp.555-5.

use of by tooth and pinion'.[86] Page solicited Baker's help in producing this microscope. He asked William Arderon, Baker's most frequent correspondent, to endorse his design and show Baker his microscope. Unfortunately for Thomas Page, Arderon was awaiting word on his candidacy for the Royal Society. As Baker was his major sponsor at the society, he risked nothing that would jeopardise his ally. Arderon wrote to Baker, recommending that he suppress the design: 'I acquaint you with this, for fear he should write to you in my name about it.'[87] Baker quickly sent a devastating reply to Page, warning him that the microscope too closely resembled that of Lindsay. This episode illustrates how personal ties and social conventions dictated technological advancement. Nevertheless, Lindsay's own model was not particularly novel, it had apparently been in use for several decades and another of Baker's instrument makers, Mathew Elliot, had also manufactured a similar microscope.

> I should act unkindly did I not warn you of the dangers. Mr. Elliot . . . has lately made a microscope, on the same plan, with glasses set nearly as yours (which was surely used 40 years ago) . . . you may however entertain yourself and your friends very softly with your own ingenious contrivances.[88]

Such restrictions certainly did not apply to Baker's more favoured craftsmen, such as Cuff.

When Cuff declared bankruptcy in 1750, Baker settled a debt of £20 to keep Cuff out of prison. For the wealthy Baker, this was a simple task; more problematic was restoring Cuff's reputation and maintaining his pool of clients. Even William Arderon considered suspending his payment for a set of microscopes during Cuff's bankruptcy proceeding. However, Baker's good name kept Cuff afloat, and Arderon kept his payments flowing, provided Cuff would continue to manufacture Arderon's microscope as promised.

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[86] JRL, GB 133 MS 19 (Henry Baker Correspondence), vol. 1: Thomas Page to Henry Baker, 22 June 1745. Norwich watch and clockmaker Thomas Page appears on the register of G.H. Baillie's antique guide *Watchmakers and Clockmakers of the World*, 4th edn. (London, 2008), p.241.

[87] JRL, GB 133 MS 19 (Henry Baker Correspondence), vol. 1: William Arderon to Henry Baker, 31 May 1745. For more on Arderon see BL Add MSS 4439, 27966 (Arderon Papers).

[88] JRL, GB 133 MS 19 (Henry Baker Correspondence), vol. 1: Henry Baker to Thomas Page, 9, July, 1745.

Arderon wrote to Baker, 'I have it in my power to serve him [Cuff] very much, and I will do it for your sake, as long as he eyes me handsomely.'[89]

Cuff's more loyal clients still aided his work. He continued to sell to physicians and naturalists on the continent long after declaring bankruptcy. Unfortunately, Martin's competition next door proved too much and Cuff was once again destitute by 1757. However, again a solid reputation could allow an instrument maker to continue his business, even without a proper shop. Arderon, for example, recorded in 1757 that Martin's microscopes would be popular, but by no means comparable to those of Cuff.

> I know Martin very well and believe he never can do Mr. Cuff any damage with men of Judgment and Taste, as his instruments bear no comparison either in neatness or goodness, yet he certainly hath done well to remove out of his neighbourhood, for there are many, if they can but get things cheap, they pay no regard to their correctness or beauty.[90]

Cuff's case illustrates that innovation and networking skills did not ensure financial gain. On the other hand, very few microscope manufacturers were free from financial difficulty. Benjamin Martin (1704-1782), who effectively had driven Cuff out of business in 1757, was himself declared bankruptcy in 1782. Unlike Cuff, Martin did not have a stable patron to resurrect his business. Martin attempted suicide that same year, and died a few weeks later.[91]

2.4. Conclusions

This chapter has focused on the physical and social problems that may have pushed early eighteenth-century microscopy into decline. Nevertheless, through an examination of manufacturing culture and personal networks, we have found that the microscope maker's

[89] JRL, GB 133 MS 19 (Henry Baker Correspondence), vol. 4, p.331: William Arderon to Henry Baker, 17 Dec. 1750.
[90] JRL, GB 133 MS 19 (Henry Baker Correspondence), vol. 6: William Arderon to Henry Baker, 25 Jul. 1757. The bankruptcy of one craftsman could be a boon for another. For example, the London firm W & S Jone's were not craftsmen per se but instrument suppliers who profited from the collapse of smaller instrument shops. When George Adams the younger died in 1795 W&S Jones bought the copyright of Adams' scientific and mathematical textbooks. Similarly, Benjamin Martin's 1782 bankruptcy might have bolstered the Jones Business as they purchased much of his unsold merchandise.
[91] J. R. Millburn, *Benjamin Martin: Author, Instrument-maker, and 'Country Showman',* p.173.

profession began to take on new shape in this century. Their clientele was not limited to dilettantes; it broadened to diverse ranks. Moreover, intense competition among new craftsmen required skilled advertisements. The most successful microscope makers appealed to their customers in a subtle manner: fashioning themselves as fellow experimenters not as tradesmen for hire. Patrons played a crucial role in the development of these advertisement schemes and sales as well as the innovations behind many eighteenth-century optical instruments. Of course, opticians had long-since depended on more prosperous gentlemen to supplement their income and keep production continuous despite fluctuations in demand; however, by the early eighteenth century, naturalists began occupying these roles, thus lending their effort to aid both microscopic research and the market for microscopes.

Chapter 3. Medical Applications of Microscopy and the Early Eighteenth Century

As shown in the previous chapter, Enlightenment-era physicians and anatomists had a number of innovative microscope designs at their disposal. Not surprisingly, craftsmen and their patrons highlighted medical and scientific uses for the microscope regardless of their actual practicality. So what were the perspectives of their clients? As we will see in later case study chapters, anatomists and physicians found a number of uses for the microscope in surveying anatomical features and studying bodily functions. In contrast, the application of the microscope to pathology is less visible before the advent of germ theory in the nineteenth century. Did physicians find a use for the microscope in the study of disease and pathology before this time? The answer is not straightforward.

The history of microscopy is a rocky and perilous field. There are large gaps in the historical record. One such area relates to the impact of the new instrument upon pre-existing contagion theory. A number of early physicians and naturalists recognised the significance of contagious particles and animalcules, and they employed the microscope to study these in the late seventeenth and early eighteenth centuries. In most cases, microscope users did not propose alternative theories to miasmatic and humoural notions of disease; more often than not, they adapted such theories to conform to their microscope observations. This is not to say that the notion of microscopic contagious particles or pathogenic living organisms was non-existent; rather, many different contagion theories arose in response to epidemics such as plague, some of which employed microscopic findings. Indeed, as this chapter will show, microscopic studies upon contagious particles and organisms were widely publicised long before the advent of germ theory.

However, a word of caution: we must avoid teleology. Although traditional historians have viewed early theorists as 'neglected precursors' of the bacteriological revolution,[1] the microscopic contagious particles that early authors described are not synonymous with our modern notion of germs. The germ theory of the late nineteenth century may have utilised similar nomenclature; nevertheless, the animalcules, 'germs', and contagious particles discussed in this chapter do not correlate to our modern identification of viruses or bacteria.

3.1. The background: early theories of contagious animalcules

Any scholar well versed in classical medical texts would have encountered some form of disease theory relating to direct contagion or dangerous miasmas well before the Enlightenment. Ancient authors, such as Marcus Varro, introduced a miasmatic theory of respiratory illnesses stemming from particles in the air near swamps. Avicenna argued that aerosolised secretions from the body formed the etiological origins of tuberculosis. The humoural theories prevalent in the early modern period and the Renaissance did not eschew these theories of miasmas or contagionism; instead, Renaissance physicians typically codified these under Galenist models of contagious bilious imbalance. There are exceptions: Girolamo Fracastoro (1478-1553) introduced new terminology to the study of contagion. Fracastoro's terminology is important to note as it would alter the path of future microscopic research. Therefore, an assessment of early theories of contagion is necessary before a discussion of microscopic medicine can take place.

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[1] R. Williamson, 'The Germ Theory of Disease: Neglected Precursors of Louis Pasteur (Richard Bradley, Benjamin Martin, Jean-Baptiste Goiffon)', *Annals of Science*, 11 (1955), pp.44–57

Although hardly a precedent for our modern microbial theory, the 'fomite theory' of Fracastoro was a noteworthy divergence from preceding Galenism and humouralism. Historian Vivian Nutton writes: 'The dead hand of Galenism is seen as stifling whatever originality there may have been among doctors', but 'to this general disparagement of sixteenth-century physicians and their theories, there has been one major exception, the Veronese doctor and poet Girolamo Fracastoro'.[2] Fracastoro's 'fomites' were far more than mere spores in Fracastoro's mind. Rather, the Veronese physician put forward three potential avenues of contagion: direct, indirect, and vectored. Indirect and direct forms of contagion signify the miasmatic from the non-miasmatic causes of disease, and Fracastoro dedicates several chapters of *De contagione* to each. However, his third mode of contagion – vector-born contagion- is the most relevant to this present study. Fracastoro calls these particles *seminaria contagionis*: the 'seeds' of contagion.[3] They are minute fomites which inhabited diseased objects such as linen, clothing, and bedding, and possibly other living beings, which then relayed disease directly to a host. To infiltrate various parts of the human body, Fracastoro speculated that these seeds differed in shape and firmness. One could study the symptoms of disease to learn more about the nature of fomites. Syphilis seeds would be the hardest of all contagious fomites because that disease seems to penetrate into the deepest regions of the body. Furthermore, other seeds would need to be as light as air in order to be inhaled and cause respiratory symptoms. Fomites also appeared specific to organs: Fracastoro knew, for example, that tuberculosis did not affect the body uniformly, but left the eyes and mind

[2] V. Nutton, 'The Reception of Fracastoro's Theory of Contagion: The Seed That Fell among Thorns?', *Osiris*, 2nd series, 6 (1992), p.197.
[3] G. Fracastoro, *De contagione et contagiosis morbis et eorum curatione* (Venice, 1546), pp.144-5; [Magdalen College Old Library, Oxford University. Q.1.33].

unscathed. Remarkably, he also theorized that certain seeds travelled along the paths of veins where they could attack some organs, but not others.[4]

> They attack and enter from the small pores, veins and arteries into the larger, and from these to others, and often reaching even to the heart.[5]

Then another insight into propagation:

> The original seeds which have adhered to the neighbouring and corresponding humours, generate and propagate others exactly like themselves, and these in turn propagate others, until the whole mass and bulk of humours is infected.[6]

Fracastoro's theory was innovative in its classification of fomite seeds in correlation to specific illnesses. In addition, Fracastoro frequently deferred to material causes for natural phenomena. He theorised, for example, that fossils must be the material remnants of long-deceased animals rather than divine signs or supernaturally-crafted stones (an insight celebrated by Charles Lyell centuries later[7]). His contagionism operated through direct cause and effect, with material 'fomites' physically obstructing bodily functions. However, as Vivian Nutton has found, Fracastoro's etiology was nevertheless based in the metaphysical world. The fomites were not living entities (animalcules) but material elements which first took their properties from sidereal astrology before they inflicted material damage.[8] Fracastoro also studied climatic affects. He found that syphilis first flourished in the warm climes of the Iberian Peninsula; therefore, the warmth of the sun in those regions nourished the virility of the seeds.[9] According to his theory, the *seminaria contagionis* are an amalgamation of elements that come together or 'activate' when heated in various climates or by the heat of the body

[4] Ibid., p.145.
[5] Ibid.
[6] Ibid.
[7] C. Lyell, *Principles of Geology,* 6th edn. (3 vols., 1840), i, pp.34-5.
[8] G. Fracastoro., pp.144-56, See also, V. Nutton, p.201.
[9] V. Nutton, p.202

itself.[10] Therefore, these are not self-propagating life-forms. We must also recall that Fracastoro was beholden to the humoural theory of disease in his model of contagion. His seeds were the catalysts of illness, not the carriers. Once present in the body, Fracastoro's seeds found the centres of humoural balance and began to manipulate them, causing excesses in bile or humoural imbalance, much like Galenic poisons.[11]

Although contemporaneous physicians such as Vesalius may be more renowned due to their highly visible and well-crafted texts and engravings, Fracastoro did impart a lasting impression on the medical establishment because of this disease theory.[12] He prompted further research into the theory that contagion operated through three modes of contact and contagious fomites were the seeds of imbalance. This, as Nutton explains, was not counter-Galenic. 'It was attractive, for it enabled Fracastoro's formulations of the insights and practices of others to be incorporated easily within traditional guidelines.'[13] Thus, perhaps paradoxically, the bare origins of contagion theory are imbedded in the classical humoural theories of medicine.

How then did the microscope affect this tradition? Few records remain to provide clues; however, if any name has been associated with the initial development of microscope-based medicine, it is Athanasius Kircher (1602-1680). Although Kircher did not document the instruments that he used, he did keep detailed records of his experimental method, published in 1658 as

[10] G. Fracastoro, p.146.
[11] Ibid., p.34.
[12] C. Singer and D. Singer, 'The Scientific Position of Girolamo Fracastoro (1478? - 1553) with Especial Reference to the Source, Character and Influence of His Theory of Infection', *Annals of Medical History*, 1 (1917), pp.1-34. More specifically, the Singers noted that Thomas Jordan's *Pestis phaenomena* (Frankfurt, 1576) owed a particular debt to Fracastoro's contagion theory.
[13] V. Nutton, p.233.

Scrutinium physico-medicum contagiosae luis. Using the microscope, Kircher attempted to classify animalcules systematically.

The first experiment saw Kircher expose a piece of meat overnight in the open air. He then observed the meat with a microscope that had been 'carefully manufactured by a thorough and practiced hand and which can represent objects a thousand times greater than they really are'.[14] Kircher wrote that he witnessed 'countless little worms' (*innumerabiles vermiculos*) in the putrefied meat. Curiously, he attributed them to the influence of the moon; his pieces of meat having been exposed to moonlight overnight.[15] Kircher states that he found similar worms in milk, cheese, and vinegar that had lain exposed overnight.[16] In a second experiment, he dissected a snake, exposed its remains to the sun for several days, and then buried it for a day. Disinterring the snake meat, he witnessed 'a mass of swarming little snakes of such multitude that no one has a sharp enough eye to count them'.[17]

Kircher found similar organisms on the surface of leaves, in plants, and in the decaying matter of horses, humans, and oxen. Like his younger contemporary Hooke, Kircher recorded his own wonderment when observing the microscopic world teeming with life; however, unlike Hooke, Kircher was not concerned with presenting close evidence of this life. Unfortunately, there is not a single illustration of his microscopic observations. In fact, Kircher was less concerned with recording the actual presence and proportions of microscopic life, and more focused on the arguments for the infusion of such life. In opposition to this, Robert Hooke's detailed engravings in the *Micrographia* were themselves an

[14] A. Kircher, *Scrutinium physico-medicum contagiosae luis* (Rome, 1658), p.43; [British Library. 1197.k.22].
[15] Ibid., p.42.
[16] Ibid.
[17] Ibid.

argument for the existence of microscopic corpuscles in matter. To sustain his corpuscularian philosophy, Hooke utilised accurate and beautiful illustrations which emphasized the detail of microscopic life and the uniformity found in certain natural patterns such as mineral crystals or cork cells. Far from this, Kircher's natural philosophy is more biological and medical than it is mechanical. While Hooke introduced his readers to a new material world, Kircher employed this world to re-evaluate the already-known subjects of putrefaction and spontaneous generation. Kircher attempted to reveal how putrefaction introduced biological as well as chemical changes; notably, that decomposing animals spawned new life forms.

But what are the mechanisms of this spontaneous generation of animalcules? The presence of diverse animals in putrefied meats enthralled Kircher, as did the particular and dissimilar characteristics of these animals. These were not simple worms, but unique life forms which were dependent on the decay of specific plants and animals for their own generation. 'Every living animal produces from its own decay some congruous animal that is different from all others'.[18]

In his final experiments, Kircher ascertained that the flesh of dead horses and oxen will give birth to bees and flies that 'give great annoyance by sucking the blood of the animals that gave them life'.[19] Similarly, human beings manufacture bedbugs and mites while sleeping. Take note of Kircher's attention to orderliness and balance within this new natural production: through putrefaction and decay, animals create animals that are either related or unrelated; when they are not, these animals nevertheless interact with the original species of living animals. Kircher found that these relationships are often

[18] Ibid., p.46.
[19] Ibid., p.47.

parasitic (as in the case of the horses and flies) but may be beneficial: it was his assertion that bedbugs and lice assist humans because, like leeches, they remove corrupted blood from the body.[20]

To a modern microscoper, the presence of infusoria in even the most generic of matter is not unsettling. What is interesting is Kircher's capacity to situate these animalcules within a greater notion of natural balance: snakes beget snakes, humans generate their own curatives, and horses their own lifelong source of annoyance. Kircher also surpassed Hooke and Leeuwenhoek in drawing medical knowledge from his microscope observations. For that reason we need now to look at the medical aspects of Kircher's natural philosophy.

Kircher insisted that maladies arose from putrefaction and fermentation. One could avoid maladies through proper ventilation and, in times of plague, the open air of the countryside. Kircher's *Scrutinium* is significant because of the microscopical method Kircher used to deduce treatments. In a post-mortem examination of several plague victims, Kircher reported that he had found microscopical worms in the buboes of plague bodies. As we have seen from the experiments described above, these worms were at the heart of Kircher's continual investigation. He believed that they did not directly cause disease; instead they were the *signs* of disease; the worms having arisen from the same putrefaction evident in rotting meat. One must, therefore. avoid putrefaction at all cost.

Kircher's microscopic evidence of worms led him to judge that these creatures filled the effluvia, the air itself, with deadly disease. Air, he argued, was full of corpuscles that had been exhaled from the putrefaction and decay of

[20] Ibid.

animals. Ventilation and the avoidance of porous substances was the only means of evading this worm-causing corpuscles.

> Obviously, this foul and virulent putridity ... is the only genuine cause of contagion since it brings about the corpuscles ... they are transformed into a brood of countless invisible worms ... they penetrate any porous material and there they bring about new seeds of contagion.[21]

Thus, armed with his microscope, Kircher had arrived at a similar conclusion as Fracastoro, although Kircher's theory rested on animalcules rather than seed-like fomites. And he was not alone. August Hauptman, a friend of Kircher, argued in his *Epistola Praeliminaris* of 1650, that ‘Animalcule worms and mites swim in the discharges and humours of measles, scabies, and smallpox’.[22] During the course of the seventeenth century, such theories of contagionism thrived, strengthened from renewed outbreaks of the plague and the interest of physicians in microscopy. Moreover, Kircher's text is one of the first examples of *animate* contagion theory, that is, the theory of contagious micro-organisms.

Modern scholars differ in their assessment of Kircher and Fracastoro’s importance in the history of medicine. Early twentieth-century historians of science (still feeling the reverberations of the bacteriological revolution) first drew attention to these figures as anticipants of Pasteur and Koch’s microbiological ideas. This discussion formed a special issue of the 1910 journal *Science*. Unfortunately, many of these early studies take a teleological approach to modern bacteriology in their assessment of Renaissance microscopic study. Historians such as William Riley and Fielding Garrison saw in Fracastoro and

[21] Ibid., pp.47-8.
[22] A. Hauptmann, *Epistola praeliminaris: tractatui de viva mortis imagine mox edendo sacrata* (Frankfurt, 1650), p.196 quoted in Wilson, *The Invisible World,* p.155.

Kircher the development of modern science and the shrouding of the archaic Hippocratic doctrines.[23] As Garrison wrote:

> Fracastorius shows himself to be a highly original thinker ... we find him towering above his contemporaries. He seems, by some remark-able power of divination or clairvoyance, to have seen morbid processes in terms of bacteriology.[24]

Early historians of medicine acclaimed Kircher's theories of contagion in a similar manner; they were thus faced with the task of explaining why these theories of microscopic contagion did not alter medical practice. According to historian H.B. Torrey, Kircher's microscopic practice was not convincing to contemporaneous physicians. According to Torrey, Kircher's ideas on contagion were the separating point between proto-science and modern science, but they were unique to him and few others.[25] More recent scholarship has abandoned the topic of modernity entirely. Wilson has argued that modern historians must abandon efforts to isolate the origins of scientific and medical advances. Yet in doing so, we must still remain aware of the theoretical influences of these early natural philosophers despite their relative obscurity in comparison to better-known microscope users such as Leeuwenhoek and Hooke, and Malpighi. After all, 'they were not regarded as non-participants in some enterprise that excluded them and all their ilk'.[26] Wilson and Nutton call for an embracive history of science. In their revision, Fracastoro and Kircher are on the same level as other 'greats' in the history of science precisely because they are part and parcel of the scientific momentum of their day, not exceptions to it.

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[23] W. Riley, 'Kircher and the Germ Theory of Disease', *Science,* 31, no. 800 (1910), p.666. F. H. Garrison 'Fracastorius, Athanasius Kircher and the Germ Theory of Disease', *Science*, 31, no. 796 (1910), pp.500-2.
[24] Ibid, p.501.
[25] H.B. Torrey, 'Athanasius Kircher and the Progress of Medicine', *Osiris,* 5 (1938), p.260.
[26] C. Wilson, *The Invisible World*, p.34.

I contend that it is not an issue whether or not past medical communities were incredulous towards these views. What is of greater importance is the systems of knowledge which these early theorists introduced and the epistemological approaches which compelled them. In the case of Fracastoro, there is no record that he used anything more than a simple magnifying lens in his study of the micro-material world. Nevertheless, he expanded the theory that microscopic seed impelled contagious diseases. More specifically, Fracastoro imprinted the notion that contagion was not merely seed-based, but that different categories of fomites (of real material structure) caused specific signs of disease. As a result, Fracastoro redefined the body's means of acquiring disease. In addition, Fracastoro's study of fomites and seeds subtly transformed the language of medical discourse. Similarly, Kircher's later microscopic research altered the language of medical analysis. Although traditional histories of western medicine have highlighted the influence of morbid anatomists such as Harvey, we must also examine contemporaneous microscope users and their influence upon medicine. Although they were less renowned in their day, lesser-known practitioners slowly progressed medicine away from the macro and humoural toward the micro and material.

3.2. The decline of medical microscopy during the eighteenth century?

How did these early theories of microscopic contagions affect medicine during the eighteenth century? Again, there is little scholarship on this topic. Although Ratcliff's study of eighteenth-century microscopes revolves around his thesis that the microscopy did not decline in the eighteenth-century, his immediate interest is not the history of medicine but natural history.[27] Wilson offers a tantalizing

[27] M. Ratcliff, *The Quest for the Invisible.*

summary of the microscope's influence upon a number of subjects: generation theories, preformationism, philosophy, religion, and disease theory, but she is resolute that eighteenth-century microscopes were ineffective as *medical* instruments. According to Wilson, the discovery of living animalcules hindered rather than helped medical knowledge. Although microscope observations reinforced the theory that animalcules caused disease, this 'theory of animate contagion survived in the eighteenth-century medical literature without being particularly cherished'.[28] Furthermore, Wilson finds that eighteenth-century physicians remained ambivalent about these contagion theories at best. The resulting theories of contagious insects and animalcules engendered 'taxanomic awkwardness' thus keeping 'the theory of animate contagion just outside the realm of the incredible'.[29]

In contrast to Wilson, I maintain that contagion theory prospered subsequent to the introduction of microscopic medicine in the sixteenth century. In particular, I argue that physicians who used the microscope remained open to the notion of contagious animalcules. Wilson is correct to argue that some of these researchers put forward wild speculations and unsubstantiated theories of animate-contagion; however, I believe that others utilised direct microscopic evidence. Such medical microscopers were part physician, part naturalist; they included diverse experimenters such as Giovan Bonomo, Sir Edmund King, and Michael Ettmüller. Each credited Leeuwenhoek as inspiration for their research, and, in some respects, they were his scholarly competitors. Their texts also reveal the influence of Baconian empiricism upon the medical field and the struggle to deduce evidence from microscopic observation.

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[28] C. Wilson, *The Invisible World*, p.141.
[29] Ibid., p.175.

Giovan Bonomo (1663-1696) was a student of Francesco Redi and was witness to Redi's renowned experimental proofs against spontaneous generation.[30] Bonomo went on to perform his own anatomical research and became a resolute adherent of Redi and his creed 'Omne vivum ex ovo' ('all life from an egg'). Bonomo is a significant early figurehead in the history of epidemiology since he represented an early link between the study of natural history and microscopic medicine. Bonomo shifted away from naturalism to medicine as he actively went in search of the eggs of contagious animalcules; this may have represented a last effort to corroborate Redi's arguments against spontaneous generation.

Heretofore we have not explored the spontaneous generation debate in much detail. Among historians of science this has been a popular field, and it is one that has been well-outlined elsewhere.[31] However, scholars have been slow to evaluate the association between medical knowledge and the early critics of spontaneous generation. Furthermore, the 'slow death of spontaneous generation' as some authors have put it, has given credibility to the assertion that developments in microscopy failed to resolve problems concerning generation.[32] This could not be from the reality of the matter. The ever-pressing need to prove or disprove spontaneous generation kept generations of naturalists glued to their microscope lenses. For our purposes, it is important to note that these early

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[30] F. Redi, *Experienza intorno alla generazione degli insetto* (Florence, 1668).
[31] See, for example, S.A. Roe, *Matter, Life and Generation: Eighteenth-Century Embryology and the Haller-Wolff Debate* (Cambridge, 1981); P. Corsi, 'Before Darwin: Transformist Concepts in European Natural History', *Journal of the History of Biology,* 38, no. 1 (2005), pp.67-83; J.E. Strick (ed.), *Evolution and the Spontaneous Generation Debate* (Bristol, 2001); W.G. Vandervliet, *Microbiology and the Spontaneous Generation Debate During the 1870's* (Lawrence, K.A., 1971).
[32] R. Levine & C. Evers, 'The Slow Death of Spontaneous Generation (1668-1859)', *The National Health Museum Website* <http://www.accessexcellence.org/RC/AB/BC/Spontaneous_Generation.php>, (accessed 11, Jul. 2009).

critics of spontaneous generation (such as Bonomo) were successful in advancing one aspect of medical knowledge: they bolstered the case for animate contagion.

In a letter to Redi, published in the *Philosophical Transactions* of 1702, Bonomo contended that diseases were not the result of humoural imbalances within the body, nor were they the outcome of putrefactions or fermentations within the blood. [33] He was explicitly critical of the followers of van Helmont for reinforcing this supposition. He maintained that diseases transferred by contagious animalcules through direct physical contact instead of through inhaling fermentations. A microscope user could readily identify this where skin infections were concerned. In scrutinizing a severely 'itchy person' through his lens, Bonomo recorded the first observation of scabies mites. Bonomo hypothesised that they could propagate 'by means of sheets, towels, handkerchiefs, gloves, etc'. and could only be eradicated through topical 'salts, sulphurs, vitriols, mercuries,' as opposed to the standard 'inward medicines' proposed by humoural doctors.[34]

Nicolas Andry de Boisregard (1658-1743) had achieved a virtually identical conclusion in France. His 1700 publication *De la Génération des Vers dans le Corps de l'Homme*[35] underscored the parasitic role of animalcules, and noted common modes of contagion, from direct transmission, to food and tobacco poisoning.[36] Andry de Boisregard's text was much greater than a mere treatise on microscopic worms; it was a passionate celebration of microscopic-medicine. He wrote:

[33] G. Bonomo, 'An Abstract of Part of a Letter from Dr Bonomo to Sigmor Redi, Containing Some Observations concerning the Worms of Humane Bodies', *Philosophical Transactions*, 23 (1702-3), pp.1296-99.
[34] Ibid., p.1298.
[35] Translated anonymously the following year in Britain: *An Account of the Breeding of Worms in Human Bodies; Their Nature, and Several Sorts* (London, 1701).
[36] Ibid., p.219.

> A physician ought to discover in a body that which is most conceal'd . . he must, with his stillet and finest instruments dilate the smallest vessels . . .he must trace the smallest threads of nerves . . . and by the fragility and benefits of all those parts he knows how to judge what is capable of breaking or preserving them, and by consequence to weaken or fortify health, and prolong or shorten life.[37]

In addition to these early authors, Wilson has investigated two other influential microscope studies of animate contagion: Benjamin Marten's 1720 text, *A New Theory of Consumptions* and Richard Bradley's 1721, *The Plague at Marseilles Consider'd*. Like Andry de Boisregard, Marten (1690-1752) examined worms and animalcules present in the lungs, arguing that a specific animalcule type must lead to specific cases of lung disease. Not surprisingly, he referenced Kircher as his inspiration for his theory of multi-structured contagious animalcules.[38]

Marten's colleague Richard Bradley (1688-1732) investigated the raging 1720 plague in France and was somewhat more desperate to prove contagious animalculism. According to Wilson, his arguments were 'circumstantial and analogical'; nevertheless, Bradley believed that swarms of parasites must be able to travel through the air; but, in contrast to miasmas, these were not ferments or noxious vapours, but must be actual living particles which infect within specific niches of the body.[39]

Clearly parasitology was a growing area of study due to these early eighteenth-century microscope enthusiasts. Nevertheless, we must remain cautious: animalculism neither flourished nor vanished at the cusp of the eighteenth century; instead, it persisted. A particular notion of parasitic

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[37] Ibid.
[38] B. Marten, *A New Theory of Consumptions: More Especially of a Phthisis, or Consumption of the Lungs* (London, 1720), p.62. For more on Marten, see R. Doetsch, 'Benjamin Marten and his "New Theory of Consumptions"', *Microbiological Reviews*, 42 (1978), pp.521–8
[39] Wilson, *The Invisible World*, pp.162-4.

animalcules surfaces which still fell short of modern Germ Theory, but it is one which maintained its strong roots in microscopic empiricism. Wilson believes that modern bacteriology may not have developed from these earlier contagion theories; however, it certainly emulates them. She writes, 'Pasteur's conception of a disease process is not essentially different from that of Bradley or Marten'.[40] This leads her to conclude that 'a germ theory of animate contagion can thus be said to have emerged between 1650 and 1720'.[41]

While corroborating her statement on eighteenth-century medical texts I take issue with Wilson's assertion that these theories of animalcule contagion subsequently declined midway through the eighteenth century. Of course, Wilson is not the only historian who makes this claim; Marian Fournier also pointed to a decline in medical microscopy. Fournier argues that by the start of the century, the microscope had become more of a decorative status symbol than an instrument of advanced research.[42]

In opposition to these authors, I have found numerous eighteenth-century physicians who upheld animate contagion in a similar vein to Bonomo, Boisregard, and the others. If we divert away from the leading academic journals and turn towards an examination of medical dictionaries, textbooks, and encyclopedias, we find that the story is completely different. While these texts did not operate as the official mouthpieces of respected learned societies, they did have the capacity to connect with just as wide an audience, and, for better or for worse, their editors more frequently published ideas from physicians on the periphery. This appears to have been an ongoing trend from the beginning of the eighteenth century to its conclusion.

[40] Ibid., p.174.
[41] Ibid.
[42] Fournier, *The Fabric of Life*, pp.16-7.

Steven Blankaart (1650-1704), an Amsterdam physician and microscopist, was one of the first authors to compile an alphabetical medical dictionary, *The Physical Dictionary* (1702). In the entry on sanguine diseases, Blankaart wrote, ‘The microscope discovers that the parts of the blood are round or globular naturally, but that in fevers ‘tis full of worms’.[43] Similarly, Leipzig physician Michael Ettmüller upheld animate contagion in his microscopic research. Although he died in 1683, his textbook on contagious diseases, *A Complete System of the Theory and Practice of Physic,* went through three printings, the last appearing in 1712.[44] Ettmüller argued that diseases followed animate modes of transmission as well as miasmatic modes of infection. Like Blankaart, he had witnessed worms in the blood through his microscope.[45] Ettmüller theorised that such worms could transmit various fevers and sanguinal diseases from person to person if they left the body. Such contagious elements would act in a similar manner to parasitic intestinal worms which are transmited via seeds and attack patients with a ‘deficiency or weakness of bile’.[46]

Similarly, Thomas Apperley, a fellow at St. Johns College Cambridge, wrote in 1731:

> The late Discoveries of Physicians, and others, who have made use of the Microscope, have let us into the Knowledge of a World of little Animals before undiscoverable to our Senses; and which demonstrate to us, that there is scarce any Body that has not its particular Sort of Worm. They are in the Air, in the Water, in the Earth, in Fish, in Fruit, in Glass itself, and in the most solid Bodies. Man is the most

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[43] S. Blankaart, *The Physical Dictionary. Wherein the Terms of Anatomy, the Names and Causes of Diseases, Chyrurgical Instruments, and their Use are Accurately Describ'd* (London, 1702), pp.272-3.
[44] M. Ettmüller, *Etmullerus Abridg'd: or, A Complete System of the Theory and Practice of Physic. Being a Description of all Diseases Incident to Men, Women and Children. With an Account of their Causes, Symptoms, and Most Approved Methods of Cure, Both Physical and Chirurgical* (London, 1712).
[45] Ibid., p.217.
[46] Ibid., p.117.

> subject to Worms of any Creature; there is scarce a Part of his Body which is not attack'd by them . . .[47]

Obviously it is difficult to assess whether microscope users were more interested in parasitological than contagionism. Were these early microscope observations influential to the study of contagious disease? Ettmüller's legacy would lend weight to the idea of contagious animalcules. In particular, Bradley and Marten's theory of animate contagion cropped up during the deadliest epidemics, such as plague or the frequent smallpox outbreaks in the Americas. Historian Donald Hopkins has found that New England leaders such as Cotton Mather and Benjamin Colman advocated animalculism, and quarantine and their 'views on this topic can be traced directly to Benjamin Marten'.[48]

Still, historians are confronted with major obstacles in their attempts to re-evaluate the popularity of animate contagionism. Regrettably, 'animalculism' as a theory of contagion has no simple definition. It is not altogether disparate from other medical theories of disease transmission. The confusion lies with the fact that some eighteenth-century authors believed in 'contagious animalcules' but they may not have deemed these to be living micro-organisms.

Furthermore, it is nearly impossible to determine theories of contagion based solely on physicians' responses to disease. Eighteenth-century notions of *contagion* and *infection* are, at times, interchangeable, at other times, opposed. The very notion of 'contagion' is itself problematic according to Margaret Pelling. In her article, 'The Meaning of Contagion,' Pelling notes that physicians tended to use 'contagion' to imply the material of transmission, and 'infection' to

[47] T. Apperley, *Observations in Physick, Both Rational and Practical. With a Treatise of the Small-pox* (London, 1731), p. 86.
[48] D. Hopkins, *The Greatest Killer: Smallpox in History* (Chicago, 2002), p.10

imply the mode or process by which these contagions were transmitted.[49]

Unfortunately,

> Confusion arises from the failure to distinguish between the material or influence (living or non-living) which is transmitted between persons or environments, and the process of transmission or affection, direct or indirect.[50]

Pelling calls for caution when assessing early claims about disease etiology. One must not so easily pigeonhole eighteenth-century physicians into 'miasmatic', 'animalculist', or 'contagionist' camps. Furthermore, as we have already seen, the uncertainty of physicians during this century did not dissuade them from issuing wild speculative claims regarding the material (contagion) and mode (infection); that is if they made any discernment between 'contagion' and 'infection' at all.

Therefore, in our pursuit to trace the 'animalcules of contagion', we must carry out a closer reading of individual author-physicians, the medical epistemology influencing their texts, and the influence of these physicians on specific cases of disease. Fortunately for the historian, certain outbreaks of disease grant insight into precisely this context. For example, the 1720 outbreak of plague in the south of France called into question every known assumption concerning contagion and infection at the very moment when enlightenment physicians were endevouring to codify such theories of disease. As we shall see, both early contagionists and anti-contagionists utilised the plague outbreak as a testing ground for medical microscopy, theory and treatment.

i. The Plague of Marseilles: An Early Case Study in Contagion Theory

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[49] M. Pelling, 'The Meaning of Contagion, Reproduction, Medicine, and Metaphor', in A. Bashford & C. Hooker (eds.), *Contagion: Epidemics, History and Culture, From Smallpox to Anthrax* (London, 2001), pp.15-38.
[50] Ibid., p.21.

The plague of Marseilles was one of the deadliest disease outbreaks in Europe during the eighteenth century, resulting in the deaths of approximately 100,000 people in the South of France and parts of Italy.[51] From witnesses of the plague it is known that death could occur in less than twenty-four hours, which, to a modern physician, would indicate that the slower bubonic infection had progressed to a pneumonic stage. Although the bacilli would have initially spread through fleas inhabiting rats, in this pneumonic stage, plague bacilli could spread through coughing, sneezing or simple respiration. The plague of Marseilles was likely a amalgamation of this deadly airborne and vector-borne infection; of course, such a modern understanding of plague is irrelevant to the historian. The historical perceptions and consequences of the epidemic as well as the methods used to prevent it are far more valuable. Desperate to combat the plague, the Parliament of Aix imposed quarantines and constructed a wall to the north of Marseilles. The 'mur de la peste' had little effect in curbing the epidemic, but its presence is lasting evidence that contagionist theories of direct contact had a major influence on state anti-plague policy.

The plague cut deep divides within Europe's medical communities, especially in Great Britain where the shadow of the Great Plague of London still haunted the medical establishment and where many doctors perceived the epidemic as inevitable. Physicians, clerics, and politicians set out to battle the epidemic with their pens, authoring numerous treatises warning of the danger of plague and outlining preventative measures. Doctors keeping an eye on the events in France distributed pamphlets to the general public informing them of the signs, symptoms, and preventative methods employed around Marseilles. They also issued regular updates and addendums as events unfolded in France

[51] J.N. Hays, *Epidemics and Pandemics: Their Impacts on Human History* (Santa Barbara, 2005), pp.135-137.

and as the College of Physicians in London further debated the nature of contagion. Thus, the Plague of Marseilles became a de facto case study in which British medics witnessed the latest theories of treatment and prevention put in practice. For our purposes, the Marseilles plague event offers insight into eighteenth-century contagion theory and a wealth of primary sources.

According to my own estimates, between 1721 and 1725 British publishers issued no less than sixty titles on the nature of plague.[52] Most of these were pamphlets or brief treatises from marginal physicians hoping to advance their own theories of contagion. Many were religious theories of the plague's origin and prevention. Interestingly, some authors took great efforts to retrospectively diagnose and analyse the Great Plague of 1665 (most famously, Daniel Dafoe's fictional *Journal of the Plague Year* published in 1722). Some of the more pragmatic authors outlined realistic schemes for dealing with the panic and casualties that such a plague would result upon their nation.[53] Other physicians wrote desperate pleas for stricter quarantine measures such as those enforced in France. Conversely, certain doctors argued against quarantine entirely. Doctor James Pringle (not to be confused with the more famous John Pringle, 1707-1782) was a pamphleteer who argued that, 'The method of prevention practiced in France is not only inhumane but useless'.[54] As an analogy, Pringle compared the quarantine walls and cordons to poorly-built military barricades which the swift miasmas of plague could easily breach. He

[52] Cross-checked through holdings at the Bodleian Library, Oxford, Eighteenth-Century Collections Online database, Early English Books Online database, and WorldCat database (June-July, 2011).
[53] For example, J. Colbatch's *A Scheme for Proper Methods to be Taken, Should it Please God to Visit us with the Plague* (London, 1721).
[54] J. Pringle, *A Rational Enquiry into the Nature of the Plague: Drawn from Historical Remarks on those that have Already Happen'd* (London, 1722), frontispiece.

went on the offensive to defend his viewpoint, arguing against the underlying notion of contagious particles:

> First, that neither goods nor persons are capable of communicating the plague. Second, that the air only is the cause; and therefore, that lines and quarantines are not only useless, but pernicious.[55]

The disease, Pringle claimed, sprang from stagnant air and foul-smelling fermentations (miasmas) that occurred naturally from the summer heat.

What does Pringle's miasmatic argument teach us about the state of contagion theory in the early eighteenth century? For one, we learn that particle contagion was not unpopular; rather, Pringle saw it as a rival theory. Surgeons had drawn their quarantine lines around Marseilles with the aim of preventing an actual particle of disease, not an intangible aerosol. Pringle believed that he was fighting *against* this popular assumption of particle disease theory. On the other hand, no theory of contagion was wholly dominant or convincing.

If we examine the events in Marseilles more closely, we find that French physicians were as divided over contagion theory as their British counterparts; they did not uniformly accept either the miasmatic or the particle theory of disease. The surgeons who first encountered the disease believed that it was indeed plague and that the illness was contagious; however, the king's physician Pierre Chirac (1650-1732) sought to investigate the matter further, and dispatched an expert team of physicians from the university in Montpellier. The commission consisted of physicians Chycoineau and Verny, and a surgeon named Soulliers. After touring the Marseilles lazarettos and conducting post-mortem examinations, the team concluded that a malignant fever had arisen, not the dreaded bubonic pestilence.

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[55] Ibid., p.3.

From this evidence, one could argue that leading eighteenth-century physicians aggravated microscopic research because they insisted on the existence of only non-contagious diseases (the miasmatic forms of contagion) and not the microscopic animalcules. Indeed, most eminent French physicians such as Chirac rejected animate contagion. While the majority of Marseilles physicians had fled the contagion, Chirac, writing in Paris, insisted that the plague had come from squalid conditions, poor hygiene and diet; however, he did not undermine the work of his subordinates and colleagues when they put forward dissimilar theories. Another Montpellier doctor, Antoine Deidier, had travelled to Marseilles and produced a rebuttal to Chirac. Deidier manufactured a set of experiments in 1725 in which he injected bile from plague victims to produce plague in dogs, thus proving that the plague was not an environmental miasma. At the very least, it was some form of direct bodily fluid-based contagion.[56] François de Chycoineau debated Deidier that same year in at the school of medicine in Montpelier. Instead of maintaining the argument for a strict miasmatic hypothesis, he instead posited that the disease was neither an animalcule nor a miasma: it was a pestilence owing to the natural unsanitary conditions of Marseilles.

Deidier was not to be outdone. He had, by then, latched on to animate contagion. In 1724, he published a study of venereal disease where he argued that microscopic worms in the bloodstream caused syphilis much as they must cause plague.[57]

Such post-plague debates provide a window through which to view contagionist thought. Although inconclusive, the plague debates reveal that anti-

[56] R. Williamson, 'The Plague of Marseilles and the Experiments of Professor Anton Deidier on its Transmission', *Medical History*, 2 (1958), pp.237-52.
[57] A. Deidier, *Dissertatio medica de morbis venereis, cui adjungitur dissertatio medico chirurgica de tumoribus* (London, 1724).

contagionism was not universal: on the contrary, contagionism, even animalcule contagionism, remained a much-discussed alternative. No single theory of disease dominated.

Meanwhile, in England, Richard Mead produced his pro-contagion treatise concerning the plague (1720)[58]; however, Mead himself was also far from converting to animalculism. According to Arnold Zuckerman, his plague tract is something of a compromise between the contagionist and miasmatic theories.[59] In addition, Mead had numerous critics, several of whom published counter arguments only a few months later.[60] These numerous arguments have led some historians, such as Zuckerman, to speculate that Mead's contagionism had not yet become mainstream medical theory; and therefore, had incurred sharp rebukes. Alternatively, a close inspection of these criticisms exposes the fact that many of Mead's detractors did in fact accept contagionism, but not where plague was concerned. Others, such as George Pye, were more critical of the repercussions of Mead's contagion theory, especially the quarantines it would necessitate

Clearly, we can no longer assume that Enlightenment physicians took either a miasmatic or an animalculist perspective in their theories of contagion. Physicians, by the eighteenth century, had commenced more elegant attempts to classify disease etiologies. Some authors speculated that a more agreeable explanation for the plague was an amalgamation of the two modes of contagion. For example, one anonymous printer in Dublin distributed pamphlets to the

[58] R. Mead, *A Short Discourse Concerning Pestilential Contagion, and the Methods to be Used to Prevent it* (London, 1720).
[59] A. Zuckerman, 'Plague and contagionism in eighteenth-century England: the role of Richard Mead', *Bulletin of the History of Medicine*, 78 (2004), pp.273–308.
[60] G. Pye, *A Discourse of the Plague; Wherein Dr. Mead's Notions are Consider'd and Refuted* (London, 1721).

public which warned residents that the origins of the disease were uncertain[61]; therefore, they should take precautions against all the probable causes: the foul miasmas, Mead's particles, and also contagious animalcules or their eggs. The publisher (and likely author) of this treatise was a Miss Catherine Hicks.[62] Having read the latest theory of Mead as well as the older text by Kircher, she argued:

> One cause of plague is that assign'd by Dr. *Mead*, and others who attribute the *plague* to a putrefaction of the Nitrous Spirit in the air. Which thereby communicates contagious particles and venomous effluvia.
>
> Another (not improbable cause of plague is this . . that corrupted foul vapours, exhalations, and effluvia, proceeding from putrid nasty places or bodies will turn to produce in the air prodigious numbers of venomous insects and animals. . .
>
> Now these pestilential animals with their eggs, being drawn and tuck'd into our bodies along with the air we breath, attack and prey upon human body, as grubs and worms in the garden devour, corrode, and destroy, all they come near, And in this (not improbably according to *Kircher*) consists the *plague*.
>
> Persons may choose which of these opinions they like best, nothing being here proposed demonstratable, but only as not improbable.[63]

Such ambivalence concerning contagion theory was not uncommon. Although Richard Mead's theory of contagious airborne particles seems to have been the most widely discussed hypothesis, it was also the most controversial.

Given the surfeit of such publications in the 1720s, one may regard Plague of Marseilles as the first testing site for analysing and implementing multifaceted theories in epidemiology. Miasmatic and the animalculist

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[61] *To all Gentlemen, Ladies and Others, ... Since the Nations of Great Britain are Under the Apprehensions of Fear, of that Contagious Distemper* (Dublin, 1722), pp.1-17.

[62] According to book historian, Mary Polland, Catherine Hicks was the real name of publisher 'C.H'. of the Dublin printing house Reindeer Press. She remained active until 1745, mostly printing manifestos which highlighted Irish famine, illness, and the plight of the poor. M. Polland, *A Dictionary of Members of the Dublin Book Trade: 1550-1800* (London, 2000), pp.285-6.

[63] *To all Gentlemen, Ladies and Others,* p.6.

hypotheses remained equally central. As I have argued, neither theory of disease seems to have dominated. In terms of prevention, most protective measures such as the *mur de la peste* assumed contagion via direct contact, not aerosolised contagion. Then, once these efforts proved futile, miasmatic theories of disease appears to have risen in popularity *after* the plague had run its course. Mead's theory of particle miasmas did, after all, remain vehemently debated well after the plague; and, as we shall see, miasmatic notions remained central to later quarantine laws. On the other hand, animalcule theories of contagion did not significantly diminish in the wake of the plague.

Although miasmatic theories of airborne disease became more pronounced as the eighteenth century progressed, I would acknowledge the sustained interest in microscopy for maintaining animate-contagion theory during this period. Although Catherine Wilson may be correct in her claim that there was an eighteenth-century dearth of *new* research into animate contagion; we must nevertheless recognise that past microscope experiments still influenced medical reforms and theory. For example, Sir Edmund King had reported to the Royal Society in 1693, several experiments on animalcules present in rain water that he had examined with a microscope. King's observations were typical of the late seventeenth century interest in infusoria; however, his article was remarkable because he outlined various fluids and acids which may kill the various microscopic animalcules. He immersed different animalcules in herb water, spirit of vitriol (sulphuric acid), salt of tartar (potassium carbonate) and even blood, and recorded the rates of effectiveness for each liquid.[64] King's scholarship would later influence sterilisation techniques throughout the century.

[64] E. King, 'Several Observations and Experiments on the Animalcula in Pepper Water', *Philosophical Transactions*, 17 (1693), pp.861-5.

From one neglected and underused source - early medical dictionaries - we see that Kircher, King, and other animalculists heavily influenced eighteenth-century medical theories of contagion. One microscoper, James Robert, published *A Medical Dictionary* in 4 volumes in 1743. Robert frequently used the dictionary to draw attention to his microscopic studies of contagious particles. A proponent of both the miasmatic and animalcule theory of contagion, Robert attempted to classify each contagious disease based upon its cause. In addition, he was evidently attracted to pure microscopic research carried out by contemporary naturalists. The worth of his dictionary was linked to its skill in uniting past and contemporary microscopic research for a single goal. Thus, Robert called attention to Leeuwenhoek's infusoria and animalcules in conjunction with Sir Edmund King's experiments with various disinfectants. Robert theorized that King's solutions would be effective forms of disinfectant against further disease-causing animalcules.[65] In particular, he highlighted Giovan Bonomo's 1687 discovery of scabies mites and inferred that King's solutions might be effective treatments.

Revitalised microscopic techniques also persuaded some physicians to abandon miasmatic explanations in favour of a return to Kircher's animalcules. One such theorist was the eminent Scottish physician John Pringle. A physician of some prominence, Sir John Pringle was president of the Royal Society and is well renowned today as potentially the earliest physician to utilise the term 'antiseptic'[66] Pringle's book, *Observations on the Diseases of the Army* (first published in 1752) went through numerous editions. In the earliest copies, Pringle records his strict miasmatic theories of camp diseases and methods of

[65] J. Robert (ed.), 'Animalcules', in J. Robert (ed), *A Medical Dictionary*, 1 (London, 1743).
[66] S. Selwyn, 'Sir John Pringle: Hospital Reformer, Moral Philosopher and Pioneer of Antiseptics', *Medical History*, 10 (1966), pp.266–74.

treating them through ventilation or fumigation. In later editions, he gives credit to microscopic theories of animalcule contagion. By the sixth edition of 1768, Pringle had changed course entirely. He wrote that he had since found a 1730 reprint of Dr. Bonomo's treatise on scabies and now believed that most skin infections 'are best accounted for by Leeuwenhoek, from certain small insects which he discovered in the pustules by the microscope'.[67] He further called into question his past miasmatic theories as they applied to other diseases. In the case of dysentery, Pringle wrote, in the sixth edition of his text, that he had become aware of renewed interest in the old works of Kircher from none other than Carl Linnaeus (1707-78).

> I was inclined to refer the *causa proxima* or *immediate cause* of the disease to this putrid ferment. But having since perused the curious dissertation, published by Linnaeus, in favour of Kircher's system of contagion by animalcula it seems reasonable to suspend all hypotheses, till that matter is further inquired into.[68]

In conclusion, leading eighteenth-century physicians were not opposed entirely to the notion of animate contagion. As the century progressed, eminent physicians such as Pringle had already begun reforming and cleansing prisons and military hospitals based upon past notion of contagious micro-organisms.

3.3. Public Knowledge

Clearly, as the above accounts have shown, microscopy and animalculism were popular topics among eighteenth-century naturalists and physicians. This reinforces Ratcliff's main argument that the eighteenth and early nineteenth century did not experience a great decline in microscopic natural history. What is equally important, I would argue, was the influence of these theories upon the

[67] J. Pringle, *Observations on the Diseases of the Army, in Camp and Garrison. In Three Parts. With an Appendix Containing Some Papers of Experiments read at Several Meetings of Royal Society*, 2nd edn. (London, 1753), p.301.
[68] Ibid., 6th ed., (London, 1764).p.265.

public understanding of disease. As microscopes increased in number and affordability, so too did widely-sold pamphlets and treatises dedicated to animalcules.

