Turbulence before 1961

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Abstract

The surge of turbulence research in the twentieth century is reviewed with the focus on the institutional environment that provided an umbrella for the activities in this field. Before the Second World War the International Congresses for Applied Mechanics served as the main stage for presenting research results on turbulence. After the War, the International Union of Theoretical and Applied Mechanics, organizations like the Division of Fluid Dynamics of the American Physical Society and new journals (the *Journal of Fluid Mechanics* and *The Physics of Fluids*) added to the institutional framework for turbulence activities and publications. The surge of these activities is illustrated with examples from the correspondence of some of the involved actors (like George K. Batchelor and Francois N. Frenkiel, the editors of the new journals; both were also involved with the organization of the Marseille events in 1961).

Introduction

This is not the first attempt to review many decades of turbulence research. Ten years ago, two heros of this discipline – if it may be called a discipline – reviewed "A Century of Turbulence" [Lumley and Yaglom, 2001]. Recently a "A Voyage Through Turbulence" explored the work of pioneers such as Osborne Reynolds or Andrey Nikolaevich Kolmogorov [Davidson et al., 2011]. Since 1969, when the first issue of the *Annual Review of Fluid Mechanics* appeared, many subfields of turbulence have become subject of critical survey. Furthermore, textbooks on turbulence tend to include historical surveys. Based on the citations in [Monin and Yaglom, 1971, 1975] the reviewers of

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"A Century of Turbulence" derived a formula for the exponential growth of the annual output of papers on turbulence since 1900 and before 1965, the year when the Russian original of this classic was published [Lumley and Yaglom, 2001, p. 242]. Their formula yields for the period between the end of the Second World War and the time of the Marseille events in 1961 an annual growth from about 20 to 76 articles, and a total of 668 articles within these 16 years. The total amount of turbulence papers from the beginning of the 20th century until 1961 is 895. Although these numbers should not be taken at face value (Lumley and Yaglom admit fluctuations of about $\pm 20\%$ near 1900 and $\pm 2.5\%$ near 1965) they certainly provide a lower limit for the order of magnitude of the quantitative output of turbulence research before Marseille.

Given the exponential growth and amount of turbulence research even before 1961 – the exponential growth agrees with the overall tendency in scientometrics in this period [de Solla Price, 1963] – it is quite obvious that a survey based on the content of these research papers exceeds the space of a single review paper. Even a selection of "some key developments in turbulence research" yields more than fifty publications prior to Marseille; for an overview along these lines I refer to the "Voyage through Turbulence" [Davidson et al., 2011, Chapter 13]. Instead I will focus here on the environment in which turbulence emerged as an ever expanding research activity. Thus the focus is shifted from the intellectual history of turbulence to the circumstances in which turbulence became an ever growing research effort.

Early traditions of scientific engineering

The history of turbulence in the 19th century is mainly connected with the names of Adhémar Jean Claude Barré de Saint-Venant and Joseph Boussinesq in France, and with William Thomson (Lord Kelvin), John William Strutt (Lord Rayleigh) and, in particular, Osborne Reynolds in Great Brittain [Darrigol, 2002, 2005, chapter 6]. In France, these contributions were rooted in the tradition of scientific engineering as it emerged in the 18th century at the École des Ponts et Chaussées and the École Polytechnique, fostered by national state policy. In Great Britain, the connex with national policy was less explicit, although here, too, science did not flourish in isolation from practical tasks. Reynolds was praised in an obituary as Great Britain's "most distinguished scientific engineer" [Launder and Jackson, 2011, here p. 35].

In Germany, the tradition of scientific engineering grew at the Polytechnic Schools founded in the first half of the 19th century after the model of the École Polytechnique. However, these Schools attained equal rights with the traditional Universities only around 1900. Ludwig Prandtl, Germany's rising star on the sky of 20th century turbulence [Bodenschatz and Eckert, 2011], began his career at the Technical Universities (how the Polytechnic Schools are called today) in Munich and Hanover, but rose to prominence at the University of Göttingen. At this university there was a strong tradition of mathematics and physics (initiated by Carl Friedrich Gauss and Wilhelm Weber) to which the entrepreneurial mathematician Felix Klein added applied sciences. Prandtl was called to Göttingen as an expert of applied mechanics. Thus, by the beginning of the 20th century, in Germany too the tradition of scientific engineering had taken hold in an academic environment.

Scientific engineering, however, was not enough to provide a common framework for turbulence research. Nor was it the only tradition. Scientists interested in problems such as flow instabilities or vortex formation did not belong to just one discipline. These problems also attracted physicists and mathematicians with no interest in engineering applications. Prandtl introduced his boundary layer approach at the Third International Congress of Mathematicians in August 1904 in Heidelberg – although he did not care much about the involved mathematics and rather illustrated flow separation by sketches and photographs [Prandtl, 1905]. Four years later, Arnold Sommerfeld, professor of theoretical physics in Munich, introduced what became known as the Orr-Sommerfeld method to account for the instabilty of flows at the Fourth International Congress of Mathematicians in 1908 in Rome [Sommerfeld, 1909]. He was not aware of William McFadden Orr's more extensive work [Orr, 1907]. Other mathematicians and physicists also did not take note of Orr's work. Even in Great Britain Orr's contribution seems to have been ignored for some years. Horace Lamb, the author of the well-known textbook on hydrodynamics, still did not mention Orr's publication in 1910 when Sommerfeld asked him about new publications on turbulence.[1]

The lack of a common framework is also apparent from the journals in which the pertinent studies were published. There was no specialized international society on fluid mechanics, not to speak of turbulence. If a research

[1]Lamb to Sommerfeld, 12 September 1910. DMA, HS 1977-28/A,189. In the fourth edition of *Hydrodynamics*, however, Lamb acknowledged that the stability equation for plane Couette flow was "given by Orr, and afterwards independently by Sommerfeld" [Lamb, 1916, p. 659]. This seems to be the first time when both Orr and Sommerfeld appeared together. On the early history of the Orr-Sommerfeld approach in Germany (where Orr's work became known only in the 1920s) see [Eckert, 2010].

result was not presented at a meeting such as the International Congress of Mathematicians or an annual conference like the gatherings of the Deutsche Mathematiker-Vereinigung [von Mises, 1912], the authors published their articles on turbulence in the diverse journals such as the *Philosophical Magazine* [Rayleigh, 1892] or the *Zeitschrift für Mathematik und Physik* [Hahn et al., 1904]. The most frequent mode of publishing was to address an academy. In Orr's case this was the Royal Irish Academy. Sommerfeld, in his capacity as a member of the Bavarian Academy of Science, presented some of his pupils's work in the proceedings of this academy [Haupt, 1912, Blumenthal, 1913, Noether, 1913]. Other pertinent papers on one or another aspect of turbulent flow appeared in the transactions of the academies in Amsterdam [Lorentz, 1897], Göttingen [Prandtl, 1914, Noether, 1917], London [Reynolds, 1883, 1895] or Paris [Saint-Venant, 1850, Boussinesq, 1870].

The International Congresses for Applied Mechanics, 1922-1938

By the early 1920s, the situation began to change. In part, the growing awareness for "applied" specialties in science resulted from the rise of aeronautical research, where distinguished academics like Lord Rayleigh or Prandtl assumed the role of advisors. After the First World War, applied mechanics was perceived as a discipline of its own right.

In Germany, Prandtl suggested to create a "federation of all like-minded," as he wrote to Richard von Mises in 1921. He had already discussed his plans with his former disciple Theodore von Kármán, who was now director of the institute for aerodynamics at the Technical University in Aachen. "We suggest the foundation of an 'Association for Technical Mechanics' with the exclusive purpose to prepare and convene meetings for that specialty".[2] The audience would be the "scientific engineers". Von Mises addressed the same group as editor of a new journal, the *Zeitschrift für Angewandte Mathematik und Mechanik (ZAMM).* In his editorial he had discerned turbulence among other problems as a particular challenge [von Mises, 1921a, p. 11-12]. Although Prandtl and von Mises did not agree entirely about the new organization (it was founded in 1922 as Gesellschaft für Angewandte Mathematik und Mechanik, GAMM, against Prandtl's and von Kármán's proposal to focus on "technical mechanics"), they shared the concern about turbulence.

