The Epistemic Stakes of Shellfish Classification: Disciplinary Disputes in English Natural History, 1803-1843

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Abstract

This article reconstructs a curious controversy between advocates of rival natural historical disciplines in England, each vying for the authority to organize the study of shellfish. Against a common reading of such taxonomic debates as dry trivialities, I contend that the dispute serves as a lucrative historical prism through which broader and more impactful trends in natural history are uniquely refracted. I show that a close reciprocal relationship exists in taxonomy between seemingly mundane details and abstract philosophical problems; how one classifies shellfish bears upon how one views the *telos* of classification as a collective enterprise, and vice versa. I also aim to demonstrate that controversies in systematics and classification are perpetually modulated by a causal feedback loop between what one may brusquely label social and intellectual factors—systematists' philosophical commitments are constantly negotiated against their collective desire to maintain a certain degree of coherence in the field, and thus fortify it against collapse into a state of epistemic anarchy in which disparate and incompatible schemes of classification exist in perpetual competition.

Introduction

How did new disciplines and methods in nineteenth century systematics emerge, on what grounds were they resisted, and what broader implications did such contestations have on taxonomic practice? In a 2014 article, entitled “The Birth of Malacology: When and How?,” ecologist Maxim Vinarski asserts that malacology, the modern discipline organizing the study of the invertebrate phylum *Mollusca* (containing squids, snails, and shellfish, among others) occurred precisely on May 10th, 1795. On this date, Vinarski relates, famed French naturalist Georges Cuvier delivered a lecture at the Museum National d’Histoire Naturelle in Paris on the arrangement of *Vermes*—the sixth class of animals under Carl Linnaeus’s sexual system—in which he proposed a synthesis of the orders *Testacea* (shelled, aquatic invertebrates) and *Mollusca* (non-aquatic invertebrates). Such an amendment seemingly disposed of the need for a discrete discipline of conchology, which was, up to that point, the traditional Linnaean home for the study of shellfish. Under the young Cuvier’s proposed system, any attention towards shelled invertebrates alone would prove too narrow in scope to merit disciplinary status.

Yet, despite Vinarski’s proclamation, systematic texts under the moniker “conchology” were continually produced well into the nineteenth century—as late as the 1840s, prominent English naturalists published multiple works declaring implicit allegiance to this supposedly-defunct discipline. If malacology was indeed “born” following Cuvier’s 1795 lecture, its maturation and ultimate disciplinary entrenchment within natural history were undoubtedly contested. Analysis of the fora, media, and content of that contestation affords not only an opportunity to shed light on a particular facet of nineteenth century biological systematics, but also to relate the controversy’s

role in shaping a broader referendum on the Linnaean system occurring in England at the turn of the century. My aim, therefore, is to simultaneously uncover a seldom-discussed clash in the history of systematics, and to reveal its crucial, curious, and contingent role in the decline of strict adherence to Linnaean systematic practice.

To that end, this article will survey early nineteenth century English works published under the banners of both conchology and malacology, with particular attention toward their prefaces and introductions—in which the majority of their respective authors' methodological framings, reflections, and commentaries are transcribed. Implicitly and explicitly, authors of these works routinely use these passages to declare allegiance to, critique, and defend an array of methodological commitments in contemporary natural history. At stake was, in all cases, more than just the credibility and status of conchology or malacology—the controversy served as a contingent epistemological battleground in which broader and more abstract disputes regarding the ideal and legitimate practice of systematics were waged. The parallel publications of both conchological and malacological texts in early nineteenth century England simultaneously refract the nature of these broader disputes and constituted important sites of contestation for them.