Travel writers also brought to light the animalcule theory of contagion as they interpreted the nature of foreign diseases. For example, in *Travels in Asia Minor* (1775), Richard Chandler depicts an outbreak of plague, ‘a disease arising from certain animalcules, probably invisible, which burrow and form their niches in the human body’.[69]

Similarly, textbooks and other juvenile non-fiction began widely discussing animalcules and microscopy at the beginning of the nineteenth century.[70] Some textbook authors, such as William Bingley, composed natural histories of infusoria for schoolchildren. In one 1803 text, Bingley described animalcules, their place in Linnaean taxonomy, and observations on single-cell division. He also attempted to classify microscopic worms based upon their parasitic or commensal relationship to man (this being seventy-five years before Heinrich Anton de Bary coined the word, ‘symbiosis’).[71] Other popular authors, such as the famed educator Noah Webster, wrote textbook summaries of contagion theory. Webster himself promoted miasmatic theories of contagion and rejected Chandler’s contagious animalcules as ‘silly opinions’[72]; however, he conceded that ‘we reason without knowing causes’.[73] Despite the dissimilarities that arose in the above accounts, this rapid emergence of animalculism in

[69] R. Chandler, *Travels in Asia Minor: Or an Account of a Tour Made at the Expense of the Society of Dilettanti* (Oxford, 1775), pp.278-9.
[70] See, for example, E. Joyce, *The Wonders of the Microscope; or, an Explanation of the Wisdom of the Creator in Objects Comparatively Minute: Adapted to the Understanding of Young Persons* (London, 1805).
[71] W. Bingley, *Animal Biography; or Anecdotes of the Lives Manners, and Economy, of the Animal Creation Arranged According to the System of Linnaeus*, (3 vols., London, 1803), iii, pp.490-2.
[72] N. Webster, *A Brief History of Epidemic and Pestilential Diseases* (2 vols., London, 1800), ii, p.244.
[73] Ibid., ii, p.251.

literature denotes a strong public discourse concerning microscopic disease contagions.

i. Quarantine and the state

If doubts remain regarding the extent of contagionism during the eighteenth and early-nineteenth centuries, we must then reflect on the swift enactment of quarantine laws during this time. Statesmen throughout the century adopted sanitary measures in the conviction that they would lessen the impact of epidemics and contagion. General sanitary measures, such as the cleansing of streets and the draining of swamps or sewage ditches occurred locally. On the national level, the most controversial health laws were the Quarantine Acts (notably those of 1710, 1753, and 1799) which tried to tackle hazardous miasmatic fevers and plague. Although a more detailed study of these quarantine acts is beyond the scope of this thesis, it is important to note the affects of changing eighteenth-century medical knowledge upon these laws.[74] The debates which gave rise to these acts demonstrate how government officials interpreted contemporary medical theories of microscopic contagion.

Subsequent to an outbreak of plague in the Baltic, the first Quarantine Act was enacted in 1710. In addition to restricting importation and immigration from those regions, the act stipulated that goods from stricken regions would be aerated for numerous days, either onboard a ship or in special open sheds. Stemming from the miasmatic theory of contagion, this act did more to combat foul odours than particle contagions.

[74] The history of quarantine has been a rich field of study. Most recently, John Booker has published a massive 624 page account entitled, *Maritime Quarantine: The British Experience, c.1650–1900* (Aldershot, 2007). Mark Harrison is also investigating the subject. M. Harrison, 'Disease, Diplomacy, and International Commerce: The Origins of International Sanitary Regulation in the Nineteenth Century', *Journal of Global History*, 1 (2006), pp.197-217.

Although to modern eyes the Quarantine Acts may seem ill-planned, these policies were far more than simply ad hoc attempts to contend with epidemics; they involved complex negotiations between state authorities, merchants, and medical experts. This fostered tensions lasting throughout the eighteenth century. As John Booker has argued, these debates delayed effective action in Great Britain because parliamentarians took into account public and commercial opinions, which often differed radically, rather than defer to the king or quickly adopt draconian laws.[75]

Thankfully for scholars, the long record of quarantine debates are an excellent means of studying medical thought in enlightenment Britain. Concerning these acts and other sanitary laws, members of the parliament frequently consulted the College of Physicians to assess the latest theories of contagion.[76] Parliament also solicited avant-garde treatment techniques such as fumigation with nitric and sulphuric acid in disease-striken areas.[77] These debates and fact-finding missions often resulted in common themes concerning the nature of contagion: first, that contagious miasmas and particles could be both endemic to the local inhabitants or transmitted from great distances; second, that diseases were transmissible although they first developed from uncleanliness and squalor. The following is an example of one such report concerning an outbreak of fever at a prison in Winchester in 1779. The Parliamentary committee that explored the issue concluded that a group of Spanish prisoners imported the disease.

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[75] J. Booker, *Maritime Quarantine*, p.96.
[76] This is particularly true during the 1721-1722 plague in Marseilles, is which the House of Commons and Lords debated quarantine measures, 'November 1721', *Journals of the House of Commons 1688-1834* (London, 1834), p.663.
[77] James Carmichael Smith, 'Report on his Position Respecting Nitrous Fumigation', in *Reports from the Committees of the House of Commons 1715-1801* (London, 1802), p.189.

> It appears to the Committee the disorder which has reigned in the prison at Winchester, originated with the Spanish prisoners, and was a consequence of an infection brought with them . . .in great measure to their own indolence and want of uncleanliness.[78]

The resulting solutions then entailed sanitary reforms and quarantines against outside pestilence.

Although these were contagionist reforms, I must stress that quarantine and sanitary measures did not necessarily endorse a theory of *animate* contagion (that is, diseases caused by micro-organisms). As Alain Corbin argued in his brilliant study, *Le miasme et la jonquille*, eighteenth-century reforms in hygiene and sanitation had as much to do with social conventions and population control as they did medical knowledge.[79] Of course, medically-minded reformers did not exclusively sanction animate contagion theory; quite the opposite. Leading physician-reformers seem to have supported quarantine measures while concurrently disavowing the theory of animate contagion. This was true for Mead's proposed quarantines during the plague of Marseilles.

Unlike the plague walls imposed around Marseilles, most naval quarantines appear to have been founded upon varying notions of contagion. On land, the French efforts to grapple with the contagious plague using walls and restrictions on population movement presupposed that the disease spread through direct contagious animalcules or particles. In contrast, the quarantine acts which directed the ventilation of ship cargo implied that disease was carried within fermentations, putrid air, or, if they were indeed particles, they could be dissipated in the air.

The fact that even doctors who upheld miasma were also frequently critical of the quarantines causes further complication. Most of these were not

[78] 'Report from the Committee on the Health of Prisoners Confined at Winchester' in ibid., p.769.
[79] A, Corbin, *Le miasme et la jonquille: l'odorat et l' imaginaire social, XVIIIe -XIXe siècles* (Paris, 1982).

speculative physicians labouring behind college doors in London, but were often former military surgeons who had encountered fevers and pestilential diseases abroad. Although these medics did not support the animalcule theory of contagion, they had first-hand experience with inoculation techniques, the incubation periods of various diseases and the rates of their communicability. For example, Matthew Guthrie, a St. Petersburg physician, had witnessed bubonic plague while serving as a medical advisor on the Russian frontier in 1772. He advised his colleagues in Britain to avoid over-implementing quarantines. 'I am convinced in my mind that the quarantine of persons might be much shortened without danger to the state'.[80]

According to Guthrie, the plague incubated within 14 days; any quarantine with a longer duration would be ineffective. On the other hand, Guthrie still supported the airing of cargo and other goods to prevent dangerous miasmas. In this manner, field experience actually bolstered the miasmatic view. As Mark Harrison writes, diseases such as Plague and yellow fever 'were thus demystified and some medical practitioners came to regard them, not as separate diseases, but merely as varieties of common or garden 'epidemic fever''.[81]

A comparable attitude can be found among the founders of the Paris clinical school a generation later. Leading French physicians such as J. A. Rouchoux and Nicolas Chervin had turned to anti-contagionism following the adoption of the new empirical trends of the Paris teaching hospitals. They had also recalled the failure of quarantine efforts to mitigate the typhus and yellow

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[80] M. Guthrie, 'Observation on the Plague, Quarantine, etc in a letter from Dr. Matthew Guthrie, Physician at St. Petersburg, to Dr. Duncan', in A. Duncan (ed.), *Medical Commentaries, for the Years 1781-2: Exhibiting a Concise View of the Latest and Most Important Discoveries in Medicine***,** 8 (London, 1787), p.351.
[81] M. Harrison, 'Disease, Diplomacy, and International Commerce: The Origins of International Sanitary Regulation in the Nineteenth Century', p.204. See also, M. Harrison, *Medicine in an Age of Commerce and Empire: Britain and its Tropical Colonies*, 1660-1830 (Oxford, 2010). Also, P. Baldwin, *Contagion and the State in Europe 1830–1930* (Cambridge, 1999).

fever outbreaks during the Napoleonic campaign.[82] Thus, by the early nineteenth century, both contagionists and anti-contagionists had raised serious skepticism as to the efficacy of quarantine.

However, one cannot assess quarantines solely from a medical perspective. In reality, contagion-theory was a strategic weapon of commerce and diplomacy. Quarantines could promote one sector of the industry while devastating another. As Erwin Ackerknecht noted, the rapidly growing merchant classes dreaded quarantine as a source of losses and a weapon of state control.

> Anticontagionists were thus not simply scientists, they were reformers, fighting for the freedom of the individual and commerce against the shackles of despotism and reaction.[83]

Disease quarantines could be used to justify state expansion, police control, and a host of other state and military tactics. Harrison writes that 'sanitary cordons were sometimes used to cloak the aggressive intentions of predatory nations'. For example, during the East European plague outbreaks of the 1770s, 'Prussia established a sanitary cordon that encroached upon Polish territory, its ostensibly defensive nature concealing Prussia's predatory intentions'.[84] Clearly, contagion theory was caught up in the midst of these schemes and utilised to suit the needs of political adversaries. Contagionism was easily exploited: there was, after all, no consensus regarding the nature of transmission, even among microscope-users.

Historian Sean M. Quinlan has recently uncovered an example of such a conflict in relation to an outbreak of yellow fever in 1804 in Spain.[85] Napoleon's

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[82] E.H. Ackerknecht, 'Anticontagionism between 1821 and 1867', *Bulletin of the History of Medicine*, 22 (1948), pp.562-93.
[83] Ibid., p.571.
[84] M. Harrison, 'Disease, Diplomacy, and International Commerce: The Origins of International Sanitary Regulation in the Nineteenth Century', p.202.
[85] S.M. Quinlan, *The Great Nation in Decline: Sex, Modernity, and Health Crisis in Revolutionary France, c. 1750-1850* (Aldershot, 2007), pp.156-9.

Ministry of the Interior contacted the Paris faculties of medicine and established a commission of specialists to investigate the outbreak, much as the Royal court had enacted during the Plague of Marseilles. In this case, the physicians included Pierre Nysten, René-Nicolas Desgenettes, and Victor Bailly. The team did not publish their report for a full fifteen months because they quickly became severely divided over the issue of etiology. Nysen, Desgenettes, and their colleagues back in Paris maintained the miasmatic hypothesis. According to Quinlan, they hoped to adapt 'a consensual and cautious tone. . . For them, environmental factors probably caused the disease, and it became contagious only in particular instances'.[86] However, the remaining commission member Victor Bailly had far more experience treating yellow fever since he had been a doctor in Saint Domingue, and had witnessed the devastating yellow fever outbreaks during the Revolution. Bailly, 'firmly believed that yellow fever was contagious' and it could not have travelled from the Americas to Spain if it were stringently endemic or the result of environmental miasmas. He asserted that it was, in fact, a 'morbid germ' of contagion which the state could curtail through quarantines.[87] Bailly's former commission member Nysten then attacked Bailly in a counter-report to the Société de Médecine stating that yellow fever was strictly miasmatic and most likely non-contagious.

Most leading physicians followed Nysten's view; however, a minority, including the famed Philippe Pinel, backed Bailly's theory of direct contagion. The ministry of Interior then reluctantly consented to send further commissions over the following years, none of which determined a categorical origin of the fever. Finally, Jean-Noël Hallé, president of the Académie des Sciences and professor at the École de Santé, weighed in on the debate. He settled against the

[86] Ibid., p.158.
[87] V. Bailly, 'Sur la Contagion de la Fièvre Jaune D'Espagne' (undated), quoted in ibid.

notion of contagionism but nevertheless advised precautionary quarantines.[88] Thus, as Quinlan concludes, state officials had adopted a balancing act: on the one hand, silencing disruptive and opposing disease theorists; on the other, simultaneously conceding to some of their more-pragmatic demands such as quarantine.

3.4. Conclusions

The present study suggests that microscope research persisted outside the core medical schools despite its decline within eminent scientific circles. Just as physicians on the fringes of the medical establishment were more disposed to accept contagionism, so too were these outsiders more likely to practice microscopy and incorporate micro-material observations into their medical theory. Microscopy may not have thrived during the eighteenth and early nineteenth centuries, but it did not vanish. As we will see in later chapters, it remained topical even as leading medical authorities disregarded it entirely.

In a study on Enlightenment microscopy it no longer suffices to remain satisfied with studying only the academic elites, especially as we have seen how these lesser-studied and oft-neglected individuals kept microscopic medicine well-entrenched in the public and medical sphere. Furthermore, we must critically analyse major medical events, such as the 1720 Plague outbreak in Marseilles or the 1804 yellow fever epidemic in Spain. These events unsteadied the medical establishment and became testing grounds for all theories of contagion and prevention. They also provided an opportunity for marginalized doctors and authors to voice their concerns about disease contagion.

[88] Quinlan, p.158-9.

Microscopy seems to have weathered these storms as well. The eighteenth-century plagues and fever outbreaks may have even renewed microscope interest in those all-too elusive ‘contagious animalcules’. Then again, we cannot ignore the frequent detractors of microscopy from John Locke to Xavier Bichat a century later. For the most part, their scepticism concerning the microscope’s medical utility was well-founded.

Paradoxically, the microscope was at once an object useful for confirming facts and one that prohibited exact verification. This is further complicated by the fact that different observers used disparate microscopes, and even microscopes made by the same manufacturer differed immensely. Some instrument makers attempted to resolve these problems with higher-powered scopes, achromatic lenses, or projection systems, but these technical improvements apparently convinced few eighteenth-century physicians. Images through the lens remained elusive throughout the eighteenth century and beyond. As Ratcliff states, these were objects, ‘which *no one* can see at the same moment in time and in the same way’.[89]

In *A History of Public Health,* George Rosen wrote, ‘the first influential demonstration of a specific organism responsible for a specific disease was not made until the nineteenth century’.[90] I dispute this claim. Despite the technical limitations, we cannot deny that early microscopers witnessed startling new species beneath their lenses. If we turn our attention to medical microscopy, we will find a host of experimenters witnessing animate agents of disease, from large insects causing scabies, to suspected micro-organisms of plague. This occurred long before the era of Pasteur and Koch, or even their intellectual predecessors, Friedrich Henle and Agostino Bassi.

[89] M. Ratcliff, *Quest for the Invisible,* p.58.
[90] G. Rosen, *A History of Public Health,* 2nd edn. (Baltimore, 1993), p.272.

However, were these observations influential? The ‘animalculists’ were not successful in dethroning miasmatic and fermentative theories of disease; nevertheless, they kept the theory of organic contagion alive. Admittedly this was achieved with mixed results, but their microscopic study of animalcules did influence early practices such as antiseptic technique and quarantine control. Whether these ‘animalculists’ were experimental polymaths such as Kircher, or professional physicians such as John Pringle, the ‘germs’ of contagion were not unknown to them.

Chapter 4. Case study: Buffon's Investigations of Human Generation, 1740-1760

Compared to microscope-based pathology, anatomical microscopy seems to have found more adherents by the mid-eighteenth century. Moreover, the use of the microscope in anatomy and physiology was not limited to physicians and surgeons. This chapter will explore a case study in which naturalists applied their microscopes to study the body and its functions.

Naturalists and anatomists often turned to London manufacturers for the latest microscope designs. As we have seen in chapter 2, London had become a major centre of microscope manufacturing by the mid-eighteenth century. Through the influence of wealthy patrons such as Baker, opticians such as Cuff achieved renown among the community of naturalists and anatomists within Britain and the continent. This case study will investigate Georges-Louis Leclerc, Comte de Buffon (1707-1788) and his use of Cuff's compound microscope in the late 1740s. Not only do we find that continental anatomists utilised eighteenth-century microscope designs, their microscopes provided evidence for new theories of animal reproduction. Together with his breeding experiments, Buffon's microscope observations formed the basis for his influential theory of generation and species formation.

In the summer of 1748, Buffon began a series of ambitious dissection and microscope experiments concerning the function of spermatozoa. His ensuing observations opened a new chapter in the study of reproduction and embryology, for Buffon witnessed what he deemed to be the missing active ingredient of generation: 'female' spermatozoa. Although contemporary critics, such as René Antoine Ferchault de Réaumur (1683-1757), were quick to attack these observations, Buffon's microscope research renewed interest in the function of sperm and bodily 'animalcules'. With the aid of the renowned naturalist and microscope enthusiast, Fr.

John Needham (1713-1781), Buffon procured a powerful double microscope from Cuff's London workshop. Working by the light of candles, lamps, and the daytime sun, the team exposed several tissue samples from the reproductive organs of male and female dogs. Buffon quickly witnessed spermatozoa swimming in the still-warm liquid of a male dog testicle. He eagerly compared results using tissue from a female dog's ovary. After placing a sample of ovarian epithelium upon the slide, Buffon claimed that he himself was startled at what he witnessed through the microscope: swimming in the female Graafian follicles, he claimed to see those same swimming animalcules as he had seen in male animals. Needham and several gentlemen observers cast doubt: they demanded that Buffon clean the slide and use a new pick and blade to cut the ovarian tissue. He acquiesced, repeating the experiment over ten times, not only on male and female dogs, but also on rabbits, rams, sheep and other species from his garden.[1]

Buffon and Needham were eager to classify sperm across species lines. Though Buffon was not a *naturaliste-voyageur*, like many of his esteemed colleagues, he was a prodigious collector and correspondent. The assembly of animals Buffon gathered at his home in Montbard or at the Jardin du Roi provided excellent resources for this study and Buffon's later breeding experiments. The team continued to extract genital tissue and semen from Buffon's diverse animal subjects and view these samples under the microscope. They attempted to record the size, shape, and movement of the 'spermatic animals'. They used vivisection to preserve the reproductive organs under natural bodily temperatures and conditions. Other animal experiments stretched over days, as Buffon recorded the decreasing vitality of sperm

[1] Buffon first reported his findings to the Académie des Sciences in : 'Découverte de la liqueur séminale dans les femelles vivipares, et du réservoir qui la contient,' *Mémoires de mathématique et de physique, tirés des registres de l'Académie Royale des Sciences* (1748). Buffon's experimental account appears in most detail in his treatise, 'Expériences au sujet de la Génération', *Histoire naturelle, générale et particulière: avec la description du Cabinet du roy* (44 vols., Paris, 1749-88), ii. pp.168-75. [The original edition used here is from the BNF, Paris : Tolbiac – magasin: S-2430-2480].

as it cooled in the dissected and vivisected genitalia or under the microscope slide.[2] Buffon was so confident that his research would revolutionize the study of generation that he dated and sealed his findings under the care of a records clerk at the Académie des Sciences,[3] thus insuring his claim as the first naturalist to observe 'female' sperm in the ovarian follicles. Buffon subsequently set out describing these experiments in a further article to the Académie and in the second volume of his *Histoire naturelle générale et particulière.*

Perhaps as expected, Buffon's observations of 'female spermatozoa' were fodder for his critics. Spermists, ovists, and preformationists attacked Buffon, denouncing his theory of dual sex generation. Unfazed, Buffon followed his microscopic investigations with an extravagant series of cross-species breeding experiments at the Jardin du Roi. He began mating various species together in his garden to determine whether male and 'female' sperm not only appeared identical, but could also interact with spermatozoa from separate species. These experiments ultimately aided Buffon in his effort to catalogue species. His observations on spermatozoa and their function would form the bulk of the second volume of his monumental *Histoire naturelle.* Together with his breeding experiments, these studies informed his general theory of generation, species development, and species divergence.

What did Buffon really see under his lens in those earlier microscopic experiments? This chapter will record my own attempts to recreate Buffon's experiments using actual eighteenth-century microscopes and modern tissue samples.

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[2] Buffon, lettre à l'Académie des Sciences' (17, Mai 1748) Paris, in, *Correspondance inédite de Buffon, à laquelle ont été réunies toutes les lettres publiées jusqu'à ce jour* edited by H.N. de Buffon (2 vols., Paris, 1860), i, letter xxxii. Also reprinted online: Pietro Corsi et Thierry Hoquet, eds. 'Correspondance de Buffon, édition électronique,' (Centre National de la Recherche Scientifique, 2006) <http://www.buffon.cnrs.fr> Lettre L29.

[3] Buffon placed six wax seals over this letter and entrusted it to Jean-Paul Grandjean De Fouchy. The letter remained lost in the files of the Académie des Sciences until 1860 when Buffon's great grand nephew, Henri Nadault de Buffon, uncovered it and asked anatomist Jean Pierre Flourens to present it once more before the Academy. Ibid.

In doing so, I will reexamine Buffon's experimental technique and his process of hypothesis and discovery.

4.1. Buffon's experiments in context

The supposed presence of sperm in both males and females offered a solution to the deep-rooted debate between spermists and ovists. Whether the life force resided in male semen or a woman's ovaries was irrelevant to Buffon because he considered spermatozoa to be genderless. From his microscopic observations, coupled with later breeding experiments, Buffon formulated a simple dual-sex theory of generation, which served as supporting evidence for his greater theories of natural history. Following Leeuwenhoek, Buffon called spermatazoa 'animalcules' but not in the sense that they were living micro-organisms: they were, according to Buffon, little machines which transported the actual organic seeds of life. Buffon theorized that 'organic molecules' (as present within spermatozoa) swam in the animal bodily fluids and carried the physiognomic identities of each parent into the womb. These particles combined through the attraction of sperm to produce healthy offspring consistent to the template of an 'interior mould' (a term Buffon appropriated from the ovist-devotee Louis Bourguet (1678-1742).[4]

According to Jacques Roger, this interior-mould idea represents a Kuhnian paradigm shift in the study of embryology. Buffon's notion of the interior-mould was not preformationism under a new name. Prior experimenters (both spermists and ovists) had employed the microscope in search of a preformed homunculus. Notably, Marcello Malphigi (1628-1794), Nicolas Andry (1658-1742) and Gautier d'Agoty

[4] J. Roger, *Buffon: A Life in Natural History*, translated by S. Bonnefoi (Ithica, NY, 1997), p.129. See Buffon, 'De la Formation du Foetus', *Histoire Naturelle*, ii, pp.324-65.

(1716-1785) had all searched in vain for the microscopic preformed embryo.[5] In contrast, Buffon's 'interior mould' was a blueprint, not a visible object. Buffon argued that these moulds reflected the composition of both parents; their offspring would possess separate parts of this mould as each of the mould's organic particles corresponding to tissue from every parental organ. In other words, these organic particles combined to form a fetus resembling each parent's 'interior mould'.[6] Buffon added a Newtonian force of attraction to explain two essential principles that governed these organic particles: first, a 'penetrating force' imprinted the parents' morphology into the new interior moulds; then, in the womb, the natural forces of attraction among spermatozoa merged the mould-carrying organic particles into a new fetus.[7] With his theory of dual sex generation, Buffon boldly distanced himself from both spermists and ovists. On the one hand, he objected to the homunculi theory popular among early eighteenth-century spermists; on the other hand, he deemphasized the matriarchal role bestowing vitality to the fetus. In place of these ideas, Buffon's interior-mould theory explained the organization of the embryo with respect to both sexes, relying on purely mechanical rules of movement and attraction. However, as Peter Bowler notes, Buffon's interior-mould idea offered only an

[5] Clara Pinto-Correia has penned a quite enjoyable book on this subject of early modern preformationism. C. Pinto-Correia, *The Ovary of Eve: Egg and Sperm and Preformation* (Chicago, 1997). See also J. Needham, *History of Embryology* (New York, 1959), chs.3-4.

[6] Historians of biology must take care not to confuse this theory with Darwin's later notion of pangenesis. Darwin often defended himself from accusations that his notion of pangenesis merely copied Buffon's idea that the animal 'interior mould' shedded reproducing 'organic molecules'. In a letter to Huxley, Darwin noted: '[T]here is a fundamental distinction between Buffon's views & mine. He does not suppose that each cell or atom of tissue throws off a little bud; but he supposes that the sap or blood includes his 'organic molecules', *which are ready formed*, fit to nourish each organ, & when this is fully formed, they collect to form buds & the sexual elements'. Darwin, C. R. to Huxley, T. H., (17 July 1865). *Darwin Correspondence Project* (Cambridge, UK: University of Cambridge) letter 4872. <http://www.darwinproject.ac.uk/darwinletters/calendar/entry-4872.html> Mary Terrall provides a deeper discussion of Buffon's defense of the 'interior moulds' theory and its context in eighteenth-century naturalism. M. Terrall, *The Man Who Flattened the Earth: Maupertuis and the Sciences in the Enlightenment* (Chicago, 2002), ch.10.

[7] Buffon first uses the terms 'moule intérieur' and 'forces pénétrantes' throughout vols. ii of the *Histoire naturelle* to discuss the general development and organ structure of mammals. His ideas on penetrating forces in relation to organic molecules and reproduction appear in greater detail in vol. ii, ch.iv, pp.53-72.

alternative, not a substantive critique of the theory of pre-existent germs. It could not fully explain the organization of morphological traits within an offspring.[8] Indeed, Albrecht von Haller (1708-1777) attacked Buffon's theory for precisely this reason: Haller censured Buffon for his inability to explain how purposeless 'penetrating forces' could replicate all the organizing features of a parent and catalyze fetal resemblance of both parents.[9]

Buffon certainly understood the flaws undermining his generation theory. He did attempt to incorporate 'external' forces into his process of generation which could reconcile these incongruities: Buffon reasoned that environmental agents affected the organic molecules, engendering differences in organization and growth through their influence on the interior moulds. In the mother, the uterine environment and temperature could act upon the stages of fetal growth and later infant morphology. According to Buffonian generation, the commixture of sperm from both the male and the female parent bred life so long as the womb remained a warm and stable environment. While the microscopic mingling of two sperm spawned an embryo, the outer environment supposedly influenced its future growth from the moment of conception. Buffon also wrote that environmental agents manifested into reproducible characteristics of the male and female parent. Like his classical models, Hippocrates and Aristotle, Buffon presumed the inheritance of acquired characteristics. He argued that parental traits descended into interior moulds within the spermatic machines, and from those machines, into the fetus.[10] He posited that disparities occurred among

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[8] P.J. Bowler, 'Bonnet and Buffon: Theories of Generation and the Problem of Species' *Journal of the History of Biology*, 6 (1973), p.268.

[9] Emma Spary briefly touches upon the Haller-Buffon debate in her larger assessment of naturalist 'economies' of inheritance and growth. E. Spary, 'Political, Natural, and Bodily Economies,' in N. Jardine, J. Secord, & E. Spary (eds.), *Cultures of Natural History* (Cambridge, 1996), p.186. Haller published his major critiques of Buffonian generation in, *Réflexions sur le système de la generation de M. de Buffon*, (Geneva, 1751). Haller published an alternative theory of chicken embryology in 1758, in which he asserted that the vital organs of a chicken embryo preformed in an egg prior to copulation. A. Haller, *Sur la formation du coeur dans le poulet* (Lausanne, 1758).

[10] Buffon, *Histoire naturelle*, ii, pp.43-5.

species because broad environmental effects upon spermatozoa easily transmitted to successive generations. Nevertheless, this theory of environmental influence only evaded the issue of embryo development and morphology. Buffon never explicated how parental traits organized themselves within an offspring. He seems to have abandoned the matter as he explored the diversity of these species in the later volumes of his *Histoire naturelle*.

Whether or not Buffon's entire series of microscope experiments came on the heels of prior assumptions or served to initiate these new theories is a matter of great debate. To a present historian looking back through the shadow of the great traveler Charles Darwin, Buffon may resemble a sedentary naturalist who presented otiose theories more often than conclusive evidence. However, while many of his contemporary naturalists limited themselves to breeding animals or observing organisms from a distance, Buffon dissected and vivisected his animals, cutting them apart attempting to discover the anatomical and histological essence of what could make one species truly separate from all other species. As intendant of the Jardin du Roi, or while at his estate in Montbard, Buffon keenly observed both microscopic life and animal behavior. Furthermore, he employed a vast interdisciplinary approach in his experiments. Buffon used the latest techniques, the most advanced technology, and the language of microscopy and the new mathematics in an endeavour to understand life forms and the organization of species.

4.2. Buffon and Needham's methodology

Let us examine Buffon's microscope experiments on sperm in greater detail. Because Buffon classified species based on their ability to reproduce, he clearly needed to investigate the fundamentals of reproduction on the spermatic level. He found the opportunity to do so after meeting John Turberville Needham (1713-1781) in 1748. Needham, a Catholic priest and expert builder of microscopes, is most often remembered for his experimental efforts to prove spontaneous generation.[11] The present chapter sheds light on his correlating studies of human and animal generation. Needham had recently published an authoritative study of microscopic particles, and had made meticulous observations and illustrations concerning the seminal fluid of calamari.[12] Shirley A. Roe elaborates on Needham and Buffon's relationship in her article 'Buffon and Needham: Diverging Views on Life and Matter'.[13] Roe concludes that Buffon read his theory of generation to Needham, but it was Needham who plotted their microscopic work, 'after all it was Needham who was experienced with examining the microscopic observation on the reproductive vessels of squid'.[14] I have found, in Baker's correspondences, that Needham's observations of spermatozoa were under suspicion by his English colleagues. Although Baker praised him in the *Philosophical Transactions* for identifying anatomical features in calamari,[15] in private letters to Henry Miles, Baker cast doubt on Needham's microscope observations. Notably, Baker refuted Needham's speculation that human sperm was identical to the spermatic fluid, or 'milt' in calamari and fish.

> Mr. Needham's book is full of ingenious discoveries and is extremely just in the drawing san descriptions. But there are some mistakes in his Reflections, and particularly where he supposes the animalcules in *Semine Masculis* to be machines like the milt vessels of the calamari.[16]

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[11] S.A. Roe, 'John Turberville Needham and the generation of living organisms', *Isis*, 74 (1983), pp.159–184. M. Ratcliff, *Quest for the Invisible*, pp.128-136. See also, R. Mazzolini and S.A. Roe, *Science against the Unbelievers, The Correspondence of Charles Bonnet and John Turberville Needham 1760–1780* (Studies on Voltaire and the Eighteenth Century, 243, Oxford, 1986).
[12] J.T. Needham, *An Account of Some New Microscopical Discoveries Founded on an Examination of the Calamary and its Wonderful Milt-vessels* (London, 1745); also, Buffon, *Histoire naturelle,* ii, pp.171, 174.
[13] In J-C. Beaune et al., *Buffon 88: Actes du colloque international pour le bicentenaire de la mort de Buffon* (Paris, 1988), pp.439-50.
[14] Ibid., p.441.
[15] H. Baker, 'An Account of the Sea Polypus', *Philosophical Transactions*, 50 (1758), p.782.
[16] JRL, GB 133 MS 19 (Henry Baker Correspondence), vol. 3, p.134: Henry Baker to Henry Miles, 3 Sep. 1747.

The milt vessels in question are small capsules (now known as 'spermatophores') which encapsulate smaller spermatozoa. Needham had claimed that human sperm were, in fact, such larger spermaphores: animalcule-like machines which housed the actual seeds of generation. Baker wrote, 'The truth is Mr. Needham had never seen the animalcules in [human] semen as he confessed to me'. [17] According to Baker, Needham did so 'to please those of the Roman Church' and when Baker showed him the milt of a gudgeon fish under his microscope, Needham 'seemed to alter his way of thinking'.[18]

Why the Catholic Church would prefer that human generation relied on spermatophores or not was beyond Baker's concern and the writings of Needham do not shed light on the subject. It is possible that Baker had simply attempted to belittle Needham's religion, although not his microscopical expertise, in order to keep the good graces of Henry Miles, who was a dissenting Protestant minister. Miles had frequently written to Baker that he could not see the aquatic animals using a Cuff microscope that he had purchased through Baker.

Needham's trip to France may thus have been an attempt to recreate his experiments under Buffon's patronage in order to prove himself an authority on human spermatazoa. Although he may have acted only as an observer to Needham’s microscopy, as we shall see, Buffon later put those observations to further use in presenting a grander theory of generation and degeneration. Eventually, the team would conclude that spermatazoa were machine-like vessels which did indeed contain the actual smaller seeds of generations.

Buffon and Needham dissected and vivisected numerous animals, and for Buffon, the more disparate the creatures the better. Buffon was not the most prodigious dissector of the eighteenth century, but through his inherited wealth, his

[17] Ibid.
[18] Ibid.

global networking, and his position at the Jardin du Roi, he had a multifarious collection of animals at his disposal. As for Needham, the poor priest undoubtedly realized that loaning his microscopes was a small price to pay for the resources of Buffon. The Comte provided him with much more than squids for his dissection table; According to the *Histoire naturelle,* the team dissected dozens of species types in search of spermatozoa, including: two men, several fish, three male dogs, two female dogs, one old cow, one young cow, six non-descript cows, thirteen rabbits, eighteen rams, and several human cadavers.[19] These human corpses were almost certainly recently executed convicts. Buffon wrote:

> Having Procured the seminal vessels of a man who had died a violent death, and the whole body was still warm, I extracted all the liquor from them, and placed it in a vial.[20]

According to Buffon, they found no great dissimilarities among the sperm characteristics of any of these species. Opening one of the human bodies, Buffon and Needham found sperm in varying states of cooling. They saw large filaments in the warmer fluid (see Fig. 1, subfigures 1-4), and as the fluid cooled, they observed the enigmatic 'spermatic animals' and 'organic particles' (subfigures 5 and 6). Buffon and Needham recorded similar organic particles in another male human body. They allowed this sperm to cool further, and found that as it did, the spermatic bodies slowed dramatically. The team witnessed the cooling of the sperm over several days until the cell bodies decayed and their remnants coalesced into globules (Fig. 2, subfigure 12).

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[19] Buffon, 'Expériences au sujet de la generation' *in Histoire naturelle* (1749) ii, ch.6.
[20] Ibid., p.176.

FIGURE 1

[From Buffon: *Histoire Naturelle* (Reprint, London, 1812), plate 9, 492b]

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FIGURE 2
From Buffon: Histoire Naturelle (Reprint, London, 1812), plate 10, 500b]

Buffon, Needham, and several occasional collaborators, including Louis Daubenton (1716-1799), spent many long hours patiently watching semen dry. Their efforts would eventually pay off: observing the coalescence and decay of cooling sperm, they learned much about the effects of time and temperature on even the smallest particles of life. These detailed microscopic observations of sperm provided microscopic evidence of Hippocrates' theory of climatic affect. Observing sperm bodies disintegrating and losing their flagella as they cooled, Buffon concluded that sperm suffered degenerative effects in cool and inclement weather. In later volumes, he described unhealthy sperm as the agent of degeneracy for the entire human species.

Buffon turned to Newton for a calculable framework for his theory of spermatic degeneration. An early advocate of Newtonian mathematics, Buffon published an analysis of Newton's physics the same summer he conducted his microscopic experiments.[21] Earlier in his career, Buffon's defense of Newton's celestial mechanics had made him an adversary of the Académie des Sciences. He never doubted that gravity, in conjunction with motion, kept the planets in their orbits (an affirmation that contributed to his rocky relationship with the faculty at the Sorbonne). Aligning his microscopic observations with Newtonian mechanics was another first step in reproductive science. While conducting his experiments with Needham, he invoked the language of Newtonian mechanics to describe the attraction of microscopic spermatozoa.[22] Just as a planet's centripetal force directs its constant motion, the viability of a spermatozoon necessitated continuous movement. According to Buffon's experimental record, once sperm cooled, their motion ceased, which then caused them to attract into an agglutinate demise (see Fig. 2, subfigures 10 and 11):

[21] Buffon, 'Réflexion sur la loi d'attraction', *Mémoires de l'Académie Royale des Sciences* (1748).
[22] Buffon asked, 'thus does not one want to admit forces penetrating and acting on these masses of the bodies, since after all, we have other examples of gravity within the body, in the magnetic pull, in chemical affinities?' Buffon, *Histoire naturelle,* ii, p.61.

> At the twelfth and thirteenth hour, the bodies were visibly smaller; but, as they diminished in bulk, their *specific gravity* increased, especially when they ceased to move, which they generally did all at once ... when their action ceased they coalesced at the bottom of the liquor and formed there a sediment that was the colour of ash.[23]

Buffon's microscopic experiments sustained mechanical explanations over vitalistic notions of generation. Nature, from the planet to the smallest sperm, appeared to obey mechanical laws. There is no metaphysical vital force in Buffon's natural history of sperm and species. In addition, his dissections and microscopic observations diminished the argument that man possessed a unique natural history different from that of other animals. He and Needham found nearly identical sperm among almost all of the animals that they dissected (Fig. 3). These spermatic particles were susceptible to the same environmental conditions that affected human sperm; thus, they endured the same vitiating force of *dégénérescence*. With the loss of heat, they too decayed and coalesced at exactly the same rates.

Because the preservation of heat was crucial to the continuous movement of sperm, Buffon and Needham needed to keep their specimens from cooling. For this, they had turned to vivisection. Cutting open several live dogs, they examined the liquid taken from around the ovaries and testicles, then returned those organs back into the living bodies to retain their heat.[24] The naturalists also found what they believed to be spermatozoa in female dogs, even in those dogs that had never copulated (Fig. 4, subfigures 20 and 21). As stated, they assumed that some error had contaminated the microscope slide:

> Mr. Needham, therefore changed both the object-glass and the pick-tooth and took a fresh drop from the fissure of the glandulous body, and examined it with his own eyes. He again saw the exact same moving bodies, and was duly convinced.[25]

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[23] Ibid., ii, pp.186-87.
[24] See experiments 26 through 33. Ibid., pp.202-10.
[25] Ibid, p.203.

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FIGURE 3
From Buffon: ***Histoire Naturelle*** **(Reprint, London, 1812), plate 11, 512b]**

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FIGURE 4
From Buffon: *Histoire Naturelle* (Reprint, London, 1812), plate 12, 512b]

The ubiquitous presence of sperm-like animalcules invigorated debates as to their exact function. Were sperm the foundations of life, or were they parasites floating in the human body? If they did generate life, were they mere vessels for some other little animalcules (as in calamari), or did they somehow transform themselves into an embryo or combine with other sperm to do so? Buffon's view of female sperm added complexity to this issue although this was not the first time that naturalists had claimed to see female sperm. Leeuwenhoek made similar observations in his first microscopic experiments over half a century earlier although critics such as Martin Lister (1639-1712) had cast doubt on Leeuwenhoek's claim.[26] Buffon's placement of 'female' spermatozoa directly within the ovarian follicle offered substantial evidence for Leeuwenhoek's dual-sex animalculism. In addition, Buffon's mistaken observation of ovarian 'spermatozoa' greatly distanced his position from that of Jan Swammerdam (1637-1680) and Regnier De Graaf (1641-1673) who had first observed that the follicle contained an oocyte (De Graaf never had the opportunity to prove this claim with the microscope; he himself believed that the entire follicle structure represented the egg[27]). While Leeuwenhoek preceded Buffon in his belief that the female body contained sperm, his simple microscope advanced the study of spermatozoa (and thus animalculism) to great heights by the early eighteenth-century, Leeuwenhoek had discerned extraordinary details, such as the tripartite structure of male spermatozoa. The master of microscopy had seen enough detail to debase the argument that sperm contained complete homunculi. Leeuwenhoek often vacillated on his own position of preformationism: although he firmly believed that sperm contained other minute structures (like Needham would later affirm in calamari), Leeuwenhoek was not able

[26] M. Lister, 'An Objection to the New Hypothesis of the Generation of Animals from Animalcula in Semine Masculino,' *Philosophical Transactions,* 20 (1689), p.337.
[27] J. Needham, *History of Embryology,* pp.163-65, 180.

to say with certainty if the matter within sperm was a preformed embryo or some part of it.[28]

In his 1748 account to the Académie des Sciences, Buffon took Leeuwenhoek's observations to task, claiming that Leeuwenhoek's had overzealously enlarged his illustrations of spermatozoa. Buffon claimed that Leeuwenhoek's microscope magnified sperm twenty-five thousand times (in actuality, the figure was two hundred and fifty times). In contrast, Buffon claimed that his double-lens microscope enlarged one-hundred and sixty thousand times.[29] These are both obvious exaggerations: the average microscope of the time could barely reach above three hundred power and, for functionality, likely was much less.[30] John Needham's presence also appeared to triumph the legacy of Leeuwenhoek as Needham had access to Leeuwenhoek's microscopes which the Dutchman had willed to the Royal Society. Needham found Leeuwenhoek's collection of scopes inferior to his own newly-purchased microscope. Buffon wrote: 'M. Needham assured me that the best of them do not have as much effect as the strongest lens that I myself used, and with which I made all my observations.'[31]

Despite Buffon and Needham's claim that their microscope surpassed those of Leeuwenhoek, the later experimenters more or less confirmed Leeuwenhoek's original supposition that spermatozoa existed in both males and females and that these sperm were vessels containing some other components. Following their detailed observations, Needham and Buffon concluded that spermatozoa acted as mechanical containers for the 'interior moulds' of life: a thesis that seems to have vindicated

[28] A. Leeuwenhoek, *The select works of Antony Van Leeuwenhoek: containing microscopical discoveries in many works of nature*. translated by Samuel Hoole (New York, 1977). A.R. Hall, 'The Leeuwenhoek Lecture, 1988: Antoni van Leeuwenhoek 1632-1723', *Notes and Records of the Royal Society of London* 43 (1989), pp.260-61.
[29] Buffon, 'Découverte de la liqueur séminale dans les femelles vivipares, et du réservoir qui la contient', pp.227-8.
[30] See J.B. McCormick, chs. 6 and 7.
[31] Buffon, *Histoire naturelle,* ii, p.172.

Needham's earlier assumption. They based this argument, not on their observations of sperm movement, but on the sperms' degeneration and decay. As Buffon argued, spermatozoa could not be whole animals in themselves, for when sperm cooled and the motion of the bodies decreased, he could see that the spermatic bodies completely broke apart or coagulated into oblong and round masses (Fig. 2, subfigures 9, 10, and 11). 'When an animal, whatever be its species, dies, it does not suddenly change its form; from being elongated like a thread, it does not become round'.[32] Furthermore, he reasoned that sperm could not be alive: the degeneration of sperm had not 'the slightest resemblance to the nature of an animal; but, on the contrary, the whole appears to be a species of machines which burst and empty themselves after having performed their functions'.[33] Thus, spermatozoa were vessels to some other structure.

Buffon and Needham continued experimenting, finding what they believed were identical male and female sperm in sheep, rams, cows and even cuttlefish. In this international collaboration, two leading naturalists had witnessed millions of sperm particles that appeared nearly inseparable across gender and species lines. Buffon, like Leeuwenhoek recognized that microscopic 'animalcules' existed in all parts of the body. However, unlike Leeuwenhoek, Buffon stressed that naturalists could use sperm particles to measure health and reproductive capability. His and Needham's most groundbreaking hypothesis equated reproductive ability with organism fitness. Extreme bodily temperatures could decay spermatozoa prematurely, or compel the body to jettison sperm altogether leading to sterility. Leeuwenhoek's wrote that he had observed spermatic bodies in his own feces while suffering from sickness. 'Hence we may conclude', Buffon stated, 'that people who are troubled with looseness of stool

[32] Buffon, 'Réflexions sur les expériences précédentes' *Histoire naturelle,* ii.
[33] Ibid. p.234.

should have less seminal liquor, and be less fitted for the purposes of generation, than those of a contrary habit of body'.[34]

After witnessing male (and what he believed to be) female sperm from his broad range of species, Buffon then posited that the laws of attraction must govern not only microscopic movement, but also the generation of all manner of life. Male sperm, he argued, could not combine their contents into a fetus because male sperm was in an ever-present state of motion in the testicles. However, the cooler and more hospitable environment of the womb slowed male and female sperm, increasing the sperm's specific gravity, and catalyzing the growth of the fetus. [35]

Buffon cited birth defects as further evidence of the mechanical existence of male and female sperm. After observing the decay of sperm during its coalescence, he argued in 'La Formation du Foetus' that birth defects and infant-like 'moles' were either the product of slower sperm or perhaps the over-dominance of one parent's sperm over the other during the sperms' combination.[36] This, of course, could result from an inhospitable bodily temperature. Furthermore, Buffon maintained that women, and perhaps even men, could produce human-like 'organized masses' (failed offspring) in their genitals *sans coitus.*

> I acknowledge that I am prone to imagine that in certain circumstances and certain states the seminal liquor of a male or female can sometimes produce something. I will be, for example, extremely tempted to believe that girls can make moles without have had communication with a male, just as the hens make eggs without having seen the cock.[37]

[34] Ibid., p.240.
[35] Ibid., p.326-9.
[36] Prior to Buffon, William Harvey and René Descartes had theorized that the male seed alone supplied either reason or the soul. Buffon hypothesized that both parents could lend characteristics to the fetus, one parent perhaps more than the other, in the case of 'monstrous births', one parent may have lent too many characteristics and the other parent not enough. Buffon, 'La formation du foetus'. For an in depth analysis of 'monstrosities' in relation to early embryology. C. Merchant, *The Death of Nature: Women, Ecology, and the Scientific Revolution* (San Francisco, 1983), pp.158,162. J. Needham's A *History of Embryology* is still the best source on general matters of enlightenment embryology.
[37] Buffon, *Histoire naturelle,* ii, p.361.

Buffon reported that virgins sometimes birthed tissue which contained bone, skin, and hair. He mused that such parthenogenesis could occur if the environment in those sex organs was conducive to conglomeration.[38] However, Buffon contended that because sperm from both the male and the female were necessary to the process of generation, this mass of tissue would never constitute life.[39] Reports of these 'unformed moles' affirmed Buffon's belief that gravitational forces caused the attraction of both microscopic and macroscopic bodies. Through his microscope, Buffon and Needham, had seen spermatozoa conglomerate into bulkier forms. Buffon surmised that birthed 'moles' were their larger progeny: a product of the mechanical spermatozoa accumulating in the womb due to their increasing specific gravity. Similarly, healthy fetuses spawned from the similar amalgamation of male and female sperm in a hospitable womb environment.

Again, such speculation opened the floodgates of criticism. One of Buffon's most vocal critics, René Antoine Ferchault de Réaumur (1683-1757) blasted Buffon's attempts to explain reproduction through a multitude of mechanical laws. Réaumur not only reproached Buffon for his speculation, but also his claim that sperm particles (animalcules) could reflect a parental mould. According to Réaumur, mechanical particles could not explain the complexity of a developing fetus. In *Pratique de l'art de faire éclore et d'élever en toute saison des poiseaux domestiques de toutes espèces,* Réaumur asked,

> How shall so many different organs be placed and assembled in their proper order? We see with the most glaring evidence, that in order to arrive at the formation of so complicated a piece of work, it is not enough to have multiplied and varied the laws of attraction at pleasure . . .We must not entertain any hopes of ever explaining the primitive formation of an animal, if it has been the good will and pleasure of the Supreme Being, that germs should be daily produced, which there is

[38] Buffon uses this theory to defend the chastity of women who had given stillborn births out of wedlock; Buffon, 'La formation du foetus'. Later in the volume Buffon explained that a women's hymen served no physiological purpose, and could not be used as to determine virginity.
[39] See Buffon, 'Génération', *Histoire naturelle,* ii, pp.493-5.

> room to doubt of, we ought to despair of ever knowing the means of which He makes use.[40]

The feud between Buffon and Réaumur would last decades. Their respective correspondences are full of colourful attacks and vituperative allusions. Réaumur continuously vitiated Buffon's grand theorizing, while Buffon once retorted that Réaumur was a 'pedant of the Académie'[41] and a mere 'dissector of insects'.[42] It is important to note that while Réaumur did not criticize Buffon's experimental method, he did condemn his propensity for speculation. As we have seen, Buffon was certainly not hesitant to speculate; nevertheless, as I have attempted to demonstrate, Buffon also offered substantial experimental evidence in support of his theory of generation.

4.3. Recreating Buffon's experiments

Was Buffon's experimentation seriously flawed? Did he forge his data? On the other hand, if his experiments had played out as he described, then what did he and Needham actual see when they described female sperm in ovarian follicles? It is possible that Buffon and Needham did observe *sperm-like* particles through the lens. Phillip Sloan discusses this possibility in his article, 'Organic Molecules Revisited'. Sloan theorizes that while in England, Needham had purchased a microscope of incredible power manufactured by John Cuff. Sloan theorizes that Needham provided Buffon with this more powerful screw-barrel microscope, which they then employed in these experiments.[43] He hypothesizes that Buffon may have seen small living particles, possibly even bacteria, which he overzealously mistook for female

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[40] R. Réaumur, *Pratique de l'art de faire éclore et d'élever en toute saison des poiseaux domestiques de toutes espèces* (Paris, 1749), pp.332-34.
[41] Buffon à M. Doussin (27 Jul 1751) in *Correspondance inédite de Buffon, à laquelle ont été réunies toutes les lettres publiées jusqu'à ce jour*, i, letter xlviii.
[42] Buffon à Richard de Ruffey, president of the Académie Royale des Science (14 février 1750).in ibid.
[43] P.R. Sloan, 'Organic Molecules Revisited', in J. Gayon (ed.), *Buffon 88*: *Actes du Colloque international pour le bicentenaire de la mort de Buffon* (Paris, 1988), pp.420-22.

spermatozoa. Indeed Sloan's theory seems plausible: Buffon may have been impetuous in his desire to legitimize his own work through the connection with Leeuwenhoek's alleged viewing of female sperm.

To better investigate Buffon's experiments, I set out to recreate them using microscopes in the Museum for the History of Science at Oxford as well as the Golub Collection at the University of California, Berkeley. Under these microscopes, I have examined human and animal tissue samples that I acquired from the Berkeley Anatomy Laboratory of Integrative Biology. I procured tissue samples specifically from the genitalia of dogs, cats, mice and humans.

The exact microscope that Buffon and Needham used has been a matter of some debate. Buffon merely tells us that he used a '*très-bon microscope-double*' that Needham had brought with him from London. We know that Needham was a frequent patron of Cuff's workshop.[44] Another of Buffon's critics, the preformationist Lazzaro Spallanzani, excoriated Buffon's faith in the compound double microscope. Believing that Buffon used a flawed Cuff compound microscope, Spallanzani instead lauded the simple single-lens microscopes of Leeuwenhoek.[45] In his examination of the debate, Phillip Sloan contends that Buffon may not have used a real compound scope, but a more simple screw-barrel microscope with a single magnifying lens. (In the following chapter we will see how these Wilson screw barrel microscopes were also influential in early observations of blood corpuscles.) Sloan bases this theory on the magnification values which Buffon provided in his 1748 report to the Académie des Sciences and an identical table of magnifications which Henry Baker recorded

[44] Buffon, *Histoire naturelle,*ii, p.176.
[45] Spallanzani wrote, 'I often used an instrument like that of Leeuwenhoek, consisting of a single lens which could easily and precisely observe [animalcules], including the most minuscule and fine details'. L. Spallanzani, *Saggio di osservazioni microscopiche concernenti il sistema della generazione di signori di Needham e Buffon* (Modena, 1765), p.86.

regarding the power of Cuff's Wilson screw-barrel microscope.[46] Sloan contends that such a microscope could reach enormous powers, (possibly as high as 500x). Sloan used a modern replica of this microscope and at 500x, witnessed bacteria and inorganic particles moving in Brownian motion. Could Buffon have mistaken bacteria for 'female' spermatozoa? Sloan suggests this is the case, and contends that it was Buffon, not his critics who possessed the better microscope. Sloan writes,

> I would propose instead that the observations [of Buffon] must be considered very carefully, and were probably non-reproducible by Buffon and Needham's opponents precisely because of the inferiority of their critic's instruments.

