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THEODORE VON KÁRMÁN AND THE ARRIVAL OF APPLIED MATHEMATICS IN THE UNITED STATES, 1930-1940

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ABSTRACT

Applied mathematics as a discipline scarcely existed in the U. S. fifty years ago. Although its rise in America is traditionally associated with World War II, Theodore von Kármán had waged a long and vigorous campaign well before Pearl Harbor to make applied mathematics respectable to engineers and mathematicians. In the course of advocating the use of mathematics and physics to solve applied problems, he challenged the prevailing philosophy of engineering programs, locked horns with recalcitrant journal editors, and generally encountered the obstacles to building a discipline that cuts across conventional boundaries.

1. Introduction

Applied mathematics is generally regarded as having come into being as a discipline in the United States during World War II. Brown University, under Roland G. D. Richardson, formally instituted a program in applied mathematics, the nation's first, in 1941. New York University, under Richard Courant, later followed suit.[1] To Theodore von Kármán, Hungarian-born engineer and applied scientist, these and other measures taken during the war represented the first concerted, nation-wide effort to resolve a longstanding scientific gap in the United States.

The first director of the Daniel Guggenheim Graduate School of Aeronautics at the California Institute of Technology, von Kármán had already spent more then ten years struggling to make applied mathematics respectable in his adopted country. As an aerodynamicist, von Kármán figured prominently in the rise of Caltech's school of aeronautics in the 1930's. Away from Pasadena, he worked equally hard to promote applied mathematics in the United States. He practiced what he preached at Caltech, and preached elsewhere what he practiced.

Von Kármán's experiences in America in the thirties anticipated the organized development of applied mathematics in the ensuing decade. Frequently pressed for his opinions on how to mobilize mathematicians for the war, von Kármán willingly contributed the lead article to the maiden issue of the Quarterly of Applied Mathematics, published in 1943 under the auspices of Brown's program. Written in the form of a Galilean Socratic dialogue, "Tooling Up Mathematics For Engineering" eloquently stated the case for the applied mathematician in the service of science.[2] Not that the aerodynamicist approved wholeheartedly of the proposals for new applied mathematics institutes drafted just before Pearl Harbor. He disliked the "exaggerated" appeal to an "emergency" created by the war. In his review of one such proposal, he noted that the problem of applied mathematics could not be solved "through the ordinary process of supply and demand."[3] Indeed, an entirely different set of imperatives guided von Kármán in the thirties.

2. The Climate in the United States circa 1930

Shortly after he had completed his first tour of the United States, which included a visit to Caltech, von Kármán wrote Richard Courant, the head of Göttingen's mathematics institute, that "what strikes me most in regard to mathematics . . . [in America] is the complete lack of 'applied' mathematicians. . . ."[4] In one sense, von Kármán certainly erred; since the late nineteenth century, electrical and radio engineering had evolved into highly advanced branches of applied science in the United States, involving the use of a great deal of sophisticated mathematics, thanks to the efforts of several dozen applied scientists, including Charles P. Steinmetz, Michael I. Pupin, and Frank Jewett.[5]

At the same time, there was an element of truth to von Kármán's perception: pure mathematics had developed by leaps and bounds in the United States during the early decades of the twentieth century. The contact that aspiring American mathematicians had with certain European mathematical schools, especially the German abstract school, which underwent a high degree of development during the latter half of the nineteenth century, provided the initial stimulus. As a result, American mathematicians were able, within a short time, to build and sustain research groups in American academic settings in the areas of analysis, number theory, and, especially, a new branch of mathematics, topology.[6]

American historians of mathematics writing during the early decades of the twentieth century emphasized the origins of pure mathematics in their own country. R. C. Archibald, for example, said in 1925 that pure mathematical research in American universities began with Benjamin Peirce (1809-1880). In fact, less than half his output is considered pure mathematics today; the applied mathematics Peirce did all but escaped Archibald's attention.[7]

