Remembering Joseph H. Greenberg (1915–2001)

On being a student of Joe Greenberg

by WILLIAM CROFT

I consider myself one of the luckiest linguists in the world: I was a student of Joe Greenberg's. It happened by circumstance. After finishing my BA/MA at the University of Chicago in 1978, and disillusioned with job prospects in academia, I spent four years working (moving up from temp to gas station attendant to civil servant). I decided that I had had enough, and that I really wanted to take my chance at an academic career. But I intended to make it into something less uncertain, and to pursue a career in natural language processing. I also wanted to stay in the San Francisco Bay Area, and so I applied only to Berkeley and Stanford for graduate school. I obtained a fellowship to Stanford, which by then (1982) was already a leader in natural language processing and artificial intelligence research, and shortly switched to a research assistantship at SRI International.

Since I already had an MA from the University of Chicago, I was able to place out of a number of required classes at Stanford. At the time, Stanford had a broad linguistics program with required classes in subjects such as sociolinguistics, language acquisition, mathematical linguistics, and historical linguistics – though not typology. In fact, by 1982 the typology class was not offered at Stanford: Greenberg was 67 years old and close to retirement. Instead, Greenberg taught the required historical linguistics class. The class was not offered until the last quarter, but even by then, I had started hearing Greenberg stories. He was already a living legend. My curiosity was aroused.

I took the class but my first performance was not auspicious. We were given the problem of analyzing Hungarian vowel harmony from a historical perspective. I eagerly went at the problem from a synchronic structuralist perspective, analyzing it as a change in productive vowel harmony rules over time. In class,

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when Greenberg asked for solutions, I raised my hand and offered my solution. Greenberg politely said that wasn't really it. Then Judy Hochberg raised her hand and described it as an originally transparent, productive system that had ossified and broken down through various vowel changes. Greenberg said "Yes, that's right" – and I learned a painful but very effective lesson in how to study language diachronically.

Fortunately, I recovered from that initial experience and did well in the class in the end. More important, I was hooked. Although I continued to study artificial intelligence and natural language processing, Greenberg was going to be a major part of my linguistics education. I asked him if I could do a reading course with him the following year. He agreed to it. I began my Greenberg education, reading everything of his I could get my hands on.

In fact, that wasn't my first exposure to Greenberg. A couple years later I ran into Noriko Akatsuka, who had taught me the second part of the introduction to linguistics class in the winter of 1976, when I was a college sophomore at Chicago. She reminded me that one of the articles that she had us read was "Some universals of grammar with particular reference to the order of meaningful elements". She said that I was so excited that I did some further reading and volunteered to make a class presentation on language universals. I had forgotten, but obviously somewhere deep down I had remembered.

And Greenberg was already a known name to me, because in high school I had gotten it into my mind to find out and write out the genetic classification of the world's languages, since there was no source such as Ruhlen (1987) at the time. So I had already encountered Greenberg via his African language classification, and also via Indo-Pacific, which had recently been published at that time. It was hard to avoid Greenberg if you were interested in languages.

I don't really remember the details of our reading course, at least at first. I gave him my papers to read, which on the whole he thought were very good. The big change came when I gave him my 1984 CLS paper, which was my analysis of parts of speech. He said it was a great paper. I had become a typologist.

Our meetings were on Friday afternoons, upstairs from linguistics in the anthropology department. Every Friday at 2:30 I would arrive and we would talk until 5 or 5:30, when Selma would call to find out when Joe would be going home. Joe had a large office, as befitted a professor of his fame, with the usual endless books and above all the grammars, which I always drooled over enviously, having barely started my own collection. These Friday afternoon meetings continued long after the reading course was over. In fact, they went on after I graduated. Since I am from the Bay Area, I returned there whenever I could, while I taught in Michigan and in England. Every single time I returned, I went to Stanford, and met with Joe in the afternoon, and we talked until it was time for him to go home. After a few years, the Stanford anthropology depart-

ment split, and he had to move to a smaller office (he gave me his philosophy and mathematics books, and his old journal issues – but not his grammars). At that point, Joe started working exclusively in Stanford's Green Library – where he worked all the time anyway, except when he met with students. From then, I would meet with him in the library, and we would go off to Tresidder student union for an ice cream cone or a smoothie and then talk.

The meetings were the high point of my education in linguistics – of my professional life, in fact. Needless to say, I let Joe do most of the talking. Listening to Joe was a deeply humbling but also profoundly joyous experience. I basically listened to his thought processes for almost twenty years, though of course the opportunities became rarer as I ended up farther and farther away from California. I spent the summer at the LSA Institute in 1987, and spent six months in 1993 at Stanford; we could relive the old days. It was hard for both of us when I left Stanford for Europe in the summer of 1993. Since then, I had been looking forward to my next sabbatical, because I had received an invitation to the Center for Advanced Study in the Behavioral Sciences at Stanford. The sabbatical was originally scheduled to be 2001–2002. But it was too late.

I would say that everything I have done in linguistics comes from Joe Greenberg in one way or another. Whenever a new paper or book of his came in the mail, I dropped whatever I was doing and read it right away. I always had time for Greenberg (and still do). Of course, as Joe got older, he had less time and energy to work and read, and he did not read all of the papers I sent him. But the greatest honor I received from him was after he finished volume 1 of his Eurasiatic book: he took a break and read *Explaining Language Change* before going on to finish volume 2.

What was Joe like as a person? He was unbelievably modest and unassuming for such a brilliant scientist. The reason, I believe, was that he always had a completely genuine curiosity and wonder at language, and indeed at everything in the world. He also had an unpretentious, down-to-earth way of talking about languages – reinforced by his thick Brooklyn accent, no doubt, and the equally down-to-earth similes he used. He once said, "A speaker is like a lousy auto mechanic: every time he fixes something in the language, he screws up something else".

Joe did revive his typology class in fall 1984, while I was still a graduate student at Stanford. Keith Denning, Suzanne Kemmer, and I all attended it. One day, Joe was describing some interesting fact about a language, and he suddenly stopped and said, "You know, you gotta muck around in grammars. You can't just focus on one specific thing and pick it out. You read around and you discover things you never would have thought of."

Joe was also a very kind-hearted and generous soul. He always lent me his notebooks, even the notebook on which his famous word order paper was based. (He once lent me his history of linguistics class notebooks, since he had

stopped teaching that class. He opened up the first one, and looked down at the page, and said, "Oh, that's in Syriac. I suppose that isn't going to help you." Unfortunately, he was right.) He lent his Indo-Pacific notebooks to a student who wanted to reanalyze his classification – fortunately, they were returned. Joe was also remarkably cheerful, although he was very hurt by the ad hominem attacks on his Amerind classification, by how the Stanford department turned its back on typology, and by the premature death of his last student, Keith Denning. After Joe was diagnosed with cancer, he told me he was depressed, and added that it was the first time in his life that he had felt depressed. He was devoted to his wife Selma, to whom he was married for over sixty years, and who was his longtime support.

But Joe was also a completely independent intellectual spirit. He was not so much an iconoclast as someone who considered nothing above questioning or below consideration. He absorbed comparative historical linguistics from Bloomfield, Sturtevant, and Edgerton, but did not let its strictures about reconstruction prevent him from pursuing genetic linguistic classification. He learned American structuralism from Bloch, Trager, and Whorf, but did not accept their ban on meaning nor their antiuniversalist stance. He continued his typological approach to universals, developed at the same time as generative grammar, while the rest of American linguistics fell under Chomsky's spell. Joe sometimes attributed his independence to the fact that he didn't study linguistics in a linguistics department. In fact, most of his learning appears to have come from reading: logic, philosophy, languages, linguistics, anthropology, history, culture, biology, and so on. He had a classical education (his first interests were Classical and Semitic studies), and was awesomely well-read. He lamented to me that students no longer received the broad humanistic education that he did – but he largely gave that education to himself.

"I learned more from languages than from linguists", he used to say. He was first and foremost an empirical scientist of language. Both his controversial work on language universals and his even more controversial work on genetic classification was based on the same thing: a nearly exhaustive examination of all the linguistic data he could get his hands on. His language universals and genetic classification, dramatic and far beyond what anyone else had done as they are, were always presented as provisional and subject to revision.

I have no doubt that Joe Greenberg was the greatest linguist I have ever met, or ever will meet. He was also a wonderful human being; he was the scholar's scholar, and a great teacher for those who had the opportunity to listen. A colleague once said he should be declared a national treasure, as they do with individuals in Japan. I will not be surprised to see his most controversial hypotheses vindicated, even his genetic classifications, to which he devoted much of the last two decades of his life.

Although his mind was as sharp as ever, age did slow Joe down. He no longer scampered down the stairs from his office. He shuffled ever more slowly from home to Green Library and back (but I'm sure the exercise kept him going!). He even stopped working in the library on Saturdays in the last decade of his life, going in "only" five days a week, and stopped working at home at night (!). In his seventies, he was unhappy that he would read a grammar of a language and not remember everything in it. He complained that he shouldn't have waited until the age of 65 to start learning Japanese, but at 85 admitted he could read an Ainu–Japanese dictionary without that much difficulty. I wish I had these problems.

It was not until last year that the first volume on the Eurasiatic language family, Indo-European and its closest relatives, came out. This volume contains the grammatical evidence for a broad and ancient family encompassing most of the language families of Northern Eurasia, with over a hundred grammatical morphemes traced through their vicissitudes in the various languages and families of the group. It has only been a year since it has come out, and I have not seen any reviews. But one linguist said to me, "Who else can you say that their last book is their greatest one?" Another said, "Who else can you say that they are at the peak of their career at the age of eighty-five?"

In his last year, Joe worked feverishly to finish volume two, the etymological dictionary of Eurasiatic. He wrote to me that he felt that he was working against time: sadly, he was right, though he didn't know it at the time. He gave the last batch of etymologies to Merritt Ruhlen for preparation the day that he first went into the hospital in October 2000. After that, Merritt came over to see him almost every day. They finished the basic work on volume two by the middle of March 2001.

I went back to California to see Joe in November 2000 and January 2001, and spoke to him every week until the end. Up to the last month, Joe was still incredibly active. Even in my last conversation with him, a week before he died, he could joke that he could have written five papers in the months since he had been diagnosed. I believe it. Joe didn't want to stop. He wanted to pursue the classification of languages all the way up to the human language family; and of course there were all those fascinating processes of language change that he encountered on the way. Fortunately he also recognized that he had lived as full a scholarly life as one could ask for, and that his published work (including the work still to appear) would leave a legacy that will extend far into the future.

When I first heard of Joe's illness, I put on a recording of Beethoven's late quartets. Thinking of Joe always made me think of Beethoven (whom Joe loved) – the greatest. Joe was unafraid to look at the big picture when everyone else was getting more and more specialized. And it led him to discover patterns that others did not see (or refused to look at).

The last week I called, Joe was too weak to talk to me. That night, he fell into a coma. I called his wife Selma every day. Every day was long and anxious, until it was late enough for the time difference to allow me to call California. On Monday evening, I listened to Mahler's *Das Lied von der Erde* (Joe also loved Mahler). I waited until the last sounds of *Der Abschied* faded away, and then I called. Joe had just died.

My wife said that it reminded her of the time when a friend called to tell us that the greatest redwood tree, the Dyerville Giant, had fallen. "The Giant has fallen", he said; "The Giant has fallen." It was true again: another giant has fallen. We went out to the park and watched the full moon rise over the trees. Joe's spirit rising to the heavens, we said.

Joe's time has passed, far too soon for all of us. We must carry his spirit, and the spirit of his work, on into the future.