Although Buffon did in fact record that he used a double microscope instead of a simple single-lens, I have taken Sloan's general conclusion to heart, giving full credit to Buffon and Needham as genuine experimenters and not as amateurs who were prone to make claims based beyond the proof of empirical observation. With Sloan's theory in mind, I took particular interest in English microscopes of all designs, especially those crafted by the brilliant instrument maker John Cuff.

Buffon would have had the resources to equip himself with a wide range of microscopes. Among the most popular in France during the mid-eighteenth century would have been the Edmund Culpeper microscope. This tripod model was a staple among eighteenth-century naturalists even after the introduction of the Cuff design. Buffon would have likely been familiar with their use; however, the maximum Culpeper range is only about 200x or less, and this would be an unlikely candidate for Buffon's experiments. Furthermore, spherical and chromatic aberration seriously impaired microscopy throughout the eighteenth century, and the Culpeper style scope is certainly no exception.

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[46] The table presenting the power of a Wilson screw-barrel microscope appears in H. Baker, 'An account of Mr. Leeuwenhok's microscopes,' p.513. Buffon's calculations appear in, 'Découverte de la liqueur séminale dans les femelles vivipares,' p.228.

Among the other instruments tested, a Cuff-style compound microscope by George Sterrop (1715-1756) offered promise as it reached a magnification of roughly 300x.[47] This particular microscope dated from 1750, just one year after Buffon published his experiments. Again, magnification testing provided only limited results. I could not find clearly defined spermatozoa cells as it was not possible to maintain a well-focused image of seminal fluid. Nevertheless, the microscope did offer remarkable views of connective tissue and other larger structures.

Claude Simeon Passémant (1702-1769) manufactured another promising compound microscope also dating from the 1750s.[48] As 'Ingenieur du Roi au Louvre', Passémant was probably even personally acquainted with Buffon. His double-lens microscope with a screw fine-focus, Passémant's was a clear imitation of the Cuff style; however, it yielded results similar to the Sterrop scope.

FIGURE 5: The John Cuff 'double microscope'. J. Cuff, *The description of a new-constructed double microscope: in which some useful improvements are introduced: as made and sold by the inventor, John Cuff* (London, 1744), plate I.

[47] GMC: no. 55.
[48] GMC: no. 131.

Among the microscopes available for my study, the best eighteenth-century screw-barrel scope (an English model, circa 1750) reached only a magnification of 100x.[49] Additional testing on most of the remaining microscopes produced like results.

Original John Cuff microscopes produced far better results. I tested two Cuff compound microscopes, one of which was dated 1745.[50] These were identical to the one illustrated from Cuff's 1744 manual (Fig. 5).[51] This instrument surpassed most others from the mid-eighteenth century in part for its high magnification, and in part for Cuff's ability to lessen chromatic and spherical aberration.[52] The Cuff double microscope offered a number of advantages for viewing small moving particles: its solid base, stable stage, and fine-focus wheel allowed the user to focus on a sample without jostling the barrel of the scope or the specimen slide. Six objectives accompanied this particular model. The highest objective available in my study could easily resolve a field of view measuring 0.3 on a stage micrometer and it could achieve a moderate focus power up to 400x. However, even with this objective in place, I could not focus a slide of human sperm to the degree of noting anything more than the bodies of the sperm, not bacteria. I could see that the spermatozoa did cluster together in a remarkably similar fashion to Buffon descriptions (Figs. 6 and 7).

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[49] MHS Inventory 54659.
[50] MHS Inventory 10237 (undated, circa 1745). GMC: no. 18 (labeled 1745).
[51] J. Cuff, *The description of a new-constructed double microscope: in which some useful improvements are introduced: as made and sold by the inventor, John Cuff* (London, 1744),
[52] James B. McCormick, pp.141-67. See also: B. Bracegirdle, 'A Brief Survey of Microtechnique before 1830,' *A History of Microtechnique* (Lincolnwood, IL, 1987).

Nevertheless, it was difficult to focus on individual spermatozoa. The clarity of the field of view was not as sharp as Buffon had illustrated. I was, however, able to view features which resembled Buffon's description of sperm breaking apart and clustering (Figs. 8 and 9).

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FIGURE 6: John Cuff 1745 microscope (GMC: no. 18) examination of an unstained drying human sperm smear. While the microscope cold not attain complete focus due to the high power and chromatic aberration, one can still note the band of clustered spermatozoa occupying the lower half of the image and leading to the upper right hand side. '..When their action ceased they coalesced at the bottom of the liquor and formed there a sediment that was the colour of ash.' Buffon, *Histoire naturelle*, ii, pp.186-87.

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FIGURE 7: Detail from Fig. 2, subfigure 11 depicting a band of spermatozoa from a human male. Buffon, *Histoire naturelle* II, 500b. Compare also to Buffon's illustrations of female spermatozoa in Fig. 4, subfigure 22.

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FIGURE 8: John Cuff microscope (MHS: 10237) examination of unstained sperm sample at lower magnification.

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FIGURE 9: Detail from Fig. 2 , subfigure 12 depicting dried spermatozoa. Buffon, *Histoire naturelle* II, 500b.

Also using the Cuff microscopes, I viewed a tissue sample from a canine epididymis (see Fig. 10 on the following page). One could clearly witness the sperm in their mature form clustered in the epididymal tubules. It is important to note that Buffon extracted spermatozoa directly from the testicles of humans and animals, not from ejaculatory ducts or post-ejaculatory smears. It is thus not surprising that he witnessed spermatozoa clustering: sperm in the seminiferous and epididymal tubules lacks the fluid base of seminal plasma which it receives only from the seminal vesicles after passing through the vas deferens. The spermatozoa here appeared quite similar to Buffon's illustration from figures 2, 3, and 4.

Addressing the subject of 'female spermatozoa' was a more difficult problem. Buffon had extracted tissue from in and outside the ovaries of dogs, cats, and rabbits, paying close attention to the Graafian follicles on the surface of the ovaries. This is where he claimed to have isolated female sperm, He stated:

> The following experiments show that these glandulous bodies do not contain eggs, but on the contrary a seminal liquor, animated and similar to seminal liquor of the male.[53]

To reenact this experiment, I tested sample slides from cat and dog ovaries using each microscope. Again, the 1745 Cuff microscope yielded the best results. One could clearly see ovarian follicles as well as the oocytes within them. In fact, the oocyte and supporting cells within the follicle appeared quite similar to the juvenile sperm cells within the testicle seminiferous tubule.

Buffon had only rudimentary staining techniques available if any. In fact, in one experiment, he made special note that he had added no mixtures or water dilutions which could have contaminated his slides.[54] Under these conditions, could Buffon have misinterpreted the granulosa cells within the female ovarian follicles for sperm

[53] Buffon, 'Découverte de la liqueur séminale dans les femelles vivipares..'. *Mémoires de mathématique et de physique, tirés des registres de l'Académie Royale des Sciences* (1748), p.211.
[54] Ibid., p.176.

cells like those within the male seminiferous tubules? The similarities are evident if we compare figure 10 with figures 11 and 12. Examine the sperm cells within the male epididymal tubule (Fig. 10) and compare them to the granulosa cells within the female Graafian follicle (Figs. 11 and 12). I theorize that Buffon and Needham mistook the follicle granulosa cells or its oocyte for spermatozoa.

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FIGURE 10: Cuff microscope view of a stained epididymal tubule from a male canine. Mature sperm appear as the dark mass within the tubule. (GMC: no. 18)

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FIGURE 11: Cuff microscope view of a stained ovarian follicle from a female canine. The oocyte has not yet appeared, but the follicle is filled with granulosa cells. (MHS microscope: 10237)

The image originally presented here cannot be made freely available via ORA because of copyright. The image was sourced at the University of California Berkeley Golub Collection, GMC: no. 18.

FIGURE 12: Cuff microscope view of another ovarian follicle in a later stage of growth from a female canine. The oocyte appears in the centre surrounded by granulosa cells. (GMC: no. 18)

On the other hand, Buffon's writings describe similarities among individual sperm cells, not the entire follicle or tubule structures. He seldom noted details about the seminiferous tubules or the structure of the ovarian follicle; nor did he question how these bodies might produce the alleged 'spermatic animalcules'. Furthermore, I have some difficulty accepting Buffon's testimony concerning the sperm he extracted from the *male* testis. Buffon's own record indicates that he saw motile sperm, complete with tails, in the male testicular tissue. Yet, this seems unlikely as sperm cells only partially form in the seminiferous tubules of the testicle; they do not mature to stages of motility until they reach the epididymus.[55] Buffon did however observe mature and motile sperm in the vas deferens; it is thus possible that Buffon observed clusters of juvenile sperm inside the tubules of the testicle and assumed that they were moving adult spermatozoa as he had seen before in the vas deferens and ejaculatory ducts. Then, when he examined *female* dogs and observed the same cluster-like matter (the ovum and granulose cells) within the female follicle, he may have assumed these also were sperm cells.

Sloan may certainly be correct in his hypothesis that Buffon possessed a high-powered screw-barrel microscope. This scope could theoretically resolve minute moving particles such as bacteria in the tissue from his female animals. However, the microscopes I used could not reach this level of magnification or resolution (owing in part to the chromatic aberration inherent in their design.) Given the above results, I would posit that Buffon used a well-built Cuff microscope such as the 1745 type that procured the above figures. This seems likely as the 1749 edition of the *Histoire naturelle* includes this wonderful little illustration of a distinct double-barrel Cuff scope in operation during Buffon's animal experiments.

The image originally presented here cannot be made freely available via ORA because of copyright. The image was sourced at the Bodleian Library, Oxford. Shelfmark: 1 DELTA 195 (v.2).

[55] Buffon made no … ted this area of the male reproductive orga… ons of the main reproductive orga…

FIGURE 13: Frontispiece illustration from vol. II of the *Histoire naturelle,* 1749 edition, p.1. This image clearly shows Buffon peering through a Cuff double microscope.

Compare the microscope illustrated here with the 1745 cuff microscope illustrated in Fig. 5. It seems unlikely that Buffon employed a different microscope design than the one illustrated, as he raised no dispute about the inclusion of this image in his work. Moreover, Buffon's collaborator Daubenton was also a fan of the cuff-style double microscope well into the late eighteenth century. A 1791 portrait of him by Alexander Roslin depicts Daubenton at his desk beside a microscope which is identical to the one illustrated in Fig. 5.

These quibbles aside, Buffon's microscopic experiments hold an important place in the history of biological science regardless of their exact circumstances. Recreating these experiments, we discover the sheer difficulty a naturalist would face in his attempt to focus a lens on any aspect of animal generation. Regardless of his actual observations, Buffon's records offer historians one of the most detailed images of the practice of experimentation in eighteenth-century science. Buffon's writings reveal that he, like an increasing number of his contemporaries, used the new reductionistic empiricism of microscopy to legitimize their theories of naturalism. Far from differentiating organisms based on their cellular components, Buffon keenly noted *universal* forms of spermatic reproduction across genders and among all the species that he examined. He concluded that these spermatozoa each contained the 'interior moulds' of life.

4.4. Buffon's next step: breeding better animals

Buffon’s observations on sperm presented a phylogenetic problem. The further Buffon investigated spermatozoa under the microscope, the more similar the male and female reproductive organs began to appear across species lines. Faced with a problem of categorization, Buffon needed to explain how phylogeny diverged if reproductive animalcules and ontogenesis appeared so similar across species on a microscopic level. Keep in mind that Buffon’s concept of phylogeny must not be confused with the famed nineteenth-century recapitulation theory of Earnst Haeckel and his disciples. While these later scholars emphasized similarities in species embryo growth, Buffon’s notions of speciation did not depend upon such theories of recapitulation. Rather, Buffon’s notion of phylogeny rested on his theory of unique ontogenesis. As stated, Buffonian generation began with the presence of 'interior moulds'. Although Buffon believed that spermatozoa were universal structures, they apparently carried those moulds that were unique to species types.

The microscope thus revealed how the initial stages of generation (the mingling of sperm) appeared similar despite species divisions. Unfortunately, they did not divulge anything about the bodily mould. How then could a naturalist observe differences between race and species in the phylogenic hierarchy? The answer lay in their offspring. Through animal husbandry, a fertile match indicated the compatibility of two animals’ spermatozoa; therefore, if one bred two supposedly separate-species animals together, and they produced an offspring, this would indicated similarities in their phylogeny. Spermatozoa (and the moulds they transported) thus became

Buffon's measure of speciation. This is why he began crossbreeding different animals in 1751.

Buffon first crossed a she-wolf with a dog. This produced fertile offspring. He concluded that the spermatic particles of the wolf and dog must be similar and the womb of a female wolf must be identical to the womb of a female dog. Buffon surmised that the two animals were the same species because of this reproductive interchangeability. In contrast, his cross between a horse and an ass produced an offspring but an infertile one: the mule. The fact that the pair could produce an offspring at all indicated to Buffon that the horse and the ass were related species, with similar wombs, moulds, and spermatic particles; however, the odd appearance of the resulting mule, and its infertility, indicated a more dramatic physiological difference between the parent horse and the ass. Buffon concluded:

> We are thus compelled to view them as different species until there is proof that the individuals of each one of these foreign species can mix with the common species, and that they will produce other beings which can reproduce between themselves. This characteristic alone constitutes the reality and the unit of what one must call species. [56]

As he was apt to do, Buffon applied this conclusion to human beings. He cautioned those who would compare interracial sex among humans to the interspecies reproduction between the horse and the donkey.

> If the Mulatto is a true mule, he would indicate then two quite distinct species: the Negro would be to the man what the ass is with the horse; furthermore, if the White would be a man, the Negro would be an animal like the monkey, and we would have the right to think that the White and the Negro do not have a common origin; but this

[56] Buffon, *Histoire naturelle,* v, p.64. Buffon's exclusive definition of speciation has prompted modern scholars to reexamine his notion of species identity. Michael T Ghiselin identified Buffon's concept of species as an early antecedent of his and David Hull's theory of 'Individuals as Species': M. Ghiselin, 'Response to Commentary on the Individuality of Species', *Biology and Philosophy,* 2 (1987), p.208. Ernst Mayr made similar claims: E. Mayr, 'The Ontological Status of Species: Scientific Progress and Philosophical Terminology', *Biology and Philosophy,* 2 (1987), p.150. More recently, Buffon's supposed tendency to define species as individuals (rather than categorical classes) has been the subject of a debate between Jean Gayon and David Stamos: J. Gayon, 'The Individuality of the Species: A Darwinian Theory? From Buffon, to Ghiselin, and back to Darwin,' *Biology and Philosophy,* 11 (1996), pp.215-44; and D. Stamos, 'Buffon, Darwin, and the Non-Individuality of Species – A Reply to Jean Gayon', *Biology and Philosophy,* 13 (1998), pp.430-70. See also, D. Hull, 'A Matter of Individuality', *Philosophy of Science,* 45 (1978), pp335-60.

> assumption is contradicted by fact. Since all men can communicate and produce together, all men come from the same stock and are of the same family.[57]

Returning to the mule, Buffon theorized that the sperm of the horse and the sperm and donkey did in fact combine (after all, their sperm appeared identical under the microscope), but the animals' dissimilar bodies produced too much of a 'frenzy' during intercourse. This created excessive heat in the womb. While this heat was not enough to prevent all the sperm cells from converging into a fetus, those that did not survive were the more fragile sperm that had been carrying the interior moulds of the reproductive systems. Hence the fetus became half-horse, half-donkey but completely sterile.[58]

Crossing species, Buffon found that some combinations produced more offspring than others, and often more of the same sex. His crossing of an ewe with a goat produced eight offspring: six males, and two females. The crossing of the she-wolf and the domestic dog produced four – three males and one female.[59] Like the spermatozoa that were to form reproductive organs in the mule, Buffon concluded that female sperm were more fragile than male sperm. Therefore, during the hazardous conditions of crossbreeding, the sperm of the female most likely died off, resulting in an increase in the male population.[60]

Clearly, Buffon believed that environmental effects complicated this phylogeny. Climate could alter the interior moulds within the sperm, condemning the species to *dégénération*.[61] However, individual animals posed an even greater harm to the species. Violent intercourse, such as between a donkey and a horse, would

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[57] Buffon, *Histoire naturelle,* iv, 388-9.
[58] For Buffon's discussion of sterility in the mule see chapters 'Le cheval' and 'L'asne' in *Histoire naturelle,* iv. His crossbreeding experiments appear in most detail in the supplement to vol. iii.
[59] Buffon, *Histoire naturelle,* iii, supplement, p.4.
[60] Ibid., pp.210-30.
[61] Buffon, *Histoire naturelle*, iv, pp.389-91.

negatively affect the temperature of the womb, leading to sterility and eventual species extinction.

Examining sperm under the microscope, Buffon and Needham had asserted that the presence of degenerated sperm, such as in the sick Leeuwenhoek, indicated that the environment could lead to an organism's procreative failure. Buffon's interspecies breeding experiments seemed to confirm this assertion. From these, he concluded that the womb environment determined the viability of sperm particles, and the resultant health of a fetus. A poor womb environment (due to interspecies breeding, inclement weather, or hazardous sex) would vitiate an offspring's chances of survival, and endanger the entire species. This notion is the earliest scientific theory of degeneration.

For procreation, the womb temperature required regulation. Buffon argued that the excessive heat of females during intercourse could provoke sterility and degeneration even among same-species crosses. He wrote:

> The female donkey is lascivious which is the reason for her want of fecundity. She rejects the liquor which she has just received in the coupling, unless blows repress the ardor of her desire and calm the continuation of the convulsions and movements of love. Without this precaution, she is seldom impregnated . . . when pregnant, she soon becomes cool.[62]

Buffon saw mankind's intervention through domestication as a means of regulating body heat. Buffon stated that his domesticated dogs could produce between twelve and nineteen pups while wild dogs could produce only five or six. Rabbits would naturally produce only one or two litters a year in the wild; while in domestication, they often produced one litter each month. Buffon found similar results among pigs.[63]

From these experiments, Buffon theorized that a temperate climate and domestication endowed organisms with increased propagation. He argued that all

[62] Ibid., iv, p.395.
[63] Ibid., iv, pp.1-10.

species exhibited agency over their environment through their pursuit of moderation. In his microscopic observations Buffon had witnessed healthy, and thus warmer, sperm dispersing from densely populated areas of his slide to the less populated areas, while sperm in a state of cooling naturally converged. In the supplement to his *Histoire naturelle,* Buffon asserted that these same properties went to work on the macro level. He argued that organisms could induce climatic changes merely by their movement or the density of their populations. Smellie stated,

> Paris and Quebec are nearly under the same degree of latitude; Paris therefore would be as cold as Québec, if France and the adjacent countries were also devoid of men.[64]

In addition Buffon postulated that the human species could change, and must change in order to avoid degeneration. According to Buffonian science, humans could escape unhealthy environments; they could alter their sexual techniques, or breed selectively, all for the improvement of the human species. Conversely, Buffon believed that humans could easily pass on environmental detriments to produce degenerate offspring. Furthermore, he contended that even the slightest change in climate or morphology could lead to human sterility.

> That two individuals cannot produce together, one needs only some light unsuitablenesses in the temperament, or some accidental defect in the bodies of the generation in one or the other of these two individuals.[65]

Buffon's hereditary theories would find acceptance in the consciousness of nineteenth and twentieth-century scientists and social theorists. The impact of Degeneration Theory on social practice warrants still further study and is not appropriate to include in this thesis on the role of microscopy. I include it here because to understand this theory, we must look first to Buffon, his microscopy, and his experimental method.

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[64] Buffon, *Histoire naturelle*, v, supplement p.240.
[65] Buffon, *Histoire naturelle*, iv, p.389.

4.5. Conclusions

Eighteenth-century naturalists constructed nature as feminine. For many naturalists such as Buffon, the pursuit of science into nature was not a total conquest over her body, but rather, an attempt to unravel her mysteries. Buffon's experimental technique was exceptional because it blurred the gender lines of nature altogether. Buffon accumulated a wealth of microscopic evidence, and he presented his observations in more detail than had ever been recorded. Furthermore, Buffon helped advance microscopy further into the realms of anatomy and physiology. He drew attention to empirical microscopic evidence at a time when the microscope had not yet found favour as a serious tool of science. Of course, there are no female sperm as Buffon claimed; nevertheless, Buffon's natural philosophy advanced new and radical notions of mechanical, dual-sex epigenesis. Through experimentation, he encouraged his readers to question the value of sex differences, and to reevaluate the nature of species along the lines of microscopy and the act of reproduction, rather than morphology alone.

Predecessors such as Descartes, Leibniz, and Newton may have paved the way for a mechanical view of the cosmos, but Buffon and Needham brought mechanism to the investigation of humans as species. They described male sperm, female sperm, human sperm, and animal sperm as nearly identical machines. Through interspecies breeding experiments, Buffon struck at the heart of creationism. He questioned the separation of species and the distancing of humankind from animalkind. Equally important, Buffon greatly emphasized the edifying use of dissections. With his scalpel and the microscope, he reduced species to their basic principles. Pursuing natural philosophy in the eighteenth century often required incredible resources and the courage to publish a grand theory of nature. Moreover, as Buffon argued, natural philosophy demanded a sharp scalpel and a clear lens.

Chapter 5. Case study: Microscopic Investigations of Blood, 1760-1780

This chapter will highlight the importance of the simple microscope in eighteenth-century anatomy. It will examine a case study involving two microscope lenses, their users, and their observation of blood. In the 1760s, a Naples priest and anatomist named Giovanni Della Torre (1713-1782) observed blood corpuscles through the microscope. Peering through his lens, Della Torre asserted that blood corpuscles were hollow rings composed of spherical globules; they were much like tiny beaded-necklaces.[1] As evidence, Della Torre drew attention to the superior magnification of his microscope lens: itself a bead of glass or 'globular' lens. He set this perfectly-spherical lens into a Wilson screw-barrel microscope. Shortly thereafter, an eminent London anatomist named William Hewson (1739-1774) purchased an identical microscope, but with a bi-convex lens. The bi-convex lens was more common and larger than the bead-like globular lens. Sceptical of these so-called 'globules of blood', Hewson refuted Della Torre's claims in the *Philosophical Transactions* of 1773. Hewson contended that his microscope provided the only true images of blood as flattened corpuscles and not beaded rings as Della Torre claimed.[2] Hewson and Della Torre's blood debate stands as an intriguing case study; on the one hand, the observations of these two anatomists occurred nearly concurrently, yet they employed differing types of microscope lenses to investigate the same corpuscular structure of blood. Furthermore, this case study provides an important glimpse into the use of microscope in eighteenth-century anatomy because these microscope users published precise descriptions of their optical instruments and techniques.

[1] Della Torre's best known publication on the subject is *Nuove osservazioni microscopiche* (Naples, 1776). He first reported to the Royal Society in 1765. F. H. Eyles Stiles 'Extracts of Three Letters of Sir F. H. Eyles Stiles, F. R. S. to Daniel Wray, Esq; F. R. S. concerning Some New Microscopes Made at Naples, and Their Use in Viewing the Smallest Objects', *Philosophical Transactions*, 55 (1765), pp.246-70. The beaded-necklace analogy appears in this article on p.253.

[2] W. Hewson, 'On the Figure and Composition of the Red Particles of the Blood, Commonly Called the Red Globules. By Mr. William Hewson, F. R. S. and Teacher of Anatomy', *Philosophical Transactions*, 63 (1773), pp.303-23.

The origins of this disagreement were two-fold: practical, and philosophical. First, because Hewson and Della Torre used different types of lenses they were allying themselves to diverging techniques in microscopy: Second, Della Torre and Hewson subscribed to radically dissimilar philosophical views that dictated their approaches to anatomy. Giovanni Della Torre firmly believed that life was corpuscular in nature; in his version of the corpuscular philosophy, tiny spheres, or globules, were the basic component of living matter. The 'globular hypothesis', as it is now labelled[3], informed Della Torre's views on the structure and function of the entire organism: the blood globules, the seeds of generation, the composition of nerves, and so forth. In this regard, Della Torre was just one of many leading eighteenth-century theorists who sought evidence of universal globules through their lenses. Similar microscope users who subscribed to a globular or corpuscular hypothesis include Della Torre's predecessors, Leeuwenhoek and Robert Hooke and his contemporaries Buffon and Spallanzani.

In contrast to the globular hypothesis, William Hewson diffidently regarded this grand hypothesis as beyond the scope of microscopy. Although, he searched for globules in order to confirm Della Torre's experiments, his conclusions were more limited in scope. Hewson emerged from this dispute as a renowned empiricist, observing and then cautiously putting forth his claims for verification. Della Torre's critics, on the other hand, harshly reproached his method as being deductive. Thus we have a case study of two competing instruments and two competing philosophies of science.

The first half of this chapter will explore the role of the instrument itself in the practice of anatomy. The latter half will examine the importance of the globular

[3] See, for example, J.V. Pickstone, 'Globules and Coagula: Concepts of Tissue Formation in the Early Nineteenth-Century', *Journal of the History of Medicine,* 28 (1973), pp.336-56. J. Schickore, 'Error as Historiographical Challenge: the Infamous Globule Hypothesis' in G. Hon, J. Schickore, and F. Steinle (eds.), *Going Amiss in Experimental Research* (Dordrecht, 2007), pp.27-45.

hypothesis to eighteenth-century anatomy and medicine and its legacy in the nineteenth century.

5.1. A tale of two lenses: the globular lens and the bi-convex lens

Della Torre and Hewson were hardly the first anatomists to study blood under the microscope. They shared a desire to re-create past studies, particularly Leeuwenhoek's observation of blood from the previous century. Leeuwenhoek himself had reported that blood corpuscles were spherical in warm-blooded animals.[4] Many eighteenth-century microscope users assumed he had used a tiny spherical 'globule' lens to make these observations. Both these aspects of Leeuwenhoek's work were contentious in the eighteenth century. In order to publish divergent observations on blood, later anatomists such as Hewson needed to move beyond the legacy of Leeuwenhoek to form more accurate and reproducible observations. Hewson sought to highlight new instruments as well as new discoveries. Conversely, Della Torre embraced Leeuwenhoek's legacy and his spherical lenses. Della Torre touted his research as final confirmation of the effectiveness of Leeuwenhoek's type of lenses.

Outside the discipline of anatomy, eighteenth-century microscope users, such as the influential Henry Baker, also addressed the legacy of Leeuwenhoek. Baker believed that the myth of Leeuwenhoek's lenses had stifled technology and had prevented advances in new lens designs. Baker attempted to show that Leeuwenhoek's lenses were not spherical after all. He wrote in 1742:

> At the time I am writing this, the Cabinet of Microscopes left by the famous Man, at his Death, to the *Royal Society*, as a legacy, is standing upon my table; and I can assure the world, that every one of the twenty-six microscopes contained therein is a double convex lens, and not a sphere or globule.[5]

[4] A. Leeuwenhoek, *The Selected Works of Antony Van Leeuwenhoek*, p.91.
[5] Baker, *The Microscope Made Easy*, p.7.

Baker's interpretation of history was not entirely unfounded. Although Leeuwenhoek did produce spherical 'globule' lenses, he was notoriously secretive of his method of producing them; these higher-powered spherical lenses may not have been included in the donation of his collection to the royal society. Unfortunately, Leeuwenhoek's priceless collection of 26 microscopes donated to the Royal Society is now lost, having vanished from the collection sometime during the nineteenth century. However, five Leeuwenhoek microscopes are preserved at the Museum Boerhaave in Leiden and one can be found at the Utrecht University Museum. Several of Leeuwenhoek's globule lenses still survive.[6] From these it is clear that he experimented with a diverse range of both spherical and bi-convex lens types of various, albeit tiny, sizes.

Examining Leeuwenhoek's correspondence, it is clear that the seventeenth-century microscope user was not as enthusiastic about the tiny globule lens as Della Torre had assumed. In a 1699 letter, Leeuwenhoek had written in favour of the larger bi-convex lenses:

> As for myself, although glasses of extreme smallness were made by me already about 40 years ago, they are seldom used by me. In my opinion they are not suitable for making important discoveries; for this purpose those which have been ground with a bigger diameter are better suited.[7]

The problems of simple lens designs persisted into the eighteenth century. Meanwhile, rival compound microscopes grew in popularity; however, simple microscopes such as these remained the dominant observational tool among anatomists.[8] Long after

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[6] For more on Leeuwenhoek's surviving lenses see B.J. Ford, 'The van Leeuwenhoek Specimens', *Notes and Records of the Royal Society*, 36, no 1 (1981), pp.37-59.

[7] Leeuwenhoek, unaddressed note dated 9 June, 1699 in *Alle de Brieven van Antoni van Leeuwenhoek* edited by L.C. Palm (12 vols., Amsterdam, 1989), xii, p.720. Also quoted in M. Fournier, 'Personal Styles in Microscopy: Leeuwenhoek, Swammerdam and Huygens' in M. Generali and M. Ratcliff (eds.), p.220

[8] See, for example, L. Spallanzani, *Nouvelles recherches sur les découvertes microscopiques et la génération des corps organisés* (Paris, 1769); H. Miles, 'Some Remarks concerning the Circulation of the Blood, as Seen in the Tail of a Water-Est, through a Solar Microscope, by the Revd. Mr. Henry

Leeuwenhoek, the debate raged on concerning the quality of bi-convex lenses versus globular lenses. Furthermore, opticians had begun to manufacture more complex and efficient microscope barrels to encase these lenses. In the present case study, our two anatomists, Della Torre and Hewson, used different lenses fitted into the same microscope body: a widely-used instrument called the Wilson screw-barrel microscope.

The Wilson screw-barrel microscope, or 'pocket microscope' was a preferred instrument among naturalists and anatomists for a number of reasons: it was simple, portable, and easily copied or modified. Perhaps most importantly, opticians and their patrons regularly advertised its effectiveness. Among those who endorsed the model was Baker. In correspondence from this period, microscope users typically refered to Baker's microscope manuals and their copper-plate illustrations in order to describe the instruments used in any given observation. This is why Della Torre and Hewson's microscopes were known to one another and fellow researchers despite the fact that the two worked at opposite ends of Europe and had never met.

Equally important, Baker's role as a patron to London opticians greatly advanced microscope designs in the middle of the eighteenth century. His renowned craftsman, Cuff, may have been better known by his contemporaries for his improvements to the compound microscope, but he also manufactured simple single-lens instruments such as James Wilson's screw-barrel design.[9] Baker illustrated the screw-barrel microscope in such detail (Fig. 1) that any aspiring optician likely could

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Miles; Communicated in a Letter to Mr. John Eames, F. R. S.', *Philosophical Transactions*, 41 (1739), pp.725-29.

[9] James Wilson was an optician in London's Hatton Garden during the late 16th century. Wilson described his screw-barrel microscope in a letter to the Royal Society in 1702, along with an account of observations made upon spermatozoa, pollen, and capillaries. Wilson himself conceded that his design borrowed very heavily from Nicolass Hartsoeker's 1694 screw microscope. J. Wilson, 'The Description and Manner of Using a Late Invented Set of Small Pocket-Microscopes Made by James Wilson'. *Philosophical Transactions*, 23 (1702), pp.277-88. Alfred Disney found that the screw-barrel design predated even Hartsoeker, and originated with Giuseppe Compani in 1686. A.N. Disney, *Origin and Development of The Microscope as Illustrated by Catalogues of the Instruments and Accessories in the Collections of the Royal Microscopical Society* (London, 1928), p.174.

have copied its design. Moreover, it sold widely. Leading craftsmen often marketed some variant of the design throughout Europe. Opticians who produced versions of the Wilson screw-barrel include Cuff, George Sterrop (1715–1756), George Adams, Benjamin Martin, and Edmund Culpeper (1670–1738) according to inscriptions on surviving instruments. These craftsmen frequently modified the design to appeal to the needs of various practitioners.[10] Single lens microscopes were generally less costly and easier to construct than compound microscopes. According to instrument historian Maurice Daumas, they were also optically equal, if not superior, to compound microscopes.[11] Unfortunately, Daumas does not elaborate on whether or not the simple microscope was superior in that it had a higher resolution and magnification or if it had a lower aberration than a compound microscope. Evidently, microscope users found them preferable to more complex designs. As Alfred Disney of the Royal Microscopical Society explained, 'The period from 1740 onwards saw the screw-barrel type of microscope, designed by Wilson, at the height of its popularity.'[12]

[10] M. Daumas, *Les instruments scientifiques*, p.332. See also, A.V. Simcock 'Edmund Culpeper and the Screwbarrel Microscope', *Bulletin of the Scientific Instrument Society*, 88 (2006), pp.1-10.
[11] M. Daumas, *Les instruments scientifiques*, p.161.
[12] A. Disney, p.174.

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FIGURE 1: Baker's illustration of the Wilson screw-barrel microscope. Note: the microscope is upside down in this image. From Henry, Baker, *The Microscope Made Easy: or The Nature, Uses and Magnifying Powers of the Best Kinds of Microscopes* (London, 1743), plate 1.

Compared to compound microscopes, the single-lens Wilson screw-barrel microscope was relatively simplistic. They were lightweight and small (typically 8 x 3cm) yet are robust and easily transported, thus allowing users to easily ship and trade them.

The Wilson model also introduced several trend-setting designs. The single magnifying lens (Fig. 1, K) could be fastened and unfastened at point G. This allowed the user to utilize any range of lenses, or to create and apply his or her own lenses. The success of this model also lies in its ease of use. In order to focus the microscope one simply had to rotate screw-barrel C into casing A. To stabilise the sample, a steel spiral spring maintained tension between the lens and the barrel. The user prepared samples in an ivory or brass slider with glass, mica, or talc covers (M). The user inserted the slider into a slot in the middle of the microscope (near EE). These sliders became popular facets of the eighteenth-century microscope trade. Craftsmen such as Cuff often included sample slides with the purchase of the instrument. In his guidebook, *The Microscope Made Easy*, Baker encouraged his readers to make and trade their own slides. He also outlined several experiments that the amateur naturalist cold conduct using pre-made slides, no doubt purchased from his and Cuff's workshop.

Inadequate lighting was the major limitation to this design. Light could pass through an aperture that, in theory, would intensify light from beneath the object. Unfortunately, this was always problematic, especially when the user employed a lens of higher magnification, or attempted to examine an especially dark sample. Baker described the challenges of lighting his samples properly whether by candles or direct sunlight; he himself preferred candles.[13] To improve lighting, Cuff manufactured a concave reflecting mirror, or speculum, which could direct sunlight through the aperture when placed on a rotating axis.[14] The limitations of the screw-barrel microscope prompted a number of other innovative modifications. To stabilise the scope, Culpeper had modified his microscope to fit a tripod stand. He was also the first to attach a second optical tube containing two lenses to the simple microscope, thereby rendering it a compound microscope.[15] Cuff also explored this modification, although his patron Baker did not endorse Culpeper's compound lens attachment as an improvement to the design likely because of the intense spherical aberration it produced. However, Cuff's greater achievement was to elevate the screw-barrel

[13] H. Baker, *The Microscope Made Easy*, pp.54-6.
[14] J. Cuff, *A Description of a New Invention*, plate II.
[15] M. Ratcliff and M. Fournier, 'Abraham Trembley's Impact on the Construction of Microscopes', in D. Generali and M. Ratcliff (eds.), p.107. Ratcliff and Fournier note that John Cuff appropriated Culpeper's notion of adding a compound attachment in his own construction of a dual simple-compound microscope. Cuff did not shy away from copying the designs of other craftsmen and naturalists. As Ratcliff and Fournier argue, his successful Cuff-Ellis microscope copied a design originally discussed by Cuff's customer, the naturalist Abraham Trembley. This Cuff-Ellis scope was the first 'aquatic' microscope: a microscope that featured a moveable magnifying lens.

microscope on a side pillar with the speculum placed beneath the scope. This design became enormously popular (Fig. 2).

The image originally presented here cannot be made freely available via ORA because of copyright. The image was sourced at the Museum for the History of Science Library, Shelfmark: E/BAK, plate II.

FIGURE 2: Cuff's modifications to the screw-barrel microscope. Note the correct position of the mounted microscope. Henry, Baker, *The Microscope Made Easy* (London, 1743), plate II.

This frequently-cited image[16] is Baker's rendition of a screw-barrel microscope as made by Cuff. Cuff has affixed to the instrument to his side pillar stand (A) with the speculum (*f*) reflecting upwards into the microscope. These modifications intensified the source of light while freeing the user's hands. This was an especially important innovation for observers who needed to manipulate their sample slides; for example, if the sample slide needed stirring or the addition of water and dyes. As we shall see, this model of the screw-barrel microscope made more advanced observations of blood possible. Investigators such as William Hewson, Baker, and

[16] See, for example, Phillip R. Sloan's reproduction of it. P.R. Sloan, 'Organic Molecules Revisited' in *Buffon 88*.

Giovanni Della Torre could more easily mix blood samples with various other substances (water, saline, plasma, etc.) or stir the sample without setting the microscope down or losing focus.

Slight variations led to radically different observational claims. This becomes clear if we examine the modifications of Della Torre compared to those of his rival Hewson. Both observers used the Wilson screw-barrel microscope, both used the side pillar and mirror which Cuff had added, and neither utilised Culpeper's compound lens attachment. However, the two microscope users employed different lenses. Della Torre inserted a spherical lens, also known as a 'globular' lens and Hewson used a larger bi-convex lens.

First let us examine the observations of Giovanni Della Torre, first reported to the Royal Society in 1765[17], and later published as a separate treatise in Italian in 1776.[18]

5.2. The investigations of Giovanni Della Torre

Della Torre's use of the English-made screw-barrel microscope would not have been novel in Italy by the 1760s. As we have already seen, it was widely popular across Europe, and the Wilson screw-barrel model appears to have arrived early in Italy. Historian of science Francesco Luzzini has uncovered a strong network of trade between London manufacturers and Italian naturalists during the early half of the century. Luzzini has discovered that Padua microscope user Antonio Vallisneri purchased several London-made Wilson screw-barrel microscopes as far back as 1713.[19] Examining scientific and medical texts from this period, we find that the

[17] F. H. Eyles Stiles, pp.246-70.
[18] G. Della Torre, *Nuove osservazioni microscopiche.*
[19] Valisnera discussed these purchases in his correspondences with other continental natural philosophers, including the mathematicians Louis Bourguet and Jacopo Riccati. See F. Luzzini,

instrument had become prevalent among Italian anatomists and naturalists by the middle of the century. Many of Della Torre's contemporaries, such as the anatomists Giuseppe Magni[20] of Pavia and Felice Fontana of Florence,[21] also recorded using the Wilson screw-barrel.

Della Torre himself claimed familiarity with a number of other French and English microscopes, both simple and compound. In the preface to his 1776 treatise, he mentioned using instruments from the workshops of Claude Passemant, John Marshall, George Adams, Wilson, Cuff, Culpeper and John Dollond.[22] This list established Della Torre's reputation as an experienced microscope user. He then described having drawn upon these various designs while making his own modifications. In doing so, Della Torre realised the limitations of the latest compound microscope models; he settled for the single-lens variety.

> After a long series of observations and comparisons and after many experiments, I have long found more use for the simple microscope than for the composite during observations.[23]

More precisely, Della Torre decided to use a globular lens set in the simple Wilson screw-barrel microscope.[24] The single globule lens would supposedly allow for greater magnification.[25]

Della Torre's choice to use the globular lens was more than a mere desire for greater magnification. It set him amongst the ranks of Leeuwenhoek and other great microscope users who had supposedly initiated this secret tradition. There were three

'Antonio Vallisneri e la questione dei vermicelli spermatici: un'indagine storico-naturalistica', in M. Generali and M. Ratcliff (eds.), *From Makers to Users*, pp.73-89. Luzzini is also one of a few historians who have recreated historical observations using period microscopes. Luzzini used an eighteenth-century Wilson screw-barrel microscope to recreate Antonio Vallisneri's observations of rabbit spermatozoa.

[20] G. Magni, *Nuove osservazioni microscopiche sopra le molecole rosse del sangue* (Milan, 1776), p.30.

[21] F. Fontana, *Nuove osservazioni sopra i globetti rossi del sangue* (Florence, 1766).

[22] G. Della Torre, *Nuove osservazioni microscopiche,* p.1.

[23] Ibid., p.3.

[24] Ibid., p.250.

[25] Ibid., p.4. Della Torre did, however, consider the compound microscope as a valuable instrument for studies involving lower magnification.

techniques for making such a lens. The maker could grind and polish a piece of crystal into a perfectly spherical shape. Microscope users often disparaged this technique. It was laborious and prone to create misshaped and scratched lenses. On the other hand, most of the famous surviving Leeuwenhoek lenses appear to have been ground by hand.[26] The second technique involved heating and blowing a glass tube in order to form a spherical bubble at one end. This of course required great experience and patience; unfortunately, it also resulted in a globule with protruding ends that the maker would need to break or polish off. Such a technique most often left a flat surface on one side of the glass resulting in a plano-convex lens as opposed to a perfect sphere. The third technique was called drawing: melting Venice glass, drawing it into a small strand, and then melting the tip of that strand to form a tiny droplet lens. Leeuwenhoek, Robert Hooke and Christiaan Huygens had all been adept at this method.[27] Della Torre himself recorded that he had discovered this last method independently:

> In fact this is the new discovery I made after a long labour of many years, namely in finding a sure way to form crystal balls by fire very small, so that their magnification is greater than any simple or compound microscope that may be used to see objects clearly, and distinctly.[28]

Della Torre then fit this tiny globule lens into the Wilson screw-barrel microscope as shown in figure 3.

[26] Leeuwenhoek himself noted the flaws in this technique in his letters to the Royal Society. J.G. O'Mara, 'On Leeuwenhoek's Magnifications', *Antonie van Leeuwenhoek*, 45 (1979), pp.161-4. For more on Leeuwenhoek's techniques see Douglas Anderson's comprehensive website, *Lens on Leeuwenhoek* <http://lensonleeuwenhoek.net> (accessed, June, 2011) See also, B.J. Ford, *The Leeuwenhoek Legacy* (London, 1991). Ford has also recreated Leeuwenhoek's microscopic observations using a surviving Leeuwenhoek microscope in Utrecht.
[27] M. Butterfield, 'Extract of a Letter from Mr. Butterfield Mathematical Instrument-maker to the French King, About the Making of Microscopes with Very Small and Single Glasses: and of Some Other Instruments' *Philosophical Transactions*, 12 (1677), pp.1026-7.
[28] Della Torre, *Nuove osservazioni microscopiche*, p.5. Della Torre recorded the date of this discovery as 1751 in ibid., p.5.

FIGURE 3: Della Torre's adapted simple Wilson microscope. Giovanni Marie Della Torre, ***Nuove Osservazioni Microscopiche*** **(Naples, 1776), plate 1.**

Drawing and melting fine strands of glass meant that the lens-maker could create incredibly small globule beads. The smallest of these glass beads had the largest magnification. Della Torre computed that his largest and lowest-powered lens could magnify objects a mere 42 times, but his tiniest and best lens had a power of

2560.[29] This was a far superior power to the average Wilson screw-barrel bi-convex lens, the best of which Baker had recorded at 400 power. Calculating magnification was problematic before the microscope micrometer had become a standard tool later in the eighteenth century. Micrometers had appeared in the previous century, and Benjamin Martin had fitted his screw-barrel microscope with a micrometer in the 1730s although its use in the microscope did not become standard practice before the 1770s.[30] To calculate magnification, Della Torre and Hewson used a method outlined in Baker's *The Microscope Made Easy*. In this method, the observer measured the distance between the lens and the specimen sample in inches, and then divided that distance into 8. Thus, if a specimen came into focus at 0.1 inches from the lens, the microscope magnified at 80 power.[31] Why did 8 become the constant of focal measurement? According to Baker, the human eye focused most effectively at an accepted average of 8 inches from an object. The use of a standard number by consensus indicates that microscope users did not actually believe that their instruments magnified at precisely the powers they recorded.

Della Torre records that he began making observations in 1760 on human blood. In July 1761, he also employed this globule lens with the aid of an observer: the English consul at Naples, Sir Francis H. Stiles.[32] The two men examined samples of blood, presumably their own, on his microscope slides using a globule lens of 512 power. They witnessed 'oblate spheroids' with dark centres 'as if a dent or concave impression had been made on their surfaces, which resembled those of young peas that have dimpled in the boiling'.[33]

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[29] Della Torre includes a table of ten lenses, their sizes, and magnification on p.43 of his 1776 treatise.
[30] M. Ratcliff, *The Quest for the Invisible*, p.168.
[31] H. Baker, *Microscope Made Easy*, pp. 32-8.
[32] F.H. Eyles Stiles was also a fellow of the Royal Society. He died shortly thereafter in the year 1762, of unknown causes.
[33] F.H. Eyles Stiles, p.253.

Next, Della Torre used a much more powerful globule lens which, according to Stiles, magnified at an incredible 1280 power. Examining another sample of blood, they no longer witnessed oblate spheroids, but hollow rings composed of tiny spheroids, 'like a bead necklace'.[34] Next, Della Torre applied his most powerful magnifiers of 1920 power and of 2560 power. The observers saw these tiny spheroids again in rings that at times disintegrated into smaller sections or individual miniature globules. In summary, blood was composed of globules and rings of globules. (Fig. 4, subfigure 7)

Applying his lens to other parts of the body, Della Torre found those same globules in nerves, muscle fiber, chime; in fact, in almost all the places he pointed his microscope: for example, they also appeared like beaded necklaces in the spinal cord and cerebellum (Fig. 5)

[34] Ibid., p.254.

The image originally presented here cannot be made freely available via ORA because of copyright. The image was sourced at the Museum for the History of Science Library, Shelfmark: E/TOR, plate 14.

FIGURE 4: Hollow blood globules. Della Torre, *Nuove Osservazioni Microscopiche* (Naples, 1776). Plate 14, described on pages 82-85.

The image originally presented here cannot be made freely available via ORA because of copyright. The image was sourced at the Museum for the History of Science Library, Shelfmark: E/TOR, plate 9.

FIGURE 5: Globules found in spinal cord cross-sections (subfigure 7) contrasted to globule strands of the cerebellum (subfigure 8). Della Torre, *Nuove Osservazioni Microscopiche* (Naples, 1776), plate 9, also described on page 62.

Della Torre joyously announced his observations in letters to his colleagues in Paris and London. He also sent gifts of his lenses abroad (which was no small feat considering the Seven Years War was in full swing at the time and naval blockades were in place). Della Torre's major correspondent in France was the Abbe Jean Antoine Nollet (1700-1770)[35] and in London, Henry Baker. Through Stiles, he sent the London Society four globule lenses so that they might verify his claims. Because these lenses were inserted into brass attachments, users in London could easily fit them onto their own Wilson screw-barrel microscopes (see the detachable lens casing in Fig. 1, K and Fig. 3, subfigure 7).

Della Torre's observations caused an immediate sensation, and the Royal Society published Stiles complete report in the *Philosophical Transactions* of 1765. The backlash came the following year from the person Della Torre perhaps least suspected: the great microscope promoter Henry Baker himself.

[35] Della Torre, *Nuove Osservazioni Microscopiche*, p.83.

Baker dutifully analyzed Della Torre's globule lenses for the Royal Society.[36] Whereas Styles had sent an exuberant twenty-four page letter to the Society, Baker's following report was a terse four and a half pages. On behalf of the Royal Society, he thanked Della Torre for his gift, but recorded that only three lenses had survived the voyage from Naples with magnifying powers of 640, 1280, and 2560. When Baker attempted to observe blood under the lenses, his findings were damaging both for Della Torre and the reputation of the globule lens. Baker could see nothing at all.

> [Mr. Baker] must declare, with some concern, that through the smallest globule, he has not been able to distinguish anything: and even through that which magnifies the least, he could never view any object with satisfaction.[37]

Moreover, Baker feared that Della Torre's lenses were not only useless, but harmful to the eyes of the user. Whether in daylight or by candlelight, Baker had to squint so carefully through the tiny lenses that 'he hopes his eyes are not injured by these examinations ... he believes there are very few who would not have been nearly blinded thereby'.[38]

The rest of Baker's report rebuked the practice of manufacturing globular lenses. 'Many years ago they were much used, and highly boasted of ... but they now long since have been laid aside'.[39] In their place, Baker called for continued interest in the bi-convex lenses such as those he had espoused in his book, *The Microscope Made Easy*. Clear lenses, he argued, were far more valuable than powerful lenses which might distort images and lead the observer into wild conjectures.

> Nothing can be more injudicious than the desire of such excessive magnifying power: whenever we can see an object clearly and well defined, we ought to be contented; all beyond this there is no dependence on.[40]

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[36] H. Baker, 'A Report Concerning the Microscope Glasses, sent as a Present to the Royal Society by Father di Torre of Naples', *Philosophical Transactions*, 56 (1766), pp.67-71.
[37] Ibid., p.71.
[38] Ibid.
[39] Ibid., p.69.
[40] Ibid.

In a final coup-de-grâce, Baker dismissed Della Torre's globular lenses, 'as matters of curiosity rather than of real use'.[41]

Criticism also arrived from closer to home that same year, as Della Torre's colleague in Florence, Felice Fontana (1730-1805), issued a contradictory observation on the shape of blood globules directed at 'a learned Italian doctor' who 'could not persuade me that those glowing little bodies, which are seen swimming in the veins and arteries of animals suddenly changed their shape, and are minute pellets or spheroids'.[42] On the contrary, Fontana found flat, disc-like shapes when he examined blood under his simple microscope.[43]

Like Baker, Fontana claimed that Della Torre's error arose from the use of the globule lens. These 'crystal balls' or 'little globes of glass manufactured by the author deflect rays of light unevenly', the light travels through the centre of the globe; however, the rays that travel off-centre through the globe are bent differently and thus reach a different focus.[44] Fontana, had correctly drawn attention to the spherical aberration of the globular lens.

5.3. The investigations of William Hewson

The most methodological refutation of Della Torre's observational technique came in 1773, when William Hewson published a contradictory observation of blood globules in the *Philosophical Transactions*. Hewson was apparently unfamiliar with Fontana's contemporaneous observations, and he reached a similar conclusion: that blood globules were hardly spheres at all.

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[41] Ibid., p.71.
[42] F. Fontana, *Nuove Osservazioni Sopra I Globetti Rossi Del Sangue*, p.7.
[43] Unfortunately, we do not know the model and type of this microscope although Fontana frequently disparaged spherical lenses as well as the use of compound microscopes.
[44] Ibid., p.11.

> It is almost inconceivable that so many ingenious men should at different times have viewed them through the microscope, and have concluded them to be spherical, if they be really flat.[45]

Not surprisingly, Hewson refused to label blood particles as globules; instead, he referred to them as vesicles to avoid any connotation with spheres or globes.

Hewson's study explored these shapes in far greater detail. Henry Baker likely encouraged Hewson in his endeavour as Baker lent Hewson Della Torre's earlier lenses. For a third and final time, a microscope user tested these lenses through the Wilson screw-barrel microscope.

Hewson asserted that Della Torre's lenses produced unacceptable spherical aberration. More problematic, he claimed, was their lack of transparency: 'The globules are full of clouds, made by the smoke of the lamp used in preparing them.'[46] In comparison, the English-made bi-convex lenses 'though inferior in magnifying power to these globules, are much superior in distinctness'.[47] Hewson used these simple lenses and the screw-barrel design shown early in Fig. 1.

In addition, Hewson used his microscope to move from simple observation to experimentation. Rather than merely observe blood in various animal matter, he deliberately mixed his blood samples with substances such as nitre, water, salt water, plasma, and distilled blood, while observing and manipulating the sample. Mixing blood with a neutral salt (epson salt) he observed that the vesicles shrivelled. Conversely, when Hewson added water to another sample of blood, the vesicles swelled into a spherical shape. Hewson inferred that other observers such as Della Torre had likely diluted their blood samples with water causing these spherules to form and burst. This effect is now known as osmotic lysis. In another experiment,

[45] W. Hewson, 'On the Figure and Composition of the Red Particles of the Blood', p.316.
[46] Ibid., p.306.
[47] Ibid., p.305.

