The Enigma of the Aerofoil

Rival Theories in Aerodynamics, 1909–1930

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Introduction: The Question to Be Answered

'Tis evident, that all the sciences have a relation, greater or less, to human nature; and that however wide any of them may seem to run from it, they still return back by one passage or another.

DAVID HUME, *A Treatise of Human Nature* (1739–40)[1]

Why do aircraft fly? How do the wings support the weight of the machine and its occupants? Even the most jaded passengers in the overcrowded airliners of the present day may experience some moments of wonder—or doubt—as the machine that is to transport them lifts itself off the runway. Because the action of the air on the wing cannot be seen, it is not easy to form an idea of what is happening. Some physical processes are at work that must generate powerful forces, but the nature of these processes, and the laws they obey, are not open to casual inspection. If the passengers looking out of the window really want an explanation of how a wing works, they must do what any lay person has to do and ask the experts. Unfortunately the answers that the experts will give are likely to be highly technical. It will take patience by both parties if communication is not to break down. But given goodwill on both sides, the experts should be able to find some simplified formulations that will be useful to the nonexperts, and the nonexperts should be able to deepen their grasp of the problem.

In this book I discuss the question of why airplanes fly, but I approach the problem in a slightly unusual way. I describe the history behind the technical answer to the question about the cause of "lift," that is, the lifting force on the wing. I analyze the path by which the experts, after much disagreement, arrived at the account they would now give. I am therefore not simply asserting that airplanes fly for this or that reason; I am asserting that they were *understood* to fly for this or that reason. I am interested in the fact that different and rival understandings were developed by different persons and in different places. I cannot speak as a professional in the field of aerodynamics; nor is my position exactly that of a layperson. I speak as a historian and sociologist of science who is poised between these categories.[2]

What are the specific questions that I am addressing and to which I hope to offer convincing answers? To identify them I first need to give some background. The practical problem of building machines that can be flown, that is, the problem of "mechanical" or "artificial" flight, was solved in the final years of the nineteenth century and the early years of the twentieth century. In the 1890s Otto Lilienthal in Germany successfully built and flew what we today would call hang gliders. From 1903 to 1905 the Wright brothers in the United States showed that sustained and controlled powered flight was possible and practical. What had long been called the "secret" of flight was now no longer a secret. But not all of the secret was revealed. Some parts of it remained hidden, and indeed, some parts are still hidden today. The practical successes of the pioneer aviators still left unanswered the question of how a wing generated the lift forces that were necessary for flight. The pioneers mostly worked by trial and error. Some had experimented with models and taken measurements of lift and drag (the air resistance opposing the motion), but the measurements were sparse and unreliable.[3] No deeper theoretical understanding had prompted or significantly informed the early successes of the pioneers, nor had theory kept pace with the growth of practical understanding. The action of the air on the wing remained an enigma.

A division of labor quickly established itself. Practical constructors continued with their trial-and-error methods, while scientists and engineers began to study the nature of the airflow and the relation between the flow and the forces that it would generate. For this purpose the scientists and engineers did not just perform experiments and build the requisite pieces of apparatus, such as wind channels. They also exploited the resources of a branch of applied mathematics that was usually called hydrodynamics. The name "hydrodynamics" makes it sound as if the theory was confined to the flow of water, but in reality it was a mathematical description that, with varying degrees of approximation, was applied to "fluids" in general, including air. Thus was born the new science of aerodynamics. The birth was accompanied by much travail. One problem was that the mathematical theory of fluid flow was immensely difficult. The need to work with this theory effectively excluded the participation of all but the most mathematically sophisticated persons, and this did not go down well with the practical constructors. The mathematical analysis also depended for its starting point on a range of assumptions and hypotheses, about both the nature of the air and the more or less invisible pattern of the flow of air over, under, and around the wing. Only when the flow was known and specified could the forces on the wing be calculated. Assumptions had to be made. The unavoidable need to base their investigations on a set of assumptions proved to be deeply divisive. Different groups

of experts adopted different assumptions and, for reasons I explain, stuck to them.

