ONE HUNDRED YEARS OF G. H. BRYAN'S *STABILITY IN AVIATION*

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SUMMARY

Few books in mathematics or the physical sciences pass the endurance test of being remembered in the literature, a hundred years on. George Hartley Bryan's classic *Stability in Aviation* is one of these. This paper traces the roots of Bryan's early research on stability and his interests in thermodynamics and statistical physics, which brought him into contact and collaboration with Ludwig Boltzmann. It is argued that Boltzmann's enthusiasm for Otto Lilienthal's experiments with gliders awakened Bryan's interest in flight and that it was Lilienthal's fatal accident in 1896 that convinced Bryan that aviation would only be feasible once the stability of aircraft was understood. Some of the events that conspired to delay the publication of *Stability in Aviation* until 1911 are chronicled, along with a redress of the largely unremembered contribution to the book made by Edgar Henry Harper.

1. INTRODUCTION

Stability in Aviation [1] written by George Hartley Bryan and published in 1911 is one of that small category of books that continue to be widely referenced, if rarely read, a hundred years on. Indeed it may never have enjoyed wide readership, with its pages of algebra enlightened only occasionally by physical insights. There was just one edition and no translations into other languages. In contrast, Frederick William Lanchester's two-volume *Aerial Flight* [2], first published in 1907-8, went through several editions with the second volume translated into German and French. The publication of *Stability in Aviation* came almost a decade after Bryan's first foray into the stability of flight in a paper written with his student William Ellis Williams and published [3] coincidentally a few weeks after the Wright brothers had flown their *Flyer* for the first time near Kitty Hawk, North Carolina on December 17, 1903. Bryan and Williams set out to show *that the longitudinal stability of aeroplane systems can be made the subject of mathematical calculation and to draw the attention of those interested in the problem of artificial flight to the necessity of acquiring further experimental knowledge concerning the quantities on which this stability is shown to depend.* Seemingly, few were interested. In the words of his obituarist, Selig Brodetsky: *This mathematical theory gained the approval neither of mathematicians nor of those brave pioneers, who demonstrated the practicability of aeroplane flight…..But Bryan persevered in his somewhat lonely work and seven years later he published his Stability in Aviation, a book that may now be reckoned as a classic in aeroplane theory,* [4]. Brodetsky's verdict has echoed down the years, the book praised in turn as *epochal,* [5], one that *completely changed the theoretical outlook,* [6] and *astonishingly modern*, [7]. Continuing references to *Stability in Aviation* stand testament to Bryan's niche in the pantheon of pioneers of flight.

Less well known is Bryan's world standing in thermodynamics and statistical mechanics in the years leading up to the publication of *Stability in Aviation* and the contrasting low esteem in which he was held at Bangor, where he served for thirty years as Professor of Mathematics in the University College of North Wales. When he retired in 1926 to Bordighera in Italy, no formal tributes were paid and shortly after his death all the Principal, Sir Harry Reichel, could manage was a graceless aside, "*Yes, we made a mistake there!"* Sixty years on, he was chronicled as having been *largely remembered because of a multitude of absurd episodes….if he sometimes seemed a colossal buffoon, he himself did not help matters by proclaiming that he did his best work under the influence of alcohol,* [8].

This outline of the vicissitudes of Bryan's career, up to the publication of *Stability in Aviation* in 1911, offers fresh insights on the man and those who influenced him, none more than Ludwig Boltzmann, heir to Maxwell and doyen of European theoretical physics at the end of the nineteenth century. The friendship that began at their first meeting in 1894 lasted until Boltzmann's tragic suicide in 1906. Not only did Boltzmann's influence promote Bryan's surge to the forefront of thermodynamics and statistical physics, his enthusiasm for Lilienthal's experiments with gliders made a lasting impression on Bryan. It was the insistent tug of thermodynamics and the stress of his early years at Bangor that combined to thwart Bryan's first campaign on the new science of flight. Even after the publication of his work on longitudinal stability with Williams, another five years went by before he returned to the subject. It took the publication of his rival Lanchester's results on stability to galvanise him into action. After the publication of *Stability in Aviation,* others, notably Leonard Bairstow, would play a pivotal role in making Bryan's results accessible to aeroplane design engineers, but as for *Stability in Aviation* itself, it was none of these but a name now unremembered, Edgar Harper, who contributed most. A hundred years on, it is more than time to redress the oversight of Harper's contributions to *Stability in Aviation*.

2. BEGINNINGS AND FIRST STEPS IN STABILITY

Bryan's obituarists and biographers alike agree on the uniqueness and durability of his contributions to aeroplane stability and the eccentricity that marked him as a man. It was a trait that owed a good deal to his early life. George Hartley Bryan was born at Cambridge on March 1, 1864. His father died young and Bryan, an only child, was brought up by his mother and grandmother. Much of his childhood was spent abroad, for the most part in Italy, with the result that he grew up fluent in Italian and proficient in both German and French. Supposedly delicate he did not attend school, a drawback that may have contributed to his at times awkward relations with colleagues in later life. Even when admitted to Cambridge in 1883 as a Scholar of Peterhouse, he continued to live at home. Indulged by his mother and grandmother alike, it is small wonder Bryan grew up expecting to have his own way. He graduated in 1886, fifth in the list of Wranglers and was subsequently named as Smith's Prizeman for a dissertation on the stability of a rotating mass of fluid. It was a topic proposed by G.H. Darwin, Bryan's early mentor and Darwin's second son, one that had engaged no less than Poincaré and Riemann, as good company as one could keep in late nineteenth-century mathematics. The seeds of his interest in stability, sown by Darwin, would flower from time to time, not infrequently with some practical application in mind, and were to come to fruition in the work with which his name is ever associated, the stability of flight.

