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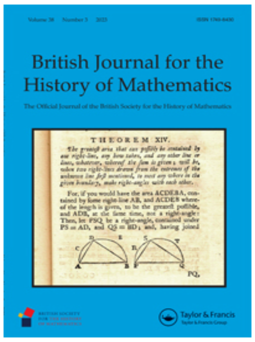

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Stability in theory, in the laboratory and in the air: William Ellis Williams' campaign for proof positive (1904–1914)

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The equations that determine the stability of aircraft were formulated at Bangor, Wales, over the first two decades of the twentieth century, a formulation that was to prove as fit for purpose in characterizing the stability of aircraft today, as for the aeroplane allsorts that took to the sky in the wake of the Wrights' *Flyer*. While these equations are commonly identified with George Hartley Bryan, this does disservice to the contributions of others at Bangor. Not the least of these was William Ellis Williams, whose achievement in identifying photographically the modes predicted by theory is little recognized. Williams later went on to construct what was in all likelihood the first aeroplane designed solely for research to be built in a university department, making the first airborne measurement of pressure distribution across aircraft wings in his *Bamboo Bird,* in 1913, an achievement not hitherto afforded due credit in the literature.

Introduction

William Ellis Williams is a name too often air-brushed out of the early history of aerodynamics, his contribution to the ground-breaking analysis of the longitudinal stability of gliders (1904a) overshadowed by that of his co-author, George Hartley Bryan.[1] At face value, such oversight is hardly a surprise. Bryan, after all, had spent years working on stability in one dynamical system or another. He had been interested in the stability of flight from about 1894 and had devoted no small amount of time to making model gliders from paper and experimenting on how to throw them. Bryan understood clearly the mathematical framework needed to unlock stability criteria. Greatly to his credit, he was fulsome in his appreciation of Williams' contributions to their work, describing as ingenious, techniques his student devised to track the flight paths of a variety of models. Not only that, Williams succeeded in identifying trajectories that correlated with those predicted by their theoretical analysis. Moreover, while Williams was an experimental physicist, by nature and by nurture, he was enough of a mathematician to undertake the stability analysis independently. Pilots' lives were at risk if conditions for glider stability were breached, so getting the analysis right was an imperative.

New content has been added. Please see Addendum (http://dx.doi.org/10.1080/26375451.2024.2326795)

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[1] Throughout the paper, unreferenced biographical details of Bryan's life and work are based on (Boyd 2011, 2017).

On the promise shown in his work with Bryan, completed in 1903, and more particularly, in experiments on the effects of stress and temperature on the resistance of iron and nickel, (Williams 1902), completed under the guidance of Professor Edwin Taylor Jones, in the Department of Physics at Bangor, Williams was awarded a University of Wales fellowship.[2] These awards were designed to afford promising young researchers the opportunity of working in laboratories outside Wales to gain experience and widen their research horizons. Williams elected to spend the first year of his tenure at Glasgow, where Andrew Gray, who had served as foundation professor of physics at Bangor from 1884, had been appointed to succeed Kelvin in 1899. Gray's presence would have ensured a warm welcome for a man he had taught at Bangor just a few years earlier. One thing we can be sure of is that Bryan had no hand in Williams' leaving for Glasgow; the move still rankled years later. 'Research', grumbled Bryan, 'was interpreted as meaning practical work in a physical laboratory, away from Bangor. The award had the effect of preventing the continuation of original work on this important problem (*viz.* the stability of flight)' (Bryan 1910).

It wasn't just Williams' move to Glasgow that irked Bryan. By and large, the response of the aeronautics community to their paper on stability had been lukewarm. One who did engage was Capt. Ferdinand Ferber, then a staff officer at the School of Applied Artillery at Fontainebleau. Ferber was a French-educated engineer with a grasp of mathematics beyond that of many of his British counterparts. Bryan held Ferber in high regard, even handing over the stability baton to him in 1904, while he resigned himself to attend 'to the requirements of junior students at Bangor, whose knowledge of Euclid and of arithmetic had been neglected at school' (Bryan 1910).

Longitudinal stability; an algebra for survival

What, precisely, was the contribution Bryan and Williams made to the understanding of the stability of flight? When they embarked on their work in 1902, there was no theoretical basis for the stability of aircraft. Not only had the dynamical equations for an aeroplane not been solved, they hadn't even been formulated; even as late as 1902, no one had taken into account, in any quantitative sense, the moments of forces acting on an aircraft. They began by limiting themselves to purely longitudinal motion and in addition, considered just the motion of a glider so that the only forces acting on the aircraft are gravity and the aerodynamic forces of lift and drag. The longitudinal constraint meant that the only moment to be accounted for was that associated with the pitching of the aeroplane.

Stability had long been recognized as a key dynamical construct, codified by Routh in 1877 in a formalism well understood by Bryan. Speaking to the Aeronautical Society in December 1903, Bryan remarked:

> In order to apply Routh's results to the stability of flying machines, it is necessary to know the moment of inertia of the aeroplane about its centre of gravity, 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, i.e. the centre of pressure for different angles of incidence. Unfortunately, our knowledge of

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[2] See (Owens 2008) as the source of biographical details of Williams' life and work unless otherwise stated.

> these points is very unsatisfactory. Until experiments are made on this point, it will be impossible to resolve the problem of stability. (Quoted in Bryan and Williams 1904b)

Despite these difficulties, Bryan felt strongly that guidelines on stability were needed to safeguard the pioneer airmen of the day. The doyen of early flyers, Otto Lilienthal, had lost his life in an accident in 1896, which Bryan attributed to the inherent instability of his glider. In 1899, Lilienthal's disciple, Percy Pilcher, an early associate of Bryan, was killed when the tail of his glider suffered structural failure. A few years later, in his annual report to the College Council, Bryan wrote that 'some at least of the fatalities could have been prevented if the results (of his and Williams' research) could have been sooner placed in the hands of those developing the problem on the experimental side' (University College of North Wales 1910, 16–17).

Broadly speaking, the analysis of stability in any dynamical system sets out to resolve whether or not that system, if its state of equilibrium is disturbed, returns to its original state (*stability*) or whether its configuration changes dramatically as a result (*instability*). Getting the algebra right—the signs, above all—is critical, the more so in the analysis undertaken by Bryan and Williams, which was quite literally, *an algebra for survival*. A solitary slip in sign might indicate stability when the reality was instability and uncontrolled flight, a combination that all too often had a fatal outcome. Applying Routhian formalism to determine the stability of flight called for scrupulous attention to detail, with independent working of the algebra not just advisable but vital. Interestingly, though Bryan made no bones about his aversion to algebra (Bryan 1915), that of itself did not necessarily mean his algebra was suspect. However, it seems that other concerns about Bryan's work—if in a different context—surfaced that same year, 1902.

Until he took up the question of stability in aerodynamics, Bryan's principal research interest lay in thermodynamics. Ludwig Boltzmann, doyen of late nineteenth century theoretical physics, held him in high regard and little doubt had a hand in Bryan being invited to contribute a review of thermodynamics to the *Enzyklopädie für Mathematische Wissenschafften,* the benchmark of its day for all things mathematical. The editor of *Band V,* on mathematical physics, was Arnold Sommerfeld, himself the most mathematical of physicists, recently appointed Professor of Mechanics at Aachen. Set on persuading Bryan to undertake the review, Sommerfeld made his way to Bangor in autumn 1899, bringing word of the award of the Gauss medal by the German Mathematical Society. Medal and invitation alike were manifestations of Bryan's standing in the world of German mathematics and physics (Boyd 2011, 2012, 2017).

By the time Sommerfeld received Bryan's manuscript in 1902, he may have begun to wonder if he had chosen wisely. Writing to the Dutch theoretical physicist, Hendrik Antoon Lorentz, in January 1903 to congratulate him on the award of the 1902 Nobel Prize in Physics, Sommerfeld soon went on to discuss shortcomings in Bryan's article, apologising to Lorentz (in his capacity as referee for Bryan's contribution) for the trouble taken over the many corrections he had had to make (a lengthy report by Lorentz on Bryan's manuscript is preserved in the Deutsches Museum at Munich) and expressing surprise 'that Prof. Bryan could deliver an ms. that on many essential points was both unclear and incomplete … So many mistakes slipped through (in translating)' (quoted in Kox 2008).

