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My interest in historical linguistics arose relatively late. During most of my school years I was convinced that I was going to become a mathematician. I liked the subject and my grandfather, Guido Castelnuovo, whom I much admired, was a Professor of Mathematics. Only in the last year or so of school did I decide that I was not good enough and I would do better to turn to a literary subject. I went to the University of Rome as a matter of course since I was living in Rome and I decided to study 'lettere antiche', a broad

course which included a great deal of Greek and Latin. In my penultimate year of school a good teacher, Mario Bonardi, who tragically died soon afterwards, had made classics alive for me; Greek was no longer epitomized by the irregular verbs which I had been relentlessly taught, but by the poems of Sappho and Alcaeus. It is only much later that the verbs became as important.

I had no intention of becoming a linguist – I was more interested in history of religion or, failing that, in straight classics. Yet I remember going out of curiosity to a lecture on Sanskrit because I had heard that Sanskrit was the mother of all languages. In his first sentence the Professor said that this was an old and wrong belief and I walked out. But I had to follow a course with the unexciting title of ‘Grammatica greca e latina’, which turned out to be an introduction to the recently deciphered Linear B script of Ancient Greece. Carlo Gallavotti was perhaps the first to lecture on the subject in Italy, was enormously excited and succeeded in exciting some of his students, me included – the most ancient form of Greek written at least five centuries before Homer; a newly deciphered script, a series of second-millennium-BC texts which had never been interpreted, a field in which everything had to be done and even an undergraduate could contribute something. It was irresistible. I felt that to work on Linear B required omniscience: history, archaeology, technology, economics were all necessary to interpret the texts, but at that point in time the correct interpretation depended above all on a correct linguistic analysis. The script rendered Greek only imperfectly. It was necessary to have a clear view of how the Greek language had developed and to know what was possible at the relevant period (1400–1200 BC). Linguists had reconstructed the Indo-European parent language and attributed it to a much earlier period than our first documents; Greek was known from c. 800 or 700 BC. The Mycenaean language was somewhere in the middle but one needed the correct methodology to find out what the possibilities were. I was now pushed in a certain direction and I returned to the Sanskrit course, this time knowing why I was doing it. There followed a number of related courses and while Linear B provided the immediate focus, historical and comparative linguistics became my main interest. I shall always be grateful for the very flexible university system which allowed me to choose subjects which originally I had not intended to specialize in. One of the things I remember from those years is the surprising discovery that I could not any longer be bored – not even when waiting for a bus or listening to a bad lecture. There was always a difficult Linear B word whose meaning needed to be puzzled out. I also remember the mistakes due to self-instruction. I spent ages reading a late Byzantine compilation, the *Etymologicum Magnum*, in the mistaken persuasion that I could use it as a modern etymological dictionary – nobody had pointed me in the direction of the real etymological dictionaries. Similarly I read more than once the whole of Homer to find the attestations of a particular word: I did not know that indexes and concordances existed.

In practice I was given a very traditional training à la neogrammarian (though the word neogrammarian was practically an insult in my faculty): sound change, morphological change, little or no mention of syntax; I tried to learn some more ancient languages. I painfully struggled with German in order to read secondary literature but the basic textbooks I used were in French: Meillet (1937) for Indo-European, Lejeune (1955), Chantraine (1947) and Humbert (1954) for Greek, Niedermann (1953), Ernout (1953) and Ernout & Thomas (1953) for Latin. As for theory, I was never clear what if anything I was taught in the official courses. There was a great deal of *Crocianesimo* in the air but it did not particularly impinge on what I was doing (I heard little or nothing about Vossler); there were some survivals of *neolinguistica* but in Rome this was not much mentioned. I heard some interesting general lectures by Antonino Pagliaro, the Professor of *Glottologia*, but I often found them obscure and I was incapable of seeing their relevance to what I wanted to do. Things changed in the last part of my undergraduate course and in the two years which followed when I became an assistant to Professor Gallavotti. Tullio de Mauro, a somewhat older contemporary, told me to read Saussure's *Cours* and then patiently explained to me its importance; later he introduced me to European and American structuralism. I remember some early morning sessions when he and I and one or two others read together the French translation of Troubetzkoy's *Grundzüge* (1957) and with even greater excitement Zellig Harris's *Structural Linguistics* (1960). It was a new world – one of which we did not speak to our teachers because we felt, rightly or wrongly, that they would have been indifferent or hostile – and also because there was not much communication between the great professors and the humble students or assistants. We had, however, the constant help of Mario Lucidi, a senior assistant of Pagliaro, almost blind, who had one of the sharpest and most original minds that I have ever encountered. He and de Mauro persuaded me that even the most factual statements presupposed a series of theoretical assumptions. Lucidi's death in 1961 when he was 47 was shattering. By that time I had taken my first degree, with an undergraduate dissertation on the morphology of Mycenaean, had had my first experiences of teaching as Gallavotti's *assistente straordinaria* (there were no graduate courses), had written two or three articles on Mycenaean and on classical philology, and had practically completed a Mycenaean lexicon, which was eventually published in 1963. Thanks to Mycenaean I had also met two scholars whom I greatly admired, John Chadwick and Michel Lejeune. But my interests had partially shifted. I was still fascinated by the Greek language, I still saw myself as a historical linguist interested in *Laut- und Formenlehre*, but now I wanted to use for Ancient Greek the rudimental structuralistic principles I had learned; above all I wanted to study a Greek dialect combining a synchronic approach with a history of its development (cf. Morpurgo Davies 1960). In 1961 and in Italy I was blissfully unaware of the