Elected to a fellowship at Peterhouse in 1889, it soon became clear that Bryan had interests beyond stability. We may never be sure what, or who, triggered Bryan's interest in thermodynamics and statistical mechanics, but Joseph Larmor seems as likely as any. Larmor was prominent in the *Maxwellians*, a group dedicated to advancing the brilliant insights in James Clerk Maxwell's books, *Theory of Heat* and *Electricity and Magnetism.* It appears that Bryan had the idea of reviewing thermodynamics in a report to the British Association, [9]. It would have been tempting fate for a mathematician in his mid-twenties to review a subject to which he himself had made no contribution, before an audience with Kelvin, its very godfather, present along with George Francis Fitzgerald, Professor of Natural Philosophy at Trinity College Dublin, two of the most formidable protagonists in the Association's Section A. On top of his eccentricity, Bryan already had something of a reputation as a loose cannon, his papers punctuated with critical comment not calculated to endear him to its targets. It may be that the British Association saw fit to temper any excesses by adding Larmor as co-author. Their report, *On the present State of our Knowledge of Thermodynamics* [10], presented at Cardiff in 1891 was well received and it was resolved that the work be continued. So without having contributed anything original in thermodynamics, Bryan's name had wider currency abroad.

3. BRYAN AND BOLTZMANN: FROM THERMODYNAMICS TO AERODYNAMICS

Part II did not appear for another three years and was presented to the British Association Meeting at Oxford in 1894, with Bryan as sole author. This report, *On the Laws of Distribution of Energy and their Limitations*, [11], was sharply focused in that he set out to address the question: *What precisely is Maxwell's law of equipartition and under what conditions is it true?* It was Bryan's good fortune that the guest of honour that year was none other than Ludwig Boltzmann, co-founder with Maxwell of the kinetic theory of gases. Boltzmann is recorded as having eagerly taken part in the discussion of Bryan's report, a discussion that was to be sustained over several months in letters to Nature. At the meeting, the debate, led by the arch-disputant Fitzgerald, rather discomfited Boltzmann. Now remembered for the boldness of his famous contraction, it is hard today to credit the influence Fitzgerald exercised over his contemporaries. With an imposing presence and the ready tongue of a Dubliner, he had few equals in debate. Bryan recalled with relish the memorable field day on kinetic theory at Oxford, singling out the onslaught by Fitzgerald…. *Prof. Boltzmann made no attempt to answer…..he several times mentioned the question to me after the debate as one that had not been satisfactorily cleared up*. By no means fluent in English, it is small wonder Boltzmann was left speechless by the onslaught in Fitzgerald's Dublinspeak. He would have been glad of the opportunity to mull things over with Bryan in German. Despite the twenty year gap in age and the even wider chasm between a man at the pinnacle of theoretical physics and a young mathematician making his reputation, it seems Boltzmann and Bryan took to one another. Their discussions at Oxford led to the publication of a joint paper, [12].

While kinetic theory was the focus in Section A at Oxford, Bryan doubtless heard from Boltzmann about the lecture, *On Aviation*, he was due to deliver on his return to Vienna, [13]. Boltzmann had not only read Otto Lilienthal's book on the flight of birds, [14], but took great interest in his experiments on gliders. Flying was on Boltzmann's mind that summer. He had written to Lilienthal before travelling to Oxford and some weeks after his return, wrote again, this time asking for a quote for a glider powered with a rotating engine. Another guest at Oxford was Samuel Pierpont Langley, the American astrophysicist turned aerodynamics pioneer. A few years earlier, Langley had published his *Experiments in Aerodynamics* [15]. Reporting his latest findings, he had an even rougher ride than Boltzmann, attacked by both Rayleigh and Kelvin. Kelvin was reactionary as far as flying was concerned; invited to become a member of the Aeronautical Society, he replied saying that *I have not the slightest molecule of faith in aerial navigation….So you will understand that I would not care to be a member*, [16]. Rayleigh, on the other hand, was interested in manned flight and brought news of Maxim's recent experiments to Boltzmann's notice, [17]. Fitzgerald too caught Boltzmann's infectious enthusiasm for Lilienthal's experiments. True to form, he lost no time in getting in on the act, ordering a glider early in 1895, the first in Britain and Ireland, for which he paid Lilienthal £25 (500Marks). The delivery that summer was billed as a *Normal Segelapparat ; its purpose described as Erprobung als Drachen; Versuche durch Studenten* (testing as a kite; experiments by students), [18]. Boltzmann seemingly decided not to go ahead with his purchase. It was little wonder that Bryan, listening to Langley and talking with Boltzmann and Fitzgerald, was fired by their enthusiasm for flight. But age, if nothing else, set Bryan apart. At 30, of a new generation, his reputation had yet to be cemented; having made his mark at Oxford, a switch of interests from thermodynamics to aerodynamics could have been seen as ill-advised, with his fellowship coming to an end. Joseph Larmor would surely have cautioned against involvement in this new interest; one of nature's conservatives, Larmor had misgivings about motor cars, let alone aeroplanes.

Flying was in the air in 1894, not only at the British Association. That summer, a 26-year old engineer, Frederick William Lanchester, read a paper to the Birmingham Natural History and Philosophical Society on *The Soaring of Birds and the Possibilities of Mechanical Flight*. Lanchester, a designer for the Forward Gas Engine Company, experimented with gliders in his spare time. In later life he recalled that he had been warned that his profession as an engineer would suffer if he dabbled in a subject that was merely a dream of madmen. Fortunately he had the means to pay for his hobby out of his own pocket and could indulge his dream regardless.

4. BRYAN AT BANGOR: BEGINNINGS

Back at Cambridge, Bryan had the satisfaction of seeing the debate his Report had sparked at the British Association re-ignited in the columns of *Nature,* exchanges that ran to twenty five letters into 1895, no fewer than six from Bryan himself. Bryan's standing as one of the leaders in kinetic theory was assured and that year saw his election to the Royal Society. It also marked the end of his fellowship at Peterhouse. He stayed at Cambridge until the summer of 1896 when he was appointed Temporary Lecturer in Mathematics at the University College of North Wales. How and why Bryan came to be appointed to a temporary lectureship at Bangor is a mystery. Was it thought advisable that the College should take a good look at Bryan before making an election to the Chair vacated that summer by George Ballard Matthews, who had retreated back to Cambridge, disillusioned after twelve years at Bangor? Whatever the reason, the Registrar at Bangor wrote in late September advising Bryan: *….The permanent appointment, to which the temporary gives no claim, will ….be made in December.* Reading between the lines, it seems not unlikely that Bryan had been given a nod and a wink that he had every chance of success. So it happened that the College Council appointed Bryan to the Chair of Mathematics at their meeting on December 14, 1896. He was to stay at Bangor until his retirement thirty years later.