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Bumping into Joe, repeatedly: Joseph Greenberg the theorist

by T. GIVÓN

I was never, strictly speaking, Joe's student. Like many of my generation, I just adopted him, piecemeal and *post-hoc*, whenever his work turned out to be relevant to my own. Again, and again, and again. These somewhat random notes thus bear, inevitably, the footprints of my own haphazard introduction to Joe and his work. To quite an extent they thus bear the marks of strictly-internal reconstruction.

1. Out of Africa

I bumped into Joe for the first time during my early days as a would-be Africanist, when part of my 1967 doctoral exam was dedicated to the Greenberg–Guthrie brouhaha about the place of Bantu in Africa, a controversy engendered by Greenberg's (1955, 1963a) classification. The near-universal outrage among senior Africanists was directed at the end product, the classification itself. But what attracted me to the topic was the clear articulation, in Greenberg's subsequent responses to his various – nearly all of them vehement – critics, of the theoretical underpinning of the hallowed Comparative Method (CM). That is, that underneath its placid inductivist surface were buried a host of theory-laden

decisions about phonetic similarity, semantic relatedness, and in particular the silent partner of CM, Internal Reconstruction.

Twenty years later, when I showed Joe a copy of that exam on a pure lark, he chuckled: "Nice" he said. "Most people still haven't noticed. One wonders how long one would have to keep stating the obvious." This conversation took place a year or so after Joe's Amerind book (1987) provoked a similar fire-storm of vituperation and outrage, centered again – as near as I could see – on the very same issues. By then I had already resigned myself to linguistics being a land of the perpetual *déjà vu*.

2. Typology

I next bumped into Joe, again woefully after the facts, in 1971 when my interest in diachronic syntax turned out to impinge on his celebrated paper on universals of word-order (1963b), whose typological correlations I was then trying to explain. My brash conclusions – that extant synchronic structures were merely the footprints of coherent diachronic change – turned out again to have been anticipated by Joe in a paper published two years earlier (1969: 186):

> Synchronic regularities are merely the consequence of [diachronic] forces. It is not so much [...] that "exceptions" are explained historically, but that the true regularity is contained in the dynamic principles themselves [...].

That is, typology – and the implicational universals extracted from it – were fundamentally NOT about the extant synchronic types, the visible artifacts at the tail end of emergence. Rather, these types were but the end-products of the diachronic pathways that gave rise to them. The real universal constrained development.

3. Diachrony

Two projects later in 1979, trying to understand where crazy syntax came from, I was still trailing behind Joe, whose two articles, "Diachrony, synchrony and language universals" (1978) and "Rethinking linguistics diachronically" (1979), had once again scooped me, with the same clearly-articulated message: In language as in bio-evolution, extant types ("species") are but the tail end of developmental pathways that give them rise ("evolution"). The universals – the regularities, not only the occasional distortions – are all universals of EMERGENCE. To an ex-biologist, this made perfect sense.

4. The project

When I proposed to join the Stanford Universals Project for a year in 1975, the best attraction it held for me was Joe and Fergie's total reluctance to be directive. They refused to tell you what to do, but merely assured you that

whatever it was you were working on was just fine by them. Of course, Joe's presence at the bi-weekly meetings was the real bonus, as was the presence of Dwight Bolinger, another adoptive mentor. What struck me repeatedly during those project meetings was how every laborious "discovery" was met with Joe's gentle chuckle: "Oh, I thought you'd never get here".

Eventually I got Joe to articulate, strictly in private, his feeling that everything was so plainly obvious and why was there so much resistance? In a 1979 book I echoed this by confessing to my embarrassment at having to state – or as it turned out after I did my homework, to RE-state – the obvious. I was being arrogant alright, but by then I had already discovered Joe's own gentle arrogance. For he had confessed, again strictly in private: "Yes, all these people who are so modest probably have much to be modest about."

Joe's incredible, forgiving gentleness and his refusal to be directive remained a puzzle through my year with the Stanford project. Though he no doubt disapproved, he never chided me for my predilection for *post-hoc* intellectual roots. Joe himself was of course incredibly well-versed in the Classical tradition and the 19th century, but he never felt bound by it, and never expected us to be bound by it either. I suspect he may have rued our – certainly mine – abominable cultural illiteracy, but was graceful enough to let it pass. I still wish I could feel, let alone practice, the same gentle tolerance.

5. Markedness

In the late 1980s, working on the text-distributional correlates of markedness, I once again bumped into Joe. This time it was his old Mouton classic, the "other" Universals book (1966a, b), and the 1974 article on the frequency distribution of Russian case-roles. In line with George Zipf's prescient 1935 book, Joe's was an unabashed functionalist-cognitive take on the hence-structuralist preserve of MARKEDNESS. To wit (1966b: 60, quoted from the 1976 Mouton edition; emphasis added):

> Viewed psychologically, there is perhaps justification for seeing a similarity between the implied, fundamental characteristic, that is the unmarked member, whether in phonology, grammar, or semantics, and the Gestalt notion of GROUND, the frequent, the taken-for-granted; whereas the marked character would answer to FIGURE in the familiar dichotomy [...].

Why else would language users favor those whoopingly skewed PERFORMANCE frequencies unless markedness was a property of on-line behavior, of the MECHANISM that produced the structures, rather than of the structures themselves? Simple, elegant, obvious – and to this day unaccountably alien to most.

6. Amerindia

Here I had better confess: Genetic relationship and language classification had never struck me as the most exciting facet of Joe's work. Still, the maelstrom that followed the publication of his Amerind classification (1987) was, to those of us who lived through its African antecedent, a *déjà vu* with a vengeance (as Paul Newman so aptly pointed out at the time). Predictably, this was once again the part of Joe's work that attracted most public attention (viz. the *New York Times* obituary of May 15th, 2001) – and professional animus.

Classification and genetic reconstruction are of course the stuff that anthropologists and historians resonate to, and rightly so. But it is still the least theoretical, least universals-driven sub-field of linguistics, in the main cataloguing unique historical accidents – though of course not quite. For as Joe himself pointed out in a recent article (2000), there were strong theoretical underpinnings to the Comparative Method, underpinnings that most of its orthodox practitioners, who ganged up on him just as mercilessly in Amerindia as they did in Africa, were blissfully disinclined to consider.

Tracing languages to their origins (or, for that matter, language to its origin) has always been a sexy topic, precisely because it has had relatively shallow theoretical foundations, and the proposal itself (12 major macro-families, three mega-stocks, Nostratic, etc.) was readily accessible to everybody. Joe had always been entranced with such projects. The more empirically tenuous they appeared, the more endearing they seemed to him. But this did not justify the veritable feeding frenzy the Amerind regional specialist launched into, visceral, vituperative, vindictive.

This is where, in spite of my indifference to CM and genetic reconstruction, I began cheering for Joe: He had done it again, had out-flanked the purists by being less-than-100 %-careful. He had short-cut through their hallowed Method, scooped them, by coming to a hypothesis without paying his strict inductivist dues. Hooray for shortcuts in science!

Of course, I had known the CM purists and their rabid anti-theoretical position from way back. Like Guthrie in Africa, they were still trying to out-19th-century the 19th century. It was around that time that I sent Joe my half-baked 1967 piece on the Greenberg–Guthrie affair; after which he asked me to join his "defense team". He was feeling beleaguered and, as usual, more than a trifle puzzled. For as usual, he thought he had been merely stating the obvious. I tried to tell him, gently, that I was with him on purely theoretical grounds but had no knowledge of the actual facts (nor any real enthusiasm for CM).

For my money, the best review of the controversy appeared several years later, in 1990, as a note to *Language* written by Jim Matisoff. No vituperation, no frothing at the mouth, just pointing out that linguistic evidence decayed

gradually, so that somewhere beyond the 4,000–5,000 years time-depth, one began to approach the level of chance similarities, where one would need some statistical justification of any claims of above-chance relatedness. Quite aptly, Jim cited the cautionary tale of his friend and adopted guru, Paul Benedict, who had done similarly deep, dare-devil reconstructions in Austro-Thai and Japanese.

7. Antecedence

In spite of the voluminous evidence of conscientious citations and apt quotations, it is not as easy as one would expect to ferret out Joe's intellectual antecedents. For they were widely spread, far reaching and delightfully idiosyncratic. A lot of it, perhaps the initial core, goes back to traditional 19th-century philology and the Classics, a literature I am alas only too unqualified to invoke. But like all imaginative, restless people, Joe plucked his intellectual roots wherever he could find them, assembling them along the way without undue worry – as long as they happened to fit the task at hand. Which is a damn good habit in the philosophy of science and comes with excellent Biblical provenance. For both Karl Popper and the Scriptures proclaim the very same methodological pragmatism: "By their fruit ye shall know them."

When people point to Joe's putative 20th-century antecedents, my own suspicion is that 20th-century linguistics had relatively little to do with his eventual mature work. He was, paradoxically, both a traditionalist and a self-invented iconoclast. He picked and chose influences when they suited him, but was largely self-invented. For there was really no clear precedent for his particular theoretical brew in 20th-century linguistics.

Joe's theoretical stance is, to begin with, a clear if implicit repudiation of Bloomfield's anti-universalist, anti-mentalist structuralism. He had little to say about Jakobson beyond the perfunctory citations, rightfully giving most of the credit where it was due – to Trubetzkoy and Zipf. From Joe's perspective, the snide rubric "There was no theory that was not beautiful enough to have to suffer contamination from recalcitrant facts" suited Jakobson just as well as it did Chomsky.

Joe's putative indebtedness to Saussure is equally problematic, given that his work in its totality is an explicit rejection of Saussure's three dogmas:

(i) *Langue* vs. *parole* (the Platonic/Chomskyan idealization): Joe believed in the theoretical significance of usage frequencies, those transparently PERFORMANCE data that hinted at cognitive universals.
(ii) Synchrony vs. diachrony: Joe explicitly rejected this kindred idealization, arguing for the diachronic underpinnings of synchronic universals.
(iii) Arbitrariness: Joe was a functionalist and iconist from the word go, who took it for granted that grammar was largely non-arbitrary – at least at

> its motivated inception. Though like many who looked at iconicity and grammaticalization together, he also knew how grammar could become increasingly arbitrary (1991).

Unlike many other contemporary functionalists and typologists, Joe was never part of the Sapir genuflection cult. While Sapir's instinctive mentalism was broadly compatible with Joe's, Joe's universalist approach was much more compatible with Jespersen's *Philosophy of Grammar*, an intellectual vein that harkens back to Hermann Paul (Bloomfield's early mentor) and the German Romanticists. Indeed, Sapir's intellectual flirtation with Whorf's relativism is profoundly structuralist and anti-universal in its slant on cross-language diversity.

One of my favorite quotes attributed to Joe was "I didn't learn about linguistics from linguists, but from languages". In his own gentle way, I suspected, Joe was just as arrogant as myself. Or rather, I was just as arrogant as him, given the obvious standard of comparison. He craved the process of discovery, the exhilaration of being there first, the mountain-top with its virgin snow and uncluttered vistas. But arrogance notwithstanding, he still came closest to practicing what I think he would have liked us all to practice – a broad, visionary, complex, gradual, and above all communal science.

8. Aristotle

Joe was above all his own person, *sui generis*. But if there was anybody he considered a guide, it was probably Aristotle. This suspicion has been confirmed to me recently in a note from Joe's cousin, a Physics professor at Penn, who had kept in touch with Joe over the years. He told me that Joe carted Aristotle's collected work in his kitbag to World War II and Europe.

Which, come to think, is a real comfort, for I have always suspected that Joe's deepest roots were philosophical and methodological. That oft-cited rubric ("I didn't learn about linguistics from linguists, but from languages") was, to a person like myself, a vindication and a license to go and do likewise; to wallow in the phenomenology, just like the Philosopher did. Like all persons with a big ego, Joe loved to be admired but expected no genuflection. I felt he concurred with my choice of teachers – the Philosopher, and the facts.