fact that in the States linguistics was moving on; I felt modern because I simply wanted to introduce the concepts of synchrony and diachrony, of phoneme and morpheme, into Greek historical grammar.

For 1961/2 I was granted a Junior Fellowship at the Center for Hellenic Studies founded by Harvard in Washington, DC. It was a purely classical institution and 1961/2 was its first year, but I was allowed and even encouraged to follow my linguistic interests. I took a trip to Philadelphia because I had a letter of introduction to Zellig Harris, one of my heroes. He was very kind to me but pointed out that, given my interests, the person that I should meet was his colleague, Henry Hoenigswald. Hoenigswald's *Language Change and Linguistic Reconstruction* had appeared in 1960, but I did not know about it. Even a brief meeting made me realize that serious work in structural historical linguistics was going on. At the LSA convention in Chicago I first came across Chomsky's name. *Syntactic Structures* (1957) was a hot topic for young linguists, but I did not understand how important it would become and a few years passed before I read it. I found it more exciting to see the great names of structuralism: Bloomfield and Sapir were dead, but Hill and Joos were there and I had met Harris. In Mycenaean studies I felt at home, but that year made me realize how ignorant I was in anything linguistic and that linguistics really existed as a self-standing discipline. I could not follow most theoretical discussions and even in Indo-European studies I had immense gaps. Hoenigswald gave me a copy of the preliminary version of *Evidence for Laryngeals* (Winter 1960). I read it but I had not been taught about laryngeals and understood little or nothing. A week spent at Harvard did not increase my linguistic knowledge (I just met Whatmough briefly and Watkins was away) but gave me my first experience of a really good library; to see Widener was a revelation. The other revelation concerned academic life: it did not need to be as hierarchical as it was in Italy.

One year later I was married to J. K. Davies, an Oxford graduate student in ancient history met at the Center for Hellenic Studies, and I was in Oxford struggling to get a job, any sort of academic job, doing some teaching and translations for a living, coming to terms with the English tongue and with the coldest winter for some 60 years. Here too the linguistic scene was different. In Italy *glottologia*, which at the time often meant Indo-European studies, was present in all universities and compulsory for all undergraduates in literary subjects; almost all of them of course had gone through a *liceo classico* and had learned or pretended to learn some Greek and Latin. Theoretical linguistics, if it was not taught under the aegis of *glottologia* (and mostly it was not) or perhaps more frequently under that of Romance philology, did not exist at all. I did not belong to any linguistic society or circle because in Rome there was none. In the States there was Indo-European and/or historical linguistics in a few universities, but linguistics was present in most large universities and even some smaller ones. The

LSA's convention had seemed to me the sign of an established discipline. In Britain there was a Chair of Comparative Philology, which roughly meant Indo-European with a strong Greek and Latin bias, in three universities (Cambridge, Oxford and London), in each case tied to classics. There was a tradition of work in phonetics, but only a few universities had institutionalized linguistics. I was advised by Giulio Lepschy, who had come to England shortly before me, to join the old and respected Philological Society (founded in 1842), which mainly heard papers on historical and philological subjects but also some theoretical papers; Giulio also mentioned that there was a small new society, the LAGB (founded in 1959), which one could join. I did so and was left with very different impressions from the first meetings I attended. At the Philological Society I heard a paper about Old and Middle Iranian by Dr Gershevitch, very scholarly and traditional in its approach. I did not understand all of it because I was too ignorant of Iranian but I had no difficulties in following the type of argumentation. The LAGB meetings discussed theory and description rather than history and comparison. I remember an almost obsessive period when everyone tried to make descriptive sense of e.e. cummings: 'Anyone lived in a pretty how town' or 'he sang his didn't he danced his did'. In spite of my interests, my conversion to structuralism, etc., I remained substantially ignorant and felt out of place in many theoretical discussions. I persevered with the Philological Society partly because I made friends, partly because the papers given were closer to my (historical) interests, partly because its meetings were less expensive in time and money.

