John Miles

About John Miles

by David S. Saxon
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(transcript)

I am very pleased to be here. John Miles and I have known each other, and our lives have been intertwined to a degree, for more than forty-five years, ever since those days in World War II when we were both graduate students — he at Caltech and I at MIT. It is a very real pleasure and a privilege for me to participate in this symposium in his honor and especially to have alone been asked to speak to his personal and human qualities rather than to the technical and scientific topics which have been at the center of his professional interests and which are the focus of this symposium in his honor.

There are three roughly equal time periods that I am going to be talking about: the early years — which means Caltech, the MIT radiation laboratory and the World War II years in general — the UCLA years, and lastly the UC San Diego years. But first let me give you some bare bones data about John — which I hope I'll be able to flesh out in due course — just in case there is somebody in this room who doesn't have the faintest idea of who in fact John Miles is.

First of all, today is actually his seventieth birthday. He was born on December 1, 1920 in Cincinnati, Ohio. He attended high school in Oakland, California in the 1930's. That's of course where Gertrude Stein said "there's no there there," but she was wrong. John was there. He entered Caltech in 1938 and was awarded a bachelor's degree in electrical engineering in 1942, a Master of Science in both electrical engineering and aeronautical engineering in 1943, and a Ph.D. in electrical engineering in 1944. He briefly held positions at General Electric, Caltech, the MIT radiation laboratory and at Lockheed before he embarked on a forty-five year life-long career as a faculty member of the University of California — first at UCLA and then at UC San Diego. He has fulfilled editorial responsibilities for a half-dozen or so journals and reviews; he has served as a consultant for a dozen or so institutions; and he has been very frequently honored. I mention only that he is a member of the National Academy of Sciences and a Fellow of the American Academy of Arts and Sciences, and that he was awarded the Timoshenko Medal of the American Society of Mechanical Engineers in 1982. And of course, he has, over the years, published a considerable body of papers. How many? Well, the number is a rapidly moving target. As of July 13, 1989, 331 papers had been published in professional journals. We now know that the list needs to be supplemented by one letter to the journal of record in Australia.

I don't know a lot about John's years at Caltech, but I know that his thesis supervisor was W. R. Smythe, author of the famous (or perhaps infamous) book *Static and Dynamic Electricity* and teacher of an equally famous (or infamous) course based on that book, a so-called "candidacy course." As such it was notoriously effective in screening out those deemed unsuited for going on to a Ph.D. — at least in physics — at Caltech. I know lots of people of my vintage who suffered through that and complained about it. Both the work and the course were hated by generations of Caltech students not only because they were forced to take it seriously, but also because the book and the course contained a very large number of extremely difficult, complicated, lengthy and — some would say — unworkable problems.

The remarkable fact, however, is that John elected to pursue his dissertation under W. R. Smythe of whom so many brilliant Caltech students stood in stark terror. At this stage, I thought I had another remarkable fact to tell you about John. I thought, and I believed for decades, that John was the only person in the world except Smythe himself to have worked every single problem in the book. Clearly that's a believable story; if I told you that were true, you would all believe it. But it turns out, when I checked with John, that I was wrong. It was Bateman's famous book — not Smythe's — in which John worked every problem. I add parenthetically that Bateman's problems were as challenging and I think more interesting than Smythe's.

Now, in pursuing his dissertation, which was being carried out during the war years, John found it desirable to come to the MIT radiation laboratory where I was also pursuing my dissertation as an MIT graduate student. That's where we met. There we both came under the influence and tutelage of Julian Schwinger. I want to tell you a little about the setting in which we met, the MIT radiation laboratory. It came into practical effective existence in a remarkably short time. According to an official document, it was born on October 11 of 1940, given a director on October 16, a home on October 17, and a staff starting on October 28, all in 1940. The IEEE just celebrated on October 9 the fiftieth anniversary of the founding of the MIT radiation laboratory and dedicated a commemorative plaque at MIT on that date. John, I have a little program folder that I'll give to you afterwards as a reminder of our days in that laboratory.