Bryan got news of his temporary appointment at Liverpool, venue for that year's British Association. The seeds sown by Boltzmann two years before had germinated, but where Fitzgerald ordered a glider, Bryan had settled for a copy of Lilienthal's book on bird flight and read a paper *On the Sailing Flight of Birds*. At Liverpool, the talk was of Lilienthal's fatal accident just weeks before, when his glider had been struck by a sudden gust. *Alas, poor Lilienthal*, Oliver Lodge had scribbled in a note to Fitzgerald just before the meeting. The news would have sobered Fitzgerald who had been attempting to fly his glider, bought the year before. Hauled by stalwarts from the TCD rugby and hurling clubs, it seems the best they could manage was to get the professor a few feet off the ground. It is doubtful if he took to the air again.

Arrived at Bangor, Bryan's immediate concerns would have been to get mathematics classes organised for the term about to start. Looking from his college window at the seagulls sailing over Port Penrhyn, he would have seen "Nature's aeroplanes" at first hand, masters of control, impervious to the gusts that had proved fatal to Lilienthal. More than anything, Lilienthal's accident had convinced Bryan that stability was key to success in flying and that an understanding of it would save lives. Intent on turning his attention from the subtleties of the *H*-theorem to the intricacies of stability, he lectured on *Progress in Manned Flight* at the Royal Armoury, Woolwich in 1896 and early in 1897 spoke to the College Scientific Society on flying, entertaining his audience by launching model gliders in the lecture theatre. Later that year he contributed a *Review of Artificial Flight,* [19], in which he drew attention to the crucial role of stability. As it happened, he was not alone in identifying the significance of aeroplane stability. That same year Lanchester submitted a paper developing his 1894 lecture, first to the Royal Society and then to the Physical Society of London, where it was rejected. It was a setback that rankled with its combative author years later. All Lanchester could salvage from his efforts in 1897 was a patent on the theory of stability. Bryan's own campaign on the stability of flight was about to suffer a setback, one that would threaten the stability of his department.

4.1 Instability at Bangor: the "Bryan affair"

The first of the happenings that blew Bryan's campaign off course stemmed from the very nature of his appointment. As Professor of Pure and Applied Mathematics he was charged with teaching and examining in mathematics, with the help of an assistant, David Edwardes, a graduate in mathematics from Trinity College Dublin, appointed under Bryan's predecessor. Ballard Matthews was so exquisitely sensitive that it would never have occurred to him to assign duties to his assistant, with the result that Edwardes pretty much suited himself. Bryan, on the other hand, to whom getting his own way was virtually imprinted on his DNA, wanted all the assistance he could get, to allow time for research. In all likelihood he had precious little inkling of what was in store for him at Bangor, with twelve or more lectures to deliver each week. An early annual report to the College Council complains that *…Setting and looking over exercises occupies more hours every week than the lectures themselves*. On top of that, cocooned at Cambridge for so long, he had little notion of how to go about organising work in a small—very small—department. So with the assistant unwilling and the professor unwitting and at times unwell during the session 1897/8, it was small wonder that mathematics at Bangor was headed for the rocks.

On top of his fractious relations with Edwardes, Bryan faced problems of his own. The class with by far the biggest enrolment, the Matriculation Class, was traditionally the preserve of the professor. A wonderfully graphic account of the goings-on was broadcast years later by a member of the class of 1899, Thomas Richards [20]:
There is not such a class in the Colleges of the University for years now, and a good job too! A fair hundred in number…in the front rows a number of red-cheeked lads from Anglesey and Arfon, shy to begin with, sons of farmers, who had been advised by the Professor of Agriculture to get acquainted a little with the world of sums. Behind them, preachers from different denominations, considered weak in number and measure. Then, in the middle, benches of women, liable to laugh irrationally at the strange happenings of the world about them. At the back, the two-year Normals….full of mischief. From their number came the happenings. As sure as anything, sometime in the Autumn term one of them would suddenly fall unconscious to the ground,….a group of his fellow-travellers rushing to carry him with the professor in front to a private room, Bryan reaching from the cupboard a vessel of strong drink and pouring a substantial ration of it down the throat of the unfortunate. Then the gradual—very gradual—coming-to and one of his fellows helping him back to his place. What hope for number and measure for the rest of that hour?

What hope for stability and control, at times restored only by the intervention of the Principal? Reichel might have been forgiven for harbouring doubts about Bryan. Straight-laced scion of the Bishop of Meath, remote in his dealings with staff and students alike, the Principal would have wanted no truck with a colleague who kept whiskey in his cupboard, whether as restorative for stricken matriculants or stimulant to his own research. Going into 1899, the very stability of the Mathematics Department was threatened by the near total breakdown in communication between professor and assistant. The College Council set up a Committee of Inquiry. Wind of the rumpus spread outside Bangor, mathematicians writing to one another with news of the "Bryan affair". Bryan suffered a nervous breakdown, retiring hurt to Cambridge. The inquiry in the end offered no resolution beyond undertakings of cooperation from both parties for the benefit of their students. The truce lasted through the next session before trouble brewed again. The breach in relations between Bryan and Reichel never mended. Bryan felt betrayed by the Principal's lack of support, while Reichel had come to rue the appointment of a highly eccentric and questionably competent Professor of Mathematics.

4.2 Equilibrium restored

Freed for the time being from the yoke of Bangor, Bryan had all the hours he needed for research and turned, not to the stability of flight, but to the stability of ships. Of all the motions a ship is subject to, the need to reduce roll is paramount as this affects seaworthiness. Naval architects down the years had aimed for a design with margin enough to ensure stability, along with bilge keels, long fins welded in pairs along the hull to damp the roll. At the same time they were puzzled why bilge keels proved more effective against roll than conventional theory allowed. Bryan was able to show that a rolling ship induces an opposing counter-current which significantly increases the pressure on the bilge keel, [21]. Working on ship stability Bryan may well have pondered the full 3D problem of aeroplane stability. If so, he was again blown off course as soon as he got back to Bangor at the end of the summer.