I am indebted primarily to two works of history in providing theoretical framing and historical context to my analysis of this disciplinary controversy. Philip R. Sloan's article in the October 1976 issue of *ISIS*, "The Buffon-Linnaeus Controversy," sheds light upon a fundamental divide in biosystematics that reverberates well into the nineteenth century. The dispute, binarily framed, regards the purported "artificiality" or "naturality" of classification systems in natural history. An artificial classification system

is self-avowedly heuristic—it serves as a useful tool in organizing natural objects—whereas a natural system, rather, *the* natural system, mirrors the true arrangement of species, genera, families, orders, and classes in nature, as they were supposedly arranged in the plan of creation. Buffon's critique of Linnaeus, in essence, was that the classification scheme presented in the *Systema Naturae* was thoroughly artificial, and that the fundamental analytical features that governed the subdivision of his taxa were necessarily arbitrary. Buffon's attack, according to Sloan, issued a non-fatal but long-reverberating blow to the hegemony of Linnaeus's sexual system of classification.[1]

Peter F. Stevens's *The Development of Biological Systematics: Antoine Laurent Jussieu, Nature, and the Natural System*, though primarily a history of botanical systematics, deals with methodological commitments and divides in turn-of-the-century classification that cut across disciplinary boundaries—the very same which this paper aims to reflect in conchology and malacology. Of particular use to my purposes is Stevens's account of the contested rise of comparative anatomy as a credible source of derivation of the analytic characters that systematists founded their classificatory schema upon.

To adequately understand English conchological and malacological texts, one must afford attention to the historical context of their production. The institutional landscape of natural history in England was largely decentralized; authors often belonged to an assortment of learned societies (in which they developed and presented their works) but often published independently and at personal expense. English universities were not prominent fora of conchological-malacological debates—rather,

[1] Sloan, "The Buffon-Linnaeus Controversy," 359.

they took place in transactions of scientific societies, in letters, and, as we will see, in the introductions to independently-published works. In the absence of such professional institutionalization, valid systematic practice in England largely needed to be negotiated without the aid of the burgeoning professionalized credibility networks on the European continent.

However, one institution bears heavily enough upon our subject to deserve mention. The Linnean Society of London, founded in 1788 by James Edward Smith (following his purchase of the totality of Linnaeus's personal collection of books, specimens, and correspondences) was the most prominent and prestigious site of systematic practice in early nineteenth century England. Its members' reverence for, and frequent defense of, Carl Linnaeus's classificatory system and method continually shaped the development of English systematics.

A number of conceptual dichotomies provide the basis for the arguments presented in this paper—Linnaean and non-Linnaean, anatomical and external, popular and scientific, etc. This is in alignment with not only contemporary historical commentary on the subject and period, but with the language and arguments deployed by nineteenth century naturalists as well. That said, while necessary, such binaries may yet belie the full complexity of the historical developments in question.

Policing Taxonomic Borders

William George Maton and Thomas Rackett's *An Historical Account of the Testaceological Writers* was read before the Linnean Society over four successive meetings in 1803. In the account, Maton and Rackett gave a history of conchology,

related chronologically via descriptions of the work of the field's various contributors, and accompanied by the authors' commentary on the quality and magnitude of each naturalists' contributions. The history, however, is more than just a record documenting any and all notable works on the subject—Maton and Rackett employ their commentary throughout the *Account* to defend a conservative interpretation of valid, and thus for them, strictly Linnaean, methodology. The history advances a program modern historians are familiar with, the reinterpretation of a succession of events in accordance with (and in defense of) a particular narrative.

In their entry on Dutch naturalist Jacob Klein's 1753 *Tentamen Methodi Ostracologie*, in which he advanced a new and extra-Linnaean system of shell classification, Maton and Rackett praised the author as "a very able naturalist" but nonetheless spent the majority of their commentary regretting that it "certainly does not possess the merit of practical utility." Critiques on the basis of a lack of practicality, utility, efficiency, or simplicity were Maton and Rackett's primary mode of attack on non-Linnaean systematics. Klein's *Tentamen Methodi* was "written professedly with views to the establishment of a system," but "The general divisions...are too numerous...species are constituted in some instances without being referable to any genus; and in one of the *parts* there is a solitary genus without any class." To Maton, Rackett, and the Linnean Society, such systematic practices were anathema to the elegant proportionality of classes, orders, families, and genera. Maton and Rackett venture no explanation of why Klein deigned to deviate from Linnaean systematic orthodoxy—they merely criticized the work for a seeming lack of simplicity and cohesion embodied, in

their minds, by Linnaeus's original division of the class *Vermes* in the *Systema Naturae*.[2]