Hewson let a vial of blood stand until the less dense plasma separated from the blood vesicles. He used this plasma to dilute samples instead of water.[48]

Placing the screw-barrel microscope on Cuff's pillar stand freed Hewson's hands, allowing him to move the sample slide while looking through the lens. When he tilted the plasma-rich sample from side to side, he witnessed the blood vesicles tumbling. 'I have in this way showed their figure to a number of curious persons and particularly to many students of anatomy.'[49] The vesicles appeared more like rolling coins than spheres, nor did they break apart into small spherules. 'These experiments ... prove that the particles of the blood are flat, and not globular.'[50]

Less successful were Hewson's attempts to integrate his observations into the field of comparative anatomy. Upon viewing the blood of fishes and amphibians, Hewson clearly saw that those vesicles contained dark spots in their centres. Because blood was universally 'extended through the creation' to almost all species with little variation, Hewson mistakenly assumed that human blood contained the same dark spots; they were merely too small for the microscope to discern. He argued that Della Torre must have mistook this centre for a round hole, which had led him to argue that blood was composed of many globules forming a ring with a hole in the centre.[51]

Hewson emerges from this story as a keen empiricist. Although he was not the first to study blood under the microscope or to accurately describe its vesicles, modern authors still credit Hewson as the 'father of hematology'.[52] Della Torre responded to this criticism in his longer 1776 treatise. He dismissed Hewson's notion of a centre spot in human blood. More provocatively, he challenged both Fontana and

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[48] Ibid., pp.308-10.
[49] Ibid., p.309.
[50] Ibid., p.310.
[51] Ibid., p.307. Hewson included plates illustrating the similarities of blood across species at the end of his article.
[52] D. Doyle, 'William Hewson (1739-1774): The Father of Haematology', *British Journal of Haematology*, 133, no. 4 (2006), p.375.

Hewson's observations, disregarded them as the result of imperfect lenses (Della Torre did not disclose the fact that Hewson had used Della Torre's own lenses to negate his claims). According to Della Torre, Hewson and Fontana had employed an improper technique or had used imperfect lenses, likely caused by inexperience in drawing the lenses using heat. 'Thus were born many complaints against some of the spheres that are worked by fire: that they do not distinguish objects well and are difficult to handle.' In Della Torre's view, If an object did not appear 'clear, distinct and natural, it was an obvious sign that the lens, or ball, is not spherical, and thus one must make another lens'.[53]

Hewson's observations upon blood may have been well-received, but Della Torre's work was not entirely unsuccessful: his support of the globule lens did not go unnoticed. In 1776, another Italian physician, Giuseppe Magni, also published a microscopic study of blood, attempting to verify Hewson's claims.[54] Magni was a professor of anatomy at the Almo Collegio Borromeo in Pavia. He had acquired a simple microscope, another screw-barrel, 'The fashion of most of those which arrive from London, and which are the invention of Wilson with some mutations, and are still called pocket microscopes'.[55] Disregarding the advice of Hewson and Baker, Magni opted to use the simple globular lens which Della Torre so favoured. In doing so, Magni observed blood corpuscles, but not the globular rings of Della Torre; he saw Hewson's flattened discs instead. Thus, in the case of Magni and Della Torre, we find experimenters using identical lenses yet reaching separate opinions concerning the globular hypothesis.

[53] Della Torre (1776), pp.17-18.
[54] G. Magni, *Nuove osservazioni microscopiche.*
[55] Ibid., p.30.

5.4. The globular hypothesis in context

What was so relevant about the globular hypothesis that it incited the paper war between these anatomists? The 'infamous hypothesis' as historian Schickore calls it was a quest for uniformity.[56] It's origins lay in the seventeenth-century corpuscular hypothesis: the theory that indivisible microscopic corpuscles composed all matter. One could thus explain all properties of matter with reference to the size, shape, or motion of these globules. The most prominent advocate of corpuscularism was Boyle, who, according to Marie Boas Hall, produced the first systematic defence of the subject in his 1661 *The Origin of Forms and Qualities.*[57] Boyle of course, drew heavily upon Epicurean atomism in his attempt to reject Aristotelian concepts; in fact, Hall speculated that Boyle's use of 'corpuscle' as opposed to 'atom' was an attempt to render his theory more palpable to Aristotelian logicians. Indeed, rough drafts of Boyle's text and other surviving manuscripts contained numerous instances in which Boyle first writes 'atom' only later to cross the word out with 'corpuscle'.[58] However, as Hall rightly points out, Boyle's corpuscular theory was far more than atomism by another name. He was steadfast in his attempt to shed all metaphysical implications from the theory. The microscope proved useful in this regard. As stated in Chapter 1, Hooke's microscopic quests may have initially begun as attempts to find these universal corpuscles to life. Corpuscularism seems to have lost its esteem among natural philosophers in the eighteenth century but retained its position in anatomy, evolving into the globular hypothesis. Early historians of biology viewed such theories as precursors to modern cell theory[59]; however, John Pickstone rightly avoids

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[56] Schickore, 'Error as Historiographical Challenge', p.27.
[57] M.B. Hall, 'Boyle's Method of Work: Promoting His Corpuscular Hypothesis', *Notes and Records of the Royal Society of London*, 41, no. 2 (1987), pp.111-43.
[58] Ibid., pp.113-5. Hall reproduces an extant copy of the Boyle manuscript in ibid., pp.124-44.
[59] J.R. Baker, 'The Cell Theory, a Restatement, History and Critique: Part 1', *Quarterly Journal of Microscopcial Science*, 98 (1948), pp.103-125.

such presentism.[60] He finds that the globular hypothesis developed as a logical framework to explain eighteenth-century microscope observations. Like Pickstone, I hope to show that the only meaning of 'globules' lies in the historical accounts and their antecedent theories; historians will gain little by correlating historical 'globules' and modern 'cells'. Rather, we must turn our historical gaze in the opposite direction and study the modes of knowledge and experimentation which composed the globular hypothesis.

Despite the reservations of Hewson, Magni, and their fellow critics, the globular hypothesis attracted many adherents, especially among later generations of anatomists. For example, Henri Milne-Edwards (1800-1885) wrote his medical thesis on these 'elementary structures' in 1823.[61] Surprisingly, he did not credit Della Torre or other earlier globular theorists; rather, Milne-Edwards described the globular hypothesis as if it were a triumph of nineteenth-century microscopy in the face of late eighteenth-century criticism.

> Some people, and Bichat himself was one of them, thought that the use of the microscope in research of this nature could not provide satisfactory results. But everyone knows the benefits we have taken from this instrument in other branches of natural history.[62]

Milne-Edwards claimed that he was able to see features such as spherical globules of blood and globules of various sizes forming beaded strings in muscle and other tissues taken from a dog. He thought that tissue formed from the coagulation of bodily liquids into separate globules which then coagulated into tissues. Milne-Edwards and other

[60] J.V. Pickstone, 'Globules and Coagula: Concepts of Tissue Formation in the Early Nineteenth-Century', p.344.
[61] H. Milne-Edwards, *Mémoire sur la structure élémentaire des principaux tissus organiques des animaux* (Paris, 1823).
[62] Ibid., p.2.

experimenters such as Henri Dutrochet (1776-1847) also tested various chemical solutions on tissues to break them into their original globules.[63]

The hypothesis that bodily matter could be reduced to elementary globules seemed plausible; however, the theory remained hotly debated. Schickore has examined the legacy of the globular hypothesis among German microscope users in the nineteenth century. In her historiographical essay, Schickore advises us not to label these debates as a 'controversy' because, 'many, if not all practitioners *agreed* that something was amiss.'[64] The quest to uncover errors, and not globules, seems to have been the primary preoccupation of these microscope users, especially during the early nineteenth century. Even if they tacitly agreed that most tissue samples could be reduced to globules, their observations differed widely. It was precisely the failings of the investigators to reach a consensus which compelled further research and improvement.

5.5. The legacy of the globular lens

As for the microscope itself, this case study revises some of our past understanding of its use during the late eighteenth-century. In these examples, leading anatomical research adhered to older lens technologies: bi-convex and spherical lenses. This contradicts historian Maurice Daumas' assertion that 'the fashion for microscopes with spherical lenses seems to have passed quickly ... by the first years of the eighteenth century they had ceased to be of interest'.[65] On the contrary, Gerard L'E. Turner found that spherical lens microscopes persisted well into the nineteenth century; for example, through the construction of globular lenses out of precious

[63] J.V. Pickstone, 'Globules and Coagula: Concepts of Tissue Formation in the Early Nineteenth-Century', p.349. See also, H. Dutrochet, *Recherches anatomiques et physiologiques sur la structure intime des animaux et des végétaux, et sur leur motilité* (Paris, 1826).
[64] J. Schickore, 'Error as Historiographical Challenge', p.40.
[65] M. Daumas, *Les Instruments Scientifiques aux XVII et XVIII Siècles*, p.47.

stones.[66] As late as 1813, Sir David Brewster called for renewed interest in this spherical type of simple lens as an alternative to the compound microscope. Brewster believed that a material other than glass would reduce aberration:

> We cannot expect any essential improvement in the single microscope, unless from the discovery of some transparent substance, which like diamond, combines high refractive power, with low power of dispersion.[67]

It was his physician, Dr. C. Goring, who first constructed several of these jewel lenses, both spherical and bi-convex, out of diamonds, garnets, and sapphires; nevertheless, the jewel lenses suffered the same aberration problems as their predecessors, and quickly fell out of fashion. Thus, paradoxically, Leeuwenhoek's bare and functional spherical-lens design had become opulent and ineffectual in its final years of use. The early success of the globular-lens microscope was its simplicity: a central characteristic that the much later jewel-lenses had incidentally abandoned. Meanwhile, the compound microscope had branched out to include many different modifications (some of which were genuine improvements such as Cuff's side pillar and his modification of the fine-focus device). In contrast, the single-lens microscope allowed little room for change except when it came to their lenses; as we have seen, switching lenses into the Wilson screw-barrel microscope allowed users to radically change the quality of their images with ease.

The process of manufacturing Leeuwenhoek's bead-like spherical lenses was well-known by the late eighteenth century. More complex and shrouded in secrecy, were the designs of the compound microscopes emerging during this same time. These were, on the whole, far inferior to the simple lens throughout the first half of the eighteenth century. Della Torre and Hewson both drew attention to their

[66] G.L'E. Turner, 'The Rise and Fall of the Jewell Microscope 1824-1837', *Microscopy, the Journal of the Quekett Microscopicial Club*, 31 (1968), pp.85-94. See also, R.H. Nuttall and A. Frank, 'Makers of Jewel Lenses in Scotland in the Early Nineteenth Century', *Annals of Science*, 30 (1973), pp.407-16.
[67] G. L'E. Turner quoting D. Brewster in G.L'E. Turner, *Micrographia Historica*, p.17.

limitations. Additionally, the simple spherical-lens microscope of Leeuwenhoek and Della Torre demanded an experienced user; it was not an instrument for display. Eighteenth-century manufacturers occasionally garnished the complex stands and cylinders of their microscopes with ornate garlands and other ornamentation in the hopes of appealing to the leisure classes. However, ornamentation remained absent on the functional spherical-lens microscopes such as Leeuwenhoek's early microscopes or the small Wilson screw-barrel microscopes discussed in this chapter.

If the debate among these microscope users was on lenses, how then do we situate the other components of the microscope? Again, Schickore's writings are useful here. She has described aspects of microscopy that constitute 'second order concerns', that is, concerns which pertain to the 'methods and tools of research, their epistemic merits, and the researcher's status as producer of knowledge.'[68] A second order tool might be a microscope attached to another instrument such as in Jesse Ramsden's (1735-1800) dividing engine. Such instruments aid in the operation or construction of the primary investigatory instrument. I would add that the body of the Wilson screw-barrel microscope constitutes such a second order tool because it was an accepted framework for the user to experiment with the primary tools: its lenses. The use of the screw-barrel microscope in the hands of Della Torre, Hewson, and Fontana illustrates an unspoken agreement that the microscope was an acceptable 'site' in which to further experiment with lens technology. The fact that one could easily swap lens covers among different screw-barrel microscopes greatly facilitated these experiments.

One reviewer of Schickore's book, Peter J. Ramberg, has noted that Schickore's notion of 'second order concerns' bears a close resemblance to Pierre Duhem and Willard Quine's well-known auxiliary hypothesis: that the value of any

[68] J. Schickore, *The Microscope and the Eye*, p.3

observation is predicated on existing value assumptions and epistemologies.[69] In other words, one cannot verify or falsify a hypothesis through experiments without first recognising that those experiments rely on a larger canon of past hypotheses, methods, and accepted truths. For Schickore, the major problem faced by eighteenth-century microscope users was whether or not their technology and preparation techniques were credible tools of the field. That both Della Torre and Baker attempted to invoke the legacy of Leeuwenhoek testifies to their joint faith in the microscope as a conventional instrument that anatomists should not abandon. What complicated matters was the acceptance or rejection of the globular hypothesis. For Della Torre, an *a priori* faith in the hypothesis might have led him to see blood as rings of spherical globules. In contrast, the mutual rejection of the globular hypothesis by Hewson and Fontana may have predisposed them to provide contrary arguments.

There is also a deeper meaning behind the lenses and globules of blood that Della Torre and Hewson debated. Della Torre also used the term 'globule' in reference to the spherical lens. 'Globule' is a loaded term, which Della Torre used to signify a universal and perfect spherical shape. This has implications for the instrument maker as well. If the globule was a perfect sphere, '*la forma sferica perfetta*', it exemplified the nature's most perfect shape.[70] If the lens was a spherical 'globular lens' it embodied perfection as well. Thus, Della Torre argued, art must imitate nature. As nature had endowed the universe with perfected spheres, the observer may create perfect spheres in order to observe nature most efficiently. Did not the creator already settle on this most perfect spherical form for many elements of the universe, from globes, to eyes, to globules of blood? Again, Hewson, Fontana, and Baker would contend otherwise.

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[69] P.J. Ramberg, 'Microscopy in Britain and Germany, 1740-1860 (Review of J. Schickore, The Microscope and the Eye)', H-German (2009) <http://www.h-net.org/reviews/showrev.php?id=15660> (accessed 23 Jul, 2011).
[70] Della Torre, *Nuove osservazioni microscopiche*. p.3.

For now we must conclude that the bead-like 'globular' lens remained a strong component of anatomical and medical research throughout the eighteenth century. Of course, the lens had obvious limitations which Hewson and Baker highlighted; however, their caution was not shared by all. Certain anatomists and physicians clung to this type of lens despite the clearer images provided through the bi-convex lens and notwithstanding the radical improvements in the design of compound microscopes. The globular lens endured.

5.6. Re-enactment of these experiments

The Museum for the History of Science houses a number of Wilson screw-barrel microscopes and their original bi-convex lenses.[71] In order to recreate Della Torre's observations, I have used a Leeuwenhoek replica microscope because it incorporated a spherical lens.[72] To recreate both Hewson and Della Torre's observations I placed a drop of human blood on a test plate beneath both types of lenses. For comparison, I also examined tissue samples using an Adams universal microscope, similar to that which Milne-Edwards used in his later investigations of the globular hypothesis.

Figure 6 is my attempt to recreate Della Torre's experiment. This image shows the blood sample, which has started to dry, as seen through the spherical lens. The microscope was able to resolve individual blood corpuscles when the sample was fresh, but not with the detail Della Torre had illustrated. I was not able to see features which might be construed as rings or smaller globules within the blood corpuscles whether the sample was fresh or not. Of course, my experiment is open to many different errors. I did not have access to Della Torre's original lenses, and even if this had been possible, time may have decayed their quality. Furthermore, the

[71] The screw-barrel microscopes used in the experiment are MHS, inventory no. 44541, no. 40842, and no.54659.
[72] MHS, inventory no. 44490.

Leeuwenhoek replica microscope may not have had a lens identical to that which Della Torre had used. It was instead the closest approximation I could find among the microscopes in the collection. On the other hand, as the blood sample dried, the corpuscles congealed to form clusters One could hypothesize that Della Torre mistook these clusters for globules, that is, smaller components which composed the red blood corpuscles as shown in Fig. 4, subfigure 7. The spherical and chromatic aberration was more pronounced in this type of lens as is apparent in the edges of the field of view. Although it is not clearly depicted in my photograph taken through this lens, this aberration created the illusion of depth, which did indeed give the impression that the corpuscles were spherical, or, at least, not flattened discs as they faded in and out of the field of view.

FIGURE 6: Human blood sample through a replica Leeuwenhoek lens in imitation of Della Torre's observations. MHS, inventory, no. 44490.

In the case of Hewson's observations, I used a Wilson screw-barrel from circa 1760 (Fig. 7). I was easily able to discern individual blood corpuscles using the second highest-powered lens (Fig. 8), and, like Hewson, I could not discern anything which might be construed as rings of globules as Della Torre had illustrated.

FIGURE 7: Wilson screw-barrel microscope with sample slide, detachable lens, and British pound coin for scale. MHS, inventory no. 54659 Lens no. 2. Compare to Fig. 1.

FIGURE 8: Human blood under a Wilson screw-barrel microscope (c.1760). Hewson MHS, inventory no. 54659 Lens no. 2.

Dipping the microscope from side to side, the blood corpuscles did indeed appear like rolling coins as Hewson had claimed.[73] They also appeared flat, instead of perfectly round when they were in the centre of the field of view, but depth is difficult to distinguish using any early microscope.

To examine the later globular theory of Milne-Edwards, I used a microscope manufactured by George Adams Jr. (1750-1795). Although my model likely dated from circa 1790, this compound microscope was popular well into the nineteenth-century and continued to be manufactured after Adams had died and the firm was sold.

The later versions of the Adams microscope came in a heavy Mahogany case which was designed to display the impressive number of components and accessories (Fig. 9). Some assembly was required on the part of the microscope user. Setting up the device took the better part of an hour and much adjustment was needed to attain focus once more after changing lenses or other accessories. Such a microscope was clearly intended for serious researchers or for a microscope user who wished to appear as such.

[73] William Hewson, 'On the Figure and Composition of the Red Particles of the Blood', p.509-10.

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FIGURE 9: Universal microscope by George Adams Jr. in box. MHS, inventory no. 40477.

Milne-Edwards had faith that his microscope was superior to that of his predecessors.

> Through this excellent microscope of Adams, it was possible for me to be more accurate in my observations, and to continue much further than I would have done with a less perfect instrument.[74]

It is interesting to note that Della Torre had praised the simple globular lens as the 'perfect' shape of a lens while Milne-Edwards considered the lenses of such predecessors as 'less perfect' than his own.

Bradbury has preceded me in examining this particular microscope. His 1967 study captured a test plate from the third lens (illustrated in Chapter 2, Fig. 5). Bradbury concludes that the multiple lenses of this microscope may have significantly reduced chromatic aberration, but they heightened flaws on the surface of the lenses

[74] Milne-Edwards, p.1.

and other optical problems. 'One might expect a marked improvement ... but this is not so.' Bradbury added, 'this may well be due to the extreme complexity of the optical system introducing cumulative amounts of spherical aberration which degrade the image to a marked degree.'[75] I found that images also appeared much darker under such a microscope owing to the multiple lenses.

This Adams microscope had an advantage aside from its supposedly achromatic lenses: one focused by moving the specimen stage rather than the barrel of the microscope. The fine focusing mechanism consisted of a single screw, as in Cuff and Martin's earlier compound microscopes, but, unlike these earlier models, the screw operated a rack and pinion which carried the stage. The grooves and teeth of this mechanism more or less locked the stage into a fixed position. However, I found that the groves in the rack and pinion were spaced so far apart that a specimen under high magnification would sometimes be too distant or too near to get in focus.

Milne-Edwards had also examined blood under the microscope and, like Della Torre, had seen globules, but not shaped like rings. Milne-Edwards was more interested in the universal presence of these globules throughout the body. He also theorised how globules form: that they arise from the coagulation of bodily fluid, and, in turn, that those globules combine to form tissues. Examining a sample of coagulated blood (Fig, 10), I could no longer see individual blood corpuscles, but the what appeared to be a solid tissue emerging with its own vascular structure.

[75] S. Bradbury, p.169.

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FIGURE 10: Congealing human blood through a George Adams microscope, MHS, inventory no. 40477

What appears to be capillaries in this image are more likely spaces between the clusters of blood corpuscles that cast shadows with the appearance of smaller vessels or a fibrin lattice structure. With such images in mind it is not surprising that Milne-Edwards and his contemporaries thought that globules were ever-present and ever-forming in the body.

Turning his attention to other parts of the body, Milne-Edwards credited Fontana for his investigations of tendons and their fibres.

> Tendons, seen under the microscope, appear to consist of a very large number of small longitudinal beams, according to Fontana, these are, in turn, composed of extremely small fibres, parallel to one another and in waves. [76]

[76] Milne-Edwards, pp.12-13.

Milne-Edwards then uses the globular hypothesis to amend Fontana's previous observation. "I found that the cylinders described by the author are formed of globules. These globules are therefore similar to those that we have found so far.[77]

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FIGURE 11: tendon section as seen through the universal microscope by George Adams Jr. MHS, inventory no. 40477

Figure 11 is a longitudinal section of a beef tendon as seen through the Adams microscope. One can clearly see the fibres that Fontana and Milne-Edwards had described, but I could not discern anything globule-like within those fibres. If one looks at the image long enough, one can see small areas on the individual fibres that are in shadow and other areas that are fully lit. Could these have been construed for separate globules? Our modern knowledge of histology alters and constrains how we

[77] Ibid.

interpret such an image; however, the possible interpretations were much more varied for an observer in the eighteenth or nineteenth centuries.

5.7. Conclusions

This case study has highlighted intersecting and competing modes of knowledge and meaning, namely the importance of the microscope to prove or disprove aspects of the globular hypothesis. Such endeavours warranted microscope-based research well into the nineteenth century which indicates the importance of the microscope in resolving anatomical and philosophical questions. The timing of these investigations is surprising. Milne-Edwards followed Della Torre by almost fifty years yet the evidence for his globular hypothesis remained unchanged. Although the two used different types of microscopes and lenses, the blood globules and other tissue fibres that they described were not dissimilar, nor were the images of tissues that I could see through their differing types of microscopes.

The etymological implications of terms such as 'corpuscle' and 'globule' are historically significant. Although one still finds the term corpuscle in countless histology textbooks and articles, its use has long been a subject of debate. The current preferred technical term 'erythrocyte' (literally 'red hollow vessel') harks back to Della Torre's theory of a ring-like or hollow globule; however, this definition arose in the early twentieth century precisely because practitioners wished to avoid the loaded terms 'corpuscle' and 'globule'. In a 1901 letter to the *British Medical Journal*, Victor Lindesay, an army surgeon, lamented, 'as up to the present, there still seems to be no name for this little physiological orphan'.[78] When dealing with the history of

[78] V. Lindesay, 'The Name of the Red Blood Corpuscle: A Suggestion', *The British Medical Journal* 1, No. 2098 (1901), p. 677. The turn of the twentieth-century was a particularly exciting age of haematological research. That same year, Karl Landsteiner published his account of blood as divided into distinct groupings which he recognized would enable safe transfusions: K. Landsteiner, 'Ueber

histology, all parts of the body are such orphans. As shown in the case of Della Torre and Hewson, although microscope users might have shared similar instruments, we cannot assume that they shared a common view of even the smallest fibre or drop of blood.

Agglutinationserscheinungen nor- malen menschlichen Blutes [On the agglutination of normal human blood]',*Wiener Klinische Wochenschrift* 14 (1901), pp.1132-1134.

Chapter 6. Case Study: Lymphatic System Discoveries, 1760-1780

Anatomists issued a bevy of new studies on the lymphatic system in the second half of the eighteenth century. This group included Johann Lieberkühn (1711-1756), Albrecht von Haller (1708-1777), William Hunter, Alexander Monro Secondus, William Hewson, John Sheldon (1752-1808), and William Cruikshank (c.1745-1800). This chapter will explore new perspectives in physiology that prompted interest in the lymphatics. It will illustrate how the microscope enabled accurate research into the function of the lymphatic vessels and, along with other techniques, aided physiologists in their discovery of the lymphatic system of absorption and digestion. As we will see, this was a subject of fierce debate, riddled with priority disputes and counter-claims. In exploring these issues, we find that anatomy in the latter half of the eighteenth century incorporated an increased display of microscopic evidence, new techniques of slide preparation, and an endeavour to recreate and confirm microscopic research.

The microscope was only one aspect of a new course of physiological research that emerged in the eighteenth century. Although it certainly played a useful role in discerning minute parts of the lymphatic system, the most widely discussed approach to this new physiology was the use of injections to trace bodily vessels: the arteries, veins, and lymph vessels. Anatomists experimented with various techniques of injecting a number of substances through body vessels (including wax, resin, dye, milk, water, air, and mercury) in the hopes of differentiating them. More ambitious experimenters relied on vivisection in conjunction with injections to see how fluids flowed in living animals. In a supporting role, the microscope was a useful aid to this new way of seeing. Experimenters could more easily follow these vessels to their terminal ends when looking through their lenses.

What prompted this line of research into the lymphatic vessels? Although the microscope might have failed to fulfil the tenets of the mechanical philosophy in the early eighteenth century, microscopic research on various bodily functions continued. New theories of bodily action arose in the middle of the century which were predicated on material and mechanical explanations: Johann Lieberkühn, for example, examined the fluid dynamics at work within the body and studied them microscopically. Similarly, Albrecht von Haller used the microscope to illustrate his concept of body irritability. Such views added a different perspective to a number of physiological questions, such as: what was the structure and function of the lymphatic system as a system of absorbing fluid and nutrients within the body? Experimenters began to explain how this system operated alongside the circulatory system, rather than as an element of the circulatory system. This was no trivial anatomical discovery: investigations upon the lymphatic vessels indicated that an entirely new system was at work within the body. It was, in the words of William Cruikshank (1745-1800), among the most contested discoveries since Harvey's work on circulation.[1]

6.1. Towards a new bodily system: hydrostatics and irritability.

Most eighteenth-century experiments on the lymphatic vessels were concerned with one of two goals: mapping the vessels, and the question of their function. Wax or mercury injections were most useful for both projects, but especially for mapping out entire systems of the body. Towards the latter half of the century, the lymphatic vessels were fairly well charted, as we can see from William Hewson's 1774 map of vessels in the dissected limbs (Fig. 1).

[1] W. Cruikshank, 'Introduction', *The Anatomy of the Absorbing Vessels of the Human Body* (London, 1786), pp.2-4. the anatomist William Cruikshank is not to be confused with his contemporary, the chemist William Cruickshank (c1745-1810).

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FIGURE 1. William Hewson, *Experimental Enquiries Part II: A Description of the Lymphatic System in the Human Subject and in other Animals* (London, 1774) plate III.

Such illustrations served as convenient diagrams for textbooks or in the classroom. However, it was far more difficult to illustrate functionality. This is where the microscope played a vital role. When one traced the lymphatic vessels from their largest to smallest parts, the vessels inevitably become lost in the tangles of blood arterioles, venules and capillaries.

It had been known since the mid-seventeenth century that part of the lymphatic system aided in digestion: the lacteal vessels drained chyle (digested fatty acids) from the intestines into the bloodstream through the large thoracic duct into the venous system.[2] However, did the smaller lymphatic vessels also somehow link directly into the circulatory system on a microscopic level? Olas Rudbeck (1630-1702) and Thomas Bartholin (1616-80) had independently discovered a vast network of these tiny lymphatic vessels in the 1650s, but few anatomists had examined their function until the mid-eighteenth century. Other questions began to perplex

[2] C. Ambrose, ‘Immunology’s First Priority Dispute: An Account of the 17th-century Rudbeck–Bartholin Feud’, *Cellular Immunology*, 242 (2006), pp.3-4.

physiologists. Why did mercury, when rubbed on the skin, enter the lymphatic vessels? It also did so when injected directly into the body cavities. Clearly fluid in the body cavities (lymph) could transverse into and out of the circulatory system as well as the lymphatic system, but the relationship between the two was not well understood.

Adding complication to this problem was the fact that most early eighteenth-century anatomists had found no purpose for the lymphatic vessels at all. Since the seventeenth century, Harvey's doctrine of circulation held that blood vessels alone could take up lymph matter from the tissues. There was no need for a separate system of absorption. As Cruikshank stated:

> I used to explain this as circulation of the blood, not absorption, in the way Harvey conceived it to be ... he conceived the blood to be thrown from the arteries into a parenchyma, or spongy texture, interposed between the ends of arteries and beginning of veins; and that the *vis a tergo* [pressure from the heart] in the arterial blood, propelled it not only through the sponge, but into the veins.[3]

Harvey did not use a microscope, nor did he delineate the structure of this 'spongy' area we now call capillaries.[4] In Harvey's view, substances caught in this area, or in the interstitial spaces, would be flushed away by arterial pressure.

Conversely, since the time of Harvey, fluid dynamics had become a burgeoning field, widely applicable to new industries as well as anatomy. It sparked some renewed interest in capillaries and other body vessels: In 1745, Johann Lieberkühn hoped to explain how blood vessels alone could absorb liquids and transport them into the bloodstream through a mechanism of fluid pressure.[5] His illustration (Fig. 2) attempted to illustrate absorption based on the principles of fluid dynamics.

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[3] Cruikshank, *The Anatomy of the Absorbing Vessels of the Human Body*, p.27.
[4] Y. Elkana and J. Goodfield, 'Harvey and the Problem of the 'Capillaries'', *Isis*, 59 (1968), pp.1-73.
[5] J. Lieberkühn, *Dissertatio anatomico-physiologica de fabrica et actione villorum intestinorum tenuium hominis* (Leiden, 1745).

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FIGURE 2. J. Lieberkühn, ***Dissertatio anatomico-physiologica de fabrica et actione villorum intestinorum tenuium hominis*** **(Leiden, 1745), p.29.**

This illustration of a brass model is meant to represent microscopic blood vessels. Tube AB represents a capillary segment carrying blood: blood mixed with lymph enters the capillary through arteriole A and exits through venule B. The two smaller capillary forks feed into the larger blood vessel. According to Lieberkühn, the blood and lymph could flow freely through both AB and down the smaller fork to the left (labelled GC). However, if the capillary closed at G then the pressure through AB would increase at the narrow point GH. This would suction fluid up from the smaller right branch FH. The entire system was a blend of Harvey’s findings and Lieberkühn’s elaborate deductions concerning arterial pressure.

Other experimenters were sceptical. While studying the arterial pressure of the arteries and veins, beginning in 1768, George Fordyce (1736-1802) found that there could be no force equal to push liquid back into the bloodstream. Although Fordyce was not the first anatomist to view working capillaries under the microscope, he did surmise that on the microscopic level, capillary pressure would not be capable of accepting matter into the capillaries, only of jettisoning it. Furthermore, Harvey's *vis a tergo* only partially accounted for capillary pressure. 'The heart is not the sole power which propels the fluids', Fordyce declared.[6] As a pioneer in research upon blood pressure, he found that capillaries exerted higher pressure than the larger arteries owing to their smaller radii while the volume of blood itself exerted constant pressure on the sides of those capillaries.[7] How then could any substance such as lymph enter into the blood-stream if it was caught in the tissues outside of the capillaries?

The answer, Fordyce and others surmised, was the lymphatic system. An entirely separate system would be needed first to absorb whatever fluid the blood ejected, and secondly, to transport it back to the circulatory system. Proving the existence of such a system was hardly an easy task, but wax and mercury injections coupled with microscopic research would play a vital role in further demonstrating its structure and function.

In addition to Lieberkühn and Fordyce's importance to the early study of the lymphatics, we must also note Albrecht von Haller's advances that prompted further investigation of the microscopic vessels. Few practitioners had as much influence on the development of physiology as Albrecht von Haller. His study of the nervous system *De Partibus Corporis Humani Sensibilibus et Irritabilibus* (1753) was enormously popular among physiologists, with English[8] and French[9] translations appearing in 1755 and numerous reprints throughout the latter half of the century. The Göttingen physician's greatest and most controversial claim was that muscle fibres contracted, that is, exhibited irritability, in a manner which was completely

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[6] G. Fordyce, *Elements of the Practice of Physic in Two Parts* (London, 1770), p.35.
[7] Ibid., pp.35-7. Fordyce's work went into multiple editions until the 1790s, often inducing new research on the circulation and lymphatic systems.
[8] A. Haller, *A Dissertation on the Sensible and Irritable Parts of Animals, by M.A. Haller, M.D. President of the Royal Society of Sciences at Gottingen,* translated by M. Tissot (London, 1755).
[9] A. Haller, *Dissertation sur les parties irritables et sensibles des animaux* (Lausanne, 1755).

independent of the nervous system which exhibited sensitivity.[10] This, of course, had implications for the role of absorbent vessels in the digestive and lymphatic system. Not only did irritable peristaltic action move the products of digestion through the intestines, but also, the villi of the intestine absorbed digestive nutrients, or chyle, without the aid of direct nervous connections. This 'animal absorption' was so powerful that the lacteal ducts seemed to transport chyle all the way from the intestines to the thoracic duct and then into the blood stream without any aid at all. This was already the subject of speculation; physician Alexander Stuart (1673-1742) had discovered this same effect in 1733, concluding that healthy peristaltic motion was necessary for lacteal absorption lest a patient starved.[11] However, Stuart did not fully understand the cause of peristalsis.

More important than the immediate effect of peristaltic irritability was its cause. For Haller, the presence of lymph and mucus around muscles or vessels triggered irritability in the body. The mucus, which he supposed surrounded muscle fibres, was produced from insensible lymph.[12] Because this lymphatic mucus was contained separately from the circulatory and nervous systems, it followed that independent irritability, not nerve or venous connections, controlled its movements. The body itself seemed doused in lymph, present in almost all interstitial areas of the body. Patients endowed with more mucosal lymph seemed more prone to displaying uncontrollable irritability. 'This opinion is strengthened by observing that children, in

[10] Haller's most vociferous critic was Robert Whytt (1714-1766), a pupil of Alexander Monro at Edinburgh. Whytt's, *Essay on the Vital and other Involuntary Motions of Animals* (Edinburgh, 1751) offered a more mechanical view of sensibility. He argued that muscle contractions depended, at least in part, on nerve connections.

[11] Stuart incorrectly surmised that bile from the gall bladder provoked peristaltic motion in the gut. He examined a soldier who had died from a wound to the gall bladder and showed no signs of peristaltic action. A. Stuart, 'An Essay on the Use of Bile in the Animal Oeconomy', *Philosophical Transactions of the Royal Society*, 36 (1729-30), pp.341-63.

[12] A. Haller, *A Dissertation on the Sensible and Irritable Parts of Animals*, p.59.

whom the mucus predominates, are much more irritable than adults; which is evidently proved from the quickness of their pulse'.[13]

From where did Haller's new irritability arise? Was it evidence of a soul or was it completely material in nature? Haller pointed to simple experiments to bolster his claim that the soul had nothing to do with the peristaltic motion of muscular irritability. The soul, he conceded, was most likely seated in the brain, but when one removed the head from the body of frogs and other experimental animals, muscular irritability still occured. General movement appears even after death. Additionally, as vivisectionists such as Haller well knew, certain tissues and organs could be pricked and cut without the animal feeling pain in that specific locale.[14] This contrasts with nerve sensitivity: the conscious action exhibited in the nervous system's sensation of pain or voluntary movement.

Historian Stephen Gaukroger has recently posited that this sudden division between sensitivity and irritability allowed naturalists to create a new 'moral science' which provided a bridge between the natural-philosophical and the moral and psychological. Haller's own position 'was to argue that the powers that organs have which are manifested in irritability derive directly from God, and require no intermediary soul'.[15] According to Gaukroger, this was just one of many eighteenth-century projects which called into question the mechanical philosophy. Although Haller and other observers did not abandon material explanations for vitalistic ones, the idea of a simple mechanical body proved untenable. Organ systems such as the digestive or lymphatic were clearly interrelated, but now, paradoxically, they acted independently as though innervated locally. As Gaukroger states:

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[13] Ibid., pp.58-9.
[14] For a more detailed analysis of Haller's irritability experiments, see H. Steinke, *Irritating Experiments: Haller's Concept and the European Controversy on Irritability and Sensibility, 1750-90* (Amsterdam, 2005).
[15] S. Gaukroger, *The Collapse of Mechanism and the Rise of Sensitivity: Science and the Shaping of Modernity 1680-1760* (Oxford, 2011), p.339.

> They manifest something that can be characterized as a life force, by contrast both with a view of matter as inert, and that of being acted upon from the outside. The local, intrinsic regulation of the operations of organs brings with it a rethinking of the nature of matter. But it also brings with it a rethinking of the idea of an organism. Centralized, extrinsic regulation of organs ... provided a source of unification for the organism whose organs these are. Local, intrinsic regulation of the kind advocated by Haller prompts the question of what the unity of the organism consists in.[16]

This is not to say that Haller was a vitalist. As we will see in chapter 9, later vitalists, especially of the Montpellier school, would take issue with Haller's assumption that irritability arose out of observable corporeal forces within tissue fibres. However, following Haller, experimenters could interpret these forces using the microscope and thereby study the irritable properties of vessels and organs. Experimenters could poke and prod tissues, nerves, and villi: in fact, any and all parts of the body. In doing so, Haller found irritability in the form of contractions in various parts of the lymphatic system but not the arterial system. 'Upon examining the circulation in animals with the microscopes, I have never observed any contraction in the arteries'.[17] Despite Haller having viewed the circulation for 'whole hours in fishes and frogs' he could not see an independent pulse (aside from the pulsation of the heart) or contractions. Haller claimed, 'I could not have failed to discover it with the microscope.'[18] On the other hand, the microscope revealed that the minute lacteal vessels of the lymphatic system displayed independent motion. Examining intestinal villi, Haller wrote:

> The *lacteal vessels* contract and empty themselves, upon being touched with oil of vitriol [sulphuric acid], and what proves that they have a considerable share of irritability is, that though they are ever so full of chyle at the time when the animal dies, they empty themselves, and contract in such a manner, that you cannot discover any cavity remaining.[19]

The medical implications of this absorbent property of the lacteals were not lost on other microscope users. Henry Baker speculated that chemical supplements could be

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[16] Ibid.
[17] A. Haller, *Dissertation on the Sensible and Irritable Parts of Animals,* translated by M. Tissot, p.42.
[18] Ibid., pp.42-3.
[19] Ibid., pp.43-4.

added to food and taken up by the lacteals into the body for medical purposes. This, of course, was hardly an original idea by the mid eighteenth century; however, Baker sought to bolster this practice with further empirical evidence gathered from microscopy. Through his microscopic research on blood, he had found that a drop of sal volatile oleosum (ammonium carbonate mixed with salt of tartar) reduced but did not destroy globules of blood on a microscope slide. Baker reasoned that adding sal volatile to foods would result in the microscopic lacteals absorbing the chemical and transporting it into the bloodstream. This could aid physicians in preventing blood coagulation.[20] The concept, however, presupposed that the microscopic villi housing the lacteals would not contract (or shrivel and fall off) when presented with ammonium as they had when presented with Haller's sulphuric acid.

Interestingly, Haller's microscopic observations proved less tenable than his general theory of irritability. As we shall see in this chapter, Alexander Monro, William Hunter and others sought to prove Haller's hypothesis that the lacteals acted as microscopic absorbent structures, but their endeavours were far from successful throughout the latter half of the century. Conversely, Haller's more general notion of irritability became reified as the autonomic nervous system. In 1777, the Marquis de Condorcet (1743-1794) praised Haller's notion of irritability. 'The book in which Mr. Haller published these findings caused an era of revolution in anatomy' announced Condorcet joyously.[21] Haller had founded the notion of a new force residing in the living body in which organs 'perform each function on its own; physiology which had too long relied on metaphysical and uncertain ideas could finally be based on a

[20] H. Baker, *The Microscope Made* Easy, p.115.
[21] N. de Condorcet, 'Éloge de M. Haller', in *Oeuvres de Condorcet,* eds. A.C. O'Connor and M.F. Arago (12 vols., Paris, 1747-9), ii, p.300. See also, O. Sontag, 'Albrecht von Haller on the Future of Science', *Journal of the History of Ideas*, 35 (1974), pp.313-22; and S.A. Roe, *The Natural Philosophy of Albrecht von Haller* (New York, 1981).

general fact and proven by experience'.[22] Indeed, by the early nineteenth century, the peristaltic action of the autonomic nervous system was undeniable. Experimenters could confirm it through multiple trials. François Magendie (1783-1855), like Haller, autopsied intestinal tissue and found that digestion occurred up to two hours after death.[23] Clearly some kind of independent motion existed in the body. And yet what about Haller's research on the lacteals and villi? As we shall see, microscope researchers did take up where Haller left off in their pursuit of the micro-structure of lacteals and digestive tract. Ultimately, their experiments led to the realisation of an independent organ system: the lymphatic system.

6.2. Examining intestinal villi

Returning again to Lieberkühn, we find in his 1745 *Dissertatio anatomico-physiologica* an emphasis on the micro-structures involved in digestion. In this work, Lieberkühn was most interested in the function of the intestinal villi; he sought to show that they connected to the larger system of lymphatic vessels and circulatory system. Fortunately, we have an illustration of the microscope Lieberkühn included in this research. He had popularised the dissecting microscope which is illustrated in a large fold-out plate in his *Description d'un M*icroscope Anatomique of 1745 (Fig. 3).

Lieberkühn's microscope had significant advantages. He could fasten his sample to the microscope itself while making minute cuts to the tissue under view. On the other hand, the microscope lacked the advantage of a reflecting mirror to backlight tissue samples or a condensing lens to focus light more directly through the sample. Thus, with the lack of back lighting, Lieberkühn was able to see surface textures clearly, but not many internal features. Apparently, this was not problematic for

[22] N. de Condorcet, *Oeuvres de Condorcet*, ii, p.300.
[23] F. Magendie, *An Elementary Treatise on Human Physiology*, translated by J. Revere, 5th edn. (London, 1855), p.333.

Lieberkühn. One of his major contributions to microscopy was the invention of the Lieberkühn reflector, a dish attached around the lens, designed to reflect light down upon the sample. Lieberkühn's attention to surface texture is evident from the illustration of intestinal villi (Fig. 4).

FIGURE 3. An early dissecting microscope of Lieberkühn.
J. Lieberkühn, *Description d'un Microscope Anatomique* (1745), plate 1.

FIGURE 4. Intestinal villi. Plate 1, Fig 2 From J. Lieberkühn, *Dissertatio anatomico-physiologica* (Leiden, 1745), p.32.

Lieberkühn wrote:

> The villi are shown in rows. . . near the bottom on the right side, a certain one of the villus is especially clearly seen, likewise, near its tip, another leans on to that one from the rest, which ought to be easy to discern. The other villi can be discerned with somewhat more difficulty from the image; however, with the help of the microscope they are most clearly revealed.[24]

Of special note here is Lieberkühn's emphasis on illustrating the vascular structure of the villi. The question which perplexed him, however, was whether these capillaries in a villus were connected to the lymphatic system or the circulatory. Lieberkuhn opted to connect both systems together, thus rendering the lymphatics as part of the broader circulatory system: a viewpoint which did not overtly contrast with Harvey's doctrine. When examining the larger vascular structure of the mesothelium which surrounds the exterior of the intestines, Lieberkühn found a convoluted network of vessels (Fig. 5). These appeared to connect with one another, sharing their contents, much like the spongy tissue Harvey had suspected.

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FIGURE 5. vascular structure of the mesothelium. From Lieberkühn, *Dissertatio anatomico-physiologica* (Leiden, 1745), p.36.

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[24] Lieberkühn, *Dissertatio anatomico-physiologica*, p.32.

Another of Lieberkühn's contributions was to hypothesise that this system branched into small fibres (the lacteals) which had small pores feeding directly into the digestive system in order to absorb nutrients. According to Lieberkühn, one could see these small pores, or *ampullae*, on the tips of the villi illustrated in figure 4 above. The question of ampullae fuelled much of the subsequent microscopical examinations on the lacteals and villi. William Hewson spent considerable time investigating Lieberkühn's claims in the early 1770s and was eventually convinced that an opening existed on at least some villi.[25] Ultimately, these observations were inaccurate (there are no villus orifices: villi absorb chyle into the lacteals through intracellular absorption), but even as late as 1784, John Sheldon illustrated what he believed were the ampullae of Lieberkühn capable of absorbing nutrients from the digestive tract into the lymphatic lacteals (Fig. 6).

FIGURE 6. Illustration from J. Sheldon, *The History of the Absorbent System* (London, 1784) table 1.

[25] W. Hewson, *The Works of William Hewson*, ed. G. Gulliver (London, 1846), p.192.

In the top three figures of this illustration, Sheldon depicted the internal side of a dissected duodenum without magnification. There are small openings like pock-marks on its internal surface. These are the much-disputed ampullae which Sheldon magnified using an unspecified microscope and illustrated in the lower four figures.

As we know today, the ampullae hypothesis was false. Sheldon most likely witnessed the openings of the larger bile ducts in the digestive tract. Using a Lieberkühn microscope, it is possible that Sheldon had actually witnessed goblet cells on the villi. Nevertheless, such illustrations as those above indicate a broad desire for microscope reporting and reproducibility among the eighteenth-century anatomical community.

6.3. Lymphatic discoveries and the problem of priority

i. Monro *Secundus* versus William Hunter

Microscope evidence concerning the absorbent lymphatic system was central to bitter feuds among a number of prominent anatomists, most famously Alexander Monro *secundus* and William Hunter. In 1755, Monro had set out to study under Hunter in London despite the fact that he was already heir apparent to his father's professorship at Edinburgh. Before arriving in London, Monro had sent Hunter his dissertation on the reproductive and lymphatic systems.[26] In this, Monro had maintained that the lymphatics were absorbing vessels. To his surprise, when Monro arrived in London, he found Hunter teaching the same doctrine. Monro assumed that Hunter had drawn this information from his dissertation and was not being credited for the discovery.[27] Monro's dissertation actually concerned the structure of the reproductive organs while his assertions concerning the lymphatics were an aside. Monro had written: 'The

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[26] A. Monro secundus, *De testibus et de semine in variis animalibus* (Edinburgh, 1755).
[27] N.B. Eales, 'A History of the Lymphatic System, With Special Reference to the Hunter-Monro Controversy', *Journal of the History of Medicine*, 29 (1974), p.283.

valvular lymphatic vessels, in all parts of the body, are a system of absorbent veins; they do not emanate from arterial twigs, as is generally believed.'[28] In other words, Monro had tried to make sense of the complex network like the one Lieberkühn had illustrated (Fig. 5) but, unlike Lieberkühn, Monro had found that the tiny lymphatic vessels were entirely separate and disconnected from the blood vessels; however, Monro did not have the time to investigate the subject further.

It is possible that Monro never would have been interested in the lymphatic system at all had an accident not affected one of his experiments. He had used mercury injections to trace the course of the vas deferens to the epididymis. When one of the seminal canals broke and spilled mercury into the surrounding tissue, Monro noticed that the minuscule lymphatic vessels of the epididymis absorbed the mercury. The priority dispute was moot as lymphatic absorption was long theorised but never proven, and Monro's observation was hardly sufficient proof. Additionally, others had begun working on the project. Historian Nellie Eales has found that Haller had conducted an almost identical set of experiments on the epididymis just two years earlier.[29] Similarly, Mark Akenside read a paper before the College of Physicians on the absorbing lymphatics in 1755.[30] Monro accused both Hunter and Akenside of plagiarism, but his aggression was especially pointed at Hunter because Hunter had laid claim in his lectures to having discovered the absorbing power of the lymphatics in the epididymis. It was this minor point that broke the student-teacher relationship beyond repair. For our purposes, the feud illustrates the problem of reporting experimental and microscopic evidence.

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[28] A. Monro, *De testibus et de semine*, p.55.

[29] N.B. Eales, p.283. See also A. Haller, *Observationes de vasis seminalibus* (Gottingen, 1745); A. Haller, 'Archiatri et Consil. Aul. Reg. in Acad. Gottingens. Medicinae Prof. Necnon R. S. Lond. Sodalis, Observationes de Viis Seminis', *Philosophical Transactions*, 46 (1749), pp.340-345.

[30] M. Akenside, 'Observations on the Origin and Use of the Lymphatic Vessels of Animals', *Philosophical Transactions of the Royal Society of London*, 50 (1757), pp.322-8.

The intellectual battle between Monro *secundus* and William Hunter is well known, but difficult to assess because most of their contemporary commentators were themselves allied to one faction or the other and had studied under either the Monro family or the Hunter brothers. If we examine their professional lives more closely, we find that Monro and William Hunter were not so dissimilar in their outlooks on physiology. Both were keen microscopists and had received a similar medical education. Each followed the Boerhaave method of searching for isolated lesions in order to trace disease. Equally important, both adhered to the doctrine of the body as a mechanical instrument. Boerhaave had directly taught Alexander Monro *primus* who then taught Hunter, who had tried to teach Monro *secundus*. The elder Monro fashioned the school of Edinburgh according to Boerhaave's course descriptions. The Edinburgh curriculum that Monro *secundus* would inherit taught Newtonian natural philosophy and the mechanical philosophy as well as a strong emphasis upon chemistry. More importantly, the Edinburgh curriculum emphasized cadaver dissection and model building, in other words, the reproducibility of the anatomical gaze. Monro taught systematically: his lecture notes detailed organ systems separately, similar to how anatomists still teach today. He also stressed a systematic approach to disease etiology; he classified various pathogens by their physical affect upon these localised systems such as infections and lesions upon vessels versus cancers upon specific organ systems.[31] Microscopy was part of this research.

Despite Edinburgh's reputation as a centre of anatomical teaching, towards the latter half of the eighteenth century London was becoming a rival to the Edinburgh school thanks largely to William Hunter. He had left Monro *primus* to acquire fame

[31] Historians have studied the Edinburgh school and the Boerhaavian doctrines in far more depth than can be presented here. See, for example, G.B. Risse, *Hospital Life in Enlightenment Scotland* (Cambridge, 1986); also, C. Lawrence, 'Ornate Physicians and Learned Artisans: Edinburgh Medical Men 1726-1776' in W.F. Bynum and R. Porter (eds.), *William Hunter and the Eighteenth-Century Medical World* (Cambridge, 1985), pp.153-176.

and fortune with his popular anatomy demonstrations, open to the paying public; in contrast, the professors at Edinburgh prided themselves on a focused curriculum for elite students. William Hunter and his brother John had built an astounding reputation for medical teaching, William for his lectures, and John in surgical demonstration and hospital teaching. William also amassed a large microscope and specimen collection: far superior to the collections in Edinburgh. To foster learning, he desired that his best students serve as assistants who would keep the instruments, prepare slide samples and anatomical preparations for the lecture. Hunter's private letters reveal that his student-assistants (such as William Hewson) typically disliked the domineering Hunter; nevertheless, they became excellent dissectors and preparers in their own right under his and his brother's tutelage. It was not surprising then that the Hunters became rivals of the Edinburgh Monro family.

To return to the feud: in addition to Monro's assertion that the lymphatic vessels operated as absorbents, the younger anatomist also claimed to have discovered tiny tubules connecting the epididymis to the testicles (these tubules are now labelled 'efferent ducts'). Again, Hunter mentioned these in his lectures *after* having read Monro's dissertation and Monro felt slighted. These observations may seem as trivial as any other priority dispute, but more was at stake than mere acclaim. The tubules indicated yet another mechanical aspect of the body. Microscopic systems were coming into focus: they provided evidence in support of the materialist and mechanist position that the body was a machine or, at the very least, could be understood in the same manner as one could disassemble and examine the components of a machine. Whole bodily systems were coming into focus and altering the concept of the body as a complex and wholly material machine. Although Monro taught ovism - that reproduction depended solely upon eggs and not the spermatozoa - these ducts revealed how spermatozoa transferred from their origin in the seminiferous tubules to the epididymis and vas deferens. The body was becoming more and more like a system of plumbing: with microscopic and macroscopic tubules connecting even the most minute parts.