Although it was hardly visible from his publications, Prandtl had a longstanding affiliation with turbulence [Bodenschatz and Eckert, 2011]. In

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

1914 he had interpreted Gustave Eiffel's discovery of a sudden reduction in the drag coefficient of spheres at high flow velocities as a consequence of the transition to turbulence in the boundary layer [Prandtl, 1914]. In 1916 he had drafted a "working program for a theory of turbulence," where he discerned the onset of turbulence and fully developed turbulence as the two major problem areas for future research. In 1921, when the Deutsche Physikalische Gesellschaft, the Deutsche Gesellschaft für Technische Physik and the Deutsche Mathematiker-Vereinigung combined their annual meetings "to bring to bear the areas of applied mathematics and mechanics to a higher degree than heretofore," as von Mises reported in his new journal [von Mises, 1921b], Prandtl used the occasion for "Remarks about the Onset of Turbulence" [Prandtl, 1921] that were intended to break the deadlock of previous studies. At the same conference, Ludwig Schiller, a physicist working temporarily in Prandtl's laboratory, surveyed the experimental investigations about the onset of turbulence [Schiller, 1921]. These contributions served at the same time to shape the profile of von Mises new journal, the *ZAMM*. Fritz Noether reviewed "the turbulence problem", as the riddle to determine the onset of instability became called, from a theoretical perspective. His review alerted the German readers to Orr's work. Future studies of Orr's and Sommerfeld's linear perturbation theory usually took Noether's paper as their starting point [Noether, 1921].

The formation of the community of applied mathematics and mechanics was not limited to Germany. In 1922, Tullio Levi-Civitá and Theodore von Kármán organized a gathering of like-minded mathematicians, physicists and engineers with an interest in fluid mechanics at Innsbruck [Battimelli, 1996, von Kármán and Levi-Civitá, 1924]. In the following year the momentum of these events was carried on to the Netherlands, where Johannes (Jan) Martinus Burgers and C. B. Biezeno organized the First International Congress for Applied Mechanics, held in April 1924 at Delft [Alkemade, 1995, Battimelli, 1988]. Two years later, the Second International Congresses for Applied Mechanics was held in Zurich, Switzerland. Thereafter, they were held every four years in another country (1930 Stockholm; 1934 Cambridge, UK; 1938 Cambridge, Massachusetts, USA) – until the Second World War caused an interruption.

From the very beginning, these congresses were regarded as the preferred sites for presenting novel research on turbulence. Their importance can hardly be exaggerated. At each one of these meetings crucial achievements were introduced and discussed. A few examples may suffice. At Innsbruck, Kármán presented his most recent theory on turbulent skin friction [von Kármán, 1921, 1922], and Werner Heisenberg, Sommerfeld's prodigy student, reported about some results of his early struggle with the turbulence problem that subsequently became the subject of his doctoral work [Heisenberg, 1922, 1924]; at the Zurich Congress in 1926, Prandtl introduced the mixing length concept [Prandtl, 1927]; four years later Kármán chose the Stockholm Congress to introduce a similarity concept that gave rise to "Kármán's constant" and a logarithmic law for turbulent skin friction [von Kármán, 1930]. These Congresses, Kármán recalled in his autobiography, served as the "playing field" for his competition with Prandtl. "The 'ball' was the search for a universal law of turbulence" [von Kármán, 1967, p. 134].

At the Fourth International Congress for Applied Mechanics in Cambridge, UK, the interest in turbulence was so great that the organizers of the Fifth Mechanics Congress decided to hold a special symposium on turbulence within that Congress. "Professor Prandtl kindly consented to organize this Symposium," the organizers reported in the proceedings. They regarded this Symposium "not only the principal feature of this Congress, but perhaps the Congress activity that will materially affect the orientation of future research." Taylor's statistical theory of turbulence had been published just three years ago; his survey lecture [Taylor, 1938], Hugh Dryden's communication about experimental measurements of the spectrum of turbulence at the National Bureau of Standards in Washington D.C. [Dryden, 1938] and numerous other contributions illustrated that the focus had shifted from the onset of turbulence to the statistical theory of fully turbulence as the major concern.

One reason for this shift was the pertinence of this theory for wind tunnel turbulence and the perfection of hot-wire anemometry. "The desire to make some direct measurement of the turbulence in wind tunnels was the incentive for the work here described," Dryden and a collaborator of his department at the Bureau of Standards had argued in 1929 in a Technical Report of the National Advisory Committee for Aeronautics (NACA). They were optimistic that the method also advanced the statistical theory of fully developed turbulence, because the "great need at the present time in the further development of the theory of turbulence is more experiments on the actual fluctuations to supplement the data already available on the distribution of mean velocity" [Dryden and Kuethe, 1929a, p. 361]. In a subsequent report they concluded "that turbulence is a variable of some importance at all times and that the careful experimenter will desire to measure and state its value in order that his experiments may be capable of interpretation" [Dryden and Kuethe, 1929b, p. 166].

The rise of the statistical theory of turbulence, therefore, was closely related with the measurements of wind tunnel turbulence. At the same time, the onset of turbulence in the boundary layer of a model in a wind tunnel was found to depend critically on the degree of turbulence in the air-stream. Taylor elaborated this view in more detail at the turbulence symposium. He could even present a formula that expressed the critical Reynolds number for the transition to turbulence in the boundary layer of a sphere exposed to the turbulent fluctuations caused by the grid in a wind tunnel [Sreenivasan, 2011, p. 151-152]. With the obvious role of external disturbances as a cause for the transition to turbulence, the Orr-Sommerfeld-approach appeared unlikely to explain the onset of turbulence. "It seems to me not only that that it is not proved that the boundary layer is unstable," Taylor concluded his survey, but that also "the experimental evidence is against the instability theory" [Taylor, 1938, p. 308].

While the stability theory lost ground at the International Congress of Mechanics, it was not entirely abandoned. John L. Synge, head of the Department of Applied Mathematics at the University of Toronto, chose this theme in 1938 for a review lecture at the occasion of the fiftieth birthday of the American Mathematical Society. "It presents mathematical problems of no small difficulty: triumphs are few and disappointments many," he alluded to the long history of this problem. However, he also made clear that it was not *the* turbulence problem. "It is concerned with the initial stage of turbulence – its generation from steady flow – but not with turbulent motion, once established" [Synge, 1938, p. 227].

Thus, the two paths of turbulence research that had been discerned early in the twentieth century, the onset and fully developed turbulence, had matured by the late 1930s into well distinct avenues. The former developed a life of its own with the focus on the Orr-Sommerfeld approach. The latter fell into the realm of statistical theory. Shortly after 1938, a most spectacular breakthrough in the statistical theory of turbulence was accomplished in Russia. It was connected mainly with the names of Andrey Nikolaevich Kolmogorov and Alexander Mikhailovich Obukhov [Falkovich, 2011]. It was published in 1941 and is known today as K41 or KO41. However, it remained unknown to the world outside Russia until after the war. Its impact on the international community and its role for shaping the future course of turbulence research, therefore, belong to the postwar history of turbulence.

World War II: jets, laminar wings and other wartime researches on turbulence

The bulk of turbulence research during World War II was performed in the context of one or another aeronautical war project. One of these concerned the investigation of turbulence in jets. It was performed under contract with the NACA at the Guggenheim Aeronautical Laboratory of the California Institute of Technology (GALCIT) and carried out by Kármán's disciple Stanley Corrsin, who acclomplished in 1942 his thesis as an Aeronautical Engineer with a study about the "Decay of Turbulence Behind Three Similar Grids". Other disciples of Kármán involved in turbulence research were Hans Liepmann, Francis Clauser and Chia Chiao Lin. In this environment many aspects of turbulent flows, theoretical and experimental, were investigated. In his jet study, for example, Corrsin employed a novel hot-wire measuring set. The results were published in confidential NACA Wartime Reports under the title "Investigation of flow in an axially symmetrical heated jet of air" and "Investigation of the behavior of parallel two-dimensional air jets." Thus began an outstanding career of a rising American star in turbulence [Meneveau and Riley, 2011].

Another NACA program was concerned with laminar wings and low-turbulence wind tunnels. By 1938, Eastman Jacobs, the head of the Variable-Density-Tunnel team at the NACA's Langley laboratory, had elaborated by trial and error a profile shape that would keep the boundary layer for most of the upper part of the wing in the laminar state. In order to investigate such profiles a novel "Two-Dimensional Low-Turbulence Pressure Tunnel" was developed. In its Annual Report for 1939, published in 1940, the NACA announced that "a new principle in airplane-wing design" had been discovered which reduced the drag of the wing "by approximately two-thirds" [Hansen, 1987, pp. 109-116]. In the same year the new principle was made available to the American aircraft industry. Attached to a fighter plane, the North American P-51 Mustang, low-drag wings became a major asset of NACA's contribution to the war effort [Roland, 1985, vol. 2, p. 549].[3]

[3] However, the laminar flow virtues were hardly realized in practice because of manufacturing irregularities [Roland, 1985, vol. 1, p. 194]. A recent analysis (Bernd Krag: The North American P-51 'Mustang' and the Laminar Flow Wing: A Success Story or just an Illusion?, available at http://wp1113056.wp148.webpack.hosteurope.de/ABL/20-forschung/laminarfluegel/laminarfluegel_en.htm) concluded "that the performance of the Mustang could not be attributed to its laminar flow airfoil. It was the overall low drag design of this aircraft with clean surfaces including the careful design of the radiator that was the key of its good performance."

