

## Hypothesis A/Hypothesis B

Linguistic Explorations in Honor of David M. Perlmutter

Donna B. Gerdtz, John C. Moore, and Maria Polinsky, editors

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## My Path in Linguistics

David M. Perlmutter

### How I Came to Linguistics

I came to linguistics through languages.

I was aware of languages from earliest childhood. About six of my first eight years were spent in immigrant communities in New York and Philadelphia where the main languages were English and Yiddish. My parents were native speakers of both English and Yiddish, were fluent in Hebrew, knew some German, and my mother, French. I learned to bridge the language barrier with my paternal grandparents, who didn't speak English, though my grandmother spoke Yiddish, Polish, and German. My maternal grandfather was fluent in Yiddish, English, and Hebrew and knew German. He was principal of a Jewish school in Philadelphia where I began to learn Yiddish and Hebrew. I still remember how severely he bawled me out when I was six or seven because I responded in English when one of his friends spoke to me in Yiddish. "You must *always*," he insisted, "answer in the language in which you are spoken to!"

For several years I lived in his home in Philadelphia, where I met Yiddish writers, poets, and educators. In the New York home of my paternal grandfather, who was active in the Yiddish theater, I met Yiddish actors, playwrights, and theater people. Both grandfathers wrote and published books in Yiddish. At home we spoke English, but it was clear to me that Yiddish was the language of culture.

Those early years made me comfortable with languages and taught me that being multilingual is natural. I studied Latin, French, and Spanish in high school, German and Russian in college. Later, in my early years as a linguist, I broadened my crosslinguistic perspective by acquiring a working knowledge of Portuguese, Dutch, Italian, Romanian, Serbo-Croatian, and Macedonian. I became proficient in Slovenian doing fieldwork in Slovenia and studied Japanese before spending part of a sabbatical year in Japan.

In my college years at Harvard I pursued a broad liberal arts education and did a social science major that combined sociology, anthropology, and psychology. I never

took a linguistics course. Looking for a summer job in 1957, I could find nothing but selling door to door. I realized that people would hire me only for those jobs they don't want to do themselves or for those they can't do themselves. Lacking a marketable skill, I was in the first category. To be in the second, I needed to acquire a marketable skill to support myself until I found a career.

Just a few months later, on October 4, 1957, the first Soviet Sputnik went up. There was a strong current of opinion that the United States had been surpassed technologically by the Soviet Union. I figured there would now be a demand for Russian, which very few Americans spoke. Knowing I was good at learning languages, I decided to learn Russian and make it my marketable skill. The next summer I enrolled in a private Russian course, and in my senior year a friend and I took an advanced Russian course and agreed to speak only Russian to each other outside of class.

With Khrushchev in power, the first cultural exchange agreement between the United States and the Soviet Union provided for an American exhibition in Moscow and a Soviet exhibition in New York in the summer of 1959. I was interviewed in Washington in both Russian and English and was chosen to be a "guide" at the American exhibition in Moscow. I worked at the IBM exhibit, where an early computer was on display. Most of my time was spent talking with Soviet visitors who had never met a foreigner before and whose only source of information about the United States had been the Soviet media—a fascinating encounter of minds from two different worlds. It was an unforgettable experience.

Speaking and hearing Russian all day in Moscow did wonders for my Russian. As planned, after returning to the United States I used Russian to support myself. From 1960 to 1965 I taught Russian at MIT, where I was the founder and live-in faculty adviser of a Russian-speaking dormitory, and at defense research and development labs in the Boston area. I was escort-interpreter for the Moiseyev Dance Company of Moscow on their American tour in 1961. I translated a monograph from Russian into English.

During this period I was in graduate school at Harvard twice—first in Sociology and Far Eastern Languages (I studied Mandarin Chinese) and later in Economics. However, I was not cut out for either field and withdrew after a year.

My interest in linguistics was stimulated by working with Alex Lipson, my first Russian teacher, on improving the teaching of Russian by using the insights of Jakobson's (1948) analysis of the Russian verb. The first analysis (as far as I know) to posit abstract underlying forms distinct from any surface realization, it strongly influenced Morris Halle and, through him, the development of generative phonology. Instead of the infinitive, we gave students Jakobson's underlying forms as lexical entries and taught them how to derive the inflected verb forms from them. Most forms that were irregular with the infinitive as lexical entry could be derived from Jakobson's stems by a few simple rules.

Curious about linguistics, I sat in on classes: Jakobson at Harvard, Chomsky at MIT, and classes in the Romance and Slavic departments at Harvard. I got to know some of the graduate students in linguistics at MIT, but I resisted going into linguistics because it didn't seem important; I didn't see how it could improve people's lives. Eventually, however, I came to the realization that this was the wrong criterion. Knowing that I liked languages and was good at them, I thought I might be suited for linguistics.

### Student Days at MIT

I entered the graduate program at MIT in 1964, when it was the center of considerable intellectual ferment. Noam Chomsky had given a plenary address at the International Congress of Linguists in Cambridge two years earlier. His increasing fame was attracting the attention of linguists and scholars in other fields, some of whom were spending their sabbaticals at MIT to learn about what was going on. Others, already in Cambridge, were attending linguistics classes, seminars, and talks.

