

Interview with **Paul Kiparsky**

Ashwini Deo (AD)

How did you get into linguistics?

Paul Kiparsky (PK)

I got interested in language at an early age because the environment I grew up in made me curious about linguistic diversity. I spoke German, Finnish, and Swedish with different family members and friends, and I often ended up having to translate or switch between these languages. As a child I was intrigued by the many varieties of Finnish and Swedish that I was hearing on the farms that we spent summers on.

AD

Your father was also a linguist, wasn't he?

PK

More than a linguist – a specialist in Slavic and Baltic languages and literatures, and a polyglot. Readers of this journal might be interested to know that he studied three years at the Charles University in Prague, where he interacted with some of the Prague Circle pioneers – Josef Vachek, Bohumil Trnka, Roman Jakobson, and especially with Ľudovít Novák, Vladimír Skalička, Dmytro Chyzhevsky, and Henrik Becker. In addition to studying Slavic and Baltic philology (under Oldřich Hujer, Miloš Weingart, Emil Smetánka and Friedrich Slotty) and phonetics (under Josef Chlumský), he attended Winternitz' lectures on Sanskrit together with Kamalabai Deshpande, a pioneering Sanskrit scholar from Pune.

AD

Really? She is known as one of the first women from India who completed an advanced degree abroad.

PK

By the time I first came to India, she had already passed away, but she was well remembered in Pune.

AD

Have you yourself had any connections with the linguists in Prague?

PK

Coincidentally, I gave my first big talk abroad at that same Prague university in a plenary session of the 1967 Congress of Phonetic Sciences, speaking for Morris Halle, who was unable to attend himself. My presentation turned out to be rather controversial, and I was grateful to Milan Romportl and Sebastian Shaumyan for their support in the discussion. Through the years I learned

a lot from Miroslav Červenka's work on metrics, and of course from Eva Hajičová, with whom I had fruitful discussions in Mumbai at the 2012 Coling conference on computational linguistics.

AD

Was your father actually a member of the Prague Circle?

PK

No. He did attend the lectures they organized, was captivated by their ideas, and tried to popularize them in Finland when he returned there. He published an article in Finnish on the principles of historical phonology (1932), exemplified with a treatment of the phonologization of umlaut in Germanic. It was far ahead of its time. His adviser J. J. Mikkola, a student of Leskien, who had even been invited to succeed him in Leipzig, loved it, even though it undermined the core of his neogrammarian views. (Incidentally, Mikkola became my godfather). He immediately wrote to Trubetsky about the article, saying that it "puzzled the older linguists but delighted the younger ones". Trubetsky in turn wrote to Jakobson, who knew my father, that he would try to get hold of a copy of it. Regrettably my father did not publish it in an international forum and felt so discouraged by the more conservative Finnish linguists' response that he turned to philological topics.

AD

You once told me that you did elicitation/analysis with a Blackfoot speaker when you were 11 and visited the U.S. with your parents. Can you tell us something more about that experience and the context?

PK

My father was teaching at the 1952 Indiana University Linguistic Institute. Carl Voegelin had three graduate students working on Algonquian Languages: Byron Bender, Zdeněk Salzmán, and Dorotea Kaschube. I persuaded Byron to let me participate in his sessions with Tom Many Guns (also known as Pinto Rider), a Blackfoot (Siksika) from Montana. Byron never published his field notes before going on to do his brilliant work on Marshallese, and I don't know whether they are preserved. I still have my extensive notes from those sessions, and checking my transcriptions against Frantz' Blackfoot dictionary, I find them reasonably accurate. As a speaker of Finnish it was easy for me to distinguish the language's contrastive length even in unstressed syllables, and luckily my Finnish spelling, augmented with an accent and a glottal stop, was enough to render Blackfoot pronunciation.

AD

Can you tell us something more about your early life – I think your father was in the Army during the war, am I right?

