Chapter 1

Morphology and words: A memoir

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Lexicographers agree with Saussure that the basic units of language are not morphemes but words, or more precisely lexemes. Here I describe my early journey from the former to the latter, driven by a love of words, a belief that every word has its own properties, and a lack of enthusiasm for either phonology or syntax, the only areas available to me as a student. The greatest influences on this development were Chomsky's *Remarks on Nominalization*, in which it was shown that not all morphologically complex words are compositional, and research on English word-formation that grew out of the European philological tradition, especially the work of Hans Marchand. The combination leads to a panchronic analysis of word-formation that remains incompatible with modern linguistic theories.

Since the end of the nineteenth century, most academic linguistic theories have described the internal structure of words in terms of the concept of the *morpheme*, a term first coined and defined by Baudouin de Courtenay (1895/1972, p. 153):

> that part of a word which is endowed with psychological autonomy and is for the very same reason not further divisible. It consequently subsumes such concepts as the root (radix), all possible affixes, (suffixes, prefixes), endings which are exponents of syntactic relationships, and the like.

This is not the traditional view of lexicographers or lexicologists or, surprising to many, Saussure, as Anderson (2015) has reminded us. Since people have written down lexicons, these lexicons have been lists of words. The earliest known ordered word list is Egyptian and dates from about 1500 BCE (Haring 2015). In the last half century, linguists have distinguished different sorts of words. Those that constitute dictionary entries are usually called *lexemes*. Since the theme of this volume is the lexeme, I thought that it might be useful to describe my own academic journey from morphemes to lexemes. Certainly, when I began this journey, the morpheme, both the term and the notion, seemed so modern, so scientific, while the word was out of fashion and undefined. Morphemes were, after all, atomic units in a way that words could never be, and if linguistics were to have any hope of being a science, it needed atomic units.

I grew up with morphemes. The structuralist phoneme may have fallen victim to the generative weapons of the 1960s, but no one questioned the validity of morphemes at

MIT. They were needed to construct the beautiful syntactic war machines that drove all before them, beginning with the analysis of English verbs in *Syntactic Structures*, which featured such stunners as the morpheme S, which "is singular for verbs and plural for nouns ('comes', 'boys')" and ∅, "the morpheme which is singular for nouns and plural for verbs, ('boy', 'come')" (Chomsky 1957: 29, fn. 3).

Aside from brief mentions here and there in *Syntactic Structures* and the cogent but little noted discussion at the end of Chomsky's other masterwork, *Aspects* (Chomsky 1965), by the time I arrived at MIT as a graduate student in 1970 there was no talk of morphology; the place was all about phonology and syntax. These two engines, which everyone was hard at work constructing, would undoubtedly handle everything in language worth thinking about. My problem was that I very quickly discovered that I had little taste for either of the choices, phonology or syntax. It was like having a taste for neither poppy seed bagels nor sesame seed bagels, and having no other variety available at the best bagel bakery in the world, but still wanting a bagel. This had never happened to me before, and not just with bagels. Maybe I should go to another store, but I liked the atmosphere in this one a lot and, like the St. Viateur bagel shop, famous to this day (www.stviateurbagel.com), it was acknowledged to be the best in the world.

What I did love was words. I had purchased a copy of the two-volume compact edition of the *Oxford English Dictionary* (OED) as soon as I could scrape together the money to buy one, even though reading the microform-formatted pages of the dictionary required a magnifying glass. I also owned a copy of *Webster's III*. I kept these dictionaries at home, not at my desk in the department. Dictionaries and the words they contained were my dark secret. Why should I tell anyone I owned them? These dictionaries served no purpose in our education, where the meanings of individual words were seldom of much use, though we did talk a lot about the word classes that were relevant to syntax: *raising verbs, psych verbs, ditransitive verbs*. The only dictionary we ever used in our courses was *Walker's Rhyming Dictionary*, a reverse-alphabetical dictionary of English, first published in 1775. Its main value, as Walker had noted in his original preface, was "the information, as to the structure of our language, that might be derived from the juxtaposition of words of similar terminations." Chomsky & Halle had mined it extensively in their research for *The Sound Pattern of English* and it was to prove invaluable in my work on English suffixes, though I did not know it at first.