Now, the goals of the MIT radiation laboratory were, first of all, to develop working and combat effective radar systems, and to do it in real time, that is, to develop systems that would have an impact on the war then being conducted, but also, to seriously pursue long term development in that kind of basic research required to understand the underlying phenomena. The lab was remarkably successful on both fronts. Let me describe some of the working systems that came out of the work at the laboratory. The first spectacular success had to do with the detection of German U-boats in the Bay of Biscay. German U-boats were in the bay; they were sinking a tremendous number of ships going to and from England; and the tactic was to come up during the night to recharge the batteries so that during the daytime they could be submerged and be undetected. The radars developed at the MIT radiation laboratory and flown over to England, enabled MIT people, who actually flew in the bombers, to see those subs at night, which then began sinking them and turned the tide of submarine warfare. That's one example of such a system, but there were many others. There were radar sytems developed at the laboratory for both tactical and strategic bombing. There were aircraft detection radars, identification friend or foe (IFF) radars, microwave early warning radars, navigational radars, and so on. All of those kinds of practical working systems came out of the work at the laboratory.

But at the same time, the laboratory was undertaking basic research and development work — work on magnetrons and antennas, guided waves, and the propagation and absorption of radar in the atmosphere, on noise. I am struck by the fact that radar as a tool to tell you something about the state of the ocean surface is the other side of what was during the war a terrible problem, what was called *clutter*. The fact that the ocean reflected radar meant that it was hard to detect aircraft. A lot of work was done at the radiation laboratory to try to understand how to separate signal from noise, because the environment was often very noisy. And then there was work done on nonreflective coatings which has had some consequences — I don't kow if they're good or bad — for such things as stealth aircraft. Of course there have been enormous ramifications of that wartime work beyond its impact on the war itself -- ramifications from microwave ovens to microwave transmissions, from transistors to atomic clocks, from NMR to linear accelerators, from blind landings to being able to see the surface of Venus.

In my judgment, the reason that the lab was so successful was, first of all, the motivation of the people there. I know it's difficult for those of you who are younger to realize that there existed a time when there was absolute and almost total unanimity of conviction that something extraordinarily important to us all was at stake, but that was the case during World War II. No one that I knew doubted that what was at stake was the future of western civilization, and therefore people were prepared to work and sacrifice. There was mighty little complaining about the effort called for. Whether it had to do with the people serving in the armed forces, with the rationing of food and gasoline, or whether it had to do with people working on such problems as radar, the motivation was simply remarkable.

Secondly, the leadership was outstanding. The lab director was Lee Dubridge, who was subsequently president at Caltech. Then there were F. W. Loomis, I. I. Rabi from Columbia, Sam Goudsmit. And the laboratory was filled with unbelievably talented people: Luis Alvarez, Hans Bethe, Julian Schwinger, George Uhlenbeck, Ed Purcell, Bob Pounds, R. H. Dicke, Norman Ramsey, Jerry Wiesner, and on a somewhat less than full time basis, Mark Kac and Norbert Wiener. A lot of Nobel Laureates in that list, a lot of people whose subsequent work turned out to be very important.

And I think that the final reason for its success was the long-term perspective taken by the laboratory coupled however to urgent present needs. That combination kept everybody working. By the end of the war almost 4000 people were working in 12 divisions divided into more than 100 groups. There were overseas branches. We had people in England, people in the Far East responsible for maintaining radars and instructing people how to use them. The laboratory was responsible for the construction of radars — not just prototypes — but the radars that were actually used. With all of that, the laboratory was unbureaucratic, informal, participatory, and the people who worked in it had maximum independence.

Now I want to focus my attention on one of those 12 divisions — the basic research division, which was headed by I. I. Rabi. There were four experimental and developmental groups which had such people in them as Purcell and Pounds and Dicke and Ramsay and Nathan Marcuvitz and so on. And then there was the theory group which was headed by George Uhlenbeck. Hans Bethe and John Slater were briefly members, and for the long term Schwinger, Kac, Alfredo Banos from UCLA, Hansen from Stanford, and Wiener. And it was there in that theory group that John Miles (during most of 1944) and I (for almost four years) had our homes. We were joined by other graduate students — Harold Levine, then a graduate student at Cornell and now at Stanford, and Herb Goldstein, then a graduate student as I was at MIT. After the war he went to Harvard and wrote the famous classical mechanics book that is still being used, and now he is at Columbia. Here is a picture (Plate 1) of that theory group. There are two important people who are not in this photograph. One of them is John Miles. The other missing person is Julian Schwinger, who was surely sleeping.

