《語言暨語言學》專刊外編之六

山高水長

丁邦新先生七秩壽慶論文集

上冊

何大安　張洪年　潘悟雲　吳福祥／編輯

中央研究院　語言學研究所

2006

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LINGUISTIC STUDIES IN CHINESE AND NEIGHBORING LANGUAGES:

FESTSCHRIFT IN HONOR OF PROFESSOR PANG-HSIN TING ON HIS 70TH BIRTHDAY

上冊 (Volume 1)

何大安　張洪年　潘悟雲　吳福祥
編輯

Edited by
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中央研究院 語言學研究所
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雙溪和合水南流
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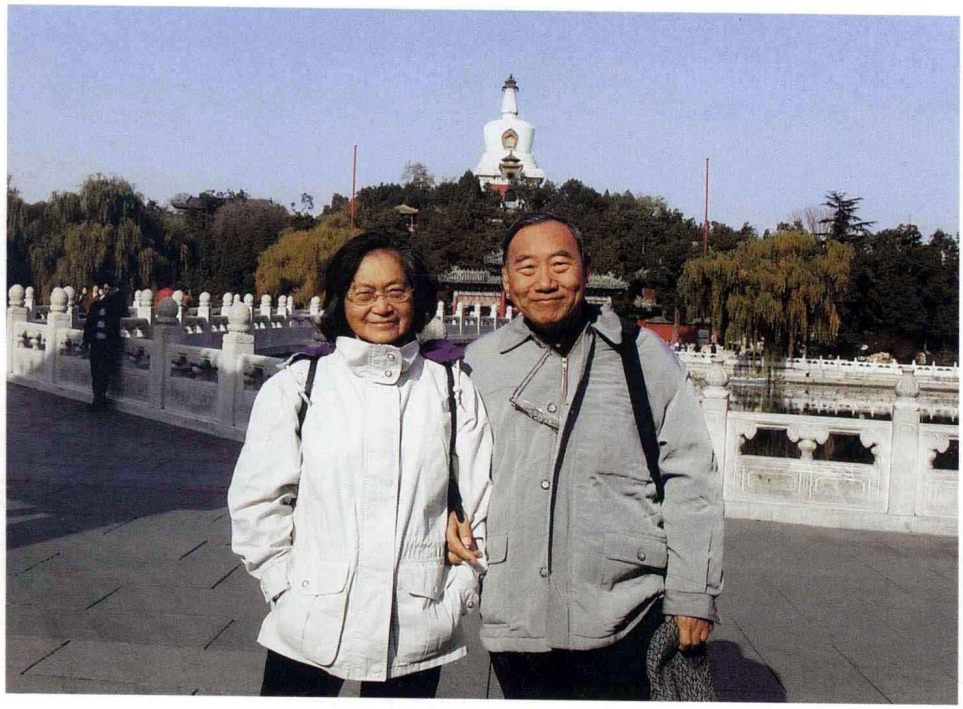

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Hooked on Language*

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This is an informal, quasi-biographical essay, tracing aspects of my experience as a linguist, from exposure to foreign languages in childhood to current research on evolutionary linguistics. My intellectual journey has been from the narrow 'autonomy' of generative grammar, with its focus on formalisms, to a broad evolutionary perspective, merging with other disciplines concerned with language, both ontogenetic and phylogenetic.

Key words: evolutionary linguistics, lexical diffusion, tones, modelling

I came across an interesting collection of essays the other day, called *Curious Minds: How a Child Becomes a Scientist.*[1] It contains self-reports by a variety of distinguished scientists on the formative influences in their lives, to become an anthropologist, a physicist, a psychologist, a journalist, etc. Reading these personal histories started me reflecting on how I became hooked on language, and devoted my professional life to it.

As we must all recognize, memories are infinitely fallible. For this reason, Steven Pinker begins his essay with the charming request: 'Don't believe a word of what you read in this essay on the childhood influences which led me to become a scientist. Don't believe a word of what you read in the other essays, either.' [p.81] Nonetheless, he went on to give a fascinating account of the events that led to his prolific scholarship in psycholinguistics.

Several authors recounted the early and decisive formative influence of family life, particularly Alison Gopnik, the other psycholinguist in the collection. The environment of my own early years in Anhui could not be more different from the highly intellectual home she described for her childhood. Huaiyuan in the 1930s was a small town in interior China, and I have essentially no memory of it, even though my early childhood

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* I offer these personal musings to join in celebrating the 70th birthday of Professor Ting Pang-hsin, who has my respect as a devoted scholar, and my affection as an old friend.

