Theory, Kernels, Data, Methods

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Abstract

In this paper, I suggest that we should think of the work of our discipline as consisting of theory, kernels, data, and method, where by *kernel* I mean the large body of results that phonologists develop that are not language-independent (hence, not part of theory), but which constitute the work on which we evaluate any and all theories coming down the pike. Focusing too closely on theory—especially on the hype self-generated by the theory and its theorists—does not encourage us to see the continuity and cumulativity that in fact exists, and without which there would be no field of phonology.

1 Introduction

The call for papers at a recent phonology conference began,[1]

> How has phonological theory developed? What are the key driving forces in its history? What are the mechanisms whereby theoretical models succeed each other in phonology? To what extent is there progress in phonological theory and in our understanding of the interfaces between phonology, phonetics, and morphosyntax? Can we draw normative lessons for the practice of phonological research from a historical study of past developments in the field? These questions have come to the fore recently as a result of rapid shifts in scholarly opinion on a number of phonological issues. ...
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> Faced with such swings in phonological opinion, we may legitimately wonder to what extent the history of phonology exhibits the developmental pattern known as 'progress'. According to a popular view, a science progresses if, in the course of its development, each new theory subsumes all of the corroborated empirical content of its predecessors whilst making new valid predictions. Phonology does not appear to progress in this way, since all too often new phonological models define themselves by the rejection of the putative results of previous work. ... if phonology does not progress linearly, how are we to interpret its toings and froings? Do they form a virtuous spiral that approximates its target as it goes round and round? If we abstract away from the accidents of sociology and fashion, is it possible to give a rational reconstruction of the historical evolution of the discipline?

There is both good news and bad as we look back on the recent and not so recent history of phonology. The good news is that we have made a great deal of progress in both the scope and the depth of what we understand about the phonology of the world's languages. The bad news is that we have not done this work as well as we should have. Perhaps that's not really bad news: that's just to say that we are all human. Bravo to the organizers of the Manchester conference for posing the questions that they have: they are not easy questions to answer, nor are they comfortable questions to ask.

The two principal themes I would like to address in this talk are, first, the role of cumulativity in the evolution and development of the field of phonology; and second, the tension between the images of continuity and rupture in our understanding of that history. These two themes are clearly related, but they are distinct nonetheless.

Why address these themes? It takes no deep analysis to recognize that lying behind the Manchester call for papers (and I think it is representative of sentiment in the field) is a sense of anxiety — an anxiety that

[1] See `http://www.englang.ed.ac.uk/mfm/17mfm.html`. An earlier version of this paper was presented at the 17th Manchester Phonology Meeting in May of 2009, and at Unicamp (Campinas). I am grateful for comments to a number of linguists for comments, including Bob Ladd, Nigel Fabb, Patrick Honeybone and Charlotte Galves; many of the points in this paper have also become clearer in discussions with Bernard Laks.

reflects a concern that we have violated some canons, but it is not clear what those implicit canons are. We may all take some refuge and some comfort in philosophy of science, but we never know quite how far to trust the philosphers, in part because we don't know if linguistics really should be measuring up to their expectations.

I do believe that we as linguists need to invest in a deeper understanding of where our ideas have come from, and I will offer some reasons today for why I believe that. There are four major reasons:

- First, more often than not (far more often than not), the original publication(s) in which an idea is introduced and presented are both more interesting and more enlightening than later recapitulations of the ideas (and certainly than in textbook presentations). Still, this is the least important of the reasons.

- Second, and much more importantly, studying the source of our ideas is the surest way to conceptual liberation, a state we always strive for (and occasionally we get somewhere close to it). This is counter-intuitive, but it is very true.[2] We saw this in the two preceding papers, those by Bob Ladd and by Tobias Scheer. They showed that in order make progress, we need to step back and peel back the genealogy of our ideas, so that we understand what those assumptions are: those assumptions were always answers to questions, but we have forgotten what those questions were (this is only natural: when we get the answers, we forget what the questions were). We have to peel back those answers, until we get to a place where we are comfortable again, and that start going forward: not to reject everything that has happened between then and now, but to understand which parts we are willing to put back in and where we would rather start afresh.