Low-turbulence wind tunnels and low-drag wings came along with new fundamental research on the transition from laminar to turbulent flow in the boundary layer. The NACA issued research contracts to Dryden's department at the National Bureau of Standards in Washington, D.C., as well as to Kármán at the GALCIT in Pasadena.[4] In the course of these investigations, performed in special low-turbulence tunnels with sophisticated hot-wire equipment, both groups discovered the boundary layer instability predicted by Tollmien and Schlichting. The results were communicated in NACA wartime reports [Schubauer and Skramstad, 1943, Liepmann, 1943, 1945]. With the experimental corroboration of the long-disputed Orr-Sommerfeld approach, von Kármán regarded a revision of the stability theory expedient. He suggested it to Chia-Chiao Lin as subject of a doctoral dissertation. Lin was at that time not yet naturalized as a US citizen and thus not cleared for confidential war research. Therefore, he could not perform his PhD-work in official collaboration with related NACA-sponsored research such as Liepmann's experimental investigation. The title of Lin's thesis, "On the Development of Turbulence," evoked no association with wartime research. In the abstract, Lin characterized his work as "based upon a study of the equation of Orr and Sommerfeld along the lines initiated by Heisenberg." Lin's work, therefore, appeared more as a fundamental contribution to an old riddle of theoretical physics rather than as a study motivated by wartime research on low-drag wings [Lin, 1944].

In Germany, the Tollmien-Schlichting theory had been further pursued during the war with the goal to determine the onset of turbulence in the boundary layer along curved surfaces. Important results, such as the instability along concave walls ("Görtler instability") were even published in the open literature [Görtler, 1940a,b, 1941]. The effort, however, was mostly theoretical. When Schlichting surveyed the state of stability theory in a wartime review lecture in 1941 he could not offer other experimental evidence than images from a water channel published in 1933 where the formation of vortices along the wall indicated some instability [Schlichting, 1941, p. 39]. The prospects of laminar profiles for practical application were regarded with more soberness than in America. The advantage of low drag involved disad-

[4] Material on this research is preserved in the Dryden Papers, Special Collections, The Milton S. Eisenhower Library, The Johns Hopkins University, here in Subject-Files, Box 43 ("Miscellaneous Correspondence and Materials re the Bureau of Standards, 1920-1947") and Correspondence-Files ("1935-59, Misc. Dryden-Von Kármán Correspondence"); see also the Dryden Correspondence in the Theodore von Kármán Collection (http://caltechbook.library.caltech.edu/273/1/vonKarmanCollectionGuide.pdf), here box 7.26.

vantages for other aerodynamic profile properties, as Schlichting concluded in his survey. But the German aerodynamicists closely observed the low-drag research in other countries. Schlichting displayed for example measurements from a Japanese laminar profile [Schlichting, 1941, p. 30]. Until almost the end of the war, numerous tests of more or less laminar wing profiles were undertaken and communicated in wartime reports, see e. g. [Schlichting and Bußmann, 1942, Breford and Müller, 1943, Kopfermann and Breford, 1943, Riegels and Liese, 1943, Doetsch, 1944]. These measurements, however, were inconclusive with regard to the role of the Tollmien-Schlichting instability because they were not made in low-turbulence wind tunnels. Although the Germans built such tunnels, they did not become operative for laminar wing tests before the end of the war [Holstein, 1946].

The wealth of wartime research on turbulence in Germany addressed the fully developed turbulence in the flow along smooth and rough walls, in pipe and channel flows, jets and layered streams. The results were circulated in dozens of wartime reports, most of them authored by Prandtl's disciples (besides Tollmien and Schlichting in particular Hans Reichardt, Karl Wieghardt and Fritz Schultz-Grunow; see the postwar reviews on turbulence [Görtler, 1953, Prandtl, 1953].) However, with the exception of a spectacular unpublished work of Prandtl during the last months of the war [Bodenschatz and Eckert, 2011], most wartime achievements concerned practical applications rather than the fundamental nature of turbulence.

Although these are just glimpses into the wartime research in the USA and Germany, we may safely assume that comparable efforts were made also in other countries such as France, Great Britain, Japan and Russia, where the same issues will have appeared on the agenda of aeronautical research establishments.

1946 - a year of revelations

The first postwar International Congress for Applied Mechanics, held from 22 to 29 September 1946 in Paris, was a remarkable event in the history of turbulence. Like in the preceding 1938 Congress, a special symposium on turbulence was organized. Dryden lectured at this symposium about "Some Recent Contributions to the Study of Transition and Turbulent Boundary Layers" – revealing the wartime work of his group at the National Bureau of Standards that resulted in the discovery of Tollmien-Schlichting waves [Dryden, 1947]. Another speaker at this symposium was George K. Batchelor from Cambridge, England, who reported about a "remarkable series of coincidences" concerning the theory of isotropic turbulence. Batchelor informed the attendees about Kolmogorov's 1941 papers and the theories elaborated independently from another in 1945 by Onsager in the USA and by Carl Friedrich von Weizsäcker and Werner Heisenberg in England during their detention at Farmhall [Batchelor, 1946].

There are fascinating first-hand accounts how Batchelor had become aware of Kolmogorov's now famous papers in the English language editions of the 1941 issues of the proceedings of the Russian Academy of Sciences, and how he became involved himself in turbulence research in 1945 [Batchelor, 1996, Moffat, 2011]. Onsager's motivation and approach has also been analysed in great detail in [Eyink and Sreenivasan, 2006]. It only remains to add a few remarks about the circumstances in which Weizsäcker and Heisenberg began to work the same problem in summer 1945. One of the ten detainees, with whom they were interned at Farmhall, was Heisenberg's former disciple Erich Bagge. From his notes in a diary we learn that it was Weizsäcker rather than Heisenberg who directed the detainees' attention to turbulence. In order to keep abreast of research in physics, the detainees arranged among themselves a regular colloquium on physical problems close to their contemporary interest. Heisenberg, for example, lectured once on the theory of diamagnetism (but never on turbulence), Otto Hahn on methods how to determine the age of the earth, and Weizsäcker on cosmogony, a theory in which assumptions on the turbulence of cosmical gas play a major role. On Friday, 25 May 1945, Bagge entered in his diary: "This day was determined by a colloquium on turbulence." On the preceding Tuesday Weizsäcker had lectured on Burgers's model theory on turbulence, "and he continued with it today," Bagge noted. "A very nice idea resulted from the discussion about this work. Burgers derives from his simplified onedimensional model a theorem, which holds perhaps also for real turbulence, namely: the loss of energy per unit of time is the same for each Fourier term of the turbulent velocity." From this assumption it was only a small step to formulate the question from which Weizsäcker and Heisenberg developed their approach: "Can one pursue the theory of turbulence by assuming that this theorem holds also for the threedimenional case? What consequences follow from this assumption?" [Bagge et al., 1957, pp. 48-53]. Thus they began to elaborate the theory which they presented to Taylor and Batchelor one day in August 1945 during a short visit [Batchelor, 1996, p. 171-172] and which Batchelor briefly reviewed a year later at the Paris Congress together with Kolmogorov's and Onsager's work.

Batchelor would have been surprised to learn that Prandtl, too, had arrived independently at some of the K41 results. Prandtl's notes on this theory began in autumn 1944 and were pursued until summer 1945. Prandtl's elaboration of fully developed turbulence down to the Kolmogorov length scale in terms of an energy cascade has to be added to the "remarkable series of coincidences" about which Batchelor had reported at the Paris Congress. Prandtl's approach, however, remained largely hidden. It had no impact on the course of events that led to the Marseille events in 1961. Except a paper published together with Wieghardt in 1945 that contained some of the involved energy considerations [Prandtl and Wieghardt, 1945], and hints at an unpublished paper "on the role of viscosity in the mechanism of developed turbulence" at the end of his FIAT report [Prandtl, 1953, p. 77], there was nothing that could have stirred broader interest. On a timeline, Prandtl's K41 contribution has to be placed after the pioneering Russian work and before the contributions of Onsager, Weizsäcker and Heisenberg. A first analysis is presented in [Bodenschatz and Eckert, 2011], but the accomplishment is worth more extended scrutiny. From a historical perspective, it belongs to the category of multiple simultaneous discoveries, such as the discovery of energy conservation in the 19th century that has been analysed by Thomas S. Kuhn in a classic study [Kuhn, 1959].

