

FIFTY YEARS WITH PHONETICS AND  
PHONETICIANS: A CAUSERIE GIVEN  
AT THE INSTITUTE OF PHONETICS  
28/2 1981\*

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I have called this talk a causerie in order to emphasize that you should not expect any systematic survey of the development of phonetics during the last decades, nor any deep reflections on aims and methods of phonetic research. I only want to present some informal and not too serious, and quite personal memories from a long life as a phonetician.

I have called it "Fifty years with phonetics and phoneticians" but more exactly 52 years have passed since I took my first course in phonetics in 1929, and if I start from my first phonetic observation, it will be 62 years: We had just moved from Lolland to Funen, and it was the first day in my new school. We had gymnastics, and I still remember the comical contrast between what I expected to be a command and the friendly singing Funish dialect of the teacher. I decided that I would not speak this dialect. I would stick to the Standard Danish of my parents. And, as a matter of fact, I never learned to speak genuine Funish, but I came to like the dialect more and more - it became, as it were, a symbol of a very happy childhood; and later, when I went home on holidays from Copenhagen, it was always a big moment when after having passed the Belt I entered the train and heard the conductor speak Funish. Then I felt I had come home.

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\* This is a translation of a talk given at the Institute of Phonetics on the day of my retirement, 28.2.1981, slightly revised, particularly at the end. I am grateful to Betsy Uldall for improving my English style.



However, this phonetic observation was a rather isolated phenomenon. I have always been much more attentive to what I see than to what I hear; and I remember I once amazed my sister by describing a fellow student as a typical Copenhagener. "But have you not noticed that he speaks a pronounced South Jutland dialect?" she exclaimed. I must confess that I had only noticed his behaviour, not his language. Perhaps I should never have been a phonetician!

When I started studying languages it was not because I was interested in phonetics. On the contrary, I had found the phonetic transcriptions in our English and French primers rather irritating, and my first phonetic course did not change this attitude. It was a course in German phonetics, given by an assistant whom we disliked, and it consisted in learning physiological descriptions by heart and making transcriptions from orthographic texts. We never heard nor pronounced a single sound. A course in French phonetics, given by professor Sandfeld, did not interest me very much either. He was a great linguist, but phonetics was not his chief concern.

I was above all interested in general theoretical linguistics and in literature, and somewhat in the history of sounds since our Latin master at school had told us about the Germanic sound shift. However, I had to choose some concrete languages. I first thought of Danish and English, but as I heard that these subjects were rather crowded, I chose German and French instead. This rather accidental choice turned out to be very lucky. There were outstanding professors in both subjects. German was my main subject, and I owe most to Louis Hammerich, from whom I learned linguistic and philological method; from Carl Roos I learned the method of literary research. Kristian Sandfeld impressed me by his fine syntactic observations, and Viggo Brøndal by his bold and original theories and his philosophical perspectives. Brøndal's course in French phonetics was more interesting than Sandfeld's. He had constructed a general vowel system based on a restricted number of abstract features that could be combined, according to universal laws, into more and more complex vowel units. But it was not until later, when I took a course in Danish phonetics with Poul Andersen that I learned solid, phonetic method and observation.

Phonetics was, however, still a secondary interest. In those years I read all the books on general linguistics I could get hold of. I was particularly impressed by Saussure, Meillet, Schuchardt and Jespersen, quite different scholars, but each in his own way very stimulating. Around 1930 the first *Travaux* of the Prague School appeared; Hammerich drew our attention to them and lent me the books as soon as they arrived, and I read all the articles, with particular enthusiasm those by Jakobson and Trubetzkoy. It is perhaps difficult now to understand how revolutionary these new ideas appeared to us at that time. They meant an enormous widening of the horizon, a completely new way of looking at the sounds of language and the way they were integrated into the functional linguistic system. At the same time I found that the authors passed too



lightly over the phonetic substance which at the start was pushed somewhat aside as belonging to natural science. I began to be interested in the interplay between phonological structure and phonetic substance. For my master's thesis, however, I vacillated for some time between a stylistic analysis of the Middle High German epos on Tristan and Isolde and Wilhelm von Humboldt's philosophy of language but ended up by writing about the importance of dialect geography for the conception of sound change.