- Third, in order to be creative linguists, and to go beyond today's received wisdom, we need a stockpile of ideas: what seem like new ideas are virtually always largely composed of ideas, or idea fragments, that were formerly well known. Most of the time, the people whose work we admire the most (and whose work has influenced us the most) have been better scholars, at least in that they have been more knowledgeable about ideas from the past (or from another area, or framework).

Paul Feyerabend—in a book, *Against Method*[6], which any reader who has not had the pleasure of reading should definitely read—expressed a similar point of view in some respects. Though I don't agree with his emphasis on all of the points in the following passage, he expresses well the sentiment that I have come round to, that to do phonology must include understanding the conceptual history of our field. Feyerabend wrote,

> A scientist who wishes to maximize the empirical content of the views he holds and who wants to understand them as clearly as possible must therefore introduce other views; that is, he must adopt a *pluralistic methodology.* He must compare ideas with other ideas rather than with 'experience' and he must try to improve rather than discard the views that have failed in the competition. . . . Knowledge so conceived is not a series of self-consistent theories that converges towards an ideal view; it is not a gradual approach to the truth. It is rather an ever increasing *ocean of mutually incompatible (and perhaps even incommensurable) alternatives*, each single theory, each fairy tale, each myth that is part of the collection forcing the others into greater articulation and all of them contributing via this process of competition, to the development of our consciousness. . . . the *history* of a science becomes an inseparable part of the science itself—it is essential for its further *development* as well as for giving *content* to the theories it contains at any particular moment. (p. 30)

And the last point I would like to address, at least briefly, involves the error (easy for any of us to commit) of confusing phonology with phonological theory, of confusing linguistics with linguistic theory. Theory as such can and does change rapidly, often with less cumulativity than we might hope for; but phonology is

[2]There are three ways to gain clarity in our theorizing: formalization in order to make our ideas more explicit; confronting them with alternatives (as Feyerabend proposes—on which, see just below); and seeking out their origins. The reason for seeking their origins is not to satisfy our curiosity: it is because by doing this, we learn what questions these ideas were the answer to. Our views are a layered patchwork of ideas, many not transparent to us. And you can never understand an answer unless you understand the question to which it is an answer.

vastly larger than phonological theory, and if we take the right perspective and consider the field as a whole (and its relationship to linguistics as a whole), we end up with a much more satisfying and satisfactory picture of how things stand.

1.1 Kernels

There is an equation in my head, according to which phonology is really the sum of four things: theory, data (description), methods, and a fourth thing, that I will call *kernels*. I probably do not need to persuade any of you that the distinction between data and theory is a treacherous and difficult one, and if it is sometimes useful to us, we must bear in mind that there is no data without theory. Still, if the attempt to make a neat and clean distinction between the two is treacherous, it is no less true that there is a difference between the two, and that this difference can often be useful. But what is most unhelpful for us phonologists is the view that work has to be *either* theoretical *or* descriptive: there is in linguistics a third possibility, which looms large in our literature, and this involves the work that contains generalizations, but not language-independent generalizations. To say that another way: in the way we understand linguistics and linguistic theory, a theoretical statement should not mention the names of any particular language: it is not a fact of theory that English is an SVO languages, nor is any other statement about English. But there is an enormous amount that we know about individual languages, about groups of languages related typologically or diachronically; these generalizations are those that allow bridges across frameworks and that defeat the worries that might plague us, worries about whether it is possible to talk across differing paradigms and frameworks.

In phonology, these *kernels* (as I would like to call them) are what we know about things like sound inventories, vowel harmony, syllables and feet, stress systems, tone systems, and on and on: the things by which that we test and inspect any new theory.

Why introduce this new term? The main reason is just to allow us to describe what we see happen in the course of the development of our field: when a new theory arrives, it does not *simply* and *merely* deny and reject any and all previous theories, and then enter into a brand new conversation with phonological data. No, the new theory is investigated to see what it can say about vowel harmony, about phonological contrast, about syllable structure, about stress systems, and so forth. Those are chunks of knowledge that exist already, and without which any new phonological theory would have no chance of moving forward.[3]

It is a legacy of our positivist inheritance that we still fall into the trap of thinking that phonological work is either descriptive *or* theoretical, and so when we want to do more than describe, we think that what is left is theory. If that were the case, then all that would remain when a theory is demoted or demolished (or whatever it is that happens to bad theories) would be the facts and descriptions earlier generations had left us; but that is not so. You may remember the old joke about culture being what you know after you've forgotten all you ever learned. Kernels are what you know about phonology after you have forgotten all the facts you ever learned: they are what you know when you approach a new language in a family you know nothing about.