The first part of this historical story, the practical achievement of controlled flight, has been extensively discussed by historians. Pioneers, such as the Wright brothers, have been well served, and the attention given to them is both proper and understandable.[4] The second part of the history, the development of the science of aerodynamics, is somewhat less developed as a historical theme, though a number of outstanding works have been written and published on the subject in recent years.[5] The present book is a contribution to this developing field in the history of science and technology.

In the early years of aviation there were two, rival theories that were intended to explain the origin and nature of the lift of a wing. They may be called, respectively, the discontinuity theory and the circulatory (or vortex) theory. The names derive from the particular character of the postulated flow of air around the wing. (I should mention that the circulatory theory is, in effect, the one that is accepted today.) My aim is to give a detailed account of how the advocates of the two theories developed their ideas and how they oriented themselves to, and engaged with, the empirical facts about flight. To do this I found that I also needed to understand how they oriented themselves to, and engaged with, one another. I show that these two dimensions cannot be kept separate. This is why I have prefaced the work with the quotation from the famous Edinburgh historian and sociologist David Hume. The more one studies the technical details of the scientific work, the more evident it becomes that the social dimension of the activity is deeply implicated in these details. The more closely one analyses the technical reasoning, the more evident it becomes that the force of reason is a social force. The historical story that I have to tell about the emerging understanding of lift is, therefore, at one and the same time both a scientific and a sociological story. To understand the course taken by the science it is necessary to understand the role played by the social context, and to appreciate the role played by the social context it is necessary to deconstruct the technical and mathematical arguments.

In principle none of this should occasion surprise. Scientists and engineers do not operate as independent agents but as members of a group. They cannot achieve their status as scientists and engineers without being educated, and education is the transmission of a body of culture through the exercise of authority. Education is socialization.[6] Scientists and engineers see themselves as contributing to a certain discipline, as being members of certain institutions, as having loyalties to this laboratory or that tradition, as being students of A or rivals of B. Their activities would be impossible unless behavior were coordinated and concerted. For this the individuals concerned must be responsive to one another and in constant interaction. Their knowledge is necessarily shared knowledge, though, in its overall effects, the process of sharing can be divisive as well as unifying. The sharing is always what Hume would call a "confined" sharing.

All too frequently, when scientific and technical achievements become objects of commentary, analysis, or celebration, these simple truths are obscured. Academic culture is saturated with individualistic prejudices, which encourage us to trivialize the implications of the truth that science is a collective enterprise and that knowledge is a collective accomplishment. Philosophers of science actively encourage historians to distinguish between, on the one side, "cognitive," "epistemic," or "rational" factors and, on the other side, "social" factors. They enjoin the sociologist to "disentangle" scientific reasoning from "social influences" and to distinguish what is truly "internal" to science from what is truly "external."[7] These recommendations are treated as if they were preconditions of mental hygiene and based on self-evident truths. Historians and sociologists of science know better. They know that the problem of cognitive order *is* the problem of social order.[8] These are not two things, even two things that are closely connected; they are one thing described from different points of view. The division of a historical narrative into "the cognitive" and "the social," or "the rational" and "the social," is wholly artificial. It is methodologically lazy and epistemologically naïve.