Had Bryan's laxity over points of detail in his review of thermodynamics carried over into his analysis of glider stability, who knows what the outcome might have been (Bryan and Williams 1904a). It was fortunate therefore, that Williams was on hand in 1902 to carry out the stability analysis independently, and only right that his contribution be recognized on a par with that of Bryan and its outcome referred to as the *Bryan-Williams equations.*[3] Their equations identified two oscillatory modes in the longitudinal motion, of distinctly different nature, one *fast,* the other *slow.* The slow mode, the so-called *phugoid* mode, is governed by the interchange between kinetic and potential energy of the aeroplane, in other words, a periodic trade-off between airspeed and altitude, over which the aircraft maintains essentially a constant angle of attack. The high-frequency mode, on the other hand, derives from an 'internal' perturbation in pitch (*internal*, in the sense that the aircraft pitches about its centre of gravity), with a natural frequency in the range 1–10 rad/s. In contrast to the phugoid mode, changes in altitude and airspeed are of little significance on the timescale of the high-frequency mode; what is significant is the rapid variation of the angle of attack.

Williams' experiments with model gliders

Disentangling individual contributions to joint research is often problematic—but in the Bryan-Williams collaboration, we do have some insights from an early paper by Bryan in which he described making model gliders from paper and getting these to fly:

> Gliders have an obstinate habit of turning on one side instead of flying straight … but even after the glider has been properly balanced a certain amount of skill is necessary in projecting it at the proper angle and with the proper speed in order to make it fly well, and let it be remembered, every failure would represent a fatal accident on a flying machine. Instead of sailing uniformly, the glider will, as a rule, describe a series of undulations … The general form of all these curves can be easily explained by the variations of the pressure with the relative velocity of the wind, and of the position of the centre of pressure with the inclination; and I would suggest that a series of instantaneous photographs of the path of the glider … would afford an interesting means of verifying the laws of aerial resistance. (Bryan 1897)

Not only had Bryan practised throwing paper gliders by 1897 but he saw clearly the need to photograph their trajectories, even if that was beyond him at the time. The wait was worthwhile; not only did Williams have a clear understanding of glider motion and so knew what he was looking for, but photography was his lifelong passion. By 1903, he assembled gliders using plane aerofoils in different configurations, typically in pairs, one behind the other, either with a tail aft, or a canard forward, of the wing. The first corresponded to that adopted by the pioneers of

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[3] Bryan himself was punctilious in referring to the Bryan-Williams equations and while Williams' contribution is generally credited in aeronautical sources, as in the early account by von Mises (1945) up to more recent literature (cf. Culick 2001), usage in historical sources is mixed. Abzug and Larrabee (2002) do cite Williams, but thereafter revert to a description of 'Bryan's contribution'; others, Bloor (2011) being just one example, make reference solely to Bryan.

flying, Cayley (1809, 1843) and Pénaud (1872) who were greatly influenced by the flight of birds; Lilienthal (1889) and Pilcher too, designed their gliders with tails aft. Of these, only Pénaud showed any clear—if qualitative—understanding of the role played by the horizontal tail. Though unknown at the time to Bryan and Williams, the Wrights used a canard forward of the wing, seemingly on grounds that sight of it was more conducive to effective control. For the Wrights, control was what counted; they had no clear understanding of the dynamics of stability as such (Culick 1988; Boyd 2011). By contrast, Bryan and Williams understood that for a configuration with the tail inclined at an angle to the wing, the centre of lift could be made coincident with the centre of gravity, so ensuring that there was no pitching moment. In their models, the tail was set at an angle to the wing, typically 5°–10°. The ratio of the areas of the surfaces varied from 10:1 to 1:1 (1904b). Regrettably, what's missing from their paper is any detail about the models as such—no photograph of the aerofoil combinations used, nor any mention of what they were made from. F.W. Lanchester, an engineer who built and flew gliders at about the same time, used mica (Lanchester 1908). It seems however, from a later and little-known paper by Williams (1904), that the Bangor models were paper-made.

Williams' unique contribution to the experiments on the stability of flight lay in the method he devised to record the trajectories, which can only have been perfected after a deal of trial and error. Making Bryan's 1897 suggestion work in practice called for both ingenuity and skill. It involved attaching a piece of magnesium wire to the model, which was then set alight before the glider was launched. Throwing a paper model smoothly is tricky enough; with a light source attached that burned for ten seconds or thereabouts, there was no room for error. Nonetheless, Williams succeeded in photographing trajectories from different combinations of aerofoils. A slotted disk rotating at a constant velocity was placed in front of a photographic plate, the trace of the path flown being then recorded as a sequence of dashes. From these images, Williams was able to estimate the velocity of the model (Bryan and Williams 1904a, 1904b).

Figure 1 shows model trajectories for cases in which the aerofoils are at an inclination of 10° to one another. That in Figure 1(a) shows the 'undulations' that Bryan had first seen in 1897. Now he and Williams had succeeded in identifying these with the slow mode, the so-called *phugoid* mode, from the stability analysis. The fast mode

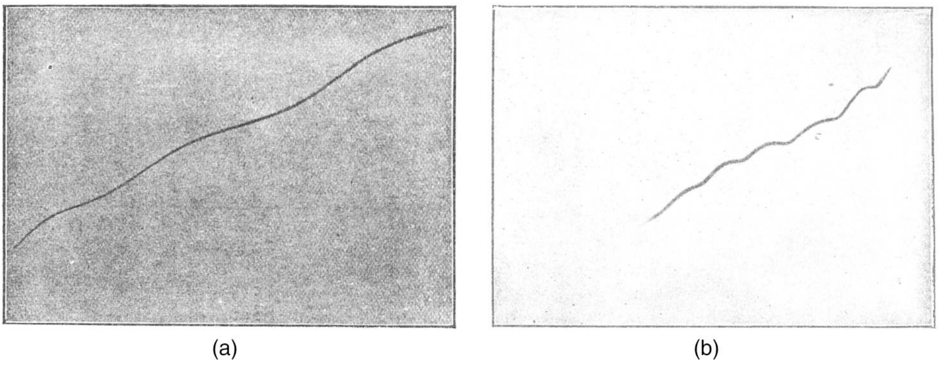
(a) (b)

Figure 1. (a) In this case, the glider consisted of two square aerofoils at an angle of 10° to one another. This trajectory (some 24 feet long) corresponds to the phugoid mode (Bryan and Williams 1904b). (b) Glider trajectory with plane aerofoils at 10°. Here, the modulation is due to excitation of the high-frequency mode (Bryan and Williams 1904b)

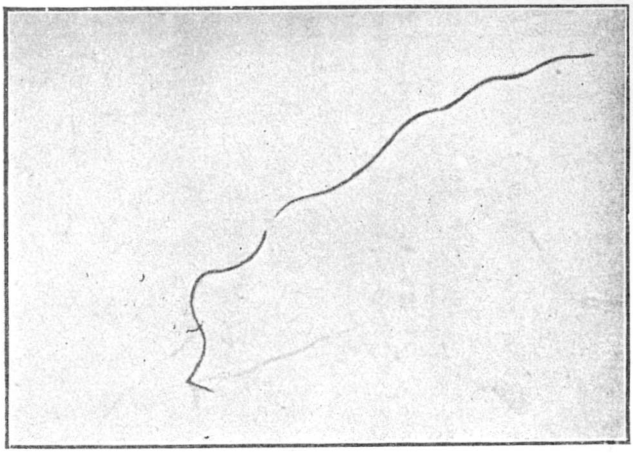

Figure 2. Trajectory corresponding to a twin-aerofoil model, showing the short-period mode excited but under conditions that breach the stability criterion, resulting in growth in amplitude of the mode (Bryan and Williams 1904b)

in Figure 1(b), on the other hand, had not been seen before. Their analysis established that gliders with two aerofoils, one behind the other, inclined at a small angle, are stable provided the velocity of the glider exceeds a threshold that depends on the dimensions and alignment of the planes, being lowest for a rear aerofoil significantly smaller than the other. Figure 2 shows a trajectory for a twin-aerofoil glider, in which the short-period (fast) mode is excited but under conditions where the stability criterion is violated (Bryan and Williams 1904b).

The data in Figures 1 and 2 were published, along with a paper read by Bryan to the Aeronautical Society at its December 1903 meeting, in the Society's journal the following year, together with plots of the time-evolution of unstable trajectories predicted by theory (Bryan and Williams 1904b). Curiously, none of this had been mentioned in the paper published in the *Proceedings of the Royal Society* in January 1904 (Bryan and Williams 1904a), whether because the experimental data was incomplete at the time that paper was read (June 1903) or for other reason, we do not know. Since Williams' photographs lent real support to the theory, their omission is the more puzzling. Whether their inclusion would have led to more ready acceptance of the theory is a matter of conjecture. What is incontrovertible is that in 1904, neither mathematicians nor practical men saw the paper for what it was, a truly landmark contribution that was to serve as a foundation for all subsequent analyses of stability of flight. In the words of Professor Fred Culick (1988), not until Bryan and Williams' work was it widely understood that an aeroplane can be made longitudinally stable with 'either an aft or forward surface'.