Electrical engineering aside, von Kármán's reading of the state of American applied science was by-and-large accurate. The science of the strength of materials, for example, von Kármán's first field of study, remained almost exclusively experimental in the U.S. American engineers, pragmatic by nature, distrusted the increasingly sophisticated theoretical and mathematical formulations overseas. Mathematically unsolvable problems had no place in American engineering practice of the late nineteenth and early twentieth centuries.[8] This may help to explain why Stephen Timoshenko, sometimes dubbed the "father of engineering mechanics" in the United States, was virtually unknown to the American scientific community, when he arrived in 1922 at the age of 44. His old-world reputation had not yet reached the new.[9] When Timoshenko began to work for Westinghouse in 1923, he " . . . noticed that all the jobs requiring any theoretical knowledge whatsoever were filled mainly by engineers educated in Europe."[10] Before moving to the University of Michigan in 1927, Timoshenko gave at Westinghouse what was probably the first course on elasticity theory in the United States.[11] At Michigan Timoshenko initiated a program in engineering mechanics similar in philosophy to von Kármán's program in aeronautics at Caltech in the 1930s. Friends even before their paths crossed again in America, Timoshenko and von Kármán had ample opportunity to compare notes in this country, too. Timshenko sent students to von Kármán; von Kármán in turn tried to bring Timoshenko to Caltech. Von Kármán in fact believed that Timoshenko made "the first attempt to gather the applied mathematicians and to institute some activity in applied mathematics" in the U.S.[12] Timoshenko along with von Kármán complained about the attitude of American engineering students. In his opinion, they only wanted, "the final result -- a formula which . . . [they] can apply mechanically, without thought, to solve practical problems." Timoshenko traced this attitude back to inadequate mathematics instruction in American high schools.[13] Indeed, during the 1930s, mathematics came under constant attack, especially at the high school level.[14] Engineering educators well into the 1920s had seriously debated whether engineering students even ought to study calculus. Some of them thought that such courses were mere "cultural embellishments to the curriculum."[15]

Von Kármán, for his part, didn't find Caltech's mathematicians particularly helpful either. Number theorist Eric Temple Bell, in particular, was not interested in training engineers. The mathematics they learned in Bell's hands, he believed, was simply too abstract.[16] Von Kármán felt strongly that applied mathematics should be taught in graduate engineering schools. But, this seldom happened, because the mathematicians told the engineers to teach the course, while the engineers concentrated instead on practical subjects.[17]

Von Kármán had his feet planted in both worlds. He had done work on the buckling of columns, on the stability of vortex patterns that form behind stationary bodies in flowing fluids, and, with Max Born, on the lattice dynamics and vibrational frequencies of crystals, providing a foundation for earlier work by Albert Einstein and Peter Debye on the heat capacity of solids, among other things. He brought a mathematically sophisticated point of view to all of these problems. Yet when he arrived in the United States, he found American engineers largely untutored in certain branches of mathematics, and quite unprepared for his unorthodox approach to the engineering sciences.

3. <u>Aeronautical Traditions and Innovations</u>

Although it is traditional to celebrate the beginnings of aeronautics at Caltech with the arrival of Theodore von Kármán in 1930, its roots go back to the formation of the "Committee on Aeronautics of Throop College" in 1917, as the United States prepared to join the war against Germany. Throop's science-minded trustee, George Ellery Hale, promoted aeronautics research as a way for the school to gain national stature. The college hired Harry Bateman, an English mathematical physicist, and Albert A. Merrill, an American aviation buff. Bateman was the theoretician; Merrill the tinker. After designing a small wind tunnel for testing models, Merrill began work on a plane design featuring a moveable wing.

By the mid-twenties, Bateman had acquired several graduate students, including Clark Millikan, Robert A. Millikan's son.[18] Following von Kármán's first visit to the campus in 1926, Clark Millikan kept him informed by mail of what was going on in aeronautics, until the Hungarian aerodynamicist returned to Pasadena permanently in 1930. His letters told of the construction of the aeronautics laboratory, breaking in and experimenting with the 10-foot Göttingen style tunnel von Kármán had urged the Institute to construct, Merrill's new airplane, and Bateman's recent work on airfoil theory.

