

## **An academic life**

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My academic career has been somewhat unorthodox. After two years as an undergraduate at Edinburgh University I was entitled to a first degree, not because I was smart but because returning World War 2 servicemen were allowed a year off from the usual three year requirement for an Ordinary degree. I got an M.A. (ordinary) (war emergency) in 1951, and was able to go on to do a year's postgraduate work in phonetics. At the end of that year I got my first job, as a lab assistant cutting vinyl recordings; and on January 1st 1953 I was promoted to Assistant Lecturer in Phonetics.

In the late 1950's I thought it might be good to work in the United States. This required my completing a Ph.D., a degree that none of my colleagues in the Phonetics Department and two thirds of my colleagues in the Faculty of Arts did not have. I went to the University Registrar and asked him what I should do. He noted that I'd been a member of the faculty for more than three years, and that I could count that as the period that I had been registered. All I needed was a thesis. So I consulted David Abercrombie, the head of the Phonetics Department, and on his advice took three papers that I had already published on aspects of vowel quality, and added an introductory survey. I also appended some work that I had been doing on Cardinal Vowels with Daniel Jones, who had recently retired from the chair of phonetics at University College, London.

Abercrombie had arranged a grant enabling Daniel Jones to be a consultant on my project to study the acoustic quality of cardinal vowels, largely so that I would have the opportunity to work with the leading phonetician of the time. Jones never defined what he meant by saying that the cardinal vowels were acoustically equidistant. He thought that the tongue made equal movements between each of them, even after the publication of x-ray views of the 8 primary cardinal vowels produced by his colleague Stephen Jones showed that this was not the case (Jones, 1929). Daniel Jones himself published photographs of only four of his own cardinal vowels, although, as he told me in 1955, he had photographs of all 8 vowels. When I asked him why he had not published the other four photographs, he smiled and said "People would have found them too confusing."

The sets of vowels that I recorded under Daniel Jones's supervision were made by his former pupils. They were probably imitating him to their best ability; but they may also have been influenced by their own notions of equidistant vowels. The results of this part of the thesis are not particularly noteworthy, except insofar as they provide an early example of the problems of analyzing vowels spectrographically.

Of course, when I had completed the thesis I had to have an oral exam. This had a slightly unusual form. The outside examiner was Walter Lawrence, the inventor of the first parametric speech synthesizer, PAT. He came up from London and said "Well, Peter, I haven't actually read the whole thesis yet, but I know the published papers. Let's go off to a pub and chat for a bit. Then I'll sign the forms." And that was how I added a Ph.D. to my M.A. (ordinary) (war emergency).

It was through Walter Lawrence that I met Donald Broadbent, at that time a psychologist working in Cambridge. He also wanted to do experiments using synthetic speech, and we teamed up to test some ideas I had about the relative nature of vowel

quality, This led to our working together on other aspects of speech perception, and through him I learned how to do work in perceptual psychology.

I've always been fortunate in working with other people. Arranging for me to be able to work with Daniel Jones was not the only help of that kind that David Abercrombie provided for me. He also got David Whitteridge, the Professor of Physiology, interested in the control of the respiratory system in speech, and arranged for me to work with him. I started working in Whitteridge's lab, at first every Saturday morning, then for days at a time, and then longer and longer as we realized that the control of the respiratory muscles was no simple matter. It was really Whitteridge who taught me to be a scientist. He was fond of quoting Lord Kelvin: "You do not really know anything until you can express it in terms of numbers." Some of our work on respiration at that time contained egregious errors, but, as later work showed (Ladefoged 2004), it was basically on the right lines, correcting the views of the time on 'chest pulses' being associated with each syllable, and showing that stressed syllables were the result of extra respiratory effort.

Before going to America, I returned to Nigeria. I had already been there for a year, on a leave of absence from Edinburgh, attached to the new Phonetics Department at the University of Ibadan. I was amazed by the large number of different languages, and the opportunities for fieldwork. I had had some fieldwork experience in Scotland. Soon after my M.A. David Abercrombie had sent me off to the Gaelic speaking Outer Hebrides. He told me to find out about the differences between pairs of words such as *dubhan tu.an* (hook) and *duan tuan* (song). Many years later I went back to the Hebrides, and noted the pitch differences between these words, the first of which counts as two syllables, and the second as one. But this first field trip was an utter disaster. I had no idea how to elicit phonetic data, and came back after only a few days, having been able to find out nothing. A year or so later I had a much more profitable experience working on the Linguistic Survey of Scotland. Ian Catford had devised a well structured task in which fieldworkers had to elicit a set of words designed to make the phonology of different dialects apparent. I still had the problem that I was not good at walking up to strange houses and asking the inhabitants if I could study their vowels. But fortunately by then I had married Jenny, a much more talented and wonderful woman than any I had ever known before, and she had no difficulty in making the initial contacts with suitable speakers. We got some good data on three Scottish dialects, and I learned the first steps about eliciting phonetic data.