Here is what I think: for a number of reasons, most of them reasonable and understandable, we over-value theory and under-value other aspects of phonology which are equally important. These other parts could be described as kernels, description and method. In addition, the imbalance regarding the importance of theory leads to an unhealthy relationship to scholarship of the past, which is much more of our disciplinary wealth, our capital—our inheritence, our patrimony—than we often understand.

1.2 Cumulativity versus continuity

I would like to briefly emphasize a point that is perhaps sometimes overlooked: there is a difference between cumulativity and continuity. For a field to be cumulative (and aware of its cumulativity) is for it to be capable of identifying advances throughout its history, and to find a place within its current conception of itself for those advances. Some questions: What makes a field cumulative in this sense? What blocks it from

[3]Are there examples of new theories that did not move forward by hooking up with these kernels of phonological knowledge? The best example I can think of is the efforts to construct a connectionist phonology in the early 1990s, which started with efforts to account for past-tense formation in English, but which never really got any traction within the phonological community, largely because it did not make a serious effort to show how it would deal with the traditional kernels of phonological knowledge.

being cumulative? What results from failure to be cumulative? How does cumulativity differ from continuity in a discipline?

Last question first: cumulativity differs from continuity (or rupture) in the life of the discipline because a new theory or way of doing linguistics may radically change the way that we express and understand the earlier advance. Let's take an example that will come back (it is, after all, at the heart of phonology): the discovery of the difference between morphophonology and phonology. We find this difference —between two ways in which sounds are related in the system of a language, one way which involves replacement of one sound by another in the context of word-formation, and another which accounts for the variation in a sound depending on the sound context in which it occurs–as far back as Baudouin de Courtenay, and of course it was at the heart of structuralism, and if it leaves the scene briefly during the period of classical generative phonology, it came back with lexical phonology (though with different terms, such as "structure preservation" and "post-lexical phonology") and it is not likely to leave again for very long.

Cumulativity is a mindset, a willingness to understand what we know now as the result of the sum total of the workers in our field who have preceded us, and a way of seeing how our ideas do indeed fit together (though there may well be more than one such way, and there may be no final, or at least unique, truth in this matter).

1.3 Continuity and rupture

Let us consider two ideas that concern continuity (and its opposite, rupture) in linguistics and in science. Alas, they sound a bit flat when stated in brief, but they take on color when we look at some concrete cases:

1. First, the degree of *continuity* we can see is heavily determined by the granularity and distance from which we observe the timeline of the discipline. It seems to me that there are typically three degrees of magnification with which we can study the evolution of thought: close-up, lifetime, and century. Both the close-up and the century views lead to an emphasis on continuity; only the lifetime perspective emphasizes abrupt change. I will try to illustrate this observation.

2. Now, why should this be? The answer is probably the following.[4] When we focus on the activities of individual researchers or scientists, especially paying attention to (or even, simply repeating) their own accounts of their activities, there is a great emphasis on the effort that they invested in changing their minds or perspectives. There is inevitably—from a purely human point of view — great effort involved in changing one's mind, and in distancing oneself from a view one initially took, especially if one was not able to originally articulate that view explicitly. In addition, there are natural forces and pressures—sociological, economic, psychological, you name it—that encourage researchers to be poor observers of the range of ideas in a field at any given moment. Researchers tend, and with good reason, to focus on their own thoughts and how their thoughts come into competition with other accounts of the facts.

Historians and philosophers of science over the last century have expressed a range of opinions regarding the degree of continuity and the degree of abrupt change that is to be found in the history of thought and work in the sciences. At the beginning of the 20th century, the dominant view was that a great revolution that gave rise to modern science had occurred in the 17th century, but that in the years since, science had evolved through gradual accumulation of knowledge.