[1] Brockman, J. (ed.) 2004. *Curious Minds: How a Child Becomes a Scientist.*

was spent there. Since I was by far the closest to my grandmother throughout my early years, my first language must have been some local variety of the Anhui dialect.

By the time I was eight or so, Anhui had fallen under Japanese occupation. My parents were in Shanghai then. Since life in an international city was presumably safer than a village under Japanese rule, they sent for us. The journey from Huaiyuan to Shanghai was a dangerous one. One of my earliest distinct memories is one of my grandmother gathering me under her, using her body as a shield, when the sky was covered with war planes and with mushrooms of anti-aircraft fire.

We lived in the so-called French Concession in Shanghai, so I must have heard some French. Half a block from us was a complex that used to be the International School, but the Japanese had taken it over for military use. Like many kids my age, we spoke varieties of Putonghua in school and at home, and Shanghainese among peers and on the street. The Anhui dialect was kept alive for many years only by my bond with my grandmother.

Shortly after the Japanese surrendered in 1945, my father, who had studied at Berkeley in the 1930s, went to New York City in search of a new life for us. In 1948, we went to join him there, sailing across the Pacific in some twenty days. The last few months in Shanghai, my family found a kindly old Yugoslavian lady to tutor me in English. Nobody knew, of course, that her English was far from typical, or whether she had ever heard much English spoken herself.

So essentially I had to learn English from scratch after we arrived in New York. At age 15, I had already passed the so-called 'critical period', though I am not sure this hypothesis for language acquisition is either as simple or as strong as some psycho-linguists advocate.[2] While I can sound 'native' in English when I try, it takes some effort, even after speaking it for several decades. It is especially effortful when I am tired. On the other hand, my growth in the Chinese language was stunted with the trans-plantation, since I had left its environment. I don't really know if there are people who are truly completely proficient in two or more languages.[3] In my own case, I always deal best with numbers in Chinese, particularly in remembering them or in multiplying them. In matters academic, however, it is much more natural to use English since that is the language I work in.

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[2] Language is clearly not the only thing that can be best learned at an early age; age dependence is true of learning mathematics, music, and sports as well. Furthermore, language has many components: motor, sensory, memorial, computational, etc. It appears that the motor component, as is most age dependent, as in many athletic skills. Failing to master good pronunciation inhibits practice, which in turn hinders the learning of grammar, vocabulary, and pragmatics.

[3] Rather than 'bi-lingual', implying complete proficiency in both languages, perhaps 'semi-lingual' is more accurate since neither language has complete coverage.

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My entire family returned to China in 1951. Uncertain of the situation there, particularly since it was during the Korean War, my father wanted me to stay behind to take advantage of a generous four-year scholarship I had just won in order to attend Columbia College. The plan sounded rational enough, but it had not taken into account the emotional turmoil in a 17 year old boy having to survive New York City completely alone. My first report card had mostly F's; one notable exception was an A-, but that hardly counted since it just showed that I had made the junior varsity team in fencing. I remember well being called in by the dean, who had no hesitation taking away my scholarship and throwing me out.

It was out of the question for me to return to China in such disgrace, even if the political situation had allowed me to, which it did not. But I persisted in my education by taking evening classes in the School of General Studies of Columbia University. Day time I had a job with the Brass Rail restaurants, basically as an apprentice book-keeper.

By then, I had developed a strong curiosity about language. One day shortly after we started school at Columbia, I went to lunch with some fellow freshmen. It was a working class Chinese restaurant with a gruff waiter in a dirty, oily apron. Remarkably, the menu was a matrix printed on a sheet of paper, where the rows are labeled by the meats and the columns are labeled by the vegetable to be stir-fried with the meat. After several unsuccessful bids in Putonghua, with the waiter barking at me in some sort of Cantonese, I had to order for everyone in English. This prompted one of the fellows to ask why Chinese people do not speak Chinese with each other. I was both embarrassed and offended by this innocent question, but I was not able to give a coherent response. I certainly wished that I could.