The 1960's had seen the brief flowering of ordinary language philosophy, whose proponents, beginning with the very late Wittgenstein (1953), were most interested in how individual everyday words were used, in opposition to the logical project of Wittgenstein's early work. Despite the popularity of such works as Austin (1962) and Searle (1969), ordinary language philosophy never went very far, at least in part because its proponents never developed more than anecdotal methods of mining the idiosyncratic subtleties of usage of individual words. But there was no contradicting the view that every word is a mysterious object with its own singular properties, a fact that most of my colleagues willfully ignored, in their search for the beautiful generality of rules. The question for me was and remains how to balance the two, words and rules.

Morris Halle had given a course on morphology in the spring of 1972, in preparation for his presentation at the International Congress of Linguists in the summer. Noam Chomsky had published a paper on derived nominal two years before, in 1970, which, though it was directed at syntacticians, provided a different kind of legitimation for the study of the individual words that my beloved dictionaries held. Maybe I could find something there, I said to myself with faint hope, though the approach that Halle had outlined did not open a clear path for me and I knew that I was not a syntactician, so Chomsky's framework did not appear at first to provide much hope, despite his attention to words.

Beginning in early 1972, I spent close to a year reading everything I could lay my hands on that had anything to do with morphology. I started with Bloomfield and the classic American Structuralist works of the 1950s that had been collected in Martin Joos's (1958) *Readings in Linguistics*, almost all of which dealt with inflection. Though I learned a lot, I couldn't find much of anything in that literature to connect with the sort of work that was going on in the department or in generative linguistics more broadly at the time.

In the end, I did find something to study in morphology, though not in generative linguistics. I have come back to this topic, English word formation, again and again ever since, but only now am I beginning to gain some real grasp of how it works. The seeds of my understanding were sown in my earliest work on the topic but they lay dormant for decades, until they fell on fertile ground, far outside conventional linguistic tradition. And though again I did not come to understand it for decades, word-formation was also a fine fit for the Boasian approach that I had learned to love in my first undergraduate linguistics training, in which the most interesting generalizations are often emergent, rather than following from a theory. Also, the nature of the system in morphology, and especially word-formation, is much better suited to someone of my intellectual predilections. This is an area of research in which regular patterns can best be understood in their interplay with irregular phenomena. I enjoy this kind of play.

Word-formation and morphology in general had had an odd history within the short history of generative linguistics before 1972, generously twenty years. One of the best-known early generative works was about word-formation, Robert Lees's immensely successful *Grammar of English Nominalizations* (1960). This book, though, despite its title, dealt mostly with compounds and not nominalizations, using purely syntactic mechanisms to derive compounds from sentences, seemingly modeled on the method of *Syntactic Structures*.[1] Lees's book directly inspired very little research on word-formation in its wake, though the idea of trying to derive words from syntactic structures has surfaced regularly ever since (Marchand 1969, Hale & Keyser 1993, Pesetsky 1995).

Chomsky's 1970 "Remarks on nominalization" (henceforth Remarks) echoed Lees's book in title only. It was in fact its complete opposite in spirit, method and conclusions, although Chomsky never said so. After all, he owed Lees a great personal debt. Lees had played a large role in making Chomsky famous with his (1957) review in *Language* of Chomsky (1957). Remarks injected for the first time into generative circles the observa-

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[1]Lees's book went through five printings between 1960 and 1968, extraordinary for a technical monograph that was first published as a supplement to a journal and then reissued by a university research center.

tion that some linguist units, in this case derived words, are semantically idiosyncratic and not derivable in syntax (unless one is willing to give up on the bedrock principle of semantic compositionality). Word-formation, it turns out, is centered on the interplay between the idiosyncrasies of individual words that Chomsky noted and the regular sorts of phenomena that are enshrined in the rules of grammar.

My first excursion into original morphological research took place in the fall and winter of 1972–73, a time when I was entirely adrift. I had begun to read widely and desperately on morphology early in 1972, hoping it might save me from myself, but had not yet lit on any phenomenon that held the faintest glimmer of real promise. This is the lifelong agony of an academic: the struggle to find something that is both new and of sufficient current interest for others to give it more than a passing glance. For some reason, I embarked on a study of Latinate verbs in English and their derivative nouns and adjectives, verbs like *permit* and *repel*, and their derivatives: *permission* and *permissive*; *repulsion* and *repulsive,* which contained a Latin prefix followed by a Latin root that did not occur independently in English. All the verbs had been borrowed into English and I can't recall for the life of me what led me to study this peculiar class of words.