I shall now briefly sketch the overall structure of the events I describe in this volume. Of the two theories of lift that I mentioned, one of them, the discontinuity theory, was mainly developed in Britain. It was based on work by the eminent mathematical physicist Lord Rayleigh. The other, the circulatory theory, was mainly developed in Germany. It is associated primarily with the German engineer Ludwig Prandtl, although it had originally been proposed by the English engineer Frederick Lanchester. It rapidly became clear that the discontinuity theory was badly flawed because it only predicted about half of the observed amount of lift. At this point, shortly before the outbreak of World War I (or what the British call the Great War) in 1914, the British awareness of failure might have reasonably led them to turn their attention to the other theory, the theory of circulation. They did not do this. They knew about the theory but they dismissed it. At Cambridge, G. I. Taylor, for example, treated the discontinuity theory as a mathematical curiosity, but he also found Lanchester's theory of circulation equally unacceptable. The reasons he gave to support this judgment were important and widely shared. Meanwhile the Germans embraced the idea of circulation and developed it in mathematical detail. The British also knew of this German reaction but still did not take the theory of circulation seriously. It was not until after the war ended in 1918 that the British began to take note. They found that the Germans had developed a mathematically expressed, empirically supported, and practically useful account of lift. Even then the British had serious reservations. The negative response had nothing to do with mere anti-German feeling. The British scientific experts were patriots, but, unlike some in the world of aviation, they were not bigots. Why then were they so reluctant to take the theory of circulation seriously? This is the main question addressed in the book.[9]

There are already candidate answers to this question in the literature, but they are answers of a different kind to the one I offer. The neglect of Lanchester's work became something of a scandal in the 1920s and 1930s, so it was natural that explanations and justifications were manufactured to account for it. Sir Richard Glazebrook, the head of the National Physical Laboratory, played an important role in British aviation during these years and was the source of one of the standard excuses, namely, that Lanchester did not present his ideas with sufficient mathematical clarity. Well into the midcentury, British experts in aerodynamics, who, along with Glazebrook, shared responsibility for the neglect of Lanchester's ideas, were scratching their heads and wondering how they could have allowed themselves to get into this position. Clarity or no clarity, they had turned their backs on the right theory of lift and had become bogged down with the wrong one.

The retrospective accounts and excuses that have been given have been both fragmentary and feeble, though Lanchester's biographer, P. W. Kingsford, writing in 1960, still went along with a version of Glazebrook's excuse.[10] Other existing accounts merely tend to embellish the basic excuse by invoking the personal idiosyncrasies of the leading actors. The problem is analyzed as a clash of personalities. It is true that some of those involved had strong characters as well as powerful intellects, and some of them could pass as colorful personalities. All this will become apparent in what follows. The psychology of those involved is clearly an integral part of the historical story, but such accounts miss the very thing that I want to emphasize and that I believe is essential for a proper analysis, namely, the interconnection of the sociological and technical dimensions. Only an account that is technically informed, and sensitive to the social processes built into the technical content of the aerodynamic work, will make sense of the history. I want to show that the real reasons for the resistance to the vortex or circulatory theory of lift were deep and interesting, but not really embarrassing at all.

Although I have posed the question of why the British resisted the theory of circulation, I do not believe it can be answered in isolation from the question of why the Germans embraced it. Both reactions should be seen as equally problematic. The historical record shows that the same type of causes were at work in both British and German aerodynamics. In both cases the actors drew on the resources of their local culture and elaborated them in ways that were typical of their milieu and were encouraged by the institutions of which they were active members. Of course, the cultures and the institutions were subtly different. My explanation of the German behavior is thus of the same kind as my explanation of the British. The same variables are involved, but the variables have different values. Seen in this way the explanation possesses a methodological characteristic that has been dubbed "symmetry." Because the point continues to be misunderstood, I should perhaps emphasize the words "same kind." I am not saying that the very same causes were at work but that the same *kinds* of cause were in operation. Symmetry, in this sense, is now widely (though not universally) accepted as a methodological virtue in much historical and sociological work. Conversely, it is widely rejected as an error, or treated as a triviality, by philosophers. I hope that seeing the symmetry principle in operation will help convey its meaning more effectively than merely trying to capture it in verbal formulas or justify it by abstract argument.