Both of the evening classes I took were in linguistics, and their instructors could not have been more different. Mario Pei was a giant of a man, impeccably suited, with a *phi beta kappa* key dangling from a gold chain. When he announced to the overflowing classroom, in a voice that boomed with authority, exactly how many languages were spoken in the world then, we were all awed by his erudition. Far be it for anyone to wonder how he arrived at such precise knowledge for something as fuzzy bordered as language, and in a world where many regions were still largely unexplored. He took a special liking to me and invited me to co-author a small piece with him in the Sunday magazine of the New York Times; it was on Chinese place names—my first publication.

The other class was taught by a young disheveled instructor who spoke with a very thick New York accent. There were perhaps seven of us in a room that holds at least forty. He rushed in late, distributed a very hefty hand-out of data from a bunch of African languages, and directly launched into a discussion of the comparative method. Even then, in the 1950s, Joseph Greenberg already had some stellar achievements in the classification of the languages of Africa. The fundamental contributions he was going to

make in the areas of typology, markedness, language universals, and deep comparisons were yet to come. It was many years later, after Joe had moved to Stanford and I had gone to Berkeley, that I was able to resume learning from him.

Columbia College decided that my repentance was genuine and took me back a semester later. This allowed me to graduate in time, with a hodge-podge of courses in engineering and social sciences. For the foreign language requirement, I took some courses in Japanese—and wished I had not avoided the language so deliberately when I was in occupied Shanghai. My memory of these courses is much sweeter for the instructor, Shirato-sensei, who had that special combination of warmth and strictness. I was able to also squeeze in a summer at the Russian School of Middlebury College in Vermont, and derived great joy memorizing the sonorous poems of Lermontov and Pushkin.

Other languages I studied to varying extents at different times are: French [several weeks at the Alliance Francaise in Paris], Swedish [I was based in Stockholm for a year], and Swahili [in a course I co-taught at Ohio State University]. I also learned enough German to pass a reading exam at the University of Michigan, but somehow that experience left very few traces in my long-term memory. From the opposite perspective, I taught English to foreign students at the English Language Institute at the University of Michigan, and introductory Chinese at the Ohio State University. Language learners are a heterogeneous lot, and it would be foolhardy to say that some one method of language teaching works best for all. Clearly, all the high-tech language teaching aids are dazzling, and can be very useful on the side. But I am old fashioned enough to believe that there can be no substitute for the kindness and devotion of a teacher; nor can anything replace the sheer labor of memorizing some beautiful poem or catchy songs, and the immense satisfaction that comes with its recall many years later.

By the time I was accepted into the Linguistics Program at the University of Michigan, I felt essentially committed to the field in a kind of natural progression. I have always assumed that I would work in a university, and linguistics is a field that allows the coming together of a variety of activities, from tinkering in a laboratory, to visiting aphasics in hospitals, to trekking off among speakers of strange languages in exotic worlds, etc. More than anything, I was simply hooked on how various languages sound, and how they enable/require you to see and represent the world in diverse and interesting ways.

At Ann Arbor, I had the good fortune to work in the laboratory of Gordon Peterson, a leading scholar of acoustic phonetics. The striking mismatch between hearing people speak in clean discrete words and seeing these words in continuous shaded smudges on the sound spectrogram made a deep impression on me. It is evident from this mismatch that the brain requires a tremendous amount of knowledge and computation to be able

to convert the smudges to words—to recognize speech. My awareness of the cognitive prerequisites for language probably traces back to these spectrograms.

In the 1960s, it did not take long to realize that programming a computer to recognize speech without making good use of this fund of knowledge was a foolhardy enterprise. Now, half a century later, the awesome power of computation, coupled with newly developed methods of statistics, especially the Hidden Markov Models, have made that goal of automatic speech recognition somewhat more plausible. Nonetheless, the computer is still quite a chasm away from what the humblest human can do in recognizing speech, particularly in conditions of noise, notably the so-called 'cocktail party' environment.

Similar lessons apply to the area of automatic language translation. Again, saying something in English and saying the corresponding something in Chinese involves much more than finding dictionary equivalents and giving a different sentence parsing. The cognitive prerequisites for language translation are, if anything, much more extensive than they are for speech recognition. There is a colorful phrase in Italian: 'traduttore, traditore!'—to translate is to betray—which captures the nature of the difficulty quite well.