What I first noticed about these verbs and their derivatives was that the individual roots very nicely determined the forms of the nouns and adjectives from the verb by affixation. Each individual root such as *pel* generally set the form of the following noun suffix (always -*ion* after *pel*). Also, a given root often also had an idiosyncratic form (here *puls*-) before both the noun and adjective suffix: com**puls**ion, com**puls**ive; ex**puls**ion, ex**puls**ive; and so on for all verbs containing this Latinate root. With a very small number of exceptions, the pattern of root and suffix forms was entirely systematic for any given root but idiosyncratic to it, and therefore predictable for many hundreds of English verbs, nouns, and adjectives. The whole system was also obviously entirely morphological. And best of all, no one had noticed it before. I had discovered something new in morphology and I quickly outlined my findings in by far the longest paper that I had ever written, almost fifty pages, filled with typos, which I completed in April 1973.

The central results of this first work were entirely empirically driven. I have prized empirical findings above all other aspects of research ever since, because these findings don't change with the theoretical wind. The generalizations I found are as true today as they were in 1973. In this emphasis on factual generalization I differ from most of my linguist colleagues. Of the empirical discoveries that I have made over the years, I am proudest of three: this one, the morphome, and the morphological stem.

It wasn't long before I realized that Latinate roots presented a fundamental problem for standard structural linguistic theories of morphology. All of these theories were – and many still are –based on the still unproven assumption that Baudouin de Courtenay had first made explicit almost a century before in linguistics, that all complex linguistic units could be broken down exhaustively into indivisible meaningful units, which were reassembled compositionally (in a completely rule-bound manner) to make up utterances.[2] The problem was that, although these Latinate roots could not be said to have

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[2]The idea that morphology and syntax are both compositional was simply assumed from the beginning, though it should be noted that Baudouin's work predates Frege's discussion of compositionality.

constant meaning, or in some cases any meaning at all that could be generalized over all their occurrences, they had constant morphological properties. The English verbs *ad**mit**, com**mit**, e**mit**, o**mit**, per**mit**, re**mit**, sub**mit**, trans**mit***, and so on, do not share any common meaning. What they do share are the morphological peculiarities of the root *mit*. The classical Latin verb *mittere* meant 'send' and the prefixed Latin verbs to which the English verbs are traceable may have had something to do with this meaning in the deep historical past of Latin, but even in classical times the prefixed verbs had begun to diverge semantically from their base and from each other. What ties them so closely together in English is only the structural fact that, without exception, they share the alternant *miss* before the noun suffix *-ion* and the adjective suffix *-ive*, and that the form of the noun suffix that they take is similarly always *-ion*, and not *-ation* or *-ition*.

The verb root *mit/miss* has very consistent, unmistakable, and idiosyncratic morphological properties in English today. Unless we choose to disregard them, these properties must be part of the morphology of the language. But the root has no meaning, so it can't be a morpheme in the standard sense. How can we make sense of this apparent paradox?

The answer is found in the empirical observation that formed the core of Chomsky's Remarks: derived words are not always semantically compositional. This observation, which Chomsky called the *lexicalist hypothesis*, is the single greatest legacy of Remarks. It is far from original; only its audience is new. Jespersen, for example, writing about compound words, had pointed out many times over several decades that the relations between the members of a compound are so various as to defy any semantically predictive analysis. Jespersen concluded that the possible relations between the two members of a compound are innumerable:

> Compounds express a relation between two objects or notions, but say nothing of the way in which the relation is to be understood. That must be inferred from the context or otherwise. Theoretically, this leaves room for a large number of different interpretations of one and the same compound [...] On account of all this it is difficult to find a satisfactory classification of all the logical relations that may be encountered in compounds. In many case the relation is hard to define accurately [...] The analysis of the possible sense-relations can never be exhaustive. (Jespersen 1954: 137-138)