PK

When the war broke out, he taught skiing and survival skills to foreign volunteers for a while, and was then transferred to the front lines. Towards the end of the war, already frayed from the

relentless artillery fire, he learned that his mother, an adherent of Tolstoyan ideals and author of pacifist pamphlets, was starving to death across the front in besieged Leningrad, and he suffered a nervous breakdown from which he never fully recovered.

AD

Is that why you spent summers on farms as a child?

PK: Yes, my father relished strenuous physical work as exercise and therapy, and was nostalgic for the rural life he had led between leaving Petrograd (Leningrad) as a young boy and taking up studies in Helsinki. Besides, in the postwar years of severe rationing, the relative abundance of food on a farm was an attraction.

AD

How do you see your father's influence on your work? Do you think your interests were shaped by his interests?

PK

The stories he told me introduced me to scientific thinking and to linguistics in particular. A few times he took me to the Helsinki University's faculty common room, which featured a large portrait of the turbaned Finnish Arabist Georg Wallin in flowing robes. Disguised as the physician 'Abd al-Wali, Wallin had traveled through Arabia, reaching Mecca and the Empty Quarter, and becoming the first westerner to study Bedouin oral poetry and dialects (which I later worked on too, though never *in situ*). Of course that portrait fired up my imagination.

AD

So when did you decide to study theoretical linguistics?

PK

I had read some linguistics as an undergraduate, but I was also drawn to literature, especially poetry, and I was impressed by Roman Jakobson's work, which brought a unified perspective to both.

AD

I can imagine how exciting such an inclusive way of engaging with language might have been for you. But you ended up going to MIT for your graduate education. Why is that? I thought Jakobson was at Harvard at the time.

PK

Actually I wrote to Jakobson in 1962 to tell him I wanted to study with him at Harvard, and he wrote back advising me to apply to MIT, where two young linguists, Chomsky and Halle, had just launched a startup department. Jakobson also gave a course there every year. So I followed his advice. My application, sketchy by today's standards, was accepted. With Halle as my adviser, I graduated in 1965 with twelve other students in the first batch of the new program's Ph.D.'s.

AD

What did you do after you graduated?

PK

In those days even entry-level academic jobs could still be filled by invitation. There were as yet few linguistics departments, but I was lucky to get offers from three excellent ones: UPenn, UCLA, and MIT. I visited UPenn and had a memorable meeting with Zellig Harris. At UCLA the big draw for me was their Indo-European program, which is still going strong to this day. But I decided to join the faculty at MIT.

AD

While you were at MIT as a student and young assistant professor, what was the political climate like? And how did you and the MIT Linguistics department respond to the political situation?

PK

In the mid-sixties, the growing opposition to the Vietnam war spawned a new left movement. Chomsky's essay "The Responsibility of Intellectuals" was a major call to action. For a while I was active in the SDS, which by then was mostly led by members of the Progressive Labor Party, who unfortunately wound up dissipating much of their considerable energy in sectarian disputes with Trotskyists and sundry other groups. A group of us that included linguists Tony Kroch, Jeff Elman, John Frampton and the philosopher Hilary Putnam, tried to organize the graduate students and faculty of Harvard and MIT, and sold the PL paper *Challenge* in downtown Boston. Morris Halle shook his head and considered all of us *idiots savants*. Perhaps he was right, but I did learn a lot about how the world works from that experience. I should add that Halle always resolutely defended us against criticism from MIT's administration, which in any case was quite mild. In fact, MIT as an institution showed real class in always standing up for Chomsky in the face of the relentless public attacks that he faced.

AD

What was it like to join the faculty in a department from which you had just graduated?

PK

At 24 I was MIT's youngest professor, and still felt more like a graduate student. This feeling never completely went away while I taught there. It had no objective basis, for I was always treated with full respect. Untenured faculty were not regarded as second-class citizens. I did have something to offer that I had learned even before coming to MIT, and while at MIT in courses at Harvard, which was a short subway ride away, namely a grounding in historical linguistics and philology. Halle valued this subfield, and even brought the eminent Indo-Europeanist Jerzy Kuryłowicz to teach for two years to MIT, as well as the philosopher-Sanskritist Frits Staal, who taught a course on the Indian grammatical tradition. Their impact at MIT was negligible because

none of the students knew Sanskrit, or even Latin and Greek. But many Harvard students came regularly to Kuryłowicz' classes.