In the shade of Aristotle, some of Joe's contradictions begin to make sense – his infatuation with the minutiae of language, coupled with the ever present but largely implicit theoretical stance. I would have of course preferred the theory to be a bit more explicit, I sometime have the same beef with Aristotle. But it is still there, it just takes a bit more digging. You have to tease it out, earn it the old-fashioned way. But that's half the fun.

As one may well imagine, a long exposure to Joe could be a discouraging experience for an arrogant person – but for the saving grace of (again) Aris-

totle. For a true Aristotelian is utterly indifferent to where he found his ideas, hints, or antecedents – as long as they happen to fit. Originality is, in my experience, vastly exaggerated in science. It is the convenient fiction of compulsive genuflectors. I was in turn both shocked and gratified a few years back to learn that Aristotle lifted the grand semiotics of *De Interpretatione*, that dense paragraph that launched both empiricism (in epistemology) and structuralism (in linguistics), out of Epicure. I'm sure Aristotle couldn't care less.

It was a relief to be told about Joe's Aristotelian bent. Though in retrospect, I should have known, given his work. Who else would possess both the broad if implicit theoretical vision and, at the same breath, the obsessive passion for the precious small quirks of the data? Only a true Aristotelian could enjoy both equally. Most linguists I know fall squarely on either one side or the other (either Noam Chomsky or Bernard Comrie?).

The real thrust of Joe's typology cum universals work is indeed unmistakably Aristotelian: You want a theory of universals? Invest some time in looking at a decent sample of diversity. For your theory must account for both. This is how Aristotle assembled his functionalist biology in *De Partibus Animalium* and *De Generatione* – by studying the vast diversity of extant species. This is also the way he came by his political theory in *The Politics* and *The Constitution of Athens* – by poring over scores of diverse extant constitutions.

The story behind Joe's 2000 article on the Comparative Method ("The concept of proof in genetic linguistics") is another Aristotelian parable. When he sent me an early copy, I was – it just so happened – in the midst of prodding the editor of a forthcoming volume into squeezing some more theoretical oomph into his collection. As it happened, the volume already had an article by Joe, another one of his trademark exercises in minutiae. I called Joe and told him the CM article would fit much better into the volume. It would, I suggested, provide a vital missing link in the collection. To which Joe responded that the CM article had already been committed to another venue, but that if I could negotiate a switch, it was fine with him.

When I reported back that we had us a deal, Joe sounded relieved. "Yes", he said, "I felt really embarrassed about that paper after the symposium, when I realized I had misinterpreted the topic." He still balked, though, at my suggested alternative title ("The theoretical underpinning of the Comparative Method"). He said he preferred to go with the more down-to-earth original. In this Joe once again echoed Aristotle, who loved to wallow in the details and often took the underlying theory for granted. As far as theory goes, Joe must have subscribed to what Pythia is reputed to have told Xenophon: "Invoked or uninvoked, the God will be there."

9. Farewell

In 1985 I received an invitation to attend a dinner celebrating Joe's official "retirement". Unfortunately prior commitments made it impossible to attend in person, though the organizer promised to read in public my short tribute to Joe. It went like this:

> It is not every day that one is called upon to honor one's spiritual Godfather, especially one as elusive, and as reticent at being ANYONE'S Godfather, as Joe Greenberg. It has never been easy to claim this particular Godfather *in absentia*, as hard as many of us have tried. For a whole generation of linguists like myself, Joe Greenberg has been just that – a reluctant, reticent Godfather, thrice as real for being absent. All this makes his profound, lasting impact on the way we view language and do linguistics all the more remarkable. Only a truly transcendent teacher could claim such a wide-scattered, disparate, unruly, and above all hardy bunch of adoptive students, most of whom have never had the privilege of studying with him in person. While I cannot begin to emulate Joe's great capacity for gravity-defying ever-presence, I would still like to be counted as having been here tonight with you, helping Joe celebrate a lifetime of scholarship. Those strange rumors about your retirement, Joe, must surely be premature.

If one could deign to credit a single person as founder of our diffuse, disparate, garrulous network, it would surely be Joe. For in his work, more clearly than in any others', the transparent unity of typology, universals, diachrony, and functionalism was manifest, indeed boldly proclaimed and painstakingly documented. His catholic tastes in languages and linguistics, his restless scholarship, his adventuresome curiosity, and above all his insistence on understanding and explanation, have inspired several generations of linguists and anthropologists. We will sorely miss him.

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Minute details, the big picture, and Joseph Greenberg

by MATTHEW S. DRYER

While all work in linguistic typology during the past few decades relates back in one way or another to the work of Joseph Greenberg, this link has been especially strong for my research, since a core part of my research has built, not only on the spirit, but also the details of his 1963 paper on word order universals. Although my typological database now contains data for many typological features other than word order, it started out as a database on word order and on affix position. Its original purpose was to test hypotheses about word order, both the specific ones that Greenberg proposed and related generalizations that others, like Lehmann and Vennemann, had proposed, and to hopefully discover new generalizations. In the grant proposal that I wrote at the beginning of the project in 1982, I characterized Greenberg's 1963 paper as a pilot study on word order, and I described the proposed research as a full-fledged investigation of the same phenomena. I have no idea whether Greenberg would have accepted my characterization of his 1963 paper as only a pilot study, but he was

supportive of my project from the start and in other areas, I saw how he was always open to new research that covered topics on which he had published. And while my research has challenged many widespread assumptions about word order correlations, in very few cases has it challenged claims that Greenberg himself made. Rather it has challenged claims that linguists have made that generalized from his specific claims, especially in attributing one feature to OV languages and the opposite in VO languages. And although people often attribute these general claims to Greenberg, the universals he proposed in his 1963 paper were generally more complex generalizations, as Jack Hawkins argued in detail in his 1983 book on word order.

One of the paradoxes of Joseph Greenberg was that he was at the same time a man of minute details and a man who looked for the big picture. My earliest memories of Greenberg were at the 1976 LSA Summer Institute in Oswego, New York. Nearly every time I went to the library there, Greenberg was sitting at a table reading, in most instances, I assume, descriptive materials. I never saw any other linguists teaching at the Institute there in the library. Among typologists, there is considerable variety in how they relate to published descriptions of languages. For some typologists, published grammars are not their primary source of data; rather their primary source of data comes from their own work with native speakers. There are other typologists for whom published grammars are a primary source, and they look at grammars in order to see how the language can be classified according to typological parameters they are interested in. But Joseph Greenberg represented a third type of typologist, who just love looking at grammars, for whom both the expected and unexpected will jump out as they read, and eventually a picture of what is typical and what is not. This was the Joseph Greenberg of minute details.

But Greenberg was at the same time more interested in the big picture than most linguists, particularly those of his generation. While the focus of many linguists trained prior to the generative era was on methods of linguistic description and on individual languages, Greenberg's interest in typological questions arose because of his interest in how individual languages fit into the bigger picture. For him, the minute details fit into a bigger picture in a way they did not among most of his contemporaries. And while many linguists of later generations have been interested in the big picture, in one sense or another, these linguists have rarely had the love of minute detail that Greenberg had.

Greenberg's interest in large-scale genetic classification is also a reflection of his interest in the big picture. Many linguists seem to see this as a different Greenberg from the Greenberg who is the father of modern typology. But to those who knew him, it is obvious how these two interests of Greenberg were manifestations of the same general interest in the big picture. In both cases, Greenberg examined huge amounts of data and saw patterns in that data, one

set of patterns involving typological generalizations, the other set of patterns involving specific sound–meaning correspondences.

As a linguist who is somewhere in the middle regarding his work on genetic classification – skeptical but not hostile – I have never been sure what to make of the patterns of the latter sort that he believed he saw. As a typologist, I know that looking at lots of languages gives one a sense of what languages are like and that it means that I can often look at a language and see what is going on in ways that a specialist in the language who is not typologically informed just cannot see, no matter how well they know the language. For the same reason, I recognize that only by looking at lots of languages can one see patterns reflecting genetic relationship, and that the expertise that specialists in particular groups have is largely irrelevant to questions of how those groups fit into higher levels. Sure Greenberg sometimes got the details wrong. But an Africanist once commented to me that in Africa further research often showed that the details Greenberg cited to support a particular classification turned out to be incorrect or irrelevant for one reason or another, but that the same research almost invariably found new evidence that supported the very classification Greenberg had proposed. Many critics also fail to realize that while the standard way of conceptualizing the genetic classification of languages is from the bottom up, Greenberg's view was top-down, as it is for biologists: he looked at the whole world and asked the question: what are the subgroups? His work in particular regions of the world was only part of a larger project to classify all of the world's languages. If it is no surprise that he never completed this project, it certainly took unusual courage to try.

The only time Greenberg offered criticism of my work – gentle criticism – it was to say that he thought I should spend less time worrying about methodological issues (notably issues relating to sampling and ways to test typological generalizations) and more time on substantive issues. It is this same difference between Greenberg and me that underlies my skepticism regarding his work on genetic classification. I don't know whether the patterns of sound–meaning resemblances he documents are any better than chance. I once asked him whether it wouldn't be worth it to examine his evidence for Amerind statistically, to determine whether the patterns really are better than chance. He shrugged and responded in effect "Why bother? It wouldn't change their minds. It wouldn't convince them." I realized he was probably right. But it would have convinced me.

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A linguist without limitations

by LARRY M. HYMAN

A great linguist has passed away who leaves his giant mark on our field. Speaking personally as someone who knew Joseph Greenberg and was greatly influenced by his thinking, the loss is particularly acute. My awareness of Greenberg's great distinction began during my first year as a graduate student in a seminar on African languages: While most of the UCLA Department of Linguistics was celebrating the Chomskyan revolution in formal linguistics, Joseph Greenberg provided a second inspiration to those of us interested in Africa – specifically his classification of African languages, which, like Chomsky's generative grammar, was triumphing over earlier approaches. In short, we had two heroes.

Shortly thereafter, it became evident to me, however, that this personification of general vs. Africanist linguistics was not accurate. I discovered and read several of Greenberg's books: *Essays on Linguistics* (1957), *Universals of Language* (1963), *Language Universals, with Special Reference to Feature Hierarchies* (1966). This was truly great stuff, I thought, and why isn't it being taught in my phonology and syntax courses? I had to meet Joseph Greenberg – and for this purpose, I remember attending a session of the San Francisco LSA meeting in 1969 largely because the session chair was Joseph Greenberg himself. He offered comments and observations after a couple of papers that left a strong impression of the richness of language and of the vast exciting areas left to be explored. Several years later, at his 60th birthday party at Stanford, his distinguished colleague, the late Charles Ferguson, enumerated the many areas where Joseph Greenberg had been a pioneer in linguistics – not just typology, language universals, linguistic change, anthropological linguistics – but also psycholinguistics and sociolinguistics. Ferguson went on to say that everything Joe Greenberg touched turned into gold. He then amused his audience by wondering out loud how anyone could have possibly gone into something as "dull" as language classification! We all laughed.

The first time I attended a talk of Joseph Greenberg had also left a strong impression on me. In 1972, Greenberg had been invited as one of several senior scholars to speak in a forum at a Summer Linguistics Program at the University of California, Santa Cruz, where the goal was to tell the audience, mostly students, how they had gotten into linguistics and how they do their work. The speaker surprised us by saying that as a student he had not taken, but only audited courses in linguistics. A soft-spoken person, Greenberg made light of his assigned charge: "So," he added, "if I am supposed to tell you how to succeed in linguistics, I guess I'd say, get a PhD in Anthropology and audit a few linguistics classes." When he turned to address his way of working, Joe went on to

say something else that stuck with me. He talked about how sometimes when you read something, you're not ready for it. You read it, but it doesn't register. Later, after you've worked and thought further about the problem and arrive at your "own" position, you say to yourself, "Oh! Is that what so-and-so meant?" He added that he had this relationship with Roman Jakobson.