Critiques of conchological authors and texts that propose amendments to the Linnaean arrangement of orders, classes, and families in *Vermes* pervade the *Account*. Jean Baptiste Lamarck was far too prominent a naturalist by 1803 to elicit condemnation of his non-Linnaean system which, Maton and Rackett took care to note, added sixty-three genera to *Testacea*. Their sole recourse was merely to "question whether it has improved the perspicuity of the original." Less reputable naturalists who proposed structural changes were more easily dispensed with. George Montagu's work in conchology, though founded upon the Linnaean system, nonetheless "deviated from it in placing many Linnean *Helices* under the genus *Turbo*, and arranging all the depressed species of the former without regard to the shape of the aperture," eliciting claims of unscientificity: they concluded Montagu's entry by condescendingly remarking "the author cannot be said to have employed...the Linnean mode of description; but this is a circumstance which will not be regretting by ordinary readers."[3]

However, amendments at and below the genera-level were regularly tolerated and, furthermore, deemed a necessary inevitability. "There can be no doubt that the *Mya Perna* of the *Syst. Nat.* [*Systema Naturae*] admits of being made a distinct genus," the authors wrote in their entry of Swedish systematist Anders Retzius. "How partial [the reader] may be to the original arrangement of Linnaeus, he will not be disposed...to accuse the Testaceologist of any *rashness* of reform."[4] At issue for Maton and Rackett

[2] Maton and Rackett, *An Historical Account of the Testaceological Writers*, 169.
[3] Ibid., 211, 214.
[4] Ibid., 204.

were super-generic reconfigurations of the Linnaean system, those which threatened the stability of its higher structures. Transpositions of species across genera—and both divisions and combinations of genera—were not considered heretical alterations. The ideal development of systematics, for the authors, would consist almost exclusively of such amendments.

On the work of the recently-deceased French naturalist Jean Guillaume Bruguière, Maton and Rackett praised its being "obviously founded on [the system] of Linnaeus," but noted with some concern that though "the author professes to deviate from it no further than he conceives himself to be required...The number of *genera*, however, is nearly double that of Linnaeus's."[5] Here, generic subdivision seemed to transgress rational necessity and verged upon excess. Bruguière's recent death may indeed have stemmed the authors' impulse to bemoan this fact and no direct attack on this point is presented, but its mere mention serves to record their discomfort. They proceed, following this point, to avoid further comment on his systematics, and only praise the quality of Bruguière's plates and illustrations. It is clear that Maton and Rackett were certainly interested in defending Linnaean systematics from fundamental or "rash" alteration, but they did not believe Linnaeus's arrangement, as transcribed in the twelfth and final edition of his *Systema Naturae*, to be perfectly accurate down to each genera and species-level.

Resistance toward structural changes to the Linnaean order *Vermes* forms the basis of the most scathing attacks found in the *Account*, but milder critiques exist as well. These are most often directed at the analytical methods by which conchologists

[5] Ibid., 207.

derived the primary characters—the fundamental units of observation—that undergird the construction of classificatory systems. Particularly, Maton and Rackett expressed reticence towards derivation of such characters from anatomical observation. “It is a task as laborious as it is unlimited to push the examination of natural objects beyond the ordinary powers of the senses;” they digressed in their entry on John Adams, “and it may, perhaps, be questioned by some, whether such pursuits lead to any useful practical purposes...”[6] Practicality was, again, invoked without precise definition here; by all indications, Maton and Rackett’s understanding of “practical” was synonymous with close congruity with the structure of the *Systema Naturae*.