In September 1899, Professor Arnold Sommerfeld, lately appointed editor of Volume 5 of the Enzyklopädie für Mathematische Wissenschaften, visited him. This monumental work spanned the known world of pure and applied mathematics. Volume 4 on applied mathematics even stretched to articles on sports science, on the stability of boomerangs and bicycles; Volume 5 was to cover physics in its early twentieth century entirety. The contributors to a man were drawn from German mathematics and physics but Sommerfeld wanted to broaden both its base and its appeal. More than likely his visit to Bangor was at Boltzmann's prompting. At any rate, Sommerfeld duly wrote inviting Bryan to contribute the article on thermodynamics, [22]. It was a signal honour for someone outside the orbit of German science, not least in a subject where there was no shortage of German contenders of distinction, Boltzmann himself or Planck, to name but two. Bryan took his time to reply, pleading that his *time had been very full*. What little time he had beyond his teaching duties had been given to a contribution to a volume celebrating the Jubilee of Professor H.A. Lorentz of Leiden, co-doyen with Boltzmann of the world of theoretical physics. In late November he wrote thanking Sommerfeld for his invitation, explaining that there would be some delay before he could make a start : *Since you were here I have an invitation to give a lecture at our Royal Institution on Artificial Flight next February and this makes me more busy than ever,* [23].

In the end thermodynamics took precedence, for Bryan's discourse on *The History and Progress of Aerial Locomotion*, [24] was delivered, not in February 1900 as intended, but a whole year later. His approach was broad-brush, though presciently Bryan drew attention to the fact that *…stability….. should not be investigated on gravity-propelled machines alone, but that the effects of a motor should be experimentally determined.* Curiously, with a title referring to the history of flight and with his own grasp of the importance of stability, Bryan made no mention of the man acclaimed as the father of aeronautics, Sir George Cayley, who had written about the problem of longitudinal stability and control in pioneering work almost a century before, [25]. The postponement of Bryan's discourse on aerial locomotion to 1901 had an intriguing knock-on effect. It appears that none other than Alexander Graham Bell, who had been experimenting with gliders at Cape Breton, had been invited to deliver a lecture on the same subject in 1901. He must have been put out on being sent a proof of Bryan's discourse in March 1901 and cabled to say that he in turn would postpone his lecture. Bell wrote to his wife [26]: *So this lecture is off for the present and I am glad of it, for… by giving such a lecture at the present time I would run a great chance of* ***making a fool of myself*** *….. Just fancy my position—with the reputation the telephone has given me I would appear before the Royal Institution as one believed to be an authority upon the subject – for why otherwise should I be invited?.... and then find that all had* *been previously discovered and* ***published*** *by others…Now I breathe freely and can go to work in what seems to me the proper way.*

More may have been said on the evening of Bryan's discourse than appears in the record [24]. One of those present, E. S. Bruce, was to recall later [27]: *In the course of a few remarks on gliding flight made in the course of his Friday evening discourse, it seemed to me evident that he had a greater grasp of the mathematical side of the problem of aerial navigation than had been previously evinced and* ***at my request****, he wrote the remarkable mathematical discourse on the subject which was read before the Aeronautical Society (on December 3, 1903).* That suggests that Bryan had already made a start on an analysis of the longitudinal stability of gliders by early 1901. He was not alone in his concern. In September 1901, in a lecture on *Some Aeronautical Experiments*, a bicycle manufacturer from Ohio, Wilbur Wright, lifted the curtain on glider trials conducted with his brother, voicing concern over the difficulties they had experienced that summer controlling longitudinal instability.

As the session wore on, skirmishes with Edwardes flared up again. Bryan had had enough. In early May he wrote to Sommerfeld to ask if he would be willing to write a testimonial in support of his application for the Chair of Natural Philosophy at Edinburgh. At Bangor, the College Council set up yet another inquiry that this time ruled that the fault lay fair and square with Edwardes. Finally, in December 1901, Edwardes submitted his resignation, bringing five years of disharmony to an end.

5. A START ON THE STABILITY OF FLIGHT

Going into 1902, Bryan's spirits would have been lifted and not just on account of Edwardes' departure. The article on thermodynamics for the Enzyklopädie was almost ready. The Association of Naval Architects had awarded him their Gold Medal for his paper on bilge keels. On top of that, Edwardes was replaced by a competent assistant who would stay at Bangor for several years. Last, but by no means least, the Mathematics department had its first postgraduate student. William Ellis Williams, who had graduated with honours the previous summer, was the very embodiment of the vision shown by the men of Penrhyn and other quarries whose hard-earned pennies had been given to the fund set up to found the College at Bangor in 1884. He was himself the son of a quarryman and had won a generously endowed prize in Mathematics, the R. A. Jones Prize, that allowed him to stay at Bangor and work with Bryan on the stability of gliders, in Bryan's words, *the first instance of a student pursuing studies in Mathematics above the honours course.* Like anyone who has had dealings with stability, Bryan was acutely conscious of the need to get the signs right. Having someone to do the analysis independently would have been a godsend.

By analogy with the analysis of the stability of ships, Bryan modelled the aeroplane as a rigid body acted on by a set of forces. By confining attention to gliders, the only forces involved were gravity and the aerodynamic forces of lift and drag. Bryan opted for a reference frame with axes fixed in the aeroplane and carried along with it. An aeroplane has a vertical plane of symmetry through its longitudinal axis. Crucially, if the aeroplane is considered as a symmetric rigid body flying parallel to its plane of symmetry, the set of six simultaneous variables reduces to two sets of three. By assuming that forces and moments in the plane of symmetry of the glider depended only on motion in the plane, Bryan and Williams confined their attention to an examination of *longitudinal* stability, i.e. of the pitching of the machine. Stability had long been recognised as a key dynamical construct, codified by Routh in 1877, [28], in a formalism that would have been meat and drink to Bryan. Central to the investigation of stability of a dynamical system is its response to a disturbance small enough not to excite the nonlinearity inherent in any system when strongly perturbed. Fortunately for the durability of the model, in the grander scheme of things, aeroplanes are by and large appealingly linear systems.