Batchelor's and Dryden's lectures on the K41 accomplishment and the experimental verification of the Tollmien-Schlichting process were not the only revelations that made the Paris Congress so remarkable. In his four-page report for *Science*, Dryden singled out several other contributions to the turbulence symposium: J. O. Hinze's on the mechanism of disintegration of high-speed liquid jets; J. Kampe de Feriet's critique of the Reynolds averaging; Alexandre Favre's description of an apparatus for the measurement of time correlations in turbulent flow; Lin's revision of the Orr-Sommerfeld approach on hydrodynamic stability; Francois Naftali Frenkiel's turbulence measurements conducted in his doctoral work under the supervision of Kampe de Feriet [Dryden, 1947].

Unfortunately, the proceeding from this Congress have never been published. But its enormous international attendance (according to Dryden there were "about 100 Englishmen; 100 Frenchmen; 52 Americans; large delegations from Belgium, Holland, Italy, Switzerland, Roumania, and Czechoslovakia; many from Sweden, Turkey, and Poland; 3 from Russia; and 2 from China") suggests that the international research on turbulence was fairly well represented at Paris – except the work of the Germans who were not yet admitted. Those singled out in Dryden's report would present their work on turbulence again fifteen years later at Marseille. If there is a single event which started the route to Marseille 1961, this is the turbulence symposium at the Paris Congress for Applied Mechanics in 1946.

IUTAM

However, there are rarely unique single causes for major events in the history of science. We have to take into account the international postwar reconstructions on a larger scale. Although the International Congresses for Applied Mechanics were organized by international congress committees, there was no institutional umbrella in the form of an official international organisation. In 1946, Burgers suggested to found such an organisation in order to lay down a more permanent international basis. He envisioned a Union of Applied Mechanics as part of the International Council of Scientific Unions (ICSU), which had served already before the war as an umbrella of several disciplines [Greenaway, 1996].

Once more, the missionaries for international science, von Kármán and Burgers, whose initiative had given birth to the tradition of international mechanics congresses in the early 1920s, were the key figures behind this plan.[5] Burgers further discussed the plan with the general secretary of the ICSU. He also succeeded to persuade sceptics like Taylor. Fluid dynamics had ramifications into many disciplines, so that Burgers regarded cross-fertilization as an attractive prospect: "It is taken in view e. g. to form a joint international committee on viscosity and related matter, out of delegates nominated by the Union of Physics, the Union of Chemistry, the Union of Biology and, if it exists, the Union of Applied Mechanics." But Taylor was still hesitant: "I dare say," he responded, "it is the right thing but I think the matter should be discussed well. If I see Kármán, I will ask him what he thinks" [Schiehlen and van Wijngaarden, 2000, pp. 42-43].

The Paris Congress in September 1946 offered an opportunity for further discussions. Still it took a while to persuade the sceptics. The British members of the congress committee feared a loss of flexibility. Others seemed to share this concern. Only when Burgers drafted the statutes of the Union in such a way that these concerns were met, the plan materialized. On 26 December 1946, the International Union of Theoretical and Applied Mechanics (IUTAM) was officially constituted; in April 1947, its statutes were approved, and in September 1947, a provisional bureau was set up, with Richard Vynne Southwell as acting president, Dryden as acting treasurer, and Burgers as acting secretary [Schiehlen and van Wijngaarden, 2000, p. 47]. With regard to politics and funding, IUTAM, as part of the ICSU, was closely related to the United Nations Educational, Scientific and Cultural Organization (UN-

[5] Karman to Burgers, 3 June 1946. Theodore von Kármán Collections at the California Institute of Technology, Pasadena (henceforth abbreviated as TKC), 4-25.

ESCO), another newly established framework for international collaboration after the Second World War [Greenaway, 1996].

Among IUTAM's first activities was the organisation of the VIIth International Mechanics Congress scheduled to take place in September 1948 in London – two years earlier than according to the accustomed four year interval because otherwise it would have interfered with the International Congress of Mathematicians scheduled for 1950. Chaired by Southwell as IUTAM's president, the congress confirmed the great interest in renewing the prewar tradition. Like the Paris Congress in 1946 it did not yet represent a truely international community. Among the 882 participants the only Germans were those who had emigrated to other countries before the war. However, the absence of Prandtl and his pupils did not result in a distorted representation of specialties. The London congress made visible to what extent the field had grown since the 1920s. The proceedings comprized four volumes. Like two years before at the Paris Congress, turbulence was an outstanding theme. Batchelor presented the General Lecture on "Recent Developments in Turbulence Research". Other remarkable presentations on turbulence were those on the decay of turbulence by Frenkiel ("Comparison between theoretical and experimental results of the decay of turbulence") and Lin ("On the law of decay and the spectrum of isotropic turbulence") or Townsend's report on turbulent diffusion ("Diffusion in the turbulent wake of a cylinder"). A number of those who had presented papers on turbulence at Paris were again performing at London, like Batchelor, Dryden, Favre and Hinze. Although it would be premature to speak of a distinct international community of turbulence research, the London Congress has to be mentioned among the events that gave rise to its formation [Pro, 1948]. The subsequent quadrennial Congresses at Istanbul (1952), Brussels (1956) and Stresa (1960) – to name only those before Marseille – confirmed IUTAM's importance in this regard.

But Burgers had broader ambitions than offering a new umbrella for the mechanics congresses. He regarded it dissatisfactory that IUTAM would organise merely these four-year events and remain inactive in the interim period. His primary motive for the foundation of IUTAM was to enable cross-fertilization by cooperation with other Unions of the ICSU. However, there seemed to be "no great desire for joint work in this domain," he complained in a letter to Kármán in February 1948. In order to encourage such cooperation, Burgers proposed to organise international symposia between the congresses. A first step in this direction was the establishment of a committee whose task was to plan a joint symposium with the International Astronomical Union. Other committees were established for other tasks, such as promoting "computing laboratories". It was left to each committee "to co-opt members from ex-enemy countries and to admit scientists from such countries to meetings arranged by them" [Schiehlen and van Wijngaarden, 2000, p. 48]. While these committees consisted of individual members, IUTAM was formed by national scientific organizations as members. In 1948, IUTAM consisted of only two "adhering organisations", the Royal Society of London and the Hungarian Academy of Sciences. Over the years, however, IUTAM grew into an international organisation of broad scope, encompassing a very heterogeneous mix of different national research cultures [Juhasz, 1988, p. 16]. Germany was officially invited to join the ICSU in 1950. In the same year, the Gesellschaft für angewandte Mathematik und Mechanik (GAMM) became a member of IUTAM.

The year 1950 may be regarded as a watershed for the reception of German science in the international arena. In the same year German mathematicians were invited to participate in the International Congress of Mathematicians held in September 1950 in Cambridge, Massachusetts. Before this Congress was opened, the International Union of Mathematics was founded in New York. German mathematicians, represented by the German Mathematical Association (Deutsche Mathematiker-Vereinigung, DMV) were invited to participate in its inauguration. "We will be able to perform in America completely on an equal footing with the international family of mathematicians," the chairman of the DMV informed the German perticipants of the Cambridge congress. However, he alerted his colleagues that the atmosphere would not be entirely relaxed: "We cannot expect that we will be greeted friendly by every paricipant."[6] In addition, German scientists faced visa problems. With the rise of McCarthyism further obstacles were put in the way of a revival of international scientific relations of American scientists with colleagues from other countries. In 1954 the chairman of the Federation of American Scientists reported that foreign colleagues who were invited to America were "deeply disappointed by the increasing narrowmindedness of U.S. authorities and by the raising of a new paper curtain between America and the rest of the world" [Weisskopf, 1954].

Nevertheless, German scientists were eager to seize any oppurtunity to participate in conferences and accepted invitations to the USA for guest lectures even if this involved hostile reactions. IUTAM was welcomed by the German fluid dynamicists as a new international umbrella. Apart from its

[6]Kamke to Heisenberg, 19 July 1950. Heisenberg Papers, Correspondence Folder 1950. Heisenberg used this opportunity to review his doctoral work which subsequently appeared in English translation as a NACA-Technical Memorandum [Heisenberg, 1950, 1951].

internationality, IUTAM also embodied a diversity of fields where fluid dynamics would find new applications. This is illustrated by the IUTAM symposia organized with other Unions. A new territory between fluid mechanics and astrophysics, for example, was opened by common symposia with the IAU. The theme of these conferences was "cosmical gas dynamics." In the course of these meetings, turbulence became firmly established as a research field of utmost pertinence for astronomers. At the first Symposium held in August 1949 in Paris on "Problems of Cosmical Aerodynamics," Burgers emphasized in his introduction how important the "present-day developments in hydro- and aerodynamics" were for astronomers. "In particular, the problems of turbulence and those of expansion phenomena and of shock waves immediately come to the foreground." With regard to turbulence, this was illustrated by the lectures of von Weizsäcker on "Turbulence in Interstellar Matter" and Batchelor on "Magnetic Fields and Turbulence in a Fluid of High Conductivity" [IUT, 1949]. The Second Symposium of this Series was held in July 1955 at Cambridge, England, and displayed turbulence as a subject of growing concern [Int, 1955].