My other links outside Oxford were in the world of Mycenologists. There was a regular Mycenaean seminar in the Institute of Classical Studies, which I often attended and which I was invited to address; there was John Chadwick in Cambridge, who, after the death of Michael Ventris, stood for Mycenaean studies everywhere and had welcomed me with open arms.

In Oxford there was a very good tradition of work in historical linguistics – above all in the English school. I just met J. R. R. Tolkien and C. L. Wrenn, but Alistair Campbell, Norman Davis, and Eric Dobson were visible and very prominent, and so was the younger Bruce Mitchell. T. B. Reid, Professor of the Romance Languages, was a medievalist but also a historical linguist with general interests and a very sharp mind; T. Burrow, the professor of Sanskrit, was a distinguished Indo-Europeanist and a Dravidologist. Two successive Professors of Comparative Slavonic Philology (Boris Unbegaun and Robert Auty) contributed, together with Idris Foster (Celtic) and Charles Dowsett (Armenian), to the general pre-eminence of historical linguistics and philology. The Professor of Comparative Philology, Leonard Palmer, was a brilliant, if occasionally perverse, historical and comparative linguist who had been trained in Vienna with Kretschmer, had worked on post-*koiné* Greek, and had written not only a standard textbook on the external and internal history of Latin but also an

introduction to linguistics, published in the 1930s, which introduced into historical linguistics a number of structuralist concepts à la Prague School; at the time he was working mostly on Mycenaean and above all on Mycenaean archaeology. There were no general courses on linguistics nor were there general undergraduate or postgraduate lectures in historical linguistics. Indeed there was no common forum where these experts could meet. Most undergraduate schools in language-based subjects had very austere and technical philological options (often on the history of the language or historical grammar, as well as on palaeography, textual criticism, etc.); principles of Indo-European reconstruction were taught as a minority option within classics and were somehow absorbed, though not explicitly taught, within English. For postgraduates, at a time when the doctorate was still not compulsory (though it was beginning to be so), there was the so-called Diploma in Comparative Philology – a two-year postgraduate course founded in 1928 on the initiative of the then Professor of Comparative Philology, Gustav Brauholtz; C. E. Bazell and Angus McIntosh had been among the first students. In addition to more technical Indo-European papers, the Diploma also required an examination on ‘the principles and history of Comparative Philology with special reference to the Indo-European languages’. The candidates (one or two a year as a maximum, though frequently there were years with no candidates) had a very few individual tutorials on the subject from Palmer.<sup>1</sup> The Committee for Comparative Philology, which administered the Diploma, was indeed the one body where some at least of the philologists, who belonged to different faculties, came across each other. The committee’s meetings were formal, at stated times, with the proctors present, but were not frequent. The Secretary was the Professor of Comparative Philology, who kept handwritten minutes.

With hindsight it is clear that the philological talent available in the university at the time was impressive, but it was not co-ordinated and a number of opportunities were lost. Things began to change in 1964 when an ex-Diploma student, C. J. E. Ball, came back from SOAS, where he had been a Lecturer in Comparative Linguistics for three years, to take up a Fellowship in English at Lincoln College. That was also the year when, after two years in which I had done odd bits of lecturing for Palmer and the Committee for Comparative Philology, at the normal rate of £24 per term, I was suddenly offered a University Lectureship in Classical Philology. The job, I believe, had become available in the period of university expansion two or three years earlier and had been advertised, but the Selection Committee had not agreed on the appointment. On my arrival I was told that such a position existed, but clearly I was too much of an unknown quantity. After two years, prompted by Palmer’s request for a year’s sabbatical leave, a decision was taken – I do not know by whom or how. I was simply asked to submit a curriculum to Palmer: no advertisement, no proper application, no references. There followed a letter informing me that I had been appointed: no

interview was required. My task was to teach first and second year classicists the special subject in comparative philology and to help the Professor to look after the rare graduates who took the Diploma. I had done most of it on a temporary basis but I still had problems with English and the responsibility was frightening – though I discovered that my undergraduate lectures became much better when I acquired enough self-confidence. Even more frightening was to become Temporary Secretary of the Committee for Comparative Philology during Palmer's leave. I had never sat on a committee in my life and never written minutes; my English was clearly inadequate. I eventually resolved never to use a sentence which had not been used before in a similar context. I succeeded and by the end of the year I had almost learned by heart all the previous minutes, a useful training in bureaucratic English.