The graduate program in linguistics, begun in 1961, tended to attract students with an iconoclastic bent at a time when students who wanted to play it safe saw the upstart program at MIT as a risky bet. They went to Cornell to study with Hockett, to Yale to study with Bloch, to Berkeley to study Amerindian linguistics, or to other programs that promised a more secure career. A visiting linguist who was sitting in on classes remarked: "These MIT students are certainly learning something. I'm not sure what it is, but it isn't linguistics." He wondered how they would ever get jobs. He was speaking of people like Tom Bever, Paul Kiparsky, Yuki Kuroda, Terry Langendoen, Jim McCawley, Barbara Hall Partee, Haj Ross, Arnold Zwicky, and others who went on to distinguished careers. Younger linguists today may not realize how radical generative grammar seemed to linguists at that time. My fellow students the next few years included Steve Anderson, Joan Bresnan, Janet Dean Fodor, Jim Harris, Ray Jackendoff, Richard Kayne, and others. I could always find someone to talk linguistics with in Building 20, where we students shared offices.

The excitement was palpable and contagious. Generative grammar was "hot" and MIT was *the* place to do it. The pervasive feeling was that previous theories were now irrelevant; the course that dealt with them was nicknamed "bad guys." There was a new theory to be built and everyone wanted to be part of the "Chomskyan revolution."

Exciting as this atmosphere was, it was also intimidating. What made MIT linguistics intellectually vibrant made it far from ideal for a beginner to learn the ropes. There wasn't a curriculum that taught students how to do linguistics. Most of our classes consisted of a professor talking about the results of his own research, but not about how he arrived at them. Chomsky's classes were dominated by questions from advanced students, PhDs, and visiting scholars. Much of the discussion was over my

head. I took the courses, passed the general exams in my second year, and looked forward to doing my own research.

The next summer I had a job at the System Development Corporation in Santa Monica, California, where I could work on any topic I chose. I worked hard but didn't get any real results. Indeed, at that point I didn't even know what a real result would have been. I realized I hadn't learned how to do linguistic research. I had to do something to get on track.

Returning to Cambridge that fall, to make sure I learned what I had missed the first time around, I sat in on the first-year syntax courses taught by the newly hired Haj Ross at MIT and George Lakoff at Harvard. I volunteered to help students who were having difficulties in those courses, which solidified my knowledge.

I also had started analyzing syntactic data on my own, although I did not yet have a well-defined topic. One day Haj Ross, always willing to hear what I was doing, casually told me that my work was getting better: "At least now you're constructing arguments." Bingo! It was an epiphany. Constructing arguments! So that's what it's about! No one had ever made that explicit to me before. That day I vowed that if I ever got a job teaching linguistics, I would make it crystal clear from the outset that it's about making the alternatives explicit and constructing arguments. The idea for Hypothesis A and Hypothesis B was born that day.

The research that led to my dissertation progressed quickly after that. Although Chomsky, my dissertation adviser, had suggested to me a topic dealing with English, I was more interested in working with data from several languages. What initially seemed like separate topics coalesced into a plan for a coherent dissertation and I began to write.

Those who had thought the MIT grad program was a risky bet had been wrong: by the end of my third year I had opportunities for tenure-track teaching positions and postdocs. I accepted a position at Brandeis University so that I could stay in the Boston area and continue to participate in the intellectual life at MIT. In 1967–1968 I gave my first LSA talk and was invited to my first conference, but most of the year was consumed with teaching at Brandeis (and activities to end the Vietnam War). The following summer I finished my dissertation.

### **Developing an Approach to Linguistics**

I have tried to emphasize four things in my work in linguistics: explicit arguments for one hypothesis over others, extending the range of languages and phenomena for which linguistic theory is to be held accountable, making explicit the ways languages differ and the ways they are alike, and explanation in linguistics. All four were already present in my 1968 doctoral dissertation (Perlmutter 1971), especially in the chapter arguing for surface structure constraints on the order of clitic pronouns in Spanish and French (Perlmutter 1970b).

In writing that chapter, I was teaching myself how to do linguistics. I was learning how to construct alternative hypotheses and test their predictions against additional data. I discovered that the question of how such a surface structure constraint is to be formulated covered a range of issues, each statable as a separate hypothesis on which evidence could be brought to bear.

The idea of a surface structure constraint on clitic order first occurred to me as an alternative to Wayles Browne's (1968) transformational analysis of Serbo-Croatian clitics. I switched from Serbo-Croatian to Spanish when I realized that the spurious *se* rule of Spanish could be used to make a stronger argument: no set of transformations could account for clitic order in Spanish. I was learning how to exploit language-particular peculiarities to construct arguments that could not be made for other languages.

With a few exceptions, work in generative grammar at that time made claims about universal grammar based on English alone. Ross's (1967, 1986) dissertation was a major breakthrough, using data from many languages to show that his constraints, originally proposed for English, held in other languages as well. My thesis took a different tack, using language-particular phenomena of Spanish, French, and Serbo-Croatian to argue for theoretical conclusions that could not be based on English.