The air resistance forces that appeared in the equations were of course unknown, so that in the perturbation equations, following a Taylor-expansion about the equilibrium state, partial derivatives of each force and moment with respect to each of the velocity components are introduced, evaluated at the corresponding equilibrium value. Typically the change in a component force, X , due to a perturbation in a velocity component u is determined by the derivative $(\partial X/\partial u) \equiv X_u$. Bryan later called X_u and its counterparts, *resistance derivatives*; these derivatives, nine in all, govern the stability analysis. The resistance derivatives themselves have either to be determined from aerodynamic theory or found from wind tunnel measurements. The determination of these derivatives was clearly key and the slow progress in that direction was to prove a constant irritation to Bryan. Speaking to the Aeronautical Society in December 1903, Bryan remarked: *It is necessary to know….. the resistance of the air on the supporting surfaces as a function of the velocity and angle of incidence and also the point of application of this force, for different angles of incidence…… Unfortunately our knowledge of these points is very unsatisfactory…. Until experiments are made ..it will be impossible to solve the problem of stability.*

Bryan and Williams set about characterising the longitudinal stability of various configurations: single laminae and gliders made up of two planes, one behind the other, of different dimensions and at different inclinations to one another. They found that steady linear motion should be stable provided $V^2 > ka$, where V denotes the glide velocity, a is a constant that depends on the linear dimension of the glider whereas the constant k depends on its shape, the glide angle and the aerial resistance. Moreover, by inclining the planes at a small angle to one another, stability was enhanced. Bryan and Williams were able to identify two undulations in the longitudinal motion of distinctly different scale lengths, observing that the most likely way for a glider to overturn is by means of a sequence of oscillations of increasing amplitude. Towards the end of the paper, almost as an aside, there is a brief mention that photographs of the paths of gliders *distinctly showed these two undulations, thus confirming the theory*. Williams had photographed the flight paths of model gliders by attaching magnesium wire to them. A slotted disc which rotated at constant speed was placed in front of the plate, the trace of the path being divided by this means into a series of dashes. From this, Williams was able to estimate the velocity of the model. One might have thought this important contribution to the paper could have done with some elaboration but the omission of experimental detail appears not to have troubled the referees, Professor Horace Lamb and Professor A.E.H. Love, applied mathematicians both, and the paper was published in the Proceedings of the Royal Society in January 1904. Looking at the paper today, what is striking is the near total absence of physical discussion; notably there is no interpretation of the onset of instability at low speeds. What there is, is algebra aplenty. Bryan, ever one for letting the mathematics do the talking, seemed to have little awareness that those doing the actual flying would soon be lost in the maze of algebra. Be that as it may, it was to prove a landmark paper, in all likelihood the most enduring ever written at Bangor. In his annual report for 1903 Bryan wrote: *….The methods adopted are entirely new…and contain the clue to the solution of the problem of artificial flight.*

For all that, the Bryan-Williams paper made no immediate impact. Sandwiched between Bryan's lecture to the Aeronautical Society in December 1903 and the publication of the paper in January 1904, was an event that put mathematical resolutions of glider stability in the shade. On December 17 1903, the Wright brothers flew their powered aeroplane, their *Flyer*, near Kitty Hawk, North Carolina for the first time. The publicity-shy brothers made only the briefest of announcements of their flight to the press on January 4, 1904. The Wrights had drawn inspiration initially from Lilienthal and like him were more focused on the practice of *control* than on designing for intrinsic *stability.* As bicycle makers they would have understood that whereas a bicycle does not score in the stability stakes, it is highly susceptible to control. As glider builders and pilots they carried forward that understanding into designing their gliders, realising that stability and control in the end work at cross purposes and that an aeroplane becomes less manoeuvrable the more stable it is. By 1901 they had experienced problems with severe longitudinal undulations that needed both full deployment of the canard control surface and required the pilot to move forward to prevent onset of instability. In reality the gliders they designed and built appear to have been inherently unstable and the fact that they flew successfully says much for the sensitive wing-warping mechanism designed to achieve lateral control, to say nothing of their aerial gymnastics.

Preoccupied with their own problems, it is unlikely that they knew anything of the Bryan-Williams paper when it appeared. They solved their control problems by trial and error, with little need for – or understanding of– mathematics and had they come across the paper, could have been forgiven a wry smile on reading: *The problem of artificial flight is hardly likely to be solved until the conditions of longitudinal stability of an aeroplane system have been reduced to a matter of pure mathematical calculation.* If Bryan expected a response akin to the enthusiasm naval architects had shown for his bilge keel work, he was to be disappointed. The "practical men" were by and large nonplussed by the mathematics. Mathematicians just yawned.

6. THE YEARS OF WAITING

By the time the paper was published, thermodynamics had yet again laid claim to Bryan's time in the shape of an invitation to contribute to a *Festschrift* celebrating Boltzmann's 60th birthday on February 20, 1904. He kept good company, with the likes of Lorentz, Planck, Mach and van der Waals leading a parade of players from Europe's top division. His contribution *On the Law of Degradation of Energy as the Fundamental Principle of Thermodynamics* would have had particular appeal for Boltzmann, [29]. *Festschrift* or not, the more significant contributions were critically reviewed in *Beiblätter zu den Annalen der Physik*. Bryan's paper was sent to one of the journal's regular reviewers, who signed his reviews, *A.E.*, none other than the Patent Clerk (Second Class) in the Bern Patent Office, Albert Einstein, writing in his own *annus mirabilis*,

1905. Einstein already had a reputation for his succinct opinions on the patent claims that crossed his desk, more than one damned by the verdict *incorrect, inexact and unclearly expressed* ! It says much that *A.E.* found in favour, noting that Bryan's argument had been developed in an elegant way, [30].