The overall plan of the book is as follows. In chapter 1 I start my account of the early British work in aerodynamics with the foundation of the controversial Advisory Committee for Aeronautics in 1909. The committee was presided over by Rayleigh. The frontispiece, taken from the *Daily Graphic* of May 13, 1909, shows some of the leading members of the committee striding purposefully into the War Office for their first meeting, and then emerging afterward looking somewhat more relaxed. The minutes of that important meeting are in the Public Record Office and reveal what they talked about in the interval between those two pictures.[11] It is a matter of central concern throughout this book. Chapter 2 lays the foundation for understanding the two competing theories of lift by sketching the basic ideas of hydrodynamics and the idealized, mathematical apparatus that was used to describe the flow of air. A nontechnical summary is provided at the end of the chapter. In chapter 3, I introduce the discontinuity theory of lift and describe the British research program on lift and the frustrations that were encountered. Chapter 4 is devoted to the circulatory or vortex theory and describes the hostile reception accorded to Lanchester among British experts. I pay particular attention to the reasons that were advanced to justify the rejection. In chapter 5, I identify and contrast two different intellectual traditions that were brought to bear on the theory of lift. One of them was grounded in the mathematical physics cultivated in Britain and preeminently represented by the graduates of the Cambridge Mathematical Tripos. The other tradition, called *technische Mechanik*, or "technical mechanics," was developed in the German technical colleges and was integral to Prandtl's work on wing theory. Chapters 6 and 7 provide an account of the German development and extension of the circulation theory as worked out in Munich, Göttingen, Berlin, and Aachen. In chapters 8 and 9 there is a description of the British postwar response, which took the form of a period of intense experimentation; it also gave rise to some remarkable and revealing theoretical confrontations. What, exactly, did the experiments prove? The British did not find it easy to agree on the answer.

The divergence between British and German approaches was effectively ended in 1926 with the publication, by Cambridge University Press, of a textbook that became a classic statement of the circulation theory. The book was Hermann Glauert's *The Elements of Aerofoil and Airscrew Theory*.[12] Glauert, an Englishman of German extraction, was a brilliant Cambridge mathematician who, in the 1920s, broke ranks and became a determined advocate of the circulation theory. As the title of Glauert's book indicates, he did not just work on the theory of the aircraft wing, but he also addressed the theory of the propeller. This is a natural generalization. The cross section of a propeller has the form of an aerofoil, and a propeller can be thought of as a rapidly rotating wing. The "lift" of this "wing" becomes the thrust of the propeller, which overcomes the air resistance, or "drag," as the aircraft moves through the air. Glauert's book also dealt with the theory of the flow of air in the wind channel itself, that is, the device used to test both wings and propellers. This aspect of the overall theory was needed to ensure that aerodynamic experiments and tests were correctly interpreted. As always in science, experiments are made to test theories, but theories are needed to understand the experiments.[13] The discussions of propellers and wind channels in Glauert's book are important and deserve further historical study, but, on grounds of practicality, I set aside both the aerodynamics of the propeller and the methodology of wind-channel tests in order to concentrate exclusively on the story of the wing itself.[14]

In the final chapter, chapter 10, I survey the course of the argument and consider objections to my analysis, particularly those that are bound to arise from its sociological character. I use the case study to challenge some of the negative and inaccurate stereotypes that still surround the sociology of scientific and technological knowledge. I also ask what lessons can be drawn from this episode in the history of aerodynamics. Does it carry a pessimistic message about British academic traditions and elitism? What does it tell us about the difference between Göttingen and Cambridge or between engineers and physicists? Finally, I ask what light the history of aerodynamics casts on the fraught arguments between historians, philosophers, and sociologists of science concerning relativism.[15] Does the success of aviation show that relativism must be false? I believe that, by drawing on this case study, some clear answers can be given to these questions, and they are the opposite of what may be expected.