In the spring of 1933 I became a member of the Linguistic Circle of Copenhagen, and this was decisive for my linguistic development. It was also in other respects a useful experience. I was a quick learner and I had found the studies very easy. In the Linguistic Circle I learned modesty (although not so much that it prevented me from intervening in the discussions). At least I realized the great difference between being able to understand a theory and being able to create a theory. The Circle was at that time dominated by a number of great and original personalities, and the intellectual level was very high. Particularly Brøndal and Hjelmslev were brilliant debaters, and I have rarely - if ever - felt as deep and pure an intellectual joy as when I listened to these discussions. The Linguistic Circle was also characterized by its openness to new ideas from outside. New linguistic literature was discussed at every meeting.

Hjelmslev did not yet have any appointment at the University, but after his thesis had been accepted in 1932 he used his right to lecture as "privatdocent", and I attended his lectures on Rasmus Rask and on Grammont's theories of "phonétique évolutive". Hammerich's textbook on German phonetics, which appeared in 1934, also contained a long chapter on Grammont, and it gave on the whole a good introduction to general phonetics.

After my MA in 1936 I received a scholarship for studies in Germany, and I decided to attempt a phonological description of German dialects. My decision to study phonology more seriously was partly due to my admiration for the Prague phonologists, but it was also negatively motivated by the fact that after having written a prize essay on the definition of the sentence I was fed up with syntax and with all the pseudo-philosophical twaddle I had had to read for this purpose, and I felt a strong need for some hard facts. I studied for two terms at Marburg but did not find much stimulation for my plans. Then I wrote to Trubetzkoy and asked whether I could study with him in Vienna. I received a positive and very kind answer and this is, according to Roman Jakobson, the last existing letter from Trubetzkoy's hand. He died shortly afterwards from a heart attack, and I never met him personally.

Instead I went to Paris in order to study phonology with Martinet and in order to learn experimental phonetics, which was not taught in Copenhagen. Martinet lectured on French phonology. He was an excellent teacher, and he and his Danish wife showed me much hospitality.



At the institute of phonetics I attended a course in experimental phonetics, consisting of exercises in palatography and kymography. There was a rumour that the institute also possessed an oscillograph which, however, was rusting away in the basement. There were two kymographs, one at the disposal of the students and one for research, which I had the privilege of using for a short time. This latter, finer kymograph was driven by means of an old rope, connected to a motor, and in order to get it going one had to pour a pail of water over the rope. To obtain the kymographic tracings we spoke into a mouth-piece connected by means of a rubber tube to a small capsule with a rubber membrane which reacted to the airflow and the vibrations. A small straw attached to the membrane followed its movements and recorded them on a turning drum with a coating of soot. After the recording we checked the speed of the drum by means of a tuning fork in the fond hope that it had been moving at the same speed during the recording. Pitch was calculated by measuring the distance between the peaks of the vibrations and at the speed normally used this distance was 0.5-1 mm, not much more than the thickness of the tracing line, so that even if we used a microscope the measurement was rather inexact.

Nevertheless, I found the course useful. Besides palatography I learned to handle the kymograph and to evaluate the curves. Margu r te Durand, who as professor Fouch 's assistant was charged with the practical training of the students, was an excellent phonetician who was capable of getting interesting results in spite of the miserable instruments because she knew their restrictions and was able to put interesting questions. In a study of vowel length she showed that the perception of duration is influenced by pitch. If the pitch varies, the vowel is heard as longer. This was a new approach at that time.

From Fouch  himself I did not learn much. He is known to have made some interesting contributions on the subject of the historical development of the French language, but as a phonetician he had no very great capacity, either theoretically or practically (he taught us - in accordance with his own dialect - to pronounce French *r* as a tongue tip-*r* and to pronounce the nasalized vowels as [ɑŋ] and [oŋ]).

The lectures which impressed me most during my stay in Paris were Emile Benveniste's lectures on Indo-European morphology. They were simply brilliant.

