How Long was the Nineteenth Century? *
(Quanto Tempo Durou o Século XIX?)

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Abstract: This paper reviews the problems of the deterministic and predictive view of language change initiated by nineteenth century linguists and shows that such a view is still present in many analyses proposed by twentieth century linguists. As an alternative to such a view, the paper discusses an approach along the lines of Niyogi and Berwick (1997), which takes the explanation for long-term tendencies to be a function of the architecture of UG and the learning procedure and of the way in which populations of speakers behave.
Key words: Historical Linguistics, Langue Change, Language Acquisition, Dynamic Systems

Resumo: Este artigo faz um revisão dos problemas da concepção determinista e preditiva da mudança lingüística iniciada pelos lingüistas do séc. XIX e mostra que tal visão ainda está presente em muitas análises propostas por linguistas do séc. Como alternativa a essa concepcção, o artigo discute uma abordagem nos moldes de Niyogi e Berwick (1997), de acordo com a qual tendências de longo prazo surgem em função da arquitetura da GU e do procedimento de aquisição, e da maneira como as populações de falantes se comportam.
Palavras-chave: Lingüística Histórica, Mudança Lingüística, Aquisição de Linguagem, Sistemas Dinâmicos

1. Introduction

Darwin and Marx had a view of history which has been very influential in the past century, a view which involves *principles of history*. Contemporary linguists were intimately involved in this distinctive, nineteenth-century approach, influencing Darwin and Marx and being influenced by them. And those linguists have shaped the way in which

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* This paper deals with the history of ideas on the connection between language change and language acquisition, two matters of central importance in Mary Kato's work. The ideas are discussed in more detail in Lightfoot 1998.

the rest of us have thought about language change. Here I want to trace the history of some of these ideas and to show how they continue to influence our work.

The ideas I identify were real, although the reality is more complex than I will describe. Anna Davies (1998) noted that "starting with the nineteenth century there is ... a chasm between what linguists do and what they say they do. The method and achievements of the historical and comparative linguists of the nineteenth century must be extracted from their technical work rather than from their prefaces or their rhetoric" (p. xxvi). The same is true of modern work.

The study of language goes back to the early Arabic grammarians, to Greece and India in the pre-Christian era. Not much of the early work on language dealt with change and historical relationships, but some did. Sir William Jones is the marker. His famous after-dinner speech, postulating a common source for Greek, Latin and Sanskrit, which may no longer exist, initiated an unbroken tradition and eventually gave birth to an independent discipline which we now call linguistics.

Linguistics began as a *historical* science. There are many questions one can ask about language: how it is acquired by children, how it is used by poets, how it varies sociologically, and so on. The central research question for the nineteenth century was: how did a language get to be the way that it is? As far as Hermann Paul was concerned, this was the only possible question: "It has been objected that there is another view of language possible besides the historical. I must contradict this" (1891: xlvi).

As the nineteenth century progressed, historical "laws" were formulated with ever greater precision. In 1822 Jacob Grimm revised his *Deutsche Grammatik* by adding a 595 page account of the phonology of some fifteen different languages and stages of languages. He postulated his famous law dealing with consonant shifts in Germanic. There were several exceptions, cases where Grimm's correspondences did not hold. Others cleared them up in the course of 50 years. The coup de grace came in 1875. Grimm's Law worked quite well for initial consonants, but there were still many apparent exceptions in medial position, and they were explained away by Karl Verner. Verner's Law yielded a more or less complete understanding of the evolution of the Germanic consonantal system and led to the triumphant notion that this was the

way things always were: sound change was always regular, exceptionless, and phonetically conditioned.

The idea that sound change was regular and systematic was formulated in 1878 in the preface to Osthoff & Brugmann's somewhat spasmodic journal *Morphologische Untersuchungen*, and the people who held the idea of exceptionless regularity were the neogrammarians. They are generally characterized in terms of their ideas about regularity and phonetic conditioning of sound change, but the manifesto stressed two other points: that a language is not an object with a reality of its own independent of its speakers (contra the Hegelian, group-psychology notions of Schleicher and others), and that the psychological and physical aspects of language must always have been the same.

