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The troublesome birth of hydrodynamic stability theory: Sommerfeld and the turbulence problem

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Abstract. More than a hundred years ago William McFadden Orr and Arnold Sommerfeld conceived an approach to account for the transition from laminar to turbulent flow in terms of hydrodynamic stability theory. But the "turbulence problem", as this challenge became notoriously famous, could not be solved by this method. By 1920, it was widely recognized as an outstanding riddle. Although famous theoretical physicists like Werner Heisenberg dedicated a considerable effort to this problem, the "Orr-Sommerfeld method" has never found the attention of historians of science. This article describes its early perception and development in Germany, and how the "turbulence problem" reached center stage after the First World war as a major challenge for theorists with different perspectives.

Introduction

Hydrodynamic stability theory is concerned with the transition of fluid motion from one state to another, particularly from laminar to turbulent flow. The fundamentals of this theory were developed during the late 19th century by such celebrities like Lord Kelvin, Lord Rayleigh and Osborne Reynolds [1]. Early in the 20th century, hydrodynamic stability theory was revived by the Irish mathematician Orr [2] and, independently, by the German theoretical physicist Arnold Sommerfeld [3]. The "Orr-Sommerfeld approach" became a preferred subject of applied mathematicians who ventured to tackle "the turbulence problem", as it came to be known. In 1938, when the American Mathematical Society celebrated its 50th birthday, John L. Synge, head of the Department of Applied Mathematics at the University of Toronto, chose the theme of hydrodynamical stability for a birthday address: "It is concerned with the initial stage of turbulence – its generation from steady flow – but not with turbulent motion, once established", he briefly characterized his subject. "It presents mathematical problems of no small difficulty: triumphs are few and disappointments many" ([4], p. 227). By the end of the 20th century it was still noted that "despite the efforts of generations of applied mathematicians" hydrodynamic stability theory was "incompletely understood" [5].

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This does not mean that the theory was doomed to failure. Certain flow phenomena could be successfully treated in terms of hydrodynamic stability theory, like the formation of vortices between rotating concentric cylinders (Taylor-Couette instability [6]) – but the onset of turbulence remained unsolved in this case as in others. Furthermore, what was perceived as "the" turbulence problem was not the same in the course of the 20th century. The onset of turbulence was only one part of the riddle. The other concerned fully developed turbulence. Despite their common origin in 19th century hydrodynamics, both problem areas developed along separate routes: the former as a case of hydrodynamic stability theory [7,8], the latter as subject of statistical theories [9,10]. Even with the hind-sight of a century of research turbulence remains enigmatic [11].

The history of the turbulence problem, therefore, does not amount to a closed narrative [12]. Even in the restricted sense, as the problem to explain the onset of turbulence in terms of hydrodynamic stability theory, we have to narrow its scope. What are the major historical breaks in a development where "triumphs are few and disappointments many"? Here I will narrow the focus to a stage when the turbulence problem became clearly articulated as the challenge to predict the onset of turbulence, and when this task was perceived as a subject matter of hydrodynamic stability theory. I further narrow the scope to Germany, where Sommerfeld's contribution[1] from the year 1908 marked the beginning of a research effort that culminated after the First World War when the persistent failures of this approach were portrayed as "the turbulence problem" in a new journal dedicated to applied mathematics and mechanics, the *Zeitschrift für Angewandte Mathematik und Mechanik (ZAMM)*. The primacy of the "applied" provided new homes (like aeronautical research facilities) for the old problem – but could not settle it.

The Göttingen heritage

Sommerfeld's career began in Königsberg with a doctoral dissertation ("The arbitrary functions of mathematical physics") supervised by Ferdinand Lindemann, and Göttingen, where he fell under the spell of another legendary mathematician, Felix Klein. Sommerfeld was deeply impressed by Klein's charismatic personality and performance as a teacher[2]. In 1894 Sommerfeld became Klein's assistant. His specialty at that time was not yet theoretical physics, but mathematics "in touch with physics"[3] or "physical mathematics", as Klein used to call it. Physics was meant as a proving ground for mathematics, not the other way around. (Sommerfeld used this notion still in the 1940s ([14], preface).) Sommerfeld's early encounter with hydrodynamics, therefore, was with the mindset of a mathematician. The difference between this attitude and that of a theoretical physicist became clear when Sommerfeld once discussed with Wilhelm Wien about the derivation of the hydrodynamical differential equations from variational principles. Wien was writing at that time a textbook on hydrodynamics and then widely regarded as a theoretical physicist. But he regarded Sommerfeld's problem merely as a mathematical exercise which deserved little interest from a physicist's perspective[4].

[1] Orr's work on hydrodynamic stability is worth a study in its own right; it was unknown in Germany until ca. 1920. See the section "Conclusion and Outlook" for further remarks.

[2] Sommerfeld to his parents, 29 October 1893. Munich, private Sommerfeld papers. On Klein's innovations for the teaching of mathematics in Göttingen see [13].

[3] Sommerfeld to his mother, 4 March 1894. Munich, private Sommerfeld papers. Also in ASWB I, 55–59.

[4] W. Wien to Sommerfeld, 23 December 1897. DMA, HS 1977-28/A,369. Sommerfeld's correspondence with Wien and Hilbert, who had brought up this issue, is reprinted in ASWB I, pp. 80–86. Sommerfeld published his derivation in [15], pp. 86–90.

However, the "physical mathematics" of the Göttingen mathematicians was not opposed to practical applications. The more practice offered opportunities for mathematical analysis the better. Klein regarded the education of engineers in Germany as mathematically deficient, and attempted to raise the standard of scientific engineering by establishing "applied" university institutes at Göttingen [16]. He also strove for a broader recognition of mathematics as a cultural asset. He displayed his wide-ranging activities in this regard, among others, by editing an *Encyclopedia of Mathematical Sciences* [17]. As Klein's assistant, Sommerfeld became involved in these activities in several ways. Klein asked him, for example, to edit a lecture on the theory of the top– an effort which lasted for many years and resulted in a four-part treatise of almost thousand pages [18]. For the *Encyclopedia* Sommerfeld was assigned the task as an editor for the volumes on physics.

With regard to hydrodynamics, Klein presented his tendencies most clearly in a lecture in 1899: "two tendencies are nowadays paramount, the practical application on the one side, and on the other the tendency to develop the pure theory as far as possible". Thus he addressed the almost proverbial schism of theory and practice in this field. Turbulence served him as an example to illustrate this in detail. He pointed to the famous experiments of Osborne Reynolds in which the turbulent motion in a pipe was visualized by streaks of colour. When the water flow was smooth the streaks were straight and parallel. Beyond a critical speed, however, the flow changed its behaviour so that the streaks became sinuous and turbulent. "An explanation for the emergence of turbulent motions in the pipe", Klein reviewed the contemporary view, "is sought by saying: Beyond a critical speed the flow in parallel streaks is an 'instable' form of motion. Why the instability occurs, is unknown"[5]. Sommerfeld echoed the message of his master in a short review for the *Jahrbuch über die Fortschritte der Mathematik* of another paper by Reynolds "On the dynamical theory of incompressible viscous fluid and the determination of the criterion", published in 1895. "In this important work the theory of viscous fluids is subjected to a comprehensive revision. How necessary this is becomes evident from observations of fluid motions in pipes", Sommerfeld characterized the scope and motivation of Reynolds's paper. In contrast to Reynolds's earlier experimental inquiry, which resulted in a criterion about the onset of turbulence in pipe flow, the present study attempted to derive this criterion theoretically from the hydrodynamic equations[6].

In 1900 Sommerfeld was called to the Technische Hochschule Aachen as professor of mechanics. In view of his close affiliation with Klein, whose rapprochement to applied sciences was regarded by engineering professors as an invasion into their own domain, Sommerfeld was regarded as a Trojan horse in this engineering environment[7]. In order to counter this distrust Sommerfeld dared to tackle a number of practical challenges, such as hydraulics. He chose pipe flow as a first example. "Physical theory predicts a frictional resistance proportional to the velocity and inversely proportional to the square of the diameter, according to the technical theory it is proportional to the square of the velocity and inversely proportional to the diameter". Thus he characterized laminar flow as subject of "physical" theory, and turbulent flow as

[5] A handwritten elaboration by Klein's assistant Karl Wieghardt is still available at the reading room. The lecture was announced as "Mechanik deformierbarer Körper, speziell Hydrodynamik" and scheduled for three hours per week.