When I was nearing the end of writing my dissertation, I wanted to get a deeper grounding in general linguistics, as well as to brave the world outside of Peterson's warm laboratory. I was impressed by the work of Zellig Harris of the University of Pennsylvania, and wrote him about opportunities to study with him. *Syntactic Structures*, written by his student Noam Chomsky, had just been published, and it struck me as an exciting area to explore; so I wrote to M.I.T. as well. Perhaps by some quirk of fate the letter never reached Harris; in any case I never heard from him. In contrast, a very friendly letter came back promptly from Morris Halle, offering me a post-doctoral fellowship at the Research Laboratory of Electronics, working in a group directed by Ken Stevens of the Department of Electrical Engineering.

Morris was working with Chomsky on *Sound Pattern of English* at that time, and with Stevens on distinctive features associated with glottal behavior. My curiosity about tones came into focus around then. I also read some syntax. A question that was always with me was how much of this theoretical apparatus being developed so aggressively at M.I.T. was useful for understanding languages other than English, and for understanding human language in general. But back then, I had little feeling of the total complexity of language, in time and in space, and I was all too ready to be dazzled by slick rules and clever arguments. I had all the zeal of a new convert, and was eager to spread the word of generative grammar, at least the 1960s version of it.

An early antidote to this zealous attitude came from Greenberg. I had just

published[4] a fancy rule of tone sandhi in the Min dialect of Chinese, which had won the seal of approval from the M.I.T. establishment.[5] The tones in this dialect chase each other in a circle when in sandhi position, and I was able to describe this circular movement by proposing a set of phonological features of tone and attaching variables on a pair of these features. Some months later, I exploited the same formal device in describing the Great Vowel Shift in the history of English.[6]

I was bursting with pride when I showed this tone sandhi rule to Greenberg when we met at a conference. He listened to me attentively, and muttered something soothing like 'very nice'; then he asked me 'and then what?' I was crestfallen at this response from my first teacher; but I was not able to understand what he must have had in mind for many years to come.

To put it in terms I now use, the rule I proposed was a solution to a *local* problem —one of tone sandhi. To achieve significance, my work should have been couched within a series of *global* considerations, beyond the rule formalism that I was playing with. Does the rule, or any part of it, tell us anything about the history of Min dialects in general? Or, more globally, does the rule reflect anything about the underlying cognitive process, any more than a simple verbal account of what the tones are doing? The 'success' in rule formalism, in this case the paired variables, gave me a false sense that the job has been done, side-tracking me from pursuing the deeper empirical questions of history, cognition, etc.

A related way of looking at the question of how to do research is via the interplay between state and process. Descriptive linguistics, structural linguistics and generative linguistics all tend to concentrate on the state, paying scant attention to the various processes that led to the state. Yet often studying the process is the best way toward understanding the state, as clearly shown in the work of Greenberg. I became dimly aware of this relation when I was looking at the development of MC tone IV. In many Min dialects, IVv[7] has a ***higher*** pitch than IVu, contrary to what is found in other Chinese dialects. Phonetic considerations would lead to the opposite expectation, since voiced initials typically lower the pitch of the voice.

A reasonable assumption for such a flip-flop in tonal development is that the pitch of IVu was originally high and that of IVv was low. If so, IVu must have gone down in pitch while IVv went up. According to the prevailing conception of sound change,

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[4] *International Journal of American Linguistics*, 1967.

[5] The tone paper was cited in the *Sound Pattern of English.*

[6] *Language*, 1968.

[7] I use Roman numerals in correspondence with the traditional Chinese names of ping, shang, qu and ru, followed by u and v in correspondence with the traditional Chinese names of qing, zhuo, or yin, yang.

inherited from the Neogrammarians of the 19th century, the words of a particular sound class always changed ***en bloc***; this is, sound change is lexically abrupt. But then the question arises as to how the two blocks of syllables could have passed each other on the pitch scale without colliding into each other and merging, resulting in just one class, i.e., IV. The sound change, I hypothesized, may have been implemented several words at a time; that is, lexically gradual. Pursuing this line of thought, I came up with various other considerations that led me to conclude that the scenario of lexical gradualness must be correct, for at least a large class of sound changes, if not all of them.