AD

You collaborated with Halle on a study of Slavic accent, didn't you?

PK: We worked on this topic intensively during a year when Halle was on sabbatical. We developed a compositional approach to movable accentuation which essentially posited that morphemes can be accented or unaccented, and relied on two simple rules: all but the leftmost accent of a word is erased, and unaccented words receive an accent on their left edge. Indo-European appears to have had such an accent system, and we found similar ones in many unrelated morphologically complex languages.

AD

Was it Staal who got you interested in Sanskrit and in Pāṇini in particular?

PK

Staal's course was indeed my main inspiration.

AD

You spent two sabbatical years in India. What made you want to go there, and what did you do?

PK

The more I found out about the Sanskrit grammatical tradition, the more determined I was to study it at first hand. Pune was recommended to me by Jeffrey Masson, at that time working on Sanskrit esthetic theory (he later became psychiatrist, rose to the top of the Freudian establishment and fell down in flames, lost a notorious 10-year lawsuit against the New Yorker's Janet Malcolm, and wrote bestsellers about the emotional life of animals). In Pune he introduced me to S.D. Joshi, with whom I connected instantly. "You teach me Chomsky, and I'll teach you Pāṇini", Joshi said, and that was the beginning of a beautiful lifelong collaboration and friendship.

AD

I also studied Pāṇini with him before coming to Stanford. We read portions of the Mahābhāṣya.

PK

He was an extraordinary teacher, a great scholar and an altogether wonderful human being.

AD

Yes, indeed. So how did the study of Pāṇini influence your linguistics? What ideas/insights from Pāṇini were already explored before your study of the system and which ideas became part of the field after you studied the system in detail?

PK

In general, Pāṇini's ideas were already foreshadowed in some way, but his formulation of them is vastly more sophisticated than what Western linguistics had to offer and fits perfectly into generative grammar. The core idea of the Elsewhere Condition was already there in the form of a privative relation between expressions or between feature values, but Pāṇini's implementation of it as a relation between special and general rules goes much further. It dictates the whole structure of a grammar, in which rules come in hierarchies of specificity, from the most general case down to individual lexical items in the limit.

I had found that when a rule provides inputs to another rule ("feeds" it) or removes inputs from it ("bleeds" it) they are applied in that order in the unmarked case, as opposed to the reverse order, where the rules do not interact in this way. It took the genius of Pāṇini to unify these two asymmetries and generalize them to others (such as the relation between syncope and stress) by means of his abstract *siddha*-relation. Eric Baković and Lev Blumenfeld have recently done remarkable formal work on rule ordering that goes still further.

Somewhat different is the case of inheritance hierarchies. They had been formalized quite precisely in different ways, for example with and without multiple inheritance. But as you showed in your article, once you apply them to a complete body of natural-language data, as Pāṇini did, it is clear what the correct version is.

As for Thematic roles, Jeffrey Gruber introduced them already in 1965, and Nick Ostler's dissertation laid bare their systematicity. But only deeper study of Pāṇini showed how they could function as part of a licensing mechanism within a lexicalist grammar that seamlessly unifies syntax and morphology as a single generative system.

AD

Jeffrey Gruber and you were part of the same cohort at MIT and if I am right, Nick Ostler was your student at MIT.

PK

That's right.

AD

You were at MIT for 17 years and then you moved to Stanford. What prompted that decision?

PK

The Stanford Linguistics faculty was an interesting mix of distinguished older scholars and recent MIT graduates whose work I admired. Some of them were collaborating on research projects with members of the Philosophy and Computer Science departments, and with linguists and computer scientists at Xerox PARC and SRI. The Center for the Study of Language and Information had just been founded to further this collaboration. It quickly attracted researchers from all over the world. Stanford Linguistics also had a lively interaction with the neighboring top departments at U.C. Santa Cruz and Berkeley. All this added up to a heady environment bubbling with new ideas, which I was excited to become a part of.