[6] JFM 26.0872.02, available online at www.emis.de.

[7] "Timeo Danaos et dona ferentes", Sommerfeld quoted in a letter to Klein the attitude of a colleague. Sommerfeld to Klein, 13 June 1900. SUB, Klein 11, 1060. The quote is from Vergil's Aeneis about the fall of Troy ("I am afraid of the Greeks even if they bring presents"). For the distrust of engineering professors at the Technische Hochschulen against university-trained mathematicians see [19].

subject of "technical" theory. "The physical theory agrees splendidly in capillary tubes; but if one calculates the frictional losses for a water pipeline one finds in certain circumstances values which are wrong by a factor of 100". The occasion for this presentation was a conference held in Aachen in autumn 1900. Although Sommerfeld did not offer a theoretical analysis he argued that the reason for this contradiction was the change from laminar to turbulent flow. Before he ventured to cope with turbulence, however, he further explored laminar flow with regard to engineering applications. He noticed "that the physical theory of fluid friction may be shown to advantage with a calculation of the lubricating action of machines" [20].

Instead of "the electrodynamics of the pure ether" which he had studied earlier he had now to cope with the "hydrodynamics of lubrication", Sommerfeld apologized for this digression into this applied engineering specialty in July 1900[8]. But he liked the applied as much as the pure if it offered a challenge for his mathematical skills. Sommerfeld succeeded in improving an older theory of lubrication conceived by Reynolds about the laminar flow of a lubricant between solid surfaces [21]. He arrived at analytical solutions where Reynolds had resorted to approximative series expansions. This enabled him a deeper insight into the nature of the problem. Sommerfeld showed that the older law for "dry" friction after Coulomb could be obtained as a limiting case of the "fluid" friction law derived from the hydrodynamics of the lubricant, "although both laws seem to be diametrically opposed to another". Thus he described the gist of his lubrication theory in a letter to Hendrik Antoon Lorentz, the renowned Dutch theoretical physicist[9].

Lorentz was one of the authors whose articles Sommerfeld was editing for the physics volumes of Klein's *Encyclopedia* – and Lorentz was an authority also with regard to the contemporary theories about the onset of turbulence [22]. Sommerfeld corresponded with Lorentz since autumn 1898, when he and Klein had payed a first visit to the Dutch theorist in his home in Leiden. Although Lorentz's articles for the *Encyclopedia* dealt with the electrodynamics of the "pure ether" and the theory of electrons, the flow of real fluids was not an alien issue in their correspondence. If the existence of "non-laminar integrals of the hydrodynamic equations" could be proven, Sommerfeld once wrote to Lorentz, one would have "solid ground under the feet". Without such a proof it was an open question whether these equations account both for laminar and turbulent flow. "Unfortunately no mathematician will dare to attempt this proof of existence"[10]. Sommerfeld had hoped to contribute in 1900 to a Festschrift for celebrating the 25th anniversary of Lorentz' doctoral dissertation with a theory about the onset of turbulence, but he had to admit that he "miserably shipwrecked" in this effort[11]. The success with the theory of lubrication three years later was only a poor consolation for this shipwreck because it involved only laminar flow. Whenever turbulence plays a role it seems "that theoretical hydrodynamics would have to declare itself bankrupt in view of the practical problems of hydraulics", Sommerfeld echoed the often-preached theme again in 1903. "Still there is no precise theoretical method to determine the critical velocity and the pressure gradient beyond the critical velocity", he discerned the challenge for theorists ([24], p. 212).

[8] Sommerfeld to Schwarzschild, 16 July 1900. SUB, Schwarzschild, Briefe 743. Also in ASWB I, pp. 171–174.

[9] Sommerfeld to Lorentz, 24 February 1903. RANH, Lorentz, inv.nr. 74. Also in ASWB I, pp. 215–221.

[10] Sommerfeld to Lorentz, 8 October 1900. RANH, Lorentz, inv.nr. 74. Also in ASWB I, pp. 180–182. In 1907, Lorentz revised his earlier paper [22] and thanked Sommerfeld for noting an error, see [23], p. 63.

[11] Sommerfeld to Lorentz, 10 December 1900. RANH, Lorentz, inv.nr. 74.

By the same time, hydraulics became the subject of Klein's seminar in Göttingen for the winter semester 1903/04. Klein declared it a "true need of our time" to bring the theory of hydrodynamics closer to real flow phenomena[12]. He sorted the involved mathematical problems into three categories:

(a) well defined problems;
(b) rather poorly defined problems;
(c) very badly defined problems.

"I count the flow of water in pipes and channels to the second category", Klein addressed the riddle of turbulence as one of the poorly defined problems. "As soon as the motion is not very slow, there is the phenomenon of turbulence. [...] The problem how the onset of turbulence should be explained theoretically appears still unsolved". The seminarists who dealt with turbulence were the astronomer Karl Schwarzschild, and the mathematicians Hans Hahn and Gustav Herglotz: Schwarzschild reviewed the general state of the art with regard to "stability and lability with fluid motions", Herglotz reported on "turbulent motions and the fundamental equations of Boussinesq", and Hahn on "theories of Boussinesq and their comparison with experience"[13]. It is not clear how much interest Sommerfeld took in Klein's seminar because he started by this time a new research effort on the theory of electrons, which after 1900 became the most fashionable research field for physically oriented mathematicians and the subject of another famous seminar in Göttingen in the summer of 1905 [25]. However, Sommerfeld was in frequent contact with Herglotz and Schwarzschild. Hahn, Herglotz and Schwarzschild elaborated their seminar presentations on turbulence in a common article in the *Zeitschrift für Physik und Mathematik*, where Sommerfeld's lubrication paper had appeared in a preceding issue, so that the Göttingen ideas on turbulence could have become widely known [26]. But it remained a singular contribution and had no impact on the subsequent research on turbulence [27].

In the winter semester 1907/08, Klein dedicated another seminar to hydrodynamics, now in collaboration with Prandtl, Runge, Wiechert and Müller, all of whom were involved in one or another of Klein's efforts for applied science. In 1904, Prandtl and Runge had been called to Göttingen where they became directors of new institutes for applied mechanics and mathematics, respectively. By the time of the seminar, Prandtl's first doctoral students began to work on the boundary layer concept, an approximate theory for the study of real flows with small viscosity ([28], Chap. 2); furthermore, Klein involved Prandtl in the foundation of an extramural aerodynamic institute for airship model research [29]. With Prandtl and his disciples, the seminar addressed issues of utmost pertinence for technical applications[14]. Although the seminar did not claim significant progress it signaled a growing urgency with regard to applications. Klein repeated in his introductory talk what he had already preached so often at earlier opportunities, that the connex of the theory with practice was paramount and that he aimed at mathematicians who know to work on practical

[12] Klein, handwritten notes. SUB Cod. Ms. Klein 19 E (Hydraulik, 1903/04).

[13] Klein's seminar protocoll book, Nr. 20. Göttingen, Lesezimmer des Mathematischen Instituts. Available online at librarieswithoutwalls.org/klein.html.

[14] Klein's seminar protocoll book, Nr. 27. Göttingen, Lesezimmer des Mathematischen Instituts. Available online at librarieswithoutwalls.org/klein.html. Among the seminar speakers were, for example, Theodore von Kármán, who made then in Prandtl's institute his first steps towards an outstanding career, and Heinrich Blasius, whose dissertation from the same year would become famous for solving the boundary layer equation for laminar flow along a flat plate ("Blasius flow"). Blasius reviewed in two sessions of Klein's seminar (in January and February 1908) the contemporary research on turbulence.

problems. In contrast to the hydraulics seminar three years ago he could now present the seminar as a joint venture with Prandtl's and Runge's applied institutes[15].