I got to know Joseph Greenberg from the trips I made to Stanford, and from workshops that we both attended. One of these was the word order conference at the University of California, Santa Barbara, in 1973. After my paper, in which I suggested that the distribution of the two orders of possessive pronoun + possessed noun in Niger-Congo languages was largely areally determined, we lined up for a buffet lunch. Alan Bell, a former student of Greenberg's, said to me, "Larry, you know, Joe Greenberg presented all that to us in one of his seminars." At this point I realized that Joe was right behind me and had heard. I looked at him, and he said in his soft voice, "Yeah, but I didn't publish it."

I was quite struck by his giving me the green light in this way and by subsequent collegial actions of his towards me. In 1974, when I was doing a crosslinguistic survey of how languages assign word stress, Joe shared a draft of a paper of his on the subject. Later he asked me to contribute to a multi-volume collection on universals, where he gave me carte blanche, in terms of content and framework. With respect to his attitude towards linguistic frameworks, Joe is supposed to have said to a fellow Africanist in 1972, "Formal frameworks determine the questions that you ask. I don't like such limitations."

This is the Joseph Greenberg that we have lost: an original scholar not subject to anyone else's limitations, an inspiring researcher who followed his own instincts, and expressed his convictions in a professional and humane way. As other colleagues throughout the world, I will continue to cherish Joseph Greenberg's work and the personal contact I had with him. Even when I went to see him during his illness, when it was clear that he was near the end, the sparkle was still there in his eyes that I had observed in each memorable encounter.

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Under Greenberg's wings

by CLAUDE HAGÈGE

When I read Joseph H. Greenberg's first works in linguistic typology, I immediately realized that he was showing me the way to follow. Since childhood, I had spent a lot of time asking people about various languages and establishing word-lists. But it was Greenberg's work which, much later, taught me what one has to do if one wants to become a typologist.

I have long been surprised that the morphological typology proposed by Greenberg (1954, 1957b) in order to improve on that of Sapir (1921) has not inspired as much research as it should have. Perhaps the ten indices introduced by Greenberg and enriched by a numerical taxonomy, while they open the way to fine-grained distinctions, were felt to be too complex to work with. It is also true that Greenberg lumped together, like Sapir, the isolating technique on the one hand, which refers to the relationships BETWEEN words, and the symbolic (affixation and/or internal changes), agglutinative, and fusing techniques on the other, which refer to the relationships among the constituent elements WITHIN a word. However, I realized that since isolating languages cannot, by definition, combine elements within words, Greenberg's method was the only possible one.

In fact, I was delighted by Greenberg (1954) because it clearly showed how diverse the morphologies of human languages can be. But what about the differences between languages with respect to the organization of utterances and the way meaning is reflected in the structural properties that underlie this organization, i.e., in syntax?

The syntactic information I had gleaned from various works which I had read before discovering Greenberg was rather scanty. Weil (1844) compared two types of word order, transitive verb + nominal object (VO) and nominal object + transitive verb (OV), and viewed the former as easier for comprehension. Schmidt (1926) claimed that the decisive factor was the position of genitive (G) relative to its head noun (N). I found this interesting, and I also realized that preposing or postposing the object to the verb does not result in a fundamental change of meaning (although it implies stylistic and pragmatic differences), whereas, if the NG or GN order is permuted, the meaning gets transformed. But despite this particular point, Greenberg (1963) appeared to me as far and away richer than everything I had read before. Indeed, I had not found much more inspiration in Frei (1929), Bally (1932), and even Tesnière (1959), than in Weil and Schmidt. I thought it was not sufficient to note, as Frei did, that most languages which order the verb before the object have prepositions, as opposed to postpositions in OV languages; nor was my hunger allayed by the few words of Bally on "anticipatory" (dependent–head)

and "progressive" (head–dependent) word orders, or by Tesnière's typology, where these orders were called, respectively, "centripetal" and "centrifugal", each of these being pure or mixed, which resulted in distinguishing four types of languages.

I was therefore amazed by the accuracy and methodological rigor of Greenberg (1963). His teaching was new to me, because I felt it made it possible for the first time to characterize every language as belonging to a definite word order type. I was even relieved, because I had been puzzled by an earlier text, which said (Greenberg 1957a: 36): "The order of meaningful elements may be considered a formal characteristic, like sound. In syntactic constructions only two possibilities usually occur in the arrangement of forms, A either preceding or following B, as contrasted with the numerous possibilities of sound combinations. Hence ARGUMENTS BASED ON WORD ORDER ARE OF MINOR SIGNIFICANCE [my emphasis – CH]. This is all the more so because the kinds of constructional meaning which may be significant are necessarily small, e.g. dependent–genitive or actor–action. Historically unconnected occurrences of such resemblances are therefore extremely likely and heavily documented."

An aspect of Greenberg's genius is that six years later he went beyond this seemingly disappointing situation. The evolution can be explained. The irreversibility of time is reflected by the linearity of speech. Human societies, in building their languages, have always taken as much advantage as possible of the necessarily very scarce means afforded by linearity. One may presume that Greenberg was fascinated by the strategies used in languages to overcome that difficulty, so to speak. Thus his interest shifted from isolated facts of word order, which can provide a basis only for "arguments [. . .] of minor significance", to statistically dominant sequential harmonies (summed up in Appendix II of Greenberg 1963), which nobody before Greenberg had so accurately brought to light.

It is another aspect of Greenberg's genius to have always tried to associate linguistics with several fields of research: anthropology, psychology, sociology, logic, biology. Thus for example, at least twice in his career, Greenberg viewed his work as an answer to questions asked by specialists of other social sciences who were interested in linguistics. During the Bloomington Seminar held in the summer of 1951, Charles Osgood had said that "while linguistics had an admirable and well worked out method, it was being applied merely to the description of individual languages. Could the linguists present tell him anything about ALL languages? That would be of the highest interest to psychologists" (Greenberg, Ferguson, & Moravcsik 1978: v).

We know that the answer came at Dobbs Ferry in 1961 (Greenberg (ed.) 1963). But already in 1957, Greenberg had striven to meet the expectations of other social scientists. In the Preface one can read: "I have written chiefly for

those anthropologists, in whatever branch of the subject they are engaged, who, because of their interest in cultural theory, are aware of the significance of so fundamental a human trait as language to any general science of man."

In fact, Greenberg went far beyond anthropologists' wishes. He inaugurated a period of unprecedented interest in typological research. This is because in his work the relationship and complementarity between the latter and the search for universals appears in full light, which was not the case before. Morphology is perhaps, with phonology, the most "linguistic" component of human languages, since it contains a set of forms which belong to the specific core of every language; these forms are always different even between very closely related languages, and this is what often makes them untranslatable. As a result, morphological typology cannot lead to language universals. Moreover, basing typological research on generalizations over the greatest possible number of languages permits one to account for changes of types and, therefore, to answer the objection that a typological approach to universals is confined to synchrony (cf. Greenberg 1995).

Such are some of the reasons why no linguist interested in language types could remain unaware of the work of Greenberg. Of course, it can be considered a rare privilege to have also met Greenberg in person. Unfortunately, I met him only twice. But both of these brief encounters were utterly illuminating. On these occasions I received so much encouragement from Greenberg that this motivated me to work ever harder. The first encounter was in 1974, at Stanford, where I was attending the Fifth Conference on African Linguistics. I knew that Greenberg was to come, and I was eager to meet him, after all I had heard about him from my Africanist colleagues and friends Talmy Givón, Larry Hyman, and Paul Newman. Greenberg, reading my name on my badge, said to me: "Oh! Tikar is interesting!" I could not decide what was more stunning. Was it the fact that Greenberg immediately identified me as the author of a modest booklet (among so many others that Greenberg knew!) concerning a Bantoid language on which I had done fieldwork? Or was it the suggestion, implied by these few words, that it was worthwhile to go on working on African language classification? In fact I did not believe that I could bring very much to that issue. I had simply adhered then, like the overwhelming majority of Africanists, to the new quadripartite classification of African languages, with which Greenberg had challenged the venerable British and French authorities in that field when he was not yet 40 (1955, written between 1949 and 1954).

The second encounter took place in 1975, at the Sixth Conference on African Linguistics, held at Ohio State University in Columbus. I was then working both on Adamawa-Eastern languages of North Cameroon and on Chinese. I asked Greenberg about an interesting point in Chinese. This language is explicitly mentioned in Greenberg (1963: 71) as an exception to Universal 24,

since the relative clause precedes the noun but the language is prepositional. I respectfully pointed out to Greenberg that in fact Chinese prepositions are the result of the grammaticalization of verbs. Moreover, it seems that Chinese confirms, rather than contradicts, Universal 24, since it also has postpositions; but these postpositions have a nominal source: the noun from which they are formed by grammaticalization is preceded by the dependent noun just in the same way as nouns are preceded by relative clauses. Greenberg listened with a smile. He simply answered: "Yes, one should associate typology with diachrony".

This kind answer emboldened me. I asked him whether he had not been disappointed by an article I had sent him some months before, and in which I had coined the term "logophoric" pronoun and introduced the corresponding notion. Greenberg only said: "This notion might turn out useful for African languages, and perhaps beyond". On my way back home, I had the impression that Greenberg was guiding me across the Atlantic, even though he scarcely knew me and even though I had never been his student.

Several years later, I did fieldwork on a Salishan language, which led me to be very interested in the masterly book Greenberg wrote in 1987 on Amerindian language classification. Just as I had found a source of inspiration and a model in Greenberg's classification of African languages, I read the 1987 book with enthusiasm. I immediately thought that Greenberg was not only a brilliant observer of details, but also a visionary, and that it was probably that which caused the envious feelings of those who poured streams of violent attacks on him. I know he was dismayed at these attacks, although he did not remain silent (see, for example, Greenberg 2000).

It is interesting to note that when Coseriu (1974: 47) lauded Greenberg's work as a useful reaction against Saussure's (1916: 134–135) excessive caution towards the panchronic viewpoint, he expressed the general assent of structuralist linguists, who welcomed Greenberg as a turning-point. But the structuralists were not the only ones to recognize the novelty of Greenberg's teaching. The generativists did, too. In the beginning, they regarded Greenberg (1963) as evidence for calling into question Joos's well-known remark on phonology but applicable more generally (Joos (ed.) 1958: 96):

> [...] new ideas [...] were coming out of Europe, specifically from the Cercle Linguistique de Prague. American linguistics owes a great debt to that stimulation; but in the long run those ideas were not found to add up to an adequate methodology. Trubetzkoy phonology tried to explain everything from articulatory acoustics and a minimum set of phonological laws taken as essentially valid for all languages alike, flatly contradicting THE AMERICAN (BOAS) TRADITION THAT LANGUAGES COULD DIFFER FROM EACH OTHER WITHOUT LIMITS AND IN UNPREDICTABLE WAYS [my emphasis – CH], and offering too much of a phonological EXPLANATION where a sober TAXONOMY would serve as well.

It soon became clear, however, that although in the beginning Greenberg had met with the generativists' approval, his universals, based on typological research, had little to do with the deep structure universals of the Chomskyan paradigm, which were to become the core of Universal Grammar. Two years after Greenberg (1963), Chomsky wrote (1965: 118), rather condescendingly: "Insofar as attention is restricted to surface structures, the most that can be expected is the discovery of statistical tendencies, such as those presented by Greenberg (1963)." It would seem that Greenberg's exciting findings either bring about immediate and enthusiastic acceptance or arouse animated discussion, in a field which had never been so thoroughly and so systematically studied before. Even in generative linguistics, the once denigrated Greenbergian implications would eventually gain respectability in the guise of parameters.