I mentioned that there were a number of graduate students working at the lab. I was working on my dissertation; John was working on his. When I was "invited" to join the lab, the war was on, and it was about the only place it was possible to work. I was told by the MIT people that I could either get paid or I could work on my dissertation, but that these were mutually exclusive alternatives. John, did you get paid?

(*John Miles:* Millikan said I could, in a moment of weakness.)

There was no one at MIT who suffered such a moment of weakness. That was a wonderful incentive to get through that dissertation in a hurry. The result of that policy, which I think was seriously misguided, was that the number of Ph.D.'s granted during those years was down to almost zero. I think there were only three MIT Ph.D.'s during those years.

(*Someone:* What did you eat?)

My wife was working. But I ate a lot of hamburger, an awful lot of hamburger, usually in casseroles.

Let me talk a little bit about the intellectual environment of the radiation laboratory. The fact is that we were working on microwaves, which meant we were working on electromagnetic theory. The two limits people knew something about were the static limit,

PLATE 1. The theory group at the MIT radiation laboratory. *Bottom row:* B. Siegle, A. Banos, D. Perkins, J.F. Carlson, G.E. Uhlenbeck, A.J.F. Siegert, P.Elfman, W.N. Furry, R. Krock. *Second row:* H. Levine, P.M. Marcus, M. Karakashian, M.-C. Wang, S. Read, A.E. Heins, N.H. Painter. *Top row:* D.S. Saxon, F.E. Bothwell, F.B.Hildebrand, P.D. Crout, J.K. Knipp, H. Goldstein.

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and the optical limit, where the wavelength is really small compared to the size of the system. Here we were dealing with circumstances in which the wavelength is comparable to the size of the system, and nobody knew very much. It was a brand new field, and therefore it was a level playing field for all participants. Graduate students and professors were all on the same level field. (There were a few experts — cyclotroneers; they had worked with microwaves in cyclotrons.) As a result, the leaders who emerged were uniformly very young, very smart, very quick, and very innovative. Past histories had nothing to do with it. It was a fascinating thing to see. One side effect for me was the realization that university professors of physics — even ones with great and deserved reputations — largely were not supermen, and that therefore I myself could rationally aspire to an academic career. That was a revelation to me. There was a second side effect, and that was how quickly real genius was recognizable. Guys like Alvarez and Purcell and Schwinger stood out very quickly like mountains on the landscape. With respect to Schwinger, the mechanism by which he influenced events at the lab was a series of lectures that he gave. In those lectures he developed the whole notion of waveguide equivalent circuits as a way of analyzing guided wave systems. Fundamentally, the physicist's approach is to break things down into elements and to deal with complicated systems by combining simple elements. Schwinger understood how to do that, and did it very systematically. Then he had techniques to deal with real problems. The first set of techniques was to recognize the power of Green's functions and integral equations in these