Part of this view began to erode under the influence of the work of Pierre Duhem, scientist and historian of science, who pointed out in detailed studies that the new ideas of Galileo and Newton were the developments of notions that were rooted in the 13th century: it did not all start in the 16th century [4]. Kuhn, in the 1960s, emphasized the lack of continuity in the shift from (what he called) one paradigm to another[15]; in this, he was both building on, and responding to, Karl Popper's influential ideas that the essence of science is the drive to falsify scientific theories, which then must be replaced with new theories[19].

There is a seeming paradox in the conflict between the interpretation of scientific change as gradual and as sudden and abrupt. Both views are widely held, and strongly defended—and with good reason and evidence. One of the reasons why both appear valid is because of a new view is always formulated by its innovators as a response to a previous view: so there is a vested interest, an important and real and totally honest perspective from which the innovators pronounce the differences between where their heads

[4]This thought occurred to me while reading *Science, Revolution, and Discontinuity*, by John Krige [14], a book on this subject; what I am saying here was certainly inspired by some of the things he wrote, and I give him full credit for anything useful I say about this.

are at that revolutionary moment and where they were just a few short months or years before: from their perspective, they have managed to leave behind them ideas and points of view that had felt right to them and to their teachers, but which now seem wrong and which it was a great struggle to overcome.

So the better the authors of a new perspective are at expressing their views, the stronger will be the message of abrupt change.

2 Case studies

In the rest of this talk, I'd like to discuss informally three topics, and a few aspects of how they have been treated in different periods, and speak to the question of cumulativity and continuity in our scholarship, with a focus on the importance of cumulativity and the forces and factors that may discourage conscious cumulativity. I will then end with some suggestions about what we can do in order to help the field do its job better. The three topics are autosegmentalism, syllable structure, and the morphophoneme.

2.1 Autosegmentalism and prosodics

The first experience on this list goes back to the time in the spring of 1976 when I was finishing my dissertation. Morris Halle was my thesis director, and he had read my work carefully, discussing every part of it with me. When it was nearly done, he told me I should be sure to add something to explain how autosegmental phonology differed from the work of Bernard Bloch and Zellig Harris on suprasegmentals in the late 1940s [1], [10]. I added an introductory chapter to the thesis in response to Halle's request, and it was for me quite an eye-opener how interesting and creative were the approaches that Harris and Bloch were exploring some thirty years earlier. I came away wondering why that work was not better known in the field.

A few years later, in the mid 1980s, researching a 1990 book *Autosegmental and Metrical Phonology*[7], I read Hockett's 1955 *Manual of Phonology* [13] for the first time, and was struck by how modern, how autosegmental and how metrical the work was, even more than I had been struck when reading Harris and Bloch. Hockett, it seemed, had developed ideas about syllables and suprasegmentals even further. Again, I came away wondering why that work was not better known in the field, and I made that point in the introduction to the book I was writing at the time.

As I thought more about the roots of what I and others were doing in phonology at that point, roots that seemed to be relatively obscure and poorly known, I spent some time rereading early papers on autosegmental phonology.[5] What I found was something obvious that I had failed to see before: in many important cases, including my own analysis of Igbo tone, the theoretical advances made in our phonological theory were directly built on analyses of African languages that had been written by linguists trained in the Firthian prosodic tradition. This realization led to a paper that I published in the *Journal of Linguistics* in 1992 in which I offered the argument that we can only understand the importance of cross-fertilization between theories in linguistics if we recognize that the areal disciplines (like African linguistics, Native American linguistics, Romance linguistics, etc.) are rich grounds in which this cross-fertilization takes place. You cannot be an African linguist and read only works by linguists who agree with your theoretical framework: when you have on your hat as an Africanist linguist, you happily read linguistic analyses of a language if you care about the language, regardless of what theoretical model is being used by the author.

It is by no means a straightforward task, in many cases, to distinguish phonological theory and work on African language phonology. To some degree, the difference between the two lies in the eye of the beholder. What may strike one reader today as a highly theoretical work may seem in forty years' time to be hardly theoretical at all, perhaps no more than a passing description of some facts, while another analysis – ostensibly a simple account of some observations – may be seen decades after the fact to be heavy-laden with new and original perspectives going well beyond the immediate subject matter of the paper.