The purpose of Remarks had been tactical. As Harris (1993) recounts in detail, at the time of writing the article, Chomsky was locked in fierce combat with a resurgent group of younger colleagues, the generative semanticists, who sought to ground all of syntax in semantics. Syntax at the time was assumed to encompass word-formation, though in truth almost no work had been done on word-formation besides Lees (1960). Reminding everyone in the room that at least some word-formation was not compositional, a purely empirical observation, cut the legs out from under generative semantics in a single stroke from which the movement never recovered. More importantly, although Chomsky never mentioned it and may not have realized it, the demonstration that some complex words are not semantically compositional also destroyed Baudouin's traditional morpheme and lent support to Saussure's sign theory of words. The non-compositional complex words

at the core of Remarks lie within the class of what Jespersen (1954) called *naked words*: uninflected words. Complex naked words are formed by derivational morphology and compounding. Inflected forms, by contrast, are always compositional, because they realize cells in the morphosyntactic paradigm of the naked word. Their properties are accidental, in the traditional grammatical sense of the term, not essential.

What I had learned from Remarks about compositionality within words, combined with my discoveries about meaningless Latinate roots, led me to realize that word-formation needed to be studied in a way that was free from Baudouin's axiom, an axiom that had held sway for over a century: that complex words can be broken down exhaustively into meaningful morphemes. Although I was entirely unaware of the consequence at the time, and remained unaware of it for decades, this discovery freed me to do linguistics in the way I loved to, not deductively as I had been taught to do at MIT, following some current theory where it led, and not inductively, but by working towards what the great Barbara McClintock had called "a feeling for the organism" (Keller 1983). My first two years at MIT had taught me that the theory and deduction game held little charm for me. Perhaps that's because I wasn't very good at it. Working on my own terms made me feel better about myself than I had for the entire preceding two years. I could stop worrying whether I was as smart as all those other people. It turned out I didn't have to be smart. Common sense was at least as valuable, and much rarer in those circles.

English had been an exotic object of inquiry for American linguistics from the start. The first American Structuralists were anthropological field workers who confined themselves deliberately to the native languages of North America. Only in his very last years did Edward Sapir turn to English. Bloomfield discussed English in his *Language* (1933), presumably to engage a broad readership, but in his technical writing he too dealt mostly with languages of North America on which he did original fieldwork. Bloomfield's successors, notably Trager & Lee Smith (1951) did important work on English, but they were in a decided minority.

Generative grammar was different. The vast bulk of research in the first two decades, beginning with Chomsky et al. (1956), had been on English. This English bias was especially true of generative syntax, whose success was due in no small part to the analyst being able to come up with novel sentences on the fly that the grammar could label as either grammatical or ungrammatical. Only a native English speaker could have come up with the most important sentence in the history of linguistics, Chomsky's *colorless green ideas sleep furiously*.[3] Even in generative phonology, whose earliest works, Chomsky (1951) on Modern Hebrew and Halle (1959) on Russian had dealt with other languages, the high-water mark of this tradition was an analysis of English, *The Sound Pattern of English*. It was therefore not entirely unexpected that I should turn my attention to English word formation. Even my earliest excursion into morphology had dealt with English, albeit Latin roots that had been borrowed into English. It would be a decade before I looked seriously at word-formation in other languages (Aronoff & Sridhar 1984).

American linguists had not written much about word-formation in the preceding quarter century. The great Structuralists from Bloomfield to Hockett had done seminal work

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[3]All the data in the most important American structuralist work on syntax before *Syntactic Structures*, Wells (1947), is from English, except for one small example from Japanese.

on morphology. Much of it was collected in Martin Joos's (1958) *Readings in Linguistics*, which I read carefully, along with the chapters on morphology in Bloomfield's *Language* (1933). But the Structuralists had dealt almost exclusively with inflection. I could find almost nothing on uninflected words. There was Lees's (1960) monograph, but his approach was not useful in a post-Remarks environment, and besides, he mostly dealt with compounds.

The most notable exception of the previous decade had been Karl Zimmer's monograph on English negative prefixes (Zimmer 1964). This book opened up an entirely new world for me, the tradition of English linguistics. This world had existed for a century and more, parallel to the one I inhabited but completely unknown to us, and it was one in which the study of word-formation had always occupied an important place.