The field of linguistics first identified itself by claiming that language history was *law governed*, even if the notion of law was scarcely that of Boyle's Law or the law of gravity, which are timeless. The term referred to specific sound changes or "correspondences" affecting specific languages at specific times. Languages were supposed to change in systematic ways and historical linguists, perhaps more than other kinds of linguists, have always been concerned with issues of explanation. The question then arises of what kind of explanation could be offered for sound changes of this type.

2. Historical explanations

Work on language history at this time reflected the dominant models of what a science should be: Newtonian mechanics and Darwin's theory of evolution. Newton had all phenomena describable by deterministic laws of force and motion, in such a way that all future states were, in principle, predictable in a "straight-line", linear fashion from a complete knowledge of the present state. This inspired the notion of sound *laws* to describe the history of changes. Darwin was inspired by work on language history, and he in turn inspired the linguists to view languages as natural organisms, on a par with plants and animals. This influence was explicit in the writing of Schleicher, Bopp and Pott.

Nineteenth-century linguists knew that language reflected psychological properties. However, the psychological notions of the time

were problematic, partly because they were wrapped up in ideas of
Hegel's. Grimm, for example, adopted a mystical belief in a Hegelian
Sprachgeist, which had some existence above and beyond individuals.
This group psychology was attacked by Paul (1891: xxxivf) as being
incoherent :

> All psychical processes come to their fulfilment in individual minds, and
> nowhere else. Neither the popular mind (*Volksgeist*), nor elements of it, such
> as art, religion, etc., have any concrete existence, and therefore nothing can
> come to pass in them and between them. Away, then, with these abstractions!

Whatever the problems, linguists separated this kind of
psychologizing from their day-to-day work. Pedersen's (1931) survey
of the nineteenth-century scarcely refers to the psychologists at all.
Similarly Davies 1998 has little to say. Whitney put the demarcation
clearly:

> The human capacity to which the production of language is most directly due
> is ... by no means a unitary capacity; on the contrary, it is a highly composite
> and intricate one. But it does not belong to the linguistic student to unravel and
> explain ... it falls, rather, to the student of the human mind and its powers, to
> the psychologist. So with all the mental capacities involved in language ...
> (1875: 303).

And this was the general view of linguists through Bloomfield and
the structuralists: leave psychology to the psychologists. So linguists
generally did not appeal to psychology to explain historical changes.
Instead, there were independent laws of history to be found.

The neogrammarians were the culmination of this research paradigm
but they confronted two major problems. First, there were regularities
of language change which could not be stated in purely phonetic terms,
which suggested that it wasn't the language or the sounds which were
changing but rather some kind of abstract system. This matter has been
discussed by twentieth century generativists but it didn't bother
nineteenth-century linguists, because they thought of language as a
collection of words, with everything else due either to universal "logic"
or individually variable "habits". So there wasn't anything to have a
history of except words, their pronunciations, and their meanings.

There was much debate, however, about the second problem, the causes of sound change. Grimm's and Verner's Laws were not general laws like Boyle's Law and therefore they required a deeper explanation. Changes were taken to be *directional* ... as in biology, where the replacement of one species by another was taken to result from a mutation which yields an organism which is more successful in the struggle for survival in a particular environment. Rask 1818 held that languages became simpler. Darwin 1874 thought that words became shorter and easier to pronounce. Schleicher 1848 identified a progression from isolating to agglutinating to inflectional types, although this was said to hold for preliterate societies and Rask's drive to simplicity was relevant for postliterate societies. There was widespread agreement that language change followed fixed developmental laws and that there had to be a direction to change, but there was active disagreement about *which* direction that was. This was a matter of live dispute. By the end of the nineteenth century there was an enormous body of work on sound correspondences between historically related languages and vast compendia of changes which had taken place in many Indo-European languages. But alongside such compendia there were few ideas of why those changes had happened. Eventually the directionality view crumbled.

The notion that languages became simpler/more natural/easier to pronounce was, first, circular. "Simpler" etc is what languages change to and there was no independent definition in a framework dealing entirely with historical change. Linguists regarded their work as essentially concerned with language change, so they sealed it off from other concerns, and did not work on language acquisition in an integrated way. So they had no independent way to define their central notions.