The years away: 1903/4, Glasgow post-Kelvin

By the time the stability paper appeared, Williams was already into his second term at Glasgow. Natural Philosophy at Glasgow was still very much the department William Thomson, Lord Kelvin, had dominated for more than half a century; indeed, he was still a presence when Williams arrived in September 1903. Kelvin had had more than a passing interest in physics at Bangor, having formally opened the first physics and chemistry laboratories at the College in 1885 (Thomson 1894). Whether he came to Williams' lecture on aerial navigation to the Royal Philosophical Society of Glasgow in January 1904 is doubtful. His views on flying were decidedly Luddite; invited to become a member of the fledgling Aeronautical Society by its president, Baden-Powell, in 1896, he had declined, replying 'I have not the smallest molecule of faith in aerial navigation' (Thomson 1896).

The Department of Natural Philosophy, post-Kelvin, was in a state of transition; after such a long tenure by one of the towering figures of nineteenth-century physics, change was overdue. In one sense, the appointment of Gray was hardly forward-looking. Though a gifted teacher, Gray had done little original work, leaving him ill-equipped to chart a new course for his department at a time when physics was set to make a quantum leap into the twentieth century. After a year in a department, better equipped than that at Bangor to be sure, but in other respects not that dissimilar, Williams was set on broadening his horizons. We have not been able to trace who, or what, set him on course for the next port of call on his scientific journey, the Research Laboratory in Physics at the University of Munich, directed by Wilhelm Conrad Röntgen the first Nobel laureate in physics in 1901.

Munich 1904–1906: a brave new world

It was by any consideration a bold choice. Capital of conservative and Catholic Bavaria though it was, *fin de siècle* Munich, with bohemian Schwabing at its heart, was a far cry from the slate galleries of Penrhyn and the dreigh gantries of Clydeside. The Research Laboratory in Physics, too, must have been a revelation, with facilities outstripping anything Williams had known.

By the late 1800s, the industrialist Siemens had been pressing for the establishment of research institutes in physics in German universities and though one had been equipped at Munich by 1894, it was on a scale that left it trailing Berlin, Leipzig and Heidelberg. Röntgen's appointment was intended to propel Munich up the ladder of German physics (Callan 1985). About the time Williams joined him, Röntgen was set on making an appointment to the chair in theoretical physics, vacant since Boltzmann's departure to Vienna ten years before, negotiations that would culminate in Sommerfeld's move to Munich in 1906.[4] Nor was physics unique in having professors of the calibre of Röntgen and Sommerfeld. Alfred Pringsheim, professor of mathematics, was not only a distinguished mathematician but heir to a fortune from Silesian coalmines and railways that supported a lavish lifestyle in Pringsheim Palace, Figure 3(a). He and his wife, the actress Hedwig Dohm, were noted patrons of the arts.

Unsurprisingly, professors of such distinction attracted students in kind, among them Abram Joffé, from St. Petersburg, with others, notably Karol Szlenkier from Poland, J.P. Donaghey from Ireland, along with two Pringsheim children, Katia and Peter (Rosenbusch and van Eekelen 2019). Szlenkier came from a family renowned for their patriotism, philanthropy and enlightened socialist capitalism;

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[4] Boltzmann himself had been invited to return to Munich more than once, and Lorentz too, had been approached. Each in turn had recommended Sommerfeld, then Professor of Mechanics at Aachen. It was a recommendation that resonated with Rőntgen, not least on account of Sommerfeld's developing interest in X-ray diffraction. (Seth 2004).

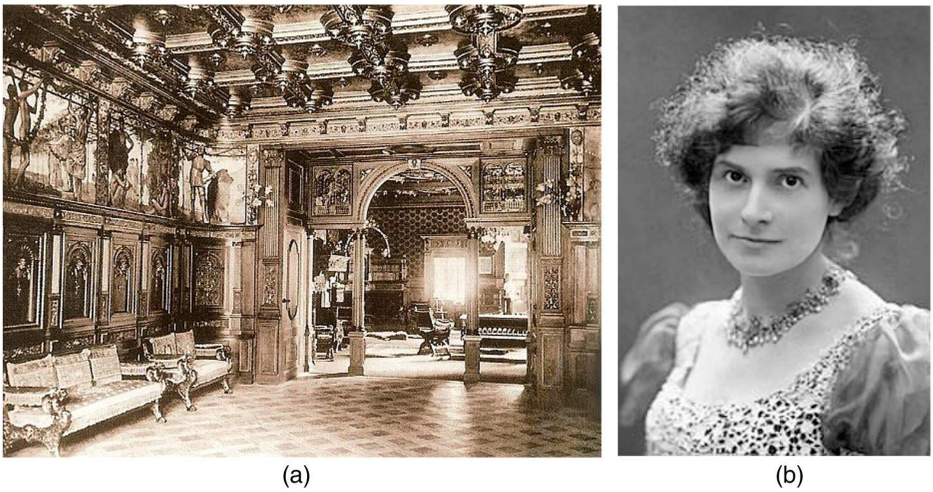
(a) (b)

Figure 3. (a) Pringsheim Palace, Munich, (Wikimedia Commons). (b) Katia Pringsheim (Wikimedia Commons).

sadly, he was brutally murdered by the Gestapo in June 1944. Katia Pringsheim, Figure 3(b), though a gifted student, gave up her studies to marry Thomas Mann in 1905.

The undoubted star was Joffé, shown in Figure 4 (Semenov and Kondratiev 1961; Joffé 1967). Born in the Ukraine, he was destined to become one of the greats of Soviet science, debarred by Kremlin diktat from nomination for a Nobel prize. Such was his standing that he could turn down with impunity an invitation from the Kremlin in 1943 to direct development of a Soviet atomic bomb.[5]

Regrettably, little record survives of Williams' doings in Röntgen's laboratory.[6] He would first have had to set about learning the language; with some fluency in German, he may have demonstrated to students in the undergraduate laboratory. Not only was the 23-year-old Williams a seasoned experimentalist, maybe even more important was

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[5] In early 1943, Stalin delegated oversight of Soviet work on an atomic bomb to V.M. Molotov, then Foreign Minister, whose first task was to nominate a physicist to direct the work. Molotov recalled in a biographical memoir (Dee 2007) that he first invited Kapitsa to undertake the task. When Kapitsa declined, he turned to Joffé who 'replied somewhat unclearly'. Next in line was Igor Kurchatov, one of Joffé's former students, who accepted and was duly presented to Stalin and had his appointment confirmed on 10 March 1943 (Medvedev 1999). Seemingly at variance with Molotov's memoir is an account by Craig and Radchenko (2008), claiming that Joffé had lobbied for an atomic bomb at the highest reaches of Soviet power but, with no background in nuclear fission and being too busy with other duties to undertake oversight of the work himself, proposed Igor Kurchatov, who had won acclaim in scientific circles for his work on fission at the Leningrad Polytechnic Institute, directed by Joffé.

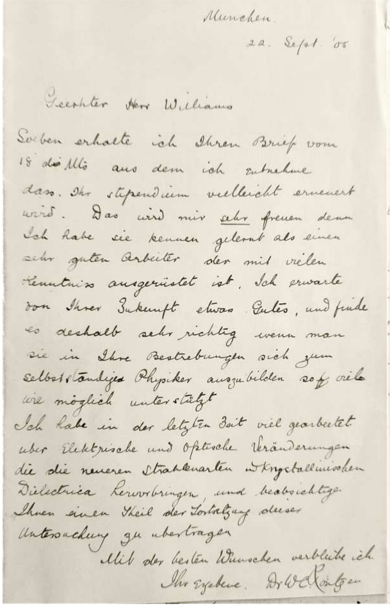
[6] An undated letter from Williams addressed to the Senate of the University College of North Wales applying for the renewal of his fellowship for a third year, supported by a testimonial from Röntgen, simply states: 'I have been working in the Physical laboratory of the University of Munich under Professor Röntgen' but gives no hint as to what he had been doing; he goes on:

> As you will see from the enclosed letter he is very desirous that I should come to Munich for another year so that I may take part in an investigation which he himself has been carrying on during the last two years on the relaxation of dielectrics. (Williams undated)

Figure 4. Abram Joffé in his early years (museum – digital Rheinland).

his hands-on knack of knowing how to get what he wanted from whatever he was doing in the lab.