Language lovers cannot but feel happy to see that Greenberg has been recognized as one of the leading linguists of the twentieth century. The reason for that is very simple: Greenberg loved languages. He used to spend five or six days a week in the Stanford University library, and "was such a fixture that several years ago the library staff put up a plaque with his name over the table where he worked". Croft (2001), who recalls this fact, adds that in his retirement speech in 1986, Greenberg "attributed his love of knowledge in part to his Jewish heritage".

Greenberg deserves the thankfulness of the whole community of linguists for what he brought to our field. 1963, the year when his most seminal article was published, happens to be the date of publication of a book written by Louis Hjelmslev, two years before his death. In this book, Hjelmslev wrote (1963 [1970: 96]): "Only through typology does linguistics rise to quite general points of view and become a science." This applies to Greenberg's work (cf., for example, Greenberg 1970) more than to any other. The solidity of Greenberg's methodology as well as the passion which pervaded his scholarly life are the two components that make a scientist. From this point of view too, Greenberg remains a model.

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Joseph H. Greenberg: A universal linguist

by HANSJAKOB SEILER

The quest for the essence of language may be approached from two sides: from inside, i.e., by internal evidence, or from outside, i.e., by external evidence. (On this distinction see Jakobson 1973: 25–26, and, more recently, François 2000:

461–462.) By internal evidence I mean evidence that is gained from a scrutiny of language data on the basis of linguistic theories and by applying linguistic methods – avoiding a *metabasis eis allo genos*. By external evidence are meant insights and methods proper to the study of adjacent fields as applied to language phenomena with the purpose of enhancing the understanding of these phenomena. Among adjacent fields we may cite philosophy, formal logic, mathematics, or general systems theory.

Most linguists probably prefer to be on the safe side, confining themselves to the search for internal evidence. The other approach, going beyond linguistics, is fraught with many pitfalls. Yet, it seems quite natural that a deeper insight into a semiotic system such as language should gain from a contrastive view on other semiotic systems in the realm of human intellectual activities. The ideal would be to draw on a maximum of external clues without, however, leaving the field of linguistics.

Joseph H. Greenberg's art of doing linguistics comes remarkably close to this ideal. His achievements, as far as internal evidence goes, are well known, and I can be relatively brief on this. Yet, his extraordinary command of quite a number of "working languages" deserves special mention – languages other than English, in which linguistic literature is published: Russian, German, French, Spanish, etc. This, in turn, enabled him to draw not only on present-day publications but also on literature temporally remote: Wilhelm von Humboldt, Raoul de la Grasserie, etc. Next, we ought to note his familiarity with Semitic languages, foremost Arabic in its varieties, which afforded him first-hand access to the rich tradition of the Arab grammarians. What further deserves mention is his ease in perusing grammars of languages around the globe and thereby ever so often detecting the very points of general interest. One may wonder if this had been possible without this vast fund of insights into external fields which he had at the ready. This, then, brings us to the dialectic between internal and external evidence.

Among his most seminal "inventions" were implicational generalizations with their tetrachoric tables designed in analogy to formal logical practice. The notion of harmonic vs. disharmonic relations is obviously connected with the psychological concept of generalization (Greenberg 1963: 76). One of these 45 "universals" that from the outset attracted my special interest was No. 20 (1963: 68): "When any or all of the items (demonstrative, numeral, and descriptive adjective) precede the noun, they are always found in that order. If they follow, the order is either the same or its exact opposite." It marked the start of my work on continua: by completing the above series with further items such as articles, possessives, evaluating adjectives, descriptive adjectives, etc., I showed that positions to the left of this linear ordering were marked by increasing referentiality and decreasing content, while those to the right had increasingly to do with the lexically inherent content of the noun and with decreasing referen-

tiality (Seiler 2000: 44–53, with references to earlier publications). Continuum eventually became the key notion of dimensional universality. When Greenberg saw me at the plenary session of the Eleventh International Congress of Linguists in Bologna (1972) I had barely begun to outline these ideas. He nevertheless greeted me as a "comrade" and welcomed the Cologne UNITYP Project as a sister project to his.

In his fundamental article on "the diachronic typological approach to language" (Greenberg 1995) he refers to notions from quite a number of adjacent fields. The overall theme there being the ways in which a language in one state can change into another, he sets out to discover "what communicative, logical, psychological and external factors may be at work to produce similar processes in different instances of typological change". General systems theory with its notion of "strong connection" is advocated as shedding light on the ways in which synchronic typology places constraints on language types and on the ways in which a language in one state can change into another.

In order to illustrate how Greenberg went about defining some essentials of language by looking beyond it, I shall point to one of his earlier and lesser known pieces of work, an article entitled "The first (and perhaps only) nonlinguistic distinctive feature analysis", which appeared in the festschrift for André Martinet (Greenberg 1967). "Phonological distinctive feature analysis", he says, "is representative of a highly general scientific model and therefore one of potential applicability to any domain whatsoever, linguistic or nonlinguistic". In fact, he notes Aristotle's doctrine of the four elements of his physics, earth, air, water, and fire, as resolvable into combinations of two binary oppositions, hot–cold and moist–dry (quoted from *De generatione et corruptione* II, 2, translated by H. H. Joachim, in Ross (ed.) 1930). The "elementary qualities", hot, cold, moist, and dry, can be equated with features. As Aristotle notes, hot and cold cannot be combined, nor can moist and dry. Hence they are values on the same dimension. Equally, a phoneme cannot be distinctively voiced and unvoiced or nasal and non-nasal at the same time. His "simple bodies" Earth, Air, Fire, and Water correspond to phonemes and each is a combination of two binary features:

	Fire	Air	Water	Earth
Hot–Cold	+	+	−	−
Dry–Moist	+	−	−	+

In the passages quoted, Aristotle develops the diachronic implications of this theory: a change involving both features at the same time, i.e., that between contraries, "though possible, is more difficult". The normal change would be from one to another correlative pair by replacement of a single quality through its opposite – which foreshadows Greenbergian diachronic typology. Noting that, after all, Aristotelian physics did not turn out to be a valid theory of mat-

ter, Greenberg is looking for other non-linguistic analogues of distinctive feature theory: atomic chemistry, gene theory, the mathematical method of factor analysis are successively examined, but on closer inspection turn out not to be exact analogues. Being thus forced to the position that there is, after all, something peculiarly linguistic about this mode of analysis, Greenberg finds two main characteristics: discreteness (in contrast to the continuity of the physical stimulus) and dimensionality. This, one might say, is a highly significant result.

Joseph H. Greenberg was a linguist of both universal and global format. Keeping this in mind, some of us might rethink our attitudes toward his work on the genetic classification of the languages of the Americas as well as on his essays in global relationships. This is certainly not the place to rehash these hotly debated issues. Instead, I want to close on a more personal note.

Joe Greenberg, besides being such a great linguist, was also an accomplished piano player; so I was told by some friends. I didn't hear him perform; but when I visited him at his home, I saw on his grand piano a stack of musical scores the execution of which would require a high degree of technical proficiency: Chopin, Brahms, and the like. A few years later I met him at the 1980 Linguistic Institute in Albuquerque. He reported on the results of his multiple or mass comparisons, letting us examine his extensive notebooks. He was sharply criticized by Indo-Europeanists and others dealing with genetic classification and protolanguages. After the lecture I went up to him and told him: "Maybe they are right on their own premises and on their own methodology. But I am certain you will be right on a different level of our science. And on top of it all you have your music." "Yes", he said, "and next time you come we shall play a Brahms sonata."

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Ahead of even Greenberg, for once: Paul ("Person") Forchheimer

by FRANS PLANK

1. Out of Africa

Early in his career, when he first made history (and, initially, few friends) with a bold new genealogy of African languages, Joseph Greenberg was already ahead of his time, in attitude if not yet achievement, in another comparative enterprise too, namely typology. This early involvement of his is not, I believe, public knowledge, but it is, perhaps, a story worth telling. It is the improbable success story of a book – though not in fact, strictly speaking, a book of Greenberg's.

2. The Category of Person in Language

The Category of Person in Language by Paul Forchheimer, published in 1953 by Walter de Gruyter, is nowadays standardly quoted in typological circles. With that categorical sort of title, it was guaranteed to make it into the references whenever person was one's language-particular or crosslinguistic subject. Those of us who have actually read it (it is in English, though on an unmistakably German substratum) tend to refer to it also under the rubric of number, because it is about the number marking of personal pronouns, also touching on number in nouns.

Joseph Greenberg surely knew what the book was about, referring to it, in the foreword of a collection on pronominal systems, as one of the three previous general typological studies that he was aware of of the cardinal species of pronouns, the personal ones (Greenberg 1986). One of the two other relevant studies cited by Greenberg, David Ingram's (1971 [1978]), itself done under Greenberg's supervision in the universals project at Stanford, heavily drew on it too. When grappling with a person category that is ambiguous as to its number, the 1st person inclusive "dual" (Greenberg 1988), Greenberg specially perused Forchheimer's book, covering 71 languages (on Greenberg's count: others have counted more, but there is no index nor an appendix listing the sample), to see how duals would typically distribute over the paradigms of personal pronouns.

In bare essence, *The Category of Person in Language* sets out the following taxonomy of the overt means for forming plurals and other non-singulars (dual, marginally trial, and dubiously quattral) of personal pronouns, with subtypes distinguished depending in particular on how nouns are usually doing as to number:

(a) MORPHOLOGICAL (typically by affixation, or also internal modification or reduplication; schematically: *I – I-s*):
  (i) no or only optional plural with nouns;

(ii) same plural morphology as with nouns;
(iii) different number morphology for pronouns and nouns;

(b) LEXICAL (i.e., suppletion; as in English: *I – we*):
(i) lexical plural only in 1st person, no plural in other persons nor on nouns;
(ii) lexical plural only in 1st person, morphological plural in other persons and on nouns;
(iii) lexical plural for 1st and 2nd person, or for all persons, morphological plural elsewhere (i.e., on nouns and perhaps 3rd person pronoun);
(iv) two lexical plurals for 1st person, differentiating inclusive and exclusive (actually, Forchheimer was in doubt whether 1st person inclusive really was non-singular: in most of his languages it grammatically behaved like a singular and only in a minority like other plural forms);

(c) COMPOSITIONAL (schematically: *I, you – I-you*):
never wholly compositional non-singular paradigms,
but only 1st person, or 1st person exclusive, or 1st exclusive and inclusive as the only composite forms,
or complete set of morphological or lexical plural pronouns plus composite forms;

(d) MIXED, with different means used for different persons or also for one and the same person:
(i) lexical and morphological plural in 1st person, for inclusive and exclusive respectively;
(ii) morphological or lexical plus composite plural for 1st person.

Despite the emphasis on person in its title, the book's central theme is how number differentiation – in its three formal realizations: more or less regular inflectional morphology, suppletion, composition – extends across the pronominal and nominal domain, of all those languages where number is grammaticalized to begin with. (While person, conceived of as deictic reference to speech-act roles is assumed by Forchheimer to be universal, number in Forchheimer's view is not.) Without specially highlighting his most important result in the introduction or in conclusions, Forchheimer in this respect finds system rather than chaos. Essentially, the extensions of number differentiation, for any language, can be expressed in terms of this one-dimensional scale – by now perhaps the most familiar construct in typology, however named (hierarchy of animacy, individuation, agentivity, empathy, ego-distance, topicworthiness, or indeed numberworthiness):[1]

1. Forchheimer was not the first nor the last to invoke this referential hierarchy for this particular purpose. See Plank (1987: 181) for a list of some predecessors and successors, the latter notably including Smith-Stark (1974).