English linguistics had emerged in departments of English language and literature, where in the 1970s it still retained the connections to philology that most of the rest of the field had left behind in the 19$^{\text{th}}$ century. To this day, it is much more rooted in texts than other kinds of linguistics, because of its closeness to literature. Much of English linguistics was historically oriented, but in a very different way from the comparative historical linguistics that lay at the root of modern structural linguistics. Its focus was on the linguistic history of a single language, the record of English since its emergence as a distinct written language around 800 CE. The connection to philology lay in this shared basis of written texts, though philologists were much more literarily oriented. People who read Beowulf and Chaucer and Shakespeare had to know something about the language these people were writing in and English linguistics served this purpose.

Every undergraduate English major—and there were many more in those days—had to take a course on the history of the English language. For the same reasons, English linguistics had sister disciplines in the other major standard European languages and language families: French, German, Italian, Spanish, Romance, Scandinavian, etc. As I learned much later, the OED was the greatest monument of this tradition of English linguistics, but much of the best work had been done on the European continent, especially in German departments of *Anglistik*. The best-known exponent of this tradition was a Dane, Otto Jespersen.

Hans Marchand reviewed Zimmer's monograph in *Language* in 1966. Marchand had fled from Germany to Istanbul in 1934 as a Catholic political refugee with the help of his mentor, the Jewish Romance philologist Leo Spitzer. He gradually turned towards the study of language rather than literature, remaining in Istanbul until 1953. Marchand returned to Germany in 1957, after a stint in the United States, to teach *Anglistik* at the University of Tuebingen. His book, *The Categories and Types of Present-Day English Word-Formation*, published in 1960 and greatly revised in 1969, has remained the authoritative description of English word-formation since its first publication. Remarkably, Marchand had written most of the book while in internal exile in Turkey in an Anatolian village from 1944 to 1945, under threat of repatriation to Germany, which had drafted him into the military in absentia in 1944. He had sought unsuccessfully for years to publish this early version while still in Turkey.

Marchand and Zimmer follow very similar approaches, quite different from that of American structural linguistics. They ask what a given derivational affix meant (what Zimmer calls its "semantic content"), what it applied to, and what it produced. The prefix *un-* that most occupies Zimmer's mind, for example, is negative in meaning and derives adjectives from adjectives.[4] This is all very traditional and in line with the treatment of derivational affixes in the OED, which contained entries for derivational affixes from the beginning, though not for inflectional affixes. The adjectival negative prefix *un-* has a very extensive entry in OED, with many observations similar to those of Marchand and Zimmer, and hundreds of examples (my favorite being *unpolicemanly*). The OED even notes the morphological environments in which a given derivational affix is particularly productive, which was of special importance to Zimmer and to my own work. For *un-*, the OED notes that it is especially common with adjectives ending in *-able*: "In the modern period the examples become too numerous for illustration; in addition to those entered as main words, those given below will serve as specimens of the freedom with which new formations are created."

This traditional approach to word-formation provided an intuitively satisfying solution to the problem of the morpheme that my work on Latinate roots had uncovered. If derivation is not a matter of combining morphemes but of attaching affixes to words, then we don't need all the morpheme components of words to be meaningful and we don't need the internal semantics of words to be compositionally derived from these components. All we need is for words to be meaningful. We don't need to worry about morphemes at all, only words and what the derivational affixes do with them.

This traditional approach circumvented the problem of meaningless morphemes for a simple reason: it predated the notion of the morpheme. The earliest citation in OED by far for any sense of the word *derivation* equates it with *formation.* It comes from Palsgrave's 1530 English-language grammar of French, *L'esclarcissement de la langue françoyse*, the first known grammar of French ever written in any language: "1530 J. Palsgrave *Lesclarcissement* 68 Derivatyon or formation, that is to saye, substantyves somtyme be fourmed of other substantyves." This has become my favorite citation of the words derivation and (word) formation and, though I did not know it at first, it encompasses the claim that words are formed from words; my observation that words are formed from words merely updates Palsgrave's remark. This claim is the essence of the traditional treatment of word-formation and it is the motto that I adopted, elevating the observation to a principle.[5]

In my dissertation and subsequent monograph, I took complete credit for the axiom that morphology was word-based. Even decades later, when I clarified the terminology and called it *lexeme-based morphology*, I did not provide any direct attribution to the tradition of English word-formation studies. My only defense is that neither Marchand nor Zimmer ever stated what for them was simply an unspoken assumption. All I did was to make this assumption clear as an axiom. I can therefore at least take credit for the realization that this was a useful axiom on which to base the analysis of word-formation.