AD

That sounds like a really fertile and creative environment to be in. How did moving to Stanford influence your research and teaching?

PK

Let me say first that it was not until 1984 that I actually began teaching at Stanford, for although the university wanted to hire me, it actually did not have a position available yet. So I spent two years in a kind of extended sabbatical at Xerox PARC, which at the time was a marvelous institution with a group of congenial linguists collaborating on a parser based on LFG. These two years afforded me time to flesh out Lexical Phonology from its programmatic beginnings to a reasonably full-fledged theory and to explore some of its empirical consequences in more depth.

AD

When you did join Stanford, was there any change from how things were at MIT?

PK

Stanford encouraged me to develop new courses, and I welcomed the opportunity. Teaching has always been closely connected with research for me, so new courses also meant new projects, and students who tackled new kinds of problems. I like to think that the more varied repertoire also made my presentation fresher and enhanced the experience for the class.

AD

What are some of those new courses that you began offering?

PK

Morphosyntax, Sanskrit, Historical Syntax, Foundations of Linguistics. What was really new for me was that I got to teach undergraduates for the first time. I was worried at first, but subjects such as Historical Linguistics, Metrics, and Languages of the World worked out well at the intermediate level and I greatly enjoyed them, thanks to our outstanding undergraduates. I'm proud that so many of them have succeeded in academia or industry.

AD

And did your research also take a new turn at Stanford?

PK

I did not drop any of my old interests, but I added some new ones. Already at MIT I had followed the debates on syntax between Chomsky and his students, first Haj Ross and later Joan Bresnan. Bresnan had already moved to Stanford, where she and Ivan Sag made common cause against Chomsky but disagreed with each other on a number of important points. I found this high-tension theoretical force field stimulating and started working out an alternative approach that unifies morphology and syntax along Pāṇinian lines, as I mentioned earlier. In such a crowded field it was

naturally slow to catch on, and it remains a partial sketch. But the main idea that case, agreement, and base position function to license Theta-role assignment to syntactic arguments has found some resonance, and Daniel Galbraith's *Optimal Linking Grammar* (in press with Cambridge UP) works out an OT implementation of it and uses it effectively to untangle insular Scandinavian syntax.

AD

Are there projects or papers of yours that you particularly enjoyed writing or whose outcome you were particularly happy with?

PK

Naturally I am always happy when some idea of mine is found interesting or useful or leads to a better one. Finding a theory confirmed by some new piece of crucial data is another thrill. Here is an example. My initial evidence for deriving the Greek dactylic hexameter from Indo-European iambic verse by the stepwise generalization of anacalasis (2018) came from early Greek lyrics and from the placement of breaks and bridges in Homer. I noticed only later that a form of the intermediate proto-hexameter that I posited is actually found on the famous Nestor's Cup, the earliest recorded piece of Greek verse.

Another case: Cleo Condoravdi and I studied the dialectal variants and historical evolution of Greek clitic syntax. Our analysis generated a typology, and we could document every type that it predicts except for one. In a small text in the Kozani dialect we then chanced to find a single instance that was consistent *only* with that missing type. But did it have its other predicted properties? No other information on the dialect was available. After a talk at a conference in northern Greece, I drove across the mountains to Kozani with a group of students. Our first consultant was the grandfather of one of the students, who was just returning from a successful hunting trip with a gun on his shoulder and a rabbit in his hand. Ten minutes into our interview, it was clear that his Kozani dialect was exactly the missing link we were looking for. We celebrated the successes of our respective hunts with ample toasts of his home-made raki.

AD

An amazing story! Is there anything in your early work in Phonology that you relate to in the same way?

PK

In a field like linguistics, any theoretical contribution is at best a step up a very long ladder whose top is still invisible. Its purpose is to be superseded when the next rung is reached. It is certainly exciting when that happens. But empirical discoveries, if all goes well, are for keeps, and the more is built on them, the solidier they become. And that is at least as exciting too.