The personalities of Merrill and Bateman are stories in their own right. Merrill, a high-strung self-taught inventor, well-versed in the practical side of aeronautics, had the field to himself at Caltech until Robert Millikan, the school's head, engaged Arthur A. Raymond, a member of the technical staff of Douglas Aircraft, and an expert in designing planes, to teach a class in aircraft design at Caltech. Raymond's first class consisted of Merrill, Bateman, and Clark Millikan. Headstrong and jealous of his prerogatives, Merrill left Caltech before von Kármán became director of the Guggenheim Aeronautical Laboratory. The 1928 crash of Merrill's bi-plane, "the dill-pickle" as his students called it, made local headlines and probably hastened his departure from the campus.

Bateman, by contrast, seemed afraid of his own shadow. Shy, unassuming, and at times indifferent to his classroom surroundings, Bateman specialized in finding particular solutions to complicated equations used by physicists and applied mathematicians. By the mid-twenties, Bateman's election to the National Academy of Sciences seemed imminent. His Caltech colleague, E. T. Bell, fearful that the timid Bateman might shortchange his own chances by listing too little on the requested curriculum vitae, took matters in hand. "Spread yourself;" he advised him in 1927, "it pays, in our glorious country, to kick over the bushel and let your light to shine before men that they may see your good works. . . ."[19] Even so, it took three more years before Bateman became a member of the Academy.

In Merrill's and Bateman's time, the field still belonged to the amateurs, and Merrill stood high among their ranks. In contrast to the trial-and-error experimentation of the 1920s at Caltech, von Kármán's students and co-workers attacked a host of theoretical problems relating to airplane design and flying which industry used to good advantage.[20] The presence of Arthur Raymond on the campus suggests that the southern California based aircraft companies -- like Douglas -- and Caltech discovered each other even before von Kármán took up permanent residence in the United States. But there is little doubt that the companies profitted even more from the creation of a first-class school of aeronautics in Pasadena. Indeed, as the Laboratory's statistics reveal, of the thirty most prominent graduates in the 1930s, nearly half became theoretically oriented aerodynamicists who joined universities, while the others worked in industry, especially the local aircraft companies. In general, students who did their work in aerodynamics went into the aircraft industry, while students specializing in fluid mechanics (e.g., problems such as turbulence and the boundary layer) became academics.[21] The 10-foot wind tunnel, designed to von Kármán's specifications, was used to test practically all of the aircraft built by the companies on the west coast during the 1930s, including the Douglas Company's DC-1, DC-2, and DC-3 series, the most successful commercial aircraft of the time. The aircraft companies also recruited Caltech's outstanding students, starting with W. Bailey Oswald, "Ozzie", Caltech Ph.D., 1932, whom Arthur Raymond hired as Douglas' chief aerodynamicist when the company began working on the DC-1. If anything, the relationship in the 1930s between Caltech's Guggenheim Laboratory and the local aircraft companies, whom the laboratory welcomed with open arms, is a pre-World War II example of the "academic-industrial complex" that we hear so much about today.

To appreciate the breadth of von Kármán's aeronautics school in the 1930s requires some understanding of the European applied mathematics and mechanics movement of the twenties. The movement found public expression in the flowering of new organizations, journals, and academic departments. Then head of Aachen's Aerodynamics Institute, in addition to his duties as the school's professor of aerodynamics and mechanics, von Kármán took the initiative in organizing the 1922 Innsbruck Conference on Hydro-and Aerodynamics. The four-day meeting, boycotted by French and British scientists, attracted thirty-three applied mathematicians and physicists from seven countries. An informal, unofficial meeting (von Kármán personally divided the organizing costs with Italian mathematician Tullio Levi-Civita), the post-World War I conference succeeded in bringing a number of people with similar scientific interests face to face for the first time. To his way of thinking, aerodynamicists like himself did not get the attention they deserved because there were not enough of them to stand out at ordinary scientific meetings. "And even among the group they are very split," he pointed out, "because the mathematicians attend mathematics meetings, the physicists attend physics meetings, and the technical people go only to technical meetings."[22] Von Kármán belonged to a new breed of scientist and decided to do something about the problem. Innsbruck was his solution.