Sommerfeld's Rome paper (1908)

By that time, Sommerfeld's career was steering in a new direction: he was called in 1906 to Munich as head of an institute for theoretical physics. But his growing self-image as a theoretical physicist did not alienate him from his Göttingen and Aachen heritage – all the more because his last Aachen work, a theory about the buckling of plates and rails, seemed mathematically analogous to the problem of hydrodynamic stability. "The buckling of the plate had an interesting sequel", Sommerfeld wrote to Runge in June 1906. "I noticed that a similar calculation also leads to a determination of the critical velocity in hydrodynamics. For the time being, however, I am left with a rather horrible transcendental equation that awaits further discussion"[16]. A month later he wrote to Wilhelm Wien: "I am now pinned in hydrodynamics, turbulence"[17]. He was optimistic to present a paper at a forthcoming conference, but his hope was again frustrated. By the end of the year he admitted in a letter to Lorentz: "unfortunately I still could not make progress with the problem to determine the critical velocity in hydrodynamics"[18]. After the winter semester 1907/08 he wrote to Lorentz again that he expected to accomplish this effort soon[19].

In the meantime Lorentz had revised and extended his theory from the year 1897, including a new section on the flow between two walls, one fixed and the other moving at constant speed – a configuration that became known as "plane Couette flow"; in contrast to the flow between two coaxial cylinders, about which Maurice Couette had performed his famous experiments in 1890, the plane flow configuration appeared accessible to a theoretical description. William Thomson (Lord Kelvin) had studied this special case as early as in 1887 and arrived at the conclusion that it was stable ([1], p. 212). Lorentz's analysis, however, was different from his 19th century precursors [23]. By determining whether the superposition of an infinitesimally small "turbulent" disturbance to a straight (=laminar) main flow leads to a growth of energy, Lorentz derived a criterion for the main flow which yielded a limit of stability. Lorentz, therefore, was very curious about Sommerfeld's investigations[20].

But Sommerfeld's hope was again frustrated. He could not proceed with his approach far enough to derive a critical limit beyond which laminar flow becomes turbulent. By this time, however, he had persuaded himself that the problem was too complex for a solution in a single step, and that it was worthwile to present at least the method with which he hoped to reach this goal. The opportunity for this presentation

15 Opening address, 30 October 1907, in Klein's notes. SUB Cod. Ms. Klein 20 F (Hydro- und Aerodynamik, 1908). The turn towards applications is also apparent from the following seminars: in summer 1908 it was on the "theory of ships and dynamic meteorology"; in winter 1908/09 on the "theory of structural design"; and in summer 1909 on "strength of materials". Klein organized all these seminars together with Runge and Prandtl [30], Appendix, p. 10. On Klein's role for the establishment of applied mathematics see [31].

16 Sommerfeld to Runge, 9 June 1906. DMA, HS 1976-31.

17 Sommerfeld to W. Wien, 5 July 1906. DMA, NL 56, 010. Also in ASWB I, pp. 253–253.

18 Sommerfeld to Lorentz, 12 December 1906. RANH, Lorentz, inv.nr. 74. Also in ASWB I, pp. 257–258.

19 Sommerfeld to Lorentz, 18 March 1908. RANH, Lorentz, inv.nr. 74. Also in ASWB I, pp. 331–332.

20 Lorentz to Sommerfeld, 27 March 1908. DMA, HS 1977-28/A,208. Also in ASWB I, pp. 333–334.

came with the Fourth International Mathematical Congress, held in April 1908 in Rome [3].

In order to make plausible what was at stake and why the past and future efforts were so often doomed to failure, it seems expedient to present Sommerfeld's approach in some technical detail. He started with a tribute to Reynolds who had recognized by dimensional considerations that the transition to turbulence in pipe flow could be formulated in terms a dimensionless quantity

$$R = \rho U h/\mu,$$

with the fluid's density ρ, its viscosity μ, its mean flow velocity U and the diameter h of the pipe. Sommerfeld emphasized that the quantity R is "a pure number which we will call the Reynolds number"([3], p. 116). This seems to be the first explicit naming of the Reynolds number [32].

In Sommerfeld's configuration of a plane Couette flow along the x-axis, h meant the distance between two walls located at $y = -h/2$ and $y = +h/2$. The velocity components of the main flow were assumed as $u_1 = Uy/h$ and $v_1 = 0$ in the x- and y-directions, respectively. To this flow a disturbance $(u_2,\ v_2)$ was superposed. Sommerfeld started with the Navier-Stokes equation, where the velocities were expressed as derivatives of a stream function $\Pi(x,y,t)$ as $u = \partial\Pi/\partial y, v = -\partial\Pi/\partial x$. By eliminating the pressure he obtained a differential equation for Π:

$$\frac{\rho}{\mu}\left(\frac{\partial}{\partial t}\Delta\Pi + \frac{\partial\Pi}{\partial y}\frac{\partial}{\partial x}\Delta\Pi - \frac{\partial\Pi}{\partial x}\frac{\partial}{\partial y}\Delta\Pi\right) = \Delta\Delta\Pi$$

where Δ is the Laplacian ($\Delta = d^2/dx^2 + d^2/dy^2$), and $\Pi = \Pi_1 + \Pi_2$, where Π_2 is the disturbance superposed to the main flow Π_1. Sommerfeld linearized the differential equation with regard to the small disturbance Π_2 (i.e. he canceled quadratic terms in the differential quotients of Π_2) and obtained:

$$\Delta\Delta\Pi_2 = \frac{\rho}{\mu}\left(\frac{\partial}{\partial t}\Delta\Pi_2 + U\frac{y}{h}\frac{\partial}{\partial x}\Delta\Pi_2\right).$$

(It is a consequence of the special choice of plane Couette flow as the main flow that the differential equation contains only $\Delta\Pi_2$; the Laplacian applied to Π_2 yields $\partial u_2/\partial y - \partial v_2/\partial x$, i.e. the vorticity of the disturbance). With dimensionless variables $\xi = x/h$, $\eta = y/h$, $\tau = \frac{U}{h}t$ the solution may be written as:

$$\Delta\Pi_2 = \phi(\eta)e^{i(\beta\tau - \alpha\xi)}, \qquad \Pi_2 = f(\eta)e^{i(\beta\tau - \alpha\xi)}$$

where ϕ and f obey the differential equations

$$\frac{d^2\phi}{d\eta^2} - \alpha^2\phi = iR(\beta - \alpha\eta)\phi, \qquad \frac{d^2 f}{d\eta^2} - \alpha^2 f = \phi. \tag{1}$$

By the substitution

$$z = \frac{\alpha^2 + iR(\beta - \alpha\eta)}{(\alpha R)^{2/3}} \tag{2}$$

the solution for ϕ could be expressed in terms of Bessel functions with index $\pm 1/3$. Inserting ϕ in the second differential equation yields

$$f = Af_1(z) + Bf_2(z) + Ce^{ikz} + De^{-ikz} \tag{3}$$

with $k = (\frac{\alpha^2}{R})^{1/3}$; f_1 and f_2 are special functions containing integrals over the respective Bessel functions. The determination of the four unknown integration constants from the boundary conditions at the walls (i.e. at $y = -h/2$ and $y = +h/2$, corresponding to special values z_0 and z_1, respectively) results in a system of four equations for A, B, C and D, which has a non-trivial solution only if the determinant of this system of equations vanishes. This leads to the "rather horrible transcendental equation" which Sommerfeld wrote down only in abbreviated form as

$$\frac{f_1'(z_1)}{f_1(z_1)} = \frac{f_2'(z_1)}{f_2(z_1)}. \tag{4}$$

The goal was to derive from this equation the critical Reynolds number R beyond which the laminar main flow becomes unstable. In a more technical parlance, (1) to (4) define an eigenvalue problem. The eigenvalue equation (4), however, contains (in the substituted quantity z) three variables: the complex time constant of the disturbing wave β, its wave number α, and the Reynolds number R, where α and R are assumed as given. The question whether the flow is stable or not amounts to the question for which values of α and R the imaginary part of β is positive or negative. In the former case the disturbance decays exponentially in time, and the flow is stable; in the latter the disturbance grows and renders the flow unstable. Therefore, the stable and unstable regions can be determined by calculating "for all possible combinations of α and R, i.e. for various regions of a '(α, R)-plain', the corresponding values of β", Sommerfeld concluded his Rome paper [3], p. 124.