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[4] *Un-* also attaches to verbs and has the sense of undoing the action of the verb. Whether these two are one and the same affix has been much discussed (Horn 1984).

[5] The idea that words are formed from words may ultimately be traceable to the Greek and Latin grammatical traditions, which were entirely word-based, even at the level of inflection (Robins 1959).

Notation meant everything in those days. Chomsky & Halle (1968) had even gone so far as to extol the explanatory power of parentheses. My most important task was therefore to create a simple notation in which traditional OED-style generalizations about word-formation could be stated in a way that generative linguists might understand. This was the word-formation rule (WFR). It bore close resemblance in form to the rewrite rules that were standard in generative grammar. A WFR took a word from one of the three major lexical categories (Noun, Verb, or Adjective) and mapped it onto a lexical category (the same or another), usually adding an affix, and making another word. The rule of *un-* prefixation, for example, could be written as $[X]_A \rightarrow [un\text{-}[X]_A]_A$ or it could be written simply as the output $[un\text{-}[X]_A]_A$. This notation was transparent and made generative linguists, myself included, think that this way of dealing with word-formation could be easily assimilated into their way of thinking. The acronym WFR added a nice touch. The title of the published version of my dissertation, *Word Formation in Generative Grammar* (Aronoff 1976) was suggested by S. Jay Keyser, the editor of the series of which this would be the inaugural monograph. It only served to strengthen the impression that I had integrated the study of word-formation into generative grammar. The monograph was a great success, thanks in no small part to its title, and most accounts treat the book as central to the treatment of morphology and word-formation within generative grammar.

Nothing could be further from the truth. The title of the monograph was deeply deceptive and in agreeing to it I was also deceiving myself. Word formation rules, as conceived of and discussed in that monograph, are incompatible with generative grammar or with any grammar-based linguistic framework, because, like the tradition they encode, these rules cross the synchronic-diachronic boundary that is central to all post-Saussurean structural linguistics. I have only recently come to appreciate this fact. I certainly believed at the time that I was doing generative grammar, as have most of the book's readers since. What is true is that I was a member of a social community self-organized around generative grammar. I did my work on word-formation within that community and it was accepted as legitimate almost entirely on those social grounds.

In his great posthumous work, Saussure 1916/1959 set up a distinction that has been accepted throughout the field ever since, between *synchronic* and *diachronic* linguistics. Synchronic linguistics deals with a single state of a language—the present—while diachronic linguistics deals with successive states—history. Generative grammar seeks to provide a theory of what is a possible synchronic grammar of a language, the basic idea being that the grammar generates the language (Chomsky 1957). The theory is also supposed to mirror the innate capacity that a child brings to the task of constructing a grammar for the input that the child receives (Chomsky 1965). But traditional research on word-formation, which preceded Saussure in its origins, is neither synchronic nor diachronic: it is about how new derived words accumulate in a language **over time**. That is why Marchand gave his *magnum opus* the subtitle "A Synchronic-Diachronic Approach" and why Jespersen called his monumental six-volume life's work *A Modern English Grammar on Historical Principles*, both titles in direct contradiction of the Saussurean split, both by scholars working within the tradition of English linguistics. In

truth, Marchand's approach was neither synchronic nor diachronic, in spite of its fashionable title, because the study of word formation lends itself to neither synchrony nor diachrony: the word formation system of the language at any given moment can only be understood through the historical accumulation of the lexicon. The study of word-formation is concerned at its core with how words are created, how they are formed, and how they are added to the language. Unlike sentences, words, once formed, accumulate, and this accumulated storehouse has an effect on new words. Words accumulate both in the mental lexicon of an individual speaker and in the collective lexicon of the larger linguistic community.

This brings us back to Chomsky's lexicalist hypothesis. To understand this hypothesis, we need to clarify two distinct senses of the word *lexical* (Aronoff 1988). One is Bloomfield's lexicon, the list of what DiSciullo and Di Sciullo & Williams (1987) later so nicely called the "unruly." The other encompasses the word-formation rules themselves and maybe all morphology including inflection too. The term *lexical component* is usually meant to include both the rules of morphology and the lexicon. Chomsky's original lexicalist hypothesis says no more than that the lexical component is responsible for forming and storing some of the complex words of the language, in addition to the simple monomorphemic words that have always been thought of as arbitrary signs stored in the lexicon. His major criterion for distinguishing lexically from 'transformationally' derived words is semantic predictability or compositionality (lexically derived words are not compositional) though most later lexicalist theorists used others as well (Aronoff 1994, Pesetsky 1995).