His contemporary Richard von Mises founded and edited a new journal, appropriately named the Zeitschrift für Angewandte Mathematik und Mechanik. The head of Berlin's Institute for Applied Mathematics, itself a post-World War I development, von Mises had a real flair for organizing like-minded scientists. By von Kármán's reckoning, it was von Mises who first mobilized physicists, mathematicians, and scientifically minded engineers working on applied problems to publish their results in the same place.[23]

Von Kármán himself disregarded the traditionally-defined boundaries for aeronautics and aerodynamics in the United States. Instead, the range of problems he tackled encompassed more than either of these branches of science usually did. Robert Millikan's criticism of the state of American engineering in the 1920s was a factor in von Kármán's decision to come to Caltech. Millikan, according to von Kármán, had identified "the 'ad hoc approach' to the practical problems to be solved" as its weak link.[24] Indeed, Millikan, like von Kármán, felt strongly about this issue. "If a man does not learn his physics, chemistry, and mathematics in college he never learns it," he told a Caltech audience in 1920, adding, "the attempt to learn the details of an industry in college is futile. The industry itself not only can, but it must, teach these."[25] Although von Kármán did not succeed in converting all of Caltech's engineers, let alone other engineers, to his point of view, the Institute's philosophy nevertheless provided him with the necessary freedom to pursue his own course.

In 1932, the Metropolitan Water District of Southern California asked von Kármán for help in designing pumps for its Colorado River Aqueduct project. Von Kármán promptly petitioned the school's elders to establish a hydraulics laboratory. He likened hydraulics to the state aeronautics had been in when engineers began to turn away from purely empirical computations and started to embrace the methods of the applied mathematicians.[26] In the "pump lab" as it came to be called, Caltech's engineers, von Kármán among them, studied a variety of water-flow problems. Among other things, they designed and built a water "wind tunnel" to test the efficiency of different pumps. The work done in the hydraulics laboratory, von Kármán once said, "showed a generation of engineers how pure scientific ideas in hydrodynamics, aerodynamics, and fluid mechanics can be used to solve problems of practical design in related fields that at first seen remote."[27]

Von Kármán had a hand in the Grand Coulee Dam project, too. After the dam opened, cracks appeared. Called in to investigate these cracks, he realized almost immediately that the forces on the dam exceeded the buckling limits for which the dam was designed. What the civil engineers had done, in effect, was use standard design factors obtained from a handbook, then extrapolate to get the figures for building a dam the size of the Grand Coulee. While they had taken into account static forces due to water pressure acting on the dam, they had failed to consider the special buckling conditions that would arise in such a large dam.[28] The aerodynamicist advised the dam engineers to think in terms of shells and to put in stiffeners. Von Kármán, in short, drew upon his experiences with stiffeners in making sheet metal usable in aircraft design to solve a civil engineering problem.

In still another non-aeronautical assignment, von Kármán and his co-workers solved the mystery of "Galloping Gertie," the collapse of the Tacoma Narrows Bridge in 1940. In characteristic fashion, von Kármán transformed a statics problem in civil engineering into a dynamic instability problem. Ironically, the solution rested on an appreciation of a complex hydrodynamic phenomenon know as vortex shedding first explained by von Kármán in 1911.[29] In recalling the episode many years later, von Kármán noted that ". . . the bridge engineers couldn't see how a science applied to a small unstable thing like an airplane wing could also be applied to a huge, solid, nonflying structure like a bridge."[30]

4. Applied Mathematics vs. Mathematical Physics

By and large, neither the mathematicians nor the engineers had a real grasp of the role of mathematics in applied science. Von Kármán continually pointed out the difficulties mathematicians and physicists had in dealing with non-linear problems, where physical intuition alone would not suffice.[31] Often, the mathematics to deal with such problems hadn't been invented yet. Such was the case, for example, of the "solitary wave" problem[32] -- the forerunner of the "solitons" that physicists struggle with today, the mathematics of which is now a lively research field. In general, he stressed that mathematicians liked to deal in generalities, not specific problems. They seldom take, in his words, "the pains to find and discuss the actual solutions," except in the simplest cases.[33]

He also worked hard at demonstrating the difference between mathematical physics and applied mathematics. The difference is by no means trivial. In commenting on the staff of mathematicians selected for Brown's applied mechanics program, von Kármán distinguished two ways of working in the field of applied mathematics, using a metaphor, including diagrams, of "a warehouse of mathematical knowledge" to make his point. The scientist could live in the warehouse and find uses for the equations on the shelf, or he could visit the place from time to time with a shopping list. Von Kármán saw himself as a shopper, not as the caretaker of the mathematics building.[34] To a rigorous mathematical physicist like John L. Synge, however, von Kármán's style left something to be desired. Writing to H. P. Robertson, a colleague at Princeton, he said in passing, "Kármán has a wonderful intuition, but to a mathematician his exposition is appalling; I think you know that already."[35] To be sure, no one ever accused von Kármán of excessive rigor.