A part – a large part – of the reason we may have so much difficulty in determining whether a particular work is a contribution to theory or to African linguistics derives from our unanalyzed assumptions regarding what the difference between theory and description is. It is difficult for most of us, I daresay, to remove

[5]Some of the following material in this section is taken from the 1992 paper in the *Journal of Linguistics* [8] that I refer to just below: "A Note on the Genealogy of Research Traditions in Modern Phonology."

ourselves from what we may call the "data versus analysis" myth: the myth that holds that there is in principle, or in practice, a line that can be drawn between linguistic description, which focuses on work with informants, and linguistic analysis, which consists of two parts: first, producing analyses of the data that have come from the informants, and second, producing and testing theoretical models which bear on the analyses of the data that the field-workers have so graciously provided us with; meanwhile, the theoretical models may bear on analyses by encouraging, discouraging, or even eliminating various such analyses.

The data vs analysis myth encourages a particular view of what the relation must be between linguistic theory and African linguistics: African linguistics must be primarily data-collection, and linguistic theory must be primarily analysis-production. If we start with assumptions such as these, then we may end up with surprising conclusions, such as "how theoretical African linguistics has become in the last ten years," or "African linguistics is certainly making a major contribution to linguistic theory these days." Now, we do hear such things, and not infrequently (underscoring the sway of this myth); I would like to offer a different perspective on the relation of linguistic theory to African linguistics, which has as its central theme the following idea: that one of the functions of linguistic theories is to establish professional affiliations and distances. Thus, while linguists working within a single theoretical framework may make serious efforts to remain knowledgeable about the work of their colleagues within the same framework, this effort is often counterbalanced by an unspoken sense that work which is not within one's own framework falls beyond one's immediate responsibility.

Theory, in such a way, can have the definite effect of fragmenting the field. A professional group such as the one which constitutes the field of African linguists serves the opposite function: it serves to unite, over space and (just as importantly) over time, the work of linguists in highly divergent theoretical frameworks. We may thus offer the following proposition: no historian of modern linguistics can understand the continuities in our field without tracing them through fields such as African linguistics, for that is where the important ideas of our times live, prosper, and remain fertile, often despite the Balkanizing effects of linguistic theory.

One personal example. In my own first work on autosegmental phonology, I was influenced by work by Will Leben, whose work in turn was a development within a generative framework of the work of such linguists as R.C. Abraham and J.T. Bendor-Samuel, to mention just two. When I looked for additional resources to develop the theory further, I went to find good grammars, and good grammars are necessarily based, to be sure, on the good linguistic insights of their authors. In the event, I found the grammar of Igbo published by M.M. Green and G.E. Igwe,[9] which in turn was heavily influenced by the earlier work of another linguist from the School of Oriental and African Studies ("SOAS"), Ida Ward, a linguist influenced, in turn, by J.R. Firth at SOAS.

Green and Igwe made little or no effort to develop a set of general rules for the material that they gathered, but the care and attention they gave to the tonal material, and the weight that they assigned to tone in the organization and presentation of their material, showed clearly their sense of the importance of these tonal factors for understanding the underlying phonological structure of the Igbo language. They also had a clear sense that apparent allomorphy in the language could reflect at times the syntactic structure of the Igbo sentence, and a sense that the apparent variety of surface tonal patterns on the verb in the various tenses must actually be the reflection of some deeper set of regularities in the language. It was this sense, I am convinced, that made subsequent autosegmental analysis using an autonomous tonal tier possible within an autosegmental framework.

Early generative thinking about African tone was not very successful, and much of it had little effect even on generative thought. Carroll's 1966 generative account of Igbo syntax and phonology, for example, did not apply early generative techniques to the point of developing new insights into the language. Work such as that of Edmondson and Bendor-Samuel's 1966 on Etung, and Arnott's 1964 on Tiv, work that was prosodically based, was more influential, even among generativists. Arnott's work on Tiv led to a reanalysis by McCawley (published in 1978), which in turn drew the attention of Leben in his influential dissertation 1973, and of Goldsmith 1976, and later Pulleyblank 1983. In the treatment of Tiv, for example, if we look for it, we can be struck—and I believe we should be—by the continuity in the description and the analyses of these authors. In the case of all the authors but the last, Pulleyblank, the focus was on the "tonal melody" as a unit, and how to treat this object that is distinct from the string of segments or phonematic units. There is a constant core of a body of data to be attended to, and to be reworked with tools that varied from case to case; but concern for the same core phenomenon lurking behind the data links all these analyses, despite changes in theoretical stance.