Long before their feud, Hunter had hopes that his young pupil Monro would continue to work in London rather than fill his father's rival post in Edinburgh. In his classes with Monro, he stressed the benefits of performing post-mortems in London and, equally important, the need for making microscopic and macroscopic preparations.

> Sir, said I, you mean to devote yourself to anatomy, and to teach it. You will therefore wish for every possible advantage. In London we have commonly a greater plenty of subjects than at Edinburgh, and for the reason perhaps have made some progress in the practical part of anatomy; particularly in the arts of making preparations.[32]

Nonetheless, the ambitious Monro left for Berlin despite Hunter's advice, possibly because he had suspected that Hunter would appropriate his future discoveries or keep him in a subordinate role. While in Berlin, in 1757, Monro published *De venis lymphaticis valvulosis* which declared his priority as sole discoverer of the epididymis duct and the absorbing lymphatic vessels. Hunter quickly responded and a pamphlet war broke out between the two which continued off and on over the next two decades.

To explore the social side more thoroughly, we find that historians differ on the nature of Hunter's difficulties in earning acceptance within the broader medical community. Roy Porter records that Hunter became a 'doyen of the profession',[33] while C. Helen Brock, editor of the Hunter correspondences, writes that William Hunter had been 'condemed not only in his own day but also through the years for his treatment of the Monros in his *Medical Commentaries*'.

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[32] W. Hunter, *Medical Commentaries: Part 1* (London, 1777), p.20.
[33] R. Porter, 'William Hunter: a Surgeon and a Gentleman', in Bynum & Porter (eds.), p.11.

> Men who were pupils of the Monros tended to give their support. Sylas Neville, in his diary for 1771, reported that Dr. Manning, an Edinburgh trained doctor, 'had no good opinion of Dr. Hunter, the celebrated Anatomist in London on account of his illiberal manner of carrying on his disputes with Dr. Monro.[34]

Indeed the *Medical and Philosophical Commentaries* published by the Society of Edinburgh dutifully reported his brother John's discoveries relating the lymphatic and circulatory systems as well as Hewson's, but are curiously reticent concerning William Hunter's achievements.[35]

On the other hand, I have found that William Hunter was not explicitly ostracised from the broader scientific and publishing community in London. Despite the fact that he received no formal acclaim from the Royal Society and was completely ostracised from the Royal College of Physicians, Hunter had considerable influence on the London medical reviews and commentaries. [36] He retained powerful allies in the publishing business: Henry Baker was certainly a supporter of his research and encouraged Hunter and Hewson in their microscopical research. Ralph Griffiths editor of the *Monthly* Review, supported him in the Monro controversy.[37] Long after Hunter's death, Sheldon, Cruikshank and other disciples continued his case against Monro.[38]

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[34] W. Hunter, *The Correspondence of Dr William Hunter, 1740-1783,* ed. C.H. Brock (London, 2008), p.18.

[35] J. Hunter 'On the Digestion of the Stomach After Death', *Medical and Philosophical Commentaries,* 2, no. 1 (1774), p.27. Also, 'Medical News', ibid., pp.198-200; J. Walsh, 'On the Electric Property of the Torpedo', ibid., p.408-9; A. Duncan, 'Case of a Puncture of a Nerve', ibid., 4, no. 1 (1774), p. 210. For commentaries on Hewson, see ibid., pp.381, 466, and ibid., 1, no. 2 (1774), p.320.

[36] The Royal College of Physicians prohibited practitioners of midwifery, which included William Hunter, from joining their ranks. See L.G. Stevenson, 'The ***Siege of Warwick Lane***: Together with a Brief History of the Society of ***Collegiate Physicians*** (1767-1798)', *Journal of the History of Medicine and Allied Sciences*, 7, no. 2 (1952), pp.105–21. Hunter interpreted the College as anti-Scottish. The College prohibited many of the students who had been accepted at Edinburgh such as dissenters, surgeons, and Roman Catholics. It also made acceptance virtually dependent on an Oxbridge degree. Hunter wrote to his former mentor William Cullen: 'You no doubt know how contemptuously the College of Physicians here have treated all Scotch degrees indiscriminately. You must have heard what efforts have been made, and are now in the making, by our countrymen in this place to support the honour of our Universities.' William Hunter to William Cullen, 1 October, 1754, quoted in ibid., p.108.

[37] R. Griffiths, (ed.), 'Monthly Catalogue: Medical', *Monthly Review,* 68 (1749-1803), p.548.

[38] W. Cruikshank, 'Introduction', *The Anatomy of the Absorbing Vessels,* pp.2-8.

Clearly, Hunter's dispute with Monro can offer much to those interested in the social sphere of medical men in the eighteenth century. For our present purposes, we may note that the feud fostered careful studies of body tissue and it signalled an ever-increasing trend towards observing localised physiological action. Both the Monro and the Hunter factions displayed great skill and determination to isolate ever smaller parts of the human body beyond the lymphatic system. For example, in 1758, the two anatomists entered into a separate dispute concerning the observation of blood vessels in cartilage. Monro rejected one of Hunter's earlier claims to have witnessed blood vessels in cartilage. Early in the 1740s, Hunter had employed wax injections to trace the route of minute blood vessels.[39] The existence of blood vessels in cartilage was a considerable topic of debate at the time: the lack of blood flow would explain why cartilage injuries took so long to heal. On the other hand, the presence of blood vessels would explain why cartilage grows, or why malignancies, such as tumours and osteoarthritis, seemed to spread within cartilage. In this case, it appears that Hunter's technique was flawed: he had actually witnessed blood vessels in a young body, not in the matured cartilage of an adult. Hunter must have realized his mistake: in all subsequent lectures and writings, he no longer maintained that mature cartilage was supplied by blood. Nevertheless, Alexander Monro attacked him for his error.

Monro's denunciation stung Hunter severely as the elder Hunter had conducted the research some time before and had considered the issue settled. The observations were made 'when I was a young man, and a very young anatomist.'[40] Hunter then clashed with Monro on a number of other issues: Monro's microscopical observation that bone marrow prevented brittleness in bones as well as Monro's claim

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[39] W. Hunter, 'Of the Structure and Diseases of Articulating Cartilages', *Philosophical Transactions*, 42 (1742 - 1743), pp.514-21. Monro's attack might not have been fuelled purely out of vengeance. Monro's father, Alexander Monro *primus* had already established himself as an expert on cartilage and ossification. Perhaps his son, Monro *secundus*, felt the need to defend his family territory.
[40] W. Hunter, *Medical Commentaries: Part 1* (London: Baker, 1777), p.53

that ossification occurred when cartilage condensed into bone.[41] And finally, yet another priority dispute took place over the discovery of the lachrymal gland.[42] To make matters worse, Monro moved the object of his attack away from Hunter and towards his student and laboratory assistant William Hewson. In a 1769 pamphlet, Monro claimed priority for Hewson's discovery of the microscopic lacteal vessels in the villi and intestines.[43]

ii. Monro *Secundus* versus William Hewson

We have already encountered Hewson in a previous chapter on the history of blood corpuscles; Hewson, like many of the anatomists mentioned in the chapter, had also studied under the Monros in Edinburgh, as well as in London. In London, Hewson quickly stood out from other students and came to the attention of John and William Hunter. When he returned from further training in Edinburgh in 1762, William Hunter gladly employed the young student as a laboratory and lecturing assistant.

While working for Hunter, Hewson had made a name for himself with his microscopic work on blood, comparative physiology, and the discovery of lymphatic vessels across various species. This time, Monro became the master accusing his student of plagiarism. Monro claimed that Hewson had gleaned much of this information from him in his own lectures during Hewson's brief time as a student at Edinburgh in 1761. Specifically, he attacked Hewson's discovery of lacteal vessels in fish and birds for which the young Hewson had won the Copley Medal in 1769.

The discovery of the lacteals in any animal was no easy task, and especially so in fish and fowl. Only mammals develop true villi along the internal lining of the

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[41] S.W. McDonald and D.Russell, 'What Did William Hunter Know About Bone?', *Clinical Anatomy* 18 (2005), pp.155-63.

[42] Hunter, *Medical Commentaries: Part 1*, pp.53-4

[43] A. Monro secundus, *A State of Facts Concerning the First Proposal of Performing the Paracentesis of the Thorax and the Discovery of the Lymphatic Valvular Absorbent System of Oviparous Animals: in Answer to Mr. Hewson* (Edinburgh, 1770).

intestines, so isolating the lacteals without their microscopic villi ends is difficult. Hewson's great achievement was to use a series of injections and microscope observations to distinguish between blood vessels and lymph vessels in turtles, skates, and geese: thereby once again giving evidence that the lymphatics were a separate organ system this time in non-mammals. Hewson employed wax injections, dyed with vermilion, to outline the veins and arteries. Next, he injected the lymphatic vessels with mercury, and he microscopically and macroscopically traced their separate paths. This would have been an exceedingly difficult experiment as the lacteals and blood capillaries share their routes along much of the intestines. This was further complicated when the mercury-infused lymphatic vessels neared the muscular coating of the intestines because here the submucosa consists of a network of lymphatic vessels entwined with the blood-carrying arterioles and venules. At this point, the mercury 'spread from cell to cell, very uniformly, over a great part of the intestine.'[44] Not surprisingly, many of the surviving Hewson microscope slides are of the intestinal submucosa. He must have laboured long hours trying to differentiate between lymph vessels and blood vessels and, equally important, crafting slide samples that effectively displayed this difference.

As for Monro, we do not do not know his original findings on the subject of the lymphatic vessels as he reported them in his lectures (which varied annually) but they were certainly not as detailed. He claimed to have witnessed lacteals in the villi of fish and fowl but did not rely on the complex system of injections that Hewson had

[44] W. Hewson, 'An Account of the Lymphatic System in Amphibious Animals', *Philosophical Transactions*, 59 (1769), p.203. Note: Hewson's use of the word 'cell' here differs from the modern concept in that he refers to a space between the capillaries not to the individual cells themselves. This is not to say that Hewson did not see individual cells (he may have in his study of gland tissue) but like most anatomists of the day, he did not theories that organic tissues were composed of universal components.

carried out with such proficiency. In answer to Monro's 1769 accusation of plagiarism, Hewson fought back in a lengthy essay.[45]

Unfortunately for Hewson, he had indeed accidentally taken credit away from Monro, but not on the issue of the lymphatics. In Monro's lecture course, he described a life-saving operation called paracentesis: a procedure to drain fluid excess from a body cavity using specialised needles. Hewson later published an identical discovery in 1767.[46] Monro called him out on the subject and several former Edinburgh students attested to Monro's case. Hewson briefly acknowledged this, but included the acknowledgement alongside his lengthy counterattack upon Monro over the discovery of the lymphatic vessels.[47] In a private letter to an American friend he wrote:

> I have been so far engaged in Anatomy as to have made out a subject of dispute with one of my brethren but fortunately I have catch'd him and have given him such a rap on the knuckles as will make him careful how he meddles with me for the future.[48]

Indeed, Monro seems to have dropped the issue and only brought up the claim again eleven years after Hewson's death.[49] Even commentators deemed the dispute between Hewson and Monro rather trivial. It received far less commentary than Hunter's well-publicised feud with Monro, possibly because Hewson did not have the pool of friends that an older and a more renowned man like Hunter could muster to his aid.

More important for us, the argument reveals the great emphasis these anatomists placed on a new aspect of physiology: viewing the lymphatics as a complete organ system and delineating it down to its microscopic components.

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[45] W. Hewson, 'Appendix Relating to the Discovery of the Lymphatic System in Birds, Fish, and the Animals called Amphibious, Being a Vindication of the Author's Right to these Discoveries in Opposition to the Claim of Dr. Aleander Monro', in G. Gulliver (ed.), *The Works of William Hewson* (First publised, 1770, reprint: London. 1846), pp.91-113.
[46] W. Hewson, 'The Operation of the Paracentesis Thoracis, Proposed for Air in the Chest, with some Remarks on the Emphysema, and on Wounds of the Lungs in General', *Medical Observations and Inquiries, by a Society of Physicians in London,* 3 (1767), p.372.
[47] W. Hewson, 'Appendix relating to the Discovery of the Lymphatic System in Birds, Fish, and the Animals', in *Experimental Inquiries, Part I,* p.92.
[48] William Hewson to Mr. Powell (undated: c.1770). Glasgow Special Collections (MS Hunter H494).
[49] A. Monro secundus, *The* **Structure and Physiology of Fishes** *Explained and Compared with Those of Man and Other Animals* (Edinburgh, 1785).

Although discussion of the microscope itself was relatively absent from the debate, the thousands of microscope slides within the surviving Hewson-Hunter collection testify to the anatomists' need to prepare sample upon sample of evidence. Hewson's personal contribution was to illustrate how non-mammals mirrored humans in their lymphatic physiology. It was a groundbreaking achievement in comparative anatomy and physiology.

In addition, the debate between Monro, Hunter, and Hewson also illustrates the problems of reporting microscopical and macroscopical evidence in lectures. Although Monro and Hunter earned their livelihood through the anatomy theatre, their fierce battles centred on topics which would not have been easy to display. As any histologist knows, the lacteals and other lymph vessels are subjects that are best discerned through the lens or, for larger vessels, through direct dissection, not in an amphitheatre. Future researchers emphasized publications and illustrations over lecture notes and the theatre began to serve a more didactic function rather than as a place to announce new discoveries.

iii. William Hewson versus William Hunter

Returning again to William Hewson, we find that he was no stranger to controversy. While in the midst of his paper war with Monro, Hewson also incurred the resentment of his own mentor and employer William Hunter. Hewson was on dangerous ground: when he entered Hunter's service at twenty-three years old he was wholly dependent on the elder anatomist for his living, lodging, and most important, his sponsorship to the Royal Society. Indeed, Hunter's employment allowed the young Hewson to 'earn money at a time when most students in surgery are spending it.'[50] Nevertheless, Hewson resented Hunter's control over his course material, the long hours of

[50] George Gulliver quoting Mrs. Hewson, in G. Gulliver (ed.), *The Works of William Hewson* (London: Sydenham Society, 1846), p.xv.

preparation work, and Hunter's monitoring of his professional work. Hewson wrote in 1770: '[I] extended the bounds of the Science which I cultivated, but even in these Dr. H. has car'd to restrain me and most unwisely.'[51]

Hunter's collection of microscopes and slides was also a source of dispute. Hewson needed Hunter's permission each time he sought to use the microscopes stored in their collection. Not surprisingly, this infuriated Hewson as some of the microscopes belonged to him and the thousands of microscope slide preparations in the collection were a product of Hewson's own labour.

> These preparations afforded a high matter of dispute … having no cases to keep my microscopes in I got a cupboard made for that purpose and in them I put them and the Preps. When Dr. H. saw this he was extremely displeased he said it was taking a property to myself . . . He said he must have them in his museum. I told it was exceed'ly improper they could not be seen there, I could not go for them every time I wished to shew them. I saw clearly his meaning. He wanted to have them lodged where he could take advantage on any future dispute of preventing my getting at them, which proceeded from an unreasonable jealousy not to say duplicity of behaviour. I was determined not to comply.[52]

So strained was the relationship between the two that their mutual friend, the great Benjamin Franklin, tried to step in to reconcile their differences.[53] He failed. Hewson left Hunter's employment in 1771 and soon after set up a rival lecture course and anatomy theatre on Craven Street. The two remained bitter enemies. Despite interventions by friends, they likely did not patch their relationship before Hewson's death by sepsis in 1774.

Perhaps Hunter's competition with Monro drove him to work his assistant too hard. Perhaps Hunter felt slighted that Hewson, his employee, received the acclaim of the Royal Society and he thus sought a share in Hewson's success; after all, the Royal

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[51] William Hewson, (1770) 'Notes By William Hewson on William Hunter', in C. Helen Brock (ed.), p.76.
[52] Ibid., p.80.
[53] Letter from Franklin to Hunter, 30 October 1772 in ibid., pp.72-3.

Society awarded the Copley Medal to Hewson, not Hunter and it had been Hunter who conveyed Hewson's testimony to the Society.

When Hunter confronted Hewson on the subject of the lymphatic discoveries, Hewson revealed that their methods of experimentation differed. Hewson was enthusiastic to vivisect live animals, whereas Hunter found the practice appalling.

> Dr. Hunter told me it hurt him that I had not invited him to assist me in the Discovery [the lacteals in birds 1767] and I seemed jealous of his robbing me of it. My answer to this was that my discoveries were made on living animals I have never once thought of inviting him from knowing his dislike of such experiments.[54]

Such quotes offer a rare glimpse into the social and scientific practices of eighteenth-century anatomy. One wonders how these practitioners found time to pursue their research while simultaneously engaging in such fierce intellectual battles. An aspiring young anatomist such as Hewson needed to foster a careful relationship with his mentor, but eventually free himself in order to make a separate reputation. Even then, the odds might be stacked against him if he was without a large group of allies in various scientific circles. Moreover, allegiances could be costly: as one could glean from Alexander Monro's attack on Hewson, Hewson's proximity to Monro's nemesis Hunter may have been the real motive behind the fray. Becoming one of the 'greats' of science or medicine appears to have assured a life of near-relentless conflict.

6.4. Re-enactments of these experiments.

With these disputes and counter-observations in mind, one must wonder what these experimenters actually witnessed when examining the controversial lymphatic system. Fortunately, Hewson and Hunter's microscope slides are preserved today in the Hunterian Museum at the Royal College in London. In the spring of 2011, I had the

[54] Ibid., pp.76-7.

opportunity to examine these slides as well as the eighteenth and early nineteenth-century Quekett Slide Collection.

Recalling Lieberkühn's microscope investigations of intestinal villi, he had been adamant that the lacteals were spongy in appearance.[55] In what could only have been a painstaking experiment, Lieberkühn had cut an individual villus in half and examined its internal structure under the microscope. In his own investigations of villi, Hewson found this 'sponginess' plausible: 'This is an appearance which may be as well explained, from knowing that each villus contains a network of arteries and veins, which being inflated might occasion the villi to assume a spongy appearance.'[56] If we examine Hewson's own slides under the microscope, we see a similar view of sponge-like villi.

[55] Lieberkühn, *Dissertatio anatomico-physiologica*, p.13.
[56] W. Hewson, 'Of the Structure of the Villi of the Intestines', G. Gulliver (ed.), *The Works of William Hewson* (London, 1846), p.192.

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FIGURE 7 **FIGURE 8**

Human villi from the eighteenth century through a modern dissecting microscope (Fig. 7, left) and a compound light microscope (Fig. 8, right) at 40x. [RCSHC/Hewson/Case 11]

Viewing figure 7, I would argue that the 'sponginess' which Lieberkühn saw was indeed caused by pockets or pores on the surface of the villus or, when he cut it in half, within it. Hewson was able to witness the vessels more clearly than Lieberkühn because he had been able to see *through* the villi using light microscopy as we can see in figure 8. To Hewson, the minute capillaries and lacteal vessels within the villus appeared to make a spongy or 'cellular'[57] appearance, while Lieberkühn likely saw only surface texture because of his reliance on a dissection microscope.

The Hewson slide collection displays his continuous struggle to differentiate between blood vessels and lymphatic vessels. The numerous tissue samples from the mesothelium (the vascular coat of the intestines) testifies to the difficulties he faced with this project. The difference between chyle-filled white lymph vessels and blood capillaries is difficult to discern, even when the blood capillaries are stained with vermilion as Hewson has done in the slide below (Fig. 9). Under a dissecting

[57] Ibid., p.150.

microscope, Hewson's mesothelium slide does appear similar to the one illustrated by Lieberkühn from Figure 5.

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FIGURE 9. Eighteenth-century stained mesothelium vessels through a dissecting microscope. [RCSHC/Hewson/18] Note: compare to Lieberkühn's illustration of mesothelium in Fig. 5.

Light plays tricks upon the eye when viewing tissue through the lens. Hewson and others wrote frequently of the 'cellular' coat of the intestines: that is to say, the mesothelium. Had Hewson and his colleagues actually witnessed cells in the modern sense of the word? Again, it is difficult to measure the magnification of specific eighteenth-century lenses with any certainty because of the inaccuracies with which these microscope users reported lens power. We know, for example, that Leeuwenhoek, Hewson and others could discern red blood corpuscles measuring between 6μm and 8μm; however, I could not view individual cells (in the current sense) from the preserved tissue samples under magnification at 400x with a modern Leitz objective. This may be due to tissue decay, the thickness of Hewson's slides, or the lack of modern staining techniques. In general, the use of the word 'cell' is misleading in the writings of Hewson, Hunter, Cruikshank and their colleagues. Had they seen the actual specialised squamous cells that compose the epithelium coating the intestines? From his description of mercury filling these ‘cells’ and from the following observation of his slide preparations, these microscope users more likely witnessed the tissue space *between* the network of lymph and blood vessels in the mesothelium. This becomes clear if we take the above slide and examine it under a light microscope.(Fig. 10.)

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12 April 2011

FIGURE 10. The same stained mesothelium slide through a modern light microscope at 40x [RCSHC/ Hewson/18].

This is similar to the view Hewson would have seen through his screw-barrel microscope lenses. Hewson recorded: 'I have traced those vessels to my satisfaction; but what becomes of them after they have got to the cellular coat is not easy to determine.’[58] Still using vermilion mixed with glue and injecting it into the vessels leading to the villi. Hewson wrote:

> [W]hen this was not pushed to great minuteness the villi appeared exactly as Lieberkühn has painted them, with a network of arteries and veins on each.[59]

[58] W. Hewson, ‘An Account of the Lymphatic System in Amphibious Animals’, p.202.
[59] W. Hewson, 'Of the Structure of the Villi of the Intestines', G. Gulliver (ed.), *The Works of William Hewson* (London, 1846), p.188.

Hewson then records that he did in fact see pores, or ampullae, on the villi:

> It might be here objected that these were only lacerations of the villi but I am persuaded they were not from having, on repeatedly examining them, observed the pores or orifices very distinct and empty; whereas, were they lacerations, I think I would have seen the injection in them, as the villi were so much distended by it.[60]

What could Hewson have seen that he interpreted as pores on the intestinal villi? We can never know for certain, but recreating this experiment, one can easily see the complex network of lacteals and blood capillaries which make up the inner fabric of the villi.

FIGURE 11. Eighteenth-century stained villi through a light microscope at 100x. [RCSHC/Hewson/14]

[60] Ibid. p.192.

6.5. Conclusions

Hunter, Monro, Cruikshank, and Hewson each overemphasised the role of these lacteals in absorbing digestive nutrients. For them, the lymphatic lacteals were the only conduits by which nutrients were absorbed from the digestive system into the body, and their quest for the ampullae was an effort to confirm this fact (in reality, the lacteals absorb only fatty acids; the blood capillaries do indeed absorb sugars from the digestive tract directly). There are several reasons why these eminent physiologists may have exaggerated the importance of the lacteals. Obviously each experimenter was engaged in priority debate: more or less laying claim to the multiple parts and function of the lymphatic system. The heightened importance of the system as a whole added weight to their claims.

On the other hand, if we assume that these experimenters were strict empiricists, they simply saw no microscopic evidence that the venous and arterial capillaries were involved in digestion. The lacteals, according to most of these experimenters, had small pores that enabled them to absorb digestive nutrients, the capillaries did not. Although this notion of lacteal pores was mistaken, we can nevertheless say with certainty that microscopic evidence was integral to ascertaining the role of the lymphatics and thus the very tissue which displayed Haller's new irritability.

For all the priority disputes, lecture courses, and publications, by the end of the eighteenth century other anatomists still doubted the existence of microscopic lymphatic vessels. For example, Paolo Mascagni (1755-1815) fully mapped the lymphatic passageways throughout the body in 1787.[61] Reviewers praised Mascagni's copperplate illustrations as being more accurate than those of Lieberkühn[62]; however,

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[61] P. Mascagni, *Vasorum lymphaticorum corporis humani historia et ichnographia* (Siena, 1787).
[62] J. Bostock, 'Absorption', in R.B. Todd (ed.), *The Cyclopaedia of Anatomy and Physiology* (5 vols., Sherwood, 1836), i, p.21.

unlike Lieberkühn, Mascagni relied on macroscopic observation rather than microscopic experiments to form his view of the lymphatic system. He could not say for certain what the vessels looked like on a microscopic level or how they functioned; he considered the subject a lost cause. Microscope researchers still doubted the existence of minute lacteal vessels in the intestinal villi, or, at the very least, the exact function of those vessels. Writing in 1821, François Magendie (1783-1855) wholly denied the existence of lymphatic vessels in birds (with some exceptions) and cast doubt on Lieberkühn's description of microscopic lacteals within the villi.[63] Even simple problems such as whether or not the lacteals opened directly into the digestive tract could not be solved. Thus we are led to conclude that vicissitude rather than foundational discovery characterized eighteenth-century studies on the lymphatic system.

Nevertheless, the injections and microscopic observations of the eighteenth century continued into the nineteenth. Incidentally, it was another Scotsman, John Goodsir (1814-67), who finally settled the dispute decades later. In his article of 1845, Goodsir wrote,

> Dr. William Hunter and Mr. Cruikshank were quite correct in describing and figuring radiating lacteals within the villi, but they were led into error in describing those vessels as opening on the free surface of the gut, partly by imperfect instruments and methods of observation, partly by the general prejudice of the period in favour of absorbent orifices.[64]

L.S. Jacyna, the renowned historian of nineteenth-century histology, has studied Goodsir's experiments in depth. According to Jacyna, Goodsir's own observations may have been correct, but Goodsir himself had to deduce the function of the lacteals from his general claim that cells had permeable membranes and acted as absorbants.

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[63] F. Magendie**,** *An Elementary Treatise on Human Physiology,* p.333.

[64] J. Goodsir, *The Anatomical Memoirs of John Goodsir*, ed. W. Turner (Edinburgh, 1868), p.394; originally published as 'The Structure and Function of the Intestinal Villi' *Anatomical and Pathological Observations* (Edinburgh, 1845). Goodsir appears to have had access to Hunter and Cruikshank's original specimen slides at the Royal College of Surgeons in Edinburgh which he may have used to interpret their observations.

Moreover, as Jacyna notes, his achromatic microscopes were not much better suited to solve the problem than those of the eighteenth century. According to Jacyna, Goodsir likely inculcated his own students with cell theory before they peered through the microscopes. He demonstrated the structure and function of cells or epithelial tissue using diagrams prior to allowing them to view the actual tissue through the instruments. What does this imply for the history of the microscope? Even by the nineteenth century we see the enduring problem of witnessing microscopic objects without preconceived notions. As Jacyna states: 'There is no realm of pure facts accessible to the mind; rather, in order to be intelligible, perceptions must be structured and categorized by reference to prior schemata and concepts.'[65] Goodsir's evidence was not particularly more detailed or persuasive than that of Hunter, Hewson, or Cruikshank. However, the broader notion of cell theory had entered into biology thanks largely to Schwann and Schleiden. Such detailed theories were non-existent in the eighteenth-century corpus of physiology.

Nevertheless, the microscopic endeavours of the eighteenth century were integral to the ongoing discoveries concerning the lymphatic system. Although microscopy did not foster consensus, it helped plant the idea of lacteals as necessary components of life across species. Microscopy also revealed the internal structure of the villus: lacteals coupled with capillaries. It did not discern how those vessels absorbed nutrients. Nonetheless, the enduring question prompted continued research into the concept of absorption which cell-theory and osmosis theory would later explain.[66] Or, as Schickore explains, 'Discordant results did not engender overall

[65] L.S. Jacyna, 'John Goodsir and the Making of Cellular Reality', *Journal of the History of Biology* (1983) p.77.
[66] For more on osmotic theory, see J. V. Pickstone, 'Absorption and Osmosis: French Physiology and Physics in the Early Nineteenth Century', *Physiologist*, 20 (1977) pp.30-7.

distrust in the microscope. Rather, they stimulated second-order discourses and practices.'[67]

Admittedly we will find very little information about specific microscopes and other instruments in eighteenth-century anatomical texts pertaining to the lymphatic system. However, as I have argued, this is not to say that microscopy had become a fledgling pursuit. True, as Edward Ruestow and Marian Fournier have shown, vessel injections and wax models might have become a more popular and visible course of research during the eighteenth century; however, results obtained from these methods were similarly inconclusive.[68] I would argue that these experiments frequently occurred in conjunction with microscopic observations, not in place of them. As Ratcliff has found in the case of microscopy in eighteenth-century natural history, the microscopy became less visible in publication, but was certainly in use. The frequent use of the microscope in these texts combined with the lack of explanation about the instruments themselves suggests that microscopy had become expected and routine, rather than sporadic.

One can make a similar comparison to the lack of discussion surrounding vivisection. Vivisecting live animals to study digestion, lymph, or general blood circulation was commonplace in eighteenth-century anatomy; it is doubtful that the moral implications of vivisection had dissuaded many anatomists from discussing its importance. Nevertheless, these same authors seldom include passages describing the exact steps one would take in vivisecting an animal to experiment upon the lymphatic vessels; rather, they took to vivisection as a matter of routine, confident that their readers would have first-hand knowledge of basic vivisection techniques.

Despite their constant bickering, the work of these physiologists had the major impact of replacing the Boerhaavean notion that the body operated as a unified

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[67] Schickore, *The Microscope and the* Eye, p.242.
[68] E. Ruestow, *The Microscope in the Dutch Republic*, p.100; M. ***Fournier***, *The* **Fabric of Life*, p.133.***

machine, with the more complex view of the body operating along semi-independent organ systems: the circulatory, lymphatic, reproductive etc. Although Boerhaave had directly trained many of the more eminent physicians and physiologists of the eighteenth century, physiology was slowly moving away from the strict mechanical explanations he espoused and towards more nuanced systems such as von Haller's notion of irritability.

In an age when Adam Smith espoused the 'invisible hand of the market' these physicians, mostly Scotsmen, clearly thrived under the competitive medical market. Their anatomical discoveries, their crowds of students and their personal wealth testify to that fact. Historians will have a difficult time assessing how many discoveries they made out of the need for rivalry and competition rather than empiricism, but we can nevertheless see that the rivalry prompted anatomists to look more closely at individual structures in the body, including microscopic ones.

Chapter 7. Case Study: Pathology and Plague, 1780-1800

The plague came to Russia in the summer of 1770. It followed on the heels of the imperial army as Russian soldiers marched back and forth across the peacetime quarantine lines in their war against the Turks. Russia had achieved victory after victory against the Ottoman Empire, pushing their Janissaries across the Danube and destroying their fleet in the Aegean. Now the plague crossed their own lines, decimating Russian battalions in Moldavia. The first official response was denial. Catherine the Great wrote to Voltaire in April that the rumours he had read were vicious libels meant to belittle her victories:

> The Gazettes of Paris and Cologne, having made up so many lost battles on our behalf . . . they are advised to slay my army with the plague. Do you not find that pleasant? In the spring, apparently those who have died of plague will rise to fight again. The truth is that none of us has had the plague. [1]

The czarina's military commanders were less insouciant; nevertheless, they advised her to conceal the disease or regard it as merely seasonal fever.[2] However, her physicians and surgeons, such as Gustav Orraeus and Danilo Samoilowitz, knew better. Most of these medics were trained in the Boerhaavian corpus and William Cullen's nosology which had asserted the contagious nature of bubonic plague. Quickly, these military physicians implemented field quarantines; however, their efforts failed. The plague had engulfed Moldavia, Ukraine, Poland and Western Russia by the end of the year. It arrived in Moscow early in 1771.

By September, nearly a thousand people a day were dying in Moscow alone. The total casualties are difficult to assess, but historian John T. Alexander places them around 50,000 in Moscow while Catherine the Great believed that nearly 100,000 had

[1] Catherine II to Voltaire [François Marie Arouet], 11 April 1770. *Electronic Enlightenment*. Ed. Robert McNamee et al. Vers. 2.3.
http://www.eenlightenment.com/item/voltfrVF1200149_1key001cor/>.

[2] This included General Johann Christoph von Stoffeln, commander of the Russian forces in Moldavia. Stoffeln himself died of the plague that year.

died in the city.[3] Admitting that Russia had been struck by an epidemic, Catherine was nevertheless reluctant to use the word '*peste*':

> There are maladies in Moscow. There are spotted fevers, malignant fevers, hot fevers with spots and without spots, which carry off many people despite all the precautions we have taken.[4]

Aggravating the situation was Moscow's near-famished population prior to the epidemic. The city had lost much of its glory and revenue after the capital had been relocated to St. Petersburg. Nevertheless, Moscow's population had swelled as people from the countryside and itinerants were attracted to the promise of industries in the city. In an attempt to obviate dangerous miasmas, Catherine had proscribed polluting industries within the city walls. This had the opposite effect as unemployed workers crowded into ghettoes. Soldiers deserting from the front also flocked to the city. The infamous plague riot of Moscow began on September 15. It began when terrified devotees flocked to the Varvarksie Gates to pray to an icon of the Virgin Mary. The Archbishop Amvrosii, believing the plague to be directly contagious, feared that the assembly would exacerbate the epidemic. He attempted to disperse them, and, when this failed, he attempted to remove the icon. Incensed, the mass of worshipers soon turned into an angry mob. They ransacked the city, murdered the archbishop, and attempted to break the quarantine. The physicians and surgeons who attended the hospitals and quarantines were also hunted down, including the surgeon Danilo Samoilowitz, who barely escaped with his life.

Prince Grigory Orlov, the former lover and favourite of the Empress, volunteered to take control of the situation in Moscow. Olov had failed in his mission as peace delegate to the Ottomans, and now sought to win back the Empress's favour. Unlike the Empress, Orlov had been quick to identify the disease as plague and, once

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[3] J.T. Alexander, *Bubonic plague in early modern Russia*, pp.257-8.

[4] Catherine II to Voltaire [François Marie Arouet], 17 October 1771. *Electronic Enlightenment*. Ed. Robert McNamee et al. Vers. 2.3. University of Oxford. 2011. <http://www.e-nlightenment.com/item/voltfrVF1220106_1key001cor/>.

in Moscow, he assembled a five-man plague commission.[5] The commission was tasked with cajoling surgeons and physicians into manning the hospitals and quarantine stations.

Danilo Samoilowitz (1744-1805)[6] was a core member of this commission because, as a military surgeon, he had followed the disease all the way from Moldova, through Poland, and to Moscow. He later wrote, ‘I was an eyewitness to all that catastrophe may cause the human species this cruel disease’.[7] Although these committee members assumed that plague was directly contagious, they were not overtly concerned with etiology in 1771. Their immediate focus was on treatment which included the standard defensive tactics of quarantine, fumigation, and the distribution of vinegar for use as disinfectant. Samoilowitz rubbed his patients’ bodies with ice and treated them with cinchona extract, a muscle relaxant, which would have prevented them from shivering.[8] It is also widely reported that Samoilowitz inoculated himself at the height of the epidemic and thus fell ill with a mild form of the plague which, he claimed, precluded a more virulent infection.[9]

The plague subsided by 1772 although the commission remained intact until 1775. Over the next two decades, the physicians and surgeons who had treated the epidemic set about recording their observations.[10] Most, such as in Orraeus’ 1784 account, attempted to explain the failure of the quarantines by viewing plague as a

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[5] The Moscow plague commission members included: Doctors Gustave Orraeus, Kas’ian Yagelski and Afanasii Shafonskii as well as military surgeons Christian Grave and Samoilowitz. J.T. Alexander, 215.
[6] Like many of these Russian authors Samoilowitz’s anglicized name has been a source of much confusion. He may also be found under Daniel Samoïlowcz, D. Samoilowits or D. Samoilovich.
[7] D. Samoilowitz, *Lettre a l'Académie de Dijon avec réponse a ce qui a paru douteux dans le Mémoire sur l'inoculation de la peste* (Paris, 1783), p.17.
[8] D. Samoilowitz, *Lettre sur les Expériences des frictions glaciales pour la guérison de la peste* (Paris, 1781).
[9] C.G. Ontijd, *A treatise on mortal diseases; containing a particular view of the different ways, in which they lead to death, and the best means of preventing them* (London, 1798), 179.
[10] For example, G. Orraeus, *Descriptio pestis quae anno 1770 in Jassia et 1771 in Moscua grassata est* (St. Petersburg, 1784); C. de Mertens, *Traité de la peste, contenant l’histoire de celle qui a régné à Moscou en 1771* (Vienna, 1784); I. Vien, *Loimologia ili opisanie Morovoi lazvy* (St. Petersburg, 1786); A. F. Shafonskii, *Opisanie Morovoi iazvy* (Moscow, 1775); H. Rutzky, *Dissertatio inauguralis medica sur de peste* (Strasburg, 1781).

pestilential miasma. As we shall see, Samoilowitz strongly refuted this position. However, he also chided other medical men who emphasised the *contagium vivum*, contagion spread by living, microscopic animalcules. He criticized their dependence on the microscope in his 1783 book, *Mémoire sur la peste*, and cautioned his readers not to search in vain for microscopic animalcules or 'fomites'.[11]

In the years following the plague Samoilowitz travelled widely: to Holland, France, and, possibly, England and Scotland. He took a doctorate in Leiden and published most of his works in Paris. Upon returning to Russia in 1784, he became chief physician of all quarantines during another outbreak in the Ukraine. However, this time he carried along a Dutch microscope which he used during post-mortems. Thereafter, he seems to have reversed his position on the utility of the microscope in a 1787 letter to the Société Royale de Médicine de Paris.[12] In this work, he praised the benefits of applying instruments to the study of disease. He wrote of using a microscope constructed by Dellebarre, a noted optician who worked in Paris and Leiden. Hailed as the first achromatic microscope, the Dellebarre microscope was a complex, but very popular, design featuring six lenses (Fig.1).[13] The Dellebarre microscope did not feature the achromatic 'doublet' of crown and flint glass which Giovanni Amici and J.J. Lister would popularise in the 1830s. It did, however, include bi-convex lenses made of both crown and flint glass (see Fig. 2). With this, Samoilowitz recorded seeing microscopic 'worms' in the buboes of plague victims.[14]

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[11] D. Samoilowitz, *Mémoire sur la peste, qui, en 1771, ravagea l'empire de Russie, sur-tout Moscou, la capitale, & où sont indiqués les remedes pour la guérir, & les moyens de s'en préserver* (Paris, 1783), xiii-xvi.
[12] D. Samoilowitz, *Lettre au sujet des reserches microscopiques sur le venin pestilentiel* (Paris, 1787).
[13] L.F. Dellebarre, *Memoire sur les differences de la construction et des effets du microscope* (The Hague, 1777).
[14] D. Samoilowitz, *Lettre au sujet des reserches microscopiques sur le venin pestilentiel*, pp.198-200.

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FIGURE 1: The Dellebarre Microscope

Museum of the History of Science, Oxford [Inventory: 47187]. This microscope is fitted with a speculum (mirror), a condensing lens, and a specimen stage attached to the stand. A Lieberkuhn reflector is attached to the barrel

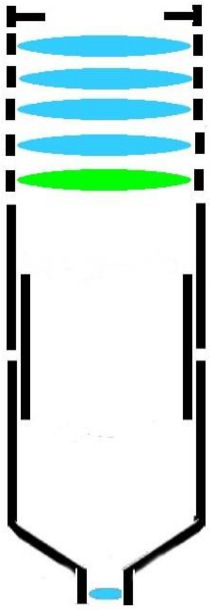

FIGURE 2: The Dellebarre lens system.

The top four eyepieces are bi-convex lenses made of crown glass. They are removable and interchangeable. A fifth eyepiece lens made of flint glass corrects for chromatic aberration.

The barrel of the microscope is composed of three brass cylinders which can be screwed to shorten or lengthen the distance to the lower objective lens

Such a case study as Samoilowitz's observations during the late eighteenth-century plague outbreaks opens many lines of enquiry. It also raises a number of questions. For example, did Samoilowitz's travels to the medical centres in western Europe reform his notion of disease or alter his position on the use of the microscope?

This case study sheds light on the extensive intellectual and trade networks between the major centres of medical science and the European frontier. Russian surgeons and physicians as far away as Moldovia and Moscow were clearly knowledgeable of the latest advances in microscope manufacturing as well as disease theory. Moreover, Samoilowitz's later travels to Holland, France, and Britain are indicative of a large network of medical training across late eighteenth-century Europe. Although microscopical observations of plague may not have been commonplace, Samoilowitz's use of the microscope in distant Russia reveals an impetus among some members of this network to apply the microscope to the study of contagion and disease in addition to its uses in general anatomy.

7.1. The persistence of medical microscopy outside the academies

i. Danilo Samoilowitz's observations of plague

Before addressing the details of Samoilowitz's observations, one must understand their context in contemporary medical theory. Historians such as Margaret Pelling, John V. Pickstone, Mark Harrison, and others have commentated on the development of contagionism, anti-contagionism and sanitation in the late eighteenth and nineteenth centuries. The topic is far too vast to adequately address here, although we must acknowledge some bare facts concerning the state of disease theory at the time. Disease etiology did not always contribute to medical practice. Sanitary practices, quarantines, and cures sometimes contradicted the miasmatic theories of the day. Furthermore, accurate etiologies were not often the concern of general practitioners and sanitarians. Margaret Pelling has found this to be often the case in her discussion of nineteenth-century sanitation reform.[15] The same holds true for many practitioners in the eighteenth century. Nonetheless, a number of physicians and surgeons did

[15] M. Pelling, *Cholera, Fever, and English Medicine 1825-1865* (Oxford, 1978), p.33.

address the origins of disease. Although it is commonly assumed that miasmatic theories of disease dominated late eighteenth-century medical discourse, we must be careful not to categorise eighteenth-century viewpoints as either wholly miasmatic or contagionist. Some diseases, such as venereal afflictions, were quite obviously transmissible through personal contact. Furthermore, in the case of 'miasmatic' disease, the question remained whether or not the 'bad' air spread disease or the catalyst was some type of fomite, particle, or living animalcule carried by the air. Similarly, the very meaning of 'contagion' took on many different forms. Contagion could mean transmission, possibly from the air or from infected objects. Or it could refer to person-to-person contact. There was considerable effort to delineate 'infectious' diseases from 'contagious' ones with no real consensus being made.[16] Observations of plague and smallpox dominated much of this discussion as did the question of inoculation. Proponents of inoculation drew attention to the degrees of danger and communicability but their efforts were not wholly convincing. A wide range of explanations concerned fevers and other distempers. Some physicians conceived of dangerous fomites or particles present in the air or infected objects. Others maintained that foul odour arose from putrefied water or decaying matter which led to disease or caused unhealthy imbalances and fermentations within the body. In addition, there were others, such as Samoilowitz, who regarded person-to-person contact in the case of plague, but not other 'diseases of the air' such as malaria.

There was little consensus regarding nosology during the eighteenth century and much of the debate during an outbreak concerned whether or not the epidemic was in fact plague or some other form of pestilential fever. This is not to say that physicians disregarded nosology. Cullen's 1769 nosology is perhaps the best-known attempt to classify disease and it defined much of Edinburgh's medical instruction;

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[16] See, for example, A. Aberdour, *Observations on the Small-pox and Inoculation: To Which is Prefixed a Criticism upon Dr. Robert Walker's Late Publication on the Subject* (Edinburgh, 1791).

however, the focus of eighteenth-century nosology was on signs, symptoms, and cures more than etiology.[17] Even physicians who held that fevers could be transmitted from one patient to another might be persuaded that a particular epidemic, such as the plague of Moscow, was exceptional or that the course of the disease consisted of transmissible and non-transmissible stages. On the other hand, contagion theory and miasma theory do seem to occupy more polarised camps later in the nineteenth century. There was much debate on this issue during the cholera pandemics of the first half of the nineteenth century; while eighteenth-century plague epidemics may have influenced this later debate and raised concerns about the efficacy of quarantine, we must be careful not to confuse nineteenth-century terminology with earlier conceptions of disease.

Turning once more to Danilo Samoilowitz's theory of the plague, we find that his disease theory cannot be categorized as either strictly miasmatic or contagionist. He believed that pestilential matter could be transmitted from person to person, but malignant air predisposed bodies to these infections. Compared to the other members of the plague commission, Samoilowitz's theory placed greater emphasis on the dangers of direct contact with plague victims and infected material. He did not deny that the air carried certain diseases, but noxious vapours alone could not account for the widespread plague epidemic, nor could it explain why plague appeared in some regions and not others:

> If the plague could come from the corruption of the air, and that this corruption engender the plague in such a manner, why is there not plague seen in Europe in those swampy places that are dried up by the sun in summer, and exude off noxious steam?[18]

[17] W. Cullen, *Synopsis nosologiae methodica* (Edinburgh, 1769).
[18] D. Samoilowitz, *Mémoire sur la peste*, p.xx.

As an enforcer of quarantines, Samoilowitz perhaps overemphasized their effectiveness in slowing the epidemic. He attributed the success of quarantines to the contagious nature of plague:

> The air does not recognise these types of barriers. We must therefore conclude that the venom of the pestilential contagion does not infect anyone by air; but that the plague carries its virus from one sick body to another healthy one immediately after contact.[19]

Samoilowitz was also adamant that foul odours from corpses would not spread the disease. While serving at the front, he noted that the army had passed by thousands of bodies without falling sick. In this, Samoilowitz directly opposed the latest research by the Paris Société Royale de Médecine. The Société was keenly interested in sanitary conditions surrounding cemeteries, especially as the area around the Cimetière des Innocents was proving inhospitable. The society issued letters and reports in the 1770s and 1780s outlining the procedures for exhumation and reburial.[20] In doing so, it took a view that diseases were both contagious by noxious air and directly by contact: disease arose from 'molecules' emanating off sick and dead bodies, even those that had been buried in the ground for years:

> It is shown that contagious *molécules* can remain infectious for a long time, and that cadavers, whether of men or animals, are capable of transmitting the diseases to which they succumbed long after their deaths... It follows from this that by digging up ground that has covered a body for a long time, all things being equal, there extends the possibility of a new contagion.[21]

Not surprisingly, certain members of the Société Royale were displeased with Samoilowitz’s contradictory claim. One doctor who had helped compose the society’s reports was Hughes Maret, the secretary of the Académie des Sciences in Dijon. He applauded Samoilowitz’s general observations of plague, but refuted his attempts at self-inoculation as well has his general etiology. 'It is still a point to which we do not

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[19] Ibid., p.xvii.
[20] Société Royale de Médecine, *Rapport sur plusieurs questions proposées à la Société Royale de Médecine, par M. l'Ambassadeur de la Religion* (Malta, 1781), p.23.
[21] Ibid., p.23.

agree with you: this is the impossibility of contagion by air.'[22] Nevertheless, news of Samoilowitz's deeds were widely read. They often met with praise in France and also Switzerland, where Samuel-Auguste Tissot was particularly supportive of Samoilowitz's treatment using ice and cinchona.[23]

Samoilowitz's preventative measures were similarly well-received. Whether one believed that the disease inhabited air or solid particles, fumigations and other disinfectants were the standard means of preventing them. In one of its first acts, the Moscow plague commission had ordered vinegar to be distributed throughout the city as a disinfectant. In addition, Samoilowitz had ordered sulphur-based fumigations of the clothing and residences of plague victims. To test his fumigation recipes, he had criminals wear these clothes and live up to fifteen days in fumigated buildings in exchange for pardons.[24] Letters from the commission to the authorities in St. Petersburg were also fumigated and dipped in vinegar and passed over flames.[25]

What was the role of the microscope in these efforts to combat the plague? As we will see later in this chapter, Russian physicians had been acquiring microscopes for their practice well before 1770; nevertheless, there is little evidence that the microscope was an effective tool to combat or understand the plague during the 1770 Russian epidemic. One can infer this from the fact that physicians rarely if ever mention the microscope in their subsequent writings on the Moscow plague. This is not to say that doctors abandoned microscopy during the epidemic; rather, microscopy failed to contribute significantly to their explanations of the disease or its treatment. Why was this so? One might hypothesise that physicians avoided applying the microscope to infected material and tissues because they feared acquiring the

[22] Quoted in D. Samoilowitz, *Lettre a l'Académie de Dijon*, p.22.
[23] Ibid., p.25.
[24] D. Samoilowitz, *Description de trois poudres fumigatives antipestilentieles, inventées à Moscou par la commission conte le peste* (Paris, 1783).
[25] J.T. Alexander, p.129.

contagion. This is unlikely as the plague tracts record that surgeons and physicians frequently lanced or removed buboes from their patients. Some who supported plague inoculation, such as Samoilowitz, also dealt directly with plague material. Doctors were more likely opposed to the use of the microscope as a diagnostic tool in the case of plague. Possibly, this arose from their prior assumptions about disease. Some physicians, such as Nicolas Gabriel Le Clerc (1726-1798), believed that pestilential particles somehow spread from the air to the body but the microscope was ineffective in viewing them:

> We are aware that we see with the microscope the molecules that make up the blood, but of the devices that one can use, one cannot manage to render sensible to sight the parties that constitute the acid of the air.[26]

Samoilowitz denied Le Clerc's miasmatic view, but he also doubted the utility of the microscope during the epidemic. In his 1783 *Mémoire sur la peste,* Samoilowitz remained sceptical of contemporary observations of microscopic 'contagious animalcules':

> Some authors have considered contagions the propagation of animalcules, and concluded by repeated experiments, using the microscope, that any contagion, however terrible it may be, owes its existence, its multiplication and its rapidity to their prodigy. They pretend to show a truth of their opinion, by the spread of lice . . .[27]

Given Samoilowitz's theory of direct contagion in the case of plague, why was he initially averse to using the microscope to view the agents of this disease? The theory of microscopic animalcules, worms, and inorganic particles was widely publicised, but not well-received in the leading medical circles of Europe. Let us explore these theories in more detail.

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[26] N.G. Le Clerc, *Histoire naturelle de l' homme considéré dans l'état de maladie*, 2nd edn. (2 vols. Montpellier, 1810), ii, p.305. See also, Le Clerc, *De la contagion, de sa nature, de ses effets, de ses progrès, et des moyens les plus sûrs pour la prévenir et pour y remédier* (St. Petersburg, 1771).
[27] D. Samoilowitz, *Mémoire sur la peste,* p.xiv.

ii. Early observations of disease 'worms'

As shown in chapter 3, observations of contagious animalcules date back at least to Athanasius Kircher's studies of plague in the seventeenth century. Early eighteenth-century experimenters continued to speculate that worms and animalcules in the blood generated disease. Richard Mead's 1720s treatise on plague bolstered the view of direct contagion and the need for quarantine.[28] Jean Saguens claimed that he saw disease-causing animalcules in the blood of plague victims in 1721.[29] Some eighteenth-century surgeons and apothecaries used these observations to legitimise the practice of blood-letting.[30] Meanwhile prominent medical instructors were divided on the issue. In 1731, Thomas Apperley asserted that the air planted 'seeds' which germinated into microscopic worms. He speculated that diseases such as leprosy and smallpox had the same causative agents, microscopic worms, as other parasitic infections.[31] In contrast, Montpellier physician Jean Astruc (1684-1766) attempted to refute this view. In his 1737 treatise on venereal disease, Astruc denounced the use of the microscope as a diagnostic tool and criticised the theory of disease-causing worms and animalcules as the whimsy of 'quacks':

> It was once admitted that venereal disease might be produced by animalcules swimming in the blood, one might with equal reason pass the same judgement not only on the plague . . . but also upon smallpox, hydrophobia, itch, tetters, and other contagious diseases, and indeed upon all distempers whatsoever; and thus the whole theory of medicine would fall to the ground.[32]

[28] R. Mead, *A Short Discourse Concerning Pestilential Contagion, and the Method to be Used to Prevent It,* pp.18-19.
[29] J. Saguens, *System pestis physicum* (1721).
[30] P.L., *A Letter from an Apothecary in London, to his Friend in the Country; Concerning the Present Practice of Physick, in Regard to Empiricks, Empirical Methods of Cure, and Nostrums* (London, 1752).
[31] T. Apperley, *Observations in Physick, Both Rational and Practical,* pp.86-89.
[32] J. Astruc, *A Treatise of the Venereal Disease, in Six Books; Containing an Account of the Original, Propagation, and Contagion of this Distemper in General*, translated by W. Barrowby (London, 1737), p.150.