There were occasional attempts to break out of the circle by invoking psychology, but the psychology was implausible. So Grimm (1848) explained his law of consonant shifts as

> connected with the German's mighty progress and struggle for freedom ... the invincible German race was becoming ever more vividly aware of the unstoppability of its advance into all parts of Europe ... How could such a forceful mobilization of the race have failed to stir up its language at the same time, jolting it out of its traditional rut and exalting it? Does there not lie a certain courage and pride in the strengthening of voiced stop into voiceless stop and voiceless stop into fricative?

Explanations of this kind never had much going for them, but they were curiously resistant. One has to see them for what they are: psychological elements introduced into essentially a historicist and a-psychological theory as an attempt to break out of a narrow circle and reach some level of explanation.

By the early twentieth century the data of linguistics were an inventory of sound-changes occurring for no good reason and tending in no particular direction. The historical approach had not brought a scientific, Newtonian-style analysis of language, of the kind that had been hoped for, and there was no predictability about changes. So the program was not viable; no sustainable explanations were available for the observed phenomena, i.e. historical changes, and there was no science of history which met nineteenth-century demands.

The historicist paradigm was largely abandoned in the 1920s, i.e. the notion that there are principles of history to be discovered, which would account for a language's development. In fact, there was a virulent anti-historicism in the writing of structuralists like Boas, Bloomfield, and Sapir. They worked on language change to their deaths, showing that the traditional methods were as applicable to the unwritten, indigenous languages of North America as they were to Indo-European. However, they abandoned *historicism*; they abandoned the earlier program of seeking to explain how it was that languages came to be the way they are. The perceived problems related to the circularity of invoking historical principles and to the psychological claims. Sapir (1929) wrote that the psychological interpretation of language change was "desirable and even necessary" but the existing psychological explanations were unhelpful and "do not immediately tie up with what we actually know about the historical behavior of language." Bloomfield (1933: 17) complained about the circularity of Paul's psychologizing, saying that there was no independent evidence for the mental processes other than the linguistic processes they were supposed to explain.

3. Determinist views of history

The deterministic view of history that the linguists articulated, the idea that there are laws which determine the way that history proceeds, is a hallmark of the nineteenth century. Biologists, political historians,

and linguists expected to find principles of history, as if there were a Newtonian-style, deterministic, predictive theory of change to be found. Give us a full, detailed, and accurate description of an organism or a political organization or a language, and, armed with our theory of change, we will give you a prediction of what that organism, society, or language will be like in two hundred years.

Darwin was too much of a Victorian not to appeal to notions of progress, but he was critical of the notion and modulated in his appeals to it. Marx too had an interesting theory of change in which ideas are socially embedded and are amended through conflict, through the clash of theses and antitheses. Marx's view of social change and revolution, in which small insults to the system build up until the system itself breaks, is quite a sensible approach. However, Marx was very much a nineteenth-century thinker in that he was caught up in notions of predestiny and determinism, particularly in theories of history, developing historical laws prescribing that a feudal society must necessarily develop into a mercantilist society, a mercantilist into a capitalist society, capitalism into socialism, and socialism into communism. For Marx, the real task of economics was to explain how society evolved over time. At his funeral, Engels eulogized him in a way that he would have liked: "Just as Darwin discovered the law of evolution in organic nature, so Marx discovered the law of evolution in human history."

Marx's approach to political history grew out of the idea that there is a political *science*, a science of the relationships of human beings to each other and to their environment, meeting usual scientific standards. This was an idea of Hobbes and Spinoza and their followers, and it became more and more powerful in the eighteenth and nineteenth centuries, as the natural sciences flourished and as the view emerged that anything not reducible to a natural science could not properly be called knowledge at all. The scientifically minded philosophers of the eighteenth century believed passionately in such laws, in a science of society.

It has been argued that not only is there a single, coherent, evolutionary process to history, but that it even has an end-point. Hegel and Marx believed that there was an end-point: the evolution of human societies would end when mankind achieved a form of society that satisified its deepest and most fundamental longings. For Hegel, this was the liberal state, and for Marx the end-point was communism. Francis

Fukuyama (1992) has argued that the natural end-point is a liberal democracy and that this end-point has now been reached, at least as an *ideal*.