The surface structure constraints I proposed perform a filtering function: sentences that do not satisfy them are filtered out as ungrammatical. Conceiving of them in this way led to a discovery: certain semantically well-formed potential sentences of Spanish and French have no grammatical realization because they don't satisfy the constraints.

This chapter also attempted explanation: why are clitic pronouns subject to such a constraint and why does it have the properties it does? I proposed that universally clitic sequences are subject to surface structure constraints stated in the notation I used, spelling out what these claims predict for additional data in Spanish, French, and other languages. This was the standard generative way of positing something universal from which language-particular facts would follow. It also owes much to Chomsky and Halle's (1968) work on English stress, where predictions were also extracted from proposed universal notation.

A basic goal of linguistics, prominent in my work in Relational Grammar (discussed below), is to make explicit the ways languages differ. The last chapter of my thesis proposed that three ostensibly unrelated differences between two classes of languages could be unified by means of a single surface constraint shared by some languages but not others. It may have been the first attempt to do syntactic typology in generative grammar, ultimately leading to the pro-drop parameter (Jaeggli and Safir 1989, among others) in the Principles-and-Parameters approach. It made testable predictions that were subsequently falsified.

Explanation internal to one language came to the fore in Perlmutter and Orešnik 1973a, 1973b, on Slovenian and in Perlmutter and Moore 2002 on Russian, which seek to solve language-particular puzzles. In Slovenian, why are masculine singular headless NPs that should be accusative in the genitive case? In Russian, finite clauses can be personal or impersonal; why can't infinitival clauses be impersonal? These language-particular facts are shown to follow from others; they are explained without reference to anything universal.

More generally, any hypothesis that makes multiple correct predictions explains the data it predicts. An illustration comes from Spanish, where clitic objects of a verb generally appear on that verb. Why can they also appear on some higher verbs but not others? The answer proposed in Aissen and Perlmutter 1976b, 1983d, is that certain higher verbs form a single clause with their complements (Clause Reduction or Clause Union). Consequently, complement objects behave like objects of the united clause not only for clitic placement, but also for Reflexive Passive, Object Raising, Clause Union in causatives, and Passive with certain verbs. The Clause Reduction hypothesis explains all these phenomena.

### Teaching

The goal of teaching, as I see it, is to enable students to experience the thrill of making a discovery, and to experience it again and again. Everything else follows from this. I can think of nothing more likely to awaken a student's intellectual interest than making a discovery, nothing better able to give a student the experience of intellectual depth than making a whole series of discoveries, each going deeper than the one before. In each case, the discovery is an event internal to the student. A teacher cannot do that for a student or ensure that it will happen. All a teacher can do is set up the conditions that make it possible.

To make a discovery, students need to be able to see generalizations in data. They need to learn how to formulate an explicit hypothesis that captures those generalizations and discern the predictions it makes. They must learn the reasons for rejecting one hypothesis in favor of another. By solving problems they acquire these skills and learn how to do linguistics.

Solving problems is not just learning basic skills or preparation for more advanced work. Students can make discoveries even within the restricted world of a homework problem. After staring at the data in a problem and going up many blind alleys, when they finally see what is going on, they can experience the thrill of discovery.

Teaching undergraduates at Brandeis University, starting in 1967, I began to write problems that required students to see generalizations in syntactic data, develop arguments for one hypothesis over others, and learn how to formulate and test hypotheses. When I began teaching at MIT in 1970, I made the intensive introductory graduate course in syntax a problem-centered course, revising the problems



again and again. Sequenced by and large along the lines discussed in Perlmutter 1974b, the problems brought students ever closer to what they would face in their own research. Some of the more challenging problems required creative solutions that made anomalies in the data fall into place.

A key element in fostering discovery is interactive teaching. In class we discussed students' solutions to the homework problems, focusing on the predictions made by different solutions and the reasons for rejecting one in favor of another. I rarely gave answers. My role was to guide the students in solving problems and in making explicit the predictions and consequences of their solutions. I sometimes introduced new data (perhaps from a language unknown to them) or posed novel problems, challenging them to figure out what was going on and account for it. They were both participants and observers as discoveries unfolded in class.

Most of the problems in introductory syntax classes at both the graduate and undergraduate levels focused on constructing a grammar of English. As new data came to light, we continually revised what had already been posited. What have to be clear are the underlying assumptions, the data already accounted for, and the devices used to account for it. When the generalizations they find in new data force them to change what had previously been posited, students experience discovery.

I have enjoyed working with stellar TAs over the years. One of them, Scott Soames, volunteered to help me write and revise problems; this collaboration resulted in *Syntactic Argumentation and the Structure of English*. Another, Jorge Hankamer, adopted and improved these teaching methods in the linguistics program at the University of California, Santa Cruz. I developed them further at the University of California, San Diego. Graduate students taught with these methods have developed them further in their own teaching.

Beyond discovery within the restricted world of a problem set or course, graduate students can experience the thrill of discovering something new to the field. This requires knowledge of the field, of current theoretical debates, and of the data and arguments previously brought to bear on them. Teaching graduate students has given me the opportunity to train future linguists and to share in their discoveries. It has made me familiar with the many languages my students have worked on, enriching my knowledge of the variety of linguistic structures. In these relationships students and I have contributed to each other's discoveries and have made joint discoveries. This is where teaching and research merge into a single gratifying enterprise.