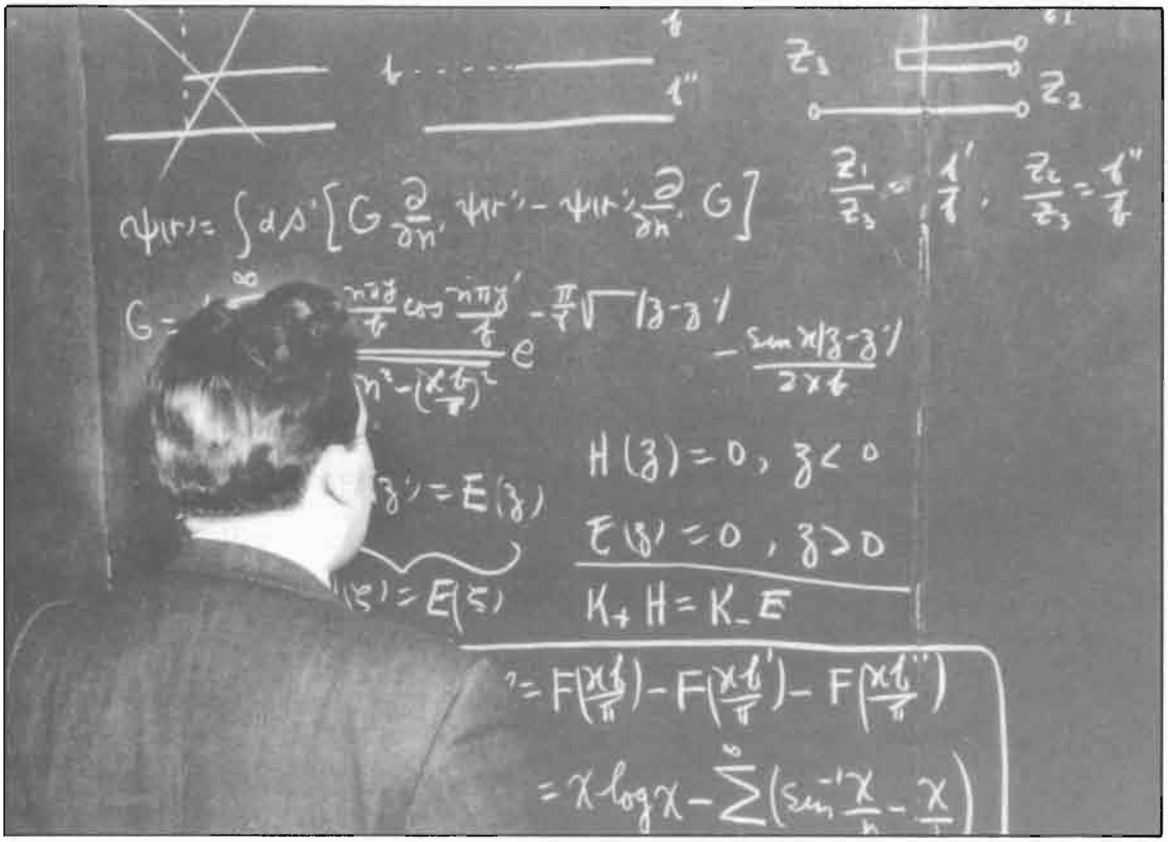

PLATE 2. Julian Schwinger at the chalkboard.

boundary value problems. He developed a set of very innovative variational principles to deal with them. Schwinger was not simply interested in formulating problems; he was interested in getting answers, and he did a lot of calculations himself. He was a master at doing these on the back of an envelope. And then Schwinger also introduced us to the Wiener-Hopf method. My second picture (Plate 2) is from one of Schwinger's lectures on bifurcated wave guides, and I think you will recognize the Wiener-Hopf decomposition. *G* is the Green's function, and equivalent circuit notation is up on the right.

All of this stuff sprang out of Schwinger's head like Venus, fully developed, clothed in beauty. There was never any buildup. He would come in and suddenly we would see something we had never seen before, and one time it was Wiener-Hopf. He just entered the room, and said the next problem would be the bifurcated waveguide, and started to do it. That was a particularly marvelous thing to me, to see Wiener-Hopf. It was comparable to what I felt when I first saw Cauchy's theorem. I couldn't believe it. Cauchy's theorem was easy to understand, easy to use, and it couldn't be right. It was too simple. The problems you could solve with it were so unbelievably difficult that I spent a lot of time trying to find integrals simple enough to do by other methods so that I could make sure that Cauchy's theorem was really right. Wiener-Hopf has a similar character to me... an unbelievably powerful thing. Almost out of thin air you create results. I was happy to find out recently that I was not alone in my reaction. In a reminiscence of