But their attitude towards such observations was more one of trepidation than outright condemnation. In their entry on Georges Cuvier, an avowed member of the burgeoning school of anatomy, Maton and Rackett labeled him “a very able comparative anatomist,” whose dissections they had “much satisfaction in particularizing.” They went on to express the “hope that M. Cuvier will continue to present us with other examples of the successfulness of his researches among this still imperfectly known order of animals.” No mention was made, however, of Cuvier’s adoption of malacology as the proper discipline to organize the study of the *Testacea*—a transition ultimately driven in large part by the entrenchment of comparative anatomy as a legitimate source of analytic characters in systematics. The suspicious absence of such a relevant stance could potentially be the result of genuine ignorance—Cuvier’s lecture was delivered and transcribed in French, and the extent of its dissemination is unclear—but could equally betray a reticence to make note of his extra-Linnaean commitments.[7]

[6] Ibid., 209.
[7] Ibid.

Further evidence of the *Account*'s ambivalence towards anatomical methods in systematics and of a growing rift between a focus on the internal anatomy of shellfish and typological analysis of their shells lies in its entries on Austrian naturalist Giovanni Antonio Scopoli and Danish naturalist Otto Friedrich Müller. On Scopoli, in whose work "the shell and the animal have been alike regarded," Maton and Rackett noted that "the author seems to have steered a sort of middle course between the advocates for a system founded chiefly on the former, and those who have made the latter the chief subject of attention."[8] They do not, however, continue to declare allegiance to one set of methods or the other, perhaps (though not conclusively) evincing corresponding tensions between the members of the Linnean Society of London itself.

In their treatment of Müller, "one of the most laborious and sagacious zoologists of his age," further tension regarding the employment of anatomical methods in conchological systematics emerges. "The characters of his *genera* are taken chiefly from the shape of the *tentacula* of the animals; in the bivalves, from the *siphon* which they protrude. Hence the Linnaean *genus Limax* is included in the testaceous instead of the molluscous order. The work...is of a very curious and instructive nature."[9] This curiosity is no doubt produced by some grasp of the implications anatomical analysis might have for the future of conchology; Linnaeus, and the conchologists that followed his methods, based their classification systems exclusively on the external features of the shells. Attention to characteristics of the invertebrate animals that inhabit such shells threatened to undermine a fundamental analytical category used in the *Systema Naturae*, and thus threatens the credibility of the Linnaean system as a whole.

[8] Ibid., 199.
[9] Ibid., 193.

Indeed, ambivalence pervades the remainder of Müller's entry; Maton and Rackett ended the passage by "expressing [their] admiration of the fidelity and perseverance with which he has added to our knowledge of that order of animals" and subsequently conceding that "as the basis of a system, his researches are not susceptible of so useful and general an application as the more artificial method of Linnaeus, they cannot fail to be of permanent importance to the common stock of natural science."[10] Where Maton and Rackett could not directly critique anatomical methods—either due to allegiances to its practice within the Linnean Society of London or genuine uncertainty about it—they were most certainly aware of the danger it posed to the validity of their system and thus founded their defense on an appeal to pragmatism.

Cracks in the Edifice

Twelve years after Maton and Rackett's thinly veiled defense of Linnaean method, disguised as a simple history of the subject, E.I. Burrows published his *Elements of Conchology, According to the Linnaean System*. Though the work advocated for Linnaean systematics (and makes no direct mention of malacology) it did so in far weaker terms than Maton and Rackett's *Account*. It begins with the following survey of its titular field that constructs a far more heterodox theoretical landscape than that of the *Account*.

> *In each class of the animal kingdom, at least in those which are most generally studied, there is some work...explanatory of the terms, and fundamental principles...but there is not one...which is calculated to throw any light upon the difficulties of Conchology. That some difficulties do exist in this branch of study,*

[10] Ibid., 194.