PRONOUNS			>	NOUNS
1st	>	2nd > 3rd		kin > persons > animates > objects
inclusive > exclusive				

On Forchheimer's evidence, variation across languages is limited insofar as number differentiation can extend across its domain from either the pronominal or the nominal end, though it far more commonly extends from the left, and though it will always extend from the left when it takes lexical or compositional form, with 1st person especially numberworthy in these modes. Wherever its point of departure, the stretch covered on the scale will as a rule be continuous, with lexical number differentiation not uncommonly remaining confined to 1st person. The scale is independently motivated by the semantics and pragmatics of reference: on its left are the most deictic (pointing) NPs and on its right those most descriptive (naming), and as deicticity is decreasing, descriptivity is increasing, and vice versa. Subjectivity and objectivity is another pair of opposites invoked by Forchheimer as structuring referential space along the same lines.

As used by Forchheimer, "extension" (his preferred term is "spread") is ambiguous between a synchronic (or rather timeless) and a diachronic reading. On the first, any language, at any stage of its development, is supposed to abide by the constraints on the distribution of number differentiation over types of NPs imposed by the deicticity/descriptivity scale. At heart, however, Forchheimer favours a diachronic way of making sense of the patterns of permissible distributions. Since number is not a universal category, it has got to have been innovated in the languages that have it; in the course of time, it is then subject to extensions from its point of origin, and conversely also to contractions and loss. Seen from this angle, the distributions of number differentiation in any language at any given time will fall out as the momentary results of grammatical change, taking as its ultimate input forms lending themselves to the grammaticalization as markers of number. And if not all logically conceivable distributional patterns are encountered, this will have to be due to constraints on relevant changes and on possible source forms.

In the course of the book, not always very explicitly and again without due systematic summary anywhere, Forchheimer recognizes these sources of nonsingular number markers and these ways of changing number marking:

(a) sources:
  (i) number words accompanying pronouns or nouns, such as quantifiers, numerals, collective nouns, semantically plural nouns, also reduplication;
  (ii) grammatical forms for categories related to number, such as collective or distributive;

(b) changes:
  (i) grammaticalization of number words, essentially univerbation;
  (ii) composition of singular or also plural pronouns to yield duals, plurals, inclusives, exclusives;
  (iii) borrowing of number, including calquing on substratum patterns;
  (iv) reanalysis of grammatical forms for categories related to number (perhaps only rather distantly, such as object forms of 1st person singular pronoun) as non-singular number forms;
  (v) analogical extension of existing grammatical number markers, including that of number-marked pronouns themselves, to other pronouns and to nouns;
  (vi) reanalysis of number-marked forms as another number, especially semantic specialization of plural;
  (vii) contextually sanctioned secondary uses of singular forms with plural meaning, of ("polite") plural forms with singular meaning;
  (viii) phonologically induced loss of morphological transparency, transforming morphological into lexical markers;
  (ix) phonologically induced loss of number-distinctive morphology;
  (x) discontinuation of number markers.

This diversity of possible sources and changes, potentially affecting individual forms of individual words, should make for considerable diversity of the patterns of number differentiation across types of NPs – unless diachrony were superintended by typological constraints on inflectional systems, valid at any and all times. Although this does not come across as being a major concern of Forchheimer's, he does posit at least three diachronic laws, which would rein in change and thereby constrain resulting systems:

(a) analogical extensions of number marking proceed along adjacent positions on the deicticity/descriptivity scale;
(b) when new plural forms are innovated, competing with old plural forms, the new forms will become the general plural (multal) and the old forms will be semantically specialized as paucal, dual, plural inclusive, or exclusive;
(c) composite forms for plural or 1st person inclusive or exclusive are usually only created as calques on substratum patterns.

What is missing most sorely, in view of the main thrust of the book, is a plausible account of how lexical (suppletive) plurals of pronouns (those of nouns remain unmentioned) originate non-phonologically, i.e., how different stems diachronically come to be combined in one paradigm.

En passant, Forchheimer suggests several implicational universals of more marginal relevance for his main theme, though most do centre around the deicticity/descriptivity scale; some are adopted or adapted from earlier authorities, notably Father Wilhelm Schmidt:

(a) If there is a 3rd person, then there are a 1st and 2nd person too, conceptually (regardless of how expressed), but not vice versa.
(b) If there is verbal marking for 3rd person, then also for 1st and 2nd person, but not vice versa.
(c) If there is a special class of pronoun for 3rd person, then also for 1st and 2nd person, but not vice versa.
(d) If any pronouns are similar in form and inflection within a language, then it will be those for 1st and 2nd person rather than those for 3rd person and 1st or 2nd person.
(e) If pronouns for 3rd person are shared between related languages, then so are pronouns for 1st and 2nd person, but not vice versa.
(f) If there is differentiation for class, gender, location for 1st person, then also for 2nd; if for 2nd, then also for 3rd person, but not vice versa; if for plural, then also for the corresponding singular person. (Here Forchheimer is aware of exceptions, though, such as Shilha, with masculine vs. feminine only in 1st person plural, or Spanish *-otros*/*-otras* only in 1st and 2nd person plural.)
(g) If there is differentiation for subject–object case for 3rd person, then also for 2nd person; if for 2nd person, then also for 1st, but not vice versa.
(h) If any pronominal distinctions (e.g., person, number, inclusive/exclusive) are made for oblique forms, then they are also made for direct forms, but not vice versa.
(i) Provided there is a differentiation of subject and object forms of personal pronouns, the preferred use for subject forms in subject function is in the imperfective aspect and/or in present/future tenses rather than the (less subjective) perfective aspect and/or past tense; in perfective aspect object forms can be used indiscriminately in object and subject functions.
(j) If there is a dual, then there is also a plural.
Diachronically: Plural develops before dual, which in turn usually develops from plural by semantic specialization of forms in competition with new plurals. (*Pace* Father Schmidt, Forchheimer finds no correlation between a dual and an inclusive-exclusive contrast, typically originating in the same specializing manner.)

The structural classification of languages by the criterion of morphological vs. lexical number distinction of personal pronouns, although not historically immutable, seems to Forchheimer to coincide remarkably well with genealogical classifications. As he sees it, morphological plurals occur in Sino-Tibetan, Ural-Altaic, Eskimo-Aleut, in some Paleo-Asiatic, a number of American Indian, and some Ancient Near Eastern languages, while lexical plurals prevail in Indo-European, Semitic, Hamitic, most African, many American, some Siberian, some Australian, some Papuan languages, and in Malayo-Polynesian.

Underlying the genealogical distribution of his main types he suspects an areal one, with the once solid block of morphological-plural families partly disrupted by the advance of Indo-European. (By contrast, and again *pace* Father Schmidt, areal patterns are harder to discern for inclusive/exclusive; this comes as no great surprise to Forchheimer, who considers this opposition diachronically rather unstable.) To make sense of the geography of person-number systems, Forchheimer reckons with astonishing migrations – of peoples, grammatical features, or both. For him, substrata are omnipresent as a powerful force shaping pronominal paradigms through calquing.

The slim book concludes with a page and a half of speculations about the use of certain consonants for 1st and 2nd person pronouns (nasals and dentals, both apparently alternating with velars), which seem so widespread that Forchheimer suspects it might have great historical significance.

3. Persona non grata

Now, what is so remarkable about a book from 1953 being a standard reference some 50 years later? Did it earn its longer than average shelf-life by asking questions and offering answers that continue to matter to the profession or beyond? Or is it a modern classic, routinely quoted out of piety towards the author or sheer habit?

Had it gone by the contemporary reception that it got, *The Category of Person in Language* should have sunken into immediate oblivion. The reviews, especially in the US, were devastating, and often unusually *ad hominem* (or indeed *ad homines*, also including Giuliano Bonfante, who had contributed a foreword): "queerly anachronistic" (Stanley Newman); "strange, even futile goals and ideals, characteristic of the amateur", "weirdly schizophrenic air" (Fred Householder, Jr.); "Forchheimer donne l'impression d'avoir caressé un rêve et d'avoir reconnu qu'il était impossible" (Eric Buyssens); "grenzelose onvoorzichtigheid, die [...] bizarre vormen aanneemt" (boundless incautiousness, which takes bizarre forms), "verregaande slordigheid warmee de auteur omspringt met zijn bronnen" (exceeding carelessness of the author with respect to his sources) (Andries Teeuw); "appalling for someone raised in the Boasian tradition", "at worst an obstacle, at best irrelevant to the growth of sound knowledge about linguistic relationships, be they typological, areal or genetic", "perhaps so weak a book should not have been written" (Dell Hymes); "a satire on unsound linguistics", "a deliberate burlesque", "a hoax" (Raven McDavid, Jr.). The few other voices expressing a polite interest, in all brevity, pale into insignificance by comparison.

All reviews I am aware of are listed in the References below. The *New York Times Book Review* would only review books available through a US distributor. When Forchheimer argued with the editor that his in a sense was, since

copies could be obtained from a New York trader in books and periodicals, he hoped to strengthen his case thus:

> Written in the Humboldtian tradition, but with the benefit of modern research, it merits a review because it is one of very few modern books that deal with the problem of human speech as such. It elucidates the previously untouched problem of the expression of person, and, as a main contribution to linguistics and psychology, it has the first consistent theory of the origin and development of number, i.e. plural and dual.
> I should greatly appreciate if you could decide to review it. Should no one on your staff be prepared for it, may I suggest Professor Mario Pei of Columbia or Prof. Robert A. Fowkes of New York University? (Prof. Albright of Johns Hopkins is familiar with the book. Prof. G. Bonfante of Princeton has written the introduction.)[2]

Mercifully, the *NYT Book Review* appears to have remained unmoved.

In substance, Forchheimer's critics raised the following kinds of objections. First, for languages where the critics professed some expertise (and every one of them had a particular family as his specialization, in apparent contrast to the author under review), Forchheimer would sometimes rely on sources that contemporary experts would consider not-so-reliable or not up-to-date. Second, Forchheimer (or De Gruyter's printers) would, sometimes or frequently, misreproduce data from published sources, with diacritics and other special symbols as the most common source of such errors. Third, inept at structural analysis, Forchheimer would, sometimes or frequently, especially for languages within the expertise of the critic, misanalyse data from published sources, especially when analysing them differently from the source. Most fatally under this rubric, he allegedly had no notion of grammatical category, imputing to all languages the categories of European school grammar, such as those of the three persons and two numbers. Fourth, Forchheimer would, inadvertently or even intentionally, fail to take into account relevant data from the sources he used, sometimes limiting himself to just one person paradigm (usually that of independent pronouns) where others in the same language would show different patterns. Fifth, Forchheimer would, sometimes or frequently, especially for languages within the expertise of the critic, misinterpret the significance of data that he reproduced and analysed accurately, often taking for fact what was a mere hypoth-

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2. Letter of 23 March 1953, Forchheimer file of Walter de Gruyter & Co. Mario Pei had become Professor of Romance Philology at Columbia in 1952. Robert A. Fowkes was Professor of Linguistics and Germanic Languages. William F. Albright's field was Biblical Studies, with a specialization in archaeology and Semitic languages. Giuliano Bonfante was briefly at Princeton's Romance Department, teaching historical linguistics. Why these, of all people, were suggested by Forchheimer as potential reviewers will become clearer as the present story unfolds.

esis. Sixth, Forchheimer would jump to conclusions on too narrow a crosslinguistic basis. Perhaps surprisingly, this objection was only raised once, by Klingenheben, the German Africanist. How the 71 (Greenberg), 80 (Klingenheben), 82 (Householder), or nearly a hundred (Newman, Forchheimer himself) languages had been selected which Forchheimer was comparing for his particular purposes evoked no comment; nor did the question of whether his inductive generalizations were warranted by his own sample, however small and unrepresentative. (Again, it was Klingenheben who at least alluded to the insufficiently inductive and overly deductive slant of Forchheimer's procedure.) Seventh, Forchheimer would make unwarranted or outright fantastic general assumptions, especially about historical changes and genealogical and areal relations. Here much blame also fell on the book's would-be promoter, Bonfante, who used the Foreword to extoll general over language-particular and areal over *Stammbaum* linguistics, listing a host of role models from Europe, nineteenth century up to the present.