The climate of the Institute of Phonetics was rather rough. If a student missed a class or two, even if it was because of illness, he might be excluded from the course. Fouch  was not very nice to Mlle Durand, and she vented her irritation on us. If somebody was butterfingered (and I was), he was cursed up and down. On the whole, studying in Paris required a certain robustness. I did not have much money, so I stayed in a filthy and beastly cold hotel room facing a dark wall, and the French students were fed up with the many foreigners and did not bother to talk to them. For several months I only knew the Martinet



family and the two small mice which chased each other up and down the curtains when I had gone to bed and which, after a while, produced a young one which ate out of my hand. - Nevertheless, I enjoyed this stay in Paris immensely. I went to the Louvre every weekend and walked for hours along the quays and through the old streets, absorbing all the new impressions with all my senses alert.

Shortly before my stay in Paris I had taken part in the third international congress of phonetic sciences in Ghent. It was very exciting to meet all the people whose works I had read, above all Roman Jakobson, and some whose works I did not know, among them Eberhard Zwirner. He impressed me very much by his extensive knowledge and sharp intelligence, and we agreed that I should come to Berlin to study with him in April 1939 after my planned studies in Paris.

Zwirner had come from neurology to phonetics, realizing that the study of aphasia had to be built on knowledge of the normal language. He was well acquainted with many aspects of the humanities, e.g. the history of science and arts, including the history of linguistics (and he had taken a degree in the philosophy of history besides his degree in medicine). He was less acquainted with modern linguistics. However, following his own way of reasoning, he had come to a concept of the study of sounds which came very close to the approach of the Prague phonologists and of glossematics, i.e. that the phonetic investigation should be based on the restricted number of sound classes in a given language that are used to distinguish meaning. Their phonetic realization should be described by means of variational statistics. In Ghent he and the structuralists found each other, and Hjelmslev published an enthusiastic article about Zwirner's theories and tried to find a post for him in Copenhagen.

Zwirner was not much impressed by the phonetics he found in Germany, particularly represented by Panconcelli-Calzia, and besides the fundamental requirement of basing the investigation on linguistic classes he set up two further requirements, which were in sharp contrast to the Panconcelli-Calzia tradition, namely (1) one should not use instruments hampering the naturalness of speech, but make acoustic analyses based on gramophone records, and (2) one should not use isolated words or small sentences but connected speech. These are ideal requirements, which are not yet fulfilled in modern phonetics, and which in fact cannot always be met. It is not possible to investigate articulation without inconvenience to the speaker, and it is often necessary to start out with small homogeneous sentences in order to isolate the complicated factors involved in speech. But it is very useful to be reminded of the ideal conditions and not to be satisfied with the results obtained from small isolated sentences but to go on to investigate the interplay of all the complex factors in connected speech. And for Zwirner's original aim, the study of aphasic speech and above all its prosodic features, these claims were really compelling.



Zwirner was also a pioneer in the use of new instruments. Inspired by electro-cardiographic recordings he used an ink writer and an intensity meter which was very similar to the one introduced more than twenty years later by Fant, and he made more reliable pitch measurements based on oscillograms taken at high paper speed. The first automatic pitch meter was constructed at the same time by Grützmacher and Lottermoser, but it was so complicated that it filled almost a whole house and required constant nursing by two engineers.

Zwirner's pioneering work did not have the influence it deserved, mainly because he wrote in German and because his books and papers were published shortly before the war and thus for many years were hardly known outside Germany and Scandinavia. And in the meantime new instruments and methods were developed, particularly by American communication engineers for whom it was natural to take account of the linguistic units of communication and to use statistical methods, thus realizing some of Zwirner's main ideas without knowing his work.

Moreover, shortly before the war Zwirner's phonetic research was stopped. He was dismissed from the Kaiser Wilhelm Institute, accused of educating his children in a pacifistic spirit. Then he served as an officer during the whole of World War Two, and after the war he had to earn his living as a medical doctor and did not get a chair of phonetics until 1963; and in the meantime he had concentrated almost all his efforts on establishing an extensive collection of recordings of spoken German dialects.