AD

Has any of your early thinking about phonological systems, phonological rules, or phonological change changed substantively over time?

PK

The biggest change for me, and for most phonologists who had worked with rule-based phonology, was the shift to constraints. But I found it easy and natural because I was already working with constraints. The radical and beautiful innovation was Prince's and Smolensky's idea that ranked constraints allow rules to be eliminated altogether. This was a godsend because mixing rules and constraints leads to a formal morass. But it was empirically deeply problematic, and many paths out of the difficulties have been explored since then. My own proposal, Stratal OT, still seems to me the most promising, and it has found favor especially with two kinds of researchers: those who are devoted to comprehensive analyses of phonological systems, and those who balk at the formal intractability of the only viable alternative, which relies on transderivational constraints such as Output-Output constraints and Sympathy.

AD

In addition to your early work on phonology, Sanskrit, and language change, you published a paper called FACT with Carol Kiparsky in 1970 that identified a potential parameter of lexical meaning – factivity – that has influenced over the past five decades the way we think about predicates that take clausal complements, and about presupposition and projection. Could you walk us a little through that journey and tell us what you think of the state of the art in that area now?

PK

In “Presupposition: What went wrong?” (SALT 26) my late friend Lauri Karttunen correctly pointed out that our article groups a heterogeneous collection of predicates as “factives”, though he granted that in spite of its theoretical obsolescence it “is still a good survey of primary data”. Recent work claims that it is not even that. Degen & Tonhauser 2022 show experimentally that what we referred to as “optional factives” is a much larger class than we thought. They even suggest that there are no truly factive predicates, though in my opinion their experiments do not establish this latter negative conclusion, nor take us beyond it.

AD

Then what is the next step?

PK

Karttunen's five-way classification provides a promising entry point into a more fine-grained analysis. He distinguishes between judgments about facts (*It is odd/tragic that S, that S counts/matters*), emotive adjectives (*NP is happy/glad/angry that S, NP is sad/delighted/disappointed to VP*), propositional attitude verbs (*NP knows/regrets/forgets/remembers that S*), verbs of discovering (*NP discovers/finds out/notices/observes that S, It is discovered/found out/ noticed/observed to V*), and verbs of communication (*acknowledges, admits, confesses*).

Predicates of the first type, judgments of fact, are really factive. Emotives and propositional attitude verbs are more complicated because they attribute a thought to someone, and the thought can include the presupposition. For example, we may say about someone afflicted with paranoia

that he is angry that the police are spying on them. Our report does not presuppose that the police are actually doing it, only that the paranoid person does. We may also say that he “knows” it (the third type), often with quotation marks to distance ourselves from the presupposition. Verbs of the fourth type have this property too (*the paranoid person “discovered” that the police were spying*), but with the additional complication that their factivity is restricted to veridical contexts, where an actual coming-to-know is asserted. In conditionals, under negation or modals, in reports of dreams, etc., the complement is not presupposed (*if I ever discover that the police are spying*). D&T are sympathetic to this line of analysis, as well as to RSA-based accounts of the probabilistic effects. I am looking forward to experiments that test the specific predictions of these and other alternatives.

AD

What do you think in general of the experimental turn in syntax-semantics? To what extent does experimental work contribute data that can help in revising our theories or adjudicating between different theories?

PK

Experiments are a valuable source of data, and in many cases an indispensable one. But experimental paradigms must themselves be tested. You have to verify that they deliver results which consistently agree in clear cases with those obtained in other ways. Otherwise you can end up with confusing results, and then spend the next six months trying to figure out what they might mean.

For example, with Francis Ganong and Jay Keyser I once designed an experiment to test the hypothesis that syllables consist of two immediate constituents, onset and rhyme. We got no usable results. We did not conclude that the hypothesis is wrong, but that the experiment was poorly designed or executed, for the hypothesis is amply justified by non-experimental evidence, including syllabically conditioned phonology, the regimentation of language in verse and language games, language change, and the cross-linguistic typology of syllabic structure.