When we returned to Nigeria I was a field fellow on the Linguistic Survey of West Africa, a Ford Foundation sponsored project, led by Joe Greenberg and Bill Welmers. Using the talents of university students in Nigeria, Ghana, Ivory Coast, Sierra Leone and Senegal I was able to record speakers of about 60 different languages, and to make palatographic and aerodynamic studies of many of them. The principal results appeared in *A phonetic study of West African languages* (1964), a book that I now recognize as flawed because it so often described a language based on the analysis of only a single speaker.

In 1962 I was appointed as an Assistant Professor of Phonetics at UCLA, and less than a year later received an NIH grant to build a working model of the vocal organs. This was the start of the UCLA Phonetics Lab. Jim Anthony came over on leave from

Edinburgh University, and we worked with rubber molds and plaster casts, and failed to achieve anything noteworthy. We were saved from ignominy by the contributions of Vicki Fromkin, John Ohala and other colleagues who were using electromyography and aerodynamic techniques to describe the muscular activity and associated gestures that we had hoped to build into the model.

In 1967 the UCLA Phonetics Lab acquired a LINC-8 computer, described in the lab report as “a large general purpose computer with 8K of memory.” Richard Harshman, who did all the early programming, performed wonders with this minuscule memory (including teaching me how to program). Later the computer was upgraded to a machine thought of as having a massive memory, totaling 32K, enabling Lloyd Rice to help us salvage the failure to build a physical vocal tract by programming a computer model.

Our articulatory modeling was based on x-ray views of the vocal tract. This gave us the problem of converting two dimensional sagittal views into three dimensional data incorporating the width of the vocal tract as seen in the other dimension. We tried to make a physical model of the vocal tract in the position for a neutral vowel, using dental impression material. This is not difficult to do for the oral cavity. But we needed in addition an impression of the pharynx. With the help of a cooperative dental surgeon we achieved this by filling my vocal tract with a quick setting impression material. I was turned upside down, so that the impression material could be inserted *up to* the tops of my arytenoid cartilages. I had to hold my breath for only 30 seconds. We made a nice model of this actual vocal tract shape — but unfortunately it was not very accurate as the weight of the impression material distorted the pharynx, making me appear to have too large a pharyngeal cavity.

We also had to specify the vocal tract shapes for a variety of sounds. We needed a way of characterizing possible tongue shapes. We obtained x-ray data on the tongue positions in the middle of each of the English vowels as spoken by 5 speakers. While working in the UCLA Phonetics Lab Richard Harshman had invented PARAFAC, a form of factor analysis that provided a unique set of factors underlying the variation in such data. Using PARAFAC we were able to show that two factors, front raising and back raising, could generate most of the possible gestures of the tongue body. A number of computer models now use these two factors (or variants of them) to specify tongue body shapes. We also found that we could use these factors for deriving vocal tract shapes from formant frequencies. Studies of the different articulatory gestures used by different speakers led us to conclude that, alongside the possibility of a motor theory of speech perception, there is support for a theory showing that at least part of the speech output is controlled in auditory terms. In other words, we have an auditory theory of speech production.

UCLA allowed me to create my own phonetics courses. They were similar to those taught at Edinburgh, but with far more emphasis on learning to distinguish the sounds of other languages. The capstone of the introductory course was the requirement to make a recording and write a paper describing the sounds of another language. Students found this a challenging task; and I was able to work with them on numerous languages, using their friends, aunts, uncles and room mates to provide an extraordinary range of data, the best of which joined my own field recordings in the UCLA phonetic archive.

I was also able to create a Phonetics Laboratory group. For me, the people mattered more than the instruments. Of course we tried to get good computers and all the hardware we needed. But getting a group that functioned together and saw themselves as a working unit was of prime importance. I enjoyed going around every day, chatting to everyone, and lunching in the lab with as many people as possible. It was also useful to have weekly lab meetings which all the staff, students and faculty were expected to attend, even if only for the first ten minutes when we talked about what was going on. Building a research group who felt that they had a stake in the development of the lab taught me their varied ideas from statistics to engineering, and the philosophy of linguistics. And interacting with bright students keeps one intellectually honest.