During the writing of this book I had the great advantage of being able to make use of Andrew Warwick's *Masters of Theory: Cambridge and the Rise of Mathematical Physics.*[16] Although historians of British science had previously accorded significance to the tradition of intense mathematical training that was characteristic of late Victorian and Edwardian Cambridge, Warwick took this argument to a new level. By adopting a fresh standpoint he compellingly demonstrated the constitutive and positive role played by this pedagogic tradition in electromagnetic theory and the fundamental physics of the ether in the early 1900s.[17]

For me, one of the intriguing things about Warwick's book is that the actors in his story are, in a number of cases, also the actors in my story. What is more, his account of the resistance that some Cambridge mathematicians displayed to Einstein's work runs in parallel with my story of the resistance to Prandtl's work. Like Warwick I found that their mathematical training could exert a significant hold over the minds of Cambridge experts as they formulated their research problems. In many ways the study that I present here can be seen as corroborating the picture developed in Warwick's book. Of course, shifting the area of investigation from the history of electromagnetism to the history of fluid mechanics throws up differences between the two studies, and not surprisingly there is some divergence in our conclusions. Whereas Warwick's attention is mainly (though not exclusively) devoted to the British scene, my aim, from the outset, is that of comparing the British and German approaches to aerodynamics. Furthermore, on the British side, I follow the actors in my story as they move out of the cloisters of their Cambridge colleges into a wider world of politics, economics, aviation technology, and war. If Warwick studied Cambridge mathematicians as masters of theory, I ask how they acquitted themselves as servants of practice.

1

Mathematicians versus Practical Men: The Founding of the Advisory Committee for Aeronautics

In the meantime every aeroplane is to be regarded as a collection of unsolved mathematical problems; and it would have been quite easy for these problems to have been solved years ago, before the first aeroplane flew.

G. H. BRYAN, "*Researches in Aeronautical Mathematics*" (1916)[1]

The successful aeroplane, like many other pieces of mechanism, is a huge mass of compromise.

HOWARD T. WRIGHT, "*Aeroplanes from an Engineer's Point of View*" (1912)[2]

The Advisory Committee for Aeronautics (the ACA) was founded in 1909. This Whitehall committee provided the scientific expertise that guided British research in aeronautics in the crucial years up to, and during, the Great War of 1914–18. From the outset the ACA was, and was intended to be, the brains in the body of British aeronautics.[3] It offered to the emerging field of aviation the expertise of some of the country's leading scientists and engineers. In 1919 it was renamed the Aeronautical Research Committee, and in this form the committee, and its successors, continued to perform its guiding role for many years. After 1909 the institutional structure of aeronautical research in Britain soon came to command respect abroad. When the United States government began to organize its own national research effort in aviation in 1915, it used the Advisory Committee as its model.[4] The resulting American National Advisory Committee for Aeronautics, the NACA, was later turned into NASA, the National Aeronautics and Space Administration. The British structure, however, was abolished by the Thatcher administration in 1980, some seventy years after its inception.[5]

If the Advisory Committee for Aeronautics was meant to offer the best, there were some in Britain, especially in the early years, who argued that, in fact, it gave the worst. For these critics the ACA held back the field of British aeronautics and encouraged the wrong tendencies. The reason for these strongly divergent opinions was that aviation in general, and aeronautical science in particular, fell across some of the many cultural fault-lines running through British society. These fault lines were capable of unleashing powerful in virtue of the practice of using the currency). The meaning and implications of the rule only exist through being invoked by the actors to correct, challenge, justify, and explain the rule to one another in the course of their interactions. This is what Wittgenstein meant by calling a rule an "institution." The implication (though these are not Wittgenstein's words) is that the rule, that is, the cognitive factor, *is actually itself a social factor.* Those who appeal to a combination of cognitive factors and social factors, as if they are two, qualitatively different kinds of things, are not being prudent; they are being muddled or metaphysical.[13]