Bryan himself turned 40 in 1904. Life at Bangor was now relatively calm and in August he set out to attend the Third International Congress of Mathematicians at Heidelberg and to visit Sommerfeld and other German colleagues. Surprisingly, he did not contribute a paper at Heidelberg, though he would have heard a young engineer, Ludwig Prandtl, present results that were to have a profound impact on the development of fluid dynamics. Prandtl's paper *On the Motion of Fluids with very little Friction* contained the first mention of the concept of a *boundary layer* that would, a few years on, make possible the determination of aerodynamic drag. From Heidelberg, Bryan went to Berlin to visit Planck. After hearing him lecture, he reported plaintively: *A German professor will only lecture 6 hours a week and will have the whole of the rest of his time for his researches.* Bryan and Boltzmann had exchanged visits the previous summer but in August 1904 Boltzmann was en route to lecture at the World Exhibition at St. Louis. Aviation featured prominently at St. Louis in the wake of the Wrights' success. Bryan did not attend though that did nothing to debar him from commenting from the sidelines to Nature, [31], noting disapprovingly of the large prizes offered in the aeronautical competition at St. Louis: *A more useful purpose would have been… to encourage investigations calculated to throw indirect light on …. longitudinal stability. Careful measurements of the coefficients of stability of actual machines are even more needed than further balancing experiments in mid-air.* So much for the Wrights' gymnastics!

Under contract to the German publishing house B. G. Teubner, the following two sessions were given over to finishing his book on *Thermodynamics*, completed by mid-1906. Two events from that summer, one happy, the other tragic, had a profound influence on Bryan's life. The first was his marriage, the other, Boltzmann's suicide in August while on holiday. Bryan contributed an obituary to the Royal Society, [32], sincere, if in places oddly naïve. Writing of his friend he asks: *Is it not probable that Boltzmann's ever-active brain had been taxed too heavily by the difficult and elusive problems which he was endeavouring to solve? Mathematical research is a dangerous occupation if carried too far……It may be that such obstacles and difficulties as the necessity of undertaking some elementary teaching constitute an analogue to the refrigerator which is necessary for the continuous and efficient working of a thermodynamic engine.* It is an odd comment from a man once sorely tried by the antics of Bangor's Matriculation Class.

Boltzmann's death in more ways than one marked a watershed for Bryan. The meteoric rise in his reputation in thermodynamics and kinetic theory following their first meeting at Oxford and the stimulus he got from Boltzmann's interest in flying, stand testament to the influence Boltzmann had on him. Following the publication of *Thermodynamics*, Bryan was again ready to give his full attention to aviation. But just as ten years earlier, upheavals at Bangor got in the way. In 1907, his assistant, Harold Hilton, who had given loyal support over several years, resigned. Hilton's successor appears to have been a flop from the start and left at the end of the following year to take holy orders. Added to the disruption this ill-judged appointment caused to the smooth running of his department, it cannot have helped that Bryan himself had been elected president of the Mathematical Association in 1908 and a busy president he proved to be. In his opening remarks to a meeting convened to discuss the correlation of mathematics and science teaching, Bryan, never one for understating a problem, announced one of the ends was to ….*improve the teaching of science and to raise Great Britain out of the position she had drifted into of being the most un-mathematical State on the face of the Earth.*

7. BRYAN AND LANCHESTER

It took another event in 1908 for Bryan to get back to the stability work set aside four years earlier. This was the publication of the second volume of Lanchester's book on *Aerial Flight*, [2]. Volume 1, *Aerodynamics,* published in 1907 had focused on the development of his vortex theory and experiments related to it. The second volume, *Aerodonetics*, dealt with the trajectories of bodies in free flight and the conditions for longitudinal, lateral and directional stability, along with observations using scale models to verify his theoretical predictions. It was the very territory of Bryan and Williams. Bryan's review of *Aerodonetics* in *Nature* [33] lost no time in claiming that Lanchester's theory conflicted with his and Williams' paper, adding: *There has been some difficulty in making out how Mr. Lanchester arrives at his results…. Mr. Harper has applied the Bryan-Williams method to the particular kind of tailed aeroplane considered by Mr. Lanchester and obtains a numerically different result, the discrepancy being accountable for by the assumptions made in Mr. Lanchester's method.* The assumption Bryan held to be unjustified was that the simple harmonic character of Lanchester's phugoid oscillations, valid when these are small initially, breaks down as their amplitude increases. With this marker put down for the generality of the Bryan-Williams method, Bryan went on to concede that Lanchester's observations and experiments nevertheless deserved careful consideration: *His book represents a serious effort to place the theory of flight on a scientific basis and should convince would-be aviators that airship design is a subject requiring hard thought, endless experiments and great care in drawing conclusions from them.* With his deep understanding of the subject matter, Bryan was uniquely placed to see that behind Lanchester's flaky mathematics lay a quite unique physical intuition.

Indeed Lanchester's ideas on stability probably resonated with the early designers in a way that Bryan's algebra emphatically did not. Lanchester's model was readily grasped; an aeroplane in which the resultant of the aerodynamic forces passed through the centre of gravity would show no tendency to pitch. When the angle of attack is increased marginally, the aircraft design must be such that the resultant moves *aft* of the centre of gravity, giving rise to a moment that acts to restore equilibrium. Thus a simple physical criterion for longitudinal stability is that the rate of change of the pitching moment with angle of attack is negative. The same physics, cloaked in Bryan's algebra, was lost on the practical men.

Theorising apart, Lanchester's brilliant insights had earned him a seat on the newly-formed Advisory Committee for Aeronautics, brought into being on April 30, 1909 by the Asquith government, and presided over by Lord Rayleigh. Its membership included representatives from both Army and Navy, the director of the National Physical Laboratory (the first wind tunnel had been built at the NPL in 1903), the director of the Meteorological Office, along with engineers of different descriptions. Lanchester is cast as "polymath", apt enough even if his impressive breadth of interests didn't stretch as far as mathematics. The mathematician appointed to the Committee was Sir George Greenhill, recently retired from the chair of mathematics at the Royal Military Academy, Woolwich. Rayleigh was probably shrewd enough to realise that Bryan and Lanchester at the same table might prove too combustible a mix. Bryan must have been mildly peeved at having been passed over. Commenting on the formation of the Committee, he wrote, [34]: *It is scarcely surprising that the cry "Too much theory" finds its way into the papers and that some ….. put in a plea for the practical man……It would be more correct to describe the present position of aeronautics as too much theorising and too little theory……In many cases it is the practical man who revels in the excessive use and abuse of formulae.* With his review of *Aerodonetics* appearing in the same issue of *Nature,* there is little doubting the identity, in Bryan's book, of the abuser-in-chief. In drawing a distinction between theory and theorising, Bryan's was indeed almost the lone voice of theory crying in the wilderness, albeit an insistent – at times strident – one. His frustration seems to have got the better of him when he chaired a joint discussion of theorists (Section A) and practical men (Section G) on *The Principles of Mechanical Flight* at the British Association meeting at Sheffield in 1910. It was by all accounts, a disaster. The sanitised report in Nature, [35], remarks primly that the discussion *wandered away from the title and developed into one on the relative positions of the mathematicians and the practical engineers in the development of new ideas on aviation.* Sir Oliver Lodge, no mincer of words, blamed Bryan for the débacle, [36]: *I do not know who was responsible for selecting Bryan to open a Joint Discussion between A and G, but...he is utterly incompetent for such a part….. I did not hear the whole of his paper, but what I did hear seemed to be flippant irrelevant nonsense and he himself not far removed from a lunatic.*