> *will readily be allowed; but at the same time we cannot but regret, that the very acknowledgement of their existence has extremely increased in number; for a supposition seems to have been universally indulged, that conchology lay open as a common field for speculation...The consequence has been, that scarcely two writers on the subject have agreed in their opinions, and that this general want of concurrence has aggravated the evils which each endeavoured to remove.*[11]

The culprit of such epistemological anarchy within conchology is the precise source of anxiety that underlies much of the commentary in the *Account*. Though the means underlying comparative anatomy may be credible, in Burrow's estimation they do not justify the ends they have produced. "The several writers who have dissented from the Linnaean school have, indeed, satisfactorily pointed out some flaws in the great fabric of the *Systema Naturae*," Burrow related, "but in attempting to eradicate the faulty parts...they have injured some of the main supports, and have nearly involved the whole edifice in ruin."[12] This "ruin" of the structure of Linnaean systematics, is, for Burrow, the precise result of the methodological heterodoxy within conchology that Burrow bemoaned in his opening passage above.

His fundamental anxiety concerned a potential state of complete systematic incommensurability in conchology, not just between two rival factions, but between each individual naturalist producing works in the field: "scarcely two writers have agreed in their opinions...this general want of concurrence have aggravated the evils that each author has endeavored to remove."[13] Each individual attempt to remedy such systematic incommensurability via the design of a satisfactory system of classification only served to further entrench the problem, and to amplify Burrow's concern.

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[11] Burrow, *Elements of Conchology*, v-iv.
[12] Ibid., iv-vii.
[13] Ibid., iv.

It is in this context that Burrow took recourse to a strict Linnaean systematics. Given that no centralized institution mediated consensus around credible practices in English conchology, Linnaeanism was the best available conceptual institution for conchological systematists to coalesce around. For Burrow, it was not the case that comparative anatomy was conceptually unsound. “Some have endeavored to found a system of Conchology upon the inhabitant rather than upon the shell,” he wrote, before immediately moving to note that “This plan has indeed generally been acknowledged as theoretically just, but as uniformly discovered to be defective in the execution…”[14] This defectiveness, I argue, emerged in actuality from the anarchical effect anatomical methods had upon the landscape of systematic conchology.

However, Burrow did not attack the malacologists in those terms. Much like Maton and Rackett, he did not (and, one imagines, could not) produce a convincing theoretical rebuke for the anatomical derivation of character traits in producing a system of shellfish. He, too, attacked malacologists from a pragmatic standpoint; the method’s defectiveness lay in “the utter impossibility of procuring, from the unfathomable recesses in which many, if not the majority, abide, a sufficient number of live and perfect specimens.” Almost regretfully, Burrow moved to argue that “Our knowledge of the animals being so extremely limited at present...it becomes necessary to resign all hopes of a zoological arrangement [of shellfish]...we have only therefore to take care...not to admit any incongruities into the system which we are compelled to adopt.” This compulsion emerged less from a dearth of specimens and more in direct response to the imminent threat of methodological anarchy posed by comparative anatomy. Yet

[14] Ibid., 3.

Burrow's argument was weak. In just over two decades, comparative anatomical methods would be thoroughly enmeshed with the traditional subject matter of conchology in the new discipline of malacology, and concerns over the feasibility of collecting live malacological specimens had largely evaporated.[15]

Modes of Attack

William Swainson, himself a member of the Linnean (and Royal) Society, published his *Treatise on Malacology; or, the Natural Classification of Shells and Shell Fish* in 1840. The work challenges traditional Linnaean orthodoxy in a variety of ways, and reflects a state-of-the-field in natural history in which anatomically-derived systematics enjoyed a far greater degree of credibility. In remarking on this paper's titular controversy, Swainson allied himself firmly with the malacologists.