2.2 Theories of the syllable

More recently, over the last two years, I wrote a chapter on the syllable for the new second volume of the Blackwell *Handbook of Phonological Theory* which attempts to bring together the major trends in the treatment of the syllable in phonology.[6] I thought I knew that the syllable had a long and important tradition long before the 1960s, but the reading that I did for this chapter led me to the conclusion that there have been several important and cumulative traditions in the treatment and modeling of the syllable over the last one hundred years, and the more I read the current literature on the syllable, the more astonished I came to be at the lack of familiarity with the literature there is in the published work today. Perhaps the most striking is the state of the art paper by Eli Fischer-Jœrgensen from 1952, in which she summarizes what is known at that point; her discussion makes some points clear that phonologists continued to be unclear about for another two decades or more, like the discrepancy between the range of consonants possible intervocalically inside a word, on the one hand, and the sum of what appears word-finally plus what appears word-initially. In a few languages those two sets are the same, but in many there is a discrepancy, and the discrepancy can lead to one set or the other being larger.

If we look at the literature in the 20th century on the syllable, it divides fairly easily into five tracks, I would suggest:

- Sonority, the first and oldest view: the syllable is a wave of sonority, from trough to peak to trough again. There is something—perhaps it is energy, perhaps it is jaw position, perhaps it is something more abstract—there is something that goes up and something that goes down, and the syllable is what you get when you make cuts at the troughs. We find this is Panini, in Whitney [17] and Saussure [3]; it is developed by Bloomfield [2] as well. It was in some respects choked at the throat by the second theory.

- The syllable is the smallest (or one of the smallest) levels of hierarchical structure in language. In some fashion this is the standard view today (though I do hate that term—"standard view"—I must add).

- The syllable is the natural unit of expression of phonotactics involving nearby segments in phonology. This idea was clearly expressed by Whorf [21] and developed in the 1950s by Einar Haugen [12].

- The syllable is the unit at which the temporal order of segments is predictable on phonological grounds.

- The syllable is an important level for predicting the duration of segments and groups of segments;

These themes (at least the first three) have a long history, and that history had nothing to do with generative phonology until the late 1970s and early 1980s. To undertand these approaches, one must understand where they came from, and what they propose to explain.

The second view, focusing on hierarchical structure, was the conscious result of a Bloomfieldian to apply Bloomfield's notions of syntactic analysis to phonology. This fact alone put a whole new light, for me, on the rediscovery in the 1980s that there were similarities between structure in phonology and structure in syntax: that had been originally hypothesized by Bloomfieldians, on methodological grounds.

There are two relatively recent collections of papers on the syllable—both from the last ten years—that we can turn to to get a sense of what kind of historical perspective a typical working phonologist adopts and expects others to adopt. Both are fine books by excellent linguists, friends and colleagues to all of us, so I hope they will forgive me for mentioning them by name. One dates back to 1999, the other is a bit more recent (2003), focusing on the syllable in OT.[7] Let's take a look at what we see. And remember, these are all excellent phonologists, and excellent books that I commend for your attention. But for some reason that they do not appear to be able to even see, they write as if the appropriate history of the relevant ideas begins with the *Sound Pattern of English*. And this simply doesn't make sense, and it doesn't make sense of the history of the post-SPE publications on the syllable, either.

The first overview, in *The Syllable in Optimality Theory* (edited by Caroline Féry and Ruben van de Vijver), begins with the observation that "the syllable has (nearly) always played a central role in phonological

[6] In this volume, we—Alan Yu, Jason Riggle, and I—have been asking the authors to ask what the questions have been that phonologists have been asking over the past several decades, and then to attempt to evaluate how well the discipline has done at answering those questions.