### **My Work in Relational Grammar in Its Intellectual Context**

Virtually no one who has thought seriously about language and its structure has been able to avoid using the terms "subject" and "object." This is a remarkable fact—that perceptive and knowledgeable observers have been willing to talk about "subjects" and "objects" in very disparate languages and feel reasonably confident that they knew what they were talking

about. It is all the more remarkable, then, that in the intellectual traditions represented by the frameworks of “Government and Binding,” “Principles and Parameters,” and the “Minimalist Program,” the notions play no (recognized) role at all. That tradition has always insisted that talk of “subjects” and “objects” is either illicit or casual, and that reference to such terms is to be cashed out in terms of more primitive notions (phrase-structural measures of prominence, featural properties of heads, the theory of A-movement, and so on).

That is how McCloskey (2001, 157) characterizes the contrast between grammarians’ use of grammatical relations and their status in the Chomskyan tradition, which Relational Grammar (RG) challenged by making grammatical relations primitive notions of linguistic theory.

RG is a theory of clause structure. It claims that grammatical relations are needed to describe and analyze clause structure in individual languages, to characterize the class of clause structures that occur crosslinguistically, and to formulate universals of clause structure. RG thus has little to say about long-distance dependencies, anaphora, tense and aspect systems, information structure, and other aspects of linguistic structure, except insofar as they are sensitive to clause structure and grammatical relations.

The origins of RG go back to 1972, when Paul Postal and I hypothesized that the NP-movement rules of transformational grammar (TG) obscured an important distinction. TG’s Passive transformation, for example, moved the postverbal NP to preverbal position, the preverbal NP to postverbal position, added the preposition *by*, and changed verbal morphology. We hypothesized that the essence of Passive is making the object a subject, the other changes being side effects. Similarly, Raising creates subjects or objects and TOUGH-Movement creates subjects, their other effects being side effects. The key distinction obscured by TG’s NP-movement rules, we concluded, was that Passive, Raising, and TOUGH-Movement create subjects or objects, while Question Movement and Relativization affect NPs’ linear position but not their grammatical relations. Crucially, where the former rules moved NPs to preverbal position, it was because this is the usual position of subjects in English.

In TG, passivization was different in each language. Transformations moved NPs to different positions in different languages and added language-particular material such as prepositions, postpositions, or case and/or verbal morphology, obscuring what we claimed is common to passivization crosslinguistically. Characterizing passivization universally as promotion of a direct object to subject captures the cross-linguistic generalization (Perlmutter and Postal 1974, 1977/1983g; Postal 1986a).

As research proceeded on a variety of languages, it became apparent that clause-level constructions drawn from a small set reappear in genetically unrelated and typologically diverse languages, despite differences in word order, morphology, and other features. This set consists of advancements to direct and indirect object and to subject, demotions to direct or indirect object (added later), ascensions (such as Rais-

ing), Clause Union (the merger of two clauses), and expletive constructions. This typology provides an account of the ways languages are alike in clause structure and of the ways they differ: in whether or not they have passivization, indirect-object advancement to direct object, and so on for each construction in the typology, and in the language-particular conditions on each. Viewing clause structures in relational terms reduces what had appeared to be wide variation to a small set of constructions and provides a basis for typology independent of Greenberg's (1963) basic word-order typology (SOV, SVO, etc.). It also brings out crosslinguistic regularities statable as laws of grammar.

Postal and I presented these ideas, with proposed universal laws of grammar and analyses of data from diverse languages, in our course at the LSA Summer Linguistic Institute at the University of Massachusetts in 1974. Later work led to a shift from the derivational approach to grammar inherited from TG to one where each construction has a distinct representation as a relational network. Instead of rules changing one representation to another, a language's clause structure could now be viewed as a set of constructions drawn from a small universally available set.

These discoveries put new tools at linguists' disposal that facilitate research on diverse languages. Studying a new language begins with identifying which of the set of crosslinguistically attested basic clausal constructions are present and determining whether that language provides evidence for expanding that set.

In this my research and teaching came together. In addition to my work on languages I am familiar with, I worked with students on a wide range of languages, including Albanian, American Sign Language, Cebuano (Austronesian, Philippines), Chamorro (Austronesian, Marianas Islands), Choctaw (Muskogean, Oklahoma and Mississippi), French, Georgian, Greek, Halkomelem (Salish, British Columbia), Ilocano (Austronesian, Philippines), Italian, Japanese, K'ekchi (Mayan, Guatemala), Kinyarwanda (Bantu, Rwanda), Polish, Portuguese, Seri (Hokan, northwestern Mexico), and Spanish. Each language offered a new opportunity to learn about the ways languages differ and the ways they are alike. Together with work by colleagues and their students on other languages, these languages contributed to RG's typology of basic clausal constructions (see Dubinsky and Rosen 1987).