Landau, the famous Russian theoretical physicist, Khalatnikov mentioned that Hopf-Wiener (as he called it) was an example of the kind of mathematics that Landau most appreciated. He said that "Landau especially admired the elegance and efficiency of the method," and Wiener's and Hopf's names were celebrated amongst Landau and his pupils. Unfortunately, Landau met Wiener in the early 1960's in Moscow, and, to his great disappointment, he found out that Wiener was preoccupied with information theory. Afterward Landau said: "I have never met a more narrow-minded man. It is quite clear that *he* could not have thought up the Hopf-Wiener method. It must have been Hopf."[1]

Let me say a word about the social environment of the laboratory. The working hours were horrendous — eight in the morning until six in the evening every single day except Sunday, no holidays, and no complaints. We came. We were there. We didn't go home for lunch; we didn't go out for lunch. Every group had a small lunch room where you could get a sandwich, and you were expected to spend half an hour at it. And people really followed that regimen. To provide some degree of relief and to help motivate people there were seminars — laboratory-wide seminars, the Schwinger lectures. There were occasional dinners. We had regular armed service films which showed what was going on in the war, and which always tried to show what the effects of radar were.

Now, in that setting, Schwinger's hours were a little different. The earliest I ever saw Schwinger arrive at the laboratory was four in the afternoon, and usually it would not be until after six. It was not uncommon to come in at 8 a. m. and find him still there; he would have been there all night. So the overlap integral was almost zero. If you wanted to see Schwinger, you had to stay through the night, *or* we had these lectures which were devised to permit people to interact with him.

All of us came under the influence of Schwinger, some more than others. I've often reflected on those days. In retrospect, it is clear to me that I sought to be "one of the boys." I was looking hard to be accepted by my betters in the lab and by those who were older than I, and I was certainly one of Schwinger's disciples. John, on the other hand, was indifferent to such things. He was a much quieter, much more independent, more disciplined man from the west (as I thought of him) who learned from Schwinger but was not dominated by him in the same way as the rest of us. I've often reflected that our subsequent histories were foreshadowed by this contrast in our attitudes and personalities.

With the end of the second world war the laboratory began to be dispersed, but it didn't happen as suddenly as it was formed, because there was a commitment to write the famous radiation laboratory series, and many people stayed on to participate in that. But John had left in the fall of 1944 to go to Lockheed, having finished his work at MIT, and I left in May of 1946 to go to Philips Laboratory, and to go on to quite other kinds of physics.

I come now to the UCLA years. John went there in 1945 as an assistant professor of engineering, and I joined the physics department in 1947 through the good offices of Alfredo Banos, whose picture I showed you a moment ago. It was there that John and I renewed our friendship. Of course, John rose very rapidly through the ranks because his productivity was quite astounding in those early years. I have looked through the record, and he was turning out a tremendous amount of very interesting work. UCLA in those years was a very different place from what it is now. The College of Engineering was totally different than any other college of engineering in the country. In a way, it was ahead of its time, but the translation of innovative ideas into reality suffered in the execution. The dean was better at formulating what an engineering school ought to be than at picking people to execute his ideas. I think that John was a little unhappy with that,

[1]Quoted from "Reminiscences of Landau" by I. M. Khalatnikov in *Physics Today,* May, 1989.

sufficiently so that it was clear to me and others that he was beginning to champ at the bit. One day he came in to see me — this was about 1961, a time when the cold war was at its coldest, threats of atomic warfare were serious — and he said he was thinking about going to the South Pacific, maybe living there. I learned later that he had had a conversation with Jerry Wiesner, and Wiesner had said: "You know, I think it's very important in these difficult times that at least some scientists go to areas in the world that are as safe as can be from the possible consequences of atomic warfare so that science can survive." And it was very typical of John that he thought about it and then acted on it. A lot of people thought about it. A lot of people talked about it. But John acted. He actually went to Australia for a couple of years, but eventually decided that was not for him, and when he came back he came to UC San Diego in 1964.