Other IUTAM-symposia explored common grounds with geophysicists, such as at a 1959 symposium on "Fluid Mechanics in the Ionosphere" and in 1961 on "Fundamental Problems in Turbulence and Their Relation to Geophysics" [Juhasz, 1988, appendix 18]. The latter, of course, is the event we are celebrating now. Before I turn to it in more detail, however, we have to add more context about the postwar reconstructions, particularly in the USA.

How American physics appropriated fluid dynamics

The development of Radar and the atomic bomb attributed to the Second World War the label of "A Physicists' War" [Kevles, 1978, chapter 20]. Turbulence is hardly mentioned in this context. However, as part of wartime researches such as those conducted by Dryden and von Kármán, it contributed to the fame of scientific contributions to the Allied victory. Another important branch of fluid dynamics that added to this fame concerned shock waves. This wartime legacy resulted in the foundation of the Division of Fluid Dynamics (DFD) of the American Physical Society (APS). It illustrates how the physicists in the USA embraced fluid dynamics, and turbulence as part of it, as an emerging subdiscipline of physics. I will dedicate a few paragraphs to this event because it signals a new awareness for fluid dynamics in physics.

The history of the DFD begins in June 1946, when Edward Condon, the president of the APS, appointed von Karman, Dryden, Howard W. Emmons, John von Neumann and Raymond J. Seeger as members of a new Committee on Fluid Dynamics. Each one of them had a record of pertinent war work. Seeger, head of the Aeroballistic Research Department at the Naval Ordnance Laboratory at White Oak, was chosen as chairman. In August 1946, after returning from the Bikini Atoll where he was involved with atomic bomb tests, Seeger organized as a first step at the forthcoming annual physics conference of the APS in January 1947 in New York a symposium on "Recent Trends in Fluid Dynamics" with von Kármán and Hans Bethe as invited lecturers. He suggested furthermore "to consider the means of continuing the advancement and diffusion of knowledge on Fluid Dynamics" at the APS business meeting during this conference.[7] In a letter to Bethe, Seeger explained the goals of his Committee: "Many of us feel that Fluid Dynamics is a subject of considerable physical interest which has been neglected by physicists in the past and revived only during the war. We are eager, therefore, to continue this interest, perhaps by having a permanent committee or even a division of the American Physical Society."[8]

The New York meeting of the APS in January 1947 brought to the fore that fluid dynamics deserved indeed more attention on the part of the physics community. Dryden's collaborators, Skramstadt and Schubauer, for example, introduced the American physicists at this meeting to their wartime discovery of boundary layer instability. The forthcoming Washington meeting of the APS in May 1947 provided another occasion to demonstrate that there was a continuous and growing interest in fluid dynamics. Seeger ascertained the interest of the editor of the *Journal of Applied Physics* and announced a growing number of papers on fluid dynamics which were likely to be submitted to this journal in the future. "In general," he explained the scope of these papers, "the physics of fluid dynamics is understood to include aerodynamics, hydrodynamics, ballistics, water entry, and explosion phenomena."[9] The journal editor was "quite enthusiastic" and pleased to learn "that the Committee on Fluid Dynamics is considering the Journal of Applied Physics as its official outlet."[10]

Shortly after the Washington meeting Seeger officially proposed to the Council of the APS the establishment of a Division of Fluid Dynamics. The

[7]Seeger to Darrow, 28 August 1946. Records of the Division of Fluid Dynamics of the American Physical Society, Bethlehem, PA, Lehigh University, Special Collection (henceforth abbreviated as DFD-Archives), 4-1.

[8]Seeger to Bethe, 28 August 1946. DFD-Archives, 4-2.

[9]Seeger to Hutchisson, 3 April 1947. DFD-Archives, 4-2.

[10]Hutchisson to Seeger, 15 April 1947. DFD-Archives, 4-2.

Council signaled green light at a meeting in Montreal on 20 June 1947. Subsequently, by-laws had to be formulated. The recently established Division of Solid State Physics of the APS served as a role model. In August 1947, Seeger submitted a first draft of by-laws and a circular to the secretary of the APS.[11] In October 1947, the secretary of the APS announced that the DFD has been authorized by the Council; he described its object as "the advancement and diffusion of knowledge of the physics of fluids including aerodynamics and hydrodynamics, phenomena of rarefied gases and explosions, plasticity, et al." and opened the new division for enrollment.[12] The Division's major activity was to organize meetings at least once a year "at such time and place as shall be ordered by the Executive Committee." The meetings should be organized together with the annual APS conferences. At each meeting, a Programme Committee was in charge of the organisation. Official announcements should be published in the *Journal of Applied Physics*, designated for the time being as the official organ of the DFD.[13] By January 1948, the by-laws were approved.[14]

With the organizational problems settled, the focus shifted to topics suitable for sessions at forthcoming Division meetings. "Turbulence", "Wave Motion" and "Relaxation Phenomena and Ultrasonics", for example, were suggested as topics for the a joint meeting with the Aeronautical Society at the forthcoming APS conference, held in January 1949 in New York. As it turned out, the interest in turbulence was so strong that it became split in a two-session symposium on two days during the week of the New York meeting.[15]

In June 1949, the American Physical Society held its "Semi-Centennial Meeting" in Cambridge, Massachusetts. Dryden, the acting chairman of the Division of Fluid Dynamics of this year, used the opportunity for a review: He praised Seeger, "a physicist who had become interested in fluid dynamics as a result of his wartime work," as the "leading spirit" behind the formation of the DFD and hoped that it resulted in a "permanent consideration of fluid dynamics by physicists." After reviewing the previous meetings and organizational aspects he discerned some problems "which should receive greater

[11] Correspondence between Seeger and Darrow, June to August 1947. DFD-Archives, 4-2 and 4-3.

[12] Circular on the "Formation of a Division of Fluid Dynamics in the American Physical Society," signed by Darrow and dated October 6, 1947. DFD-Archives, 4-3.

[13] By-laws, undated (probably 1947). DFD-Archives, 2-8.

[14] Seeger to Darrow, 14 January 1948. DFD-Archives, 4-4.

[15] Emmons to the members of the Executive Committee, 17 August 1948 and 27 December 1948, DFD-Archives, 4-5 and 4-6.

attention from the most capable physicists," first and foremost "that of the turbulent motion of fluids." Heisenberg's recent contribution served him as an example how a physicist who was renowned for work in quantum and nuclear physics could do pioneering work in this field. Alluding to Heisenberg's internement at Farmhall, he mused that Heisenberg was "fortunately for turbulence research forcibly diverted from nuclear research and compelled to seek new interests."[16]

The emergence of a turbulence community

Events like the 1949 Paris congress on "Problems of Cosmical Aerodynamics" organized under the umbrella of IUTAM and IAU, the turbulence symposium organized by the DFD in the same year at the APS meeting in New York, and similar gatherings signaled the rise of turbulence as a concern for a scientific community with quite different roots – but a common goal. In order to illustrate the spirit among turbulence researchers during the decade prior to Marseille, I will add some detail from various archival sources about the turbulence symposia at New York in January 1949 and subsequent similar events. A major actor in this effort was Francis Clauser whom the DFD charged with the organization of the New York symposium.

Clauser, a student of von Kármán, had been called after the war to Johns Hopkins University in order to build up a new Department of Aeronautics. Next to supersonics, he regarded turbulence as a most important field of research. In order to firmly establish turbulence on the agenda of his Department, Clauser hired in 1947 as assistant professor Stanley Corrsin.[17] Clauser was also very eager to make his institute part of the network of military research establishments in his vicinity, such as Seeger's Aeroballistic Department at the Naval Ordnance Laboratory (NOL) in White Oak, Maryland, with whom he established a "joint venture" in turbulence research. "I believe that some kind of mutual arrangement could and should be worked out," Seeger agreed with Clauser's suggestion. "In that connection, I still have hopes that we will be able to avail ourselves of the part-time

[16] Hugh L. Dryden: The Division of Fluid Dynamics of the American Physical Society. Remarks at the Cambridge Meeting, June 17, 1949. Draft of speech, Dryden Papers, Pubs-files, 1949, box 11, folder "Div of Fluid Mechanics – Amer. Physical Soc. – Cambridge 1949".