For me, being able to learn about and work on such a collection of languages was thrilling. When I began to get away from generative grammar's emphasis on English by using data from a few European languages in my doctoral dissertation, I never dreamed that in my lifetime it would be possible to figure out *anything* about such a wide range of languages.

The tools provided by RG facilitated analyses with wide crosslinguistic applicability, of which I will mention just two from my own work.

The first argues against the widespread assumption that a nominative NP with which the verb agrees is necessarily the subject. I argued that in Italian such NPs

are subjects when in preverbal subject position but not when postverbal (Perlmutter 1983b). What had been assumed to be freedom of word order is really a systematic contrast between personal clauses, in which these NPs are subjects, and impersonal clauses (with a phonologically null expletive subject), in which they are not. This result can be extended, *mutatis mutandis*, to many other languages.

The second is the Unaccusative Hypothesis, developed in joint work with Postal (Perlmutter and Postal 1984c, 1984d), which claims that there are two kinds of intransitive clauses: unaccusative clauses have an initial direct object rather than subject, while unergatives have an initial subject. The argument for it in Perlmutter 1978 and Perlmutter and Postal 1984c, 1984d, was based on the prediction it makes together with the 1-Advancement Exclusiveness Law or 1AEX: impersonal and pseudopassives of unaccusative clauses will universally be ill-formed. Supporting data was given from Dutch, Turkish, English, and Welsh. Thus, the Unaccusative Hypothesis is a hypothesis about syntactic representations. Together with syntactic principles, it predicts universal gaps in the set of well-formed passives. Maling (chapter 15, this volume) argues that putative passives of unaccusatives, claimed to counterexemplify this prediction, are not passives and therefore not counterexamples.

Different kinds of evidence for the Unaccusative Hypothesis have been found in a wide range of languages, including Albanian (Hubbard 1985), Basque (Levin 1983; Mejías-Bikandi 1990), Choctaw (Davies 1986), English (Levin and Rappaport-Hovav 1995), French (Olié 1984; Legendre 1989; Legendre and Smolensky, chapter 13, this volume; Postal 1989, 1990), Georgian and other Caucasian languages (Harris 1982, 1985; chapter 11, this volume), Germanic (Perlmutter 1978; Abraham 1986; Grewendorf 1989; Haider 1985; Levin 1988; Steinbach 2003; Zaenen 1993), Halkomelem (Gerds 1984, 1988, 1991), Irish (McCloskey 1984, 1996; chapter 17, this volume), Italian (Perlmutter 1980, 1983b, 1989; Belletti 1988; Belletti and Rizzi 1981; Burzio 1986; Rosen 1982, 1984, 1988) and Romance (La Fauci 1988), Russian (Pesetsky 1981; Schoorlemmer 2003), Turkish (Knecht 1986; Özkaragöz 1980, 1986), and Tzotzil (Aissen 1987), among many other works and languages.

In Perlmutter 1978 three forms of the Unaccusative Hypothesis are distinguished. Under the strongest, which reflects the Universal Alignment Hypothesis (discussed below), initial unergativity versus unaccusativity is universally predictable from semantic roles. However, subsequent research has shown that this version of the hypothesis cannot be maintained (Perlmutter 1982a, 297–298; Rosen 1984).

The Unaccusative Hypothesis has given rise to a literature too large to do it justice here. A fundamental issue concerns the division of labor between syntax and lexical semantics (Rosen 1984; Van Valin 1990; Levin and Rappaport-Hovav 1995; Alexiadou, Anagnostopoulou, and Everaert 2003; among many others). The key issue is whether the relevant phenomena can be accounted for in semantic terms without syntactic representation of unaccusativity. For some phenomena attributed to un-

accusativity in some languages this has been shown to be the case. On the other hand, there are languages where lexical subcategorization for subjects or objects is needed, at least in some cases, because the unergative-unaccusative contrast is not always predictable from semantic notions. Most importantly, there are languages where syntactic representation of unaccusativity (whether predictable from semantic notions or not) captures generalizations uniting transitive and intransitive clauses.

Early on I focused on Italian as providing evidence for syntactic representation of unaccusativity. The partitive clitic *ne*, the participial absolute construction, and the use of past participles as adjectives show unaccusative nominals behaving like direct objects of transitive clauses and contrasting with the subjects of both transitive and unergative clauses. The syntactic representation of unaccusatives is also crucial to solving a language-particular puzzle of Italian: what do unaccusative clauses have in common with clauses with a reflexive clitic such that both have the auxiliary *essere* instead of *avere*? I presented these results at a colloquium at Harvard in May 1978 and in other talks, with publication coming later (Perlmutter 1980, 1983b, 1989; Rosen 1982, 1984, 1988).

In Choctaw (Davies 1986), another language that provides evidence for syntactic representation of unaccusativity, unaccusative NPs that are arguably surface subjects behave like direct objects with respect to two types of agreement, switch reference, and interclausal reflexives. Exceptions to semantic predictability further support syntactic representation.