Chris Ball's arrival from SOAS, where he had been the colleague of Robins and Bazell and had been breathing a very Firthian atmosphere, brought linguistics to Oxford. He founded the Oxford Linguistic Circle, of which he became the Secretary and I the Treasurer. We had papers of a more general nature by invited speakers (paying their expenses was a problem) and also had a small group which met to read more advanced monographs. Most of the philology professors joined (Palmer gave us some money to help us along) and others took part: Jonathan Cohen, the philosopher, Freddie Beeston, the Arabist, Roy Harris, who taught romance linguistics, the much younger John Marshall, and then Geoffrey Sampson, who had a Research Fellowship. Initially the excitement was great and we even attracted people from outside the university: Mr Eckersley, who had just founded the Eckersley School of English, was one of them. At some stage Ian Mulder, who was the librarian of the Oriental Institute, a devoted follower of Martinet, became a prevailing voice and tried to instruct us all in a form of functional linguistics. Meanwhile I was teaching the basic historical grammar of Greek and Latin, Greek dialects, etc., to students who probably knew more Greek and Latin than I did but to whom 'philology', i.e. historical and comparative grammar, was entirely new. I was offered a Supernumerary Fellowship at St Hilda, and that carried a new task (I became College Lecturer in Classics) but also some benefits; to my surprise I found college life quite congenial and very much liked St Hilda's.

I was working hard but there was time to learn and take part in other activities – there was little or no administration. I learned Old English and some Old Norse from Celia Sisam, a fellow of St Hilda's; Oliver Gurney, then Shillito Reader in Assyriology, taught me Hittite, and I took part in a seminar started by Palmer about Hieroglyphic Luwian; we did not get very far until David Hawkins joined us from SOAS in 1965. Obviously I kept up with classics. A strong influence was that of Eduard Fraenkel, who had retired from the Corpus Chair of Latin in 1953 but kept his seminar going and effectively taught us how a real classicist reads a text. I was bowled over by his learning and his intelligence but also by his deep earnestness, his sense

of tradition and his devotion to scholarship. At the same time I came to know and admire Arnaldo Momigliano, the ancient historian, who was based in London but came frequently to Oxford. Fraenkel and Momigliano were very different (not only in age), but both combined seriousness and immense warmth; it was impossible not to respond. I felt I was growing up: previously I had frequently yielded to the hero-worship typical of non-British students, but for Fraenkel and Momigliano I managed to feel deep devotion and affection while retaining some at least of my critical faculties.

In 1970 Palmer announced that he would retire in September 1971; the Chair was readvertised and I waited with some anxiety to see who the new Professor would be. To my surprise I was offered the Chair, though I had not applied; I accepted it, still not understanding what had happened. Hugh Lloyd Jones, Regius Professor of Greek, had hinted that that was a possibility, but I had not taken him seriously. Much earlier, I had accepted an invitation to teach for the first semester of 1971/2 at the University of Pennsylvania, replacing Hoenigswald; I could not back out, and I went. The experience was exhilarating: a real department, some very good students, including Ivan Sag and George Dunkel, frequent contacts with Hoenigswald who was on leave and ready to discuss everything, further contacts with Indo-Europeanists at Harvard (Calvert Watkins) and Yale (Warren Cowgill, Stanley Insler). I felt happy in the States and felt that I had friends there. When I came back things were different. I had to change college. The Chair was attached to Worcester, which was a men-only college; the rule was that women's colleges would take it in turn to have the rare women Professors: Helen Gardner, Merton Professor of English, had gone to Lady Margaret Hall (also from St Hilda's) and I, who came second, went to Somerville, which was somewhat shaken by the amount of book space I needed. I found myself doing the job of two people in two terms instead of three – and this at a time when we had doubled the syllabus because we promised to offer classes for all four years of the classical undergraduate course. It was a very hard year, but after the appointment of John Penney to my previous Lectureship life became much easier.

The 30 years between 1971 and 2001 disappear in a blur; I find it difficult to know retrospectively how much of what happened was planned and how much was due to chance, though I suspect that the latter played a considerable role. Time passed very fast, though a constant feature was the brightness of undergraduates; now they know far less in the way of Greek and Latin than in the 1960s but the ability of some of them is still impressive in the extreme.