I chatted about these musings with Yakov Malkiel in the hallway of Dwinelle Hall, some time around 1967. He gave me an encouraging smile, and suggested the term 'lexical diffusion' for the hypothesis. It was somewhat later that I read a little into the literature of Romance Linguistics, an area much more hospitable to this way of thinking than classical Indo-European Linguistics. In fact, Yakov himself has just published a short essay on a very similar theme.[8]

Given such a view of the process of change, there will be words which lead the change, words which lag behind, as well as words which have both the changed and unchanged pronunciations. So a change is characterized by a three stage process of UVC, U for unchanged, V for variation, and C for changed, whether the innovation is in the system of sounds, meanings, word formation, or syntactic patterning. This view of change predicts that all living languages will always exhibit a considerable amount of 'orderly heterogeneity', a very useful term introduced by Weinreich, Labov and Herzog.[9]

The view is also consistent with what evolutionists say about changes in the biological world, how variations result from innovations, and how only a few innovations are selected in the transmission across generations. In fact, Darwinian thinking was frequently in the back of my mind when I mused about language evolution. No one is foolish enough to believe that language evolution and biological evolution are exactly alike, but clearly there are many significant parallels between them, as Darwin himself repeatedly noted in his writings. In fact, it may very well be that there are useful parallels with the evolution of physical and social systems as well, as recent thinking on complex adaptive systems seems to suggest.

With the exception of a handful of scholars who were willing to consider it constructively and objectively,[10] the hypothesis of lexical diffusion first met with

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[8] Malkiel, Yakov. 1967. Each word has a history of its own. *Glossa* 1:137-149.

[9] Weinriech, Uriel, W. Labov and M. Herzog. 1968. Empirical foundations for a theory of language change. *Directions for Historical Linguistics*, ed. by W. P. Lehmann and Y. Malkiel, 97-195. University of Texas Press.

[10] A balanced discussion of lexical diffusion in its early days of development was given by W. Labov in his presidential address to the Linguistic Society of America, published in *Language*.

skepticism, and sometimes, derision.[11] Part of the difficulty had to do with the fact that many of the early examples of lexical diffusion came from Chinese, and these did not resonate in a field that was still quite Euro-centric. Also, some of these early Chinese case studies were not analyzed in enough depth to be totally conclusive.[12]

We had the objective of making a large amount of systematic phonological data available to fellow researchers on Chinese dialects so that scholars working far away from each other can collaborate. For this purpose, we compiled in the late 1960s what may have been the first computer data-base for linguistic research; we simply called it Dictionary on Computer, or DOC.[13] Nowadays, of course, the technology has advanced so dramatically that what took several of us many years to do can be done within a few weeks. Collaboration across great distances via the internet has also become commonplace.

But a linguistic theory must be relevant to all languages of course, not just Chinese, and indeed many languages from diverse families have been studied from the viewpoint of lexical diffusion. Lien Chinfa has put together a list of such studies in his contribution to the Encyclopedia of Language and Linguistics.[14] There can be no longer any doubt that the UVC scenario of lexical diffusion is real, not only for sound change, where the theory was first argued, but even more so for morphological and syntactic change.[15]

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[11] At a conference held at UCLA in the early 1970s, the audience was amused by Tom Bever, when, in satirizing lexical diffusion, he stressed the rimed words in "Professor **Wang** is obviously **wrong**." I joined the mirth by answering "Professor **Beaver** is much too **eager**", continuing his play on surnames.

[12] As an example, a paper of 1972 by C. C. Cheng and myself on tone change in Chaozhou did not consider the relevant complexities of dialect contact, simply because we had no access to the relevant data at the time. Professor Ting Pang-hsin, among several other scholars, was kind enough to discuss this issue in his 1978 paper, *BIHP* 50:257-71.

[13] C. C. Cheng was the prime mover for the original version of DOC; see his introduction to DOC (1994). In recent years he has harnessed the new technologies of speech compression and Geographic Information Systems to build such data-bases with powerful refinements that we could only dream of.

[14] *Encyclopedia of Language and Linguistics*, pp.2141-2144. More recent studies include: G. Sambasiva Rao. (ed.) 1994. *Language Change: Lexical Diffusion and Literacy*. Delhi: Academia Foundation. Lee, Sang-Oak, et al. (eds.) 2003. *The Lexical Diffusion of Sound Change in Korean and Sino-Korean*. Journal of Chinese Linguistics Monograph 20. The 2005 doctoral dissertation by Au Ching-Pong, City University of Hong Kong, explored the question of lexical diffusion by means of computer simulation of multi-agent interactions, with interesting results on a variety of sound change.

[15] The suggestion on syntactic change was made early by Mei Tsulin in the context of lexical diffusion. Detailed studies on lexical diffusion in syntactic change have been reported by

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