The fact that von Kármán and Courant, who emigrated to the United States in 1934, did not see eye-to-eye on the development of applied mathematics, also suggests that there were some basic differences. Simply put, the two scientists did not speak the same language. Courant was fundamentally interested in mathematical physics.[36] He used mathematics to make the underpinnings of physics more rigorous. In lectures on this subject, he discussed mathematical problems which had their roots in classical physics. Unlike von Kármán who used mathematics to solve physics problems, Courant stressed general theories. The algebra and analysis in the Courant-Hilbert Methods of Mathematical Physics (1924) later provided the physicists with the tools for the further development of quantum mechanics, despite the book's classical physics origins.[37] To the extent that the quantum physicists incorporated the mathematics into the body of their own work, Courant saw himself as an applied mathematician. But von Kármán would not have defined Courant as an applied mathematician on this basis; Courant, in his view, was really preoccupied with the kinds of questions mathematicians ask, and not those of applied scientists. More often than not, the likes of von Kármán had to devise their own mathematics of approximate solutions in working out specific technical problems.[38]

Caltech's Harry Bateman illustrates another aspect of mathematical physics in practice. Bateman used his mastery in solving partial differential equations to push Maxwell's equations of electrodynamics to their limits. During the early 1920s, he applied his considerable mathematical skills to devise ingenious theories of radiation to account for the Compton Effect, in an effort to save classical physics.[39] Paul Ehrenfest, a visitor at Caltech in 1923, marvelled at Bateman's uncanny ability, but was not persuaded that the mathematician grasped the physics that underlay his calculations. In describing how they wrote a paper together, Ehrenfest remarked: "By my completely desperate questioning, I chased him around for so long in the primeval forest of his calculations that the thing grew clearer and clearer. The connections among his curious isolated results stood out ever more sharply (for him, too!!!)."[40] Bateman, to use von Kármán's metaphor, lived in the mathematical warehouse. He had little in common with applied mathematicians, whom, on one occasion, he described as mathematicians "without mathematical conscience," von Kármán's student and colleague M. A. Biot recalled.[41]

Von Kármán's taste in applied mathematicians ran to people like von Mises and Hugh Dryden. Dryden, like Raymond, belonged to that small band of early American aviation enthusiasts who brought a solid background in physics and mathematics to their work. Something of a child prodigy, Dryden took his Ph.D. at Johns Hopkins in 1919, at the age of 20, with an experimental thesis on air flow. Appointed head of the Aerodynamics Section, within the National Bureau of Standards, he continued to work on airflow problems, including turbulence and the boundary layer. In 1941 he succeeded J. C. Hunsaker as the editor of the Journal of the Aeronautical Sciences. Von Kármán held him in high esteem.[42]

A gregarious man, von Kármán used public forums to get his point of view across. In his 1940 Gibbs lecture, for example, he addressed an audience of pure mathematicians. He welcomed invitations to speak before engineering groups also.[43] These talks often found their way into print. As Thornton Fry, director of industrial mathematical research at the Bell Laboratories observed, such papers were "rather scarce."[44]

5. Getting into Print

The problem of promoting applied mathematics in this country in the 1930s was not trivial. No American journal comparable to Mises' Zeitschrift existed. Moreover, the banding together of American engineers by specialty hindered the founding of interdisciplinary journals. Von Kármán saw the problem clearly: "American engineers are organized in separate societies. Mechanical, civil, electrical, aeronautical, and automotive engineers have their own organizations, and very little contact exists between them."[45] The applied mathematics issue, in the end, boiled down to publishing interdisciplinary papers in existing journals.