The "inexorable laws of history" never rose above the level of truisms ("most revolutions are followed by reaction"). As a system of knowledge, their fundamental problem is that they are too principled and exclude contingency, accidental, unpredictable factors which may have large consequences. That is where proponents of a determinist science of history are quite wrong.

This problem is aggravated by another nineteenth-century problem: formulating the primitives in terms of gross categories like classes and types of society (mercantile, capitalist, etc). In contrast, Jon Elster has emphasized the need to approach political psychology through what he calls methodological individualism: one must "study the individual human action as the basic building block of aggregate social phenomena" (1993: 8). This is a self-consciously reductionist account, seeking to explain the complex by the simple. When we talk about linguistic change, we face a precisely analogous problem in deciding the scale of our categories. I echo Elster's view and argue that we understand "language change" best as an aggregate of changes in individual grammars.

At the other extreme, many scientists do not consider history a part of science, because it deals with particulars and contingency. But this too is not right and reflects a false taxonomy; history may resist straightforward analysis because it is so contingent, but that does not necessarily make it not part of science, rather just a different type of science, as Stephen Jay Gould has argued in many places.

History is chaotic in a technical sense. Systems are sensitive to slight variations in initial conditions. The fact that deterministic rules are indeed at work does not mean that there is predictability – they do not govern sequences of events. The deterministic laws governing weather formation are unstable and miniscule changes at one location may percolate through the system to cause major effects elsewhere – sensitive dependence on initial conditions. There is no hope of making long-term weather forecasts taking such elements into account. But there is order behind chaos; deterministic rules are at work, and there may be simple causes for complex and unpredictable effects.

If language or social history works like this, through what we may think of as scientific chaos, we shall never predict the future development of German grammars or of the US political system, but we may be able to understand local effects, why some change took place in the way and at the time that it did, if we can identify the source of the perturbation. Given a good theory of grammar or society or a species, then we may be able to predict that if the initial conditions are changed a little, then a particular grammar, society or species may change in some fashion; but in general, one cannot predict changes in initial conditions. Change is explainable but not predictable. This means that if one wants to know what will happen with the weather, the stock market, US political history, or the development of German, the best one can do is turn the system on and just watch it unfold.

4. Historicism

This kind of approach, however, is not generally adopted. The nineteenth century has lasted a long time and much modern work is more historicist and perpetuates that special, nineteenth-century, predictive view of history. There has been a revival in work on change over the last few decades, but the nineteenth century teleological ideas about history have re-appeared in the modern work.

In the 1970s much work focused on the notion of "drift", originally due to Sapir 1921: ch.7. Unlike Sapir, the typologists invoked a drift as an *explanans*, not the *explanandum*. The typologists remained faithful to the methods of the nineteenth century. They retained the same kind of determinism and they dealt with the products of the language capacity rather than with the capacity itself. Other modern approaches have been less conservative. They have formulated change in terms of changes in grammars, abstract systems of some kind, but they have retained the commitment to a nineteenth-century, deterministic, predictive history.

These approaches compare the grammars of various stages of a language and identify tendencies at the grammatical level. So some people have argued that grammars tend to simplify over the course of time. Bauer (1995) offers a historicist approach in a novel guise, appealing to biological factors which predict historical developments. Bauer adapted the work of the typologists and avoided postulating

principles of history. Like the typologists, Bauer was interested in big, comprehensive changes. Bauer argued that Latin was a thorough-going left-branching language, which changed into a thorough-going right-branching system in French.

There are several differences in branching structure between the grammars of Latin and French. In fact, right-branching structures emerged at very different times and at different rates. Those details, if they could be established, might enable us to track changes in primary linguistic data, which is what we need for an explanation of the grammatical shifts.

That, however, is not Bauer's view of how the changes should be explained. Rather, she views the change in direction of branching as irreversible and adopts an "evolutionary concept of language change", under which general and irreversible language changes are viewed as analogous to evolutionary change at the phylogenetic level. Under this view, general, linear, irreversible, and unidirectional changes are due to a natural selection process. She speaks repeatedly of the "advantages" of a right-branching system and she implements these ideas by arguing that the switch from left branching to right branching represents evolutionary progression because left-branching languages, at least as manifested in Latin, were hard for children to acquire. Hence the progression to right-branching structures:

> Latin must have been a difficult language to master, and one understands why this type of language represents a temporary stage in linguistic development (p. 188).