Nevertheless, the theory of animalcules or worms as the primary agents of disease remained well-publicised throughout the eighteenth century, [33] despite the dominance of miasma theory. However, I must again state that 'miasma' is a loaded term, which could mean a host of direct and indirect forms of contagion and disease acquisition. Moreover, those who supported miasma theory did not necessarily avoid the use of the microscopy. For example, Friedrich Hoffmann (1660-1742) had used a microscope to examine certain pathological conditions, such as tooth decay[34] and poisonings,[35] but he was a staunch supporter of the miasmatic theory of disease, and did not attempt to find the cause of any disease using his microscope. On the other hand, when addressing the chronic illnesses of tobacconists, Hoffmann warned of the 'flying atoms' and advised covering one's mouths and nostrils when around tobacco smoke and 'rinsing their throat with vinegar and even drinking of it; for nothing is better qualified to carry off and blunt those particles which adhere to the throat and stomach than such mixtures as have vinegar'.[36] This was a remedy which was apparently not lost on the Moscow plague physicians in 1771. In his own study of the plague, Samoilowitz speculated that the disease was carried by the air, but he was never in favour of the theory that plague could be transmitted via living microscopic animalcules or worms. Although his publications shortly after the Moscow plague were critical of the microscope, he reversed this position after travelling throughout Western Europe in the 1780s. He acquired his Dellebarre microscope from his patron, Prince Potemkin, and began conducting new experiments following another outbreak of plague in the Ukraine. Samoilowitz could not find any 'seeds' or 'fomites' in the

[33] See, for example, T. Hammond, *A Medical Essay on the Singular and Highly Exalted Virtues of Hammond's New-discovered Specific Pill, and New-invented Antimonial Essence* (London, 1777).
[34] F. Hoffmann, *A Treatise on the Teeth; their Nature, Structure, Formation, Beauty, Connection and Use*, 4th edn. (Dublin,1760), p.50.
[35] F. Hoffmann, *A Dissertation on Endemical Diseases; or, those Disorders which Arise from Particular Climates, Situations, and Methods of Living* (London, 1746), p.90.
[36] Ibid. p.125.

bodies of plague victims. He did, however, find microscopic worms, which he believed were generated by those seeds. These were thus the signs, not the causes, of plague. He reported his investigations to the Société Royale de Médecine in 1785.[37]

7.2. Re-enactment of these experiments

So what did Samoilowitz actually see when he looked through his lens? Did he actually witness plague bacteria, *Yersinia pestis* in the bodies of his plague victims? The answer, of course, is no. 'Bacteria' did not exist in the eighteenth century. Nevertheless, Enlightenment-era experimenters did record observing animalcules which we now know to be bacteria. As shown in Chapter 3 (Fig 1) Leeuwenhoek's observation of *semolina* bacteria in the 1680s is one such case; however, at no time did Leeuwenhoek or Samoilowitz, a century later, allude to animalcules as causative disease agents. To satisfy my own curiosity, I acquired several bacteria cultures. Unfortunately, *Yersinia pestis,* or plague, is very difficult to find these days; however, I did attempt to view a cousin of this bacteria, *Yersinia enterocolitica* through an extant Dellebarre microscope. These Dellebarre microscopes are not difficult to find as they were widely sold. Original models exist in a number of museums, including the Museum Boerhaave in Leiden and the Museum of the History of Science in Oxford. The museum Dellebarre microscopes date from the late eighteenth and early nineteenth centuries. Unfortunately, it is difficult to discern much detail when examining tissue and blood samples under these instruments. This difficulty may be due to the inferiority of the lenses or, possibly, because time has taken its toll on the instruments. Nevertheless, I could not resolve anything resembling 'worms' when

[37] D. Samoilowitz, 'Lettre au sujet de recherches microscopiques sur le venin pestilentiel et l'anatomie des cadavres pestiseres', reprinted in D. Samoilowitz, *Opuscules sur la peste qui, en 1771, ravagea Moscou; avec un discours aux éléves des hôpitaux de l'empire de Russie* (Paris, 1787), pp.196-206.

viewing a sample slide of *Yersinia enterocolitica* (Fig. 3) regardless of which objective or eyepiece combinations I used. The results are not close to the view through a modern microscope of the same sample slide (Fig. 4), although, as one can see in the latter figure, bacteria do form elongated clusters which may have been mistaken for worms if one were to see them clearly. More likely, Samoilowitz had seen some other tissue or parasite through his microscope. One cannot help but recall Catherine Wilson's assertion concerning late seventeenth-century microscope users: that they had been faced with the task of extracting meaning 'from the optically indeterminate'.[38] My re-enactment experiments suggest that this quandary continued into the late eighteenth century. Nonetheless, we must note that microscope users such as Samoilowitz were not dissuaded from their endeavours.

[38] C. Wilson, *The Invisible World*, p.256.

The image originally presented here cannot be made freely available via ORA because of copyright. The image was sourced at the Museum for the History of Science, Oxford Inventory no. 37937.

Figure 3: Sample slide of *Yersinia enterocolitica* seen through a Dellebarre microscope: MHS: Inventory no. 37937.

The image originally presented here cannot be made freely available via ORA because of copyright. The image was sourced at Oxford Genetics Laboratory, 01 May, 2012.

Figure 4 : The same slide of *Yersinia enterocolitica* seen through a modern Zeiss microscope. Oxford Genetics Laboratory

Of course, unless I had Samoilowitz's own microscope, it would be impossible to make anything more than a suggestion as to what he actually saw beneath his lens. For our purposes, it is more important to note how truly difficult microscope observation is using eighteenth-century instruments, particularly if one does not know what to look for. The complex lens system of the Dellebarre microscope may have exacerbated this problem for me, but in the hands of its advocates, such as Samoilowitz, it apparently did not.

Viewing samples through a Dellebarre compound lens would have been more difficult than through a simple lens (such as those discussed in the preceding chapters). One can use up to five lenses in the Dellebarre microscope, but this does not greatly improve magnification or even correct much of the chromatic aberration.

Nevertheless, the Dellebarre microscope was immensely popular particularly among chemists and minerologists: we find it in the laboratory of Antoine Lavoisier and illustrated in Étienne-Claude Marivetz's (1728-1794) *Physique du monde*.[39]

The next section will further explore the role of such instruments and medical education in eighteenth-century Russia.

7.3. Instruments in Russia

i. Importing knowledge to Russia

How did a Dellebarre microscope end up in the hands of a Russian plague physician? This example is just one of many which reveal the extensive trade of philosophical and mathematical instruments beyond the major centres of European instrument production. Russian interests in scientific instruments from England and the Netherlands dates back to Peter the Great and his celebrated travels to those countries in the 1690s. The eighteenth-century Tsars favoured not only foreign ideas, but also equipment and personnel. Founded in 1725, the Russian Academy of Sciences was staffed almost entirely with foreigners, mostly notably Euler, Bernoulli and a large coterie from the German-speaking principalities. However, the Prussians did not dominate Russian science for long. This is most evident during the reigns of Empresses Elizabeth and Catherine II who had favoured the intromission of French, Dutch, and English culture over Prussian influences. The subsequent surfeit of foreigners into the Russian clerisy is well known, thanks largely to the research of Anthony Glenn Cross, who has also traced the emigration of Russian intellectuals abroad.[40]

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[39] É-C. Marivetz's *Physique du monde,* (7 vols., Paris, 1783), iii, plate XII.

[40] Among others, see A.G. Cross, *St Petersburg and the British : the City Through the Eyes of British Visitors and Residents* (London, 2008); *By the Banks of the Neva: Chapters from the Lives and Careers*

The academy needed more than just educated personnel; it also required philosophical and mathematical instruments. As far as purchasing instruments is concerned, London apparently comprised the better workshops. By the early eighteenth century, Henry Styles, Carver, Marshall and John Rowley had sold instruments to the Russian Czars or their agents. These were mostly navigational or astronomical devices, specifically quadrants, telescopes, clocks, and sundials.[41]

Not content with mere purchases from abroad, the newfound Russian Academy of Sciences recruited a number of foreign engineers and craftsmen to open workshops in St. Petersburg. The academy recruited a craftsman named Leutmann from Leipzig and some forty workers to manufacture instruments.[42] The recruitment scheme was expanded in 1734 under the guidance of polymath Prince Dmitry Kantemir. Among the émigrés was the London mathematical instrument maker Benjamin Scott, who eventually rose to become head of the Academy's Instrument Workshop in 1747, much to the chagrin of his Russian-born competitors, such as Andrei Nartov who was an excellent craftsman and lathe-maker in his own right.[43] With its eyes on Western Europe, the Academy of Sciences modelled itself on various western academies. Although its purchases included many English-made instruments; the academy itself maintained a relationship to its own workshops that was much like the Académie des Sciences in Paris. Workshops, such as Scott's, were not free commercial enterprises like those in London but were dependent on the Academy and directly catered to its needs.

These Russian workshops must not have been as successful as intended because the demand for foreign microscopes remained steady. This is evident in the

of the British in Eighteenth-century Russia (Cambridge, 1997); and *By the Banks of the Thames: Russians in Eighteenth-Century Britain* (Newtonville, MA, 1980).
[41] A.G. Cross, *By the Banks of the Neva*, p.226.
[42] P. de Clercq, 'The Trip to Russia: A Report on the 18th Scientific Instrument Symposium', *Bulletin of the Scientific Instrument Society*, 64 (2000), p.21.
[43] A.G. Cross, *By the Banks of the Neva*, p.229.

fact that Prince Potempkin gifted the Dellebarre microscope to Samoilowitz sometime in the 1780s. There are extant sales records from John Cuff's workshop in London to the Moscow Medical Chancery (the forerunner of the Russian Ministry of Health) as early as 1755. One of Cuff's customer's, James Mounsey, was a recruit of Kantemir and an army physician. Mounsey eventually rose to become personal physician to the Empress Elizabeth and Director of the Medical Chancery under Peter III. Along with Baker's well known text-books on microscopy, Mounsey ordered several complete sets of microscopes and at least two small pocket magnifiers with convex lenses 'such as anatomists often make use of'.[44] He also ordered barometers and thermometers 'constructed to rise 212 degrees and to descend 50 degrees below 0 or greatest degree of cold according to Fahrenheit'. In conclusion to the order, Mounsey took great pains to ensure that Cuff would not falter in his commission or in packaging his merchandise. He must have known that Cuff's financial situation was dire and that Cuff had recently declared bankruptcy. He may also have heard rumours that Cuff often had difficulty in supplying his clients' demands. Mounsey reminded Cuff's patron Henry Baker:

> Pray let Mr Cuff take care to execute this commission right and with diligence as it may be a recommendation to his work in this country. Let them be well disposed in proper lenses and well packed up. These are for our Medicine Chancery.[45]

Mounsey's order testifies to the superiority of Cuff's workshop, or, perhaps the prestige of owning English-made microscopes in Russia. In his order, Mounsey also stressed that the sets of microscope should be well-packaged for shipping and for later use: 'They are much needed for travel ... my practice is so extensive that I pass the greater part of my time in my coach on the streets.'[46] Their use among travelling

[44] JRL, GB 133 MS 19 (Henry Baker Correspondence), vol. 6, p.145: James Mounsey to Henry Baker, 10 Apr 1755.
[45] Ibid.
[46] Ibid.

practitioners therefore suggests that these microscopes served the purpose of field diagnosis rather than anatomical study.

So long as their trade secrets were not threatened, manufacturers such as Cuff benefited from an immense potential clientele abroad. Their reputations attracted clients as much as the quality of their instruments. For example, John Dollond's technique of manufacturing achromatic lenses from a doublet of crown and flint glass was already an open secret by the time his son, Peter Dollond, won patent rights to the idea. Regardless, Peter's business thrived before and after the patent claim, largely based on his father's reputation. Peter Dollond had earned the right of first refusal for the purchase of flint glass and, in theory, his telescope lenses would be the highest quality. Then the patent expired in 1772 and the price of achromatic couplets plummeted. Regardless of the legal proceedings in England, achromatic lenses could have been easily copied by foreign entrepreneurs who were not beholden to English patent law. Nonetheless, the Russian Academy of Science purchased 'une lunette Achromatique à triple objectif' from Dollond's workshop in 1781 for a hefty £46.[47] This came at a time when Peter Dollond was selling achromatic, albeit smaller, telescopes in London for a mere £2.[48] On the other hand, émigrés to Russia still produced competitive microscope designs, such as the one-metre long achromatic microscope of émigré Franz Aepinus dating from 1784;[49] Aepinus sought to popularise new achromatic microscopes in Russia with his *Description des nouveaux microscopes* in 1784; however, it is difficult to gauge the popularity of such instruments. In London, optician George Adams Jr. (1750-95) was nevertheless

[47] A.G. Cross, *By the Banks of the Neva,* p.*233.*
[48] R. Pearsall, *Collecting and Restoring Scientific Instruments (*London, 1974), p.88.
[49] P. de Clercq, 'The Trip to Russia', p.21.

critical of these achromatic microscopes and he harshly critiqued both Aepinus and Dellebarre.[50]

Of course, there were also national interests at work which may have bolstered trade with the London workshops. Britain was the first European power to ally with Russia against the Ottomans and, although its support waned later during the war, Britain was relatively dependent on Russian mining and timber to supply its industry and navy. By 1760, Russia had become the primary importer of British iron.[51] It was in the interest of state officials and academics in both countries to sustain a healthy relationship. This commercial and cultural trade had demographic consequences as well. By the mid-eighteenth century, the British presence in St. Petersburg was so visible that an 'English Embankment' had been established on the left bank of the Neva. This featured an English Club (founded in 1770), a Masonic lodge, an English playhouse, and, of course, merchant shops specialising in British wares. Equally significant was the influential role of British professionals themselves in Russia. In the words of historian Anthony Cross, ‘to the traditional types of the soldier, the sailor, the engineer, the doctor were added the stonemason, the watchmaker, the saddler, the blacksmith and craftsmen of every kind’.[52]

But how successful were the Russians in absorbing scientific notions from England? Historian Valentin Boss has drawn attention to a thriving community of natural philosophers in Russia at this time. Boss has found that Russian intellectuals both within and outside the Russian Academy of Science were quick to debate Newton’s principles. Yet, the Academy itself was critical of Newton as many of its members being German-speaking supporters of Leibniz. Conversely, Mikhail

[50] G. Adams, Jr. *Essays on the Microscope; Containing a Practical Description of the Most Improved Microscopes* (London, 1787), pp.23-4.
[51] C. Evans, O. Jackson, and G. Rydén, 'Baltic Iron and the British Iron Industry in the Eighteenth Century', *The Economic History Review, New Series,* 55, No. 4 (2002), pp.642-65.
[52] A.G. Cross, *Anglo-Russian Relations in the Eighteenth Century* (Norwich, 1977), p.2.

Lomonosov (1711-1765), stands out as an example of a German-educated member who set about trying to replicate British observations despite his critical attitude toward Newtonianism. He replicated several of Robert Boyle's chemical experiments leading him to deny Phlogiston theory. In addition, Lomonosov attempted to improve Newton's reflecting telescope, leading him to create a tilted mirror reflecting telescope in 1762, long before William Herschel more famously introduced an identical design.[53] In contrast, the medical sciences were mostly beholden to English, Scottish, and Dutch theories. Scotsmen initially held the most prominent posts as court-physicians under Emperor Elizabeth and Peter III. Some physician émigrés thrived in the environs of Russia. Mounsey's practice apparently flourished in Moscow. He wrote, 'I can have little leisure, our city is so large and my practice so extensive that I pass the greater part of my time in my chariot on the streets'.[54]

It was a different story in the Russian medical academies as French and German émigrés still dominated medical education. The first of these was former Halle and Leipzig student Johann Kerstens who became the first professor of medicine and chemistry at the University of Moscow in 1758.[55] Moreover, John T. Alexander has found that a number of Prussian and French émigré physicians predated Samoilowitz in their investigation of contagious disease. They appear to have had more influence than their private-practicing English counterparts, especially in the case of plague. Peter Friedrich Körber (1732-1799) dedicated a 1771 book on plague and other diseases in Livland (modern day Estonia) to the Russian Medical Collegium.[56] Similarly, Le Clerc dedicated *De la Contagion* to the Empress Catherine that same year. In addition, German author, Johann Rost (1726-1791) lectured at the

[53] V. Boss, *Newton and Russia: the Early Influence, 1698-1796* (Cambridge, MA. 1972).
[54] JRL, GB 133 MS 19 (Henry Baker Correspondence), vol. 5, p.145: James Mounsey to Henry Baker, 10 Apr 1755.
[55] Kerstens was the first of many Prussian and Dutch-educated medical men who received appointments at the new university. See J. T. Alexander, pp.49-50.
[56] P.F. Körber, *Abhandlung von der Pest* (Reval, 1771).

University of Moscow a miasmatist account of harmful atmosphere shortly after the plague, in 1772.[57] On the other hand, the fledgling Russian medical community was certainly not averse to English and especially Scottish practitioners. As one traveller stated, 'Physic is a study the Russians have no fancy to, the physicians are therefore scarce, and generally Scotch'.[58] As exaggerated a comment as that may be, Scottish and English physicians did hold some of the most esteemed positions at court. Catherine employed Scottish physicians Matthew Guthrie (1743-1807), James Mounsey (1710-1793) John Rogerson (1741-1823) and Matthew Halliday (1732-1801). A network of medical émigrés from Scotland to Russia was clearly in place; interestingly, the latter three of these physicians were from Lochmaben.[59] Scotsmen and English practitioners were also valued for their knowledge of epidemic disease. It was, after all, an English physician, Thomas Dimsdale, who inoculated the Empress Catherine and her family against smallpox in 1768, an act which earned Dimsdale a Baroncy in Russia and considerable acclaim back in London.[60] Similarly, Russian physicians turned to British medicine in their quest to understand plague. Even much older texts such as Richard Mead's 1720 treatise on plague appeared in Russian translation shortly after the 1770 plague.[61]

Clearly, the plague compelled foreign educators in Russia to produce a number of texts for the benefit of the Empire. However, an internal market for medical tools and other scientific equipment did not arise. Scientific equipment from abroad reached St. Petersburg and Moscow with the same regularity as scientific publications. In this regard, British instrument manufacturing dominated, although

[57] J.T. Alexander, pp.283-4.
[58] J. Richard, *A Tour from London to Petersburgh, from Thence to Moscow, and Return to London by Way of Courland, Poland, Germany and Holland* (London, 1778), p.35. A. G. Cross discusses the quote and offers a far more detailed account of English and Scottish practitioners in Russia. See A.G. Cross, *By the Banks of the Neva*, ch. 4.
[59] D. Doyle, 'Scots Doctors and their Compatriots in the Service of Eighteenth-Century Czars', *Journal of the Royal College of Physicians of Edinburgh*, 35 (2005), pp.169-74.
[60] Ibid., 56.
[61] Ibid., 283.

physicians and natural philosophers certainly acquired instruments from Holland, Germany and France as well. Yet, by the late eighteenth century, British instrument makers carried more renown and appear to be more organized in their sales to Russia. Certain agents were employed to foster this relationship such as John Hyacinth de Magellan (1722-1790),[62] a Portuguese entrepreneur who settled in London in 1763. He became a member of the London Royal Society in 1774 and earned much of his income through the dissemination of British knowledge abroad, particularly to Russia and France. Magellan was responsible for keeping the Russian society up to date with the latest copies of the *Philosophical Transactions.* More importantly, he supplied instruments and laboratory apparatuses. Most likely, he earned a percentage or was compensated by London booksellers and instrument workshops. He also published his own French-language books on instruments and experiments which he then sold abroad.[63] R.W. Home has found that Magellan's role is indicative of an active attempt on the part of the British scientific community to entice Russian buyers: more often than not, it was Magellan, not the Russian Academy which took the initiative in these purchases.[64]

Aside from importing knowledge and instruments, one must not overlook the influx of educated foreigners into Britain. Russian visitors were particularly visible in Britain as we will see in the next section.

[62] Born João Jacinto de Magalhães.
[63] For example, Magellan's *Description des octants et sextants Anglois* (London, 1775).
[64] R.W. Home, 'Scientific links between Britain and Russia in the second half of the eighteenth century' in A.G. Cross, (ed.), *Great Britain and Russia in the Eighteenth Century: Contacts and Comparisons, Proceedings of an International Conference Held at the University of East Anglia Norwich, England, 11-15 July 1977* (Newtonville, MA, 1979), pp.212-20.

ii. Continental manufacturing in the late eighteenth century

a. England and Scotland

Politically and economically Russia began to turn more favourably to Britain and Holland rather than Prussia. The Prussians were dangerously close to encroaching upon Russia’s western territories and wary of a wholesale victory for Russia during the Ottoman war. In terms of science, Prussia’s intellectual circles did not hold the same prestige they once did during the early reign of Catherine II. When the Empress sent agents to study and train, she explicitly chose the British and Dutch academies and universities over those of Prussia. The ulterior motive for this was not only the importation of knowledge, but the construction of a new academic elite within St. Petersburg modelled on the British and Dutch traditions. Gerard L’E. Turner likened the scientific societies of England and Holland to a sponge, ‘where the advanced ideas of Newton, Boyle and Hooke were generally absorbed by many routes into the mass’.[65] In the case of Russia, this was particularly difficult as there was no pre-existing pool of academics or universities with which to implement and disseminate ideas. ‘An institution that combined both roles was necessary. And to get things going, foreign scientists had to be persuaded to come to Russia’.[66] Vice versa, Russian students and workmen had to be sent abroad.

London, Oxford, Edinburgh, and St. Andrews were important centres for training students and craftsmen using the most recent instruments of science. Obviously the Royal Society or the medical schools in England and Scotland were choice destinations for eminent physicians and naturalists, often at the end of tours in France or Holland. Similarly, an elite group of well-funded travellers acquired instruments and expertise for their employers or institutions back home. One should

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[65] G.L’E. Turner, 'Forms of Patronage and Institutionalisation of Science in the Eighteenth Cnetury' in A.G. Cross, (ed.), *Great Britain and Russia in the Eighteenth Century*, p.199.
[66] Ibid., 198.

view these voyagers as agents rather than tourists or students. The 1745 tour of naturalist Abraham Trembley to Britain is one of many well-documented examples which Marc Ratcliff and Marian Fournier have researched.[67] They note that Trembley's primary goal was to acquire instruments especially microscopes and telescopes. These instruments were for Trembley's Dutch employer, Count William Bentinck, who then sent them along to a host of other friends and allies across the continent. In this way, London manufacturers were able to use visiting agents such as Trembley to maintain sales even when trade might be restricted as during the War of Austrian Succession or the Seven Years War. Similar networks extended to Russia. The continued primacy of British-made instruments on the continent is a testament to their quality or the prestige of owning them.

British instrument manufacturing remained strong in the latter half of the eighteenth century as well. As in the case of Trembley, later visiting envoys often made the rounds to various scientific circles but reserved their instrument budget for the workshops of London. Felice Fontana (discussed in Chapter 5), spent the whole of 1775 to 1780 travelling throughout Europe, but he appears to have spent most of his money in London acquiring instruments for the Florentine royal family and the Museum of Physics and Natural History that he helped found in 1775. Royal Astronomer Nevil Maskelyne and Jesse Ramsden aided him in this task. Biographer, Peter Knoefel writes:

> His major effort was devoted to obtaining items for the Royal Museum, for instance, 'hundreds of pieces of natural history' but chiefly physical instruments, some for the observatory and some for the instruction of physics of members of the Royal Family.[68]

Thanks largely to the research of historian Anthony Cross, we now know the experience of Russian visitors to eighteenth-century Britain as well. Cross has

[67] M. Ratcliff and M. Fournier, 'Abraham Trembley's Impact on the Construction of Microscopes', pp.91-112.
[68] P. Knoefel, *Felice Fontana: Life and Works* (Trent, 1984), p.53.

found that British scientific circles and workshops were integral to the new Russian society, particularly under Empress Catherine II. British innovation would have bolstered both the academic and military institutes of Russia. From Cross we learn of the Russian Engineer Corps and its agent Nikolay Ivanovich Korsakov (1749-1788) who travelled extensively throughout England and Scotland between 1775 and1780.[69] His journal and letters from this time are invaluable primary sources. Korsakov's first destination was Oxford. Lectures at Oxford enabled him to learn theoretic science, if not practical demonstration, although we do not know what specific lectures he attended. Oxford may have been a preferred destination because Danilo Dumaresq (1712-1805), a former fellow at Exeter College, was one of Catherine's most prominent educational advisers.[70] A small community of Russians matriculated at Oxford during this time, which seems to have conglomerated around the University Masonic Lodge. Korsakov introduced himself to the group's leader, Vasily Nikitin, who himself was a student of Savilian Professor of Astronomy, Thomas Hornsby. Nikitin likely aided Hornsby in designing the Radcliffe Observatory. He also managed at least five other Russian students and later assumed a post as an instructor in the Naval Cadet Corps.[71]

Peter van Woensel (1747-1808) also made progress as a former student in Leiden. During his travels abroad, Woensel learned Robert Sutton's new method of smallpox inoculation: to pierce the upper epidermis of a fasting patient with fluid from an immature pustule. As a testament to the legacy of Booerhaave in his

[69] A.G. Cross, 'A Russian Engineer in Eighteenth-Century Britain: The Journal of N.I. Korsakov 1776-7', *The Slavonic and East European Review,* 55, no. 1 (1977), pp.1-20.
[70] As a fellow of both the St. Petersburg Academy of Science and the London Royal Society, Dumaresq was also instrumental in organising and reporting Russia's 1761 Transit of Venus expedition to Siberia. J. Appelby, 'Danilo Dumaresq, D.D., F.R.S. (1712-1805) as a Promoter of Anglo-Russian Science and Culture' *Notes and Records of the Royal Society,* 44 no. 1 (1990), pp.34-5.
[71] G.R.V. Barratt, 'Vasily Nikitin: A Note on an Eighteenth-Century Oxonian', *Eighteenth Century Studies,* 8, no. 1 (1974), pp.75-99.

training, Woensel also began applying Boerhaave’s remedy of mercury and antimony to treat the disease. Back in St. Petersburg, Woensel worked as a physician to the cadet corps and attempted to inoculate as many of his patients there as possible. The resulting treatise *New Experiments with Mercury in the Small Pox* (English edition, 1793) received high praise even from the vitalist camps in Paris.[72]

> The Royal Society of Medicine declare, that the experiments are made with sagacity. . . M. Van Woensel has opened a new road, by the most valuable observations, which is able to conduct us to a more efficacious treatment than that which has hitherto been employed. We are therefore of opinion, that he merits the approbation of this Society.[73]

Cross suggests that Danilo Samoilowitz travelled to London and Edinburgh in 1783.[74] Unfortunately, the proof of this is scarce. On 12 April 1784, William Cullen displayed to the Royal Society of Edinburgh Samoilowitz’s gift of five of his books, [75] ‘presented at the request of the author’.[76] However, this seems to be the only sign that Samoilowitz was in Scotland. He may, alternatively, have sent those books without having visited himself. Nevertheless, Samoilowitz’s correspondence with Cullen represents a strong link between Russian disease pathology and medical instruction in Edinburgh.[77] Samoilowitz had sought to enter the Royal Society of Edinburgh as an associate member, but it is not known whether or not Cullen sponsored this endeavour. On the other hand, the fact that Cullen presented Samoilowitz's plague treatises to the society may indicate his

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[72] P. van Woensel, *New Experiments with Mercury in the Small Pox; By which is Demonstrated its Specific Virtue in that Disease,* translated by W. Fowle (Salisbury, 1793).
[73] Ibid., p.7.
[74] A.G. Cross, *By the Banks of the Thames: Russians in Eighteenth Century Britain,* pp.141-2.
[75] These included *Memoire sur la Peste* and four other unnamed 'small publications on the same subject.' Royal Society of Edinburgh, 'Donations Presented to the Royal Society of Edinburgh', *Transactions of the Royal Society of Edinburgh,* 1 (1780), p.80.
[76] Ibid., p.24.
[77] For more on this, see J. Appleby, 'John Grieve's correspondence with Joseph Black', *Medical History,* 29 (1985), pp.401-13.

interest in the contagionist perspective of the plague and the use of the microscope in diagnostic medicine.[78]

Cullen's nosology was not predicated upon microscopic diagnosis, nor did he assign fevers solely to vague miasmatic causes but to more specific types of contagious effluvia. Regardless of the pathogenic causes of disease, Cullen and other disease theorists were quick to point out that ‘their production is favoured, and their power increased, by circumstances which favour putrefaction’. [79] Those circumstances were typically conditions affecting the body prior to contagion, such as an excess of cold or heat. In light of Cullen's nosology, microscopy seems to have had little importance in explaining the origins of disease, but that is not to say that the microscopy was absent in terms of diagnostic medicine.

If Samoilowitz had travelled to London it is very likely that he hoped to acquire instruments there for the Russian army and academies. Of course, this also might have prompted his sojourn to Holland, but by the mid-eighteenth century few workshops in Amsterdam, Delft, or Leiden could compete with London manufacturers. The possible exception is the Musschenbroek’s workshop in Leiden which had designed microscopes for the famed Jan Swammerdam. However, the Musschenbroek firm closed in 1748 and thereafter Dutch-build microscopes seem to have declined in popularity.[80] Morrison-Low has explained that English competition ultimately led to this decline of manufacturing in the Netherlands. She has found that the changing nature of the clientele, not the manufacturers, increased the rate and quality of instrument manufacturing. The demand for scientific instruments, especially telescopes, by the wealthy patrons and societies of London and Paris ‘were

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[78] Two letters from Samoilowitz to Cullen are preserved in the Royal College of Physicians Library at Edinburgh. These are cited in Ibid., pp.410-1.
[79] W. Cullen, *First Lines of the Practise of Physic*, 4th edn. (4 vols., 1784), i, p.86.
[80] See P. de Clercq, *At the Sign of the Oriental Lamp: the Musschenbroek Workshop in Leiden 1660-1750* (Rotterdam, 1997).

to win this leading position for the London élite makers'.[81] Moreover, Anita McConnell, Morrison-Low, and others have also noted the increase in foreign workers and visitors to London. The Ramsden workshop, for example, employed two Germans, two Portuguese, two French, and one Swiss. Similarly, foreign travellers also made certain to visit these leading shops. As Morrison-Low writes, 'Foreign visitors interested in astronomy and precision instruments always made a point of seeing Ramsden, considered by contemporaries to be at the pinnacle of the trade'.[82] Thomas Brugge (1740-1815) in his 1777 journal dwells little on his trip through the Netherlands, but much more on his interactions with the London instrument makers. He purchased £88 4s worth of instruments from makers including Ramsden and Peter Dollond.

Such a popular destination for instrument hunters was a double-edged sword. Although the leading workshops certainly benefited from foreign trade, the threat of corporate spies from abroad was well known. Historians Daumas, Morrison-Low and others have documented this. Foreign visitors hoped to unlock the secrets of London manufacturing in order to increase their own nation's prestige in the manufacturing of scientific instruments. Upon Bugge's return to his native Denmark he 'was determined to establish an indigenous instrument trade'.[83] As Denmark's Astronomer Royal, Brugge arranged £100 per annum funding for at least one apprentice, Jesper Bidstrup (1763-1802), to study instrument manufacturing in London beginning in 1787. Bugge also wrote to Sir Joseph Banks in the hopes of settling the apprentice into one of the leading workshops. Unfortunately, for Bugge and Bidstrup, the offer failed to attract the notice of any leading masters in London. Bidstrup could not find the experience he was looking for in the workshop of Ramsden because the master

[81] A. Morrison-Low, p.139
[82] Ibid., p.152.
[83] Ibid., p.154.

closely guarded his techniques and wisely dispersed labourers into separate tasks so that none could envision the final design. Bidstrup complained to Bugge, in 1789:

> Ramsden is the only one to have the most important instruments manufactured in his house, he has often got 40 to 50 workers, each of them manufacturing various parts of an instrument, some are plane filing, others turning on the lather, making screws and so forth . . . I would have had to do what was ordered of me, by which I would have been deprived of increasing my knowledge of other parts of the art, not because Ramsden could not give the best instruction, but because no mortal would make him do that.[84]

After years spent apprenticing for less-reputable masters, Bidstrup quickly ran through his stipend. He was not able to purchase dividing engines and glass-grinding machines and send them back to Copenhagen. His knowledge of the trade died with him only three years after his return from London in 1798.

b. Holland

Although much more research needs to be done in the case of Dutch manufacturing, the evidence reveals that Holland remained a primary centre for the development of scientific instruments well into the eighteenth century. Samoilowitz's Dellebarre microscope might have come from Leiden, where there are now numerous other Dellebarres preserved in the Museum Boerhaave. On the other hand, the museum also contains a large collection of Cuff and Adams microscopes which indicates that Dutch craftsmen were keen on copying English designs. Historians Maria Rooseboom, Maurice Daumas and Huib J. Zuidervaart have catalogued Dutch instruments and their makers during this period. They have found that Dutch instrument makers were dispersed across the small country with workshops in nearly every major Dutch city, but primarily in Leiden, Amsterdam, and Utrecht. This contrasts with French and British manufacturing which still centred in Paris and

[84] J. Budstrup to T. Bugge, 6 March 1789, quoted in A. Morrison-Low, p.155.

London. Nevertheless, Dutch manufacturers continued to excel in navigational and astronomical instruments throughout the century.

Perhaps the most famous Dutch workshops were those of Danilo Gabriel Fahrenheit (1686-1736, The Hague) and Jan Van Musschebroek (1686-1736, Leiden). In addition to his thermometers, Fahrenheit may have been the inventor of the first solar microscope although this was disputed by Johann Lieberkühn. Just as successful, Van Musschenbroek's workshop 'remained for over forty years a famous port of call for cultured travellers, and contributed largely to maintaining the reputation of the town of Leiden in scientific circles throughout Europe'.[85] Indeed, Desaguliers visited Musschenbroek in the early 1730s, bringing with him a reflecting telescope.[86] Thereafter, Musschebroek popularised the reflecting telescope in Holland and profited considerably (his prices for the telescopes range between 70 and 225 guilders).[87] Later opticians were equally skilled in telescope and microscope manufacturing although they were not as well known as Fahrenheit and Musschenbroek. Many were quick to incorporate Dollond's achromatic lenses. Zuidervaart informs us that the first Dutch manufacturers to discover Dollond's secret in 1761 were Amsterdamers Carl Ulrich Bley and Jan van Deijl who seem to have stumbled upon the achromatic system by experiment rather than by spying.[88] Also, Jacobus van de Wall in Amsterdam, attempted to procure quality crown and flint glass in order to construct an achromatic telescope as Bugge noted following a visit to van de Wall's workshop in 1777.[89] So enthusiastic were these Dutch opticians that they may have pre-empted the British in the construction of the achromatic microscope. Daumas writes that Jan van der Bildt (1709-1791) and his sons Jan and Harmanus constructed achromatic microscope

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[85] M. Daumas, *Les instruments scientifiques aux xvii et xvii siècles,* pp326-7.
[86] H.J. Zuidervaart, 'Reflecting "popular Culture": The Introduction, Diffusion, and Construction of the Reflecting Telescope in the Netherlands', *Annals of Science,* 61, no. 4 (2004), p.414.
[87] H.J. Zuidervaart, p.415.
[88] H.J. Zuidervaart, pp.434-435.
[89] H.J. Zuidervaart, p.435.

doublets as early as 1762, but ‘they struggled till the end of their respective careers to perfect that invention’.[90]

From such accounts it is clear that Dutch microscope manufacturing was still competitive by the close of the eighteenth century. In this light it is not surprising that Samoilowitz and other travelling physicians would acquire their instruments in Holland, even though they might have looked for medical training elsewhere.

7.4. Conclusions

Late eighteenth-century microscope users such as Samoilowitz had access to a thriving network of microscope manufacturing and trade. Moreover, new inventions, such as the Dellebarre instrument were becoming popular because of their achromatic potential. Whether or not such early microscopes were truly achromatic has been some concern for past historians such as Daumas and Bracegirdle who tend to place credit for the development of achromatic lenses with J. J. Lister in the 1820s.[91] In contrast, I choose to avoid presentism by arguing that achromatic lenses arrived when the first users claimed that they had, thus, at least with the Dellebarre microscope of the late 1770s if not before. Of course, such early instruments did not eliminate achromaticity to the degree that later microscopes by Lister, Amici, and other nineteenth-century makers would claim, but this is not relevant to the eighteenth century. Rather, I have followed Deiman's thesis that the problem of chromatic abberation is a retrospective oversimplification. What is relevant is the fact that new technology such as the Dellebarre microscope and its supposed triumph over chromatic aberration, was a product of the eighteenth-century science. Such new 'discoveries' in microscope technology prompted fresh attempts to investigate disease,

[90] M. Daumas, *Les instruments scientifiques aux xvii et xvii siècles*, 328.
[91] B. Bracegirdle, 'J.J. Lister and the Establishment of Histology', *Medical History,* 21 (1977), pp.187-94.

at least in Samoilowitz's case. Although, I could see very little in my own re-enactments using a Dellebarre microscope, the eighteenth-century physician apparently saw, or chose to see, a great many worms and signs of plague.

Like many eighteenth-century medical and anatomical texts, Samoilowitz's does not dwell much on describing his Dellebarre microscope. Rather, when reading such treatises, one finds the occasional mention of the microscope as though it were just one of many medical tools available. Again, we find passive references to microscopy which suggests its established presence in the late eighteenth-century medical profession, not its absence.

This is not to suggest that microscope users were on a quest to find disease-causing particles, fomites, or germs, under their lenses. As shown in chapter 3, the argument for animalcules of contagion was debateable; and, by the late eighteenth-century, physicians such as Samoilowitz seem to have favoured the search for microscope signs, rather than causes, of disease.

Chapter 8: The Decline of Microscopy in France? 1770-1820

This chapter focuses on non-believers. The most vocal critics of microscopy appeared at the dawn of the nineteenth century. This is perplexing when one considers eighteenth-century improvements to microscope design, the past contributions of the microscope to anatomy, or the pan-European market for microscopes at the turn of the century. The physicians and anatomists who faulted microscopy were Xavier Bichat (1771-1802), and, to a lesser extent, Pierre Cabanis (1757-1808), and René Laennec (1781-1826). These luminaries were also key agents of medical reform: they were among the founders of the new French teaching hospitals. Their medical curriculum emphasized lesions, tissue classification and physiology, yet eschewed microscope discoveries. Other founders of this curriculum, such as Vicq d'Azyr (1748-94) and Jean Noël Hallé (1754-1822), were either indifferent or ambivalent when it came to microscopy. Their medical and anatomical treatises ignored or consciously omitted mention of the instrument. Thus, we must now address this antipathy towards microscopy in France during the late eighteenth and early nineteenth centuries.

As shown in previous chapters, the technological development of the microscope was not a steady progression nor did it correlate to linear microscopic discovery. Conversely, it would be ahistorical to posit that microscope technology was simply too primitive to provide any use to anatomy and medicine before the development of modern achromatic objectives. In the previous chapter we saw that late eighteenth-century physicians, such as Danilo Samoilowitz, did in fact utilise the microscope as a diagnostic tool. Nevertheless, one cannot easily gauge how prevalent the use of the microscope may have been among contemporaneous clinicians and field surgeons. Most late eighteenth-century medical authors who mentioned the use of the microscope did so passively or only briefly. We cannot surmise from their texts that there was a 'revolution' in medical microscopy or a 'paradigm shift' among medical elites towards thinking in terms of microscopic anatomical features. Rather, late eighteenth-century texts suggest a quiet and variable incorporation of microscope observations among physicians and anatomists. Moreover, the scope of discussion concerning the microscope was quite limited in medical texts compared to works of natural history. Whereas late eighteenth- and early nineteenth-century naturalists systematically classified microscopic infusioria and other animalcules, no contemporaneous physicians seem to have done so for microscopic tissues or pathogens. Therefore, what may be surprising is not that Bichat and his contemporaries openly rebuked the practice of anatomical microscopy, but that they saw the need to do so. Why was this so? As Bichat stated in his 1801 *Anatomie générale*,

> I will not expose all the anatomical assumptions which have been made by an abuse of the microscope, and which, besides, if they had basis in reality, would serve no inductive use to science.[1]

Bichat's critique is a sharp contrast to the late eighteenth-century rhetoric of the British anatomists we have encountered, such as William Hunter in London or the Monro family in Edinburgh. If we limit our context to France itself, we find that Bichat's criticisms also undermined the merits of past microscopice experiments such as those of Buffon and Réaumur as well as more general and positive discussions of microscopy: for example, various entries of Diderot and d'Alembert's in the *Encyclopédie* which had formerly praised the microscope for its usefulness in anatomy and natural history. Regardless of his break with the past, Bichat's criticism certainly carried weight. He has been called, anachronistically, the 'father of histology' for his emphasis on identifying discrete tissues. His contribution to reforming clinical education in Paris is equally well known. According to Jean Baptiste Bouillard (1796-

[1] X. Bichat, *Anatomie générale*, ii, p.581.

1881), Bichat, along with Pinel, 'founded a school of medicine and physiology, which had all Europe as its disciple'.[2]

The story of the microscope's diminished role in the new French schools of medicine becomes more convoluted if we consider Maurice Daumas' assertion that from the 1770s on, French workshops experienced a 'revival of instrument making' (and particularly optical instrument-manufacturing) following Turgot's suppression of the *jurandes*, or guild monopolies, in 1776.[3] In addition, the encouragement of patrons such as the Duc de Chaulnes (1714-69) and Abbé Rochon (1741-1817) bolstered advances in micrometers and general optical knowledge. As we shall see, this revival continued long after the revolution, placing France once more on par with Britain and the Netherlands as a competitor in the instrument trade.

Thus, in the case of France, we have conflicting accounts concerning the role of the microscope in the late eighteenth and early nineteenth centuries. On the one hand, there was a renewed market for scientific instruments, on the other, an epistemic shift away from microscope-based medical knowledge. This chapter will shed light on this situation by addressing three questions concerning medical microscopy in France. Firstly, were there conditions during the *ancien régime* and revolutionary period which deterred the manufacture and use of the microscope? Secondly, why did the transformation in medical epistemology obviate microscopy? Finally, if we concede that French medical microscopy declined, when and why do we see its return?

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[2] J. B. Bouillaud, *Essai sur la philosophie médicale sur les généralités de la clinique médicale* (Paris, 1836), p.26.
[3] M. Daumas, *Les instruments scientifiques aux xvii et xvii siècles*, pp.353-60.

8.1. The revival of instrument making in France, 1700-1800

i. Microscope improvements during the *ancien régime*

Maurice Daumas found that the production of scientific instruments in France remained at its lowest level between 1750 and 1770.[4] Characteristic of the period were French copies of English compound microscopes, specifically the 'universal' microscopes discussed in chapters 3 and 4. As we've seen, Cuff-style compound microscopes became immensely popular and widely copied in France. Alexis Magny and Claude Siméon Passement's compound microscopes were, during this period, nearly indistinguishable from those of John Cuff. Opticians seem to have been more interested in adapting existing models to improve usability and accuracy rather than finding alternate designs to improve lens resolution or clarity. Magny's patron, the Duc de Chaulnes, did modify the microscope design in the late 1760s, wrapping what is essentially a Cuff compound in a set of micrometers and focus mechanisms.[5] [Fig. 1].

The Chaulnes modifications were a major departure from the simple wire lattice micrometers of predecessors such as Cuff and contemporaries such as Benjamin Martin. Cuff and Baker had employed the micrometer to measure specimens. Martin, however, argued that micrometers were more useful for measuring the magnifying power of different lenses.[6] Chaulnes incorporated these lattice-type micrometers as optional attachments, but his modifications to the microscope departed from both Baker's and Martin's use of them. His micrometer inventions should more aptly be called callipers, as they measure the specimen slide position in relation to the objective lens. These Chaulnes micrometers measured the precise relationship

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[4] Ibid., pp.353-4.

[5] M, duc de Chaulnes, *Description d'un microscope, et de différents micrométres: destinés à mesurer des parties circulaires ou droites, avec la plus grande précision* (Paris, 1768).

[6] B. Martin, *Microscopium Pantometricum; or, a New Construction of a Micrometer* (London, 1775), p.4.

between the objective lens and the slide in a three-dimensional Cartesian coordinate system. One could set the slide and record its position (right, left, up, or down) on an X,Y axis (the stage) or the distance from objective to specimen, the *Z* axis (the focal length). This was a major advancement over Cuff's fine-focus screw adjustment, which had provided an ease of use and focusing, but did not allow the user to record the slide position or the distance from the objective to the slide.

Using Cuff's focusing mechanisms today, one finds that they vary widely from model to model. Although Cuff's focusing mechanism greatly improved upon the screw-barrel design, his microscopes occasionally slip out of focus while one attends to the specimen slide. This is not to say that the Chaulnes design was by any means easier to use, manipulate, or manufacture. One is struck by how unwieldy and overbuilt the instrument appears. These were not simple attachments, but dozens of complex minute components [Fig. 2]. Furthermore, Marc Ratcliff adds that, regardless of the method used to manufacture micrometers, there was no standard measure of calibration.[7] Nevertheless, the extreme detail with which Chaulnes renders these micrometers through his illustrations is far superior to contemporaneous English microscope manuals, such as those of Baker, Adams, and Martin. In providing exactitude of detail in his text and illustrations, Chaulnes may have been attempting to foster the replication of his instruments, thereby compensating for the lack of standardisation. Essentially, it was a process which endeavoured to remove estimation and human error. The point of his micrometers was to provide a means to record and replicate incremental measurements of the microscope's focal settings and slide position as well as measure the specimen itself. As evidence of this is the fact that he incorporated his microscope and micrometers into the design of his dividing engines

[7]M. Ratcliff, *The Quest for the Invisible*, p.169.

between 1765 and 1768. These subsequently influenced Jesse Ramsden's designs for the dividing engines used by the British Ordnance Office.[8]

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FIGURE 1. Michel-Ferdinand d'Albert d'Ailly, Duc de Chaulnes, *Description d'un microscope, et de différents micrométres : destinés à mesurer des parties circulaires ou droites, avec la plus grande précision* (Paris, 1768), plate 1.

[8] A. McConnell, *Jesse Ramsden (1735-1800),* p.40.

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FIGURE 2. Michel-Ferdinand d'Albert d'Ailly, Duc de Chaulnes, *Description d'un microscope, et de différents micrométres : destinés à mesurer des parties circulaires ou droites, avec la plus grande précision* (Paris, 1768), plate 4.