Mechanical Turk data is in my opinion problematic in many respects, and it is not strictly speaking experimental, because we cannot fully control and vary the conditions under which the anonymous subjects make their responses. It does have compensatory advantages. It can yield lots of data, and with naive speakers we eliminate the possible theoretical biases that may (consciously or unconsciously) contaminate the judgments of linguists. But it introduces other potential confounds, such as subjects’ ideas about “correct” usage, and contextual and pragmatic factors, which linguists are trained to disregard.

As linguists we are blessed with the availability of abundant data, and we can afford having the cycle of hypothesis and refutation run at hyperspeed. A novel broad generalization often gains initial acceptance and on closer examination proves inaccurate. At that point we should not automatically reject it but try to rearticulate it in a way that respects the counterexamples. For example, I once made a small observation that led to much experimentation and debate in the psycholinguistic literature. Irregular plurals can serve as first members of certain types of compounds and derived adjectives (with the singular an option), whereas regular plurals cannot: *teeth care* (**eyes care*), *people person* (**cats person*), *women gymnasts* (**girls gymnasts*), *men*

friends (**boys friends*), *teethlike* (**lipslike*), *feetless* (**handsless*). The contrast provides evidence for a layered morphological derivation in which regular plurals have more structure than irregular plurals. Of course, pluralia tantum are unavoidable in compounds, as in *clothespin*, *skittles game*, *savings account*. However, there are many real prima facie counterexamples: *record(s) office*, *complaints department*, *park(s) department*, *appropriations committee*, *career(s) adviser*, *admissions racket*, *job(s) program*, *document(s) repository*, *appeals court*, *collections specialist*, *cookie(s) policy*. Instead of throwing out the generalization and leaving the data unexplained, it is better to refine it by recognizing types of compounds. The apparent exceptions have counterparts with *phrases*, which may naturally be plural or singular: *public record(s) office*, *foreign exchange department*, *city park(s) department*, *accounts receivable department*, *customer complaints department*, *higher education career*, *higher education career(s) adviser*, *college admissions racket*, *transitional jobs program*, *financial document(s) repository*, *small claims court*, *rare book(s) specialist*, *functionality cookie policy*. This suggests that the apparent exceptions are actually phrasal compounds whose first member happens to be a one-word phrase. That would explain why compound types that disallow phrasal first members, such as **a conjugal bedpost* ('a post for a conjugal bed'), **to sick babysit*, **a pickzipperedpocket*, also disallow -s-plural first members, such as **a bedspost*, **to babies-sit*, **a pickpockets*. The reverse is not the case, for *Norwegian rats-infested* is at least not totally impossible. So there seems to be a residue of as yet unexplained restrictions on plurals in compounds, which merit closer study.

AD

Can you think of domains in syntax and semantics where you feel that linguists have benefited from taking the experimental approach? This is not the sort of question one would ask about work in phonetics, for instance, where it is crystal clear that data that comes from measurements from controlled experiments is superior to what might be generated by a linguist.

PK

Indeed, phoneticians can measure the physical (acoustic and articulatory) parameters of speech. Syntacticians and semanticists – and even phonologists – can't measure the physical properties of the objects they study, so they have to measure variables of speech production, perception and recall, and construct theories that relate these things causally to structure, with due regard to the contextual factors that we know also impact them.

One thing that this body of experimental work has shown us that context, pragmatics, processing effects, and variation are everywhere. Linguists have been trained to ignore those things in acceptability judgments and to abstract away from them in grammatical theory. But processing in particular turned out to provide explanations for constraints on extraction that rival the purely syntactic principles that were privileged by syntacticians since Haj Ross' 1967 dissertation, as shown by Tom Wasow and others.

AD

Speaking of variation, I know that since the early 1990s and the emergence of Optimality Theory, you have done a lot of work looking at patterns of phonological variation. Can you tell us something about that aspect of your work?