I don't want to talk about his career as a scientist here at UCSD; this symposium is about that. But the other side of John that needs to be talked about is John as an academic leader. He was the chairman of his department, Applied Mechanics and Engineering Science, for four years. That's one kind of leadership position. That's something that doesn't happen in this university unless your colleagues want you to be chairman. But he was also chairman of the Academic Senate. That doesn't happen in this university unless your colleagues across the whole faculty want you to do that. It's a completely different kind of leadership role. And John was chairman of the Academic Senate during a rather difficult period in campus history. There were problems of transition in leadership. Then in 1980 John became the vice-chancellor for academic affairs. The vice-chancellor for academic affairs is the principal administrative officer on this campus after the chancellor. The chancellor, Dick Atkinson, had just been appointed in 1980. John was his first choice as vice-chancellor, and that was a crucial appointment for Atkinson to make. His career had not been spent here; he was a stranger to the people here; and that first appointment was a key one. As vice-chancellor, John participated in the establishment of the first eleven endowed chairs, appointed the usual quota of deans and provosts, and so on. He was, I am told, a stern taskmaster. He had very high standards, but he was regarded as manifestly fair. He was widely respected, revered by his staff, and in talking to some of them I heard phrases like, "It was an honor to work with him." And the words that were used in talking about him were words like "integrity," "honesty," "respect," "love," and "admiration." His staff also pointed to the ever-present influence of Strunk and White's *Elements of Style*. John didn't just have it for his graduate students; he brought it in to the office, and he made it clear to the staff that no camp and no cliches and no pomposity were going to be allowed. In fact, he issued a directive that no use whatsoever would be permitted of such words as *hopefully, ongoing, impact* or *de-impact*, and *prioritize*. That was in 1980. Now I suppose we could add a few words, John, like *down-sizing*.

I have picked up a few anecdotes. My source here is Pamella Jung, who is a special assistant to the vice-chancellor, and was John's colleague during those years. She has been very generous and very helpful. A couple of examples. Pamella says that one day during his first week as vice-chancellor in July of 1980 she smelled smoke in his office. It smelled like a cigar. John admitted that it was, said that he only smoked cigars, and that he only smoked when he was relaxed. Pamella says that she did not again detect cigar smoke in his office until three years later, in 1983, the week he stepped down from his position as vice-chancellor. She also emphasized John's passion for regimen, bicycle riding and swimming. I knew John when he was a surfer, but now he is a swimmer. Every day, she says, *every day*. She says that he loved penny candy, especially licorice, but he worked out a deal with her by which he was to get precisely a one-and-one-half inch piece at precisely the same time every day. Hiking is another of his passions, but hiking to schedule and to route — not wandering — *hiking*.

Let me conclude by talking about something I expect you've all wondered about — a question you've often wanted to ask but haven't dared to. You must be curious about what you have to do to achieve the publication of more than 330 papers in a career. Well, I've already given some hints and clues. Obviously you have to be disciplined. You have to be focussed. You have to be smart. But I've gone over John's publication record — I may be the only person who has done that for quite a long time, John — and, judging by that record, I've come up with a few rules for you ordinary folks.

The first rule is: Write your own papers. Don't fool around with co-authors. Paper number 4 had a co-author. The next paper with a co-author was paper 102. Well, I suspected that maybe John was winding down, so I looked at the last 50 papers. He *is* winding down. There were 4 co-authored papers in the last 50. Among John's few co-authors are Walter Munk, Herbert Huppert, Ted Buchwald, Freeman Gilbert, George Carrier, and such people as that.

The second thing you need to do is to understand your field in all of its ramifications, which means broadly as well as deeply. John, I was especially impressed by one of your early papers, number 7, which was published in 1946, when you were really just getting rolling. I didn't read the paper; just the title impressed me: "Analogy among torsional rigidity, rotating fluid inertia, and self-inductance for an infinite cylinder."

On the other hand, you need to avoid John's mistakes. He allowed himself to be distracted by his administrative responsibilities. From 1946 to 1988 John averaged a bit more than 7.6 publications per year. But when he was chairman of his department from 1968 to 1972, his average plunged all the way to 6.6 publications, and it fell to a dismal 6.5 publications per year when he was vice-chancellor. Don't let yourself be distracted.