The processes that Wittgenstein brilliantly distilled into his example are the same ones that occurred on a larger scale in my case study. That which is recognizably social, for example, the disciplinary identities, the institutional locations, the cultural traditions, the schools of thought, are not "external" to the reasoning processes that I have studied but are integral to them. They are constitutive of the step-by-step judgments by which the different bodies of knowledge were built up. Experts gave reasons to explain and justify their views and found that sometimes they were accepted and sometimes rejected. Facts and reasons that inclined the members of one group to orient in one direction inclined the members of the other group to orient in a different direction. As one would expect from Wittgenstein's example, these acceptances, rejections, indications, and orientations fell into patterns. The patterns form the customs, conventions, institutions and subcultures described in my story.[14]

Subcultures and Status

One of the subcultures I identify in my explanation (German technical mechanics) belongs to the general field of technology, while the other (British mathematical physics) falls more comfortably under the rubric of science. My explanation therefore presupposes a society in which technological and scientific activity are understood to be different from one another. The picture is of culture with a division of labor in which the roles of technologist and scientist are treated as distinct or distinguishable. These labels are the categories employed by the historical actors themselves. Their role in my analysis derives from their prior status as actors' categories.[15] Although the members of the two subcultures interact with one another, my data justify attributing a significant degree of independence to them. To speak of "subcultures" carries the implication that the practitioners within each respective subculture routinely draw upon the resources of their own traditions as they perform their work and confront new problems.[16] A symmetrical stance requires that

both science and technology be placed on a par with one another for the purposes of analysis. This injunction is directed at the analyst and is consistent with the historical actors themselves according a very different status to the two activities: for example, some of the actors may see science as having a higher status than technology. The point of the methodological injunction to be "symmetrical" is that it requires the analyst to ask why status is distributed in this particular way by the members of a group and to keep in mind that it could be distributed differently.[17]

Attributions of status can be expressed in subtle ways. They may take the form of assumptions (made by both actors and analysts) about the dependence of one body of knowledge on another. Is technology to be seen as the (mere) working out of the implications of science? Is the driving force of technological innovation typically, or always, some prior scientific innovation?[18] An inferior status may be indicated by an alleged epistemological dependence and a reluctance to impute agency and spontaneity to technology. The symmetry postulate does not assert the truth or falsity of any specific thesis about dependency or independence, but it does require that such a thesis is not introduced into the analysis as an a priori assumption. At most the dependency of technology on science is merely one possible state of affairs among many other possibilities; for example, science may depend on technology rather than technology on science, or the two may be completely fused together or completely separate. The actual relation is to be established empirically for each episode under study. In the case of the theory of lift it is clear that the technologically important ideas worked out by Lanchester, Kutta, and Prandtl were not the result of new scientific developments. On the contrary, they exploited an old science and old results, namely, ideal fluid theory, the Euler equations of inviscid flow, and the Biot-Savart law. The shock engendered after the Great War by the belated British recognition of the success of this approach was not the shock of the new but the shock of the old.[19] The science that was exploited was not only old; it was also discredited science—discredited, that is, in the eyes of Cambridge mathematical physicists pushing at the research front of viscous and turbulent flow.

The advocates of the circulatory theory of lift brought together the apparently useless results of classical hydrodynamics and the concrete problems posed by the new technology of mechanical flight. The theory of lift in conjunction with the theory of stability constituted the new science of aeronautics. Given the way that scientific knowledge was harnessed to technological concerns, the new discipline might be called a technoscience. Some commentators have argued that "technoscience," the fusing of science and technology, is a recent, indeed a "postmodern," phenomenon, exemplified by the allegedly novel patterns of development shown in information technology and computer science. Others have argued that, because the division of labor between science and technology is a relatively recent development, so their fusion into "technoscience" is, in fact, a return to the original condition of science.[20] Did not science, in its early modern form, derive from a fusion of the work of the scholar and the craftsman?[21] Whether or not this account of the origins of science is true, identifying early twentieth-century aerodynamics as an instance of technoscience would support the thesis that technoscience is not a novelty.