Halle's (1973) lexicon, which he described as "a special filter through which the words have to pass after they have been generated by the word formation rules" (p. 5), is a Bloomfieldian list of words, separate from the morphological rules. Halle suggested that "the list of morphemes together with the rules of word-formation define the set of *potential* words of the language. It is the filter and the information that is contained therein which turn this larger set into the smaller subset of *actual* words" (p. 6). This way of looking at the relation between word-formation and the lexicon appears to permit us to include word-formation in a synchronic grammar: the morphemes and the abstract rules of word-formation will be part of the grammar, not the lexicon, while the actual results of the application of the rules to the morphemes, which can be quite messy and idiosyncratic, as Chomsky had already emphasized, will be housed outside the grammar in the Bloomfieldian lexicon. Words will be formed by rules in the grammar, just as sentences are, though perhaps by a distinct lexical component, along the lines of the theory of Remarks. On this story, though, once words are formed they are stored in the lexicon and should accordingly have no further interaction with the grammar or the rules.

Over the years, this general strategy of strictly separating the rules from the unruly in order to better assimilate word-formation to syntax, what Marantz much later called the *single engine hypothesis* (Marantz 2005) has faced a number of problems, all of which are traceable to the fact that the strategy allows for no interaction between the rules (and the morphemes they operate on) and the set of words formed by the rules, which are stored in the lexicon. The insulation of the rules from the lexicon makes it impossible to

ask many interesting questions with even more interesting answers. I will discuss briefly here only the two most important ones, morphological productivity and blocking.

Unlike most rules of syntax, rules of word-formation vary widely in their productivity. A standard example is the trio of suffixes -*ness*, -*ity*, and -*th*, all of which form nouns from adjectives in English. of the three, -*th* is the least productive; only a handful of words end in this suffix. The only one I can identify as having been added to the language in the last couple of centuries is *illth*, which was coined on purpose by John Ruskin in 1862 to denote the opposite of *wealth*. The word is almost never used today, except in close proximity to *wealth* or *health*. Speakers of English know that new or infrequent words in -*th* have an odd flavor about them. The OED remarks about the word *coolth*, for example, that it is "Now chiefly literary, arch[aic], or humorous."

The suffix -*ity* is more productive, but limited in the morphology of what it can attach to. The OED lists approximately 2400 nouns in current use ending in the letter sequence <ity>, most of which contain the suffix, compared with about 3600 ending in the letters <ness>. But a closer look reveals that <ity> is much more likely to appear after a select set of suffixes. With -*ic* it is preferred by a ratio of almost 7/1 over -*ness*. This preference is reflected in speakers' judgments and in the relative frequency of members of individual pairs. The word *automaticity* feels much more natural than *automaticness* and a simple Google search shows 109,000 "hits" for *automaticity* but only 242 for *automaticness*. Even for very rare words, the same pattern emerges. While *oceanicity*, a word I have never heard of, gets only 762 hits, its counterpart, *oceanicness*, gets only 5!

Once we leave the few affixes that -*ity* is attracted to, though, -*ness* is ascendant. *Greenness* outnumbers *greenity* 1000/1. Google even thinks that you have made a mistake when you search for *greenity and* asks: "Did you mean: greenify?" A similar pattern of results is found for all the other color words. In the same vein, we can find examples of humorous uses of words like *sillity* or *slowity* in the Urban Dictionary, but not in many other places on the Web.

There are numerous ways of distinguishing the productivity of these three suffixes, but productivity is clearly related to the number of words that are already present in the language: the more you have, the more you get. Productivity depends on the accumulation of words. It is a dance between the lexicon and the grammar. If we try to make a strict separation between the two, we will never understand how the dance works. Both Marchand and Zimmer knew about the nuances of productivity. Marchand closes his review of Zimmer's book with the following somewhat backhanded compliment: "Zimmer's investigation is a valuable contribution not to the study of semantic universals, which it planned to be, but to the problem of productivity in word-formation" (Marchand 1966: 142).