Then you must love what you do, master both your subject and the tools of your trade, keep at it, and, above all, don't let fame or recognition or high honor turn your head. John, this symposium is a great and deserved tribute. It's pretty heady stuff. But take my advice. Enjoy it tomorrow. That's Sunday. And then forget about it as fast as you can. You be at your desk as usual on Monday morning, after your swim, before your cigar and one and one half inches of licorice, and get back to work.

(*Editor:* He did.)

David Saxon

John Wilder Miles

BIOGRAPHY

December 1, 1920	Born in Cincinnati, Ohio
1935-1938	Oakland High School
1942	B.S. in Electrical Engineering, Caltech
1943	M.S. in Electrical Engineering and Aeronautical Engineering, Caltech
1943	Married Herberta Marie Blight Children: Patricia Marie, b. 1949 Diana Catherine, b. 1953 Ann Leslie, b. 1956
February-October, 1944	M.I.T. Radiation Laboratory
1944	Ph.D. in Electrical Engineering, Caltech
1944-1945	Engineer, Lockheed Aircraft Company
1945-1964	Professor of Engineering, UCLA
1951	Fulbright Lecturer, University of New Zealand
1952	Visiting Lecturer, University of London
1958-1959	Guggenheim Fellow, Cambridge University
1962-1964	Professor of Applied Mathematics, Australian National University
1964-present	Professor of Applied Mechanics and Geophysics, UCSD
1966-present	Associate Editor, *Journal of Fluid Mechanics*
1968	Fellow, American Institute of Aeronautics and Astronautics
1968-1972	Chairman, Department of Applied Mechanics and Engineering Sciences, UCSD
1969	Fulbright Research Fellow, Guggenheim Fellow, Cambridge University
c. 1970	Fellow, American Academy of Mechanics

1973	Fellow, American Academy of Arts and Sciences
1977-1978	Chairman, Academic Senate, UCSD
1979	Member, National Academy of Sciences
1980-1983	Vice Chancellor for Academic Affairs, UCSD
1980-present	Overseas Fellow, Churchill College, Cambridge University
1982	Awarded the Timoshenko Medal by the American Society of Mechanical Engineers
1983	Otto Laporte Lecturer, American Physical Society

Russ Raitt, John Miles and Chip Cox in 1967

John W. Miles

Scientific Papers 1943–1990

1943

1. "Applications and limitations of mechanical-electrical analogies, new and old," *J. Acoust. Soc. Amer.* **14**, 183–192 (Jan., 1943)

1944

2. "The reflection of sound due to a change in cross section of a circular tube," *J. Acoust. Soc. Amer.* **16**, 14–19 (July, 1944)

3. "Junction analysis in vacuum-tube circuits," *Proc. I.R.E.* **32**, 617–620 (Oct., 1944)

1945

4. (With S.S. MacKeown) "The plane wave resolution of guided waves," *Proc. I.R.E.* **33**, 805–808 (Nov., 1945)

1946

5. "The analysis of plane discontinuities in cylindrical tubes, Part I," *J. Acoust. Soc. Amer.* **17**, 259–271 (Jan., 1946)

6. "The analysis of plane discontinuities in cylindrical tubes, Part II," *J. Acoust. Soc. Amer.* **17**, 272–284 (Jan., 1946)

7. "An analogy among torsional rigidity, rotating fluid inertia and self-inductance for an infinite cylinder," *J. Aero. Sci.* **13**, 377–380 (July, 1946)

8. "The equivalent circuit for a plane discontinuity in a cylindrical wave guide," *Proc. I.R.E.* **34**, 728–742 (Oct., 1946)

1947

9. "Contributions to wave-guide theory," *Proc. I.R.E.* **35**, 378 (April, 1947)

10. "The aerodynamic forces on an oscillating airfoil at supersonic speeds," *J. Aero. Sci.* **14**, 351–358 (June, 1947)

11. "The diffraction of sound due to right-angled joints in rectangular tubes," *J. Acoust. Soc. Amer.* **19**, 572–579 (July, 1947)