Each engineering society had a separate journal. Finding sympathetic editors to deal with manuscripts which straddled more than one discipline was difficult. In 1935, for example, John M. Lessells, the journal editor for the American Society of Mechanical Engineers (ASME), pleading space problems, recommended to Clark Millikan that he withdraw a manuscript and send it to the The Journal of the Aeronautical Sciences instead. Millikan declined the invitation. "As chairman of the aerodynamics section of the editorial committee of the J. Ae. S.," Millikan wrote back, he had "naturally considered its suitability in connection with the publication of this manuscript. It seemed to me that the manuscript was too mathematical and had too much of the nature of theoretical mechanics for the rather general engineering list of readers of the Journal of the Aeronautical Sciences. I still feel that the Journal of Applied Mechanics would be a more suitable medium for the publication of this paper than would the J. Ae. S."[46] But Lessells, who apparently cared little for applied mathematics,[47] didn't bite, and the paper was ultimately published in Hunsaker's journal.[48] On another occasion, von Kármán sent Lessells a manuscript by one of his outstanding students, William Bollay. Bollay's theoretical paper, on a non-linear wing theory with application to airplane design,[49] was "refused for reasons unknown to me," von Kármán complained to Lessells later, adding that he had "sent it to Prandtl, who found it very good and will publish it in the German magazine for Applied Mathematics and Mechanics [the Mises' Zeitschrift]." Failing to grasp Lessell's editorial policy, von Kármán inquired if Caltech's contributions were treated differently: "I would be glad to hear what the principles are which rule the editorial policy. . . ."[50] There is no record that Lessells complied with the request.

To be sure, von Kármán and his students did succeed in having some of their papers published in the ASME's journal. However, the Society and the journal had a definite scientific orientation. Both institutions reflected the interests of that segment of the community "interested in elasticity and vibrations," von Kármán told J. P. Den Hartog, associate professor of applied mechanics at Harvard and a prominent figure in the Society.[51] Den Hartog had found the "Bollay Affair" particularly embarrassing,[52] since he was instrumental in hiring Bollay at Harvard. Notorious for not answering his mail, von Kármán nevertheless cared enough about "the art of editing scientific journals," as he put it,[53] to correspond at length with his colleague. In the course of the correspondence with Den Hartog, von Kármán pointed out more than once that the journal "either refused . . . papers on fluid mechanics, including aerodynamics, hydrodynamics, . . . transferred [them] to other journals, or published [them] after waiting from 1 to 1-1/2 years." The papers that did appear consequently were not, in von Kármán's opinion, "representative of the progress in applied mathematics."[54] As von Kármán saw things, the ASME had an overly restricted notion of "applied mechanics".

In von Kármán's opinion, only the Journal of the Aeronautical Sciences had "the proper attitude for theory" and not a "panicky fear of mathematics."[55] Interestingly enough, this engineering journal had only come into existence in 1933, as the publication arm of the newly founded Institute of Aeronautical Sciences, and as we saw earlier, Clark Millikan despaired of its readership's interest in mathematics.

Throughout the 1930s Caltech's aerodynamicist remained unhappy with the few publication outlets then available. "Many papers are undoubtedly misplaced," von Kármán told Brown's Richardson in 1942, because of the way the societies and their journals are organized. Here was the proof, if any was needed, he told Richardson, of a niche "for such a [new] journal as you suggest."[56]

Von Kármán encountered other obstacles during the 1930s as well. Not a few of his novel solutions to structural and civil engineering problems of those years were looked at askance by civil engineers, whose conservatism limited their view. Many of the older engineers were initially skeptical of von Kármán's proposal to build a water wind tunnel in connection with the Colorado River aqueduct project.[57] When a number of experts, including several Caltech civil engineers, could not solve the mystery of the cracks in the Grand Coulee Dam, and von Kármán was called in as a last resort, some protested: "But he has no civil engineering experience."[58] When von Kármán recommended testing a model of the new Tacoma Narrows bridge in a wind tunnel, one engineer said: "You don't mean to say that we shall build a bridge and put it in a wind tunnel?" Von Kármán later noted that the scientist "knew better, but long tradition was dictating his remarks."[59] The structural engineers assigned to investigate the collapse of the bridge simply found it hard to get away from their deeply held beliefs in static forces.