RG developed three kinds of explanation. First, explanation is achieved by universals. For example, the Stratal Uniqueness Law explains why promotions to subject or object and demotions to direct or indirect object do not result in clauses with two surface subjects or direct or indirect objects, as well as limitations on the class of Clause Union constructions (Gibson and Raposo 1986). Second, crosslinguistically applicable hypotheses such as the Unaccusative Hypothesis explain data in a variety of languages. Third, data in individual languages can be explained as instantiations of constructions drawn from the small set of basic clausal constructions: advancements, demotions, ascensions, Clause Union, and expletive constructions, themselves constrained by universal laws.

Many RG ideas, incorporated into other frameworks, have outlived RG itself. The key distinction that launched RG—that between TG's movement rules that create subjects or objects and those that do not—is fundamental to current frameworks, albeit in different form. The distinction between clause structure and non-clause-level phenomena in RG corresponds, *mutatis mutandis*, to the distinction between A-movement and  $\bar{A}$ -movement in frameworks in the Chomskyan tradition and that between lexicon and syntax in LFG and HPSG.

The success of grammatical relations in bringing out crosslinguistic commonalities in clause structure led to the adoption of devices with similar effects in other frame-

works, including LFG, which gives grammatical relations an analogous role, and devices in the frameworks in the Chomskyan tradition, starting with Government-Binding Theory, which achieve many of the same effects. Consequently, there has been a dramatic increase in the number of languages brought to bear on theoretical issues in multiple frameworks.

Widely accepted RG ideas such as the Unaccusative Hypothesis and Clause Union (Perlmutter and Postal 1974; Aissen and Perlmutter 1976b/1983d; Fauconnier 1983; Gibson and Raposo 1986; Davies and Rosen 1988; Moore, chapter 19, this volume) have been incorporated into other frameworks, *mutatis mutandis*, as have some universals proposed in RG such as a counterpart of the Stratal Uniqueness Law (Perlmutter and Postal 1974, 1977/1983g, 1983f) in LFG and the effect of the Final I Law (Perlmutter and Postal 1974, 1983f) in Chomsky's (1982) Extended Projection Principle. Baker (1988) develops a theory to account for the effects of the 1AEX (Perlmutter and Postal 1984c, 1984d; see also Marantz 1984 and Baker, Johnson, and Roberts 1989).

Analyses of individual languages developed in RG have been translated into other frameworks as well. For example, Burzio (1986) develops the unaccusative analysis of Italian in Government-Binding Theory, and RG analyses of Chamorro, Chichewa, Indonesian, Kinyarwanda, Southern Tiwa, and Tzotzil provide much of the foundation for Baker's (1988) theory of incorporation.

At the heart of RG are the three hierarchical "term relations" (subject, direct object, and indirect object) to which nominals can advance, demote, or ascend. Nominals bearing each of them in a given language have a variety of thematic roles. In contrast, the nominals bearing a given oblique relation (instrument, locative, etc.) all have the same thematic role.

The Universal Alignment Hypothesis or UAH (Perlmutter and Postal 1974, 1984c) claimed that the relation borne by each nominal in a clause's initial stratum is universally predictable from the clause's semantics. Adopted by Baker (1988, 1997), *mutatis mutandis*, as the Uniformity of Theta Assignment Hypothesis (UTAH), the UAH has since proven wrong (Rosen 1984; Farrell 1994; Newmeyer 2001; among others).

In some cases the UAH alone was used to motivate multistratal analyses, initially a natural carryover from derivational TG. The emergence of monostratal syntactic theories raised the issue of whether there is evidence for multistratal representations independent of the UAH. My own work presents evidence from Russian reflexives (Perlmutter 1978b, 1980, 1982a, 1984a), the Unaccusative Hypothesis and auxiliary selection in Italian (see the references above), nominals that behave like subjects in some ways and indirect objects in others in Russian, Italian, and other languages (Perlmutter 1978b, 1979/1984b), and switch reference in Seri (Farrell, Marlett, and Perlmutter 1991c), among others. Other such phenomena for which monostratal

and multistratal theories make different predictions are found throughout the RG literature, but there have been few attempts to account for them in monostratal theories. One exception is Farrell (chapter 8, this volume), who proposes a novel term relation for Italian I-nominals, analyzed in Perlmutter 1979/1984b as initial subjects demoted to indirect object.

The expansion of the set of languages under intense syntactic study has challenged RG's claim that there are exactly three universal term relations. Postal (1990) proposed two more. Gerdts (1992, 1999) proposed a bistratal theory with a stratum representing morphologically licensed argument positions (MAPs) that function essentially as term relations. Based on a survey of typologically diverse languages, she concluded that languages with two, three, and four MAPs are all attested. Farrell (chapter 8, this volume) proposes a monostratal theory in which language-particular rules map semantic roles onto hierarchical term relations whose number and identity vary crosslinguistically. Marlett (chapter 16, this volume) captures both regularities and exceptions in the ways thematic roles are aligned with grammatical relations in Seri. These proposals and those of Gerdts (chapter 10), Rhodes (chapter 22), and Zaenen (chapter 23) in this volume capture language-internal generalizations and cross-linguistic contrasts that have emerged from research on a wide range of languages. To the extent that they are successful, they add to the evidence against the UAH, with which they are incompatible.