In a letter to Williams dated September 1905 (Figure 5), Röntgen wrote: ‘I know you to be a very good worker equipped with a lot of knowledge … ’ (Röntgen 1905). Seemingly Röntgen had first-hand experience of what Williams was capable of,



Munchen.
22. Sept. '05

Geehrter Herr Williams

Soeben erhalte ich Ihren Brief vom
18 d. Mts aus dem ich entnehme
dass. Ihr stipendium vielleicht erneuert
wird. Das wird mir sehr freuen denn
Ich habe sie kennen gelernt als einen
sehr guten Arbeiter der mit vielen
Kenntniss ausgerüstet ist, Ich erwarte
von Ihrer Zukunft etwas Gutes, und finde
es deshalb sehr richtig wenn man
sie in Ihre Bestrebungen sich zum
selbstständiges Physiker auszubilden so viel
wie möglich unterstützt
Ich habe in der letzten Zeit viel gearbeitet
uber Elektrische und Optische Veränderungen
die die neueren Strahlenarten in Krystallinischen
Dielectrica hervorbringen, und beabsichtige
Ihnen einen Theil der Fortsetzung dieser
Untersuchung zu ubertragen
Mit der besten Wunschen verbleibe ich.
Ihr ergebene. Dr W C Röntgen

Figure 5. Röntgen letter, 1905 (Bangor University Archives and Special Collections).

insights that may have come from engaging with his research students in the Praktikum, an institution introduced into German universities in the nineteenth century to provide hands-on experience in physics (Rosenbusch and van Eekelen 2019). Röntgen's Praktikum was a set of a hundred tasks aimed at honing his students' experimental skills—in glassblowing, electric circuitry, spectroscopy and the like—technique that he himself excelled at. It was a requirement, demanded of all who came to work with him. Though not a candidate for a higher degree, it's likely that Williams too had to undertake a *Prüfung durch Praktikum*, and small wonder that his innate experimental ability made a lasting impression on Röntgen.

This September letter is interesting too, in that Röntgen gives the briefest of outlines of the research he would like Williams to be involved in, writing:

> I have been doing a lot of work lately on the electrical and optical changes in crystalline dielectrics, brought about by the new types of radiation and it is my intention to entrust you with part of the continuation of this investigation. (Röntgen 1905)

Though given to repeating his students' measurements to convince himself of the accuracy of their work, the only one Röntgen actively collaborated with was Joffé. The fact that he now planned to entrust Williams with part of that work is a measure of the promise he saw in his visitor from Wales. Williams' letter to the Senate at Bangor, petitioning for a renewal of his fellowship for a third year elaborates:

> the subject of the research is the electrical conductivity induced in dielectrics such as quartz and rocksalt by exposure to the action of Röntgen and radium rays … a wide field for investigation which is likely to lead to important discoveries regarding the mechanism of electrical conductivity in such bodies. (Williams undated)

The mystery is that nothing came of it, with no clear pointers to why the collaboration on dielectric crystals didn't materialize. Crystals were a lifelong interest of Röntgen, Joffé's thesis having been devoted to elastic effects in quartz (Joffé 1906). Having received his doctorate in June 1905 with the commendation *summa cum laude*, a distinction rarely conferred, Joffé was set on going home to take part in the1905 Revolution. However, following setbacks to the cause in late summer, along with anti-revolutionary pogroms against Jews that erupted across Russia that autumn, he was persuaded by Röntgen to delay his departure. It's a fair assumption Joffé would have had more than a passing interest in the work Röntgen had pencilled in for Williams, even to the point of feeling he had first refusal. Would there have been room for both at Röntgen's bench? Who is to say?

A short communication read to the Mathematical-Physical Section of the Bavarian Academy of Sciences by Röntgen, on 4 May 1907, may hold the answer. In this, Röntgen referred to the influence of X-rays on the behaviour of various so-called insulators, behaviour that had prompted him 'with the participation of Dr A. Joffé, to examine to what extent one could speak of an electrical conductivity of these bodies'—the very topic singled out for his collaboration with Williams two years before!

Whatever, or whoever, led Röntgen to a change of heart in autumn 1905, his relations with Williams remained cordial throughout. At the end of Williams' time in Munich he would write a warm testimonial in support of his visitor's application for

Figure 6. Williams at Davos, 1906 (Bangor University Archives and Special Collections).

an assistant lectureship at Bangor, mentioning that Williams had been his private assistant for two semesters.[7] Socially too, Williams seems to have been made welcome. While Röntgen and his wife lived a world apart from the Pringsheims, their lifestyle in a large apartment on the Prinzregentenstrasse was far from frugal (Rosenbusch and van Eekelen 2019). A semester break at Davos was part of their year and there they were joined from time to time by members of Röntgen's group. Figure 6 shows Williams on the piste at Davos in 1906, in a photograph that may just possibly have been taken by Röntgen, himself a keen photographer (Lee and Crean 2012).

Heimweh, hiraeth and home

Summer's end 1906 marked too the end of Williams' time at Munich. Without doubt, the aims of his fellowship had been met, his development as a scientist enriched by working under one of the leading physicists of his day. Aside from physics, he had had the experience of a lifetime, his two-year stay in Thomas Mann's *Athen am Isar*, a world apart from the Wales of his upbringing. But by now, a sense of longing, of *Heimweh*, of *hiraeth* for Gwynedd and the hills of home, called him back. Once back, he would have found the Bangor he left in 1903 seemingly little changed. But a closer look points to 1906 having been a year of change. By then, Bryan's long engagement with thermodynamics, nurtured by the interest shown by Boltzmann, had come to an end. Over the years, they had become close friends. Bryan was both shocked and saddened to get news of Boltzmann's suicide during a bout of depression, holidaying at Duino late that summer. In a sense, Boltzmann's death figuratively marked the end of Bryan's work on thermodynamics. From 1906

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[7] Röntgen wrote 'If he is given the opportunity of (undertaking) scientific work, he will excel in physics' (Röntgen 1905), and a prescient forecast it would prove, if not perhaps in the way Röntgen might have expected; there were to be twists and turns along the way.

on, what time he had would be given to aerodynamics and the stability of flight (Boyd 2012, 2017).

Early in 1906, Bryan had come to realize that the analysis of lateral stability entrusted to Ferber two years before, had as he put it, ‘fallen very wide of the mark when applied to aeroplanes’. Disillusioned by this setback, he was at the same time irked by the seeming lack of urgency at the National Physical Laboratory in providing wind-tunnel data, vital if the Bryan-Williams’ equations were to become a practical tool for predicting the stability (Bryan 1911).

Back in Bangor, Williams was no doubt glad to be home but there must have been times, thinking about what research might be possible once time permitted, when he looked back at the facilities he had enjoyed in Röntgen’s laboratory by comparison with the limited resources at Bangor. Were there times, too, when he missed the buzz of a lively research environment, not to mention the benefit of guidance from a physicist of Röntgen’s stature? Be that as it may, there is no evidence of his ever having gone back to Munich, neither of any correspondence with Röntgen after 1906. While there must have been days when life at Bangor seemed quiet by comparison, for the foreseeable future at least, his duties as assistant lecturer left him little time for original work.

The Bamboo Bird: a long hatching

What it was that led William Ellis Williams to set about building an aeroplane, we do not know; nor do we know when he began work on it, though the long vacation of 1909 seems likely. By then, flying could be said to be well and truly in the air (Hoffman 2003). Barely three years on from the exploits of the brothers Wright in December 1903, the Brazilian Alberto Santos Dumont had made the first powered flight in Europe on 23 October 1906, in his *Oiseau de Proie*, from the Bagatelle sports ground in Paris. But the real boost to aviation in Europe came two years later, in August 1908, with the arrival in France of Wilbur Wright. Over the next few months, Wright made a number of exhibition flights from the Hunadières racecourse south of Le Mans and from Camp d’Auvours, to the east.[8] His exploits ensured that 1908 was the year the aeroplane truly caught the public imagination in Europe as people thronged to see for themselves the wonder of flight. The year ended with Wright completing a flight of 76 miles in just under two hours and twenty minutes on 31 December earning him the Michelin Cup and prize.