Institutionally I felt, I do not know how consciously, that something needed to be done on two fronts. First, subjects like historical linguistics or comparative philology or Indo-European were at risk, since the detailed knowledge of languages, and particularly ancient languages, which they required was disappearing. In the previous century Berthold Delbrück



(1875: 3f) had given as example of how useful and exciting Indo-European studies could be the fact that they allowed teachers to explain to their students why a word like Greek *menos* was a neuter though it ended in *-s*. In the second part of the twentieth century this argument did not seem very appealing, but it still seemed possible to show to classicists and others the importance of a method which identified genetically related languages and contrasted them with languages joined by chance similarities. The same method could lead to the reconstruction of the earlier stages of an ancient language and lengthen its history. Clearly the comparative and historical study of related languages has a great deal to offer, but labour-intensive teaching has now to be done at all levels, undergraduate and graduate. So much was always clear, but I had moments of worry. My colleague John Penney, who has a far more balanced attitude than I do, calmed me down: 'This is a good subject; if they forget it they will have to reinvent it.' I found, and find, the thought singularly comforting.

Second, it seemed clear to me, after my American experiences, that it was time that linguistics got established in British universities in general and in Oxford in particular, and that comparative philology could not dispense with linguistics. Yet that required in each university a group of academics prepared to act as a pressure group. In Oxford a few enthusiastic dons worked far harder than anyone could have expected them to:<sup>2</sup> linguistics has had its ups and downs but now exists as a relatively independent discipline with a few posts reserved for it (previously there were none), a phonetic lab and a graduate centre. Any creation due to pressure from lower down rather than diktat from higher up necessarily leads to less consistent structures, but the gain is a wider range of activities and a broader outlook than would have been possible otherwise. Serendipity plays a role: it is our good and unexpected luck that currently the Khalid bin Abdullah Al Saud Professor for the Study of the Modern Arabic World, Clive Holes, is a distinguished sociolinguist. Other faculties selected linguists for their posts: witness the English Chairs held by Jean Aitchison and Suzanne Romaine. I have now shared a number of administrative tasks with three successive Professors of General Linguistics (Roy Harris, James Higginbotham, Stephen Pulman), and we have seen that the system may work.

My own work developed along different lines. A Professorship involves graduate supervision, and I was exceedingly lucky in the first four students I supervised: John Penney, Kim McCone, Elizabeth Tucker and Katrina (Mickey) Hayward. They required little or no help but worked in different fields: Indo-European, Celtic, Greek morphology, Greek dialectology – I felt very stretched but learned a great deal.<sup>3</sup> Later on I was again out of my depth when David Langslow, who also became a colleague, started working on technical languages and Latin medical terminology; I was more confident when I followed Ivo Zucha's study of Hittite word formation.

Ancient Greek was my main interest and still is. In the history of Greek or

indeed of any language, I never cease to be amazed and fascinated by the interaction between sociolinguistic and/or historical development and the repercussion on the language's structure, the contrast between systemic changes and arbitrary new developments. As an undergraduate my main ambition was to introduce or correct one or two points of detail in the great edifice of Greek historical grammar, to be responsible for the addition of one or two footnotes in one of the great *Handbücher*. I still feel exactly the same way, though I now know that this panders to a concept of progress which most people would label antiquated and positivistic. On the other hand the old view that in every subject there are a number of problems to be solved, of fields to explore, is unlikely to disappear. I am immensely impressed by the work done on Greek by my graduate students; some of it ought to rate more than a footnote. Problems of morphology have been studied by Elizabeth Tucker (contract verbs), Torsten Meißner (*s*-stems), Jason Zerdin (*skelo*-verbs) and in a different framework Philomen Probert (accentuation); those of linguistic variety by Katrina Hayward (verse inscriptions), Rudolf Wachter (vase inscriptions), Stephen Colvin and Andreas Willi (both Aristophanes); those of morphosyntax, syntax, semantics and pragmatics by Eleanor Dickey (forms of address), Pietro Bortone (prepositions), Maria Karali (word order) and again Jason Zerdin. The extraordinary thing is how they all start with the feeling that the secondary literature is overpowering and everything has been done before. Yet by the time they write their conclusions they all state – and believe – that an enormous amount remains to be done and that the most elementary questions have not been asked or answered. I am much older, but every time I embark on a new project I go through the same mental reactions. However, I do envy the recent graduates; they have received that formal training in linguistic theory which I never had and are capable of using in their historical work what they learned.