Sommerfeld did not proceed further. Two months after the congress he wrote in a letter to Wilhelm Wien: "I have tortured myself continually with the turbulence problem and spent almost all of my time with it, but I could not accomplish it"[21].

At first sight it seems as if Sommerfeld had followed Lorentz because he chose the same plane Couette flow configuration as subject of his analysis. But Sommerfeld's approach was different. Instead of analysing the growth or decay of the energy resulting from the superposed perturbation, Sommerfeld used the classical "method of small oscillations" which had proven successful already in numerous cases of mechanical instabilities. It had been applied to hydrodynamics by Lord Kelvin and Lord Rayleigh as early as in 1880 in order to derive criteria of stability or instability of certain flows. Both Kelvin and Rayleigh had explored ideal flow configurations with more or less success. Kelvin, for example, analysed the stability of two inviscid plane flows moving parallel at different velocities ("Kelvin-Helmholtz instability"). Rayleigh investigated the case of several layers of parallel plane flows moving with the same velocity at each interface, but with a different constant velocity gradient from layer to layer. He found that such flows are unstable if the profile of piece-wise linear velocities reverses the direction. More generally, he proved that any plane flow whose second derivative changes sign between the bounding surfaces is unstable ("inflexion theorem"). However, these results were obtained in the limit of inviscid flow, so that no critical velocity for the onset of turbulence could be derived. Rayleigh's results seemed to contradict Lord Kelvin's earlier conclusions. But Rayleigh's analysis was based on infinitesimally small disturbances, whereas Kelvin regarded the amplification of finite disturbances as the cause for the instability. Both Kelvin and Rayleigh were authorities with regard to these questions. Their disagreement left it open to further analysis whether and how plane Couette flow became unstable ([1], pp. 208–218).

Sommerfeld, therefore, was not the first to apply the method of small oscillations to the analysis of flow instability. But he approached the problem in a more fundamental manner – aiming at the analysis of *viscous* flow instability. The ensuing history would mark the "Orr-Sommerfeld" approach as a milestone. From a contemporary vantage point, however, it seemed wanting: neither could it decide the dispute

[21] Sommerfeld to W. Wien, 20 June 1908. DMA, NL 56, 010. Also in ASWB I, pp. 341–343.

among the British Lords, nor could it offer an answer at what critical velocity, or, with Sommerfeld's more general notion, Reynolds number, laminar flow becomes unstable? The only definitive answer which Sommerfeld gave was that his analysis confirmed Rayleigh's theory "in the simplest special case of vanishing main motion", i.e. any disturbance superposed to a fluid at rest ($R \to 0$) will decay so that the result is stability. Hardly a revolutionary result from the perspective of physics, but a reassurance about the mathematical approach. Sommerfeld readily admitted in his Rome paper that this "present communication" only leads to a condition for instability in the form of a transcendental equation; "its complete discussion, which in my view is the true content of the problem of turbulence, is not yet accomplished" ([3], p. 117).

Sommerfeld did not immediately try to accomplish this feat in the years after the Rome Congress, but he assigned a related topic to a doctoral student as subject for a dissertation. The first generation of Sommerfeld's disciples often accomplished both experimental and theoretical investigations for their doctoral work [33], and Ludwig Hopf, whom Sommerfeld trusted with this subject, was no exception in this regard. The theme of his dissertation was "Hydrodynamic investigations: turbulence of a river. On ship waves" [34]. Only the latter part on ship waves was meant as subject matter of a theoretical study. "The more important and difficult part of the work", Sommerfeld remarked in his report about Hopf's work in July 1909, "lies in the experimental investigation of turbulence". In contrast to the flow in closed ducts, open channel flow had not been studied before with regard to the onset of turbulence. For the investigation of turbulence in a "river" Hopf used a straight 5.2 cm wide brazen channel. The goal was to determine how the flow resistance varied with increasing Reynolds number (varied by increasing the channel's inclination, i.e. the flow speed). "Here, too", Sommerfeld summarized the main result of Hopf's work, "there is under certain circumstances (small depth, slow flow velocity) a stable laminar motion of the kind of Poiseuille's law, which becomes unstable beyond a critical limit and gives rise to another form of motion"[22]. Hopf found that the onset of turbulence in open channel flow was similar to that in closed ducts. With regard to the theory he merely mentioned in an introductory section titled "The turbulence problem" that Reynolds and Lorentz had approached this problem by an "energy consideration" without a convincing result; Kelvin's and Rayleigh's stability approaches, too, seemed inconclusive, "and the consequent analysis of the problem according to the method of small oscillations by Sommerfeld is not yet accomplished" ([34], pp. 6–7).

Hopf left the problem at this point and steered his career towards goals that seemed more attractive for a theoretical physicist: He went to Zurich as Einstein's assistant and focused his research on the theory of radiation[23].

Sommerfeld also returned to subjects which were closer to the research front of contemporary physics, such as X-rays, relativity, and quanta. About a year after the publication of the Rome proceedings he asked his British colleague Horace Lamb, a renowned expert on hydrodynamics, for news about turbulence. "With regard to the turbulent flow of water", Lamb responded, "I do not know of any further references"[24].

[22] Sommerfeld's "votum informativum" to the faculty, 5 July 1909. Munich, University Archive, OC I 35 p.

[23] His collaboration with Einstein resulted in two publications that added the last straw to the conviction that Planck's radiation formula could not be classically explained. These papers are co-authored with Einstein and reprinted in [35], pp. 347–367.

[24] Lamb to Sommerfeld, 12 September 1910. DMA, HS 1977-28/A,189. Lamb did not mention the work of Orr [2], which was also ignored in Germany for another decade (although both Orr's and Sommerfeld's papers were briefly reviewed in the *Jahrbuch über die Fortschritte der Mathematik*, JFM 38.0741.02. and JFM 40.0806.02, respectively).

Henceforth, the turbulence problem occurred rather seldom as a research topic in theoretical physics.

The stability deadlock

Among mathematicians like Klein, however, turbulence did not lose interest. In March 1911, Klein presented to the Göttingen Academy of Science a paper "On the turbulence problem" by his former assistant Georg Hamel [36], who had become professor of mechanics at the Technische Hochschule Brünn [37]. In this paper Hamel attempted to marry the energy method used by Reynolds and Lorentz with techniques from the theory of integral equations, but did not arrive at a result. Hamel's assistant, Richard von Mises, also payed tribute to turbulence. In a report "On the problems of technical hydromechanics", von Mises defined the turbulence problem as the task to solve the hydrodynamical differential equations for pipe flow so that both laminar flow and "the actual pulsating motion" are covered. In contrast to Sommerfeld he believed that the onset of turbulence in pipe flow could not be explained by the method of small oscillations but required finite disturbances. He speculated that such disturbances are caused by the roughness of the wall that surrounded the flow. "Instead of a smooth wall one has to choose as boundary condition a sine curve, for example", von Mises argued, "and then let its amplitude and period go to zero in such a way that various 'degrees of roughness' may be characterized" ([38], pp. 323–324).

Two years later, von Mises reported further evidence for this view. Without solving Sommerfeld's transcendental equation (4) he proved that the complex time constant β must have the same sign for all Reynolds numbers R and wave numbers α. Because the stability of plane Couette flow had been ascertained for small Reynolds numbers, this result meant that the stability extends to all Reynolds numbers. With this proof von Mises became convinced that turbulence did not result from an instability within the flow but originated at the walls. Laminar flow between "absolutely smooth walls" would not become unstable, "i.e. an arbitrary initially imposed disturbance decays with proceeding time. But the conditions in real flow are different. The disturbances of the laminar motion happen continuously due to molecular unevenness of the wall which one calls 'roughness'. The question is not when an instantaneous disturbance that is spread across the entire interior of the fluid is maintained by itself, but when a disturbance that is caused permanently at the boundary is able to spread across the entire interior. The exact formulation of this task, which is connected to Prandtl's notion of the 'boundary layer', meets with enormous mathematical difficulties. Perhaps I have once more the opportunity to report to you on this" ([39], pp. 247–248).