[7] [20] and [5]

theory, but with the recent advance of Optimality Theory (OT), its role has become crucial." The authors then step back to give a more historical perspective, and write,

> In the seventies, several phonologists, such as Vennemann (1974), Hooper (1976), and Kahn (1976), proposed including the syllable as a prosodic unit in generative phonological theory. The relevance of the syllable for linguistic theory has increased ever since. (p. 3)

They do not cite any earlier phonologists—except Jakobson, and in the context of the observation that all languages allow for CV syllables.

Van der Hulst and Ritter go back a few years earlier, to 1968, and they—quite correctly, in a sense—entitle the section, "Reintroducing the syllable."

> The, by now, traditional manner of motivating the need for the syllable in phonological analysis is to memorize the position that Chomsky and Halle took in their *Sound Pattern of English* (1968)(SPE). Assuming that the best theory of phonological representation is the most minimal one, Chomsky and Halle proposed that a phonological representation is simply a string of unordered feature bundles, provided with a set of boundary symbols ...

They offer just a few references to work that predates 1968 (the exceptions are Hockett's *Manual of Phonology*, 1955, Kurylowicz [16] (which is in Polish, unfortuntately for us), Pike and Pike [18], and Vogt 1958; at one point (p. 22), they write, "an historical overview of the concept syllable before generative phonology can be found in Adewyck 1975," a book that I have not been able to find anywhere.

2.3 The phoneme and morphophonemics

I referred a little while ago to a distinction that I called basic in phonology, the difference between the morphophoneme and the phoneme, and I described these as two aspects of the description and analysis of a sound system, one way (morphophonology) which involves replacement of one sound by another in the context of word-formation, and another (phonology) which accounts for the variation in a sound depending on the sound context in which it occurs. Now, I don't expect anyone to be terribly happy with that as a definition (I certainly am not), but I intend it more as a characterization, a way of describing a distinction that should be recognizable to phonologists subscribing to very different theories and working within very different traditions.[8] Another way of putting this is that we understand that some sound alternations are logically linked to word-formation processes, while others are not. Many a theory has been based on filling in the details of such an account.

Now, certainly one can disagree with this distinction (between phoneme and morphophoneme), in the sense that one could think that it is a useless distinction (one that has no interesting consequences), or one could think that any attempt to make it clear to the point where it can be applied unambiguously in a good range of cases has led, or will lead, to failure. But it seems to me I think it is a good example of a *kernel* of phonology: a result of our disciplinary research that exists independently of a theory—and in fact, an example of the sort of thing that a theory is developed in order to account for.

One of the striking ironies of apparent phonological amnesia in my lifetime involves the way in which lexical phonology has resuscitated the phoneme and given it much of its previous importance, but without making the point explicitly. The lexical part of a lexical phonology is in essence what Zellig Harris would call the morphophonology, while the post-lexical phonology is what Harris would call phonology; from this, it follows (for a structuralist) that lexical phonological rules are structure-preserving insofar as they do not create segment types outside of the underlying inventory of the language. Indeed, it seems to me that if structuralist phonologists had had something to say to early generative phonologists, it probably would have been to create something like lexical phonology, only 20 years earlier. But they did not. *Dommage.*

[8] There will occasionally be a phonologist or two with a framework that does not make the difference, but within a larger community such a phonologist will at least understand the difference to the point of saying he has no need for that distinction.

3 Kernels

It should be entirely obvious from the examples we have considered so far that I am of the opinion that we desperately need a word to refer to the knowledge and analysis that we phonologists (and more generally, we linguists) have learned that is at right-angles to phonological theory. We need a word to refer to the kind of knowledge that we have of—for example—vowel harmony, African tone languages, syllables, quantity-sensitive and quantity-insensitive stress systems, and so forth: those areas that we know about regardless of the particular phonological theory we espouse at a given moment, those phenomena we may study without being satisfied with our current accounts of them, those phenomena which we should expect our students to master, those areas that we feel a new theory needs to say something about if it claims to be a theory of phonology.

I propose (even though I am not entirely satisfied with the choice) the word *kernel*. Of course, Zellig Harris used it in an entirely different sense, and the term stayed in linguistics for a while in the area of syntax, and the use that I am suggesting has nothing to do with that Harrisian/Chomskian use (which itself came from algebra, as you may know). I suggest *kernel* because it suggests something dense and something which does not go easily down a kitchen sink. It also suggests discreteness, which may (or may not) be a good thing.