Throughout my years at UCLA I spent much of my time wandering around the world trying to hear and analyze all the sounds that could distinguish words in some language or other. To begin with I had a portable phonetics lab which required a porter. It weighed more than 100 pounds, and included a Nagra tape recorder, a battery powered oscilloscope, and an ultra violet recorder, plus all the paraphernalia required for palatography and pressure and flow recording. Nowadays one can get by with much less, just a laptop computer, air pressure and flow measurements made with battery operated equipment and recorded on the computer, software that provides spectrograms, LPC and FFT spectra, and pitch and intensity displays, and a video camera not only for recording dynamic movements of the lips but also for static palatography.

I have enjoyed wandering to many corners of the earth, though fieldwork has not always been comfortable. I remember once sitting in a small boat in the Niger delta, made for perhaps 12 people. The 24 of us crammed in there were huddled under a ground sheet as torrential rain was pouring down. I had my expensive tape recorder and microphones in a theoretically waterproof bag in the bottom of the boat, with the water slowly rising. Wet and worried I wondered whether our insurance really covered the thousands of dollars of equipment. But later we sat in the village chief's hut, poured a libation of some strange potent liquor, and recorded a dozen speakers of Defaka, a dying language spoken by only a few hundred people on one of the islands in the Niger delta. When the skies had cleared we went back in an old dugout canoe. Warm and dry I watched the sun setting, thinking how lucky I was to have these opportunities.

Another delight of fieldwork is the charm of the people one meets. The !Xóõ, who were willing to have tubes put through their noses; the Hadza who have fewer possessions than anyone I know, except perhaps the Pirahã, who live with little thought for the morrow; the Toda whose courtesy and helpfulness were unparalleled; the Tsou, who could not understand why anyone would come to their mountain to record their sounds; and all manner of peoples from the Aleutian Islands to the Australian outback.

Very little of my fieldwork would have been possible without the cooperation of many great linguists. Most notable among them are Tony Traill and Jan Snyman, who took us to parts of Namibia and Botswana that we would never have visited on our own; Bhaskararao, who guided us through India, looking after our food and drink, as well as our linguistic needs; Kay Williamson, who demonstrated many of the phonetic treasures of Nigeria; Dan and Keren Everett who led me through the Amazonian rain forest; and scores of other local linguists who have been helpful, including many members of the

Summer Institute of Linguistics and other missionaries who did not worry about working with a member of Atheists for Jesus.

Fieldwork allowed me to make quantified observations of a wide variety of languages. Sounds that I have been able to investigate include several not previously measured and numerically described in the phonetic literature, such as voiceless implosives, bilabial trills, and velar laterals. Some of my nicely quantified results have later been found to be inadequate, often because I had not realized the necessity of studying several speakers of a language. Two people saying a set of phrases six times are nothing like as useful as six people saying the same set of phrases twice. There is far more variation between speakers saying the same thing than between repetitions of the same utterance by one speaker. But despite the limitations of some of the work, many of the instrumental observations have been found to be useful.

Phonetic fieldwork and the study of the sounds of a wide variety of languages led to the construction of linguistic phonetic feature systems. It was not difficult to see that the Jakobsonian and later SPE feature systems were inadequate. In *Preliminaries to Linguistic Analysis* (1971) I proposed a new feature system that accounted for a wide range of phonation types and several articulations previously unknown in the phonetic literature. This work also demonstrated the importance of recognizing an auditory basis for some features. Chomsky and Halle thought that features could be defined equally well in articulatory or auditory terms. This is incorrect. Some features group sounds together because of their auditory similarity and others group sounds that have similar articulations.

Fieldwork studies were part of my impetus for urging revision of the IPA. The IPA chart is like a one page theory accounting for all the possible speech sounds that can contrast in a language. By the mid 1980's it was apparent that the existing chart was out of date, and the International Phonetic Association needed to catch up with the times. As president, I was able to help by convening the 1989 Kiel Convention, which led to major revisions of the IPA chart.

Much of what I have learned about the *Sounds of the world's languages* is included in the book of that name, which I wrote with my colleague Ian Maddieson. Another of the good chances of my life is that I have been able to work with him. He, too, has first-hand knowledge of many languages. But his talents are different from mine. He has a more thorough, scholarly approach. He was able to curb my wilder flights of fancy, so that we produced a more balanced book, summing up the present state of our knowledge of the sounds of the world's languages.

Now that I am too old (or, to be more exact in my limitations, too deaf) to do good fieldwork, I am happily trying to sum up many aspects of my life in phonetics by writing books. I enjoyed writing *Vowels and Consonants* for those who found my *Course in Phonetics* too hard. It was also kind of the publishers to allow me to include personal anecdotes in my *Phonetic Data Analysis*, a book that was modeled on practical guides such as *An idiot's guide to ...* Other books are planned to follow. I won't die with a pen in my hand, but I may well have my hands as they are now — on my computer keys.