[17] Clauser to Kouwenhoven, 26 May 1947. Johns Hopkins University, Records of the Department of Aeronautics, henceforth abbreviated JHU-RDA, Box 3, Folder: Kouwenhoven, William B., 1947-1953. On Corrsin's career and work on turbulence see [Meneveau and Riley, 2011].

services of Corrsin. It turns out that we are becoming interested now with turbulence."[18]

With regard to the forthcoming turbulence symposium in New York, Clauser began in autumn 1948 to address suitable speakers. "I have received acceptances from Chandrasekhar, Liepmann and Kovasznay to give papers," Clauser informed the Chairman of the DFD by November.[19] Liepmann was an old acquaintance of Clauser from common years at Kármán's institute. Leslie S. G. Kovasznay was a recent import at Johns Hopkins University. Clauser had just hired him at his Department "to handle a research project on the measurement of turbulence in high speed air streams by means of a hot wire anemometer," as he explained in the yearly report of his Department. "Dr. Kovasznay is a graduate of the Royal Hungarian University at Budapest where he was a member of the faculty. Afterwards he was engaged in research work for one year at Cambridge, England before coming to this country."[20] The Indian astrophysicist Subrahmanyan Chandrasekhar had come to America already before the war to work at the Yerkes Observatory in Williams Bay, Wisconsin, run by the University of Chicago [Wali, 1991, chapter 9]. Another invitee to the New York turbulence symposia was the Princeton astrophysicist Martin Schwarzschild. Both, Schwarzschild and Chandrasekhar, had just begun to correspond extensively about turbulence as a subject of major concern for astrophysics.[21]

Besides astrophysicists and aeronautical scientists whose affiliation with turbulence was taken for granted, Clauser also invitated the mathematician Mark Kac and the physicist George Uhlenbeck to the New York turbulence event although they had little prior expert knowledge. Kac responded that he knew "next to nothing about turbulence" but neverless accepted to deliver an invited lecture because Clauser regarded Kac's specialty of random noise as most pertinent.[22] Uhlenbeck was invited for his expert knowledge in statistical mechanics: "At present there is an effort to carry over into turbulence many of the concepts that have been developed in statistical mechanics," Clauser explained, "I believe that many of the people interested in turbulence would be eager to hear a discussion of the conceptual difficulties

[18]Seeger to Clauser, 26 September 1947. JHU-RDA, Box 1, Folder: National Science Foundation, 1952-1961.

[19]Clauser to Emmons, 2 November 1948. JHU-RDA, Box 7, Folder: Turbulence Symposium, 1948-1949.

[20]JHU-RDA, Box 2, Folder: Isaiah Bowman, Report to, 1948-1949

[21]Chandrasekhar Papers, Regenstein Library, University of Chicago, Box 28, Folder 13: Schwarzschild, Martin.

[22]Kac to Clauser, 19 November 1948. JHU-RDA, Box 7, Folder: Turbulence Symposium, 1948-1949.

that now exist in the statistical mechanics and that would continue to be troublesome when application is made to turbulence."[23]

Although Chandrasekhar and Uhlenbeck finally could not attend, the invitations reveal the intention to reach out for physicists with quite different orientations. The lecturers at the New York turbulence symposium were Corrsin ("Some measurements in a round turbulent jet"), Kac ("Statistical analysis of random functions"), von Kármán and Lin ("Statistical theory of isotropic turbulence"), Kovasznay ("Optical methods of measuring turbulence"), Schwarzschild ("Turbulence in the atmosphere of stars") and G. C. Williams ("Combustion-generated turbulence in relation to flame propagation") [Society, 1949, p. 1282]. The DFD regarded the event so successful that turbulence entered the agenda of future meetings as a subject of special sessions. In summer 1949, for example, at the inauguration of new facilities of Seeger's Department on Aeroballistics, the DFD convened a two-day conference at the NOL with another special session on turbulence chaired by Burgers. The invited speakers at this event were Kampe de Feriet ("Spectral Tensor of a Homogeneous Turbulence"), Batchelor ("The Nature of Turbulent Motion at Large Wave-Numbers"), Chandrasekhar ("Development of Heisenberg's Theory of the Decay of Isotropic Turbulence"), H. B. Squire (Investigation of the Turbulence Characteristics of an Experimental Low-Turbulence Wind Tunnel") and Frenkiel ("Some Remarks on Turbulent Diffusion").[24] Batchelor used this occasion to promote K41 in the USA. "You will have seen previously from the program of the dedication ceremonies that Dr. Batchelor is speaking in the turbulence symposium, on the Cambridge work in extending Kolmogoroff's theory," a NOL scientist wrote to Clauser. "He has asked me to arrange, if possible, for some lectures during his stay in the USA."[25]

The role of Batchelor's promotion of Kolmogorov's theory can hardly be exaggerated. "When you are in Cambridge, you should try to see Batchelor, he is an awfully good man and very sound contrary to the other Cambridge luminaries in astrophysics," Chandrasekhar advised Schwarzschild in May 1950 when Schwarzschild went on a trip to Europe.[26] When G. I. Taylor asked Chandrasekhar in September 1951 for a report on Batchelor concerning Batchelor's application for a fellowship at Trinity College, Chandrasekhar

[23] Clauser to Uhlenbeck, 16 November 1948. JHU-RDA, Box 7, Folder: Turbulence Symposium 1948-49.

[24] DFD-Archives, 1-3 and 4-7.

[25] Smelt to Clauser, 13 April 1949. JHU-RDA, Box 2, Folder: Smelt, Ronald, 1949.

[26] Chandrasekhar to Schwarzschild, 1 May 1950. Chandrasekhar Papers, Regenstein Library, University of Chicago, Box 28, Folder 12: Schwarzschild, Martin.

was again full of praise: "I am very well acquainted with Dr. Batchelor's work on the theory of turbulence. Indeed, when about two years ago I started seminar classes on 'The theory of turbulence', I found that the most satisfactory way to go about learning the subject was to read and discuss the various papers of Dr. Batchelor, one by one."[27] Batchelor's book on *The Theory of Homogenous Turbulence*, first published in 1953, "is substantial and it merits the recognition it received," wrote Chandrasekhar later in another report; otherwise, however, he had become "disillusioned very rapidly by the sterility of his approach to scientific problems."[28]

Chandrasekhar's disillusionment may well have been the result of some rivalry, because Batchelor's book rendered Chandrasekhar's plan for a similar monograph obsolete. "I have in mind a book on 'The Statistical Theory of Turbulence'. At the moment there exists no monograph on the subject from which one may learn the newer developments of the past 15 years," Chandrasekhar had confided to Neville Mott in August 1950. The plan matured into a book proposal to the Oxford publisher Clarendon Press. In February 1952, Chandrasekhar was "fairly condident" to submit the manuscript to the publisher "by the end of September of this year." Four years later, in December 1956, Chandrasekhar admitted: "I have since changed my mind and would rather write my book on stability first."[29] Chandrasekhar's *Hydrodynamic and Hydromagnetic Stability* appeared at Clarendon Press in 1961 and became a classic. But the planned book on turbulence never appeared.[30]

It would exceed the scope of this review to mention all events during the early 1950s that focused on turbulence. In addition to the first monographs on turbulence – abandoned and carried out – I merely add the reviews on past

[27] Chandrasekhar to Taylor, 24 September 1951. Chandrasekhar Papers, Regenstein Library, University of Chicago, Box 31, Folder 3: G. I. Taylor.

[28] Chandrasekhar to O. J. Eggen, 13 September 1968. Chandrasekhar Papers, Regenstein Library, University of Chicago, Box 11, Folder 11: Batchelor.

[29] Chandrasekhar Papers, Regenstein Library, University of Chicago, Box 68, Folder 8.

[30] In an interview Chandra gave to the question whether he "ever started into an area and found that it wasn't particularly interesting" the following answer: "An instance of this kind occured when I started working in the field of turbulence in the late forties and early fifties. I did publish a few papers in the subject for two or three years; but I found that I was not making much progress. Moreover, the area was becoming controversial. I therefore left the subject and went on to problems in hydrodynamic and hydromagnetic stability which occupied me all during the fifties." [Interview with Dr. S. Chandrasekhar by Spencer Weart, October 31, 1977, available at http://www.aip.org/history/ohilist/4551_3.html.] Chandrasekhar's papers on turbulence are reprinted in his *Selected Papers*. For further references see his obituary, where it is remarked that "Chandra perhaps wisely limited his work in this field and instead concentrated on linear stability problems" [Tayler, 1996, p. 88].

turbulence efforts that emerged by the 1950s. Tollmien, for example, presented such reviews in 1951 at a GAMM conference, in 1952 at the IUTAM-Congress in Istanbul, and in 1954 at the occasion of the fiftieth birthday of Prandtl's boundary layer concept [Tollmien, 1952, 1953, 1955]. Dryden reviewed in May 1950 "The Turbulence Problem Today" [Dryden, 1951] at the Midwestern Conference on Fluid Dynamics, held at the University of Illinois, Urbana, in a joint meeting with the Division of Fluid Dynamics, and "Fifty Years of Boundary-Layer Theory and Experiment" in a banquet speech at a DFD-meeting in November 1954 at Fort Monroe, Virginia.[31] Like Lumley and Yaglom another half century later, Dryden presented some statistics about the papers published on various aspects of boundary layer flow. "The turbulent incompressible boundary layer receives considerable attention with about 45 papers," he concluded. With regard to the turbulent compressible boundary layer he counted 35 papers. "Stability and transition form the principal topic of about 70 papers," he reported about what was regarded in the 1920 *the* turbulence problem. "The current total rate of production of papers is about 10 papers per month, nearly 9 times the rate immediately preceding World War II," Dryden concluded his survey [Dryden, 1955]. Two years later, IUTAM chose the same theme as subject of a special symposium at Freiburg in Germany [Görtler, 1958].