> *Although we so far concede to the popular taste for conchology (as the "art" of arranging shells and other testaceous bodies is called) as to devote this volume exclusively to the testaceous Mollusca, we cannot sacrifice our conviction on the principles upon which these animals should be studied...independent of all consideration for the animals by which they are inhabited. To dignify any pursuit of this kind by the name of Science, seems to us quite misplaced; since it would be hardly more absurd to classify birds by the colour of their eggs, or beetles by the shape of their wings, than to propound an arrangement of shells, without a paramount regard to the animals which they cover. It is quite right that collectors of these elegant objects...should arrange them in their cabinets upon some plan, and give them names; but no one will maintain that this amusement deserves to be called science.*[16]

Swainson's tactic was to reclassify conchology as a popular art, a hobby of amateur naturalists that enjoy the collection and display of shells for "amusement."

[15] Ibid., 4.
[16] Swainson, *A Treatise on Malacology*, 8-9.

Relying rhetorically on the freshly emergent art-science binary, the properly scientific study of shellfish, resultantly, was malacology. His focus on "testaceous *Mollusca*," he claimed, was a concession; the result of the "popular taste" for shell collecting. This was a strategic choice. Attention to this particular group of animals increased the potential size of Swainson's audience. His aims for this popular appeal were no doubt twofold. Firstly, the work was published at his own cost and he may have been attempting to recoup some of his losses, and secondly, it amplified the reach of his scholarly intent to discredit the Linnaean system. Indeed, the controversy serves as grounds for Swainson to attack Linnaeanism as a whole. He wrote,

> *When we call our recollection to the lamentable error committed by Linnaeus and his disciples...we become convinced that there was about as much hope of their ever arriving at the truth by the means they chose to adopt, as that a collection of the wings of different insects should ever instruct us fully in the natural history of the animals to which they belong.*[17]

There is no ambivalence in this condemnation, no gentlemanly performance of modesty which so thoroughly permeated the writings of Maton, Rackett, and Burrow. He continued his attack by mocking the supposed absurdity of the Linnaean method, employing an analogy of the ornithologist arranging his cabinet using only the "feet and beaks" of various birds—absurd and laughable, in his view. The conchologist, by no stretch of the imagination, was deserving of the same ridicule. And yet Swainson's critique quickly intensified:

> *In some cases, even, as in the genus Limax, it is sufficient for the shell to be small, in order to set it wide asunder from animals of the same family: but it would be endless, as well as useless, to detail the violations of natural order*

[17] Ibid., 9.

> *manifested by Linnaeus in his systematic arrangement of the mollusks, which have so truly constituted his stumbling block.*[18]

Swainson did not exclusively dedicate his efforts in natural history to mollusks, his writings cut across sub-disciplinary boundaries within the whole of zoology. But mollusks offered him a unique opportunity to attack the Linnaean system available nowhere else in the *Systema Naturae*. The neglect of the living occupants of seashells as valuable (if not primary) sources of character traits for systematic subdivision reflected a broader Linnaean neglect of internal structures in favor of external features in the construction of his classificatory system. Conchology was thus a microcosm of the methodological errors found in the Linnaean arrangement of living things. The development of such problems in the early nineteenth century, from curious anomalies in Maton and Rackett to sites of damning condemnation in a figure as prominent as Swainson, evinces the gradual crumbling of strict adherence to Linnaean systematics in England. The new systematic order in England would cast aside traditional methods of Linnaean character derivation, and much of the arrangement of the *Systema Naturae* along with them.

Lacking the institutional authority and disciplinary credibility imbued increasingly by university positions on the European continent, Swainson resorted to alternative methods of discrediting conchology and Linnaeanism. In this effort, the steadily growing popularity of shell collection in England proved a convenient opportunity. In her 1843 *Popular Conchology*, Agnes Catlow expressed gratitude for “the instruction she has received from her friend Mr. Swainson...who first turned her thoughts to the study of

[18] Ibid., 10.