That year too saw the first powered flight in Britain on 16 October 1908, when Samuel Cody flew a BAA No.1 aeroplane at Farnborough (Crouch 2008; Boyd 2017). Nearer home, Charles Horace Watkins was at work on his *Robin Goch* (*Red Robin*). Seemingly he had begun building the engine in his workshop at Maindy, Cardiff in 1907, though exactly when *Robin Goch* first flew was neither witnessed nor recorded. Whether Williams ever got wind of Watkins’ *Robin* is hard to say. In any event, Williams’ purpose was not simply to fly, but to fly with purpose, a purpose that required making measurements on an aeroplane in steady flight. In so

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[8] On 8 October 1908, Wilbur Wright carried a passenger, Charles Stanley Rolls, from Monmouth. Rolls lost no time in buying a *Wright Flyer*, built under licence by Short Brothers, and went on to log more than two hundred flights, including the first (non-stop) return crossing of the English Channel on 2 June 1910, weeks before the crash that claimed his life on 12 July (Baines 2007).

doing, he was following a lead set by Capt. Ferber in his airborne experiments at Chalais-Meudon (Chanetz 2017). But in contrast to Ferber, whose work had the backing of the French state, Williams had a pressing need to find funding to underwrite his project. In this he had a stroke of luck, in the person of Henry Rees Davies (1861–1940), one of a wealthy family of ship owners and timber merchants.[9] The Davies family ran a fleet of vessels sailing from Menai Bridge, carrying emigrants and slate to the Americas, with return cargo of timber, grain and guano. Their ships were Liverpool-built; the last of them the clipper *Merioneth*, could make San Francisco (round Cape Horn) in under 100 days. Davies had graduated from Cambridge in 1884. Twenty years on, he was a prominent member of Council at University College, Bangor and a generous benefactor to its library. As a member of Council, he would have known of Bryan's concern with the stability of flight; indeed, he may well have been one of the few people in Wales to have had some grasp of the issues involved, from a basic understanding of mechanics, having read the Natural Sciences Tripos at Cambridge. Given that merchant shipping underpinned the family wealth, he may have had more than a passing interest in the new horizons opening up in the air. It's hardly a surprise that Henry Davies seized the opportunity to underwrite the initiative of a young member of staff, not least since a Bangor-built aeroplane would be a high-profile contribution to an endeavour that had caught the public imagination.

As the work got underway in late 1909, Davies turned out to be no mere underwriter. His correspondence with Williams reflects an enthusiasm for what he called their joint enterprise—an enthusiasm all the more surprising given Davies was then in his late forties, fully twenty years Williams' senior. It appears that it was he who coined the name, *Bamboo Bird*. Much of the structural work was done in a basement laboratory of the original College building, a converted hotel, the *Penrhyn Arms*, overlooking Port Penrhyn (Figure 7). Figure 8 reproduces the Aeroplane Account, kept by Williams showing the cost of materials and labour incurred from October 1909 to March 1910, the total just over £138, equivalent to about £21,000 today. The joints were cast from aluminium by Williams himself and the propellor shaped from a piece of ash, supplied by Watkin Jones' yard in Bangor. Aside from the engine, bamboo was the dearest item, hardly surprising given that the aircraft frame, A-shaped in cross-section, was 37 feet long. The wingspan was some 32 feet, with an area of about 200 square feet. The wings were modelled on an Eiffel No.7 aerofoil —used in the Antoinette monoplane that Bryan had seen under construction on his visit to Blériot's factory in 1907—the top surface curved, the under-surface plane. The tail, triangular in section, was sizeable, at around 45 square feet.

As work went on, Henry Davies' correspondence with Williams revealed the measure of his engagement in their joint enterprise. Writing in early April 1910, he questions the position of the centre of gravity and the need for stability, commenting on the inherent instability of Blériot's monoplane. In reply, Williams corrects Davies' views on the location of the centre of gravity, enclosing a copy of a note on that very topic that he had recently published (Williams 1909). Davies

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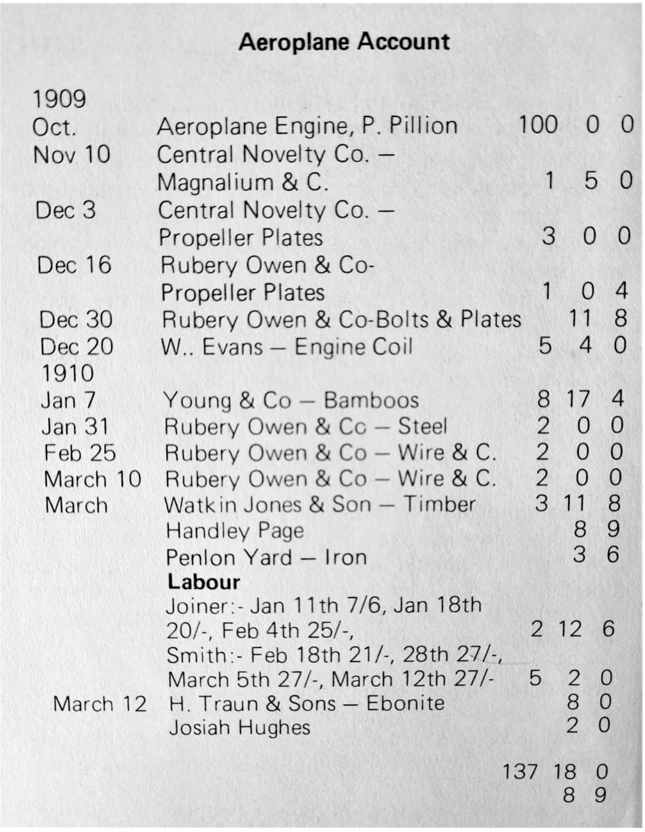
[9] This, and succeeding, paragraphs rely heavily on the Davies–Williams correspondence in Bangor University Archives and Special Collections, BMSS/20801; for Davies' life see Jenkins, R. T., (1959). Davies, Henry Rees (1861–1940), antiquary. *Dictionary of Welsh Biography.* Retrieved 6 October 2023, from https://biography.wales/article/s-DAVI-REE-1861.

Figure 7. *Penrhyn Arms*, the original College building overlooking Port Penrhyn (Bangor University Archives and Special Collections).

wrote again on 8 April, expressing reservations about Williams' arguments. The sub-plot to the issue of stability is interesting. Williams shared Bryan's frustration with the sluggish response of the National Physical Laboratory in providing input needed to prime their equations in a way that would allow stability of individual aircraft to be predicted. Getting at least some of this data from a test flight of *Bamboo Bird* was foremost in his mind. Davies' concerns, on the other hand, were rooted in hopes he harboured for flying. Having been to the Aero Exhibition at Olympia in March 1910, he sounded Williams out, asking if he would be willing to design a plane to be built by Short Brothers. 'I should like to have a try on a safe machine', he wrote,

> I should like to put up to, say £700 (over £100,000 today) as my contribution to the Upward Movement. I am ambitious to fly a few yards myself but not on a home-made bird. Please don't mention the matter, or my people would try to stop it off if it came to their ears.

Williams didn't rise to the bait. Instead, they got down to the more pressing concern of finding a suitable site to fly from. Davies thought the stretch of firm sand at Red Wharf Bay, on Anglesey, some two and a half miles in length, might do; Williams agreed. Davies approached the landowner, Sir Harry Verney, for permission to construct a shed adjoining the beach, near Llanddona, to house *Bamboo Bird*. Verney



Aeroplane Account

1909				
Oct.	Aeroplane Engine, P. Pillion	100	0	0
Nov 10	Central Novelty Co. – Magnalium & C.	1	5	0
Dec 3	Central Novelty Co. – Propeller Plates	3	0	0
Dec 16	Rubery Owen & Co- Propeller Plates	1	0	4
Dec 30	Rubery Owen & Co-Bolts & Plates		11	8
Dec 20	W.. Evans – Engine Coil	5	4	0
1910				
Jan 7	Young & Co – Bamboos	8	17	4
Jan 31	Rubery Owen & Co – Steel	2	0	0
Feb 25	Rubery Owen & Co – Wire & C.	2	0	0
March 10	Rubery Owen & Co – Wire & C.	2	0	0
March	Watkin Jones & Son – Timber	3	11	8
	Handley Page		8	9
	Penlon Yard – Iron		3	6
	Labour			
	Joiner:- Jan 11th 7/6, Jan 18th 20/-, Feb 4th 25/-,	2	12	6
	Smith:- Feb 18th 21/-, 28th 27/-, March 5th 27/-, March 12th 27/-	5	2	0
March 12	H. Traun & Sons – Ebonite		8	0
	Josiah Hughes		2	0
		137	18	0
			8	9

Figure 8. Aeroplane account kept by W.E. Williams showing construction costs 1909–1910 (Sloan 1989).

not only willingly consented but expressed his admiration at the enterprise shown by Williams.

When it came to transporting the *Bamboo Bird* from Port Penrhyn to Llanddona, Davies offered his launch to tow the plane round 'on a fine day', an important proviso, over an exposed stretch of water. The *North Wales Observer* reported on 12 August 1910 that 'part of the monoplane built by Mr. W.E. Williams had been removed to Red Wharf Bay, Anglesey, where experiments in aviation are to be conducted shortly'. Although work went on at the Aeroplane Shed, Llanddona for the rest of that summer, the experiments flagged by the *Observer* didn't materialize. Professor Taylor Jones, in his annual report to Council for 1910, wrote that 'unfortunately, owing to deficiencies in the motor, it has not as yet been found possible to make any satisfactory trials' (University College of North Wales 1910, 17–18).