But several of those who read his early works at the end of the thirties were deeply impressed by them, and I for my part feel that it was from him that I learned phonetic method. Therefore I have mentioned his work in more detail. But our personal contact was rather sporadic during my stay in Berlin because he was soon called up for military service. And since all the instruments were packed and stored, I could only study the curves already taken and discuss them with him when he was on leave. But I wrote my first two phonetic articles on the basis of his curves, and this encouraged me to go on with experimental phonetics.

At the same time I attended lectures by the well known specialist on African languages, Dietrich Westermann, who among other things has described the use of sound symbolism in West African languages, and I also took a course in phonetics with Wethlo, who was quite a character, and who had a small laboratory where, for the first time, I saw a wire recorder. I remember that we recorded the word 'Anna', and when we played it back reversed, the Germans still said 'Anna' but I said 'Hanna'.

Berlin was not a very pleasant place at that time. The classes were constantly interrupted by parades or Hitler's birthday or speeches by Goebbels, and, although most Germans still believed in Hitler's promise not to start a war, the imminent danger was evident to everybody else. - I came home two weeks before the war broke out.



After my return I got a job as teaching assistant in German for professor Hammerich. In 1943 I was appointed lecturer in phonetics, a new post attached to the chair of linguistics. In those years Hjelmslev became the leading Danish linguist. His well known book "Prolegomena to a theory of language" appeared in 1943, and there was a growing interest in glossematics among the young generation of linguists. The glossematic theory was discussed in a number of meetings in the Linguistic Circle and later, in the beginning of the fifties, the glossematic committee of the Circle had a long series of very stimulating meetings in which we discussed with enthusiasm for hours, particularly syntactic theory. Besides Hjelmslev, who was the sovereign leader, the most active discussants at that time were Knud Togeby and Paul Diderichsen; I had long arguments with Hjelmslev about form and substance.

The salary of the lectureship was very moderate, but I could just live on it, and there should have been time for research. But I did not write much during those years except for a number of reviews. One reason was that I had constant sinus trouble, and for more than 10 years I woke up every morning with a splitting headache which was not very conducive to productive work. Another reason was that I was more interested in reading and learning than in writing, and for many years after my exam I continued to hold the opinion, which I shared with a number of my fellow students of that generation, that the worst thing that could happen to me was to get a permanent post with a pension and become settled and bourgeois, so I did not aspire to any chair. I would have liked to do some experimental research, though, but we had no instruments. Moreover, there were more important things to do during the war.

However, Hjelmslev made great efforts to procure some instruments, and he worked out a detailed application to the Ministry of Education. He encouraged the director of the Institute of Speech Pathology to send in the same application to the Ministry of Social Affairs, and they agreed that the one who got the grant should put the instruments at the disposal of the other. The grant was given to the Institute of Speech Pathology, and at the end of the war it became possible to start a university course in practical experimental phonetics at that institute.

In the beginning we only had a kymograph. It was an electrokymograph, running at a constant speed and thus better than the one in Paris, but as a matter of fact it was almost out of date when we acquired it. However, it was quite useful for the students to learn kymography since a large number of the older papers they had to read contained kymographic tracings, and there were quite a number of phonetic properties that could be investigated with a reasonable degree of exactitude by means of the kymograph, e.g. duration, voicing, nasality, and - to a certain extent - pitch.

I enjoyed working with an instrument which did not require any deeper technical insight or technical assistance. When the straw broke, I could simply go out into the fields and pick a



new one, and I have always loved to pick flowers. The rubber membranes posed a more difficult practical problem: I was too shy to enter a rubber shop, so I used to send one of the young students. It turned out later that he had been just as shy, and he had assured the shop keeper that it was not for himself, but for "a machine". We were very innocent at that time.

Later we got a "Frequenzspektrometer", permitting a frequency analysis of sustained vowels with an accuracy of 1/3 octave, and in 1951 we got our first tape recorder.

The fact that the instruments were situated at the Institute of Speech Pathology in Hellerup brought us in contact with Svend Smith, who had just finished his pioneering thesis on the Danish stød. He was in charge of the laboratory on behalf of the Institute, and neither the students nor I would have liked to miss the amusing memories of our small squabbles. We only had access to the laboratory in the evenings, and at the start we had no key to the door. When the door was locked at 10 P.M., we had generally just got the kymograph working, so when we left around midnight we had to help each other out of a window in the basement leading out into a flowerbed of roses. I suppose the gardener complained, so we got a key.