The same conclusion was reached at about the same time by entirely different means. When Hopf found no opportunity to pursue a career as a theoretical physicist and accepted an assistantship for mechanics at the Technische Hochschule Aachen, he chose the turbulence problem as a research theme for his habilitation – and arrived at the result that plane Couette flow is stable for all Reynolds numbers. Hopf acknowledged that Sommerfeld had forwarded to him a package of notes for further evaluation which he reviewed in a section of ten pages, before he tackled the evaluation of the transcendental equation (4) – a monster which in its explicit form extended over four lines and involved case differentiations for different ranges of the involved complex quantities ([40], p. 21). It was accessible to further mathematical analysis only by approximating the involved Hankel functions by the first term of their asymptotic series expansions. In contrast to the result of Richard von Mises, however, Hopf's analysis revealed how the superposed disturbing waves to the main flow were decaying. He discerned three types of disturbances: one type affected the entire fluid, another type was damped strongest at the walls and a third type along the middle line of the channel ([40], pp. 38–60).

With these results, Sommerfeld's approach was in a stability deadlock: it yielded stability for all Reynolds numbers. Unlike von Mises, who regarded this result as a corroboration of his view that the onset of turbulence was not due to an intrinsic instability but an effect of the bounding walls, Hopf did not comment on the contradiction between theory and practice. A grain of uncertainty concerned the generality of Sommerfeld's approach: was the assumption correct that if a wavelike disturbance of the form $f(\eta)e^{i(\beta\tau-\alpha\xi)}$ proved as stable, *any* disturbance was stable? The possibility to compose an arbitrary disturbance by Fourier's method as a sum of waves seemed to confirm this assumption. But as long as this conclusion was not corroborated by rigorous mathematical arguments, there remained perhaps a loophole through which instability could occur despite the results of Hopf and von Mises. However, this loophole was closed even before Hopf had published his results. Otto Haupt, a mathematician who fell under the spell of Sommerfeld during a one-year sojourn in Munich before he became professor at the Technische Hochschule Karlsruhe [41], proved that any function which fulfilled the boundary conditions of the turbulence problem could be developed after the eigenfunctions found by Sommerfeld in his Rome paper [42]. Sommerfeld presented Haupt's paper to the Bavarian Academy of Science with the remark: "because recently the stable character of the eigenfunctions of the turbulence problem was proven from several sides, we are entitled to conclude from this treatise that also an arbitrary disturbance leaves the flow stable. A hydrodynamical explanation of the turbulence phenomena in terms of the method of small oscillations, therefore, appears impossible" ([42], pp. 10*–11*).

A year later, Sommerfeld presented to the Academy another paper which seemed to indicate a way out of the stability deadlock. The author of this paper was Fritz Noether, the son of Max Noether and younger brother of Emmy Noether – all famous mathematicians. Fritz Noether had come under the spell of Sommerfeld during his study in Munich. While he was still a student, Sommerfeld made him his coauthor for the final volume of the Theory of the Top. Noether regarded it as "premature to conclude that an explanation of the turbulent phenomena in terms of the generally accepted hydrodynamic equations is impossible". If the instability began from a transient rather than from a stationary laminar state, a new loophole would be opened for Sommerfeld's approach. He used the analogy of a ball in a shallow pit in order to illustrate his argument. According to the method of small oscillations the motion of the ball in its pit would be stable, but if the pit was on top of a mountain the situation was different. By analogy, Noether analysed the stability of plane Couette flow in a state that was already disturbed into a transient, but still laminar, state: "will this nonstationary laminar motion still be stable?" Noether introduced the study of this modification. "We prove for a special case, by assuming a simple law for the initial state of flow, that this is no longer the case for sufficiently large wall speeds". He chose as a special case an initial velocity distribution of the form of a cubic parabola (which could be imagined as a distortion of the linear Couette profile) and derived a critical limit beyond which the flow would become unstable. For the first time, the theoretical stability deadlock seemed broken [43].

Both Hopf's and Noether's interest in hydrodynamic stability theory was rooted in their close association with Sommerfeld. Hopf thanked Sommerfeld for his "steady interest" and the Bavarian Academy of Science for funds that enabled him to engage students for "orienting numerical preparatory work" ([40], p. 3). Sommerfeld, despite his growing involvement in the theory of quanta and atoms, was on the verge of focusing his own research again towards the turbulence problem. Even before von Mises and Hopf reported their results about the stability deadlock, Sommerfeld must have obtained the same result and shared it with Prandtl, because Prandtl responded in a letter in April 1911: "your result on turbulence has interested me very much. So the dreaded stability indeed has occurred!"[25]. When Noether claimed success in 1913, Sommerfeld must have pondered the thought to let the problem further explore by one of his students. Apparently he was aiming at the case of pipe flow, because Noether suggested as a preliminary study to extend the approach from the two-dimensional plane Couette flow to the cylindrical configuration of Poiseuille flow in a tube[26]. In his own work, Noether further aimed to demonstrate how laminar flows between two parallel walls can become unstable via transient nonlinear flow profiles [44].

But in December 1913 Sommerfeld presented to the Bavarian Academy another study "On the turbulence problem" which contradicted Noether's claim to have exposed a case of instability. The author, Otto Blumenthal, was Sommerfeld's friend and colleague from their common time in Göttingen. He had been called in 1905 as professor of mathematics to the Technische Hochschule Aachen. Without denying Noether's approach as a whole, Blumenthal showed that the elaboration of the special case of the cubic parabola was erroneous. "Thus there is still no case known in which a laminar flow can be transformed into a turbulent flow" ([45], p. 564). With this verdict, Noether seems to have complied with the general view that there was no way out of the stability deadlock. Sommerfeld, too, refrained from pursuing Noether's suggestion to make the case of cylindrical Poiseuille flow the subject of research for one of his disciples. Once more, the struggle with the turbulence problem was in a dead end.

The turbulence problem in World War I

Despite this backlash, Noether did not give up. "The turbulence, of course, kept bothering me throughout the summer", he wrote to Sommerfeld in December 1914. The outbreak of the first World War temporarily interrupted his effort, but even at the site of his deployment in Northern France he found some leisure to think about the turbulence problem. "I have turned around the question of my flawed paper for the Munich proceedings, and instead of analysing a special distribution of flow, like y^3, I have determined the distribution so that the respective boundary value problem has a solution". Thus he turned the failure of the past year into a starting point of a new research paper. "A draft about this is accomplished and lies in my desk drawer. Has there been published anything on turbulence in the meantime?"[27].

The war delayed the publication of this effort for another two years. Noether sent the resulting paper in December 1916 to Runge, and Runge presented it in February 1917 to the Göttingen Academy of Science. The paper started with the question, whether there are real functions $U(x)$, defined in a finite interval $-1/2 \leq x \leq +1/2$, such that a certain fourth order differential equation has a solution which satisfies the given boundary conditions at $x = \pm 1/2$. $U(x)$ represented the velocity profile of a plane flow between these boundaries. Neither the linear profile of plane Couette flow nor the cubic parabolic profile of his earlier analysis yielded solutions, so that "one might suspect that there is a general cause which excludes solutions of this boundary value problem", Noether summarized the present situation. "That this is not the case we prove in the following by determining a function U such that the boundary value problem becomes resolvable". The function which Noether now presented as a candidate for achieving this feat represented a symmetric flow profile with a vanishing velocity at the boundaries that jumped unsteadily to a finite speed at some distance

[25] Prandtl to Sommerfeld, 5 April 1911. DMA, NL 89, 012.
[26] Noether to Sommerfeld, 12 July 1913. DMA, HS 1977-28/A,246.
[27] Noether to Sommerfeld, 12 December 1914. DMA, NL 89, 059.

from the walls. Noether apparently regarded this profile as an approximation to that of fully turbulent pipe flow: "as is well-known, the actually observed flow profile also displays a nearly constant velocity in the interior of the channel, with a rapid decline to 0 at the boundary". He was not attempting to determine the critical Reynolds number like in his flawed paper for the Munich Academy three years ago, but contented himself "to prove the possibility of solutions". He regarded this only as a first step towards a solution of "the real turbulence problem, by which we understand the establishment of stable modes of flow which are different from laminar flow" ([46], p. 211).