6. Conclusion

Von Kármán would have liked to incorporate the phrase "applied mechanics" in the name of Caltech's aeronautical program, but "it was impossible at the time to use" these words.[60] Neither the times nor Caltech were ready for it. Indeed, by 1940 the debate had scarcely begun. The issues were thrashed out at the national level during world War II, when words and letters flowed over what the scope, objectives, and theory-to-practice ratio of Brown's program should be, as well as what to call Brown's new journal. Some thought the journal should be the sequel to the Mises *Zeitschrift*, and hence called the "Journal of Applied Mathematics and Mechanics." Others thought the word "mechanics" ought to be dropped from the title, in deference to the ASME's *Journal of Applied Mechanics*. One critic felt that the words "applied mathematics" alone had "no generally accepted meaning."[61] People familiar with British terminology disagreed.[62] In some sense the difficulties that von Kármán faced throughout the 1930s had come to a head. What's in a name, anyway? If the controversy seems illusory to us now, it was not mere quibbling over semantics to von Kármán.[63]

NOTES

1. World War II as a stimulus for applied mathematics is discussed in M. Rees, American Mathematical Monthly, 87, 607-621 (1980); in N. Reingold, Annals of Science, 38, 313-338 (1981); and in J. B. Rosser, American Mathematical Society Notices, 29, 509-515 (1982).
2. T. von Kármán, Quarterly of Applied Mathematics, 1, 2-6 (1943).
3. Von Kármán to M. Morse, 13 September 1941, Theodore von Kármán Papers (TvK), Millikan Library, California Institute of Technology, Pasadena, Box 70.11.
4. Theodore von Kármán to R. Courant, 14 February 1927 (TvK, Box 6.14. Trans. W. Clark). Von Kármán's contacts with German applied mathematicians is covered in detail in P. A. Hanle, Bringing Aerodynamics to America (MIT, Cambridge, MA, 1982), especially chapters 3-6.
5. See C. Susskind, Twenty-five Engineers and Inventors (San Francisco Press, San Francisco, 1976) for biographical sketches of a number of the applied scientists in question.
6. M. H. Stone, in Men and Institutions in American Mathematics, J. D. Tarwater, J. T. White, J. D. Miller, Eds. (Texas Tech, Lubbock, 1976), pp. 7-29, 31-39.
7. P. S. Jones, in The Mathematical Association of America: Its First Fifty Years, K. O. May, Ed. (MAA, Washington, D.C., 1972), p. 5.
8. E. Layton, Technology and Culture, 12, 573 (1971).

9. S. P. Timoshenko, As I Remember, trans. Robert Addis (D. Van Nostrand, Princeton, 1968), p.v.

10. Ibid., p. 248.

11. Ibid., p. 252. See also C. R. Soderberg, Biographical Memoirs of the National Academy of Sciences, 53, 335-337 (1982).

12. See ref. 4.

13. Timoshenko, As I Remember, p. 26.

14. C. B. Boyer, in May (Ed.), The Mathematical Association of America, p. 32.

15. E. T. Layton, The Revolt of the Engineers. Social Responsibility and the American Engineering Profession (Case Western Reserve, Cleveland, 1971), p. 4.

16. T. von Kármán; L. Edson, The Wind and Beyond (Little, Brown and Co., Boston, 1967), p. 149.

17. T. von Kármán to R. Courant, 16 March 1937 (TvK, Box 6.15).

18. The Clark B. Millikan Papers (CBM), Institute Archives, California Institute of Technology, Pasadena, are a rich source of information about the pre-von Kármán period.

19. E. T. Bell to H. Bateman, 4 July 1927, Harry Bateman Papers, Institute Archives, California Institute of Technology, Pasadena, Box 1.1.

20. W. R. Sears; M. R. Sears, Annual Review of Fluid Mechanics, 11, 7(1979).

21. Bulletin of the California Institute of Technology, 49(2) (1940); also Caltech aeronautics department statistics on employment of GALCIT students after graduation.

22. T. von Kármán to T. Levi-Civita, 12 April 1922 (TvK, Box 18.8).

23. T. von Kármán to R. G. D. Richardson, 8 September 1942 (TvK, Box 80.38); T. von Kármán to H. M. Westergaard, 16 February 1945 (TvK, Box 20.37).

24. T. von Kármán; F. L. Wattendorf, in Miszellanneen der Angewandten Mechanik. Festschrift Walter Tollmien (Akademie-Verlag, Berlin, 1962), p. 103.