Not until the following summer could another attempt be made to get *Bamboo Bird* airborne. Taylor Jones' 1911 report to Council records that '[d]uring the recent long vacation Mr. Williams hired a more powerful motor, which enabled him to perform some successful flights' (University College of North Wales 1911, 19). Regrettably, dates and times were not logged, though the flights likely took place in late August or early September. Though *Bamboo Bird* had been proved airworthy at last, no experiments were carried out in 1911. The summer of 1912 was one of the wettest on record; that, compounded with endless haggling over the hiring of engines, meant that no further flying from Llanddona took place until Summer, 1913.

Pilot and aero-physicist, 1913

If the decision in 1909 to build his own aeroplane was more a gradual dawning than any eureka moment, Williams had been clear from the outset that its purpose was to serve as a laboratory in the air. The *Liverpool Daily Post* wrote at the time (23 June 1910) that its aim was 'to be mainly in the furtherance of the study of stability and efficiency of flying machines and to obtain experimental data for a theory of their motion'. Ever since coming back to Bangor he would have been aware of Bryan's growing impatience with the National Physical Laboratory over wind tunnel tests. Where the NPL was dilatory, aeronautical work in France and Germany at the time was forging ahead on a number of fronts. Bryan was alert to developments both in Paris, thanks to his links with Ferber, and at Aachen, where Hans Reissner, Sommerfeld's successor as professor of mechanics, was building his own aeroplane, with help from his colleague Junkers.

Bryan indeed had paid a visit to Blériot's factory outside Porte Maillot in July 1907 accompanied by Ferber (Bryan 1911), whose commitment to aviation knew no bounds; not only had he taken up the challenge of developing a theory of lateral stability —incorrectly, as it happened—he designed, built and flight-tested his own aeroplanes. He was the very embodiment of his mantra: 'Concevoir une machine volante n'est rien; fabriquer est peu, l'essayer est tout' (*to design a flying machine is nothing; to build one, nothing much; but to try one out in the air is everything*) (Ferber 1904). As early as 1904, Ferber had built a test-rig with an aeroplane suspended from a sliding carriage along a cable under tension supported by pylons and by 1906, had been seconded from the army to the aeronautical research centre at Chalais-Meudon to continue experiments on his so-called *aérodrome* (Maurin 1912). At the time of Bryan's visit, Ferber was building a bamboo-framed bi-plane powered by an Antoinette III engine, the so-called No. IX that flew the following year—the aeroplane in which he lost his life on 22 September 1909 at Boulogne, preparing to fly the Channel (Besançon 1909).

Blériot's factory was not far from the Champs de Mars where Gustave Eiffel had built his own aeronautical laboratory in 1905; it was here that Eiffel designed and built his first wind tunnel in 1908, with the first tests conducted the following Spring (Eiffel 1911). He would go on to complete over 4000 tests in all,[10] before moving his lab to Auteuil in 1912. It was a body of work that not only set new standards for aeronautical research but helped establish a firm scientific base for the achievements that underpinned the supremacy of French aviation up to the outbreak of war in 1914, at the same time cementing Eiffel's reputation as France's leading aerodynamicist. His achievements were in large part a legacy of his genius as an engineer. His wind tunnel was no copy of the designs of the day but incorporated a variety of novel features, above all, the test chamber. Chief among Eiffel's concerns was how a wall might

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[10] Eiffel's output stood in sharp contrast to the leisurely pace of wind tunnel work at NPL that provoked repeated despairing outbursts from Bryan. Indeed, Bryan's was not a lone voice; the editor of *Aeronautics*, writing in 1916, was more forthright still: 'if one were to ask Eiffel for the air resistance of an airship hull, the job would be done in a couple of days in what would take the NPL heaven knows how many weeks' (quoted in Ledeboer 1916).

affect air flow over a model. His solution was to construct a sealed experimental chamber with inflow and extraction ducts on opposite walls, enabling him to have a column of air flowing through the test chamber that did not have direct contact with any wall.

While many of the tests carried out were routine, others were key to the development of aerodynamics, none more so than Eiffel's measurements of the pressure distribution across an aerofoil (Eiffel 1911). These measurements, made in 1910, were very landmarks in experimental aerodynamics, providing insight into lift and drag on an aerofoil. Eiffel established that lift was a consequence of lower pressure across the top surface of an aerofoil rather than from higher pressure underneath. He showed too, that close to the leading edge, pressure changed rapidly. Indeed, credit for first determining pressure across an aerofoil is rightly attributed to Eiffel, for his measurements by means of small orifices bored in the aerofoil surface, connected to a manometer, the pressure at each being determined *sequentially*.

Pressure distribution, as Eiffel had recorded on models in his *Soufflerie,* was precisely what Williams intended to measure across the wing of his *Bamboo Bird*, once proving trials were completed. The burning issue was the question of scaling; in 1910, no one had any idea whether pressures measured across a model aerofoil corresponded to the actual pressures on aeroplane wings. Would a model, a fraction the size of an aeroplane, in a wind tunnel with air flow that same fraction of the speed of the aircraft, experience identical lift and drag to that of the aeroplane itself? Since pressure and drag are macroscopic manifestations of molecules in motion and since molecules are beyond scaling, the answer was far from clear. Good empiricist that he was, Eiffel turned to fudge factors—*augments*, he called them—to get agreement between his test reults and airborne measurements of thrust. Ferber's mantle at Chalais-Meudon had fallen on Commander Emile Dorand. Dorand had designed and built his own experimental bi-plane in 1911 (Nordmann 1916). Following tests on a scaled model in Eiffel's *Soufflerie*, Dorand's aeroplane was flown by *Pilote-aviateur* René Labouchère, at the age of twenty already an experienced pilot, then completing his military service at Chalais-Meudon. Measurements of propellor thrust, aircraft speed and angle of attack were logged but there is no record of pressure distribution across the wings having been made.[11]

One for whom Eiffel's *augments* had little appeal was Armand de Gramont, Duc de Guiche, a Sorbonne-educated physicist, (Maurin 1912; Marchis 1917). De Guiche had the means to fund his own laboratory and the experiments he undertook between 1910 and the outbreak of war in 1914 contributed materially to a better understanding of the aerodynamics of the day, not least the key issue of pressure distribution. He neither took to the air nor used models in wind tunnels, but instead mounted metal plates on his car, high enough to ensure that turbulence from the motion of the car

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[11] Writing about experiments undertaken on an aeroplane in flight, a commentator at the time (Marchis 1917) noted:

> Unfortunately, the field of such investigation is limited. It cannot be carried through at the will of the experimenter, that is to say, of the pilot, who must first of all guard against danger of fall. Such experiments give complex results, often difficult of analysis.

Indeed, Labouchêre's military service had been postponed on account of injuries sustained in a crash in late 1910.

did not affect his measurements. Like Eiffel, de Guiche drilled orifices across the plates but where Eiffel had measured pressure at each orifice in turn, de Guiche recorded pressures at each *simultaneously*, on both front and rear sides of the plates, so generating a more credible profile. As many as twenty orifices were used, each connected by rubber tubing to twenty little manometers mounted side by side in a frame (Figure 9). Readings from these were recorded photographically by placing them in a chamber in which their open arms were exposed to atmospheric pressure. De Guiche published his results privately, in a series of *Essais d'Aerodynamique* between 1911 and 1914, with work on pressure distribution appearing in the first and second series (1911, 1912).