Our professional responsibility must be to identify and nurture these kernels that we and the phonologists who have preceded us have handed on to us. Yes, part of that is integrating them into various and sundry phonological theories, but it is also refining, sharpening, correcting, and extending them that we need to do.

We need to think of a large part of our patrimony as phonologists as being ensemble of kernels of phonological knowledge, much like the knowledge that a chemist has of the compounds of the natural chemical world. Just as a chemist or a biologist must learn, while a student, what we know about the organic and inorganic chemicals of the natural world around us, we phonologists need to assemble coherent handbooks of what we know about the phonology and the phonologies of the world's languages. This means writing papers that do *not* focus on evanescent theoretical constructs. I will come back to where and how to publish this material – it is not obvious that journals, as they are currently conceived of, would be the right place for it.

4 What might be done?

There are, I think, two problems that we wish to solve at this point, more than any others: one is how we establish better working relations with our ancesters (how do we get in tune with them)? They spoke another language; it's really hard nowadays to sit down and read Zellig Harris ([11], for example).[9] The other problem has to do with the fact that we're entering a digital age—we're already in that digital age, indeed. How do we deal with the explosion of publications? I would like to suggest something that would speak to both problems at the same time. It's inspired a bit by amazon.com: I find the discussions of books by readers on the English version of Amazon very useful (this is just one example of what's called Web 2.0 phenomena).

Many countries, like my own and Great Britain, have a professional organization for linguists. I propose that membership in the LSA and the LAGB comes along with a bagful of 25 points per year to any dues-paying member. Each member can take his or her points over the course of the year and assign them to any publication they find in a journal, a book, or simply on the Internet. The LSA will keep track of these points, much like Amazon keeps track of the evaluations that users offer of the books that they sell. But in this case, each person has only a finite amount (I have said 25 points, but that could be modified, as long as it's fixed and not too large) of points to give away. The LSA will then be responsible to serve as the permanent clearing house of the points that we all have offered to various publications as a vote on the fundamental worth of a paper or book. It would keep on its website various ways that would be of interest to accessing this information. The most obvious is an up to the minute ranking of all publication by the number of Linguist Points given to it, as a way of keeping in touch with what a cross section of the LSA thinks we should read. Interest in an older publication can be raised—there is no preference in such a procedure

[9] By the way, it's not hard to read William Dwight Whitney. If you've read Bloomfield, and thought that that was about as far back as you could go, do pick up Whitney—he was a great writer on linguistics.

for newer publications, at least none built into the system. A system of this sort could encourage internet publication, by allowing a kind of evaluation that is less than refereeing but nonetheless of some professional value. An on-line journal (or an off-line journal) could be evaluated in part by how its articles are voted on by the field. The information could be given to Google and other search engines, and we could let them integrate that into a search procedure if the user cares to use it as a search criterion. The world has become digital, the world has become able to access more information, but we need the help of each other to keep access to the best of what is out there. This proposal, the distribution of Linguist Points, is a proposal to do that, and to put it in the hands of Everyman – which means you and me.

Other displays of the information could be useful: We could look at the top publications of 1951, for example. Or another possibility: you find a paper you think is important, perhaps one that is not widely known, but one which has some Linguist Points attached to it: you ask the database what other papers were voted for by the same people who voted for this one. Perhaps that would be useful information to have.

I think that there are a number of effects such a system would have, but the one that I am most interested in at present is as a way of bringing back to our present awareness work from the past that we should know about. None of this is possible without the internet, clearly, but it has become a central part of our professional lives over the last 10 years anyway.

5 Conclusions

I'll try to summarize what I have said now. I believe that we phonologists need to have a more global view of our field, and we need to take phonological theory in a more balanced fashion. Theory is vital, essential, and by no means the whole story. And even where it is well done, theory must not be allowed to follow its usual exclusionary and totalitarian bias: no one theory, no single theory tells the whole story, and no theory remains honest and hard-working as long as it is the only theory in town. All theory needs hungry competition to stay lean and effective.

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