On virtually all of these counts, Forchheimer's critics did have a point – if perhaps not always such a strong and lasting one as they themselves thought, for some of their own then superior language-particular expertise has meanwhile become anachronistic. What remained unappreciated was that Forchheimer had in fact done some, admittedly perfunctory informant work and, more importantly, had sought language-particular advice from several experts, all identified in the book. Certainly the charge that he did not recognize a person when he saw one, or rather that he recognized person when there was none, was unjustified: Forchheimer was aware of Southeast Asian languages where the deictics referring to speaker and addressee are not genuine pronouns but nouns-of-sorts; but as these nouns for social roles were metaphorically transferred to speech-act roles, they acquired deictic force on top of their descriptive meaning – and being such a "pointer" (or, if you prefer, "shifter") was Forchheimer's basic criterion for recognizing a form as an expression of the grammatical category of person, and of only three persons, regardless of further subdifferentiations of 3rd person (which some of his critics would rather have seen numbered "4th" etc.).

The question, then, is what all these points added up to, especially those well taken. Most reviewers concluded that grammatical cross-language and especially cross-family comparison as such, as undertaken by Forchheimer, was a sort of enterprise that was either out-of-date in the mid-twentieth century or unviable, currently or forever, in view of practical limitations of individual researchers. Those going for unviability differed in just how much blame to lay on Forchheimer himself for an especially poor showing or on generally adverse circumstances.

In an exceptionally constructive vein, Arthur Capell did the obvious, namely test Forchheimer's universalist claims against further crosslinguistic evidence,

recently unearthed by himself in the Western Desert of Australia. He found that the development of plural, dual, and inclusive-exclusive in the personal pronouns of these Australian languages, as he would reconstruct it, did not fit in well with parts of Forchheimer's scenario; but he granted that it may hold elsewhere. In fact, Capell's new evidence was not at odds with the basic idea of number differentiation spreading on the deicticity/descriptivity scale.

When Fred Householder, Jr. and Dell Hymes, among the harshest critics of Forchheimer but avowed sympathizers of the typological programme as such, sought to emulate Forchheimer, in superior "professional" style, the generalizations they suggested – reproduced below for the record – were mere restatements of Forchheimer's own or were not about the form and extension of number marking at all:

Householder's Laws (Householder 1955: 99–100)

(a) Person as a grammatical category is lacking very rarely.
(b) Genuinely two-person systems (speaker – others, addressee – others, non-speech-act participant – others) do not exist anywhere, except perhaps in Tasmania (speaker – others).
(c) The commonest system is 1st and 2nd vs. 3rd person, with 3rd unmarked. 1st and 3rd vs. 2nd, 2nd and 3rd vs. 1st are rare, except when coexisting with other patterns.
(d) Common expansions of pronominal paradigms add number, 1st inclusive (often structurally singular), obviative, reflexive, indefinite, "ye" [?], gender or class (although this is usually a separate category).

Hymes' Laws (Hymes 1955: 298)

(a) All natural languages distinguish in the pronominal system three persons in the singular, and singular and plural in the 1st person.
(b) Natural languages may distinguish two genders in all three persons, and both singular and plural; for any person, more frequently in singular than plural, and for any number, more frequently in 3rd than in 2nd, in 2nd than 1st.
(c) Natural languages may distinguish three genders (including neuter) only in 3rd person, and do so less frequently than two.
(d) Natural languages may distinguish four numbers, singular, plural, dual, trial, in order of frequency.
(e) Natural languages may distinguish inclusive-exclusive 1st person in three numbers, plural, dual, trial, in order of frequency.

What was the core of Forchheimer's book, however ineptly done, seemed destined to be lost on critics such as Householder and Hymes, however well-intentioned they may have been. But it is perhaps for that – the demonstration,

on a broad empirical basis, that the extension of number differentiation on the referential hierarchy is not random – that *The Category of Person in Language* has survived and continues to be cited.

4. Persona incognita

The book's latter-day success is unlikely to be due to the high esteem in which today's typologists hold the person of its author. For who of us knows who Paul Forchheimer was?

He was not a hoax. This is what the *Directory of American Scholars: Foreign Languages, Linguistics and Philology* (volume 3, 8th edition, 1982) and *Kürschners Deutscher Gelehrten-Kalender 1954: Lexikon der lebenden deutschsprachigen Wissenschaftler* (8th edition, 1954) tell you about him.

Paul Forchheimer was born on 25 July 1913 in Nürnberg (Franconia, Germany). He got an M.A. from New York University in 1939 and a Ph.D. in linguistics from Columbia University, New York, in 1951. He was principal of Beth Jacob High School, Brooklyn, New York. At Dowling College, Oakdale, New York, he was Assistant, then Associate Professor of Linguistics and German from 1963–76; from 1976 he was Emeritus Associate Professor at Dowling College and from 1973 also Visiting Associate Professor of Linguistics at Adelphi University, from which Dowling College (formerly Adelphi College) had split off.[3]

The *Directory of American Scholars* identifies his areas of research as "etymology; nature of primitive languages; theory of language change", *Kürschner* as "Allgemeine und vergleichende Sprachwissenschaft". This is his brief list of publications, culled from the *Directory of American Scholars* and *Kürschner*, and checked against other bibliographic sources:

Zu Morgensterns "Steinochs". *Modern Language Notes* 54 (1939): 198–199.

The semantic development of Hebrew *gerem*. *Word* 4 (1948): 209–211.

Klar wie Klössbrüh'. *Modern Language Notes* 64 (1949): 493.

Himalayan languages. In *Collier's National Encyclopedia*, Volume 10, 63–64. New York: Collier, 1950.

The etymology of *Saltpeter*. *Modern Language Notes* 67 (1952): 103–106.

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3. Here I cannot resist quoting Dowling College's President, quotes and all, going public on their homepage with this message (29 August 2001):

> As "The Personal College," Dowling College's mission is clear: To bring out the very best "Person" in each of our students through a sound academic curriculum, inspired teaching and an environment that accommodates and celebrates individual career aspirations.

The Category of Person in Language. Berlin: Walter de Gruyter, 1953. 142 pp.

French *glaire*. *Romance Philology* 18 (1964): 33–34.

Primitive language(s). In Robert C. Lugron & Milton G. Saltzer (eds.), *Studies in Honor of J. Alexander Kerns*, 29–34. (Janua Linguarum, Series Maior, 44.) The Hague: Mouton, 1970.

Apropos of the entry for 1950, Forchheimer is listed as a contributor to *Collier's Encyclopedia* (along Bonfante, Martinet, Greenberg, and other big names) also in the 1965 edition, the latest I have seen. Reluctant to do a complete search of 24 volumes, the only contribution I can attribute to him is "Himalayan Languages", referred to in *The Category of Person* (pp. 112, 117). But this is not retained in the 1965 edition, where no key or characteristic entries are by Forchheimer (like *Linguistics*, *Language*, *Languages of the World*, *Etymology*, *German*, *Nürnberg*; *Pronoun* is by Bonfante, who has nothing to say on the categories of person and number).

The Dowling College Library Catalog lists another work of Forchheimer's, though unpublished:

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Languages of man / by Paul Forchheimer. [S.l. :s.n.,]
                                        c1972.
Special Collections; P140.F67 1972
Description   445 leaves ; 28 cm.
Note          "This is a preliminary edition for the exclusive use of
              students in Linguistics 12 at Dowling College and may
              not be reproduced or sold out"--Cover.
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The same catalog also lists two of Forchheimer's publications on Jewish Studies, his other major field of interest:[4]

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Living Judaism: The Mishna of Avoth with the commentary and selected
other chapters of Maimonides, translated into English and
supplemented with annotations and a systematic outline for a modern
Jewish philosophy / by Paul Forchheimer. Jerusalem ; New York:
Feldheim Publishers, 1974. 240 p. ; 24 cm. [2nd, corrected edition as
Maimonides' Commentary on Pirkey Avoth, Jerusalem: Feldheim, 1983. 241 pp. – FP]
Main Stacks; BM506.A23 F66

Our miraculous world / by Paul Forchheimer. New York: [s. n.] ;
Lakewood, N. J.: Distributed by Chinuch Publications, 1983. vii,
48 p. ; 23 cm.
Main Stacks; BM538.S3 F67 1983
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4. That this is indeed the same Paul Forchheimer was confirmed by Wolfgang Schellinger. In correspondence with Walter de Gruyter he had expressed an interest in getting books at an author's discount in theoretical physics (Heisenberg, Planck, and such) rather than in linguistics.

Further proof of the professional existence of Paul Forchheimer as a linguist is given in the *Bulletin de la Société de Linguistique de Paris* 42-2 (1942–45): xvi–xix. On 16 June 1945, Paul Forchheimer (30, avenue Kléber, Paris VIII e) was elected a member of the Société de Linguistique de Paris, having been introduced by MM. Mossé and Mirambel at the preceding session on 26 May 1945. Upon his election he gave a paper on particular and general terms for principal animals in so-called primitive and developed languages. Comments by MM. Vendryes, Martinet, Sauvageot, Benveniste, Lacombe, Cohen. Years later, he would ask Claude Hagège to read short notices on various problems of the historical phonology and syntax of German, English, French, and Italian to meetings of the Société on his behalf, which Hagège did with great pleasure; "they were written in a very elegant and literary French". [5]

American learned societies also counted Paul Forchheimer among their members from the late 40s and early 50s onwards. The membership lists of the American Oriental Society, the Modern Language Association, and the Linguistic Society of America, as published in their journals or bulletins, give his name and address. The Linguistic Circle of New York, fashioned after the Société de Linguistique de Paris, did not publish a directory in its journal *Word*, but he was a member too. [6]

A few further particulars emerge from these autobiographical lines of Paul Forchheimer's:

> Ich bin in Nürnberg gebürtig und besuchte dort das „Alte" humanistische Gymnasium. Dort war aber schon einige Jahre vor Hitler so starker Antisemitismus, daß ich schliesslich in der Schweiz „Matura" machte. Dann bezog ich die Technische Hochschule in Aachen, wo ich drei Semester Chemie studierte. Im Frühjahr 1933 studierte ich ein Semester an der Eidgenössischen Technischen Hochschule in Zürich, dann ein Jahr an der École Supérieure de Chimie in Mulhouse. Ich ging dann nach England und stellte mich auf Pharmazie um. Das bedeutete ein Jahr als Provisor in einem Krankenhaus, dann weiteres Studium.
>
> 1937, als in Europa Krieg drohte, ging ich nach Amerika. Ich bekam ein Stipendium an der Deutschen Abteilung an der John[s] Hopkins Universität in Baltimore, aber hörte auch Vorlesungen in Romanischer und allgemeiner Sprachwissenschaft, Gothisch, Angelsächsisch, etc.
>
> Ich setzte diese Studien in New York fort. Dann kam Militärdienst. Nachdem ich Artillerie „studiert" hatte, wurde ich an die Universität Princeton geschickt, wo das Militär besondere Kurse eingerichtet hatte. Dort setzte ich mein arabisches Studium fort, dann kam Türkisch. Eine persönliche Freundschaft verband mich mit Prof. G. Bonfante, aber mit Prof. Hitti stand ich auch persönlich.[7]

5. I owe this information to Claude Hagège (letters of 29 and 30 October 2001).
6. As emerges from correspondence between Forchheimer and the publishers of *The Category of Person*.
7. Philip K. Hitti was then chairman of the Department for Near Eastern Studies at Princeton,

Nach verschiedenen anderen militärischen „Erfahrungen“ kam ich nach Europa, England und Frankreich, wo ich Gelegenheit hatte in die Société de Linguistique de Paris aufgenommen zu werden.