New Journals

In terms of journals open for research articles on turbulence the situation by the mid 1950s was the same as before the war. When the Division of Fluid Dynamics of the American Physical Society was founded in 1947, they had chosen the *Journal for Applied Physics* as their publication outlet. But this situation was regarded as unsatisfactory. According to the minutes of a DFD-meeting in January 1949 there was "a feeling among a considerable number of members of the Fluid Dynamics Division that there was a real need for a new journal in this field."[32]

However, it took another couple of years until this feeling was turned into action. The initiative was launched by a survey of the American Institute of Physics to determine how the American physicists used the present journals and what needs they expressed for the future. Based on this survey the AIP

[31] DFD-Archives, 1-3.

[32] Shortley to Members of the Committee on Publications in Fluid Dynamics, 4 April 1949. Physics of Fluids records, 1956-1981, Niels Bohr Library & Archives. American Institute of Physics (henceforth abbreviated: AIP-PF-records). Folder: Correspondence prior to January 2, 1958.

envisioned “a new pattern of journal publications.”[33] The DFD seized the opportunity and revealed their plan of “a Journal of Fluid Physics,” as the Secretary of the DFD wrote to the Chairman of the Governing Board of the AIP. “We would wish to see included in the proposed journal basic research papers in magnetohydrodynamics, shock wave phenomena, compressible and high temperature fluid flows, plastic flow, turbulence, liquid state physics, ionized fluid and plasma flows, as well as certain basic aspects of fluid physics bordering geophysics and astrophysics.”[34]

Frenkiel, who was then representing the DFD as its Secretary, became the major actor for the foundation of the planned journal. He had actively contributed to turbulence and presented papers at preceding IUTAM Congresses and numerous other occasions. His career mirrored the turbulent historical events of the preceeding decades [Emrich et al., 1987]. After his doctoral work with Kampe de Feriet and the German invasion of France he moved with his mentor to the Aeronautical Research Station at Toulouse. When the German occupation was extended to Southern France, he was imprisoned and spent the remaining two years of the war in Nazi concentration camps. His wife and their unborn child were killed by the Nazis. After the war, Frenkiel emigrated to the United States, where he was employed for short periods at the Department of Aeronautical Engineering of the Cornell University and at the Naval Ordance Laboratory. In 1950, he joined the Applied Physics Laboratory of the Johns Hopkins University at Silver Spring which had close ties with Clauser's Department of Aeronautics. The latter move was once more caused by the political events in the Cold War: In the course of the Paperclip Project, the NOL employed German aerodynamicists who had participated in the V-2 project at Peenemünde. In the course of a reorganization one of them (Hermann H. Kurzweg) would have become Frenkiel's chief – a situation which he was unwilling to tolerate. “And five years have not wiped out from my memory Buchenwald and Auschwitz,“ Frenkiel wrote to Kármán shortly before quitting the NOL.[35]

As Secretary of the DFD, Frenkiel made the plan for the new journal his mission. Together with the geophysicist Walter M. Elsasser he drafted a memorandum as a first step. “There is at present no journal in the United States in which fundamental contributions to the field of fluid physics can be published with a view of circulation among those interested in the physics

[33] Memo by J. H. McMillen, 20 March 1956. AIP-PF-records. Folder: Correspondence prior to January 2, 1958.

[34] Frenkiel to Seitz, 27 April 1956. AIP-PF-records. Folder: Correspondence prior to January 2, 1958.

[35] Frenkiel to Kármán, 17 February 1950. TKC, 94-12.

rather than the engineering aspects," they explained the need. The list of subject matters to be covered was as broad as in the earlier outline, except that they now specified "statistical theory of turbulence" as a particular item. "Emphasis should not be on applied mathematics but on that combination of experiment, conceptual models, and formal analysis that is thought of as characterizing good physics."[36]

By the same time, in May 1956, they learned "that Batchelor in Cambridge is about to start a new journal on Fluid Mechanic," as Elsasser informed Frenkiel. "I presume that it would be a good idea to inquire with Batchelor about his program, but since you are nearer to the center of things, this perhaps could better be done by you than by me."[37] Batchelor spent a week in September 1956 in Washington, D.C., where he attended a Symposium on Naval Hydrodynamics. At this occasion he informed Schubauer "that the Journal of Fluid Mechanics is intended to be Anglo-American in character" with two Associate Editors at the Universities of Princeton (W. C. Griffith) and Harvard (George Carrier). "Dr. Batchelor took the view that it might be wise to wait and see whether the Journal of Fluid Mechanics could meet the needs in this field before setting up another journal."[38] Batchelor also informed Frenkiel directly early in December 1956 about the background and tendencies of his journal.[39]

But the "wait-and-see" attitude did not fit well with the AIP intentions which concerned the need for new journals in other physical sub-disciplines also. Nor did it please Frenkiel. "The Executive Committee of the Governing Board of the American Institute of Physics looked with very strong favor upon the idea of starting a new journal of the general scope of that on fluid physics," Frenkiel wrote two weeks later to Seeger, who had by this time assumed science-policy-responsabilities at the National Science Foundation. "The Chairman of the Governing Board and the Director of the Institute informed me that the whole matter is expected to be brought to a climax at the time of the next meeting of the Governing Board and that there is a likelihood that the journal could be started during 1957."[40] He also cir-

[36]Proposal for a Journal of Fluid Physics, 10 May 1956. AIP-PF-records. Folder: Correspondence prior to January 2, 1958.

[37]Elsasser to Frenkiel, 17 May 1956. AIP-PF-records. Folder: Correspondence prior to January 2, 1958.

[38]Schubauer to Frenkiel, 26 September 1956. AIP-PF-records. Folder: Correspondence prior to January 2, 1958.

[39]Batchelor to Frenkiel, 4 December 1956. AIP-PF-records. Folder: Correspondence prior to January 2, 1958.

[40]Frenkiel to Seeger, 18 December 1956. AIP-PF-records. Folder: Correspondence prior to January 2, 1958.

culated a questionnaire to the members of the DFD concerning the planned scope and title of the new journal.

Batchelor was "dismayed and disturbed" about this move and appealed to Frenkiel as a representative of their common field of research. "Do you not think that the Fluid Dynamics Division would be doing science a service by strengthening an international Journal rather than by setting up another national journal?"[41] But Frenkiel was already carrying the initiative a step further. He formed a committee with the task to present the Governing Board of the AIP with a more definite proposal about the new journal. At the first meeting of this committee in February 1957, Batchelor's concerns were dismissed. There was "unanimous agreement" that the proposed AIP journal and the *Journal of Fluid Mechanics* could both flourish.[42] Three weeks later, the Governing Board of the AIP authorized the establishment of the new journal, titled "The Physics of Fluids"; the first issue was due to appear in January 1958.[43]

Batchelor was frustrated. "In these circumstances," he wrote to Frenkiel, "it is perhaps not necessary to go on talking about the pros and cons of the new journal." In an effort to cut his losses he proposed a division of labor along the lines suggested earlier by Raymond Emrich, that the *Journal of Fluid Mechanics* would focus "on the mechanical aspects of fluid motion" and the new AIP-Journal "on the more physical aspects." Although he admitted that such a discrimination was problematic, he hoped that this would reserve most of the articles on fluid mechanics proper, such as "hydrodynamics, dynamics of compressible fluids, boundary layer and turbulent phenomena," for his own journal. "It seems to me that these particular topics are essentially mechanical,- and might well be left to JFM."[44]

If such a division of labor had been adopted, Batchelor's JFM would have become the main Anglo-American publication outlet for turbulence research. However, Frenkiel responded that he had "no authority to commit the future editorial policy of the journal" and expressed his confidence "that the competition between JFM and the new journal be of a constructive nature."[45]

[41] Batchelor to Frenkiel, 7 February 1957. AIP-PF-records. Folder: Correspondence prior to January 2, 1958.

[42] Minutes of the Meeting of the AIP Planning Committee for a Journal of the Physics of Fluids at the Cosmos Club, Washington, D. C., 21 February 1957. AIP-PF-records. Folder: Correspondence prior to January 2, 1958.

[43] Frenkiel to Uhlenbeck and others, 5 April 1957. AIP-PF-records. Folder: Correspondence prior to January 2, 1958.