After the war I received a scholarship from the World Federation of University Women for a year's studies abroad. Hjelmslev advised me to go to London because he found that I needed practical phonetic training badly, in which he was certainly right. So I first went to London, and I enjoyed this stay very much. At University College I attended Daniel Jones's lectures and took courses in English and French phonetics. It was characteristic of the London School that the whole emphasis was on practical phonetics. The important thing was to be able to "make noises". - I can still hear Mlle Coustenoble saying "Encore! Encore! Encore!" until we could pronounce the word exactly as she did. She was known for being able to make strong and sturdy men break down in tears, but she was very efficient. And we always had dear old "Parky", Jones's lovable secretary, to comfort us. Daniel Jones was a quiet, reserved, polite and frail old English gentleman with a weak voice (which is known all over the world from his gramophone records of cardinal vowels). It was a great favour to obtain an interview of a few minutes, and I think it was only due to Hjelmslev's recommendation that he gave me private lessons in clicks. At the School of Oriental Studies I took courses in Yoruba and Chinese phonetics and attended lectures by J. R. Firth, the founder of the prosodic school. He was very different from Jones, a big, noisy man who laughed a great deal at his own jokes; but his lectures were interesting. I spent most evenings mending my only pair of stockings; it was just after the war, and everything was rationed.

From England I went to Holland. There was not much going on in phonetics in Holland at that time, but I gathered material for a book on the accent of compound words in the Germanic languages, and particularly in Danish, inspired by a theory of



Hammerich's concerning the influence of Dutch on the accent of Danish compounds which I did not find quite convincing. Later I put the material aside, mainly because Hjelmslev did not find the topic suitable for a phonetic thesis. In Holland I moved from one city to another, according to where they had sufficient fuel to heat the University library. It was a very cold winter; but spring was beautiful with lots of tulips and fruit trees in blossom. I also enjoyed the art galleries, the beautiful architecture of the towns and the canals where I skated during the winter and went canoeing in the spring.

In the years immediately following the war, acoustical phonetics made rapid progress, mainly due to the technical development which had taken place during the war. The most important new instrument was the sonagraph which made it possible to make a frequency analysis of  $2\frac{1}{2}$  seconds of connected speech in five minutes, whereas a few years earlier it could take several hours to analyse a single vowel. But we had no possibility of getting a sonagraph in Copenhagen. In 1952 I received a Rockefeller scholarship for studies in America. I had been looking forward to a six weeks' stay in Cambridge, but unfortunately I was ill most of the time. However, I managed to make a number of spectrograms of Danish words which I could use later for teaching purposes and for research. I was also glad to be able to meet Roman Jakobson, who was very kind and hospitable, and whom I have admired since I was a young student and still admire for his enormous knowledge, his incredibly fertile brain, his temperament, and his personal charm.

One of the last days I gave a talk at MIT. Among the audience I noticed an unusually intelligent face - it was a young research assistant named Chomsky.

From Cambridge I went to New York and worked for a few weeks in the Haskins Laboratories. It was a new institution at that time, comprising a relatively small group of people: Frank Cooper, Alvin Liberman, Pierre Delattre, and a few more. Their teamwork was excellent, and the atmosphere was very stimulating and very pleasant. They had just constructed the first really usable speech synthesizer, the so-called pattern play back, which was of a relatively simple construction. Stylized spectrograms were painted with white paint on cellophane and converted into sound by means of a tone wheel. I learned the technique and took part in some of the tests. In the late evening when the machine was not occupied I painted the words 'Københavns Universitet' and 'Fonetik', which I used for several demonstrations later. It was a very hot summer, and beads of perspiration dribbled down from my face and got mixed up with the paint, which may have added a special accent to the words.