Mycenaean work brought me to the regular Mycenaean colloquia every five years, and there too after the first enthusiasm I normally went with the feeling that lots had been done and little was new; every time I came away excited and in a sense refreshed. They were and are good venues to meet colleagues or teachers whom I admire or admired. Some have disappeared, and I still miss them: Oswald Szemerényi, Ernst Risch, Michel Lejeune, John Chadwick, Olivier Masson; others closer to me in age or sometimes much younger have become good friends. It is extraordinary how much Mycenaean has done to create good international relationships. I also owe to Mycenaean my most recent interest, that in the observation of onomastic development. I always thought that the study of personal names was meritorious but unexciting, until I discovered how interesting it is to compare and contrast the patterns of phonological and morphological development in the standard lexicon of a language (Greek in my case) with that in the onomastic system of the same language: classical Greek does not have a special feminine form for *hippos* 'horse or mare', but the sequence

*hippē* appears in feminine names like *Xanthippē*, where pragmatic reasons dictate that the feminine is morphologically marked. Currently the diachrony of names contrasted with the diachrony of nouns seems to me a very fruitful field for analysis; in both cases there is continuity, but pragmatic causes determine different forms of continuity. To observe this type of phenomenon horizontally across the Greek dialects and vertically from Mycenaean to later Greek is extraordinarily valuable (Morpurgo Davies 1999; 2000).

In the late 1960s I was invited by Arnaldo Momigliano to give a paper on one of the great linguists of the nineteenth century at the seminar which he and Sally Humphreys run at the Warburg Institute. I have never been good at saying no and I agreed, under protest, since I felt, and was, ignorant in the subject. I ended up working on Karl Brugmann and the history of the neogrammarians' controversy in the 1870s. The story had a gossipy, malicious side which was naturally appealing, but for me the important discovery was that most of what I had absorbed from textbooks or from my teachers about our predecessors was quite simply wrong. I had a vision of boring old scholars, Teutonic in appearance and mentality, conservative and dull; I discovered a set of arrogant, irritating and self-confessedly revolutionary youngsters. I had thought that most of what I knew about Indo-European had been established at the beginning of the century, but reading the periodicals of the late nineteenth century I found myself in the midst of a subject in complete flux. Everything was under discussion and everything was reconsidered to a point which I had never experienced. New questions were asked but solutions were not always forthcoming. At the time the excitement must have been overpowering, and I began to understand what vibrant scholarship meant. At the same time I acquired a new understanding of what I thought I knew; I now saw how it had been reached, and that opened new vistas about certainties and above all uncertainties. In a way I was relearning my subject, making it mine in a way which I had not previously experienced. I did not like my Brugmann paper and only returned to it some 15 years later, but in the late 1960s I felt flattered and pleased when Tom Sebeok wrote to ask me whether I would contribute the chapter on the historiography of language classification to volume 13 of *Current Trends in Linguistics*. It was very hard work, partly because I had a great deal to learn from the primary sources, partly because I had to read an immense amount of secondary literature. At the end, however, I had the feeling that I was beginning to understand what the linguistics of the nineteenth-century was all about; this induced me years later to accept Giulio Lepschy's invitation to contribute the nineteenth century chapter to the volumes he was editing for Il Mulino and Longman about the history of linguistics. That too turned out to be an exciting but almost impossible task – I had little time (my administrative tasks were increasing daily) and I kept missing all the deadlines. Time was also wasted in trying to settle on a

language, until I found out that I had greater difficulties in writing in Italian than in English. Since I still make mistakes in English the conclusion – not very cheerful – was that I did not have any longer a language in which I felt entirely confident. The Lepschy chapter grew until in a later version it became a book (Morpurgo Davies 1998). In writing it I felt again, as I had when I first worked on Mycenaean, that I needed to be omniscient. There was a difference: in my early days I had simply wished for all-encompassing erudition; now I also realized that I did not know enough about historical method or philosophical theory. At times I felt very despondent. I was cheered up by a few friends – Giulio Lepschy, Bobby Robins, Peter Matthews – though when I compared my difficulties with the apparent ease with which they worked I became even more despondent. Compared with other authors, my one advantage was that I knew, and cared for, what nineteenth-century scholars were doing. Historians of ideas had a much better understanding than I did of the general intellectual atmosphere, but I could see what developments were prompted by the internal economy of the subject. In the end I took comfort in Hoenigswald's dictum that in the history of nineteenth-century linguistics what really matters is what linguists did, not what they thought they did.