Conchology, which she trusts her humble efforts will have rendered interesting to those for whose information this volume was written."[19] Given his acerbic critique of the field of conchology just two years prior, it seems strange that Swainson would encourage Catlow's work. But, as the title indicates, *Popular Conchology* was expressly not intended for a learned audience. Rather it professed to be written for hobbyists:

> *Among the many works which have been published on Conchology, there is none which appears exactly to answer the purpose intended by the present volume. Most of them are addressed exclusively to men of science, and are encumbered (to them enriched) with a vast mass of details, the attempt to embrace which would only confuse and embarrass a beginner; while others, which profess to be elementary, are inapplicable to the purposes of exact arrangement.*[20]

Furthermore, Catlow grounded the relevance of her work without any reference to zoology or the systematics of living things. Instead, she justified its publication by stating that "[Conchology] is daily becoming more attractive and more important; not only from its intrinsic interest, but also from its multiplied relations with Geology."[21] This connection smacks of Swainson's influence, who conceded in his 1840 *Treatise* that "The study of shells appears, indeed, to be indispensably necessary to the geologist," whilst simultaneously mocking the absurdity of basing zoological analysis exclusively upon it. [22] Through Catlow's *Popular Conchology*, it seems clear that Swainson saw value in both renegotiating the field's disciplinary alignment away from the systematics of living things, and in diminishing its credibility in the eyes of wealthy naturalists by entrenching its associations with a hobbyistic natural history of the working class.

[19] Catlow, *Popular Conchology*, vii.
[20] Ibid., v-vi.
[21] Ibid., v.
[22] Swainson, *Treatise on Malacology*, 9.

Without recourse to the navigation of credibility networks within the far more professionalized institutions of natural history of France and Germany, such were the available resources Swainson could draw upon to advance his malacological, anti-Linnaean program.

Despite Swainson's efforts, the English conchology-malacology controversy was not fully settled by the 1840s. Lovell Reeve's 1841 *Conchologia Systematica* attempted to evade embroilment in the disciplinary dispute; though its title seemingly indicates an allegiance to conchology (and, accordingly, Linnaeanism), the work is resolutely malacological in method. Reeve acknowledged this directly in his preface.

> It is not my intention to add to the controversy on the true definition of this science, nor to acknowledge and distinction between the study of *Malacology* and that of *Conchology*: I conceive the natural history of the animal to be inseparable from that of the shell, and have adopted the latter term as the original and most suitable.[23]

This suitability was quite likely grounded in the very same motivations William Swainson felt in limiting the scope of his work to "testaceous *Mollusca*"; the term "conchology" was both more recognizable and more appealing to the larger literate population of England. Such strategic positioning certainly promised a larger readership, and greater revenue.

To assume Reeve's choice of title constituted a declaration of allegiance to the traditionally construed field of conchology and the Linnaean methods associated with it would be a mistake. Reeve cited Georges Cuvier and Henri Marie Ducrotay De Blainville as primary inspirations for his work, both of whom strove for employment of

[23] Reeve, *Conchologia Systematica*, v.

anatomical method and widespread uptake of the term “malacology.”[24] Indeed, he redefined conchology on the first page of the book in direct accordance with the anatomical principles of malacology: “By the term...we mean to imply the history and classification of the shell, in connexion with the physiology of its animal.” Though Reeve may have refused to engage in definitional disputes, his work is nonetheless one of malacology—it took for its systematic structure the arrangement of Lamarck and not Linnaeus, and it amends Lamarckian order on the grounds of anatomical, as well as external, characteristics.[25]

Like Swainson, Reeve used the field as a platform to discredit Linnaean systematics, stating of it (albeit, less acerbically) that “[The Linnaean] system...although it embodied all that was at that time known of the nature and habits of the animal, was based almost entirely upon the relative characters of the shell; and as new forms began to appear...it was found wholly inefficient.”[26] If the label “conchology” connoted a disciplinary alignment with Linnaean methods of classification in the early decades of the nineteenth century, as evinced by Maton, Rackett, Burrow, and Cuvier, by the mid-century such definitional mappings were less evident to contemporary authors. Anatomical methods of deriving the primary characteristics that structure systems of classification had, by the 1840s, become irrevocably entrenched in the study of shellfish, and provided an impulse for migration from Linnaean methods. Many conchological works were nonetheless works of malacology.