8. STABILITY IN AVIATION: BRYAN AND HARPER

Rail as he might, Bryan's plea for measurements of the resistance derivatives went unheard. No one took up the challenge. The whole focus for those interested in aviation after the success of the Wrights was to get into the air and fly, controlling unstable behaviour as best they could. Regrettably it often wasn't good enough as the frequent fatalities attested. Writing of Bryan, Bairstow [6] later made a perceptive comment on the importance of "times and seasons" where new ideas are concerned: *The year 1904 was well in advance of the beginning of public flying which might perhaps be dated 1908, whilst 1911 was a little beyond it. The dates are important, for aeronautical practice was only just ripe for Bryan's analysis at the later date.* With Lanchester's stability work in print, times and seasons were indeed ripe in 1908 for Bryan to get back to where he and Williams had left off four years before. Williams, as it happened, was back in Bangor after spending a year in Munich as assistant to Roentgen. In a Physics department short of resources, he turned again to his first love, no doubt encouraged by Bryan. By 1909 his enthusiasm for building an aeroplane came to the notice of a member of the College Council, Henry Davies, a Menai Bridge ship owner with the means to fund Williams' enterprise. This machine, built in 1910 and known as the *Bamboo Bird*, was to be used *mainly in the furtherance of the study of the stability and efficiency of flying machines and to obtain experimental data for a theory of their motion.* Williams' goal of providing data enabling the resistance derivatives to be determined was in the end eclipsed by the wind tunnel measurements that finally got underway at

NPL in 1912, but the very fact that a practical man with a sound grasp of theory was back at work in Bangor would have given Bryan added impetus to buckle down to *Stability in Aviation.*

Of greater significance in bringing *Stability in Aviation* to publication was the appointment of Edgar Henry Harper as Bryan's assistant in January 1909. Harper, a young Irish mathematician from Dungannon, lost no time in getting down to work. Bryan's review of Lanchester's *Aerodonetics* (cf. Section 7) had singled out Harper's success in drawing attention to a discrepancy in Lanchester's work. His contributions to *Stability in Aviation*, earned the handsome tribute Bryan paid him in his Preface, where he claimed priority for Harper in many of the results, as well as acknowledging his independent working of all the formulae in the book.

Stability in Aviation is short, 192 pages in all. The Preface stands testament to Bryan's near-messianic belief that stability equated with safety: …*In reading the accounts of accidents, fatal and otherwise, that appear…in the daily papers, it is difficult to avoid coming to the conclusion that much of this loss of life and damage could be avoided by a systematic study of stability….of aeroplanes particularised in this book.* After a mathematical statement of the general equations of motion that shows how the general formulation may be broken down into the separate consideration of longitudinal and lateral stability, the next six chapters are given over to working out the characteristics of each configuration. Of course the longitudinal stability of gliders had been dealt with in the Bryan-Williams paper seven years earlier and it was a simple generalisation to extend their results to powered flight. Although a wind tunnel had been constructed at the National Physical Laboratory in 1903, the intervening years had proved barren as far as getting data on the resistance derivatives. Bryan's frustration is palpable: *When an aeroplane begins to pitch, the effects of this rotation appear to be at present unknown….at the time of writing no indications have reached us of the matter receiving attention at the Government Laboratory.*

Knowing that Lanchester was forging ahead, without need for public funding would have fuelled his impatience with the Government Laboratory. *Stability in Aviation* makes repeated reference to Lanchester's work. Conceding that while Lanchester's longitudinal stability condition did in fact confirm the Bryan -Williams result, he points out that the method used by Lanchester is quite distinct from theirs and is limited in that it can only deal with the phugoid (long-period) oscillations. One senses that Bryan can barely contain his disbelief that his rival had somehow winkled out the stability condition *from a highly original point of view, the use of the equations of Rigid Dynamics being practically avoided.* To Bryan, any derivation not ground out in all its algebraic detail was suspect. Later in the book he deconstructs Lanchester's method, showing precisely why it works for the long-period oscillations while missing the short-period ones entirely. In addition to several extensions to the Bryan-Williams results, there is a important addition that allows for the effect of an inclined flight path on stability. Bryan is emphatic that not only the result itself, but the realisation that the earlier theory needed to be extended, is due entirely to Harper. The two chapters that explore the characteristics of lateral stability were entirely new. The techniques are the same as those used for longitudinal stability, only now intuition is less of a help in trying to unpack the asymmetric lateral stability problem. The lateral motion involves *side-slip, roll* and *yaw* and in particular the lateral modes involve roll coupled with yaw. Bryan saw clearly the need that … *the interdependence, not only of the two rotational* *oscillations, but also of sideways displacements, should be taken into account. It is only through the latter displacements that gravity can have any effect on the direction of an aeroplane.*

In parallel with the longitudinal case, there are again 9 resistance derivatives which now describe the rate of change of the side force *Z* and the couples *L* and *M* due to air resistance, with the sideways velocity, *w*, and the components of angular velocity, *p* and *q*. The nine derivatives are $(Z, L, M)_{w,p,q}$ where $L_w \equiv \partial L/\partial w$, for example, denotes the rate of change in the rolling moment with sideslip velocity. This particular derivative, describing the *dihedral effect*, is important, though not all nine components are of equal weight, Z_p being one that can be neglected in general. By contrast, the lateral force due to sideslip is crucial and can only be found accurately from wind tunnel measurements. Bryan realised that the separation in the longitudinal case into long and short oscillations no longer held in the lateral case. Moreover for lateral stability it was essential to consider in turn the effects of the main planes (wings), whether straight or bent-up (i.e. dihedral) or with bent-up tips, vertical fins and planes attached to the extremities of the wings, namely ailerons or "stabilisers" in Bryan's language. An entire chapter is given to Harper's analysis of stability of the dihedral configuration, in which he established that the tail needed to be above a certain size to ensure stability. As early as July 1907, Bryan had paid a visit to Blériot's factory outside Paris and seen an aeroplane of the "Antoinette" type under construction; this machine had a distinct dihedral wing configuration and a vertical tail plane and was a prototype of the aircraft flown by Blériot in crossing the Channel in 1909.