I was very tired when I went from New York to Oklahoma, where Kenneth Pike had his big Linguistic School for missionaries, so I was not very enthusiastic about the very hard schedule he had set up for my visit, starting early in the mornings at 8 A.M. I simply refused to do anything before 9 o'clock. I had



read and reviewed various of Pike's works and was very much impressed by them, especially by his excellent book on tone languages, and I was eager to see how the teaching was organized. The day started with prayer, in the hope that God would be interested in the teaching of implosive *b* and voiceless *ʔ*, and it ended in the evening with discussions, e.g. about the structural parallelism between the Trinity and a phoneme with three variants. In between it was very competent linguistic research and teaching. For a heathen sceptic, who believed neither in God nor in the tagmeme, and who had sometimes asked herself whether her work was really of any use to anybody, it was a peculiar and at the same time very positive experience to meet people who believed in both and who were convinced that linguistics was not only useful on this earth but that it could help - through Bible translations - to bring souls to eternal salvation. This extra motivation made the students study with an unusual enthusiasm and assiduity.

You may wonder that I could travel so much. But at that time there was no exam in phonetics in Copenhagen, and no fixed schedule, and the students only came for fun, so it was possible to skip a term.

After my return I had six years with good possibilities for research: still no exams, good health, and support from the Science Foundation.

Inspired by Roman Jakobson I had become very much interested in the auditory dimensions of sounds, including their symbolic value and a possible parallelism to colours, and I started a large number of tests on these problems. However, it turned out that the available statistical methods were not yet very satisfactory, so I put it aside and only used some of it in a short paper for a Roman Jakobson Festschrift. - Instead I took up a more traditional subject, viz. an investigation of Danish stop consonants with special reference to the fortis-lenis problem. For this purpose I needed more spectrograms, and in January 1954 I went to Stockholm for a couple of weeks. This was the start of a close cooperation with Gunnar Fant, who was on his way to becoming an international authority on acoustic phonetics. I remember particularly this first stay in Stockholm. Since the sonagraph was used all day, I had to work at night. I slept in the morning and went skiing in the afternoon and had a very pleasant time.

In this same period I made an extensive perceptual test based on cutting and splicing of tapes in order to throw some light on the importance of formant transitions and explosion noise for the identification of Danish stops. This was before the time of the electrical segmentator, so it had to be done by hand. Since the laboratory in Hellerup was not accessible on weekdays, I had to do the final splicing during the Easter vacation, working day and night and living on canned food with three students helping me in shifts. At one time I had 4000 pieces of numbered tape hanging all over the place, and I was pleasurably surprised when everything had been spliced together



and only one piece was running backwards. That night I found that the Science Foundation had not bestowed its money in vain.

Together with Oluf Thorsen I also undertook an investigation of intraoral pressure in obstruents. We used a manometer constructed for medical purposes (a new possibility in 1955) and were permitted to work in the University Hospital and later in the Hospital in Gentofte. We used oscillographic recording and, since the laboratory had to be ready for use next morning, the films had to be developed and dried the same night. In Gentofte they did not have any machine for drying the film, so I got the idea that I could stick them up on the tiled walls of the bathroom (that was the way we dried and ironed our handkerchiefs at college). The result was disastrous: the films stuck to the tiles and had to be torn off in small pieces. When, at three o'clock in the morning, I had finished this sad task and cleaned the walls, all the doors were locked and I had to climb out of a window in the basement. I was very afraid of being taken for a patient escaping from the psychiatric ward.

I managed to write 100 pages of the planned book on stops. Then I was thrown from a horse and got concussion, the effects of which lasted for a long time. During the following five years I could only work for short periods. I could lecture on topics I knew, but for long periods I could not read. When I recovered I got so much administrative work that I gave up finishing the book, but most of the material has later been used in papers.