In the 1930s Hyman Levy, who had earlier coauthored *Aeronautics in Theory and Experiment*, wrote a number of books of popular science. Along with Bernal, Blackett, J. B. S. Haldane, Hogben, and Zuckerman, Levy belonged to a remarkable group of scientists who played a significant role in British cultural and political life during the interwar years.[22] One of Levy's books was titled *Modern Science: A Study of Physical Science in the World Today*.[23] Aerodynamics was one of his main examples. He did not call it a technoscience, but he did offer it as an exemplary case of the unity of theory and practice. Writing from a Marxist standpoint, he cited the work of Prandtl and von Kármán and offered the strange transitions from laminar to turbulent flow as evidence that nature embodied the laws of dialectics. While it is plausible to see the later developments of aerodynamics as moving toward a unification of theory and practice, the fact remains that in the early years there was a discernable difference between the stance of the mathematical physicists and the engineers. The history of Levy's contributions, and his own earlier, negative stance toward the circulation theory, underlines this point. When Levy was active in the field and working at the National Physical Laboratory, there was still a significant difference in approach between mathematical physicists and technologists—at least, between British mathematical physicists and German technologists.[24] It is clear that behind the emerging "synthesis" of theory and practice, there still lay the "thesis" of mathematical physics and the "antithesis" of engineering. Historical contingency rather than historical necessity determined the balance between them. I now look at one such contingency.

A Counterfactual Committee

If Haldane had not followed the advice of a Trinity mathematician when he set up the Advisory Committee for Aeronautics but had, say, recruited Cambridge engineers rather than mathematical physicists, he might have got a very different committee. In principle he could have done this because Cambridge had a distinguished school of engineering.[25] Predictably, there had always

been a tension between the demands of a practical engineering education and the demands of the traditional, Cambridge mathematical curriculum. There was not time in the day to succeed at both, except for the outstanding few. It was not until 1906 that a satisfactory accommodation was reached when Bertram Hopkinson, the professor of mechanism and applied mechanics, gave a viable and independent structure to the Mechanical Sciences Tripos. This took the engineers out of the competitive hothouse, though inevitably it meant they operated at a somewhat less sophisticated mathematical level. The products of Hopkinson's department played a distinguished role in the development of British aeronautics. Busk (who made the BE2 stable), Farren (who championed full-scale research at Farnborough), Melvill Jones (who worked on low-speed control and gunnery), Southwell (who worked on airship structures), and McKinnon Wood (the experimentalist who went with Glauert to see Prandtl) were all products of the Cambridge school of engineering. Hopkinson himself, though of an older generation, learned to fly during the Great War and did important work on aircraft testing at Martlesham Heath. He met his death at the controls of a Bristol Fighter in a flying accident in August 1918.[26]

Perhaps a counterfactual Advisory Committee, made up of men like Bertram Hopkinson, would have embraced Lanchester and the circulatory theory. This is consistent with my analysis, although the conclusion would only follow if Cambridge engineers were significantly different in their judgments and orientation from their Mathematical Tripos colleagues and significantly similar to the German engineers from Göttingen and Aachen. Such a premise is plausible but it cannot be taken for granted, and there is some evidence that calls it into question. In certain respects Cambridge engineers adopted attitudes that were similar to those of the more traditionally trained mathematicians. This is not surprising in the case of the older products of the Cambridge school of engineering because they too were steeped in the earlier Mathematical Tripos tradition. Bertram Hopkinson's father, John Hopkinson, had been both an engineer and a senior wrangler. After a fellowship at Trinity he become a professional, consulting engineer in London but retained a strongly mathematical bent. He developed a mathematical analysis of the alternating current generator and predicted that it should be possible to run such generators in parallel. Practical engineers knew that, given the available machines, they could not be run in parallel, but none of this blunted the confidence of the mathematically able Hopkinson.[27] Bertram Hopkinson himself had also read for the Mathematical Tripos and had been a highly placed wrangler. Although more practically inclined than his father, he had worked on topics in hydrodynamics and had written a paper on the theory of discontinuous flow.