12. "The equivalent circuit for a bifurcated cylindrical tube," *J. Acoust. Soc. Amer.* **19**, 579–584 (July, 1947)

13. "Equivalent circuits for plane discontinuities," *Proc. I.R.E.* **35**, 792 (August, 1947)

14. "Coordinates and the reciprocity theorem in electromechanical systems," *J. Acoust. Soc. Amer.* **19**, 910–913 (Sept., 1947)

15. "The equivalent circuit of a corner bend in a rectangular wave guide," *Proc. I.R.E.* **35**, 1313–1317 (Nov., 1947)

16. "Plane discontinuities in coaxial lines," *Proc. I.R.E.* **35**, 1498–1502 (Dec., 1947)

1948

17. "Acoustical methods in supersonic aerodynamics," *J. Acoust. Soc. Amer.* **20**, 314–323 (May, 1948)

18. "Harmonic and transient motion of a swept wing in supersonic flow," *J. Aero. Sci.* **15**, 343–347 (June, 1948)

19. "The rolling moment due to sideslip for a swept wing," *J. Aero. Sci.* **15**, 418–424 (July, 1948)

20. "The diffraction of a sound wave by an infinite set of plates," *J. Acoust. Soc. Amer.* **20**, 370–374 (July, 1948)

21. "The coupling of a cylindrical tube to a half-infinite space," *J. Acoust. Soc. Amer.* **20**, 652–664 (Sept., 1948)

22. "The aerodynamic forces on an oscillating flap at supersonic speeds," *J. Aero. Sci.* **15**, 565–568 (Sept., 1948)

23. "Transient loading of airfoils at supersonic speeds," *J. Aero. Sci.* **15**, 592–598 (Oct., 1948)

24. "On vector transforms," *Phys. Rev.* **74**, 1531 (Nov., 1948)

1949

25. "Induction effects in aeroelasticity," *J. Aero. Sci.* **16**, 63–64 (Jan., 1949); also **16**, 126 (Feb., 1949)

26. "On chordwise divergence," *J. Aero. Sci.* **16**, 126–127 (Feb., 1949)

27. "On electromagnetic diffraction through a plane screen," *Phys. Rev.* **75**, 695–696 (Feb., 1949)

28. "On diffraction through a circular aperture," *J. Acoust. Soc. Amer.* **21**, 140–141 (March, 1949)

29. "Transform and variational methods in supersonic aerodynamics," *J. Aero. Sci.* **16**, 252 (April, 1949)

30. "The diffraction of a plane wave through a grating," *Quart. Appl. Math.* **7**, 45–64 (April, 1949)

31. "On harmonic motion at supersonic speeds," *J. Aero. Sci.* **16**, 378 (June, 1949)

32. "On the oscillating rectangular airfoil at supersonic speeds," *J. Aero. Sci.* **16**, 381 (June, 1949)

33. "Quasi-stationary thin airfoil theory," *J. Aero. Sci.* **16**, 440 (July, 1949)

34. "A note on a solution to Possio's integral equation for an oscillating airfoil in supersonic flow," *Quart. Appl. Math.* **7**, 213–216 (July, 1949)

35. "A formulation of the aeroelastic problem for a swept wing," *J. Aero. Sci.* **16**, 477–490 (August, 1949)

36. "Quasi-stationary airfoil theory in compressible flow," *J. Aero. Sci.* **16**, 509 (August, 1949)

37. "On the oscillating aileron at supersonic speeds," *J. Aero. Sci.* **16**, 511 (August, 1949)

38. "On the diffraction of an electromagnetic wave through a plane screen," *J. Appl. Phys.* **20**, 760–771 (August, 1949) [See errata, #51]

39. "On nonsteady motion of delta wings," *J. Aero. Sci.* **16**, 568 (Sept., 1949)

40. "On damping in pitch for delta wings," *J. Aero. Sci.* **16**, 674 (Sept., 1949)

1950

41. "Unsteady flow theory in dynamic stability," *J. Aero. Sci.* **17**, 62 (Jan., 1950)

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