25. "Assembly Address," 6 January 1920, pp. 4-5, Robert A. Millikan Papers, Institute Archives, California Institute of Technology, Pasadena, Box 20.9.

26. T. von Kármán to the CIT Executive Council: A proposal to establish a hydraulics laboratory, 11 January 1932 (TvK, Box 72.10).

27. Von Kármán and Edson, The Wind and Beyond, pp. 205-206.

28. Ibid., pp. 207-208.

29. A. F. Gunns, Pacific Northwest Quarterly, 72, 168 (1981).

30. Von Kármán and Edson, The Wind and Beyond, p. 214.

31. T. von Kármán, Bulletin of the American Mathematical Society, 46, 617 (1940).

32. Ibid., pp. 60-62.

33. T. von Karman, Quarterly of Applied Mathematics, 1, 4(1943).

34. T. von Kármán to R. G. D. Richardson, 8 September 1942 (TvK, Box 80.38).

35. J. L. Synge to H. P. Robertson, 7 April 1941, Howard P. Robertson Papers, Institute Archives, California Institute of Technology,

Pasadena, Box 4.47.

36. C. Reid, Courant in Göttingen and New York. The Story of an Improbable Mathematician (Springer-Verlag, New York, 1976), p. 226.

37. Ibid., pp. 98, 113-114.

38. T. von Kármán, Quarterly of Applied Mathematics, 1, 5-6 (1943).

39. R. H. Stuewer, Proceedings of the XIVth International Congress of the History of Science, vol. 2, 320-323 (1975); R. H. Stuewer, The Compton Effect: Turning Point in Physics (Science History Publications, New York, 1975), pp. 312-314.

40. P. Ehrenfest to T. Ehrenfest, 16 February 1924, quoted in M. J. Klein, "America Observed: Paul Ehrenfest's Visit in 1923-24," presented at the Conference on the Recasting of Science between the Two World Wars (Florence and Rome, 1980), unpublished.

41. M. A. Biot, Journal of the Aeronautical Sciences, 23, 406(1956).

42. T. von Kármán to J.P. Den Hartog, 24 February 1940 (TvK, Box 7.2); T. von Kármán to R. G. D. Richardson, 8 September 1942 (TvK, Box 80.38).

43. For example, T. von Kármán, Mechanical Engineering, 308-310 (1940).

44. T. Fry to T. von Kármán, 21 May 1940 (TvK, Box 7.2).

45. T. von Kármán to J. P. Den Hartog, 24 February 1940 (TvK, Box 7.2).

46. C. B. Millikan to J. M. Lessells, 16 September 1935 (CBM, Box 12.6).

47. W. Prager to T. von Kármán, 12 March 1943 (TvK, Box 80.39).

48. C. B. Millikan, *Journal of the Aeronautical Sciences*, *3*, 91(1936).

49. W. Bollay, *Zeitschrift für Angewandte Mathematik und Mechanik*, *19*, 21(1939).

50. T. von Kármán to J. M. Lessells, 8 July 1938 (TvK. Box 18.6).

51. T. von Kármán to J. P. Den Hartog, 14 February 1940 (TvK, Box 7.2).

52. J. P. Den Hartog to T. von Kármán, 14 February 1940 (TvK. Box 7.2).

53. T. von Kármán to A. Kuethe, 5 March 1940 (TvK, Box 43.11).

54. T. von Kármán to J. P. Den Hartog, 14 February 1940 (TvK, Box 7.2).

55. *Ibid*.

56. T. von Kármán to R. G. D. Richardson, 8 September 1942 (TvK, Box 80.38).

57. Von Karman and Edson, *The Wind and Beyond*, p. 205.

58. *Ibid*., p. 207.

59. *Ibid*., p. 214.

60. H. Liepmann, *Engineering and Science*, *42*,(5). 20(1979).

61. W. Prager to H. Dryden, 9 December 1942 (TvK, Box 80.38).

62. Memorandum from J. L. Synge, 15 December 1942 (TvK, Box 80.38).

63. These issues never really die. For a recent discussion of the problem with respect to physics and applied physics, see D. L. Goodstein, *Physics Today*, *29*, 9-13 (October 1976).