[24] Vinarski, “The Birth of Malacology.”
[25] Reeve, *Conchologia Systematica*., v, 1.
[26] Ibid., 2.

Systematic Realignments

The malacology-conchology controversy serves as a prism through which broader systematic debates are refracted; substantively, the use of comparative anatomy in the derivation of analytical characters, in opposition to traditional Linnaean emphasis on external features. In many ways, this contestation can be subsumed into a larger debate: a referendum on the legitimacy of the Linnaean systematic method. English conchologists at the onset of the nineteenth century were largely Linnaean: non-anatomically inclined systematists that privileged externally derived analytical characters. Malacologists were of a new movement within systematic natural history, eager to adopt the methods of comparative anatomy and often oriented in research towards the unveiling of a non-Linnaean natural system of living things.

Malacology's eventual supersession of conchology can be viewed as reflective—but not necessarily as the prime causal agent—of the decline of strict adherence to Linnaeanism in England. Peter F. Stevens notes that, while Lamarckian systematics more rapidly replaced Linnaeanism on the continent, English naturalists retained confidence in the methods of the "father of taxonomy" deeper into the nineteenth century.[27] Whatever the complex and multifaceted reasons for such conservatism were (the prominence of J.E. Smith and his Linnean Society of London no doubt played a role), the unique and contingent mapping of the phenotypic characteristics of mollusks onto that conceptual divide spurred debates which fundamentally questioned the reigning epistemic order of Linnaean systematics.

[27] Stevens, *The Development of Biological Systematics*, 63.

Robert Darnton, in a 2001 article published following conferences held jointly at Berkeley and Uppsala regarding “The Structure of Knowledge,”, coined the term “epistemological agnst” to describe the anxiety incurred by a subject or object that evades clear and discrete classification by the prevailing systems pertinent to its conceptual arrangement. Such sources of angst, Darnton argues, often provoke renegotiations of the classificatory systems that fail to assimilate them. Indeed, he submits this process as the primary way by which classification systems change over time. Eighteenth and nineteenth century natural history was no doubt ripe with such processes and, as I have shown, methodological heterodoxy within the systematics of shellfish abounded in Maton and Rackett’s presentation of the *Account* to the Linnean Society. Mollusk specimens that evaded the strict schema of the Linnaean system continually emerged as collection expeditions intensified in the late eighteenth century, causing in turn a profusion of genera within *Testacea* as naturalists grappled with their assimilation into the existing Linnaean scheme.

The Darntonian anxiety evinced by Maton and Rackett’s condemnation of any conchological work that introduced genera too liberally, or of those that felt their specimens necessarily challenged the order of the *Systema Naturae* at higher taxa—particularly families and classes—was not simply rooted in a distaste for individual or method. In those early conchologists’ struggle with the systematic implications of the edge cases in their collections, defenders of Linnaeanism foresaw the impending “ruin" the Reverend E.I. Burrows bemoaned in his 1815 *Elements*. And yet, despite the Linnaeans’ fears, a more robust and more enduring systematics of shellfish, buttressed by the methods of the new school of comparative anatomy, emerged from such ruin. In

the context of the conchology-malacology controversy in nineteenth century England, and in the practice of systematic natural history more broadly, I submit attempts to classify the mollusk as a prototype case of the "epistemological angst" Darnton describes.

In this vein, future historical attention afforded to systematic controversies ought to foreground the dialectical and recursive relationship between particulars—the genera, classes, and orders to which natural objects are seen to belong—and the necessary ramifications such placements portent for taxonomic theory at the highest levels. Read closely, apparent squabbles over classificatory minutiae can be seen to refract complex theoretical struggles in natural history, and can texture the networks and factions marshalling force on these epistemic battlegrounds. Disciplinary realignment in sciences of this kind is negotiated as much in dialogue with such processes as it is with nature itself.

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