Aside from Chaulnes' innovations, there were few major additions to microscope designs arising from the mid to late eighteenth-century French workshops. Historians have had a mixed view of the success of this instrument market. Daumas and A.J. Turner have drawn attention to the security of courtly patronage, as well as the inability of instrument makers to become commercially independent without patronage from this elite and exclusive group. Turner finds that this ultimately allowed British manufacturers to out-compete these smaller French workshops.[9] However, Daumas points to a renewal of interest in optical instruments and manufacturing. Indeed, one could argue that this was a relatively stable and lucrative period for opticians provided they could balance the complicated demands of courtly patrons and commercial enterprise. Fostering this was the dissolution of the *jurandes* which opened the instrument market to some competition. Nevertheless, Morrison-Low has argued that it was not until the French Revolution that instrument makers freed themselves 'from the inextricable straightjacket in which the Paris trade had found itself'.[10] Clearly, historians have had mixed views regarding the more general interaction of science and the state during the *ancien régime.* Charles Gillispie saw a system of 'partnership rather than one of partisanship'[11] whereas more recent scholars, such as Morrison-Low and Keith M. Baker, have called this into question.[12]

One factor which must be considered is the dominance of the Académie Royale des Sciences over instrument makers and scientific publications. As patents and rewards fell under the purview of the king, the Académie Royale became the channel which directed their recipients. The Académie was thus integral to fostering and protecting the production of instruments as well as texts. Chaulnes, for example,

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[9]A.J. Turner, *From Pleasure and Profit to Science and Security: Etienne Lenoir and the Transformation of Precision Instrument-Making in France, 1760-1830* (Oxford, 1989), p.3.
[10] A. Morrison-Low, *Making Scientific Instruments*, p.146.
[11] C.C. Gillispie, *Science and Polity in France at the End of the Old Regime* (Princeton, 1980), p.550.
[12] K.M. Baker, 'Scientism at the End of the Old Regime: Reflections on a Theme of Professor Charles Gillispie', *Minerva* 25 (1987), pp.21-34.

published the majority of his texts through Académie sponsorship. He did not have to resort to out of pocket costs in order to include elaborate and detailed illustrations of his micrometer. Despite the dissolution of the guild corporations, the Académie helped exclude competition from non-sponsored instrument makers. Its members also exerted considerable control over eighteenth-century scientific publications. As Roger Hahn wrote in his *Anatomy of a Scientific Institution,* the Académie Royale evolved to suit this need; it was not 'consciously pursuing a secret plan for the domination of the press and other scientific activities in the nation'; rather, it ‘reacted on an ad hoc basis, asserting and extending its powers as the situation permitted'.[13]

Of course, this departmentalisation of knowledge into offices and academies incurred unforeseen disadvantages. It dispersed innovation and publication across a range of different academic institutions. These include the Académie Française (1635), the Académie Royale des Sciences (1666), the Académie Royale de Chirurgie (1731), and the Société Royale de Medécine (1776). Nonetheless, as Hahn found, the Académie Royale des Sciences maintained its dominance over journal publications despite these other publishing societies. At times, it also threatened to block the printing privileges of alternative publishers, such as the Jesuit-led *Journal de trévoux* which had attacked René Reaumur's materialist theories of human generation.[14] Undoubtedly though, the sheer number of official and quasi-official publications in France meant that even scientific and medical advances which were congenial to the aims of the Académie might be either widely dispersed or quietly tucked away. This contrasts somewhat with the dominance of the *Philosophical Transactions* and the Royal Society over scientific pursuits in Britain. It would be difficult to assess the most prestigious French venues in which to publish scientific results or advertise new

[13] R. Hahn, *Anatomy of a Scientific Institution: the Paris Academy of Sciences, 1666-1803* (Berkeley, 1971), p.65.
[14] Ibid., pp.64-5.

technology. Eighteenth-century French innovators who were in danger of falling into priority disputes might rush their results to press in the *Journal des sçavans* which regularly published brief records of the Académie's closed meetings as well as longer articles. Furthermore, the *Mémoires de l'Académie Royale des Sciences* (later the *Comptes rendus*) was the official publication of Académie proceedings. Instrument makers, such as Magny and Passement received publicity in the Académie's *Machines et inventions approuvées par l'Académie Royale des Sciences.*[15] Alternatively, their optical innovations might be published in subject journals such as the astronomy journal, *Connaissance des temps*. It is particularly difficult to trace anatomical and surgical discoveries as these might appear in a number of sources such as the *Histoire et mémoires de la Société Royale de Médecine* or the *Journal de médecine, chirurgie et pharmacie*. For historians, however, some exciting and often overlooked sources of information are the *encyclopédies* and *dictionnaires* of the late eighteenth and early nineteenth centuries. As we shall see later in this chapter, these are especially relevant sources in the study of competing scientific trends during the revolution and restoration eras.

ii. The revolution and republic

Turning our attention to the events of the French Revolution, we find that the manufacture and sale of optical instruments did not diminish with the overthrow of the court and king. Despite the social turmoil of the revolution, the young republic and its new bureaucratic agencies fluidly supplanted the patronage hierarchies of the *ancien régime*. While the revolution had disastrous consequences for some patrons and natural philosophers (most obviously Lavoisier), it was a boon for certain

[15] See, for example, the Académies praise of Passement's telescopes and microscopes in Académie Royale des Sciences, 'Télescope de réflexion appliqué au quart-de-cercle (1746)', reprinted in *Machines et inventions approuvées par l'Académie Royale des Sciences,* 7 (1777), pp.341-6.

instrument manufacturers. Étienne Lenoir (1744-1832) ran a thriving workshop before and after 1789 which testifies to the revolutionaries' demand for instruments, particularly surveying equipment. Both the royal court and the later republic had favoured Lenoir's mathematical instruments. Lenoir had once earned the title Ingénieur du Roi; then, after the revolution, he retained some prestige through his work with the Commission du Mètre.[16] Such examples have prompted Turner to argue that the Republic fostered a welcoming environment for instrument makers to compete and profit from their trade on par with their rivals in the English guilds. Turner points to the increased demand for military, surveying, and educational instruments of the Republic which boosted the careers of instrument makers. Moreover, the Republic protected the competitive market within France which put an end to the closed system and relative stagnation of the *ancien régime* instrument trade.[17]

One might suspect that the revolutionaries encouraged instrument making for purely utilitarian or military reasons. What affect did this have on the microscope? The application of the instrument to military and medical pursuits was debateable; therefore, one might conclude that the manufacturing of microscopes declined. However, there is no indication that microscope manufacturing and trade floundered after 1789. This may be due to the fact that most opticians who manufactured microscopes and lenses similarly produced surveying, nautical, and mathematical instruments. Large commissions from military and state agencies may have provided a stable source of income which allowed for continued speculation in the trade and manufacturing of optical equipment. There are a number of opticians and instrument makers who seem to have been involved in such a livelihood. Like Lenoir, optician

[16] For example, the catalogue of revolution-era instruments in J.A. Bennett, *Le Citoyen Lenoir: Scientific Instrument Making in Revolutionary France: A Special Exhibition to Mark the Bicentenary of the French Revolution, Whipple Museum of the History of Science* (Cambridge, 1989).
[17] A. Turner, *From Pleasure and Profit.*

Noël-Simon Carochez (c.1740-1813) successfully navigated his career from the court to the Convention Nationale, turning from the rank of Ingénieur du Roi to a position at the Bureau des Longitudes. Carochez worked to refine achromatic objectives and the Convention promised Carochez 10,000 francs to sustain his workshop, although it is unknown whether or not he actually received this payment in full.[18] The Comité d'Instruction Publique then commissioned Carochez to construct a 20-foot reflecting telescope, possibly to compete with Herschel's large reflecting mirrors. Carochez completed this task by 1800.[19] There are other comparative examples of opticians receiving sponsorship specifically for the perfection of achromatic lenses. Noël-Jean Lerebours (1761-1840) constructed achromatic telescope objectives for the Conservatoire des Arts et Métiers, and received a national reward of 5,000 livres in 1793. One little-known optician, by the name of Fréminville, received a loan of 60,000 francs from the Comité de Salut Publique to continue his research into achromatics. Another, Robert-Aglaé Cauchoix (1776-1845), received government funding for his workshop at the Collège des Grassins after 1800 and is most noted for his achromatic telescope objectives made from flint glass and rock crystal.[20] Cauchoix also sold microscopes, sometimes to foreign buyers (a Cauchoix microscope is preserved today in the Museum for the History of Science at Oxford[21]). More famously, the workshop of the Chevalier family at the quai de l'Horloge remained in existence from 1765 until well into the Bourbon restoration, by which time the Chevaliers operated under the sign '*au microscope achromatique*'.[22] Moreover, many of these opticians had studied in Holland or England or had links to workshops there. Some, such as Étienne-Antoine Putois (1763-c.1798), were able to import new

[18] M. Daumas, *Les instruments scientifiques aux xvii et xvii siècles*, p.372.
[19] Ibid., pp.228-9.
[20] Ibid., pp.372-7.
[21] MHS inventory no. 92484.
[22] M. Daumas, *Les instruments scientifiques aux xvii et xvii siècles*, p.377.

technologies even as France moved closer to war with its neighbours: in 1792, Putois reported Adams' use of hollow achromatic lenses filled with refracting liquid to produce telescopes with much clearer focus.[23]

On closer inspection, it becomes clear that much of this revolutionary-era interest in optical discovery concerns its application to navigational, surveying, and astronomical equipment rather than microscopes. Achromatic and reflecting telescopes such as Carochez's were increasing in size, clarity, and complexity whereas the magnification and resolution of the microscope remained comparatively unchanged. Although instrument makers such as Adams, Dellebarre, and the Chevaliers claimed that they had solved the problem of producing achromatic microscopes, their microscope technology simply did not rival the better-known improvements in achromatic telescope lens design. Yet, these advances in telescope technology are not enough to conclude that the demand for microscopes waned. Moreover, astronomical instruments and large-scale orders for optical surveying equipment required more capital and material to manufacture. This may, in part, explain why late eighteenth-century opticians are more often remembered for their telescope models rather than their more modest microscopes. On the other hand, some idealistic authors called for a modernization of commerce which would be dependent on the precision of instruments. The Institut de France sponsored studies for the improvement of manufacturing, agriculture, livestock, commerce, and experimentation. It also edited and reprinted pre-revolutionary texts. In one such reprinted text from 1782, Louis Daubenton, Buffon's old collaborator at the Jardin du Roi, wrote that microscopy must be further integrated into the textile industry.

> We cannot do without the microscope ... I strongly suggest that all merchants, all owners of herds and shepherds, have microscopes to recognize the different kinds of wool. There are the merchants and large

[23] Ibid., pp.375-6.

> manufacturers who must use this instrument which will be very useful to them, and even absolutely necessary, whenever they will have to know exactly the degree of fineness of their yarn in important cases for their business or their factories.[24]

Nevertheless, microscopy must have been in a sad state of affairs if the most direct written support for it appeared in a manual on sheep herding.

Commerce aside, we also find that the Republican government actively sought to preserve microscope collections. This is not surprising considering the scientific background of republican leaders such as Jean Paul Marat and Pierre Cabanis. Marat most famously had attempted to improve the solar microscope before the revolution although with limited success. He had taken a degree at St. Andrews in 1775 and then held a private practice in London. Although too late to cross paths with Hewson and Hunter, he certainly shared their social milieu. Returning to Paris, Marat enlisted a royal optician, M. Sikes, to construct a solar microscope that could detect emanations of heat: an instrument called a helioscope. Marat's subsequent chemical treatise, *Recherches physiques sur le feu* (1780), also caught the attention of Benjamin Franklin. It initially received favourable approval from the Académie Royale des Sciences before Lavoisier deemed it to be uninspired.[25] On the other hand, Lamarck wrote flatteringly of Marat's chemical studies and his solar microscope in 1794, the same year Lavoisier was executed. Of course, doing so after Marat's assassination may have been politically advantageous.[26]

In the field of medicine, we find very little innovation related to the microscope during the late eighteenth century, but there was some discussion

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[24] L. Daubenton, *Instruction pour les bergers et pour les propriétaires de troupeaux* (Paris, 1802), pp.346-7.

[25] J. P. Marat, *Recherches physiques sur le feu* (Paris, 1780). Marat's second attempt to win approval with his *Découvertes sur la lumière* (Paris, 1780) also failed to attract the attention of the Academy.

[26] J. B. Lamarck, *Recherches sur les causes des principaux faits physiques, et particulièrement sur celles de la combustion, de l'élévation de l'eau dans l'état de vapeurs* ...(2 vols., Paris, 1794), i, pp.344-68. Pietro Corsi has noted Lamarck's own objections to Lavoisier's new chemistry in P. Corsi, *The Age of Lamarck: Evolutionary Theories in France, 1790-1830* (Berkeley, 1988), pp.8-10. Clifford Conner discusses Marat's chemistry and microscopy at length in his biography of Marat: C. Conner, *Jean Paul Marat: Scientist and Revolutionary* (Atlantic Highlands N.J., 1997), pp.33-113.

regarding its merits. Medical authors typically acknowledged predecessors in their fields. In other cases, editors reprinted past works in microscopy. Joseph Tourdes (1770-1851) edited and translated a 1773 text of Spallanzani on blood circulation to include much more description on the microscope and even an illustration of Spallanzani's microscope [Fig. 3]. Tourdes' 1799 translation, *Expériences sur la circulation observée dans l'universalité du système vasculaire* describes Spallanzani's instrument as a simple microscope with modifications designed by Lyonet. This instrument 'is strictly speaking, nothing more than the microscope of Lewenhoeck [sic], in that it has a single lens, but it is much more convenient and has a far more extensive application'.[27]

[27] L. Spallanzani, *Expériences sur la circulation observée dans l'universalité du système vasculaire*, translated by J. Tourdes (Paris, 1799), pp.116-7.

The image originally presented here cannot be made freely available via ORA because of copyright. The image was sourced at: Countway Library, Harvard University, HOLLIS Number : 005656600.

FIGURE 3. A Lyonet-style microscope. L. Spallanzani, *Expériences sur la circulation observée dans l'universalité du système vasculaire,* translated by J. Tourdes (Paris, 1799), pp. 116.

Tourdes was quite correct in noting the advances of mid-eighteenth-century anatomical microscopes. In the 1740s, Pierre Lyonet (or Lyonnet, 1708-98) had fitted his lens around his specimen. In the case of Lyonet's microscope, a lens was mounted on a ball-and-socket arm which rose above an anatomical stage. The stage bears a resemblance to Lieberkühn's stage which was designed to hold specimens more firmly under the microscope [illustrated in Chapter 6, figure 3]. It was Lyonet's colleague Abraham Trembley who had perfected the adjustable arm, or *porte-loupe*, for use in observing aquatic specimens, although Louis Joblot had produced a more basic model as early as 1718.[28] The *porte-loupe* was quite an innovation as pillar-based microscopes and pocket microscopes necessitated that the user cut the specimen to a specific size in order to fit it neatly under the lens. The design also allowed delicate

[28] M. Ratcliff and M. Fournier, 'Abraham Trembley's Impact on the Construction of Microscopes', in D. Generali and M. Ratcliff (eds.) *From Makers to Users,* pp.91-113.

specimens to remain perfectly still, mounted on the stage, while the observer manipulated the *porte-loupe*. Conceivably, this would allow users to trace the path of microscopic vessels along much larger areas of the organism. The experimenter would not need to disturb the sample after it was placed under the lens. Not surprisingly, the nineteenth-century version of Trembley's *porte-loupe* microscope would be taken up by Robert Brown in order to view pollen movement in water, thus leading to the famed discovery of 'brownian' motion. In the hands of Lyonet and Spallanzani, it was used to vivisect small animals.[29]

The reprint of Spallanzani's text and Tourdes' details surrounding the *porte-loupe* microscope are noteworthy considering Tourdes' position as an instructing physician in Montpellier. As we shall see, the leading vitalist professors in late eighteenth-century Montpellier were hardly amenable to the use of the microscope. Yet, the example of Tourdes suggests that we may find more signs of medical microscopy if we examine less prominent figures in the medical profession. On the other hand, although Tourdes may have been interested in these advances in anatomical microscopy, his own writing on topical medicine did not mention the use of the microscope. Tourdes was also interested in improving medical reform during the revolution.[30] He wrote a nosology for use in the new curriculum at the École de Médecine in Strasbourg in 1802 and it is possible that he prompted students and instructors to continue to incorporate the microscope in anatomy courses.[31]

Although eighteenth-century anatomical microscopes such as the Lyonet design may not have improved magnification or limited aberration, these models improved sample stability if not ease of use. Like the Chaulnes microscope, the

[29] P. Lyonet, *Traité anatomique de la chenille* (The Hague, 1760).
[30] J. Tourdes, *Lettre sur les médicaments administrés à l'extérieur de la peau dans les maladies internes* (Pavia, 1797).
[31] J. Tourdes, *Esquisse d'un système de nosologie, fondé sur la physiologie et la thérapeutique, pour servir de suite au plan général de l'enseignement de l'École de Médecine de Strasbourg* (Strasbourg, 1798).

Lyonet microscope did not make any changes to existing lens technology. Late eighteenth- and early nineteenth-century French microscope users seem to have forgotten the lens entirely. If we examine French anatomical texts which do mention the microscope between 1770-1820, we find that few authors noted the names of the optician who made their microscopes and fewer still noted the design of their lenses. This contrasts to the story we have seen earlier in France, when naturalists such as Buffon pointed to the superior magnification of their lenses in order to justify their observations. The narrative also differs from the disputes surrounding lens technology between anatomists outside of France such as Hewson, Della Torre, and Fontana. Why were late eighteenth-century French anatomists less forthcoming about microscope technology than their counterparts outside of the county? It could be that French authors saw no need to use medical texts as a medium for advertising microscope technology. While British patron-naturalists such as Henry Baker were invested in the success of their opticians, this was not the case in the more static relationship between opticians and academies in France. As evidenced in the case of the Dellebarre or Chaulnes microscopes, the French academies celebrated new models with very little dispute. One might also suspect that with Dellebarre's supposedly-achromatic design, microscope users saw no need for improved lens technology. Moreover, translations and republications of foreign texts, such as those of Spallanzani, Adams, and Martin, did not diminish as the century progressed.

The dearth of microscope-related texts emerging from late eighteenth-century French medical circles also lends credence to the theory that microscopy was simply not as popular amongst French anatomists and physicians. There is little evidence to dispute this hypothesis. However, a closer look at the narrative structure of texts which do mention the microscope reveals a casual attitude towards the use of the instrument. As we shall see, the offhand way in which some French anatomists mentioned their use of the microscope indicates that their methodology needed no explanation. With the exception of Tourdes' editorial, this passive narrative suggests that readers were expected to have already familiarized themselves with the use of the microscope as a medical tool.

Returning to the French Revolution, anatomists and physicians after 1789 may have had an ambiguous relationship to the microscope but their contemporaries in chemistry and natural history were well acquainted with the merits and debates surrounding microscopes and other instruments. This interest seems to have led to a state policy of preserving, rather than eradicating, the *ancien régime* fashion for instrument collecting. As Daumas has found, the Republic was careful to preserve instrument collections and cabinets of curiosities from being dispersed or privately sold. Revolutionary conquest also brought technological rewards. Shortly after the revolution in the Netherlands, the Hague Stadhoulder Collection of optical, navigational, and electrical instruments moved to Paris. Buffon's student, Barthélemy Faujas de Saint-Fond (1741-1819) and the noted botanist André Thouin (1746-1824) housed and catalogued these works, possibly to determine their utility to the state and academe.[32] Similarly, the revolutionaries did not plunder the royal instrument collection which was housed at Versailles and the Hôtel de Mortagne, but rather preserved the 245 instruments under the care of the polymath Alexandre-Théophile Vandermonde (1735-96). This includes the telescope and microscope collection of former Opticien du Roi, Claude Simeon Passement.[33]

Undoubtedly, the revolution and subsequent wars did have a negative impact on scientific collaboration. These events hindered travel to and from France but they did not completely curtail the flow of knowledge and scientific visitors. Travel narratives indicate a steady interest among republicans in reforming scientific

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[32] M. Daumas, *Les instruments scientifiques aux xvii et xvii siècles*, p.190.
[33] Ibid., pp.142-144.

education and institutes. However, these narratives provide little information concerning the advancement of scientific instruments. One account comes from Thomas Bugge, the Danish astronomer who had earlier hoped to bring London manufacturing secrets to Copenhagen. By 1798, he was in Paris, having been invited by the Directory as part of an international commission for the implementation of the metric system. His resulting account, published as *Science in France in the Revolutionary Era*, concerned his observations while touring such institutions as the Institut National, the Observatoire de Paris, and the first public exhibition of industrial products on the Champ de Mars (1798).[34] Bugge recorded little information concerning specific instruments as he was far more interested in institutional and educational reforms and their impact on science. As Robert Fox noted in a review of the text, Bugge's account sheds light on the key years of scientific progress between 1794 and 1802 as these years are relatively undocumented.[35] Bugge's account depicts a somewhat half-hearted attitude in Paris towards reforming the scientific curriculum. The Convention and Directory embraced scientific education with the establishment of various institutions; however, many, such as the École Normale, were 'raised upon a hasty and unstable foundation, and hence, in less than a year, they were dissolved'.[36] Bugge's own committee on the metric system was in such disorder that he returned to Denmark less than a year later.[37] On the other hand, Bugge's account of the Institut National was favourable. He placed the scientific debates at the Institute on par with those of the Royal Society.[38] Unfortunately, Bugge mentions only telescopes he saw there and at the observatory, not microscopes.

[34] T. Bugge, *Science in France in the Revolutionary Era,* edited by M.P. Crosland (Cambridge, MA, 1969).
[35] R. Fox, 'Review of *Science in France in the Revolutionary Era Described by Thomas Bugge*', *British Journal for the History of Science* 5, no. 3 (1971), p.306.
[36] T. Bugge, *Science in France in the Revolutionary Era*, p.25.
[37] R. Fox, p.307.
[38] Bugge, *Science in France*, pp.81-84.

Such accounts paint a picture of revolutionary-era science as highly idealistic but lacking in organization. Optical instruments were certainly of interest to the new republican government and its military; however, their use might have been more utilitarian than innovative. At first glance, it would seem that few advances in microscope science arose in France between 1789 and 1815 despite the fact that workshops continued to function. Did the revolution thus effect a shift away from the microscope as a trustworthy scientific instrument? This would be surprising given the republican endorsement of the natural sciences. The Convention established twelve chairs of biological science and transformed the Jardin du Roi into the Muséum d'Histoire Naturelle. Surely, such actions were a boon to natural history and its vogue for the study of infusoria and other microscopic life. Natural history courses at the museum included microscopy; Lamarck's own title was, 'Professeur d'Histoire Naturelle des Insectes, des Vers et des Animaux Microscopiques'[39] and his early study of microscopic animalcules likely led him to endorse spontaneous generation. Similarly, the new chemistry of Lavoisier had embraced laboratory instruments including microscopes. Lavoisier himself owned a 1778 Dellebarre microscope.[40] Despite Lavoisier's execution, the new chemistry was soon entrenched along with its wide range of laboratory instruments. Standing in contrast to these naturalists and chemists, the sceptics of microscopy before and during the revolution were typically physicians and anatomists.

8.2. Medical doubt concerning the microscope

i. The rise of clinical education

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[39] J.B. Duvergier, *Collection complete des lois, decrets, ordonnances, reglements et avis du Conseil-d'État* (Paris, 1793), p.417.
[40] Collection Lavoisier (20085-0000), CNAM, Paris.

If we are to examine the fate of the microscope at the dawn of the nineteenth century, we cannot ignore the revolution in medical training and practice that occurred shortly before and during the French Revolution. Much of the historiography surrounding this topic has responded to Michel Foucault and Erwin Ackerknecht's pioneering studies on the rise of Paris clinical education.[41] These studies spawned a number of debates concerning the scope and validity of this epistemological shift in medical thinking. Historians continue to question whether or not clinical education emerged gradually from its antecedents in the eighteenth century or arose in opposition to a prior medical *episteme*. They have produced a number of astounding biographies on key leaders of the Paris school: Bichat, Cabanis, Pinel, and Laennec to name but a few.[42] Other scholars have drawn attention to the far-reaching legacy of the clinicians and the social and political ramifications of the medical 'gaze'.[43] The current historiographical issues surrounding the Paris clinical schools are far too vast to adequately address in this thesis; however, we cannot ignore the role of French clinicians as critics of microscopy. Thus far, few historians have attempted to explain the absence of medical microscopy during this period with the exception of Ann La Berge.[44]

La Berge argues that Paris clinical education was not fundamentally opposed to microscopy as Paris physicians utilised the microscope in the early to mid-nineteenth century. She describes, in excellent detail, the gradual introduction of

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[41] M. Foucault, *Naissance de la clinique: une archéologie du regard médical* (Paris, 1963); E. Ackerknecht, *Medicine at the Paris Hospital, 1794-1848* (Baltimore, 1966).
[42] E. Haigh, *Xavier Bichat and the Medical Theory of the Eighteenth Century* (London, 1984); M. Staum, *Cabanis: Enlightenment and Medical Philosophy in the French Revolution* (Princeton, 1980); D. Weiner, *Comprendre et soigner: Philippe Pinel (1745–1826) et la médecine de l'esprit* (Paris, 1999); J. Duffin, *To See with a Better Eye: A Life of R. T. H. Laennec* (Princeton, 1998).
[43] For example, J. E. Lesch, *Science and Medicine in France: The Emergence of Experimental Physiology, 1790-1855* (Cambridge MA., 1984); J. H. Warner, *Against the Spirit of System: The French Impulse in Nineteenth-century American Medicine* (Princeton, 1998); L. Jordanova, 'Medical Mediations: Mind, Body, and the Guillotine', *History Workshop* 28 (1989), pp.39-52; S. M. Quinlan, *The Great Nation in Decline: Sex, Modernity and Health Crises in Revolutionary France c.1750-1850* (Aldershot, 2007).
[44] A. La Berge, 'Dichotomy or Integration? Medical Microscopy and the Paris Clinical Tradition', in Hannaway, C. and A. La Berge (eds.), *Constructing Paris Medicine*, pp.275-312. Also, La Berge, 'Medical Microscopy in Paris, 1830-1855', in A. La Berge and M. Feingold (eds.), *French Medical Culture in the Nineteenth Century* (Amsterdam, 1994), pp.296-319.

microscopy upon a wary medical community after 1830. Before this time, Parisian physicians either disdained or neglected its use. La Berge points to Bichat and Laennec as key opponents: to these reformers, microscopy was 'neither factual nor empirical, but illusory and imaginary, not the stuff of which science, as they understood it, was made'.[45] La Berge cites three explanations for this: first, that microscopy had not proven itself useful in eighteenth-century humoral and environmental disease theory; second, that leaders of Paris medicine claimed that it interfered with the natural senses; finally, that technological inadequacies had hindered microscopy until the 1830s.

Expanding on La Berge's history, one might question whether or not this disregard for microscopy was unique to France as the contemporary British hygienists and miasma-theorists whom we have already encountered also found little use for the instrument. La Berge is certainly correct in noting the lack of microscopy among the French disease theorists in Paris and Montpellier, such as Jean Astruc who warned against Kircher's theory of microscopic germs as early as 1736. Later instructors in Montpellier and Paris repeated Astruc's caveats and reprinted his texts.[46] On the other hand, as we have seen in the preceding chapter, physicians outside of France had employed the microscope to provide evidence for direct contagious particles: animalcules, worms, fomites, and the symptoms of disease.

One might also take La Berge's history further back in time, to examine the role of the microscope in French medicine before the rise of the Paris school. Bichat and Laennec's early nineteenth-century critiques are perplexing when one considers the uses of the microscope in mid-eighteenth-century anatomy and generation theory. French physicians, such as Astruc, had warned of charlatans who were overly keen on

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[45] Ibid., p.277.
[46] J. Astruc, *De morbus venereis libri sex* (Paris, 1736), pp.91-4.

using new, multi-lens, microscope models, but Astruc nevertheless acknowledged the microscope for its past uses in anatomy and embryology.[47]

Moreover, La Berge acknowledges Brian Ford's argument that achromatic microscopes became popular after 1820s because they possessed more aesthetic and social value rather than because of technological superiority. However, La Berge does not find this to be the case in 1830s France. I have taken a third view in this thesis: whether or not pre-achromatic microscopes were technologically inferior to later models is irrelevant as those physicians who did employ microscopes were often convinced of their effectiveness. More often than not, the critics of the microscope did not question its ability to see more details of the human body; rather, they questioned the merits of doing so. Microscopy was not nascent or merely neglected during the time of Bichat. It was challenged.

I contend that the decline of microscopy arose from epistemological shifts in late eighteenth-century French medicine. Pre-revolutionary schools in Paris and Montpellier are key to this story. Long before the French Revolution and its clinical reforms, medical students looked to the wards for their training in tandem with private instruction. Faculties and guilds educated and united both physicians and surgeons while state reforms, such as licensing requirements, expanded the power of these institutions towards the latter half of the century. However, as Laurence Brockliss informs us, new scientific research was not integral to this education; rather, instructors and faculties imparted the dominant medical orthodoxy. Of the French universities, only fifteen offered medical instruction on the eve of the revolution and most admitted only a few dozen new students a year. Paris and Montpellier had the highest rates of admission and the best training facilities but, again, these faculties

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[47] J. Astruc. *Traité des maladies des femmes* (5 vols., Paris, 1765), v, pp.55, 184.

emphasised theory and demonstration over innovative research.[48] Brockliss notes that professors at Montpellier were given chairs according to seniority rather than specialty. Thus, a chemist, Gabriel-François Venel (1723-1775), taught hygiene, while a botanist, Antoine Gouan (1733-1821), taught materia medica.[49] In such an environment, inventive research-based microscopy had little chance of attracting new disciples. On the other hand, John Lesch paints a brighter picture of scientific research during the *ancien régime* in regard to the advancement of physiology: 'the sciences were vigorously pursued in the Medical University, in the Collège de Chirurgie, and in the new Société Royale des Sciences, with physicians playing a prominent role'.[50] Eighteenth-century Paris and Montpellier featured a loosely-allied faculty with many external courses factoring into the student experience. There was no set of doctrines or official course curriculum which might aid us in locating the immediate rationale for the exclusion of the microscope. In the early and mid-eighteenth century, French medicine seems to have followed the model of experimental physiology and mechanism laid out by Hoffmann, Boerhaave, and Haller. Physicians were becoming increasingly interested in irritability and function although there was no discipline of 'physiology' in the modern scene. Even late in the century, the works of Spallanzani, Fontana, Trembley and other microscopists were widely reported in scientific literature. However, in the 1750s and 1760s, physicians and lecturers began distancing themselves from the Boerhaavian and Hallerian corpus at Montpellier and Paris. In general, physicians moved towards clinical treatment, vivisection, and physiology. Two primary interests seem to exemplify this late eighteenth-century medical

[48] L. Brockliss, 'Medical Education and Centres of Medical Excellence in Eighteenth-century Europe: Towards an Identification', in O. Grell and A. Cunningham (eds.), *Centres of Medical Excellence?: Medical Travel and Education in Europe, 1500-1789* (Aldershot, 2010), p.27.
[49] Ibid., p.30.
[50] J.E.Lesch, *Science and Medicine in France*, pp.24-6.

philosophy in France: the natural senses (sensitivity) and the doctrine of vitalism. These medical philosophies marginalised the use of the microscope.

ii. Sensitivity and vitalism in late eighteenth-century France

Eighteenth-century physicians were becoming increasingly aware of the philosophical discourse surrounding *sensitivité* and *sensibilité*. This was largely thanks to the writings of Condillac, particularly his *Traité des sensations* (1754).[51] Condillac broke with Locke's notion that sense perception was an intuitive process. Rather, he argued that sensation varies based on individual experience and the combination of those experiences. This discourse eventually moved into the realm of medicine as physicians became more curious about the affects of stimuli on individual organs and bodily systems. Vitalists called into question Haller's notion of nerve-based sensibility as distinct from irritability (described in Chapter 6). To the vitalists, all tissue appeared endowed with some degree of sensation and motility that could not be explained in the structure of their fibres. The sensibility of the mind or soul utilised the 'vital force' to react, consciously or unconsciously to stimuli. Textbooks would preach this doctrine to the next generation of physicians during the Paris Clinical reforms. Paul Joseph Barthez (1734-1806) wrote in *Nouveaux éléments de la science de l'homme* (1778):

> An infinite number of observations prove that the *sensibilité* of the Principle of Life excites an organ and often determines the sympathetic principle which contracts other organs to which stimulus is not applied.[52]

[51] One should note, however, that Francis Glisson (1597-1677) first investigated irritability and Haller demarcated it from sensibility. F. Glisson, *Tractatus de ventriculo et intestinis*, part 2 (London, 1677), ch.5, pp.158-64.
[52] P. Barthez, 'Notes', *Nouveaux éléments de la science de l'homme*, 2nd edn. (2 vols., Paris, 1806), i, p.186.

In other words, sensitivity appeared omnipresent in the living body and its constituent parts. Barthez held this to be true even if these organs were severed from the nervous system.

Again, the historiography relating to sensation and irritability is vast and these concepts have been fully addressed elsewhere.[53] It suffices to say that eighteenth-century physicians and anatomists from both the vitalist and materialist schools were becoming more interested in physiological sensation as well as mental cognisance. Their interests were both anatomical and philosophical: they aspired to map the sensitive forces of the body (irritability, contraction, and other vital forces) and second, to draw attention to the powers of the natural senses in relation to the mind. As Alain Corbin notes, 'all the scientists, all the philosophers found themselves obliged to confront sensualism. They yielded to its grip however great their reluctance'.[54] This trend would have far-reaching ramifications for the use of scientific instruments.

A pivotal figure in Corbin's work is Jean Noël Hallé who would later become president of the Académie des Sciences in 1813. Hallé was not embroiled in the debates concerning irritability, but he was keenly interested in the general concept of sensation. Hallé was one of the foremost experts on dangerous miasmas. A defender of Lavoisier, even after his arrest, Hallé was influenced by Lavoisier's new system of nomenclature and similarly attempted to identify and classify atmospheric poisons based on their affect upon physiology. In addition, Hallé was a firm believer in the natural abilities of the senses, especially smell, to determine disease conditions. His early works make no mention of microscopes but neither did he note the use of any instruments. To Hallé and other late eighteenth-century miasmists, if it was not

[53] *Science in Context* recently dedicated an issue to the subject of enlightenment sensibility and irritability. *Science in Context*, no. 4 (2008), pp.461-664. See also, H. Steinke, *Irritating Experiments.*
[54] A. Corbin, *Le miasme et la jonquille,* p.3.

possible to detect noxious miasma with the senses, one could deduce that unhygienic conditions were present from the resulting outbreaks of disease.[55] This contrasts with contemporaries such as Felice Fontana and other mechanists outside of France. Following his microscope studies, Fontana modified a eudiometer, an invention of Marsilio Landriani and Allesandro Volta, to detect and measure noxious air.[56] Similarly, Corbin informs us that Fr. Jules-César Gattoni (a student of Volta) also designed an eudiometer to measure bodily emanations from children that he found on the street. In France, Théophile de Bordeu (1722-1776) and later Bichat ridiculed such modes of science, not because of the miasmatic principles in question, but because they incorporated instruments rather than direct sensory perception.[57] Similarly later hygienists, such as François Emmanuel Fodéré (1764-1835) were silent on the worth of instruments such as the microscopic in the study of infectious disease and sanitation.[58] Their critiques indicate a shift in thinking away from the mechanist's dependence upon mathematical and philosophical instruments and instead towards a more natural *sensibilité.*

Hallé had also been a prominent figure in Paris hygiene debates shortly before the revolution. He wrote several reports on the unhygienic emanations coming from the Rivers Seine and Bièvre.[59] Hallé authored a 1787 entry on 'Hospital Air' for Vicq d'Azyr's *Enclyclopédie méthodique, médecine.*[60] Again, he did not discuss microscopy, thus lending evidence to La Berge's argument that the microscope had simply failed to provide any benefit to the study of environmental causes of disease.

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[55] J. Hallé, *Recherches sur la nature et les effets du méphitisme des fosses d'aisance* (Paris, 1785).
[56] Corbin, *Le miasme et la jonquille*, p.16.
[57] Ibid., p.50.
[58] F.E. Fodéré, *Traité de médecine legale et d'hygiène publique ou de police de santé adapter aux codes de l'opera Française et aux connaissances actuelles* (Paris, 1798); also, *Recherches historiques et critiques sur la nature, les causes et le traitement du choléra-morbus d'europe* (Paris, 1831).
[59] J. Hallé, 'Rapport sur l'état actuel du cours de la rivière Bièvre' and 'Procès-verbal de la visite faite le long des deux rives de la rivière Seine', *Histoire et memoires de la Société Royale de Médecine,* 10 (1789), pp.lxx-xc.
[60] J. Hallé, 'Air des hôpitaux de terre et de mer', *Enclyclopédie méthodique, médecine* (13 vols., Paris, 1787-1830), i, pp.569-71.

Nevertheless, we must not be hasty to conclude that physicians were ignorant of the microscope as a potential tool because Hallé also co-authored the entries for 'Microscope' and 'Microscopiques' in the *Dictionnaire des sciences médicales*.[61] In these eleven pages, Hallé and his colleague Auguste Thillaye outlined the construction of simple, compound, and solar microscopes, noting different lens designs. Perplexingly, they made no attempt to criticise the application of this instrument in medicine or anatomy. Why then did Hallé and his colleagues find so little use for the microscope in their own work? Omissions do not necessarily indicate a lack of practical knowledge. Barthez, for example, was obviously an experienced dissector, yet he seldom discussed the process of dissection in his *Nouveaux éléments*. Nevertheless, when he did so, it was typically for a goal: to note vitalist discoveries, such as the 'spasmodic and convulsive' nature of intestinal organs.[62] Microscopes were not necessary in such discoveries.

Aside from the lack of instruments, another trend which characterises late eighteenth-century French medical texts is the lack of illustration. Owing to the scarcity of microscopy, one would not expect elaborately illustrated *micrographiae* as found in Britain; however, illustrations of gross (macroscopic) anatomy were also absent from major anatomical and medical treatises. Bichat, for example, did not incorporate a single illustration in his treatises and textbooks. This may have been unique to the French clinicians; concurrently, British physicians such as William Hunter were preoccupied with pictorial representations and the concept of realism in anatomical illustration.[63] Similarly, French anatomists did not appear to have used wax models to the degree they were used elsewhere on the continent such as in the medical schools of Florence; however little research has been done on this subject as

[61] J. Hallé and A. Thillaye, 'Microscope' and 'Microscopiques', *Dictionnaire des sciences médicales* (60 vols., Paris, 1812-1822), xxxiii, pp.371-82.
[62] Barthez, *Nouveaux éléments*, i, p.154.
[63] W. Hunter, *Anatomica uteri humani gravid* (Birmingham, 1774).

it relates to France.[64] Rather than diminish medical education, the lack of illustrations and modelling exemplifies the notion of *autopsia*, or 'seeing for oneself' over virtual witnessing. The educational reforms of the late eighteenth century emphasised direct instruction, patient interaction, and observation. As a student under Pierre Desault (1738-95) at the Hôtel-Dieu, Bichat would have experienced long hours of patient attendance, gross dissection, and surgery. A typical student's schedule began at 6am with rounds around the wards, cadaver or patient demonstration from 9am until 3pm, then anatomy lectures and outpatient consultations until 8pm.[65] Such demanding schedules would have had little time for library study or formal training in the use of instruments for research purposes.

French medical instructors also used their lectures and textbooks to undermine earlier theories of animalcule contagion. In 1777, Antoine Louis (1723-92) translated Jean Astruc's warnings against the use of the microscope to study viruses. Louis also added a note on pedagogy, chiding charlatans for being too keen on finding the first minute signs of disease. For Antoine, and many members of the Montpellier and Paris faculties, medicine needed to emphasise the signs and symptoms of disease rather than the causes.

> Why not admit that the first impressions are unknown to us? That one cannot determine the first ravishes of a venom when it is delivered in the vessels of a body . . . Let us leave these vain speculations and attach ourselves to that observation and experience in which we find the effects of the virus, and the most effective ways to address them.[66]

Louis then added,

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[64] Jordanova discusses the eighteenth-century realism, illustration, and wax modelling in chapter 3, 'Body Image and Sex Roles,' in L. Jordanova, *Sexual Visions: Images of Gender in Science and Medicine between the Eighteenth and Twentieth Centuries* (Hertfordshire, 1989), ch.3 pp.43-65.
[65] Lesch, *Science and Medicine in France*, pp.52-3.
[66] Note by A. Louis, in J. Astruc, *Traité des maladies vénériennes*, 4th edn. (4 vols., Paris, 1777), ii, pp.37-8.

> This remark is made for young surgeons, to whom this book is intended to focus attention on more specific things instead of being educated on a superfluity of explanations.[67]

The microscope is also absent from Louis' own works on anatomy and physiology as well as those of his immediate circle.[68] As we will see, such doubts as those of Louis and his interest in lesions and physiological notions would greatly influence Bichat and other reformers during the revolution.

In addition to Louis, perhaps the most noticeable rejection of microscopy during the *ancien regime* arose from the faculty at Montpellier, the great stronghold of vitalism before the rise of the Paris clinic. The founders of Montpellier vitalism, particularly Bordeu and Barthez, opposed the Boerhaavian corpus because of its mechanistic and materialist view of the body. In their quest for vital forces, the Montpellier anatomists forged new ground in physiology, but took little interest in microscopy. Neither Bordeu nor Barthez were overt critics of microscopy but the instrument is almost invisible in their works and those of other leading Montpellier physicians.[69]

Incidentally, the early Montpellier faculty had close ties to the encyclopedists. Bordeu is a main character in Diderot's *Rêve de d'Alembert* and he contributed numerous entries to the *Encyclopédie.* This may be surprising considering the *Encyclopédie*'s lengthy entry on the microscope; however, it should be noted that the entry was written by d'Alembert as well as Louis de Jaucourt (1704 –1779) who was a mechanist and admirer of Boerhaave. While the divide between sensualists and mechanists did not impede the collaboration of Diderot, d'Alembert, and Jaucourt, it was a point of contention for the Montpellier physicians. Their vitalism, more so than

[67] Ibid., p.38.
[68] For example, A. Louis (ed.), *Recueil d'observations d'anatomie et de chirurgie, pour servir de base a la théorie des lésions* (Paris, 1766).
[69] See, for example, T. de Bordeu, *Recherches sur quelques points d'histoire de la médecine* (Paris, 1764); Barthez, *Nouveaux éléments de la science de l'homme*.

Condillac's sensualism, was fundamentally opposed to earlier mechanical views of the body and the quest to discern its most microscopic parts.

Prior to the rise of vitalism, prominent early eighteenth-century mechanists such as Boerhaave, Hoffmann, and Haller may not have been averse to microscopic research, but neither did they significantly advance medical and anatomical microscopy. The point of conflict between these mechanists and vitalists lay in the notion of irritability. Whereas Haller could use his microscope to trace nervous connections and discern between sensitivity and irritability in bodily fibres (see chapter 6), for Bordeu and most other Montpellier vitalists, irritability and sensitivity were interchangeable. Similarly, for Barthez, a single organ such as the heart must be endowed with both sensitive and motor forces like all parts of the body because it will continue to contract even if severed from its nerves. The vital principle occupied the more minute tissues and organs of the body whilst still constituting the whole of the soul. One metaphor that illustrates this vital force is the oft-mentioned swarm of bees, found in the writings of Bordeu, Maupertius, and in Diderot’s *Le Rêve de d'Alembert*. One can easily examine a single bee, but it is far more important to understand how they operate together to build a solid body. Disease may attack these individual parts or show signs in particular lesions but illness is a disruption of the equilibrium. Anatomy similarly cannot be reduced to an individual organ or microscopic part. With such a holistic viewpoint microscopy is not entirely useless; it is instead pedantic.

Other Montpellier vitalists went further than Bordeu towards holism. Barthez was unwilling to localize any vital forces or delineate physiological properties as independent from the body as a whole. In the 1806 edition of his text, *Nouveaux éléments,* Barthez included a polite critique of the late Bordeu, chiding his fellow vitalist as a 'Solidist' and ‘Helmontian’ who posited that individual organs possessed a vital force which was independent from that of the entire body.[70] For Barthez, the 'vital principle' traversed the body and the soul. One could deduce its presence from the irritability of organs but could not explore it directly or extrapolate its essence merely from the study of individual organs. Rather, following Newton, Barthez argued that the same natural effects must be assigned the same causes whether one finds them under the microscope or in an entire organ. The 'sensitive forces' present across minute parts of the body must share a universal cause: the vital force. The quest to better understand the purview of this force upon the functions of the human body became the physician’s overall goal. Again, such exigencies did not require the microscope.

Vitalism also offered a convenient platform to critique Cartesian materialism. Materialism had found vocal adherents in the mid-eighteenth century, particularly Julien Offray de La Mettrie (1709-1751) whose 1741 text, *L'homme machine,* denied even Descartes’ dualism to argue for a strict materialist view of physiology without the influence of the soul. La Mettrie had pointed to microscopic observations of embryos to posit that foetuses grow in stages, without preformation, and in no way different from those of animals. He compared generation to the construction of a machine. La Mettrie also argued for pan-spermism, likening the spontaneous generation of life in nature to the regeneration of a polyp’s body as Trembley had witnessed under the microscope.[71] Additionally, he doubted the senses, claiming that microscopes prove that vision does not actually see objects as they really are. Not surprisingly, the vitalists attacked La Mettrie’s materialism although they were only tacitly opposed to Cartesian dualism. Antoine Portal (1742-1832), founder of the

[70] Barthez, *Noveaux élements de la science de l'homme*, i, p.24.
[71] A. Vartanian, 'Trembley’s Polyp, La Mettrie, and Eighteenth-Century French Materialism', *Journal of the History of Ideas*, 2 (1950), pp.259-86. See also, M. Gregory, *Evolutionism in Eighteenth-century French Thought* (New York, 2008),pp.25-7, and pp.52-7.

Académie Royale de Médecine, found little use for La Mettrie's materialism in the study of anatomy and physiology. In 1770, he wrote of La Mettrie,

> The author was ignorant because he witnessed few cases of medicine, he despised the most respectable Doctors, and has published an indecent censure of their best writing & their most laudable actions.[72]

Portal however was not a strict vitalist, and his attack on La Mettrie may have been meant to appease the Montpellier physicians as well as vindicate his own doctrines. As professor of anatomy at the Jardin du Roi, Portal was not far from Buffon and his interest in generation, embryology, and the microcosm. Buffon himself had been refashioned as an enemy of mechanism and materialism by the Montpellier physicians owing to his criticism of dualism, but, as we have seen in chapter 4, Buffon had held a mechanical view of human reproduction which was not dissimilar to that of La Mettrie. Portal nevertheless held Buffon, Haller, and other mechanists in high regard while refuting the texts of strict materialists such as La Mettrie. In contrast to the Montpellier vitalists, Portal described his own microscope observations in his 1803 *Cours d'anatomie médicale*. Writing only two years after Bichat criticism of the microscope, Portal described wax injections and microscope observation in his anatomical textbook.[73] He also noted the ability of the microscope to observe the finest capillaries.[74] Nonetheless, to most other anatomists and vitalists of this period, anatomical microscopy became synonymous with untenable materialist and mechanist viewpoints.

When vitalists at Montpellier, such as Bordeu and Barthez, did mention the use of microscope, it was typically in a historical context. Leeuwenhoek, Malpighi, Boerhaave and other greats in the history of anatomical microscopy are scattered

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[72] A. Portal, *Histoire de l'anatomie et de la chirurgie: contenant l'origine and les progrès de ces sciences* (5 vols., Paris, 1770), v, p.304.
[73] Antoine Portal, *Cours d'anatomie médicale: ou élémens de l'anatomie de l'homme* (5 vols., Paris, 1803), iii, p.121.
[74] Ibid., p.187.

throughout many vitalists texts. Professor Charles-Louis Dumas (1765-1813) cited these earlier experimenters, noting only the role of the microscope in discovering blood vessels and valves. Dumas vaguely acknowledged 'some observers' who had since confirmed William Harvey's theory of circulation.[75] Leading Paris physicians and surgeons likewise mentioned past contributions of the microscope to anatomy. François Chopart (1743-95), the leading expert on urology the École pratique, credited Boerhaave's microscopic observation of animalcules in human urine.[76] Similarly, Antoine Portal's 1770 *Histoire de l'anatomie* also recounts Boerhaave and other mechanists' use of the microscope without disdain.

One of the few places in which Barthez discussed anatomical microscopy was a note on Haller's previous observations of microscopic muscular contractibility.[77] Vitalists such as Barthez would not dispute contractibility, but would assign it different causes. Elsewhere, Barthez referred to 'excellent microscopes', such as those of Fontana, and their uses in discovering microorganisms and aquatic life.[78] This suggests that Barthez held no animosity against microscope-users; he did not critique microscope observations unless they challenged the notion of the vital force.

Montpellier graduates certainly wielded great influence during the revolutionary-era reforms, but this did not necessarily imply a rejection of microscope techniques. In addition to vitalism and physiology, several Montpellier instructors embraced natural history and Lavoisier's new chemistry, neither of which sought to vitiate the use of instruments. For example, Jean-Antoine Chaptal (1756-1832), who later went on to reform medical education as minister of the interior under Napoleon,

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[75] C-L. Dumas, *Principes de physiologie ou introduction á la sciencie expérimentale, philosophique et médicale de l'homme vivant* (4 vols., Paris, 1800), iii, p.392.
[76] F. Chopart, *Traité des maladies des voies urinaires* (Paris, 1791), p.36.
[77] P. Barthez, 'Notes', *Nouveaux éléments de la science de l'homme*, i, p.116.
[78] Ibid., ii, p.104.

was not averse to using the microscope in his chemical research, applying the microscope to study crystals in his 1790 text, *Éléments de Chimie.*[79]

In contrast to Montpellier physicians, the demonstrators at the Jardin du Roi were more amenable to microscopy, possibly because of the legacy of Buffon and their distance from Montpellier. This is not to say that Jardin du Roi lecturers, like Portal, Lacépède, and Daubenton were critical of vitalism; instead, their texts were typically broader in scope than those of the Montpellier physicians. They often blended botany, naturalism, and anatomy. Bernard Germain de Lacépède (1756-1825), for example, pointed to the 'very good microscope of Leeuwenhoek' which proved the porous nature of the skin. These, like common blocks of wood were able to absorb topical substances such as mercury.[80] Porosity was evidently a fundamental property of life whether it be animal or vegetable, and was therefore of importance to the anatomists.

> Why present so many facts, when simply put, there is not a physicist who has not perceived or who could not perceive the multitude of small voids with a magnifying glass or a microscope...?[81]

This was hardly a revelation, but it is surprising that Lacépède would then tone down the role of the microscope in his natural history of reptiles and fish: a multivolume work which was intended to continue Buffon's *Histoire naturelle*.[82] Unlike Buffon's earlier volumes, Lacépèdes volumes of the *Histoire naturelle* seldom referenced microscopic organisms or anatomical features. This was also a departure from earlier studies of reptilian anatomy such as those of Needham, Monro, Hewson, and Fontana.[83]

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[79] J-A. Chaptal, *Éléments de Chimie* (3 vols., Paris, 1790), i, p.54.
[80] B.G. Lacépède, *Physique Générale Et Particuliere* (2 vols., Paris, 1782), i, p.186.
[81] Ibid., p.189.
[82] B.G. Lacépède, *Histoire des quadrupèdes, ovipares et des serpents,* 2 vols. (1788–1789) and *Histoire naturelle des reptiles* (1789).
[83] W. Hewson, 'On the Figure and Composition of the Red Particles of the Blood, Commonly Called the Red Globules,'pp.303-23; J.T. Needham, *An Account of Some New Microscopical Discoveries*; F. Fontana, *Traité sur le venin de la vipère*.

Not all natural historians and anatomists were so reticent concerning the microscope. Vicq d'Azyr (1746-1794), professor of anatomy at the Jardin du Roi, recorded using a solar microscope and a Dellebarre microscope in his influential 1792 *Système anatomique. Quadrupèdes*.[84] This text would lay the groundwork for future generations of comparative anatomists, including Cuvier. Vicq d'Azyr was one of the strongest voices for medical and scientific reform before and during the revolution. He issued scathing critiques of *ancien régime* medicine and natural history yet he clearly had no qualms with microscopy. We must nevertheless note that his reformist curriculum emphasized microscopy for comparative natural history and not clinical research. On the other hand, Vicq d'Azyr's disease etiology was based on 'viral molecules' and 'foreign and malfeasant molecules' which the doctor could attack with molecules in saline and other disinfectants[85]; however, Vicq d'Azyr made no attempt to study these with the microscope.

He, like Barthez, also edited the *Encyclopédie méthodique* (Vicq d'Azyr directed the medical series of the *Encyclopédie*) which was a prominent mouthpiece for vitalistic notions and did not frequently include references to instruments. Although he briefly noted microscopic observations of fibres, tissues, and other anatomical features, Vicq d'Azyr's comparative anatomy rested on more general morphological and physiological concerns. As the permanent secretary for the Société de Médecine (and Daubenton's grandson by marriage) Vicq d'Azyr was instrumental in establishing comparative anatomy, morphology, and physiology as the foundations of natural history: in other words, a methodology that did not rely on microscopes. He emphasised gross dissection, vivisection, comparative and pathological anatomy rather than a Buffonian approach to the study of generation or microscopic aspects of

[84] F. Vicq d'Azyr, 'Discours sur l'anatomie simple et comparée', in *Oeuvres de Vicq-d'Azyr*, ed. J-L. Moreau (6 vols., Paris, 1805), iv, p.70 and p.249.
[85] Vicq d'Azyr, 'Considérations relatives à un parallèle entre les symptômes des épizooties et ceux de la peste humaine', *Oeuvres de Vicq-d'Azyr*, v, pp.149-58.

anatomy.[86] This project had already begun in the 1780s when Vicq d'Azyr began calling for a deductive approach to the study of organs. The primary interests of comparative anatomists such as Vicq d'Azyr were the measurement of organs and their composite parts followed by an analysis of their combined functions in the search for homologies. He began by measuring organs as a whole rather than a sum of their microscopic parts. One could take an organ, such as the liver, and measure its surface anatomy, its size and angles, as one might measure a piece of crystal. One could then divide that organ into its constituent parts, its lobes and then its lobules. From this one could draw homologies among species and genera. One could further describe functionality, discovering physiological similarities in the livers, bile ducts, and hepatic veins of various organisms. In doing so, the observer would discern basic vital properties associated with that organ system. Vicq d'Azyr divided these vital characteristics into nine functions including irritability and sensibility. However anatomy still took priority for the student: he wrote, 'anatomy precedes and physiology comes after'.[87] This approach allowed for simpler, geometric, measurements before a student became mired in complexity. Of course, Vicq d'Azyr was not the first to practice comparative anatomy. Notable contemporaries, such as Monro *secundus* and Fontana had already been using various mammals and reptiles in their dispute of the structure of nerves as seen through the microscope. Even later comparative anatomists, such as Cuvier and Blumenbach, would employ the microscope to study anatomical features across species. However, for Vicq d'Azyr the microscope was not essential. Microscopy, with its observations of static anatomical features may have retained a place in his curriculum, but it no longer served to answer the major questions at stake.

[86] F. Vicq d'Azyr, *Traité d'anatomie et de physiologie avec des planches coloriées représentant au naturel les divers organes de l'homme et des animaux* (Paris, 1786). See also, C. Hannaway, 'Vicq d'Azyr, Anatomy, and a Vision of Medicine', in A. Berge and M. Feingold (eds.), *French Medical Culture in the Nineteenth Century*, pp.280-95.
[87] F. Vicq d'Azyr, 'Discours sur l'anatomie', in *Oeuvres de Vicq-d'Azyr*, iv, p.42.

However, comparative anatomists were also expanding on traditional methods of classification. While Vicq d'Azyr's *Système anatomique* moved from the general to the specific, his classification system began with microscopic worms and animalcules. Thus, in reality, the 'old' and 'new' courses of instruction were not so discrete. As Corsi explains, Vicq d'Azyr's history of life on earth still bore some resemblance to Buffonism, beginning with primitive crystals, then lower life forms, followed by more complex organisms.[88] Microscope observations of animalcules and infusoria retained their place. Therefore, it is difficult to argue that naturalist-anatomists reformed their disciplines at the expense of microscopy. Moreover, reform may have been a more gradual and compromising process than Ackerknecht and Foucault have stated. For example, while Lavoisier and his followers had implemented radical taxonomical changes before the revolution, Vicq d'Azyr and Daubenton resisted this trend towards new classifications, including those of Linnaeus. This is not to say that he also abandoned nosology; rather, his primary concern was in methodological reform, supplanting the traditional criteria of natural history with comparative anatomy and physiology.