My third line of research is largely due to serendipity. The Hieroglyphic Hittite seminars started by Palmer continued for a while but in the late 1960s/early 1970s, things were changing. Hawkins had begun to look at the actual inscriptions and realized that past editions were inadequate; he used his outstanding draughtsmanship to produce new drawings and naturally new interpretations. The two of us continued to meet because we wanted to make sense of the texts; I was also eager to make sense of the language in comparative terms. How could an Indo-European language have a word like *atimā* meaning 'name'? The discovery of two words written in the Hieroglyphs and identified with two measures which we knew from cuneiform script texts led to a new reading of four signs; no longer ⟨i, ī, a, ā⟩ as previously believed, but ⟨zi, za, i, a⟩. The consequences were dramatic because the signs in question were used mainly in the inflectional endings; suddenly the grammar changed and the so-called Hieroglyphic Hittite was proved to be Hieroglyphic Luwian. We reported, with considerable trepidation, our theories at the Symposium on the Undeciphered Languages organized by the Royal Asiatic Society in 1973 to celebrate its sesquicentenary. A few experts were there; one of them, Günther Neumann from Würzburg, agreed with us but persuaded us that the reading ⟨a⟩ was wrong and should be ⟨ia⟩. We ended with a joint publication (Hawkins, Morpurgo Davies & Neumann 1973). The word for 'name' now turned out to be *adaman-za*. Shortly afterwards Hawkins demonstrated that some signs which had been taken as indicating relative pronouns were in fact negative particles. This time what changed was not the grammar but the meaning of the texts. Later discoveries (including some digraphic seals) have shown that the new readings were

correct. Hawkins and I wrote and are still writing a number of joint articles, and though my task was mostly that of understanding the linguistic consequences of his interpretations, we found it useful to discuss with each other every part of the work – few things have been more satisfactory in my life than the discovery that it is possible to work on a regular basis with a colleague who is also a friend. The culmination is of course Hawkins's monumental corpus of Hieroglyphic Luwian inscriptions, which appeared in 2000, some 35 years after his work first started. The historical consequences of the understanding of these texts are very great; the linguistic consequences are also important. Equally interesting is the fact that the script is a rare and perhaps unique example of a script developed in the first instance for an Indo-European language. Now that Hawkins's *Corpus* has appeared, we must plan for a systematic account of the linguistic facts.

Historians of linguistics know that the institutionalization of linguistics has made it impossible to concentrate exclusively on the views of outstanding linguists. 'Normal' practitioners are also influential, because they decide on the composition and direction of departments and on the subjects to be taught. In the nineteenth century it was natural to link the history of linguistics with that of the universities because most contacts depended on the university to which one belonged or from where one originated. Nowadays, with increased mobility, we must also think in terms of scholarly networks – once again noting that recognized geniuses are laws to themselves. On the assumption that this account is meant to provide some down-to-earth material for future historians, I note that my own 'network' – a word which I do not like – has been changing over the years. It never was a purely Italian one, because Mycenaean, with which I started, was such an international subject, but initially it was largely limited to classicists, historical linguists and Indo-Europeanists in continental Europe. These contacts and friendships remain, of course, and in recent years my links with Italy have increased. Yet in the last decades I have also had frequent and close personal and scholarly contacts with American Indo-Europeanists: Hoenigswald, of course, who has been a mentor and a friend all along, but also Warren Cowgill before his premature death, Stanley Insler, Calvert Watkins, Jay Jasanoff, Alan Nussbaum, Andrew Garrett, Craig Melchert, Don Ringe and of course others. I was lucky to meet J. Schindler in Harvard, and the friendship continued when he returned to Vienna.

As Lejeune once noticed, conferences are 'bons instruments de voyage'; yet real progress is made in specialized colloquia. I have gained a great deal from the quinquennial Mycenaean Colloquia, which were started by Lejeune in 1956 (before my time), and from the Greek Dialectology Colloquia started by Claude Brixhe in 1986. In the States, Warren Cowgill and Stanley Insler founded in 1982 a yearly series of East Coast Indo-European Conferences (ECIEC), which offer a chance to discuss Indo-European problems informally. I have had to miss most of them, but those I could

attend and the odd weeks or months or semesters which I spent at the Universities of Pennsylvania, Yale, Harvard and Berkeley have been among my most important experiences. I keep asking myself whether the strength of my link with American universities and American scholars depends on linguistic factors (I feel more at home in English than in French or German) or on some other form of natural affinity, but I do not seem to be able to reach an answer. I can simply note, for the future historian, that this variety of contacts is probably more typical of the twentieth than of the nineteenth century. I have been lucky in having the chance, but I know that the real luck consists in having found a subject which allows endless possibilities and whose interest never diminishes.

I have occasionally tried to describe academic work to an outsider: teaching is easy, administration is not very different from what is done elsewhere, research is much more difficult to explain. 'Do you like doing it?' I am asked. 'What is it like?' I have learned not to give an honest reply; if I did I would probably say that it is a great deal of drudgery alternating with brief moments of excitement so overpowering that it is impossible to keep still, and with other moments when one's head seems about to explode because it is required to absorb too many things at once. All of this is in various ways painful – and yet I doubt that we do it only to keep the RAE happy.