Hopf also tackled the turbulence problem further during the war. In May 1915 he thought that he was now on "a path which seemed promising", as he reported in a letter to Sommerfeld. He did not go into details but only revealed that he found "instability at large R". Lack of time prevented him from finishing this work, "but hopefully I return without heroic death and then it will proceed well". ("[...] aber hoffentlich kehre ich ja ohne Heldentod zurück und dann wird es schon gut gehen".) Unfortunately hopes had been dashed too often in this area of research, and so he added: "if it will again not succeed, then I will definitively leave the turbulence problem in peace"[28]. Assignments to different locations in the war prevented him from continuing this research. "The turbulence is still in its war sleep", he wrote to Sommerfeld a few months later, "if I would manage to get through it one day, this would give me more pleasure than the Iron Cross"[29].

But Hopf did not find the time to elaborate his ideas, as he reported to Sommerfeld in September 1916. He had "a ravenous appetite for physics" and expressed his admiration of the recent achievements in atomic physics and general relativity theory. "I tend to believe that all this will exert a longer lasting influence on the development of mankind than the whole senseless war, whose end is not in sight and which after all will change nothing in the world"[30]. A few weeks later he was assigned to the aviation troop at Berlin-Adlershof for a variety of aeronautical tasks including flight tests, but he could "hardly get down to scientific work besides this job"[31]. His thoughts about the turbulence problem had to wait until after the war before they were elaborated and published. "In the earlier studies we always assumed a wall which forces the disturbance to disappear at the surface", Hopf revealed his new strategy to break the stability deadlock in his first paper on the turbulence problem after the war. "The opposite limit would be a wall that does not resist the disturbance, a kind of free surface; at such a wall the disturbance is not zero, but the pressure exerted by the disturbance vanishes". With this boundary condition he obtained instability. The gist of his analysis, therefore, was to blame the wall rather than the flow profile for the stability deadlock ([47], pp. 541–542).

Both Hopf and Noether worked on the turbulence problem during their spare time. This does not mean, however, that this problem was irrelevant for wartime applications. Shortly before the war a striking turbulence effect had been discovered and analysed in wind tunnel experiments. At a critical air speed the drag coefficient of spheres in the air stream suddenly dropped to a much lower value. Prandtl explained this phenomenon by the assumption that the initially laminar boundary layer around the sphere becomes turbulent beyond a critical air speed, and that the turbulent boundary layer flow entrains fluid from the wake. As a result, the turbulent boundary layer stays attached to the surface of the sphere longer than in the laminar case. In other words, the onset of turbulence in the boundary layer reduces the wake behind the sphere and thus also its drag [48]. Both the experimental study of this phenomenon

[28] Hopf to Sommerfeld, 31 May 1915. DMA, NL 89, 059.
[29] Hopf to Sommerfeld, 13 November 1915. DMA, NL 89, 059.
[30] Hopf to Sommerfeld, 30 September 1916. DMA, NL 89, 059.
[31] Hopf to Sommerfeld, 22 Septemer 1917. DMA, NL 89, 059.

and Prandtl's (qualitative) explanation were subject of some dispute on the eve of the war ([28], pp. 49–52; [29], pp. 83–86). Sommerfeld corresponded with Prandtl in May 1915 about "the fall of bombs in water and air" where the same phenomenon was involved[32]. A few months later, the aerodynamics of bomb shapes became officially part of Prandtl's war work[33].

Falling bombs were not the only "practical" application of this turbulence effect. The first World War has been described from the perspective of the history of aeronautics as "the age of strut-and-wire biplanes" ([49], pp. 267–318). Depending on their shape, these struts and wires would also experience at some critical air speed a sudden change of drag due to the transition to turbulence in the boundary layer. Therefore struts and wires became subject of systematic investigations in Prandtl's wind tunnel. In a technical war report Prandtl's collaborator, Max Munk, pointed out why these measurements were so important: "in particular, a reduction of the speed, for example, when the plane changes from horizontal flight into a climb, results in a sudden increase of the drag coefficient, and often of a considerable increase of the drag itself". It was therefore not sufficient to minimize the drag by streamlining the profile of a strut, but also to give it a shape that did not experience the sudden change of drag when the airplane passed through the critical speed range [50].

In view of such practical relevance, Prandtl sketched in March 1916 a "Working program about the theory of turbulence"[34]. According to these notes, Prandtl approximated the velocity profile of laminar boundary layer flow along a flat plate by a piece-wise linear profile, and thus was led to Rayleigh's older analysis. "Concerning the turbulence of the boundary layer along curved surfaces", he remarked on one page of this manuscript, "the velocity profile within the boundary layer has an inflexion between the site of the lowest pressure and the site where it peels away from the surface". In the inviscid limit ($R = \infty$), Rayleigh's inflexion theorem predicts instability for such flow profiles. This must have motivated Prandtl to approach the turbulence problem from this end, but he does not seem to have pursued his working program in more detail during the war.

The turbulence problem as a new challenge after the war

After the war, the turbulence effect on the drag of spheres, struts and wires was openly discussed. "At higher velocities the flow becomes eddying already before the flow detaches from the surface of the body", a textbook explained the phenomenon. "The stream of air, therefore, huddles against the surface, and the site of flow detachment is shifted rearwards until a new state of equilibrium is attained. The resulting reduction of the wake system yields a reduction of the resistance. The critical region of the boundary layer is that where the fluid flows against an increasing pressure before the site of detachment; it is located between the maximum of velocity and the site of detachment". This quote is not from a chapter on hydrodynamics in a physics textbook, but from a textbook on *Aerodynamics* authored by Ludwig Hopf and Richard Fuchs, a mathematician with whom Hopf had collaborated at the aviation troops in Berlin-Adlershof during the war. Although the phenomenon was known since 1914 from Prandtl's memoir in the proceedings of the Göttingen Academy of Science, it was now communicated from the perspective of practical aeronautics and

[32] Sommerfeld to Prandtl, 9 May 1915; Prandtl to Sommerfeld, 14 May 1915. GOAR 2666.
[33] He received, for example, contracts from the Bombenabteilung der Prüfanstalt u. Werft der Fliegertruppen, dated 23 December 1915, concerning "Fliegerbombe, M 237", and on "Carbonit-Bomben, Kugelform", dated 1st September 1916. GOAR 2704B.
[34] Cod. Ms. L. Prandtl, 18, Acc. Mss. 1999.2, SUB.

illustrated with a diagram from wartime wind tunnel measurements on struts ([51], pp. 186 and 226).

To scientists like Prandtl and Hopf, who had actively been engaged in this effort, a reappraisal of the turbulence problem seemed most expedient[35]. Prandtl began to elaborate his working program about the onset of turbulence in plane flows with piecewise linear flow profiles. "Calculation according to Rayleigh's papers III, p. 17ff", he noted in January 1921 in his manuscript in the beginning of several pages of mathematical calculations[36]. The reference to Rayleigh's study [52] confirms Prandtl's strategy: He approached the stability analysis from the limiting case of infinite Reynolds numbers. Like his boundary layer concept this approach would be restricted to flows at high Reynolds numbers–unlike Sommerfeld's method which encompassed the full range of Reynolds numbers.

Theodore von Kármán, Prandtl's former disciple, also participated in this discussion when he returned after the war to the Technische Hochschule Aachen in order to resume the buildup of the aerodynamics institute. Prandtl's and Kármán's groups were in close contact – and soon engaged in a fierce rivalry with regard to the theory of turbulence ([28], Chap. 5). Kármán "immediately rushed" his collaborators to undertake a stability analysis for certain piecewise linear flow profiles, Hopf informed Prandtl in February 1921 about the plans of his boss at Aachen[37]. Prandtl had by this time already asked a doctoral student to perform a detailed analysis. "Because it deals with a doctoral work, I would be sorry if the Aachener would publish away part of his dissertation", he asked Hopf not to interfere in this effort[38]. Kármán responded that the Aachen stability study was aiming at "quite different goals", namely the formation of vortices in the wake of an obstacle (labeled later as the "Kármán vortex street" after Kármán's earlier theory about this phenomenon ([28], Chap. 2)). The new study was motivated by "the hope to determine perhaps the constants that have been left indetermined in my old theory", Kármán calmed Prandtl's worry. Why not arrange a "division of labor" between Göttingen and Aachen, he further suggested, so that his group deals with these wake phenomena and Prandtl's doctoral student with boundary layer instability[39]. Prandtl relented and asked Kármán to feel free with his plans. With regard to his own approach he expressed confidence: "We have now a method to take into account friction approximately"[40]. A few months later, however, Prandtl reported that the calculations of his doctoral student yielded "a peculiar and unpleasant result". If the flow was unstable according to Rayleigh's inviscid theory, the instability was not reduced by taking viscosity into account – as they had expected – but increased, "and so, once more, we do not obtain a critical Reynolds number. There seems to be a very nasty devil in the turbulence so that all mathematical efforts are doomed to failure"[41].