One reason that the curriculum reforms appear so stark is that Vicq d'Azyr and his colleagues, such as Antoine Fourcroy (1755-1809), intentionally undermined *ancien régime* science to achieve their own goals. As author of the *Élogies* for the Société de Médecine, Vicq d'Azyr had further powers to lionise or vilify his predecessors and contemporaries. As Brockliss writes,

> Such self-interested witnesses are unlikely to provide reliable testimony ... People and institutions were either good or bad, virtuous or wicked, useful or useless ... Revolutionaries like Fourcroy, if not reformers like Vicq d'Azyr, belonged to a Manichean political culture which always thought in polar opposites. Their statements about *ancien régime* medical education cannot be taken as objective fact.[89]

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[88] P. Corsi, *The Age of Lamarck*, p.29.
[89] L. Brockliss, 'Before the Clinic', in C. Hannaway, and A. La Berge (eds.), *Constructing Paris Medicine*, p.73.

Can this help us explain Bichat's well-known critique of medical microscopy? Bichat's followers and eulogists depicted him as a revolutionary, but, as a vitalist, he participated in a movement that had been gathering adherents since Bordeu and others in the mid-eighteenth century: an anti-mechanist view of the body that had already abandoned the need for the microscope. His more overt criticism of microscopy, however, set Bichat apart from these predecessors.

iii. Bichat and reform in revolutionary Paris

Bichat was not remiss of past microscope observations. Like many of his reformist colleagues, he paid due respect to the discoveries of Haller, Malpighi, Spallanzani, Leeuwenhoek, and others. The renown of such luminaries, whether they had been vitalists, mechanists, or materialists, was unavoidable. For example, Bichat made no attempt to refute observations of microscope capillaries his 1801 *Anatomie générale*.

> These irregular oscillations of the movement of blood in the capillary system are sensible to the eye which has been armed with the microscope. They have been shown a hundred times by Haller and Spallanzani and others whose experiments are too well known for me to report here.[90]

Bichat further implied that he had recreated some of these historical experiments himself or had seen them performed. It is clearly from his narrative style that he had first-hand experience with the instrument. He freely admitted to using magnifying '*loupes*' in a number of observations: for example, to study of the affects of haemorrhages on tissue surfaces, although these magnifiers were likely simple field lenses.[91]

Despite crediting past microscope researchers such as those mentioned above, Bichat included diatribes against Lieberkühn, Hunter, and Hewson's microscopic

[90] X. Bichat, *Anatomie générale,* ii, p.513.
[91] Ibid., p.563.

studies of villi and lymphatic vessels. These appeared in both his 1799 *Traité des membranes* and the *Anatomie générale.* Bichat found that these past microscope observations were invalid, not because they propagated false discoveries, but because they lacked methodological rigour. This included the famed work of Leeuwenhoek: 'Science can draw nothing of it and we cannot rely upon its accuracy'.[92] The microscope deceived the eyes 'because when one looks into obscurity each viewer sees in his own manner'.[93] To unshackle himself from these problems, Bichat chose instead 'to study nature in the place where it begins to come within the range of our senses'.[94] Bichat was therefore dubious about any instrument that distorted or manipulated the natural senses.

A closer examination of Bichat's anatomical treatises reveals that he was, in fact, seldom critical of the microscopic work of his immediate colleagues and correspondents. Wisely, he directed his rebukes towards past authors rather than contemporary reformers in Paris. Despite Bichat's protestations, he nonetheless reconciled previous microscope observations with his own vitalistic notions. To Bichat, nature displayed either a vital and organic property or a physical inorganic property. He wrote in the *Traité des membranes*:

> Cast your eyes on all around you, or direct them towards the most distant objects; with the aide of the telescope survey those bodies floating in space, or, armed with the microscope, penetrate into the world of things so small that they threatened to conceal themselves forever from sight, you shall everywhere find on one hand the physical, on the other the vital principle.[95]

Bichat's criticisms of microscopy seem puzzling, particularly if we consider his most celebrated contribution to anatomy and medicine: his emphasis upon localised lesions and distinct tissue types. In retrospect, Bichat's success was to flip the medical gaze

[92] X. Bichat, *Traité des membranes en général et de diverses membranes en particulier* (Paris, 1799), p.30.
[93] Ibid., p.32.
[94] Ibid.
[95] Ibid., i, pp.xxxvi-xxxvii

away from a holistic view of the organism in equilibrium or disequilibrium and towards these lesions and specific tissues. Each tissue type (muscle, skeletal, nervous, etc.) exhibited unique degrees of sensibility. Foucault argued that Bichat's specificity actually ran counter to previous vitalistic notions because it isolated pathology in specific tissues. Past doctrines of vitalism and mechanism allowed room for ontological uncertainty regarding the nature and seat of disease. Bichat rectified this by identifying the locus of life and death in specific tissues. As Foucault wrote:

> The very idea of vitalism lost its signification and the essence of its content. In giving life, and pathological life, a status so fundamental, Bichat freed medicine from the vitalist discussion and other related problems.[96]

John V. Pickstone, following Foucault, similarly argues that this new pathology equipped physicians and surgeons with an entirely new way of seeing patient bodies; moreover, it prompted specialism in lesions, tissues and organ systems. It was a breakthrough, not only in analysis, but in the professionalisation and institutionalisation of medicine.[97] Bichat may have had a more localised notion of disease from the other Paris and Montpellier anatomists we have discussed, yet the microscope remained unnecessary to his studies. His treatise on membranes is a case in point. Predecessors such as Bordeu had examined tissues and membranes, but not to the same extent as Bichat. On the other hand, these forerunners did not shy from applying the microscope to tissue samples once in a while. Even Bordeu mentioned, in passing, his own use of the microscope to observe the 'cellular tissue' in 1767;

> It appears, when examined under the microscope, composed of atoms or small bodies, glued to each other, arranged without any strong symmetry.[98]

[96] M. Foucault, *Naissance de la Clinique,* p.157.
[97] J. V. Pickstone, *Ways of Knowing: A New History of Science, Technology, and Medicine* (Manchester, 2000), pp.110-11.
[98] T. de Bordeu, *Recherches sur le tissu muqueux ou l'organe cellulaire et sur quelques maladies de la poitrine* (1767), p.3.

For Bordeu, this observation of 'cells' in the mucus membrane may have been a curiosity, but it did not contribute to his understanding of vital forces within the membrane. For Bichat, such a close scrutiny would be meaningless because it did not foster an understanding of that tissue's role in pathology in addition to its vital forces.

Elizabeth Haigh, in her excellent biography of Bichat, has also addressed Bichat’s disdain for the microscope. Haigh concludes that Bichat’s critiques stemmed from his sensualism, that is, his trust of the natural senses only. Yet, as a vitalist, he nevertheless resorted to speculation rather than direct observation. Bichat was certain that vital activities such as absorption, secretion, and nutrition must pervade the whole body including those organs and glands which were too small to see. As Bichat was incapable of resorting to the microscope to observe the vital forces in the smallest organs and their components, he had to deduce that they were nevertheless present. As Haigh states, ‘he therefore transgressed his own sensationalist principles and rules of procedure’.[99]

8.3. Medicine and microscopy after Bichat

Bichat cast a long shadow over Paris clinical education in part because of his dedicated coterie of students and colleagues. They propagated a heroic image of Bichat following his untimely death in 1802. Bichat's doctrines, including his censure of microscopy, remained relatively unchallenged. On the other hand, we find that many of these same vitalistic doctrines pre-dated Bichat's texts. Similarly, given the eighteenth-century trend in vitalism, it is unlikely that microscopy would have ever become integral to the French medical curriculum regardless of Bichat's influence. One could argue that vitalists became politicised during the revolution increasing their

[99] E. Haigh, p.99.

calls to supplant old modes of investigative anatomy with new ones. As historian Elizabeth Ann Williams writes:

> Montpellier physicians began working to detach themselves and their institution from what came to be characterised, amid the angry reaction to Jacobinism and the Terror, as the errors and excesses of the Enlightenment.[100]

Cabanis was one such ideologue who attempted to lead reform both politically and medically before Bichat. His *Rapports du physique et du moral de l'homme* (composed as lectures between 1796 and 1802) noted that microscopy as well as wax and mercury injections had reached their useful limits and would soon be replaced.

> We have, I believe, exhausted the use of the microscope and the art of injections. If one is to further study human and general anatomy, and the nervous system in particular, one must imagine other methods and other instruments.[101]

On the other hand, Cabanis' critiques might be more rhetorical than factual. In the same work, he noted the use of the microscope to accomplish various tasks, such as to study the composition of mucus or nervous tissue. He was predisposed to examine similarities and differences in tissue types, sometimes employing the microscope to do so.

> It is not only with the scalpel, the eye, and the microscope that the (nervous) substance appears similar throughout. Considered chemically, there is also no point of difference...[102]

He also applied the microscope to study the effects of dehydration on the body's mucus. Dehydration, according to Cabanis, operates mechanically, desiccating the body's material substances. Here we see an example of Cabanis inducing physiological effects from microscope observation:

> [W]hen the mucilage gradually loses moisture by evaporation we find here and there elongate ridges which intersect, and it does not take long to realize that these streaks multiply and increase in

[100] E.A. Williams, *A Cultural History of Medical Vitalism in Enlightenment Montpellier* (Aldershot, 2003), p.327.
[101] P. Cabanis, *Rapports du physique et du moral de l'homme* (2 vols., Paris, 1802), i, p.187.
[102] Ibid., p.207.

> number, transforming the mixture into a body of sufficiently regular-divided by cavities, or *rayons*. Transparent walls can easily be perceived under a microscope.[103]

Similarly, Cabanis' colleagues and collaborators did not immediately heed his call to move beyond microscopy. Jean-Baptiste Fray (1764-1835), a military surgeon and administrator, had experimented with the microscope in his study of spontaneous generation. He sent an early draft of his research to Cabanis in 1804 who gave it a favourable review and encouraged Fray to publish his experiments without leaping to any conclusions. Cabanis' caution regarding Fray's work was due to Fray's endorsement of spontaneous generation via organic globules and monads. Fray believed that sunlight generated these animalcules and he used the microscope to confirm this in vegetable broths. Although Cabanis himself conceded that life must have developed spontaneously at some time on earth, he did not condone Buffon's theory of organic molecules (see chapter 4 of this thesis) or Lamarck's theory of continual spontaneous generation.[104] However, Cabanis did not seem to censure Fray's use of the microscope. Thus, when Fray eventually did publish his results in 1817, they were replete with details concerning microscopy.[105] Fray notes that he conducted his experiments using a Dellebarre microscope, which must have been exceptionally difficult considering the fact that he had lost the use of his right hand during his military service.[106] For our purposes, it is noteworthy that Cabanis and Fray were not dissuaded from the use of the microscope in the spontaneous generation debates, only as it applied to medicine and anatomy.

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[103] Ibid., p.232-33.
[104] P. Corsi, 'Idola Tribus: Lamarck, Politics and Religion in the Early Nineteenth Century', in A. Fasolo (ed.), *The Theory of Evolution and Its Impact* (Milan, 2012), p.18.
[105] J-B. Fray, *Essai sur l'origine des corps organisés et inorganisés* (Paris, 1817).
[106] Ibid., p.8.

i. Publishing microscope research during the age of medical reform

A broader survey of periodicals might help us better assess the state of microscopy outside of Bichat and Cabanis' immediate circle. With the exception of the *Dictionnaire des sciences médicales,* few medical periodicals and texts had endorsed microscopy before Bichat, or would after him. The *Encyclopédie méthodique, chirurgie* (1790-99) contained no entry for the microscope and paltry references to past microscope observations. Microscopy was also absent in Desault's influential *Journal de chirurgie* (1791-2), although contributors infrequently discussed using field lenses and magnifiers (loupes), to examine lesions and tumour metastasis.[107] Jean-François Lavoisien's 1771 *Dictionnaire portatif de médecine* included only a brief entry on the microscope which remained unchanged with its reprinting in 1793. Jean-Baptiste Rivet similarly made only scant reference to the microscope in his 1803 *Dictionnaire raisonné de pharmacie-chimique*. Among other uses for the microscope, he found that it aided in the preparation of salves and other drugs, such as determining the proper consistency for a mercury-based salve used to treat venereal disease.[108] Louis Robert's 1805 *Manuel de santé ou nouveaux élémens de médecine pratique* mentions only its utility in differentiating intestinal worms and other parasites.[109] The 1806 *Nouveau dictionnaire de médecine, de chirurgie, de physique, de chimie et d'histoire naturelle* of Joseph Capuron (1767-1850) included merely brief references to microscopic parasites. Bichat's former collaborator, Pierre-Hubert Nysten (1771-1818) co-edited the 1810 edition and made few changes, although elsewhere he also had noted using a Dellebarre microscope to diagnose diseases in non-vertebrates.[110]

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[107] Thiébault, 'Observation sur un hydro-encéphalocele', *Journal de chirurgie*, 3 (1792), p.328.
[108] J.B. Rivet, 'Onguent mercureil double', *Dictionnaire raisonné de pharmacie-chimique* (2 vols., Lyon and Paris, 1803-05), ii, p.101.
[109] L. Robert, 'Des vers intestinaux', *Manuel de santé ou nouveaux élémens de médecine pratique* (2 vols., Paris, 1805), ii, pp.706-711.
[110] P-H. Nysten, *Recherches sur les maladies des vers à soie et les moyens de les prévenir* (Paris, 1808), p.13.

Entries on topics such as 'microscope' or 'microscopic' typically mentioned past authorities such as Leewenhoek and gave a brief survey of various compound, simple, and solar models. They rarely noted the use of the microscope in anatomy and medicine. Still, few entries repeated the vitalists' admonitions. In this way, if we look solely at the entries where we expect to find microscopy, we will be sorely disappointed. It is revealing that the short 1821 entry 'Microscope' in the *Encyclopédie méthodique, medicine* was written by Daubenton who had been deceased for over twenty years.[111] Although it was not uncommon for editors to purchase or copy long-outdated articles, this entry pales in comparison to Hallé and Thillaye's 1819 entry in the *Dictionnaire de science médicales.* Jacques-Louis Moreau de la Sarthe (1771-1826) edited the *Encyclopédie méthodique, médicine* after Vicq d'Azyr's death is 1794. It is therefore curious why Moreau would incorporate such a brief entry on the microscope when other, competing periodicals had expanded on the topic.

Moreau also occasionally edited older or foreign texts to suit his own vitalistic philosophy. Historian Caroline Warman has found that Moreau edited, and in some cases rewrote, sections of Johann Caspar Lavater's, *Physiognomische Fragmente* (1775- 1778) for his 1806 French translation.[112] Moreau's translation contained far lengthier discussions concerning anatomy and dissection than Lavater's previous German editions. As Warman contends, this is because Moreau was also keen to supplant the mechanism of Boerhaave. 'Thus we find Moreau actively altering, annotating and extending Lavater's original work to bring it into line with vitalist medicine'.[113] Indeed, examining the French edition, we find many similarities to Vicq d'Azyr's comparative anatomy. The text calls for close scrutiny of individual parts

[111] L. Daubenton, 'Microscope', *Encyclopédie méthodique, médecine, par une société de médecins* (1821), x, pp.101-02.
[112] J.C. Lavater, *L'art de connaitre les hommes par la physionomie,* translated by L-J. Moreau de la Sarthe (10 vols., Paris, 1820).
[113] C. Warman, 'What's Behind a Face: Lavater and the Anatomists', in M. Percival and G. Tytler (eds.), *Physiognomy in Profile: Lavater's Impact on European Culture* (Danvers, 2005), pp.99.

through dissection in which one examines bones and cartilage, muscles, intestines, glands, veins and arteries, nerves and ligaments'.[114] Incidentally, in the 1789 English translation, we find Lavater discussing the use of the solar microscope to aid in the illustration of physiognomy; a topic which is absent from the French translation.[115] Not surprisingly, Moreau's volumes of the *Encyclopédie méthodique, médicine* rarely includes any mention of the medical or anatomical uses of the microscope.

Medical *encyclopédies* and *dictionnaires* typically recorded the historical development of medical practice and often listed past practitioners who had composed the etiology of any given disease. Thus, despite the absence of microscopy in most of these texts, some kept eighteenth-century microscope investigations current even if they did not put forward original microscope research. As in the case of Portal's *Histoire de l'anatomie*, dictionary readers learned of the successes and failures of anatomical microscopy and which parts of the body had yet to be explored with the instrument. As late as 1822, the *Dictionnaire des sciences médicales* recorded the contributions of famed microscopists such as Malphigi and Leeuwenhoek without noting the supposed inferiority of their instruments.[116] Lesser-known eighteenth-century microscope users were also preserved in this dictionary, such as Wijer Willem Muys' 1730s work on muscle fibres and tumours.[117]

Dictionaries also recorded discoveries in natural history which included the anatomy of microscopic organisms. Cuvier's entry, 'Animal' referenced his contemporary discoveries, both macroscopic and microscopic.[118] Julien Joseph Virey

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[114] J.C. Lavater, *L'art de connaitre les hommes par la physionomie*, i, p.224.
[115] J.C. Lavater, *Essays on Physiognomy*, translated by T. Holcroft (3 vols., London, 1789), ii, p.113. See also, J.K. Stemmler, 'The Physionomical Portraits of Johann Caspar Lavater', *Art Bulletin*, 75 (1993), pp.151-68.
[116] The biographical component of this dictionary did cast doubt on Leeuwenhoek's observations, although not the utility of his microscopes: 'Leeuwenhoeck', *Dictionnaire des sciences médicales, Biographie médicale* (7 vols., Paris, 1822), v, pp.561-3.
[117] J.M. Delpech, 'Cancer', *Dictionnaire des sciences médicales*, iii, pp.586-7. For more on Muys, see Ruestow, *The Microscope in the Dutch Republic*, pp.100-04.
[118] G. Cuvier, 'Animal', *Dictionnaire des sciences médicales*, ii, pp.142-56.

(1775-1846) followed this entry with another on their medical and pathological aspects, noting different microscopic parasites and other disease-causing animalcules.[119] Virey earned his M.D. in 1814 after several years spent in military service and his subsequent entries for the *Dictionnaire des sciences médicales* championed both vitalism as well as the importance of the microscope. He penned the entry 'Micrographie' in the 1819 edition. Virey had high praise for seventeenth-century microscope users such as Malpighi and Hooke, as well as eighteenth-century experimenters, such as Felix Fontana, Spallanzani, and Buffon. In the words of Virey, these predecessors had allowed science to 'recognize the nature of the most delicate parts of our tissues, and in particular, natural history was cleared of the great error of spontaneous generation'.[120] Virey's interest in the microscope likely stemmed from his close reading of Buffon. With the help of Panckoucke's publishing firm, Virey kept many of Buffon's works in print throughout this period. Although Virey disagreed with Buffon's monogenic views of racial origins, Buffon's use of the microscope seems to have piqued his interest. Virey himself did not conduct any major anatomical studies with the microscope; however, he was certainly adept at using the instrument in the study of animalcules and fermentation. He made frequent mention of its importance in his 1823 text *De la puissance vitale,* noting the role of the microscope in the study of nervous tissues and natural history. Virey argued that the microscope may have led predecessors, such as Needham, to 'incomprehensible' materialist ideas but it also allows users 'to demonstrate the falsity of those notions'.[121] Again, in Virey's work we see that medicine and natural history were not entirely separate disciplines. As he was also a naturalist and chief pharmacist at the Val-de-Grâce military hospital, Virey was favourable towards chemistry, microscopic

[119] J-J. Virey, 'Animal (propriétés médicales et qualités nuisibles', *Dictionnaire des sciences médicales,* ii. pp.156-61.
[120] J.J. Virey, 'Micrographie', *Dictionnaire des sciences médicales*, xxxiii, p.270.
[121] J.J. Virey, *De la puissance vitale* (Paris, 1823), p.37.

anatomy, and the microscope’s contribution to natural history. He believed that physicians should expand their training into naturalism, chemistry, and anthropological science. Like his intellectual precursor Buffon, Virey valued microscopic research from his own experiences in natural history, observing animalcules in fluid mediums.[122] As is typical though, Virey did not record the varieties of microscopes used in these studies. Without knowing the powers of these microscopes, one would consequently have a difficult time assessing what these experimenters actually witnessed beneath their lenses.

Later dictionary authors made efforts to highlight their improved microscope designs in order to legitimize their observations. In 1826, Bory de Saint-Vinent (1778-1846), editor of the *Dictionnaire classique d’histoire naturelle*, used the newly-invented achromatic microscope developed by Alexandre François Selligue (1784-1845) and he later used the improved design of Giovanni Amici (1786-1863).[123] These instruments produced the first truly achromatic images using the addition of a third concave lens made of more refractive flint glass. Saint-Vincent relished the capabilities of the new microscope designs, commenting that experimenters could now correct the microscopic errors of the eighteenth century using new microscopes capable of reaching 1000x magnification or more. Saint-Vincent and his fellow authors were eager to draw attention to microscopic research, so much so that they proposed to reclassify the ‘vague and misleading’ terms ‘animalcules’ and ‘infusoria’ under the simple taxonomical name ‘microscopiques’.[124]

[122] J-J. Virey, 'Animal', *Nouveau dictionnaire d'histoire naturelle* (Paris, 1803), i, p.460. For more insight on Virey’s natural history see, P. Corsi’s article, 'Julien-Joseph Virey, Le Premier Critique de Lamarck', in S. Atran, et al. (eds.), *Histoire du concept d'espèce dans les sciences de la vie* (Paris, 1987), pp.181-92.
[123] Bory de Saint-Vincent, *Dictionnaire classique d'histoire naturelle,* 10 (Paris, 1826), p.536.
[124] Ibid., p. 533. See also, Bory de Saint-Vincent, 'Essai d'une classification des animaux microscopiques', in Bory de Saint-Vincent (ed.), *Encyclopédie méthodique, histoire naturelle* (Paris,1826).

When compared, these dictionaries produce drastically dissimilar views of natural history. For example, one cannot reconcile Bory de Saint-Vincent or Fray's faith in spontaneous generation with Virey's natural history. However, when examining their use of instruments, we see a more unified belief in the utility and effectiveness of the microscope to address these problems; this contrasts with the dominant views of Bichat and the Paris clinicians.

This is not to say that dictionary authors were unified in their belief that microscopy was a useful method of medical diagnosis. Such entries appear only infrequently across these periodicals. Jacques Mathieu Delpech (1777-1832) mentioned its use in diagnosing 'rare cases' of skin infections.[125] Dictionary authors sometimes commented upon the use of microscopy in studying epidemic disease. Louis Guersant (1777-1848), a surgeon at the *hôpital des Enfants,* mentioned microscopy in his 1815 article 'émanations' for the *Dictionnaire des sciences médicales*. Guersant noted that Jean-Louis Alibert (1768-1837), a former student of Bichat and Pinel, had used the microscope to examine decaying matter found in swamp conditions, an experiment that seems surprising given Bichat's criticism of the microscope and eudiometer. Clearly, not all of Bichat's students shared his disregard for medical instruments. Guersant called Alibert's attempts, 'undoubtedly ingenious,' but chemical analysis would shed greater light on the perplexing miasmas, 'it is here that chemistry wields power over medicine when its agents are inadequate'.[126] Although such authors had reservations about the use of the microscope, we find that it had become such an 'agent' of medicine and anatomy in a number of investigations. Also in the *Dictionnaire des sciences médicales,* Antoine Jacques Louis Jourdan (1788-1848) noted the use of the microscope to study the thymus gland. Like Vicq d'Azyr, Jourdan investigated this gland starting with its most visible parts and leading

[125] J.M. Delpech, 'Cas Rares', *Dictionnaire des sciences médicales,* iv, p. 254.
[126] L. Guersent, 'Émanation', *Dictionnaire des sciences médicales*, xxi, p. 465.

to its smallest components; however, unlike the major reformers of the clinical school, he went so far as to describe microscopic detail.[127]

Why did the *Dictionnaire des sciences médicales* more frequently refer to microscopy than other periodicals? The simplest answer is that it was the largest medical periodical present in post-revolutionary France. The sheer diversity of topics and authors enclosed within its 60 volumes encompassed a number of competing medical trends. Another reason may be that this dictionary was not under the control of Montpellier or Parisian vitalists and physiologists, although the work was the official publication of the Société de Médecins et de Chirurgiéns. The society entrusted Panckoucke with its publication, who, in turn, gave editorial control to François-Pierre Chaumeton (1775-1819). Chaumeton was a pharmacist, and his real passion lay in herbals and natural history, fields which did not call the utility of the microscope into question. Chaumeton also had a strained relationship with many members of the Paris medical establishment and other physicians such as Pinel also wielded considerable control over the *Dictionnaire des sciences medicales.* Nevertheless Pinel himself seems to have favoured microscope investigations. He often noted using microscopes and field lenses in his *Nosographie philosophique* to investigate inflammation caused by inoculations.[128] Pinel was also involved in editing the *Journal Complémentaire du dictionnaire des sciences medicales* which frequently included references to anatomical microscopy. This journal, like its accompanying dictionary, did not publish articles which cast doubt on the efficacy of the microscope. One such article by August Albrecht Meckel (1789-1829, brother of Johann Meckel

[127] A. Jourdan, 'Thymus', *Dictionnaire des sciences médicales,* lv, pp.129-36.
[128] P. Pinel, *Nosographie philosophique, ou la méthode de l'analyse appliquée à la médecine* (3 vols., Paris, 1807), ii, p.40.

the Younger) on intestinal villi also included illustrations of what Meckel had seen through the microscope.[129]

Clearly, the role of the microscope in medicine and anatomy is more complex if we examine these lesser-known texts. Microscopy seemed to gain more attention in these publications, especially in the 1810s and 1820s. Yet, did this matter? Such texts might have kept microscopy within the realms of medicine and anatomy, they might even have been a sounding board for innovative microscopic research, but the major voices of the Paris clinical schools rang louder.

ii. Laennec and the return of medical instruments

In addition to Bichat, La Berge highlights Laennec as one of the main opponents of microscopy. As a dominant figure in Paris medicine after Bichat, his criticisms would certainly reverberate for the next generation of clinicians. Let us explore his view of instruments and anatomy in more detail.

Laennec shared Bichat's interest in organs and tissues but Laennec's main contribution was to ally pathology with disorders in bodily function. One might therefore credit him as a 'pathophysiologist' rather than a pathological anatomist. Unlike Bichat, Laennec did not disapprove of instruments which enhanced the senses. His famed invention, the stethoscope, allowed one to see or explore ('scope') inside the chest ('stetho'). Laennec was also critical of other vitalists such as Vicq d'Azyr, whose entry on pathological anatomy for the *Encyclopédie méthodique* was, in his opinion, lacking method and taste.[130] He was also frequently involved in priority disputes, notably with Guillaume Dupuytren (1777-1835). The source of the dispute

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[129] A. Meckel, 'Sur la structure de la membrane muqueuse des intestins dans l'homme et dans quelques animaux', *Journal complémentaire du dictionnaire des sciences médicales*, 7 (1820), pp.209-24.
[130] Duffin, p.67. F. Vicq d'Azyr, 'Anatomie pathologique', *Encyclopédie méthodique, médecine, par un société de médecines* (13 vols., Paris, 1821), ii, pp.237-614.

was Laennec's reform of Bichat's pathological anatomy. While Bichat had examined tissue pathology related to inflammations and cancerous growth, Laennec called for four distinct branches of pathology: nutrient deficiency, form/position, texture, and foreign bodies.[131] The microscope played a role in the final of these with its utility in diagnosing inanimate and parasitic infections. Laennec's first claim to fame was the study of parasites which earned him admission into the Société de l'École in 1804.[132] The manuscript included several plates depicting intestinal worms as seen under the microscope. Laennec later outlined his four branches of pathology in same edition of the *Dictionaire des sciences médicales* in which Cuvier and Virey had discussed microscopic animals and parasites.[133] Laennec also included an entry on the parasitic worm, *Ascaris lumbricoides* in which he noted microscopic examinations.[134] Laennec's interests in the microscopic world reveals close ties between natural history and medicine despite the professional reforms being instituted in Paris. Cuvier may have acted as a patron and role model for the young Laennec as both leaned politically right and both seemed particularly adept at weathering the political climate of Paris during the Napoleonic and Restoration eras.[135] Cuvier held the chair of natural history at the Collège de France and may have guided Laennec's admission to the faculty.

However, if we move beyond his search for parasites, did Laennec concede to the vitalists' disdain for anatomical microscopy? While Laennec is often assumed to have shared Bichat's vitalism, historian Jacalyn Duffin has made a contrary argument. In her biography, Duffin contends that Laennec's pathology did not conform to the 'school' of vitalism. The term has been applied to a broad group of early physiologists

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131 Russell Maulitz outlines Laennec and Dupuytren's anatomy in more detail. R.C. Maulitz, *Morbid Appearances: The Anatomy of Pathology in the Early Nineteenth Century* (Cambridge, 1987).
132 R. Laennec, *Mémoire sur les vers vésiculaires et principalement sur ceux qui se trouvent dans le corps humain* (Paris, 1804).
133 R. Laennec, 'Anatomy pathologique', *Dictionnaire des sciences médicales,* ii, pp.46-61.
134 R. Laennec, 'Ascaris'. *Dictionnaire des sciences médicales,* ii, pp.339-52.
135 C. Duffin, *To See with a Better Eye* , pp.58-9.

such as Laennec simply because their discoveries conformed to the same language of 'vital principles' or 'life forces'; however, one should not immediately suspect that Laennec's instruments allowed one to see ‘vital forces’. It is also frequently claimed that Laennec was an anti-materialist. Duffin argues that this was not an endorsement of the vitalistic tradition; rather, Laennec rejected atheism and a strict materialist notion of the absence of the soul. In his work, Laennec pointed to both 'vital lesions' and solid ones which must have purely material origins. This is perhaps why Laennec was more willing than other members of the Paris clinical tradition to investigate the body with more detail using instruments as an aid to the clinical 'gaze' and dissection.

Duffin does not rule out the likelihood that Laennec applied his microscope to anatomy and pathology. Unfortunately, Laennec never listed the type of microscopes he used or their makers. We do know, however, that one of his microscopes was auctioned off after his death to a buyer named Flandrin.[136] Nevertheless, if Laennec used his microscope to study pathological features other than parasites and infectious animalcules, he was not forthcoming about his results. Laennec warned members of the Société de l’École de Médecine that pathological anatomy must not be done with a '*loupe*'. [137]

Duffin contends that Laennec ultimately distanced himself from microscopic pathology, not because he adhered to vitalism, but because he was against organicisim. Often seen as the paradigm which replaced the vitalism of the Montpellier school and early Paris tradition, organicism characterized all diseases as arising from some change to the normal structure of an organ. It is most often associated with anti-vitalists because of its insistence on the exploration of minute parts. Thus, every disease must have a visible lesion or mutation. Yet Laennec had found, through dissection and microscopy, that organ structures varied widely. Bodies

[136] Duffin, n.14, p.354.
[137] Quoted in Duffin, p.274.

could suffer internal damage or abnormal deformities without incurring illness. Thus, ‘because structural damage could be tolerated without illness, he doubted that a search for minute changes could elucidate disease’.[138] Given Laennec's early use of the microscope and his eventual disdain for organicism, it is possible that Laennec was not in fact doubtful of the microscope's abilities. He saw it as a useful anatomical tool but one which could lead to misinterpretations when applied to pathology. Like Bichat and many of the other physicians mentioned in this chapter, he did not doubt the ability of the microscope to see; he doubted what it could see.

8.4. Conclusions

To further study the sceptics of microscopy, I would call for greater attention to institutional reform. What role did the microscope have in curricula after the clinical reforms of the late eighteenth century? As we have already seen, there was no institution of well-defined instruction in 'medical microscopy' in late eighteenth-century education, but neither does one find distinct disciplines labelled as 'physiology' or 'biology'. In addition, eighteenth-century themes of vitalism and sensualism informed Paris medicine at the dawn of the nineteenth century. Thus, Paris hospital education was not an entirely new system of anatomy and clinical practice; rather, it was a codification and institutionalisation of past hypotheses. This process was as much exclusionary as inclusive; microscopy seems to have fallen by the wayside, yet it was not forgotten.

Thus far, the attention of historians has been to explore the rise of the Paris medical 'school' in such depth that we may often forget that medicine and anatomy were not so distinct from natural history and chemistry as historians might assume.

[138] Ibid., p.274.

Microscopy, which transcends these disciplines, is absent only if we focus our attention solely only on the clinic or the dissection theatre. Moreover, the writings of Bichat, Cabanis, Laennec and other members of the new clinical tradition may seem to break with past medical *epistemes*, but their professional ties necessitated influences from outside this small group. We must further turn our attention to scientific literature outside the major treatises, particularly those works which illustrate the multi-disciplinary nature of late eighteenth- and early nineteenth-century science such as dictionaries and encyclopedias. These texts reveal a world in which anatomical knowledge was shared across disciplines. Through such texts we find both skeptics and proponents of the microscope, but not an absence of microscopy altogether.

As we also have seen, late eighteenth-century microscopy retained its place in natural history; why then does the shift away from microscopy in French medicine seem so remarkable compared to other fields of science? First, *ancien régime* intuitions had already laid the foundations for the new Paris schools. Vicq d'Azyr, Hallé, and other anatomists and hygienists had promoted gross dissection, comparative morphology, and sensory perception; in doing so, they had already begun to abandon the dependence upon instruments, including, but not limited to, the microscope. Moreover, the vitalist physicians at Montpellier certainly influenced later Paris clinicians, most of whom were Montpellier graduates. Second, Paris medicine did not suffer from the same factions as found among naturalists or chemists. Certainly, Paris clinicians fought intense personal rivalries; however, no single clinician attempted to wield as much institutional authority as, for example, Cuvier, although he himself was often powerless to silence dissenting voices at the *Muséum national d'Histoire naturelle* and elsewhere. The Paris clinicians more often than not stressed collaborative reform. When famous rivals such as Laennec and Dupuytren, or

Broussais did spar, it was often over nomenclature, nosology, and accusations of plagiarism rather than methodology. In the words of Jacalyn Duffin, their rivalries 'reflected grudging but profound respect and sometimes even tacit agreement'.[139] Although they might not have been as successful in implementing reforms as Foucault and Ackerknecht assumed, they did seem to share a disdain for mechanism and a fundamental trust in their natural *sensibilité*. Of course, not all of the leaders of the new Paris curriculum shared the same vitalistic doctrines; nevertheless, a broad and admittedly vague notion of the 'vital forces' dominated the new medical curriculum until well into the nineteenth century. In other words, they shared a medical gaze which emphasised function, movement, and sensation, not microscopic detail.

However, these clinicians were not ignorant of advances in medicine and microscopy outside of France. Recent historians such as Othmar Keel have uncovered ever closer ties between Paris and London concerning the development of pathological anatomy. One might credit Bichat as the father of histology and pathological anatomy, but Matthew Baillie and the Hunter brothers had already been investigating tissue lesions. Moreover, pathological anatomy traces its roots at least to Morgagni earlier in the eighteenth century.[140] Yet, Morgagni, Hunter and, to lesser extent Baillie, were willing to embrace the microscope. Although it may not have been central to the development of pathological anatomy before Bichat, the absence of the microscope (and in Bichat's case, its condemnation) reveals that an epistemic shift was indeed underway in France.

In conclusion, Bichat and other critics of microscopy were not ignorant of the microscope's potential; rather, the microscope was passed over as their vitalistic doctrines which stressed bodily reactions and responses over dead, observable, matter.

[139] Duffin, 'Laennec and Broussais: the "Sympathetic' Duel', *Constructing Paris Medicine*, pp.251-52.
[140] O. Keel, 'Was Anatomical and Tissue Pathology a Product of the Paris Clinical School or Not?', *Constructing Paris Medicine*, pp.117-84.

To them, the microscope was emblematic of an 'old' way of seeing. Nonetheless, they were attempting to fight an uphill battle against an instrument that had already become ingrained in natural history and previous modes of anatomy. In abandoning the microscope, the Paris physicians were codifying their doctrines. They were setting their profession apart.

Chapter 9. Conclusions

The history of the microscope is not a story of progression or origins. It is a story of brass, glass, and human beings. We must study each use of the microscope in its own terms. Examining historical microscopes alongside historical texts may help us discern the evolution of microscopy, but we must be cautious not to fall into the trap of teleology. So what can we conclude?

Microscopes are difficult to use. That, above all, characterised each of my attempts to re-enact these historical experiments. The difficulties were two-fold. First, these microscope models each required different techniques in order to focus upon a specimen. Far more time was spent prepping these microscopes and slides in order to attain focus rather than actually studying the images that they presented. Practice and familiarity with these models mitigated some difficulties. I found that the mid-eighteenth-century microscopes, particularly the single-lens Wilson screw-barrel and Cuff's compound microscope, were the least troublesome devices. I was able to discard a tissue sample, prepare another, and find nearly-identical images through these instruments. With this in mind, I could see why craftsmen often included the same types of specimens in sample slides that were sold alongside the microscopes: fleas, the wings of a fly, or pollen grains may have been common novelties for the amateur client, but they also provided duplicable points of reference to test different lens objectives and different microscopes, much like standardised micrometers and diatom slides today. Replication as much as novelty determined the success of eighteenth-century instruments. This explains why simple microscopes such as the screw-barrel remained popular despite the arrival of more complex and ingenious devices. A patron's ability to correspond with clients would have facilitated replication efforts as Baker had done. User manuals such as those of Cuff, Adams, and Martin served as advertisements but they also addressed usage problems. They were particularly helpful in re-enacting these experiments.

The second difficulty concerns interpretation. Even if one were able to duplicate an image, interpretation rests on some precedent or a theoretical framework. Modern histology has become a map with which to guide the microscope. In this way, the microscope becomes a tool to observe the known rather than a true philosophical instrument capable of imparting new discoveries and eradicating old paradigms. Yet if we remove the map then a convoluted world suddenly appears beneath the lens. These case studies illustrate the importance of preconceived notions in determining interpretation. These include Buffon's dual-sex theory of generation, Della Torre's globular hypothesis, Monro and Hunter's theory of lacteals as absorbent vessels, or Samoilowitz's contagionism. Such notions, correct or not, prompted microscope research, but they also dictated interpretations. This is precisely what early microscopy had to overcome to be of any inductive use to medicine and anatomy. Whether it has ever done so remains debateable.

9.1. Further questions

Did microscopical discoveries alter the nature of anatomy and medicine during the Enlightenment? There is no definitive answer. In the hands of an ardent microscope user such as Baker, Monro, or Hewson, there was no doubt that the microscope was revolutionising their professions. To these experimenters, the microscope was a philosophical instrument capable of imparting new discoveries. Often their immediate opponents were not unsympathetic to microscopy, but critical of one particular type of instrument over another. To other opponents, such as the clinical vitalists, the instrument was either flawed or it served only to confirm what was already known. Although few, if any, of the Enlightenment-era figures discussed in this thesis denied that the microscope had not served to advance science and anatomy. Their doubts concerned whether or not the microscope would continue to do so.

When it came to a contested issue such as the corpuscular philosophy, contagionism, or spontaneous generation, the players in the debate had likely made up their minds long before peering through their lenses. Moreover, during many of my re-enactment experiments, I found few major differences among the types of instruments used in these debates aside from magnification. Different eighteenth-century era microscopes did not display radically different images. Rather, I suggest that the differences of interpretation lay with the microscope users, not the microscopes.

This thesis has made a case for re-enacting experiments from the history of medicine and science. Of course, by current standards, my experiments were flawed and would never pass as proper histology; it was certainly not the sort of histology I have studied in modern labs. Yet I hope that through these experiments I have captured a glimpse into past methodologies, not past discoveries. My goal of these re-enactments has not been to test if a historical experiment was flawed or empirical, modern or short-sighted; rather, I have attempted to address the difficulties that microscope users faced when assigning meaning to new images of the body and value to indeterminate data. Even here, my efforts have fallen short as it is impossible to recreate the exact conditions or even to find the same instruments that past experimenters had used. I have benefited from being able to access some exceptional instrument collections, which allowed me to examine the same microscope models used in these past case studies, but these instruments were certainly not in the same condition as they were when first used. More troublesome is the fact that my own investigations have been marred by foreknowledge of modern medicine and anatomy.

In short, we cannot make any conclusions from such re-enactments. We can however pose new questions.

Given the difficulties in microscope observation, how did microscope users reach consensus? As my case studies have shown, the microscope provided a medium for debate, but not conclusive evidence. Few users changed their mind when confronted with contradictory evidence; one could just as easily point to new models and modes of specimen preparation in order to vindicate a claim. With this in mind, one wonders if microscopy has ever impelled paradigm shifts. If it has in more recent history, what conditions differed from those of the eighteenth century? On the other hand, a social constructivist approach might dismiss the notion that anatomical discoveries and technological advances have induced new paradigms. Nevertheless, we must acknowledge that the microscope had become a facet of knowledge display by the end of the nineteenth century. It is tempting to locate its widespread acceptance among medical circles in the eighteenth century. There is evidence for this: passive and unchallenged references to the microscope in medical texts, or the extensive collections of microscopes in the cabinets of prominent medical authorities such as William Hunter. Nonetheless, the repudiations of later leading clinicians revoked its status. Similarly, the high praise for achromatic lenses in the middle of the nineteenth century seems to overshadow the presence of simpler designs a century before. When do we see the final acceptance of the microscope as an instrument of medical discovery and an icon of authority?

9.2. Looking forward

As shown in these case studies microscope users hailed new types of compound microscopes, lenses, or focusing mechanisms, yet these intensified rather than curtailed controversy.

Perhaps because of the failure of the microscope users to reach consensus, leading clinicians at the start of the century found little use for the instrument. Yet, as I have argued in Chapter 8, medical and anatomical microscopy still found adherents often outside of the medical schools, or the leading scientific circles. Although it is clear that the use of the microscope fell under suspicion among eminent medical professionals, we must remember that medical and anatomical discovery was not the exclusive purview of physicians at the leading schools. Rather, the lines between natural history, chemistry, medicine, anatomy remained blurred. Further research, like that of Ann La Berge, could expand upon on the reasons why leading physicians and anatomists took up microscopy again in the 1830s and 1840s despite the warnings of Bichat to the preceding generation. Did the return to medical microscopy follow the development of achromatic lenses or did it coincide with changes in the social structure and philosophy of science? As shown in Chapter 5, Milne-Edwards and Dutrochet certainly continued to use the microscope to examine the globular hypothesis; Milne-Edwards pointed to the supposed superiority of his Adams microscope to justify this pursuit despite the fact that the Adams microscope was actually a late eighteenth-century design. It is conceivable that the achromatic microscopes issued by Lister and Amici served a similar purpose: to justify a fresh look at old ideas.

However, if we accept Jan Deiman's thesis that chromatic aberration was not as detrimental as assumed, then we must call into question the importance of achromatic innovations. Deiman finds that non-achromatic instruments remained of use long after Lister and Amici's discoveries.

> In Utrecht, with its well-equipped laboratory, the professors mentioned on an inquiry form of 1848/1849 not only the microscopes made by Amici

> and Oberhauser but also the old ones made by Martin, Dellebarre, Jones, Adams and Van Deijl.[1]

The continued use of older models in the face of new designs is certainly relevant. My re-enactment experiments have often shown the clarity and ease of use of older simple-lens microscopes in comparison with their more celebrated compound lens designs. Taking this project further into the nineteenth century might offer a similar story. Some nineteenth-century microscope users such as François-Vincent Raspail (1794-1878) favoured the simple single-lens microscope. Raspail wrote against both the compound microscope and the new trend for achromatic lenses.

> Truly, I cannot say that the most perfect achromatic lens system could ever be as achromatic as a simple glass lens ... In summary, the advantage of the compound microscope, both exaggerated by the quackery of the observer and the know-how of the manufacturer, is reduced to the value of undoubted utility, but can never be regarded as an absolute necessity.[2]

Aside from achromatic lenses, future research could address the impact of new chemical dyes such as aniline dyes and Golgi staining techniques towards the latter half of the nineteenth century. Historians have already done so, but often from a deterministic perspective.[3] Shall we assume that the discovery of chemical dyes resolved medical and anatomical questions? Did they allow for new discoveries or did they simply allow evidence for new hypotheses? Like the fashion for new instruments in the eighteenth century, did they lend weight to older notions and reignite long-standing debates?

Further research on nineteenth-century microscopy will help elucidate how instruments affected disease etiology. Whereas early nineteenth-century physicians, such as Hallé, may have known about the microscope's role in diagnosing disease, their sanitary measures were not dependent on microscope diagnoses. This is not to

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[1] J.C. Deiman, p.8.

[2] F. Raspail, *Nouveau système de chimie organique: fondé sur des nouvelles méthodes d'observation* (Paris, 1838), p.245.

[3] J.R. Baker, *The Discovery of the Uses of Colouring Agents in Biological Micro-technique* (London, 1945); G. Clark and F. Kasten, *A History of Staining*, 3rd edn. (Baltimore, 1983).

say that miasmatic theories of disease had overshadowed the role of the microscope. As we have seen in the case of the plague of Marseilles in the early eighteenth century, microscope users still sought out particulate forms of contagion that composed these miasmas. Similarly, as in the case of Samoilowitz and the plague of Moscow, physicians theorised that microscopic worms could be the signs of disease even if the epidemic arose from bad air, fermentation, or other miasmatic causes.

A detailed look into nineteenth-century epidemiology would better illustrate when and how the microscope became further associated with disease etiology. In light of the eighteenth-century use of the microscope to study pathogens and the signs of disease, it is perplexing that the instrument did not play a greater role in diagnosing yellow fever, typhoid fever, or cholera in the nineteenth century. Or is this an oversimplification? There are a number of case study figures who could be re-examined with special attention to their use (or disuse) of the microscope. John Snow (1813-1858), for example, made almost no mention of the microscope in his famed cholera studies; instead, Snow is most often remembered for his quantitative study of occurrence patterns.[4] However, some of Snow's earliest publications concerned anatomical microscopy[5] and he used the instruments during autopsies.[6] Cholera debates of the late 1840s illustrate that microscopy was used in epidemiology long before Koch and the bacteriological revolution. Pelling notes that microscopical societies were key to debating the origins of cholera. Members of the London Microscopical Society had actually disputed the observations of their colleagues at the Bristol Microscopical Society such as Joseph Swayne (1819-1903) and William Budd (1811-1880) who had posited that micro-organisms or fungi were the cause of

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[4] J. Snow, *On the Mode of Communication of Cholera*, 2nd edn. (London, 1855).
[5] J. Snow, 'On the Circulation in the Capillary Blood-vessels, and on Some of its Connections with Pathology and Therapeutics', *London Medical Gazette*, 1 (1843), pp.810-16.
[6] J. Snow, 'On the Cause and Prevention of Death from Chloroform', *London Journal of Medicine*, 4, no. 42 (1852), p.567.

disease.[7] With this in mind, Snow's statistical work does not seem to be predating medical microscopy but consciously avoiding it.

As in the eighteenth century, we may find that nineteenth-century supporters of miasma theory or contagionism employed the microscope. Thomas Southwood Smith (1781-1861), for example, led sanitation reforms to eliminate social and physical elements which might favour putrefaction and fermentation, but in some cases, he employed the microscope to study how unsanitary conditions might lead to disease.

> The moisture in the air of a crowded room may be condensed by ice ... If a portion of this deposit be put on a piece of platinum and burnt, a strong odour of organic substance is given off, and a quantity of charcoal remains. If the deposit be allowed to stand for a few days, it forms a solid, thick, glutinous mass, having a strong odour of animal matter. If examined by a microscope, it is seen to undergo a remarkable change. First of all, it is converted into a vegetable growth, and this is followed by the production of multitudes of animalcules,—a decisive proof that it must contain organic matter, otherwise it could not nourish organic beings.[8]

This is not 'germ theory' or an animalcule theory of contagion; rather, Smith used the microscope to reveal that malignant air could generate animalcules which, in turn, could generate disease if they had a favourable host. Ventilation thus worked by dispersing malignant air and stemming putrefaction and organic growth.

In addition to these studies, one might take the current thesis further to explore the evolution of doubt surrounding the microscope. Who were the reactionaries to new microscope discovery? Why, for example, did Pasteur and Lister's microscopic observations reach a favourable medical audience while similar experiments had been overlooked in the past? This is not to assume, anachronistically, that earlier experimenters had tried and failed to establish germ theory. Instead, we must examine

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[7] M. Pelling, 'The Cholera-Fungus Controversy of 1849' in *Cholera, Fever, and English Medicine*, pp146-202.
[8] T.S. Smith, *The Common Nature of Epidemics, and their Relation to Climate and Civilization from Written and Official Reports by Southwood Smith,* ed. T. Baker (Philadelphia, 1866), p.14.

the doctrines of their differing audiences and the capabilities of their different instruments.

Those who had disclaimed microscopy in the eighteenth and early nineteenth centuries were more often than not persuaded that the instrument had indeed seen deep beyond nature's visible realm. The great plethora of animalcules and infusoria which Hooke, Leeuwenhoek, Trembley and others had witnessed were undeniable and their experiments were frequently duplicated. Nevertheless, those who discounted medical microscopy, such as Bichat and Laennec, believed that it had outlived its usefulness to pathology. Thus, microscope research among later nineteenth-century physicians was a renewal of interest rather than a birth of the modern. Here we must be careful not to assume presentism. There is no proof that eighteenth-century microscopy survived its critics to inform nineteenth-century pathology, cell theory, or bacteriology. For such 'revolutions' to occur, methodologies would first have to change. The interests of physicians and anatomists would need to revert from the macroscopic to the microscopic.

Elevating the microscope to become a useful scientific instrument was a difficult process, not necessarily because of technological flaws inherent in early microscopes, but because of the problems one faced when interpreting images. Corpuscles of blood, spermatozoa, parasites, cells, and bacteria are now all visible under the lens, but their presence means little without a conceptual framework.

I am reminded of the tale of Caesar Cremonini, the philosopher who supposedly refused to look through Galileo's telescope. That legend, of course, may be apocryphal, but it is useful for anyone encountering doubts surrounding the utility of scientific instruments. Elizabeth Haigh uses this story to explain Bichat's refusal to use the microscope. Bichat, like the Aristotelian, 'could not be persuaded that the images, that suddenly become visible when some lenses are interposed between one's eye and an object, exist in reality.'[9] I have taken a different perspective from Haigh to explain such doubts. We must remember that if Cremonini did not wish to see, it was not because he could not reconcile the image with reality, nor is it only that he feared the new technology would contradict Aristotelianism or scripture. Cremonini rejected this wonderful instrument of science because it did not satisfy his pursuit of truth. The telescope could certainly observe the heavens, but through Cremonini's eyes it could teach us nothing about the final cause, the all-important question of 'why' an object exists as it does. It is a challenge which exists to this day as we delve deeper into the human body, the brain, and our genetic code, yet still we cannot easily decipher what we see or why it matters. In conclusion, if the revolution in medical and anatomical microscopy was on its way, it was likely not technology-driven. It was not a case of new microscope models and achromatic lenses bursting onto the scene and tearing down the dominant paradigms. Science and medicine had to first question how one saw before embarking on new ways of seeing. The revolutions to come were revolutions delayed.

[9] E. Haigh, 98.

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5. Museum Collections

Museum Boerhaave, Leiden

Museum for the History of Science Oxford

Dr. Reginald S. Clay Collection (former)

Royal Microscopical Society Collection (former)

Royal College of Surgeons Hunterian Museum

RCSHC/Hewson (Hunter Collections, Hewson Microscope Slides)

University of California Berkeley

Golub Microscope Collection