## NOTES

- 1 In my first two years in Oxford I gave tutorials in Greek philology to two graduates, Michael Mann and David Ferris, who were specializing in Greek and Slavic and Greek and Sanskrit respectively. We were pretty close in age and I found the experience immensely rewarding. After the end of the Diploma Michael Mann was offered a position in SOAS and became a specialist in Bemba; David Ferris obtained a Lectureship in General Linguistics at the University of Exeter. In both cases they had to learn their new job more or less from scratch.
- 2 Most of them are still there and working towards the same aim; others were mentioned before in the list of the first members of the Linguistic Circle; yet others resigned or retired, like C. J. E. Ball, Cathy Slater and Rebecca Posner; the list could be much longer. Sadly a few died prematurely: Stephen Ullmann (d. 1976), Ann Pennington (d. 1981), Leslie Seiffert (d. 1990); it is impossible to forget them.
- 3 Few things are more painful than to see one's students disappearing before oneself. The recent (2001) death of Katrina Hayward at the age of 49 is difficult to bear.

## REFERENCES

- CHANTRAINE, P., 1947. *Morphologie historique du grec*, Paris: Klincksieck.  
 CHOMSKY, NOAM, 1957. *Syntactic Structures* (Janua Linguarum, No. IV), The Hague: Mouton.  
 DELBRÜCK, B., 1875. *Das Sprachstudium auf den deutschen Universitäten*, Jena: Dufft.  
 ERNOUT, A., 1953. *Morphologie historique du latin*, 3rd ed., Paris: Klincksieck.  
 ERNOUT, A. & THOMAS, F., 1953. *Syntaxe latine*, 2nd ed., Paris: Klincksieck.  
 HARRIS, Z. S., 1960. *Structural Linguistics*, Chicago: University of Chicago Press. 1st ed. as *Methods in Structural Linguistics*, 1947.

- HAWKINS, J. D., 2000. *Corpus of Hieroglyphic Luwian Inscriptions*. Vol. I *Inscriptions of the Iron Age*, 3 parts, Berlin: de Gruyter.
- HAWKINS, J. D., MORPURGO DAVIES, A. & NEUMANN, G., 1973. 'Hittite hieroglyphs and Luwian: new evidence for the connection', *Nachrichten der Akademie der Wissenschaften in Göttingen*, Phil.-hist. Kl. 6.
- HOENIGSWALD, H. M., 1960. *Language Change and Linguistic Reconstruction*, Chicago: University of Chicago Press.
- HUMBERT, J., 1954. *Syntaxe grecque*, 2nd ed., Paris: Klincksieck.
- LEJEUNE, M., 1955. *Traité de phonétique grecque*, 2nd ed., Paris: Klincksieck.
- MEILLET, A., 1937. *Introduction à l'étude comparative des langues indo-européennes*, 8th ed., Paris: Hachette.
- MORPURGO DAVIES, A., 1960. 'Review of Thumb-Scherer, *Handbuch der griechischen Dialekte*', *Parola del Passato (PdP)*, 458–70.
- MORPURGO DAVIES, A., 1975. 'Language classification in the nineteenth century', in T. Sebeok (ed.), *Current Trends in Linguistics* 13, The Hague: Mouton, 607–716.
- MORPURGO DAVIES, A., 1998. *Nineteenth Century Linguistics*, vol. IV of G. Lepschy (ed.), *History of Linguistics*, London: Longman.
- MORPURGO DAVIES, A., 1999. 'The morphology of personal names in Mycenaean and Greek: some observations', in S. Deger-Jalkotzy, S. Hiller & O. Panagl (eds.), *Florent studia mycenaea*, Vienna: Oesterreichische Akademie, 389–405.
- MORPURGO DAVIES, A., 2000. 'Greek personal names and linguistic continuity', in S. Hornblower & E. Matthews (eds.), *Greek Personal Names: Their Value as Evidence*, Oxford: British Academy and Oxford University Press, 15–39.
- NIEDERMANN, M., 1953. *Phonétique historique du latin*, 3rd ed., Paris: Klincksieck.
- TROUBETZKOY, N. S., 1957. *Principes de phonologie*, trans. J. Cantineau, Paris: Klincksieck.
- WINTER, W., (ed.) 1960. *Evidence for Laryngeals: Work Papers of a Conference in Indo-European Linguistics*, Austin: University of Texas. Rev. ed. 1965, The Hague: Mouton.