Given Harper's analysis of the lateral stability of Blériot's aeroplane and the extent of his other contributions, not least the labour involved in checking every formula in the entire book, it is in a sense surprising that his name does not appear on the title-page as co-author. Instead, Bryan encouraged Harper along with Allan Ferguson, an assistant lecturer in physics, to write a popular account of the principles underlying the new science of aeronautics. Their book, *Aerial Locomotion,* [37], with a foreword by Bryan, was published by Cambridge University Press in 1911 a few months before *Stability in Aviation* appeared. It was truly remarkable that two books on aviation should appear in the same year by authors from the same small Mathematics Department. On top of that, the 1910/11 Report to the Court of Governors of the College records that *during the recent long vacation , Mr. Williams hired a more powerful motor, which enabled him to perform some successful flights*, though that summer seems to have been the last time the *Bamboo Bird* took to the air. The following session saw what, in all likelihood, was the first Honours course on *Equilibrium and Stability of Aeroplanes* offered in a British, or indeed any, university.

Bairstow's judgement that times and seasons were only ready for Bryan's analyses by 1911 was rooted in his own contributions to the stability of flight. Bairstow tested a Blériot dihedral model using the large NPL wind tunnel commissioned in 1912 and applied the test results to determine Bryan's resistance derivatives, [38]. He was then in a position to determine the dynamics of the model. From that point on, the pace of advance quickened so that by 1914 an inherently stable aeroplane, the B(lériot) E(xperimental)2c had been designed at Farnborough by E. T. Busk, incorporating Bryan's ideas and had flown successfully. (The BE2c was a very different aircraft to the BE2a and b, designed by de Havilland.) In Bairstow's words, the B.E.2c *showed conclusively that the line of argument developed by Bryan could be used to calculate the degree* *of stability of an aeroplane and so paved the way for the design of aeroplanes with the desired characteristics of indifference or stability as required for fighting or bombing.* Bryan was no longer a lone voice; his achievements were marked by the award of the Gold Medal of the Royal Aeronautical Society in 1915, an honour shared with Busk, sadly posthumously, as Busk was killed while test flying a B.E.2c in November 1914. Fittingly enough the medal was presented by Lanchester who couldn't resist one final dig in his address: *…We have all heard lately the insistent cry, "Wanted – more shells!" Those who would read Stability in Aviation would find that throughout his work the insistent cry was "Wanted – more algebra!".* Behind the facade of algebra lay a very sound physical insight and in the end Bryan's resolute campaign on the stability of flight succeeded in spite of the disinterest shown by most practical men and the stony disregard of his fellow mathematicians.

All told, it was an achievement to be proud of, not least for its realisation in a newly founded College with slender resources. It was an achievement only made possible through the vision and tenacity of a man who was to retire to Bordighera on the Italian Riviera without tribute or acclaim from Bangor after a tenure of thirty years and whose appointment Principal Reichel, to his lasting shame, judged a mistake. A hundred years since publication, *Stability in Aviation* lives on in the annals of aviation, testament to the genius of George Hartley Bryan.

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The author

Professor T. J. M. Boyd.

G. H. Bryan was the first and T. J. M. Boyd the last of the four applied mathematicians to have held the Chair in Mathematics at the University of Wales, Bangor in its 120 year history (from the foundation of the College in 1884 until the Mathematics Department closed in 2004). Professor Boyd held the chair from 1968 – 1982 before succeeding to the Chair of Physics at Bangor. In 1990 he was appointed Professor of Plasma Physics at the University of Essex where he is now Professor Emeritus. He continues as Adjunct Professor in the Instituto Nacional de Ciencias Nucleares, Mexico. His research interests lie in ultra-relativistic laser-plasma physics, and include the generation of high harmonic spectra and gigagauss magnetic fields.

The Great Depression had a tremendous effect on aviation industry in the 1930s, delaying the production of the first financially successful commercial aircraft. Only in 1939, Boeingâ€™s rival, McDonnell Douglas Corporation created first profitable commercial airplane Douglas DC-3, used to carry over 90% of U.S. air travelers by the beginning of WW2. In response to Douglas DC-3, Boeing developed a long range 314 Clipper that was largest civilian aircraft of its time. It featured dressing rooms, dining salons and was able to carry 90 passengers. The success in the scheduled flight field did not la The evolution of military aviation these days consists mainly of updating existing fleets. We are unlikely to see any breakthrough technologies in this field in the next 50 to 60 years.Â The Role and Place of Military Aviation in the 20th Century and the Trends that Could Lead to its Qualitative Development in the 21st Century. Military aviation will be replaced by high-precision weapons, which will be controlled by space systems. Military aviation will be replaced by high-precision weapons, which will be controlled by space systems. We should note here that, in the future, the means of warfare will not always be formed upon objective laws and their development. [2] Boyd T. ONE HUNDRED YEARS OF GH BRYAN'S STABILITY IN AVIATION [J]. Journal of Aeronautical History Paper No, 2011, 4. [3] NIELSEN J. Missile aerodynamics [J]. (1988). [4] Thbodohsbn T. General theory of aerodynamic instability and the mechanism of flutter [J]. (1935). [5] Albano E, Rodden WP. A doublet-lattice method for calculating lift distributions on oscillating surfaces in subsonic flows [J]. AIAA journal, 1969, 7 (2): 279-85. DOI: https://doi.org/10.2514/3.55530. [6] Yang Yongnian, Zhao Lingcheng.