Fritz Noether, too, resumed his attempts to come to grips with the onset of turbulence. He was well informed about the Göttingen effort as is evident from his correspondence with Prandtl[42]. The cause for this correspondence was a review on "The Turbulence Problem" which Noether prepared for the first volume of the *Zeitschrift für Angewandte Mathematik und Mechanik (ZAMM)*, a new journal edited by Richard

35 Hopf to Prandtl, 27 October 1919. GOAR 3684.
36 Pages 22-26, dated "5-8. 1. 21", Cod. Ms. L. Prandtl, 18, Acc. Mss. 1999.2, SUB.
37 Hopf to Prandtl, 3 February 1921. MPGA, III, Rep. 61, Nr. 704.
38 Prandtl to Hopf, 9 February 1921. MPGA, III, Rep. 61, Nr. 704.
39 Kármán to Prandtl, 12 February 1921. GOAR 3684.
40 Prandtl to Kármán, 16 February 1921. MPGA, III, Rep. 61, Nr. 792.
41 Prandtl to Kármán, 14 June 1921. MPGA, III, Rep. 61, Nr. 792.
42 Prandtl to Noether, 14 June 1921; Noether to Prandtl, 21 June 1921; Prandtl to Noether, 23 June 1921. MPGA, III, Rep. 61, Nr. 1155; Noether to Prandtl, 29 June 1921. GOAR 3684.

von Mises. Noether reviewed the previous attempts from a theoretical perspective. He also included in this review for the first time references to Orr's work – with the remark that it "has been unknown in Germany" ([53], p. 131). In order to provide a broader view, he formulated the stability problem for a general plane laminar flow, $U(y)$, instead of the more special plane Couette flow chosen in Sommerfeld's Rome paper. With this generalization the second-order equation for the amplitude ϕ of the disturbance in (1) has to be replaced by a fourth order equation

$$-\frac{i}{\alpha R}\left(\frac{d^4\phi}{dy^4}-2\alpha^2\frac{d^2\phi}{dy^2}+\alpha^4\phi\right)=\left(U-\frac{\beta}{\alpha}\right)\left(\frac{d^2\phi}{dy^2}-\alpha^2\phi\right)-\frac{d^2U}{dy^2}\phi. \quad (5)$$

In this form the "stability equation" or "perturbation differential equation" (to quote some contemporary designations) became famous as the "Orr-Sommerfeld equation" for the future research about the turbulence problem.

Von Mises's journal was also chosen as the appropriate organ for other articles on the turbulence problem. Ludwig Schiller, a physicist working temporarily in Prandtl's laboratory, surveyed the experimental efforts to measure the onset of turbulence [54]. Prandtl also revealed here for the first time some details about the effort of his doctoral student, Oskar Tietjens, whom he had asked to elaborate this part of his working program. In addition to Rayleigh's instable profiles they also analysed profiles that are stable in the inviscid case (i.e. profiles without an inflexion) – with the surprising result that these profiles also became unstable if viscosity was included. Contrary to the stability deadlock of the earlier studies concerning the plane Couette flow, Prandtl's approach left the theory in an instability deadlock. "We did not want to believe in this result and have performed the calculation three times independently in different ways. There was always the same sign which indicated instability" ([55], p. 434).

In September 1921 the Deutsche Physikalische Gesellschaft, the Deutsche Gesellschaft für Technische Physik and the Deutsche Mathematiker-Vereinigung convened their annual meetings of this year in a joint conference at Jena. The common event was supposed to be an opportunity "to bring to bear the areas of applied mathematics and mechanics to a higher degree than heretofore" [56]. Some of the Jena presentations were published in the *Physikalische Zeitschrift*, together with the ensuing discussion. Prandtl's "Remarks about the Onset of Turbulence" attracted particular attention because of the contradiction to the "dogma" of stability which resulted from the studies based on Sommerfeld's approach. Prandtl noted that these only addressed the "the so-called Couette case" with its linear velocity profile, but Sommerfeld found it "very strange and at first glance unlikely" that all flows are unstable except Couette flow: "what causes the special position of Couette flow?" Kármán hinted at Prandtl's piece-wise linear profiles with kinks at arbitrary positions as a cause of some "arbitrariness". Hopf regarded Prandtl's approximation $R \to \infty$ as problematic ([57], pp. 22–24). Noether had already expressed some doubts about Prandtl's approach in his correspondence before the Jena conference, although he belittled his dissent as merely a "difference of mindset and expression"[43]. Despite the conciliatory tone, however, it was clear from the outset that the turbulence problem lent itself for heated debate. The discussion showed that Prandtl's approach rather deepened the riddle. Hydrodynamic stability theory would not be cured from its troubles by reversing the stability dogma into the opposite.

The Jena conference and the articles in the first volume of *ZAMM* left no doubt that the old turbulence problem was perceived as a new challenge. Richard von Mises alluded to these and other riddles involved with turbulence, when he declared it "undecided whether the viscous flow approach is able to explain turbulence at sufficient

[43] Noether to Prandtl, 29 June 1921. GOAR 3684.

mathematical depth" ([58], p. 12). Of course, there were more riddles with which mathematicians, physicists and engineers were struggling when confronted with the gap between theory and practice, but few would surpass the challenge of the turbulence problem. Prandtl proposed the foundation of a "federation of all like-minded" as an umbrella organization for future combined meetings of physicists and mathematicians. "I have discussed about it with Kármán and received his full approval", he informed Richard von Mises a few weeks before the Jena meeting. "We suggest the foundation of an 'Association for Technical Mechanics' with the exclusive purpose to prepare and convene meetings for that specialty". He aimed at "scientific engineers" as addressees[44]. Von Mises shared Prandtl's general motives, but disagreed with regard to the label "technical mechanics" for the new association. He favoured the same name for the new society as for his journal. "Against your proposal 'applied mathematics and mechanics' I will insist on mine (technical mechanics)", Prandtl countered[45]. It took another year until the mutual interests were brought in line. With regard to the title, Prandtl finally gave in. The new association was founded in 1922 as Gesellschaft für Angewandte Mathematik und Mechanik (GAMM). But with Prandtl as chairman and Richard von Mises as managing director, the GAMM became just the kind of organization that Prandtl had envisioned from the very beginning[46].

Conclusion and outlook

Despite vigorous efforts, the turbulence problem remained a subject of frustration and controversial results. Tietjens was not able to elaborate Prandtl's approach so that a discrimination between stable and unstable flows was obtained [60]. However, this frustrated effort paved the way for another Prandtl disciple, Walter Tollmien, who applied the Orr-Sommerfeld method to more realistic flow profiles without kinks. Tollmien finally succeeded to derive a critical limit of stability. He was able to display the stable and unstable states of flow as a function of the Reynolds number and the wavelength of the disturbance [61]. In the early 1930s, Hermann Schlichting, another disciple of Prandtl, further extended this theory so that the instability of laminar boundary layer flow could be analysed in more detail [62].

The boundary layer flow along flat plates was, of course, not the only flow configuration which was made subject of stability analyses. Werner Heisenberg, Sommerfeld's prodigy student, studied in his doctoral dissertation plane Poiseuille flow (i.e. a parabolic velocity profile) by the Orr-Sommerfeld method – and also found a limit of stability [63]. "The hydrodynamic explanation of turbulent flow in tubes or channels is a problem that is famous for its difficulty", Sommerfeld praised in his report to the faculty Heisenberg's work. "I could not have proposed such a difficult theme to another one of my disciples". But he added the sobering remark that it "would not have been possible to arrive at the many remarkable results if the author had stood to exact error estimates". In view of the involved asymptotic approximations there remained "much to be done in mathematical regards"[47].

