A Response to McGee on Ferreiro, *Ships and Science*

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David McGee's critique of my book *Ships and Science: The Birth of Naval Architecture in the Scientific Revolution 1600–1800* [2006] begins with a flawed premise; and from that he derives a series of equally flawed analyses of the text, which along with several outright falsehoods, paints an altogether erroneous picture of my work. My purpose here is to spotlight the flaws in David's analysis and to correct the unfavorable light he casts upon the book.

David begins his review with a 'full disclosure' expressing surprise at the title of my book *Ships and Science* and noting the original use of the title in his unpublished thesis, which he had sent me seven years ago. It is necessary that I respond in kind. The publisher of my book correctly felt that my original title was rather unwieldy, so my editor and I kicked around a few ideas until we arrived at '*Ships and Science*'. I attempted, as a courtesy, to inform David, but his place of employment (Dibner Institute) was closing down, my emails bounced back, and no one could provide me his forwarding information. However, I must point out that in my book I acknowledge his many contributions and that I remain grateful to him for the assistance that he has provided over the years.

David's flawed premise is that 'naval architecture' has *nothing* to do with engineering and science, and he fabricates his entire critique from that point. He does not, however, define the term himself, but merely quotes a dictionary definition that it is 'the design of ships and the superintendence of their construction'. As I carefully explain in my preface, such a dictionary definition is far too expansive to allow any serious study of the subject, as it would involve all aspects of conceptualization, design, and fabrication, and would cover the range from log rafts to ocean liners. I further explain that the term 'naval architecture' quickly evolved from its first usage, which originally meant 'an architecture of the sea', to encompass elements of geometry, mathematics, engineering, and science. A cursory examination of

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any naval architecture text today would show that the term refers to the *prediction* of a ship's characteristics and performance *before it is built*, and it is the evolution of this capability that forms the thesis of my work. It is simply wrongheaded to assume, as David does, that constructors in the past had no interest in prediction but were simply content to sketch, draw, or lay out a ship without any forethought as to how it might behave once it was built.

David completely misrepresents my words when he claims that I 'admit that 18th-century theories relating to the behavior of ships were... rarely, if ever applied to actual ship design'. In fact, my entire book is specifically devoted to showing how ship theory *was* extensively applied to actual ship designs during that century in many different navies (in France, Spain, Sweden, Denmark, Venice, for example), and supplies numerous instances of the actual calculations performed by naval constructors during the design process. It is impossible to understand how David came to exactly the opposite conclusion.

David continues to fires damp squibs into my work by claiming that I include extraneous material on the mathematical and scientific concepts underlying the relevant theories of ship resistance, stability, and so forth; but later he contradicts himself by stating that 'too little attention is paid to the underlying concepts'. He then provides a series of essentially meaningless summaries of the different chapters in my book that willfully ignore the basic themes and simply state what *he* thinks the book should contain. The important point which he misses, and which I am afraid the readers of his review will also miss if they do not read other book reviews, is that each chapter provides not just a summary of the major developments but also the *context* in which they were developed. This was essential as I intend this book to be read by historians as well as by practicing naval architects. I quite deliberately sought at every turn to explain history to engineers and engineering to historians, without sacrificing accuracy or clarity in either case. I will, therefore, correct David's long series of mistakes and outright falsehoods by briefly describing the chapters.

The first chapter, 'Mere Carpenters', serves as an overall introduction, establishing the underlying thesis that naval architecture was developed and implemented in response to a bureaucratic need by naval administrations for greater control over their constructors,

rather than as a means of optimizing the engineering of ships. It then describes the changing naval and maritime situation in Europe and explains how it provided the catalyst for the development and acceptance of naval architecture as part of ship design.

The next three chapters describe, in roughly chronological order, the evolution of the major lines of research into the theory of ships. Chapter 2 shows how the theories of maneuvering and sailing were debated and evolved in the context of published journals and professional bodies such as the French Academy of Sciences; thus, the chapter begins with a description of these structures during the Scientific Revolution. Chapter 3, 'A Shock to the System', demonstrates how the evolution of the theory of ship resistance became a small but strategically vital part of the development of rational mechanics. I carefully explain how Newton's 'shock' theory of resistance evolved into the notion of streamlines and pressure, through changing mathematical analyses as well as experimentation. At the core of this research were the great names of Huygens, Euler, D'Alembert, and the Bernoullis, all of whom contributed immensely to the understanding of ship theory. The navies of the era— principally France— supported such research with the obvious goal of making their ships go faster, so I critically examine historical data using modern analysis to determine whether these theoretical efforts paid off in faster ships. (Plot spoiler: French ships *were* faster than British ships, but not due to their constructors' use of ship theory.) Chapter 4 is a detailed explanation of how stability theory came to be developed. Once again, it was necessary to put this development in context, by carefully explaining that actual 'stability accidents' such as that experienced by the Swedish warship *Vasa* were quite rare, and were *not* the impetus for examining the science of ship stability (by contrast, navigational accidents, very common at the time, *did* spawn major state-sponsored research and improvements in astronomy and navigation science). Thus, I carefully lay out how stability theory evolved in discrete, comprehensible steps starting with Archimedes; and illustrate how the final synthesis occurred as a 'multiple' (Robert Merton's term) of three men working exactly simultaneously, but completely separately, to arrive at nearly identical solutions.

The final two chapters tie the work together. Chapter 5 describes how the elements of naval architecture were assembled into the great works of synthesis that laid the foundations of the profes-

sion and became the touchstones for further work. Chapter 6 explains how the development of ship theory occurred hand in hand with the growing professionalization of ship constructors (including the first engineering schools, professional corps, and learned societies), and describes how improved knowledge of ship construction quickly passed from one country to another through an almost continuous exchange of people and technologies. The chapter winds down by setting the stage for the passage of naval architecture from the age of wood and sail to the dawning age of iron and steam.

David wraps up his review with yet another series of misunderstandings and outright fabrications. He clearly does not understand stability theory, stating that ship stability is due to 'the movements of the centers of gravity and center of buoyancy', when in fact any basic text on the subject will show that it involves factors such as the distribution of waterplane area. He wrongly claims that 'naval science... would not even begin to be applied in a meaningful way until 1870', even though I provide specific examples of the use of resistance theory in the works of Robert Fulton, Isambard Kingdom Brunel, and John Scott Russell, dating from as early as 1809. Finally, he continues to make the discredited positivist assumption that ship theory *must* have been developed in order to solve problems with ship designs, when in fact (as stated above) these theories, e.g., ship stability, were *not* developed to solve otherwise insurmountable problems, but primarily in response to a bureaucratic need by naval administrations to gain greater control over the processes of designing and building ships.

David McGee's review of *Ships and Science*, in summary, is distorted, riddled with falsehoods, and completely misrepresents my work to the readers of *Aestimatio*. I encourage those readers to view the many other critiques available in professional publications.[1]

[1] For example, the American Library Association's *Choice: Current Reviews for Academic Libraries* [June 2007 Vol. 44 no. 10] rates the book as 'Highly recommended'. The influential maritime history journal *Mariner's Mirror* [August 2007 Vol. 93 no. 1] says 'This is a superb volume... to be regarded in coming years as [a] starting point for the study of applied science and engineering'. The Society of Naval Architects and Marine Engineer's *Marine Technology* [July 2007, Vol. 44 no. 3] says 'This volume should be required reading for all students of naval architecture'. Updated citations from reviews may be found on the book's website, or the MIT Press website.

BIBLIOGRAPHY

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