Syntactic theory has moved in new directions since the development of RG. Grammatical relations liberated syntax from dependence on language-particular features such as constituent order and brought out commonalities and differences in the clause structures of genetically and typologically diverse languages. This led to an explosion in the number and variety of languages cited in theoretical discussions of syntax. RG has made a signal contribution to the transformation of syntactic theory from a field dominated by studies of English to the field comfortable with crosslinguistic data that we know today.

### **The Role of Sign Language**

Sign languages pose a direct challenge for the basic question of linguistics: in what ways do natural languages differ and in what ways are they all alike? No linguistic theory can answer this question without offering an account of how sign languages differ from spoken languages. The obvious differences are that sign languages are articulated not in the vocal tract, but with the hands, face, and body, and are perceived not auditorily but visually. The question for linguistic theory concerns the extent to which these differences make their grammars different from those of spoken languages. Is there a general blueprint that all languages follow, regardless of modality, or does the modality affect the form of language in significant ways?

Important as this question is, what led me to work on American Sign Language (ASL) was not a research agenda but my teaching. I had directed work on ASL syntax by Judy Kegl at MIT, but I had no direct experience with ASL until I was at UC San Diego, where Carol Padden, a native signer, asked me to be her dissertation adviser. I found ASL hard to deal with. When I directed student work on spoken languages, however exotic, data in written form enabled me to see what was going on. Not so with ASL. With no way to write the signs down, I had difficulty recognizing and remembering them. I found the data hard to grasp. Padden was sometimes unaware of any difference between forms that looked different to me. In other cases her examples depended on a contrast I had failed to notice. I often couldn't tell whether a sign that differed slightly from one I had seen before was an optional variant, had a somewhat different meaning or grammatical function, or was an entirely different sign. To get enough of a working knowledge of ASL to understand what Padden was telling me, I took an ASL course at a community college. This enabled me to direct her thesis, but initially I had no intention of working on ASL myself. The leap from spoken language to sign was daunting.

But I had seen enough to pique my interest. I thought it would not be surprising if ASL has a syntax like spoken languages, but sign phonology raises deeper issues because key theoretical constructs of phonology have been assumed to reflect properties of speech. If there is evidence for them in ASL, it would show that there are phonological principles that transcend the difference in modality—potentially an interesting discovery. This drew me to ASL phonology rather than syntax.

Remembering that after one semester of French, Spanish, and other languages I had some idea of what was a possible word in each language, I wondered to what extent my semester of ASL had given me similar intuitions about possible signs of ASL. I began to look for patterns and regularities in the vocabulary I had acquired. I was excited by what I found. I wrote notes to myself, which I shared with Padden. We began to work together.

Our paper (Padden and Perlmutter 1987), my first on ASL, had two key results. First, incorporating earlier work on ASL, we showed that there are rules of derivational morphology and phonology that interact to produce a welter of similar but different forms like those that had bewildered me. Some result from derivational morphology, which typically alters a sign's movement rather than adding prefixes or suffixes, some from phonology, some from both. It was all regular and systematic. Second, we showed that rules of derivational morphology feed each other and the phonology, but crucially, outputs of the phonology must be prevented from undergoing rules of derivational morphology. With derivational morphology in the lexicon and a postlexical phonological component, no constraint is needed to account for this; it follows from the architecture of grammars. This is evidence that ASL grammar has a postlexical phonological component. In having derivational morphology,



phonology, and the same kinds of interactions between them, ASL is just like spoken languages!

A key issue for phonological theory is the extent to which the different means of articulation of signed and spoken languages affect their phonology. I was especially intrigued by the question of whether signing is organized into syllables as speech is. Are syllables a property of language or only of speech? What is essential to syllable structure and how could I recognize it if it exists in sign? I immersed myself in this puzzle for a long time.

In spoken languages, phonological segments are organized into syllables, with relative sonority peaks as syllable nuclei, the less sonorous adjacent segments serving as onsets and codas. I argued that ASL has two segment types that differ in sonority, as vowels and consonants do, and that relative sonority peaks are syllable nuclei, as in spoken languages. This was supported by distributional arguments for syllable structure: secondary movement (finger wiggling is one type) or a change from one hand-shape to another can occur on syllable nuclei but not on syllable margins (onsets or codas) (Perlmutter 1992c, 1993). This is evidence that phonological constructs such as sonority and the contrast between syllable nuclei and margins are properties of language, not speech.

I was intrigued by sign language poetry as a novel test of the hypothesis that any natural language can serve as the vehicle of poetry (Dante 1305). Analyzing an ASL poem in detail (Perlmutter 2008), I found clear evidence for both the line and the hemistich—not obvious in poetry published as performance on videotape or DVD in a language whose metrical system, if any, is unknown. I examined the way the poet exploits both visual and manual aspects of sign to achieve poetic effects, but limits himself to what is allowed by ASL grammar, which he tweaks in limited but interesting ways. I also explored the extent to which signed and spoken languages offer different resources to their poets and sought to explain why any natural language can serve as the vehicle of poetry.