Interestingly, De Guiche's 1912 publication is the sole reference cited in Williams' own account of de Guiche's work, published two years later (Williams 1914). It seems likely that Williams came to know about de Gramont's work from the *Aeronautical Journal.* The July 1911 issue carried a review of the first series of de Gramont's *Essais* (de Gramont 1911), in which the reviewer commended the care taken in making the measurements, noting:

> The main object of the author was to ascertain the *distribution* of the pressures, as this is a point to which experimenters, as a rule, have paid very little attention, and most elaborate work was done in this direction. (J D F 1911)

The publication of de Gramont's results could hardly have been more timely. With *Bamboo Bird* grounded in 1912, in part due to Williams' difficulties in hiring a suitable engine, he applied himself to adapting de Gramont's multi-manometer to the vastly more challenging environment of *Bamboo Bird*. Given that the stability characteristics of the aeroplane were something of an unknown quantity, Williams would have been under no illusion that once airborne, 'guarding against fall' was his overriding priority, with the consequence that obtaining manometer readings had to be as undemanding of attention as he could manage. In this he succeeded brilliantly, making full use of his experience and skill as a photographer. He devised a box camera of sorts to record the manometer readings; in his own words:

> In the front of the box instead of the lens a slit ½ inch wide was cut, extending about half the width of the box, and in front of it was placed a plate glass mirror inclined at an angle of 45° to the side of the box, so that it reflected light through the slit into the interior of the box. At the other end of the box were placed twelve glass gauge tubes for measuring air pressure and a couple of U

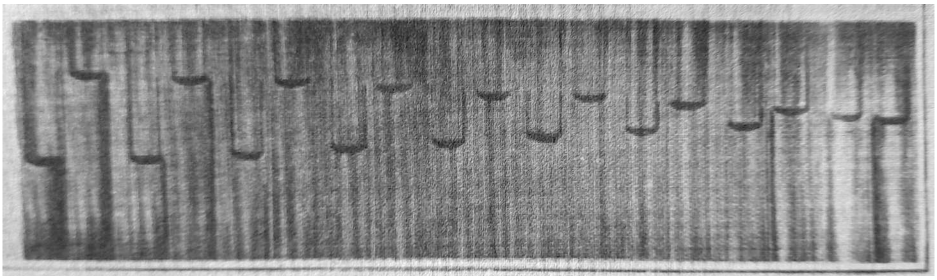

Figure 9. Photograph of multi-manometer array used by de Gramont (1911).

> tubes for measuring the angle of inclination … In the interior of the box two film spools were mounted so that a strip of bromide paper could be wound from one on to the other. The bromide paper passed behind the gauge tubes and was pressed against them by a spring. The gauge tubes were filled with petrol, and the light from the slit cast a sharp shadow of the surface of the liquid on the bromide paper, so that after exposure and development the difference of level of the petrol in the two limbs of the gauge could be measured to a quarter of a millimetre. One end of each gauge tube was open to the box and the other connected by rubber tubing to the holes in the wing. The inside of the box communicated by means of a wide tube with the interior of the wing and thus the gauges measured directly the difference in pressure between the two sides of the wing fabric. A gauge similar to the others connected to a Pitot tube served to measure the velocity of the machine. The Pitot tube was fixed to an upright screwed to the *cabane* of the aeroplane and was about 6 feet higher than the body, being placed well out of the propellor slipstream. A flap shutter was placed in front of the slit and could be opened from the pilot’s seat by pulling a string, an exposure of half a second being sufficient to give an image on the paper.
>
> … The aeroplane having lifted a few feet above the ground, was kept on as level a course as possible. After going a few hundred yards the shutter was opened and an exposure taken. The paper was then developed and fixed and the heights of the columns measured with a sliding vernier caliper.
>
> The section of wing is similar to Eiffel No. 7 and the pressure curve is of the same general form as the one given by him [Figure 10]. The suction on the upper part is greatest at the forward edge, it diminishes rapidly to a minimum at the second hole, then increases to a maximum near the middle of the wing, and finally diminishes nearly to zero at the back edge. The pressure on the lower surface is greatest at the forward edge and diminishes continuously across the wing. (Williams 1914)

Williams had also built a wind channel of his own, in which he tested a model wing, a tenth the size of that on his aeroplane, with holes drilled in the wing connected, in sequence, to a pressure gauge, the other end of which was connected to the static pressure tube of the Pitot tube. In his comparison of the pressure distributions in flight and in the lab, Williams applied *augments* of his own, scaling by a factor of relative speed of aircraft and airflow in his wind channel and by a shift to optimize agreement between the two distributions. He argues—not entirely convincingly—that this is required as the pressure inside the wing from which the pressures at the different points are measured depends on the positions of the orifices on the wing and is not necessarily the same as that in the static tube. Despite discrepancies at orifices 5 and 6, which he attributes to transitory wing warping at the instant of exposure, agreement is held to be satisfactory and Williams concludes that wind channel experiments may be relied on to give accurate results when applied to full-size aeroplanes (Williams 1914).

The pressure distribution was recorded on a flight at sunset on 3 September 1913, when conditions at Llanddona were as near perfect calm as one could hope for on an exposed stretch of coast. No flight log has survived, though the speed attained—37 mph—and the height at which measurements were made, were both recorded.

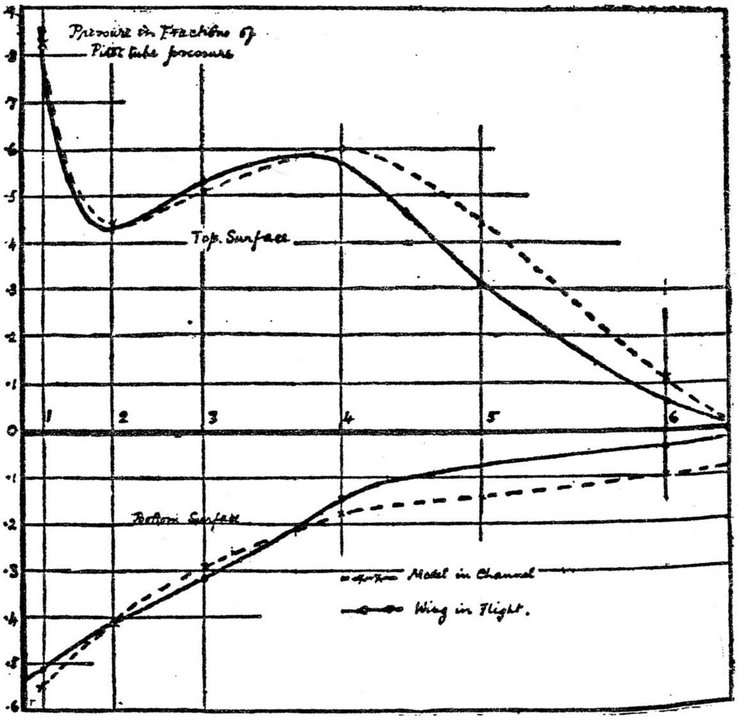

Figure 10. Pressure distribution across wing and model (Williams 1914).

Williams would have known that pressure measurements are exceedingly sensitive to quite small changes in the angle of incidence. We are told that *Bamboo Bird* was held on a level course at no more than 7 feet above the beach, which begs the question as to just what height Williams actually measured. He would have been aware not only of the trouble Eiffel had taken to counter the effects of walls in his wind tunnel design, and of the attention paid by de Gramont to possible perturbation of the pressure distribution on plates attached to his car by the automobile itself, as well as external effects from the test track. He concedes that the proximity of the ground may indeed perturb airflow over the wing but concludes that the effect would be insignificant as ‘the actual height of the wing above the ground is much greater than the span’ (Williams 1914). But since the design specification of *Bamboo Bird* shows a wingspan of 32 feet, this was manifestly not the case.

Puzzle we may about contradictions like this, wonder why pressure readings were logged on one flight only, recoil at the seeming foolhardiness of installing manometers filled with petrol in a cramped cockpit when a stray spark from the engine might have ignited a fireball like the one that would down the BE-2 a year later,[12] the fact remains

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[12] The BE-2 was an experimental aeroplane built at the Royal Aircraft Factory, Farnborough to support Edward Teshmaker Busk’s design of an airframe that embodied the principles of stability theory. Busk succeeded beyond all expectation; the BE-2 flew from Farnborough to Salisbury Plain, a distance of over 40 miles, under rudder control only. Tragically, Busk was killed on a flight on 5 November 1914, when a fuel leak ignited shortly after taking off from Farnborough (Boyd 2011).

that William Ellis Williams did something truly remarkable that late summer evening in a quiet corner of Anglesey.

Not only had he built his own aeroplane in a basement under the department of physics, in a university college, founded barely thirty years before, where resources were tight, he had done it virtually single-handed, albeit with indispensable funding from his benefactor, Henry Davies. Where Reissner was supported by Junkers' *Motorwerke* and Dorand had the resources of the state at Chalais-Meudon to call on, Williams was on his own. He bought the materials for *Bamboo Bird* himself, the timber he needed at Watkin Jones' yard down the road in Bangor, nuts and bolts, plate and wire from Rubery Owen at Darlaston. He built his own multi-manometer, fashioned on de Gramont's instrument, and recorded readings using a box camera of his own design. He put together his own wind channel. True, Dorand's bi-plane had flown before *Bamboo Bird,* though Dorand did not pilot it himself. Nor did he have the benefit of Williams' long apprenticeship as physicist, with a skill set shaped at Bangor and honed at Munich under a master-experimenter. Added to all that was the *need to know,* the *proof positive* of our title. Williams' work with Bryan on stability called for the input of resistance derivatives for it to become a practical tool and a key element of that input depended on knowing the pressure distribution across the wings of the aircraft.