PK

Generative grammar originally treated variation as irrelevant noise in the data. Its systematic nature was recognized early by William Labov, but he focused on its significance for sociolinguistics rather than for grammar. In the first OT conference (at Rutgers in 1993) I showed that variation could be modeled as free constraint ranking, and more interestingly, that the proportion of a given output in the set of fully ranked grammars (its ranking volume) *predicts* the pattern of variation. My first showcase exhibit was the relative frequency of *-t/-d* deletion in different environments that had been observed in half a dozen different sociolects. This result was then confirmed by Greg Guy, by Arto Anttila for Finnish morphology, and many others. Ranking volume then turned out to be a robust predictor of frequency patterns in typological space, and, most excitingly, a predictor of the direction of long-term unidirectional drift in language change. This robust connection then provides yet a new way to probe the theory of grammar with well-understood historical data.

AD

This connection of language-internal variation to crosslinguistic typological variability and change is interesting. I have heard you say that it is not possible to build a theory of how a particular phenomenon or subsystem works unless one knows how it is manifested across the world's languages. Can you tell us more about what you see as the relationship between typology and theory?

PK

I'll give an example. In a famous broadside against linguistic universals, Nick Evans and Stephen Levinson correctly pointed out that Principle B of Binding theory is false as stated. Even Old English allows pronominals to be coreferential with a subject in their binding domain, even stressed ones, as in "She first dressed her, and then the children". But in a paper on the typology of anaphora I had already provided a parametric understanding of how pronominal systems are put together, which predicts the existence of Old English-type anaphoric systems, and in particular of non-obviative pronominals, which are found all over the world. It is now undisputed that linguistic theory cannot be based on the study of a single language and that typological variation must therefore be taken into account from the beginning. Phonology has always recognized this, and fortunately syntax and semantics have now in large part done so as well.

AD

You have worked on the analysis of metered verse or metrics in several languages. Questions about verse structure are not at the center of linguistic theory. Can you tell us a little about your work, who you take the audience to be, and what insights the poetic use of language can give us about the nature of language?

PK

The study of meter was a natural extension of my work on phonology. I found it intriguing that rhyme and meter in oral poetry can be based on a phonological representation more abstract than the phonetic or traditional phonemic level. This was attributed to the transmission of verse composed at an earlier stage when it was still pronounced as the meter requires. I showed that there is more to it: as long as the phonological processes that mask those representations remain synchronically active in the postlexical phonology, they remain accessible to poets, and new verses can still be composed in the old style. This was new evidence for the “psychological reality” of phonological derivations, and of the output of the lexical phonology as a significant level of representation.

The regimentation of language in verse provided a whole new arena for testing the predictions of different linguistic theories. English meter revealed variation along parameters defined by the phrasal and level-ordered phonology that was then emerging. Not only did each period turn out to have its own metrical “dialect”, but the major poets in each period have metrical “idiolects” that differs subtly from each other.

It also turned out that ranking volume predicts the pattern of preferences in metrical variation, just as it does in phonological variation. I was able to show this in even more fine-grained detail in meter than in phonology, because the poetry provides homogeneous corpora that are orders of magnitude bigger than anything available for sociolinguistics.

AD

Oh, it hadn't occurred to me until now that poetic corpora could be superior to speech-based corpora in terms of homogeneity. Moving on, you have worked on a lot of things in a range of subfields over the past six decades. Is there a common thread running through them?

PK

They are all ways to test grammatical theory against natural data. Language structure is revealed by typology and universals, by meter, and by historical processes such as sound change and analogy.

AD

Already your dissertation was concerned with sound change and analogy. How do you see these phenomena?

PK

Much about these processes eludes the classic theory of change that the neogrammarians formulated and Saussure adopted without questioning. Jakobson had made a case that sound changes can be driven by structural factors, especially impressively in his *Remarques*, which examined the major divergent developments from Common Slavic to its daughter languages.