[44] Batchelor to Frenkiel, 3 May 1957. AIP-PF-records. Folder: Correspondence prior to January 2, 1958.

[45] Frenkiel to Batchelor, 13 May 1957. AIP-PF-records. Folder: Correspondence prior

One day later Frenkiel drafted a memo about the editorial organization of *The Physics of Fluids*. With regard to the scope of the journal he resumed what he had drafted earlier together with Elsaser and what had been authorized with little modification by the planning committee: "The scope of these fields of physics includes: hydrodynamics, dynamics of compressible fluids, shock and detonation wave phenomena, hypersonic physics, rarefied gases and upper atmosphere phenomena, transport phenomena, hydromagnetics, ionized fluid and plasma physics, liquid state physics, superfluidity, boundary layer and turbulence phenomena, as well as certain basic aspects of physics of fluids bordering geophysics, astrophysics, biophysics and other fields of science. Emphasis will not be on applied mathematics, but on that combination of experiment, conceptual models, and formal analysis that is thought of as characterizing good physics."[46]

On 1 July 1957, Frenkiel formaly announced that the Editorial Board of the *The Physics of Fluids* became operative.[47] Since then, there were two new opportinities for authors to submit their papers on turbulence. The editors of both journals, Batchelor and Frenkiel, had contributed to turbulence research themselves with pioneering work. Turbulence ranked high on the list of topics in both journals. Among the first papers submitted to *The Physics of Fluids*, for example, was Corrsin's article "Statistical Behavior of a Reacting Mixture in Isotropic Turbulence." In the four years from 1958 to 1961 both journals published 76 articles concerned with turbulence (35 in *The Physics of Fluids*, 41 in the *Journal of Fluid Mechanics*). As Frenkiel had predicted, there was no dearth of articles. Both journals flourished and had to enlarge their volume in the following years and decades [Scott, 2008, Davis and Pedley, 2006].

Conclusion

By the time of the Marseille events in 1961, turbulence had become an active research field. Besides a host of journal articles and conference contributions there were monographs [Batchelor, 1953, Townsend, 1956, Hinze, 1959] which provided an introduction to the field for a new generation of researchers. Yet it is difficult to assess turbulence in terms of one or another discipline. By their institutional affiliation, turbulence researchers belonged to aeronautical

to January 2, 1958.

[46]Draft, 14 May 1957. AIP-PF-records. Folder: Memorabilia for The Physics of Fluids.

[47]Announcement by F. N. Frenkiel, Editor The Physics of Fluids, 1 July 1957. AIP-PF-records. Folder: Board of Editors.

engineering, applied mathematics, physics, mechanics, or some other discipline. As illustrated by the formation of the Division of Fluid Dynamics of the American Physical Society, fluid dynamics was appropriated only after the Second World War as a subdiscipline of physics. It remains to study whether this was unique for the USA. I guess it was a general phenomenon and reflects the neglect of classical fields of physics with the rise of atomic physics during the early decades of the 20th century. Despite the surge of turbulence research during the 1950s, it was not yet a specialty of its own right. The foundation of a journal dedicated exclusively to turbulence, and the establishment of a specialized series of conferences like the Turbulence Conferences organized by the European Mechanics Society (EUROMECH) was still decades in the future. Turbulence came of age within fluid dynamics as a flourishing research field that was important for several other disciplines. Transdisciplinarity was (and still is) a characteristic feature of fluid dynamics at large. This became apparent, as we have seen, in 1949 in the joint venture of IUTAM and IAU at the Paris Symposium on "Problems of Cosmical Aerodynamics."

The disciplines with a genuine interest in fluid dynamics besides astronomy were meteorology, oceanography and other geophysical disciplines. In the Russian school of turbulence, for example, fundamental research on turbulence was advanced with a strong focus on geophysical applications [Falkovich, 2011]. With the proclamation of 1957 as the International Geophysical Year (IGY), fluid dynamics became internationally engaged in joint ventures with geophysics. "It may be significant that the active operation of our Editorial Board happens to start on the first day of the International Geophysical Year," Frenkiel remarked in his editorial announcement about his journal on the same day. "The scope of The Physics of Fluids includes several fields of fundamental importance to theoretical and experimental geophysics and it should be expected that our journal will contribute in some measure to the scientific interpretation and to the understanding of the numerous data obtained during IGY."[48]

The Marseille events – the Colloquium held at the Institute for the Statistical Mechanics of Turbulence (ISMT) from 28 August to 2 September 1961 and the Symposium organized by the International Union of Geodesy and Geophysics (IUGG) and IUTAM from 4 to 9 September 1961 – also reflect this transdisciplinarity. The initiative for the latter was launched by the International Association of Meteorology and Atmospheric Physics, a sub-

[48]Announcement by F. N. Frenkiel, Editor The Physics of Fluids, 1 July 1957. AIP-PF-records. Folder: Board of Editors.

organization of IUGG, with a plan for a common IUTAM-IUGG-symposium "on basic problems of turbulence and their relation to geophysics." In August and September 1960, the General Assembly of the IUGG and IUTAM approved this plan at their meetings in Helsinki and Stresa, respectively. A scientific committee, chaired by Frenkiel with Batchelor, Kenneth F. Bowden, Graham Sutten, Alexander M. Obukov, J. Peres and J. C. Schönfeld as members, was charged with the execution of this plan [Frenkiel, 1962]. Once more, Frenkiel and Batchelor found themselves involved in a challenging organizatorial effort, this time however not as rivals but as members of the same committee.

It would be fascinating to see how Batchelor and Frenkiel collaborated in the preparation for the Marseille IUTAM-IUGG-Symposium, and how it was related to the Colloquium held during the preceding week on the occasion of the inauguration of the ISMT. With Alexandre Favre, the director of this institute and organizer of this Colloquium, another promotor of turbulence research enters the stage whose activities deserve closer historical scrutiny – especially with regard to his international relations. In May 1954, for example, Favre visited Clauser's department and subsequently spent a two-months sojourn at the California Institute of Technology in Pasadena. "We had a very pleasant visit here from Professor Favre from Marseille," wrote Liepmann in July 1954 about Favre's sojourn at the GALCIT. "He has made some really nice space-time correlation measurements and his equipment for these measurements seems very good and at present pretty unique. Favre told me that ONERA wants him to go into high speed work on turbulence..."[49]

Such haphazard remarks hint at the overall connex between turbulence research and Cold War programs during the 1950s. The work of Kovasznay, Liepmann and others in the USA should be regarded in the same context. I did not mention explicitely the Cold War as a major factor that motivated research on turbulence, although it is obvious when we consider institutions like the Naval Ordnance Laboratory, where the DFD had organized an turbulence symposium 1949, or the Applied Physics Laboratory (then mainly concerned with research for guided missiles) as the home of Frenkiel's Editorial Office for *The Physics of Fluids*.

Such contexts should be analysed in more detail in future work on the history of turbulence. Another aspect that deserves further historical analysis concerns the changes of research orientation as a result of contemporary perspectives. By 1920, for example, the onset of turbulence was perceived

[49]Liepmann to Kármán, 15 July 1954. TKC, 18-20. See also Clauser to Martin 24 May 1954. JHU, Box 5: Folder: Maryland, University of, 1952-1954.

as *the* turbulence problem; in the 1950s the riddles of the statistical theory of turbulence were regarded as more challenging. I dare to say that historians of science may expect a rich harvest from the correspondence between the turbulence researchers in the different periods – although I would like to add a warning: the challenge for the historian with the interpretation of this material is perhaps comparable to that for the physicist or mathematician engaged in turbulence research. The bits and pieces of this review, collected from this and that archive, should serve as appetizer for what is still waiting to be uncovered about this fascinating subject in the archives all over the world.

Nomenclature

APS	American Physical Society
DFD	Division of Fluid Dynamics
DMA	Deutsches Museum (Munich), Archive
DMV	Deutsche Mathematiker-Vereinigung
EUROMECH	European Mechanics Society
GALCIT	Guggenheim Aeronautical Laboratory at the California Institute of Technology
GAMM	Gesellschaft für Angewandte Mathematik und Mechanik
ICSU	International Council of Scientific Unions
IGY	International Geophysical Year
ISMT	Institute for the Statistical Mechanics of Turbulence
IUGG	International Union of Geodesy and Geophysics
IUTAM	International Union of Theoretical and Applied Mechanics
JHU-RDA	Johns Hopkins University, Records of the Department of Aeronautics
MPGA	Max-Planck-Gesellschaft, Archiv (Berlin)
NACA	National Advisory Committee for Aeronautics
NOL	Naval Ordnance Laboratory
TKC	Theodore von Kármán Collection
UNESCO	United Nations Educational, Scientific and Cultural Organization
ZAMM	Zeitschrift für Angewandte Mathematik und Mechanik

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