The sixties were a period of expansion. From 1944 I had given regular courses in elementary phonetics for students of modern languages. I started out with three participants, but the number has grown slowly but constantly, and at the end of the fifties it was around 60-80 each autumn. In the sixties this course was included in the list of recommended courses for the various languages, and at the same time the number of students increased rapidly, so in the beginning of the seventies about 700 took part in the course. They had to be divided up into small groups, and more teachers were needed. From the end of the fifties some courses were given by teaching assistants, but in 1963 a fixed post was established for Jørgen Rischel, and during the following years 5 more posts (lectureships with tenure) were obtained. In 1966 I got a chair of phonetics (during my illness I had in fact become interested in a job with a pension). Since 1960 it had been possible to take a diploma in phonetics, and later this was recognized as a subsidiary subject for the MA. An MA in phonetics as a main subject was established in 1968. During this period our possibilities for instrumental research were also improved. In 1953 we had received a sonograph from the Rockefeller Foundation, and from 1960 we could place a few other instruments in a small room in town in the Institute of Linguistics and Phonetics. The room could not be heated, however, and it needed repair badly. But in 1966 we got a real laboratory: 4 small rooms in the basement (which, for a change, were overheated), and at the same time a separate institute of phonetics was established and we got a technician and a secretary, and in



the following years - in connection with a rapid expansion of our equipment - two engineers. In 1971 we got better rooms for the laboratory and in 1975, as everybody knows, we moved to Amager to premises which had been built specifically for our purposes.

This has been a very rapid development compared to the extremely slow improvements during the first 18 years. The change in conditions for recording can perhaps be used to symbolize the whole development: until 1966 the speaker had to put his head and the microphone into an old sheepskin coat serving as a "soundtreated room". In 1966 staff members built a (remarkably good) room out of old bicycle stands covered with Rockwool and curtains and, finally, now we have a super-modern anechoic room with suspended wire-mesh floor and sound-damping wedges.

The rapid expansion was only possible due to the general economic growth during the sixties. But another necessary condition was that capable young people were ready to take over the new posts and willing to do a hard job helping to build up the institute.

The fact that we now had an institute and a laboratory involved a new way of life. I had been used to working at home, except when I had to make recordings, and I must confess that I still prefer my writing table at home in quiet surroundings when I have to do serious work. But administration and meetings often required my presence at the institute, and I also realized that the rapid development of phonetic techniques and the abundance of new literature made teamwork a necessity in many cases. I have enjoyed discussing with my younger colleagues and learning from their expert knowledge within particular fields. And, on the whole, I think that we have managed to build up together an institute with a good climate, which functions well, and which stood the test when we arranged the Ninth International Congress of Phonetic Sciences in 1979. That was a job which made heavy demands both on our time and on our capacity for cooperation. But I think it was worth while.

Soon after its start the institute began to develop connections with institutions and scholars abroad. - Phonetics has always been a field with close international cooperation. When I started in phonetics there were very few phoneticians, in some countries only one or two, and most of us knew each other and read each other's publications (and so we all became world-famous in a very simple way), and many of us travelled quite a lot. Besides the journeys already mentioned I recall with pleasure a three-months' stay in India and a two months' stay in Japan as very exciting experiences. Now that we had an institute the personal relations could be supplemented by cooperation and exchange of reports with other institutes of phonetics. We have had especially close and enjoyable relations with the Institute of Phonetics in Lund and particularly with Kerstin Hadding and Eva Gårding and, on the whole, with our Swedish colleagues, and we have been able to invite foreign



scholars to stay with us for a longer time, e.g. John Ohala from Berkeley and Hirose and Niimi from the Tokyo group, who helped us with our EMG-project. This has prevented us from being too provincial.

Looking back at these 50 years the most striking fact is the fantastic development of phonetics as a discipline. It can hardly be said to be the same subject as the one I became acquainted with for the first time in 1929. At that time the dominating trend was classical phonetics, as represented by Otto Jespersen, i.e. the description of speech sounds in terms of their production by the peripheral speech organs, based on auditory identification, kinesthetic impressions, and perhaps a mirror for control. Experimental phonetics existed, and had existed for some decades and it had obtained valuable results, but its methods were still restricted and classical phonetics, which dominated the University teaching completely, at least in Copenhagen, did not take much notice of it.

The beginning of the period - the thirties - was characterized by the appearance of phonology, i.e. the development of a theoretical framework for the description of the functions of speech sounds. Then - in the forties - technical progress made it possible to undertake an acoustic analysis of speech sounds within a reasonable time. And for a while acoustic phonetics was quite dominant. The acoustic aspect is perhaps not so interesting in itself, but it is a necessary link between sound production and sound perception, and the filling in of this missing link made it possible to proceed to an investigation of the whole speech chain.