The other problem that productivity poses for modern linguistics is that it is variable. Mainstream formal linguistics, with its roots in the triumphal 19th century neo-grammarian slogan that sound change laws have no exceptions (Paul 1880) has never dealt well with variation. If anything, formal linguists continue to be blind to the fact that variation is a part of language (I-language). One response to variability is simply to deny that a phenomenon like productivity exists. Another is to admit that it exists,

but to deny that the phenomenon is variable, claiming instead that it is all or none. That is what Marchand does. Referring to Harris (1951: 225), Marchand notes disapprovingly that "a descriptivist like Zellig S. Harris maintained that 'the methods of descriptive linguistics cannot treat of the degree of productivity of elements'" (Marchand 1966: 141) . But he himself only dichotomizes word-formation rules into those that are productive and those that are, in his words, restricted:

> Zimmer's merit is to have seen an important problem in word-formation, that of productivity. . . . Zimmer's study . . . calls our attention to the fact that what seems to be the same type of combination, viz. derivation by means of a negative prefix, is in reality split up into two groups, one of restricted productivity (instanced by *unkind*) and another, deverbal group (instanced by *unread*) which is of more or less unrestricted productivity (Marchand 1966: 141).

Even here, Marchand is not talking about one productive rule vs. a different unproductive rule, but rather a single rule, which is more productive in one environment (with past participles and -*able* derivatives, both of which have a passive reading) and less productive in another (with underived adjectives like *kind*). As Zimmer demonstrates, there is not in fact a dichotomy, but rather a cline in productivity that depends on both environments and rules. In the half century since, the nondiscrete nature of productivity has been demonstrated time and again, most definitively in Bauer (2001).

Productivity is a question of fecundity, how many words there can be and how easily they can be created. A pattern is highly productive if there can be many new words in that pattern. It is unproductive if there can be only a few new words. When we say that the English nominal suffix -*ness* is highly productive we mean that the pattern can form many nouns from adjectives; when we say that the suffix -*th*, which also derives nouns from adjectives, is unproductive, we mean that it cannot. And because words are formed from words, there is a direct relation between how easy it is to form words in a pattern and how many already exist in that pattern, in either the mind of a speaker or the language of a community. As we have just seen, there are many -*ness* nouns in English. The OED lists over 4000 nouns ending in the letters <ness>, the great majority of them containing the suffix. There are no more than a handful of -*th* nouns derived from adjectives. If how many words there can be of a given type depends on a combination of how many words there are already of this type and how many there are for the type to feed on, then words differ sharply from sentences. For starters, it makes little sense to even ask how many sentences there are of a given type. Sentences are not stored, they are produced and then vanish.

Blocking is the second phenomenon that demonstrates how the formation of individual words depends intimately on the words we already know. For four decades, since the moment that I first stumbled on this phenomenon, it has been clear to me that blocking is a real empirical phenomenon and that it is just what I first defined it to be: "the nonoccurrence of one form due to the simple existence of another" (Aronoff 1976: 43). A few pages later, I made an explicit connection to synonymy: "Blocking is basically a constraint against listing synonyms in a given stem" (Aronoff 1976: 55). And on the

same page I wrote: "To exclude having two words with the same meaning is to exclude synonymy, and that is ill-advised." A few pages later, I referred to "the blocking rule." Clearly, I had no idea precisely what blocking was, beyond an empirical phenomenon. Only now, though, do I understand why my empirical observation might be true: the avoidance of synonymy in general and blocking in particular are the result of competition, a topic I have spent the last half decade investigating.

The tradition of word-based morphology dates to the first grammarians, although it was eclipsed for much of the twentieth century by the rise of synchronic linguistics. In Cambridge, Massachusetts one didn't learn much about what was happening in Cambridge, England, but soon after leaving for Stony Brook I learned that word-based morphology had been revived in England in the decade or so before my own research, notably by R. H. Robins (1959) and Peter Matthews (1965, 1972). This line of research, especially in derivational morphology, has grown in the decades since, notably in France, led by Danielle Corbin (1987), Françoise Kerleroux (1996), and Bernard Fradin (2003). Together, they created a new thriving research community, of which I am proud to be a member.

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