Eiffel had led the way; an engineer to his fingertips he had succeeded in unravelling much of the physics of pressure distribution on aerofoils using models in his wind tunnels. De Gramont had added insights all his own. Others would follow in the wake of developments in France. Among them, Melvill Jones and Paterson (1913) carried out experiments on model aerofoils using the new wind tunnel at NPL in early 1913, while Betz (1915) made extensive measurements of the pressure distribution across a Joukovskii aerofoil in the wind tunnel at Göttingen.

It would be another three years before the runes of pressure distributions in flight were read again, this time by G.I. Taylor (1916) in a landmark experiment aboard a BE-2c. Taylor matched Williams' achievement in being both pilot and physicist, even if he had a ready-made aeroplane at his disposal. In 1913, he had served as meteorologist aboard the *Scotia*, an ice-patrol vessel on the North Atlantic shipping routes, commissioned following the loss of the Titanic the previous year. With experience at sea, he was, one might say, tailor-made for meteorological service at the outbreak of war. Seconded to the Royal Aircraft Factory at Farnborough, he soon came to realize that to understand how an aircraft behaved, he needed to learn to fly. Having got his pilot's licence in 1916, Taylor set to work, designing a multi-manometer to record pressure distribution over the wing of a BE-2c, a successor to the BE-2 (Batchelor 1976).

One has to wonder quite what *design* was called for. Where de Gramont had adapted the multi-manometer method to record the pressure distribution across car-borne plates as early as 1911, Williams had not only installed a similar instrument aboard *Bamboo Bird* but had devised a camera of his own design to record pressure readings during his 1913 flight. Curiously, neither advance merited a mention in Taylor's paper, an *insousiance* that has led some to assign Taylor priority for a measurement that is hardly due.[13]

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[13] Over time, Taylor's oversight has contributed unwittingly to a widely accepted belief that his was the *first* measurement of pressure distribution over the wing of an aeroplane in flight. Hansen (2003) records that

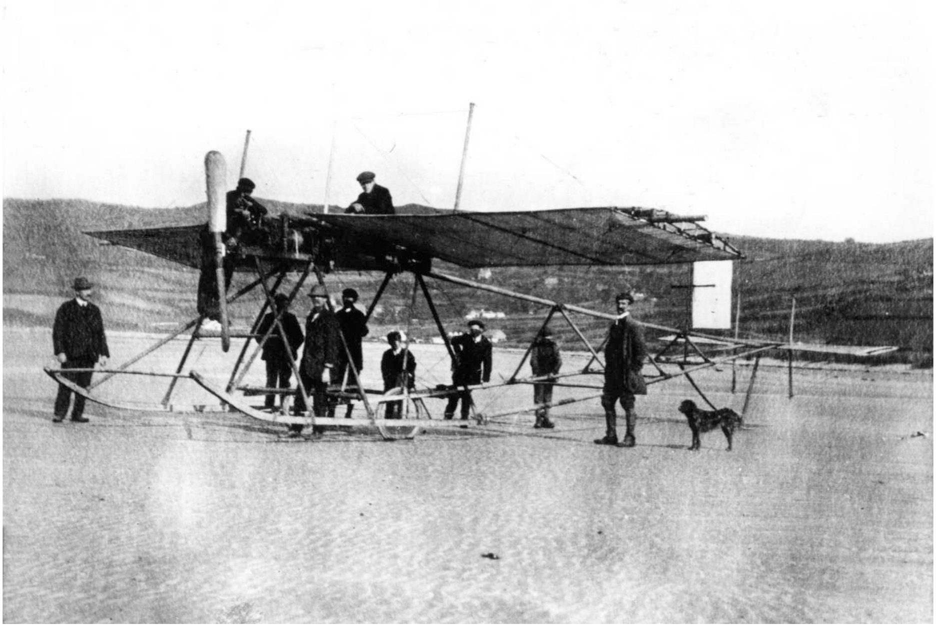

Figure 11. Williams sitting in the cockpit of *Bamboo Bird* on the beach at Llanddona. (Bangor University Archives and Special Collections).

There is no gainsaying the fact that Taylor's was the definitive in-flight measurement of pressure. His instrumentation, machined to perfection in the Farnborough workshops was fitted aboard the most stable aeroplane of its day. The pilot of a BE-2c could give most of his attention to his role as aero-physicist. Pressure data were recorded both at different heights, observations being made at whatever height the air was calmest, and over a range of speed. By any measure, Taylor's was a landmark experiment; but it was neither the first measurement of pressure in flight, nor was the 'clever means to record and measure pressure-distribution data in flight' (Hansen 2003) one of Taylor's devising.

In retrospect

In this paper we have recounted a number of key contributions made by William Ellis Williams to developments in early twentieth-century aerodynamics, starting with that for which he is best remembered, his 1903 analysis of the pitching of gliders. We have drawn on a little-known source, a 1903 letter from Sommerfeld

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'Taylor devised a rib with pressure taps that could be mounted in a wing along with a clever means to measure and record pressure-distribution data in flight'. Turner (1997) claims more specifically that Taylor 'made the first measurement of the pressure distribution over a wing in flight', while Batchelor (1976) in his obituary notice, wrote that 'Taylor took photographs of a multi-tube manometer in the aircraft cockpit—acting both as pilot and experimenter—connected by holes distributed over a section of the wing … These were the first such measurements'. Batchelor's mistaken claim echoes to this day. As recently as 2019, Royle, in a succinct summary of Taylor's work, wrote: 'Taylor firstly had to design a system that could physically measure the pressure at various points over the wing and record the results in real time—thus was born Taylor's multiple manometer' (Royle 2019).

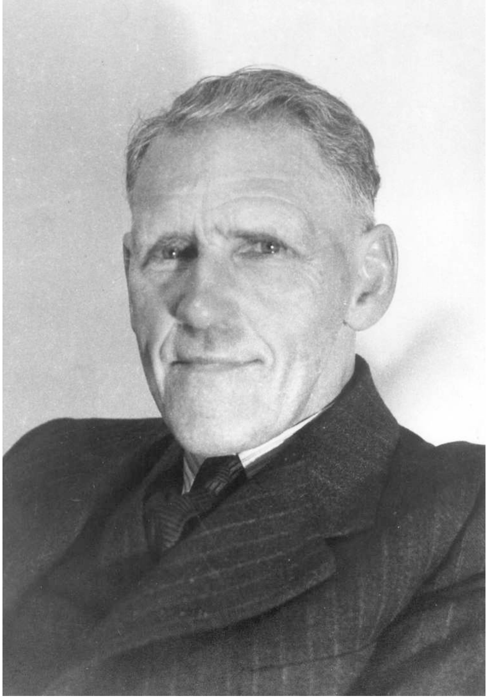

Figure 12. William Ellis Williams (1881–1960) (Bangor University Archives and Special Collections).

to Lorentz, reproduced by Kox (2008) that gives chapter and verse to a perceived laxity over points of detail in an ms. written by Bryan, contemporaneous with his stability work with Williams. Analyses of stability—in whatever context—demand scrupulous attention to detail, all the more where heavy algebra is involved and we have argued that Williams' independent working of the algebra in this context was critical. Seen in this light, the all too common reference to the results of this joint research as the Bryan equations does distinct disservice to Williams' contribution.

That Williams' photographs identifying modes and their instability in model glider trajectories were left out of the Bryan-Williams Royal Society paper in 1904 is both a puzzle and a matter of regret. Even though this crucial evidence found its way into their *Aeronautical Journal* paper that same year, and would later be fulsomely acclaimed by Bryan, the fact remains that Williams' photographs and the technique deployed to record them, have not been given the recognition they deserve.

Even in Wales there is little by way of marking Williams' achievement in building *Bamboo Bird*, still less any insight into its *raison d'être,* aimed at providing data on pressure distribution across the wings. In this paper we have advanced the claim that the Bangor *Bamboo Bird* was not only the first aeroplane designed as an aero-laboratory to have been built in its entirety in a university department, but one in which the first *in situ* measurement of pressure distribution across the wing of an aeroplane was recorded. Regrettably, G.I. Taylor's oversight in not making reference to—let alone acknowledgment of—Williams' pioneering measurement of pressure distribution in 1913 has, in turn, led to egregious claims being made for the priority of his own measurement, a full three years later.

William Ellis Williams was a man of quiet endeavour, a physicist who won the esteem of Röntgen for his skill in the laboratory and for his enthusiasm and who made the several contributions to the stability of aeroplanes and to the physics of flight revisited in this paper. Above all, in the words of Andrew Gray, who having taught him as an undergraduate at Bangor, knew him best, he was a man of plain good sense, without affectation.

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