The relation between speech production and the acoustic result has been investigated intensively since the fifties, e.g. by Gunnar Fant and K. N. Stevens. And as for the connection between the acoustic stimuli and perception, the main contributions came from the Haskins group. - For a while there was less interest in aerodynamic studies and in the function of the larynx, but new technical possibilities soon revived the interest in these fields. Finally, in the last few years, there has been a growing interest in neurophonetics, including the problem of hemispheric dominance. Our knowledge in this field is still restricted, but it is growing, and we approach a situation where we can add this last link to the speech communication chain. This also means that, whereas fifty years ago the phonetic description of individual languages was the main concern of phonetics, the interest has now shifted to general phonetics (although there is still much to be done in the former field). One now tries to set up models for both speech production and speech perception, and modern computer technique offers new possibilities for testing the models. On the whole, I find that phonetics has become much more interesting and has much wider perspectives than 50 years ago. It has been exciting to follow this development.



If at last I should try to look back briefly on my own research during these years, my dominating impression is that it looks very much like a village shop with smoked hams, clogs, and salt herrings. My relatively numerous papers and my few books deal with the most varied subjects: phonology, articulatory, acoustic, and perceptual phonetics, sound history, and particularities of Danish, German, French, Dutch, and Gujarati phonetics. It does not look as if there has been any plan - and in fact, there hasn't. In the first place I generally do not plan much in advance and, by the way, in a quickly developing discipline long-term planning is not too easy. Moreover, for many years the lack of appropriate instruments set narrow limits for the realization of the projects I was interested in. Finally, my research was often directed by the requirements of the courses taught. For almost twenty years I was the only teacher of general phonetics, except for the elementary courses given by assistants from the end of the fifties. And I have always felt that teaching a rapidly developing subject to a small selected group of intelligent and really interested students should not consist primarily in information about facts but rather in an introduction to scientific method and in discussions of new approaches and unsolved problems. I therefore felt obliged to keep up to date as best I could with the different areas of phonetics, and there has always been a close relation between my research and my teaching. On the other hand, it also happened that I had to interrupt a project because preparation for classes took too much time.

There are, however, a few subjects to which I have returned repeatedly because I could not solve them, e.g. close and open contact and the fortis-lenis difference (I still do not know the precise difference between French *p* and Danish *b*, and that irritates me). - And I have returned to other subjects because they appealed to me, e.g. perceptual phonetics. There has also been an obvious development in my interests from phonology to experimental phonetics. It is true that I have also written about phonology in later years, but this has mainly been because of the requirements of teaching. The growing interest in experimental phonetics may be partly due to a general tendency for old people to get tired of very abstract theories. I have seen too many theories replace each other in the course of time, and each time the adherents think that this is the only adequate approach and that everything that has been said before is nonsense, although it may be nothing but a new fashion, a complementary way of looking at the same facts. The models set up in experimental phonetics are of a different kind. They are hypotheses that can be verified or falsified by later research. There is still much we do not know, new facts, not only new points of view, that can be discovered. But I must confess that my contributions to these discoveries have not been very conspicuous. The results of my research remind me of a village shop also in the sense that the shop is not characterized by new elegant models from Paris but by old homespun cloth, sometimes with a small new detail in the pattern but maybe so small that it could also be an error in the weaving.



My predilection for experimental phonetics may also partly be due to the fact that I am predominantly a visual type - and I like to look at curves. Finally, it is more exciting and dramatic because it often happens that the machine breaks down or you have forgotten to turn a knob, and everything has to be started over again, - I enjoy surmounting difficulties.

I am glad that I have been offered a small office in the institute so that I can continue this exciting job for a few years, although I will miss the inspiration which I have always found in teaching gifted students.

I want to thank my former students and my colleagues for their loyalty, help and inspiration, and for the many merry hours we have spent together. And finally, I want to thank the University of Copenhagen, to which I have been attached as a student and a teacher for more than 50 years, for giving me continually improved working conditions, for having made it possible to build up this institute, and for having preserved the chair of phonetics under difficult circumstances.

May I finally express the wish that the future working conditions for University teachers and students will be better than we fear at the moment.