Six years after his survey of the turbulence problem, Fritz Noether scrutinized the Orr-Sommerfeld approach in another study in *ZAMM* and arrived at the conclusion that this method was in principle unable to yield a critical limit for the stability of plane flows [64]. Theodor Sexl, a theoretical physicist from Vienna who spent a

[44] Prandtl to Mises, 2 August 1921. MPGA, III, Rep. 61, Nr. 1078.

[45] Prandtl to Mises, 9 August 1921. MPGA, III, Rep. 61, Nr. 1078.

[46] It is not the subject of this paper to trace the foundation and early history of this organisation. For an overview see [59].

[47] Sommerfeld, report to the faculty, 23 July 1923. Munich, University Archives, OC-I-49p.

research sojourn with Kármán in Aachen, analysed the stability of three-dimensional Poiseuille flow in circular pipes [65–67] – and corroborated Noether's verdict for this flow configuration also. Once more there seemed to be no way out of the stability deadlock. With these results, both Heisenberg's and Tollmien's studies were cast in doubt. Their use of the Orr-Sommerfeld method must have involved some error, or perhaps unjustified approximations, if it yielded a critical limit between stable and unstable modes of flow – or the host of evidence for the stability dogma was wrong.

By the 1930s the Orr-Sommerfeld approach had become notoriously famous for its difficulty. "Every investigation on hydrodynamical stability has a tang of excitement", Synge introduced his address at the Semicentennial of the American Mathematical Society, "the result obtained may confirm or undermine a theory now a century old" ([4], p. 228). When he prepared this address he asked Richard von Mises about his proof of the stability of plane Couette flow from the year 1911: "I have not been able to convince myself that this important result is established by your work", he ventured to critisize Mises's historic paper. He also raised doubts about Hopf's result: "on account of the necessity of employing asymptotic expressions for the Hankel functions, it seems that Hopf's proof is not complete"[48]. The reply was astonishing: "Professor von Mises has informed the writer", Synge reported in a footnote of his published address, "that he does not regard his own proof of the stability of P.C.M [= Plane Couette Motion] as adequate, nor does he accept the proof of Hopf" ([4], p. 262).

There is no happy end to this story. The troubles with the Orr-Sommerfeld approach have never been resolved satisfactorily. The disputed Tollmien-Schlichting theory was corroborated experimentally in wind tunnel experiments during the Second World War, and an ensuing review of the Orr-Sommerfeld approach confirmed most of Heisenberg's and Tollmien's results [68, 69] – but the contradictory evidence for the stability dogma could not be disproven. At the centenary of Sommerfeld's birth in 1968 Heisenberg remarked with regard to Noether's "proof" in 1926 that "it is not known what is actually wrong in the work of Noether" ([70], p. 47). But it was not only the involved mathematics which caused trouble. The critical Reynolds number for the onset of turbulence in pipe flow, for example, could not be determined experimentally at a precise value but only within an interval between 200 and 25 500, depending on the entrance into the pipe, the roughness of the wall, and other factors. Both from an experimental and a theoretical perspective, the physical mechanisms which render a laminar flow turbulent remained a challenge. Although the Orr-Sommerfeld approach was able to overcome the stability deadlock for plane Poiseuille flow and boundary layer flow, the experimentally observed critical Reynolds numbers for the onset of turbulence are not in agreement with the theoretical predictions ([5], p. 578).

If the approach, by and large, failed to meet the high expectations with which its proponents had pursued it as a key for explaining the origin of turbulence, it nevertheless became a resource for further efforts like few other concepts in fluid dynamics. In retrospect, even apparent dead-ends turned out to show one or another way out of an earlier dilemma. Prandtl's and Tietjens's work of 1921, for example, foreshadowed the view that viscous amplification (at the critical layer where the celerity of the disturbance equals the velocity of the main flow) plays a crucial role for boundary layer instability ([1], p. 295; [61], p. 26). Even more gems were contained in Orr's study from the year 1907: unlike Sommerfeld, who formulated the stability problem for viscous plane Couette flow in a straight forward manner, Orr reviewed and scrutinized the stability theory in a more systematic and comprehensive manner. The difference between Sommerfeld's and Orr's papers is already apparent from their length: 10 pages versus 129 pages, respectively. About half of Orr's work was focused on inviscid flow.

[48] Synge to Mises, 30 May 1938. Harvard University Archives, Pusey Library, Richard von Mises Papers.

In the other half, Orr considered both plane Couette and plane Poiseuille flow. Both the inviscid and viscous parts contain gems which were recognized only much later. Orr exposed, for example, short-term instabilities of inviscid flow which seem to play an important role also for the transient viscous instability ([8], p. 120). As was noted in Orr's obituary in 1935, "much of Orr's work was of the unseen type, but everything he wrote contained something of permanent value" [71]. The difference in scope and motivation also explains why there was no rivalry between Orr and Sommerfeld. By the 1920s, when Orr's treatise finally received the attention of German mathematicians, neither Orr nor Sommerfeld seem to have regarded their approach worth to recollect the circumstances which led to its formulation two decades earlier – all the more after Noether declared it as inappropriate to solve the turbulence problem. But, as I remarked in the introduction, Orr's work merits a study in its own right in order to expose its hidden gems and evaluate its role for the history of hydrodynamic stability theory in more detail.

From another vantage point, the troubled history of the turbulence problem (as defined in the wake of the Orr-Sommerfeld approach a hundred years ago) amounts to conclusions concerning the equally troubled history of science-technology relations. Hydrodynamic stability theory emerged from the 19th century as a specialty which appealed particularly to mathematicians and theoretical physicists. Since the 1920s, it became a particular concern in applied institutions, in particular in aeronautical research laboratories. With the ramifications into fields of tremendous practical importance, the theory became subject of considerable research efforts in many countries. The establishment of a series of International Congresses for Applied Mechanics further contributed to provide an arena where problems like turbulence became subject of wider debates. The internationalization under the umbrella of applied mechanics further relocated the problem within the realms of engineering science. The rapprochement to the "applied" was, of course, dependend on the different national traditions, but it seems to have been a universal phenomenon in the period between the two world wars. In Germany it became manifest since 1920 with the foundation of *ZAMM*, GAMM and other technical journals and societies. Subjects like turbulence appealed more to the mathematician and the engineer than to the physicist ([72, 73]; [28], Chap. 4).

Although theoretical physicists (like Heisenberg and Sexl) further contributed to the old and new riddles involved with the theory of turbulence, those who jumped on the bandwagon of this research were more likely rooted in "applied" institutes like those of Prandtl in Göttingen or Kármán in Aachen and Pasadena (where Kármán directed since 1930 the new Guggenheim Aeronautical Laboratory of the California Institute of Technology, GALCIT). When Sommerfeld conceived in 1944 a paragraph "On Turbulence" for his textbook on the "Mechanics of Deformable Bodies", he asked Prandtl for help with this "particularly problematic paragraph"[49]. Heisenberg's work was corroborated by the same time in a doctoral dissertation supervised by Kármán at the GALCIT. The National Advisory Committee for Aeronautics (NACA) translated Heisenberg's *Annalen der Physik*-paper and published it in its series of *NACA Technical Memoranda* [74]. One could not think of a more telling expression for the relocation of a topic, which had once belonged to the core of theoretical physics, under the realm of the applied.

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[49] Sommerfeld to Prandtl, 15 February 1944. MPGA, III, Rep. 61, Nr. 1538.

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Abbreviations

ASWB: Arnold Sommerfeld. Wissenschaftlicher Briefwechsel. Band I: 1892-1918; Band II: 1919-1951. Herausgegeben von Michael Eckert und Karl Märker. München, Berlin, Diepholz: Deutsches Museum und GNT-Verlag, 2000 und 2004.
DMA: Deutsches Museum, Archiv, München.
GOAR: Göttinger Archiv des Deutschen Zentrums für Luft- und Raumfahrt (DLR), Göttingen.
MPGA: Max-Planck-Gesellschaft, Archiv, Berlin.
RANH: Rijksarchief in Noord-Holland, Haarlem.
SUB: Staats- und UniversitÄtsbibliothek, Göttingen.
TKC: Theodore von Kármán Collection, Pasadena.

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The troublesome birth of hydrodynamic stability theory: Sommerfeld and the turbulence problem

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