Analogical change seemed every bit as important, because of its close relationship to language acquisition. The formalism of four-part proportions by which historical linguists have represented it is based on an earlier view of language as a store of memorized examples from which novel utterances are projected, first in acquisition, then by speakers and hearers in language

use. It is inconsistent with the rule-based view of generative grammar, and inadequate as a model of diachronic analogical change. My idea was that these two weaknesses are connected, and that modeling acquisition as inference to the optimal grammar consistent with the data (or more accurately with the data that the child is developmentally attuned to, in Lidz' terms the *intake* as opposed to the *input*) could reconcile it with the empirical givens of language change: analogy is incomplete or partial acquisition. That has turned out to be even more true than I expected, and I now think that no analogical change is ever due to four-part proportions with words or utterances as terms.

AD

This idea seems to have been less well received than your work on historical phonology and syntax.

PK

Yes, it was both attacked and ignored, though never refuted, in my opinion. More generally, my ambition to integrate diachronic linguistics with general linguistics has been only partly successful. Happily many linguists nowadays straddle the division between them in their research, but it remains ingrained at the institutional level, with courses and job offers still routinely separating them. I am optimistic that in the future, courses in phonology, syntax, and semantics will take into account the historical dimension as appropriate.

AD

You have even argued that grammaticalization is analogical change.

PK

If there is no proportional analogical change, there should be analogical changes which *cannot* be expressed by proportions at all. One such type of case is the generalization of a process in such a way that the new forms, unlike the old ones, feed an already existing process. Such an analogical change is a generalization, but introduces a surface disparity, so that it cannot be represented as four-part analogy between expressions. The most spectacular class of intrinsically non-proportional analogy is grammaticalization. It is an optimization (hence unidirectional) which has no pre-existing model at all, hence inexpressible by proportions.

AD

I have a question about the trajectory of the field. Linguistics seems to have moved further and further away from humanistic scholarship of language and from the broader humanities as well. What do you think might have led to this relative alienation?

PK

In the sixties, when I was starting out, the new ideas of generative grammar were met with great interest not only in the humanities, but also in philosophy and psychology. Linguistics squandered much of this opportunity because it took an inward turn, failed to address empiricist critiques adequately, and had little to say about meaning. In recent decades, thanks to spectacular advances in semantics, in the study of language perception, production, and acquisition, and in

neurolinguistics, we have re-engaged with philosophy and psychology, though not as much with literary studies and the humanities generally, which have struggled with their own isolation. Yet this is the most natural affiliation of all, as the Prague School recognized.

AD

A few final possibly personal questions. You retired a couple of years ago, but you seem to be still doing linguistics. U.S. universities have no obligatory retirement age. So why did you retire?

PK

I am glad you put the question that way, instead of asking me why I am still doing linguistics. One reason I retired is so as not to have to go to faculty meetings and committee meetings any more.

AD

But don't you miss teaching and meeting students?

PK: Indeed I love doing that, and I'm lucky to have been able to continue teaching some of my favorite courses, one a year.

AD

What are those courses?

PK

Last year I co-taught the Structure of Finnish with my colleague Arto Anttila, and this year I will do Languages of the World. Both have always gone well, and every time I have taught them I have learned a lot of new things myself too.

AD

And what are your current research projects?

PK

I want to finish several book manuscripts, one which has been announced as "forthcoming" for an embarrassing number of years.

AD

Oh, the book on Stratal OT? I seem to recall hearing about it from when I was in graduate school.

PK: [pained silence]

AD

Do tell us about your other projects.

PK

Well, there is a book on morphosyntax, which deals with case, agreement, and word order as syntactic licensing mechanisms. It has been in the works for even longer. A half-finished MS entitled *Pāṇini for Linguists* is waiting in the wings. It differs from all other books on Pāṇini in that it assumes no knowledge of Sanskrit, but introduces as much of it as needed in the course of the exposition.

And I very much want to write a book on metrics.

AD

I look forward to reading them when they are ready. My best wishes for completing them! And thank you for this interview.

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