Through my interest in ASL, I have come into contact with Deaf people, their community, and their culture. Some of what I encountered connected with earlier experiences. Discussions about whether ASL has all the properties of a “real” language brought me back to my childhood, when I had heard similar discussions about Yiddish: is it really a language or just a *zhargon* ‘jargon’? Sign language also brought me back to my twenties, when I had resisted going into linguistics because I did not see how it could improve people’s lives. Sign language research, beginning with Stokoe’s (1960) pioneering monograph that launched the field, has done just that. Before researchers started to study ASL, even Deaf people who used it daily and knew they could express themselves fully in sign were unaware it was a language. They tended to interpret any differences they noticed between sign and English as evidence that sign was ungrammatical English. The discovery that ASL is a language distinct

from English has had a profound and salutary effect on the Deaf community. Deaf people began to think of themselves as a linguistic and cultural minority in the larger society, with a new pride in their language, culture, and traditions (Padden and Humphries 1988, 2005). It stimulated a literary culture (stories, theater, and poetry) in ASL, rooted in their vibrant storytelling tradition.

Undergraduates generally find ASL and Deaf people's lives more interesting than most of linguistic theory. I viewed this as an opportunity to address my department's chronic problem of low enrollments. Taking advantage of a state law requiring undergraduates to take a course dealing with some aspect of the cultural diversity of the United States, I introduced a lower-division course (mostly freshmen and sophomores) called "Sign Language and Its Culture" to satisfy this requirement. It ran on two parallel tracks: one compared ASL with English and other spoken languages to bring out what is essential to human language, while the other dealt with Deaf history and culture, including literature in ASL (primarily poetry), which drew on what the students had learned about Deaf people's language and history.

The key to giving students the feeling of discovery was figuring out what they assumed about language in general, about sign language, and about Deaf people. This enabled me to introduce material that shattered their assumptions. Their most common reaction ("I had no idea of any of this!") told me they had experienced discovery. They spread the word to their friends. Despite its reputation for being difficult, the course grew until it attracted 350–400 students each time it was offered. Most were content to satisfy the requirement and move on, but a significant number went on to take other linguistics courses and some majored or minored in linguistics.

### **Excitement**

When undergraduates ask me whether they should go to graduate school in linguistics, I have a standard answer. I tell them that many more PhDs are awarded in linguistics than the job market can absorb. I explain the difference between a job and a career: Charles Ives and Benjamin Lee Whorf, for example, made their living in the insurance business; in their spare time Ives wrote music and Whorf did linguistics. Then I ask: "How much do you like doing linguistics? If you had to get some other job to support yourself, would you do linguistics in your spare time? If so, by all means go to grad school in linguistics. If not, what's the point?" This gives students a realistic view of their prospects. And even if they get a job in linguistics, there will be so many demands on their time that they will end up doing it in their spare time.

My question is meant to identify those students whose interest in doing linguistics is strong enough to make them productive linguists. If there is a domain of data or an area of linguistics that truly excites them, they will make discoveries and survive in the field.

I am lucky to have found in linguistics a field that excites me and to have been able to make contributions to linguistic theory and to the understanding of individual languages. As I look at the list of my publications, I see that many are attempts to solve a puzzle.

Language-particular puzzles in Italian, Russian, Slovenian, and Spanish have been mentioned above. The biggest crosslinguistic puzzle concerns the ways languages differ. Do rules that reference the notion “subject” target the same set of NPs crosslinguistically (Perlmutter 1982a; Farrell, Marlett, and Perlmutter 1991c)? Why do some dative NPs behave like subjects in certain respects (but not others) in Russian, Italian, and other languages (Perlmutter 1978b, 1979c/1984b, 1982a; Moore and Perlmutter 2000b)? Crosslinguistically, plural inflection tends to occur outside derivational affixation. Why, then, can diminutive suffixes appear outside plural inflection in Yiddish (Perlmutter 1988a)? Is there evidence for the syllable in a mora-counting language like Japanese (Perlmutter 1991b) and a sign language like ASL (Perlmutter 1992c, 1993)? Of course, my solutions to these puzzles and others have had varying degrees of success in withstanding the test of time.

Looking back, I see that my research has been driven by my desire to experience the thrill of discovery by solving puzzles like these and discovering the consequences of the solutions for linguistic theory. My teaching has been driven by my zest for enabling others to experience the thrill of discovery for themselves.

When I came up for tenure at MIT in my midthirties, Provost Walter Rosenblith asked me what I would be doing for the next thirty years or more. I told him that the main focus of my research would continue to be increasing the range of languages for which linguistic theory is to be held accountable. That statement turned out to be a better predictor of my future research interests than I could possibly have foreseen at the time. I have learned and discovered more about a wider variety of spoken languages than I would ever have thought possible. I have also made the leap to sign language. All this has taken me beyond my wildest dreams.

## Notes

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Most of the cited works authored by me alone or with coauthors are included in the list of my publications at the end of this book and therefore are not repeated in the references at the end of this essay, where only unpublished works of mine are listed. Since this essay focuses on my life and work, I have not attempted to cite all relevant works by other authors.

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References to works in the list of my publications at the end of this book are not repeated here. To find “Moore and Perlmutter 2000b,” for example, look for “2000b” in that list.

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