Nach dem Krieg, als ich Mittelschullehrer war, bezog ich „part time“ die Columbia Universität, wo ich mit Professor Karl Heinz Menges befreundet war, und studierte Altaisch, aber auch Romanische und Semitische Sprachwissenschaft.

Meine Dissertation ist Ihnen ja bekannt. Die führenden hiesigen Professoren (Sturtevant etc.[8]) hatten kein Verständnis für die Allgemeine Sprachwissenschaft und waren streng mechanistisch orientiert. Aber ich hatte das Glück an der Columbia Universität Verständnis für meine mehr sprachphilosophische Einstellung zu finden. (Professoren Menges, Martinet, Greenberg et al.) Aber ich unterrichtete viele Jahre an Mittelschulen, bis ich eine Professur angetragen bekam. Jetzt bin ich schon lange im Ruhestand.

Nun ad rem: Wie kam ich zu meinem Fachgebiet? Ich wurde hier sofort beraten, daß ich mit einem fremden Akzent kaum eine Stelle für Naturwissenschaften (oder andere Fächer) finden würde, nur Sprachen kämen in Frage. Ich hatte schon immer ein Interesse an Sprachen, und, nachdem ich im Sommer pädagogische Fächer studiert hatte (um Lehrer sein zu können), traf ich im Herbst den Vorstand der Deutschen Abteilung, der mir sofort ein Stipendium verschaffte. So fing es an.

Nach dem Krieg lehrte ich an verschiedenen privaten Mittelschulen, bis ich endlich eine akademische Berufung erhielt.

Da ich sprachphilosophisch eingestellt war, suchte ich ein Thema für meine Dissertation das mir lag. So kam ich auf die Kategorie der Person. Die Kritik schwankte wie bei Wallenstein. Ich hatte Glück, daß ich Verständnis fand. (Falls sich das nicht gegeben hätte, hatte mir Professor Bonfante angeboten mich in Princeton promovieren zu laßen.)

In hiesigen Bibliotheken, sowie der in der McGill Universität in Montréal fand ich reichlich Quellenmaterial.

This is from a letter of Paul Forchheimer of 160, Bennett Avenue, New York, N.Y. 10040, postmark of 2 October 1995, in prompt reply to a letter of mine, asking him about himself, about how he had come to study the category of person the way he did, how he had felt about the critical reception of his book, and whether he had retained an interest in that line of work. Locating him had not in fact been difficult, and the past was the wrong tense for referring to him above: in 1994 he was still listed in the LSA Bulletin’s membership directory.[9] He now lives in an old people’s home in New York, quite infirm.[10]

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specializing in Indo-European and Semitic and the relations between them.

8. Actually, Edgar H. Sturtevant (died 1952) had long left Columbia for Yale.
9. The American Oriental Society’s no longer lists him the year after their journal had published McDavid’s review of *The Category of Person*. The Modern Language Association he seems to have left about the same time.
10. Thanks to Wolfgang Schellinger for the original New York lead (as well as for assistance with Forchheimer’s bibliography), and to Edith Moravcsik for making a telephone enquiry at

Actually, Claude Hagège had located him before me, and had met him in person:[11]

> I know and appreciate Paul Forchheimer very much. Many years ago, in his house in Brooklyn, which is full of souvenirs of his father, this man, who is both religious and able to apply a certain lofty conception of ethics in his everyday life, offered me his masterpiece of 1953, *The category of person in language*.[12] [...] Forchheimer is a scholar of high culture, whose knowledge of European languages and civilizations, his own background, is remarkable.

Unfortunately, Forchheimer's letter was not very forthcoming concerning my questions about *The Category of Person*. Well, as for the amateur charge, he did have a background outside linguistics, namely in chemistry and pharmacy. But then as an emigré to the US, linguistics was what he studied, specializing in Germanic, Romance, Semitic, and Altaic, and it was the profession that he eventually practiced as a college professor. He did not publish much, especially after his ill-received book of 1953 – but then he had not succeeded in finding an academic job until some ten years later. What I regret most not getting him to comment on is why none of his other writings, with the possible exception of his unpublished class materials, bore even remote resemblance to *The Category of Person* in subject matter or orientation. That book's author might as well have been a different person.

5. Out of Columbia

But it was him – and that book was his Ph.D. thesis in linguistics, done part-time at Columbia. This is how it is listed in the Columbia Libraries Online Catalog:

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Author:        Forchheimer, Paul.
Title:         The category of person in language.
Description:   133 p. 28 cm.
Published:     Cambridge, Mass., 1951.
LC Subjects:   Grammar, Comparative and general--Person.
Notes:         Thesis--Columbia university.
               Bibliography: p. <119>-133.
LCCN:          51008138
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Nowhere in the book as published in 1953 by Walter de Gruyter in far-away Berlin (who otherwise had few non-German titles on their list, if any), is this

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Forchheimer's home this summer. Thanks also to Mrs. Herzog, daughter of Paul and Regina Forchheimer, for subsequent information about her father.

11. Letters from Claude Hagège of 29 and 30 October 2001.
12. Forchheimer's theme of number and person is developed further in Hagège (1982: 110), though without an emphasis on the referential hierarchy.

origin stated. Perhaps it could have been inferred that it was a thesis from the style of the acknowledgments of the author's teachers, among whom only Princeton's Giuliano Bonfante and Columbia's Karl H. Menges are singled out by name (with Father Wilhelm Schmidt's works mentioned as really the strongest influence), and from the dedication of this his first book to his recently deceased father (died 1944 in Montréal).

Actually, when Walter de Gruyter & Co. were approached by Albert Daub, General Manager of Stechert-Hafner, Inc., Books and Periodicals, of New York on behalf of Dr. Paul Forchheimer, in the summer of 1950, it was not mentioned either that the work being proposed for publication, highly recommended by Professor Menges (formerly connected with the Prussian Academy of Sciences at Berlin), was Forchheimer's Ph.D. thesis. Asked for an opinion by the publishers, Wilhelm Wissmann, Professor of Indo-European Linguistics at the Humboldt-Universität at Berlin, recommended publication without reservations. On two hand-written pages, dated 30 September 1951, Wissmann found Forchheimer's subject "bedeutsam und interessant" and, differing greatly from most future reviewers, felt that a vast area was in essence well covered, based on grammars by recognized authorities that had been utilized reliably. The author's awareness of relevant German books, however, left something to be desired; but then, Wissmann himself felt regrettably unfamiliar with the American scene, having long been cut off from all foreign publications. The translation of *The Category of Person* into German, as offered by Forchheimer, seemed to Wissmann unnecessary, for those interested in the subject would surely be able to read English. A second advisor for Walter de Gruyter, Franz Dornseiff (Professor of Classical Philology and Germanist at Leipzig and De Gruyter author), concurred: the book struck him as "purely descriptive" and so simple as to need no translation.

The ensuing negotiations between Forchheimer and his agents and Walter de Gruyter were largely routine, relating to such matters as an author's subsidy towards the cost of printing (DM 4,700), royalties (20 % of selling price of DM 13.50), the copyright (to remain with the author in view of a possible separate American edition, which did not materialize), the number of copies to be printed (1,200 + 120, of which 63 had sold by 12 August 1953), the number of complimentary copies (25, one to be sent to Father Schmidt, personally unbeknownst to the author, another to Eric[h] Fromm, the psychoanalyst then resident in Mexico, whom Forchheimer knew, yet another to Karl H. Menges, who would receive a copy with several pages missing or in wrong order), addressees of review copies (psychological and anthropological journals in addition to philological ones), special advertising of the book in English (none), questions about reviews a few weeks after publication (none had appeared, yet). Inevitably, conducting transatlantic transactions so soon after World War II added a few complications.

Only when the arrangements for proofreading were discussed, in a letter from Forchheimer's agents of 8 November 1951, quoting Dr. Forchheimer verbatim, did it transpire that the book had a prehistory:

> I am willing to read proof, if required, or, otherwise, expect this to be carefully done by the publishers. In case of difficulty, I am to be consulted. As a great part, however, has been printed as dissertation (all in all ten copies only, and not for sale), I should like to give you for forwarding to de Gruyter a copy of this edition. While many passages have been eliminated there that are contained in full in the planned book, and while the introduction differs, the bulk is practically identical, yet a number of minor mistakes or a few missing lines that were overlooked in the manuscript have been corrected here. Thus this can serve as a guide for the proof reader and save unnecessary communications and delay.

In reply, Walter de Gruyter welcomed getting the prepublication as a model for their typesetters and were hopeful that this would facilitate the proofreading, eventually done by Forchheimer himself, with results that would be less than fully convincing. That the orthography should be consistently British rather than American, as requested by Forchheimer, was perhaps the least problem on this count. Reading proof he also discovered several infelicities that he feared would give away the book's origin as a dissertation, but the publishers discouraged extensive rewriting at this stage.[13]

The involvement of Karl H. Menges had been acknowledged in the published version of what the author did not want to be immediately recognized as a dissertation. However, Menges was an Altaicist, but Forchheimer's Ph.D. was in linguistics. That the linguists involved as his uncharacteristically appreciative teachers at Columbia were André Martinet and Joseph Greenberg is only revealed now, in Forchheimer's reply to my queries.

Again, it might have been inferred from the book version of *The Category of Person* that Greenberg had a hand in it since few people at the time would accept his African re-classification as unconditionally as Forchheimer did throughout, whenever touching on an African language. Actually, Greenberg's involvement was not so marginal as that of only serving as the local expert in African classification – as he himself confirmed in a letter of 23 October 1995 to Edith Moravcsik, who I had asked to ask him about Forchheimer:[14]

> Regarding Paul Forchheimer, I really do not have any interesting recollections. He did his dissertation with me at Columbia at a time when I had many students,

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13. Many thanks to Dr. Anke Beck for tracing the Forchheimer file in the Walter de Gruyter archives and for having it photocopied for me.
14. Letter quoted with the permission of Edith Moravcsik. Thanks again, Edith, also for subsequent conversations and comments.

mostly in anthropology. I mainly recall how respectful and polite he was, contrasting to some extent with the somewhat brash American students of the period. In regard to his thesis, at least as regards to the topic, he was ahead of me since I had not yet realized the importance of language universals.

In a way, then, the author of *The Category of Person in Language* was the first typological student of Joseph Greenberg, yet to venture into typology himself. Given the general climate in post-war linguistics, as he was to experience it again in the subsequent reception of his thesis, Forchheimer rightly considered himself lucky to be taken on as a doctoral student with such a subject.

Regardless of the extent to which he succeeded or failed (and it will not have escaped his advisor that his thesis had weak as well as strong points), what Paul Forchheimer was attempting, for the category of number relative to person, was not something long over and done with. It was not something unviable, either, but exceedingly difficult to do well. Even where a Greenberg would leave off, some fifty years later, there would still be a few questions of detail left for the likes of us to answer or indeed ask, even about the typology of pronominal systems.

But then, I seem to remember a monograph not so long ago announced by Greenberg as forthcoming, entitled *Diachronic Typology of Pronominal Systems*. Naturally, answering all our questions on that cardinal part of speech, that will also replace *The Category of Person in Language* for good. But it was not really such a disaster to have had Forchheimer's revised Ph.D. thesis in the meanwhile, for quoting and perhaps reading.

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