So, she explains her diachronic change not in a mysterious theory of history but rather in terms of human biology: our brains work in such a way that complex structures in left-branching languages are hard to acquire. This is more sophisticated but, of course, it immediately raises the question of why early Latin would have been left branching:

> If left-branching structures are less recursive and are acquired with greater difficulty, it is indeed legitimate to wonder why languages, in an early period, exhibit this kind of structure (p. 216).

She concludes that this "still remains to be explained".

5. Diachronic reanalyses

Consider now a very different kind of work, more mainstream generative, but of a kind which also appeals to deterministic views of history. A grammar emerges in a child on exposure to some particular trigger experience (PLD); a different grammar may emerge if a child is exposed to a different trigger (1).

(1) PLD_1 (UG —> $grammar_1$)
     PLD_2 (UG —> $grammar_2$)

It is a fallacy to think that there could be formal operations that relate sets of PLD.

(2) PLD_1 —> PLD_2

The notion of a formal device operating on sets of PLD has re-appeared in the recent work of Ian Roberts and it is linked to an attempt to explain some changes entirely through UG, independently of changes in trigger experiences. Roberts 1993 invokes a new technical device, what he calls a Diachronic Reanalysis, taking place in the sixteenth century. Modal verbs and periphrastic *do* originally moved to a Tense position (T) (3a) but came to be base-generated there (3b). A Diachronic Reanalysis maps one analysis into another.

(3) a. NP_i $[_T$ do/M_j $T^{-1}]$ e_j $[e_i$ VP$]$ =>
     b. NP $[_T$ did/M$]$ VP

Diachronic Reanalyses, we are told, are provoked by principles of acquisition, often by the "Least Effort Strategy;" the Least Effort Strategy led children to reanalyze (3a) as (3b) in the sixteenth century. So early grammars had structures like (3a) and later grammars had simpler structures like (3b). The learning strategy is: when faced with highly ambiguous PLD, children acquire a grammar with covert movement rather than one with overt movement. They "follow a least effort strategy in that they try to assign the simplest possible parse to the input string" (Clark & Roberts 1993: 335).

In reality, Roberts' Diachronic Reanalyses and parametric shifts more generally are NOT provoked by the Least Effort Strategy. Specifically, the Least Effort Strategy cannot be "the sufficient condition for the move from one step to the next" (Roberts 1993: 159). If there were no change in trigger experiences, there would be no changes in grammars. If representations like (3a) disappeared, it was because they ceased to be triggered; the children who did not acquire them must have had different experiences from earlier generations – it wasn't because they were more sensitive to the demands of the Least Effort Strategy.

Hale (1998: 13) discusses this nicely. He points out that in the model that Roberts & Clark assume, parsing requires that a child posit a numeration, find that that numeration can converge at LF when run through the computational operations, and posit the relevant features on the functional heads to allow convergence at PF. "As Chomsky has pointed out (Chomsky 1995: 227), Economy of Derivation is relevant only to the evaluation of derivations involving the same numeration. It cannot, therefore, be invoked to choose between these two competing hypotheses since they involve different numerations" (p. 14).

Paul Kiparsky is also impressed by what he takes to be long-term tendencies and by asymmetries like the prevalence of object-verb systems changing to verb-object and the rarity of the reverse development (Kiparsky 1996, 1997). Like Roberts, he also appeals to internal causal factors, forces of "endogenous optimization." However, Kiparsky's position is different. He does not rely exclusively on these endogenous forces and he requires other, external factors to interact with the internal forces. He argues that the "enabling cause" of the change to verb-object syntax in certain languages was verb-fronting in embedded clauses. Yiddish, for example, shows general verb-fronting, where verb-fronting appears in all types of embedded clauses, while German grammars show verb-fronting only in matrix clauses and in embedded clauses with no complementizer. Yiddish has sentences like those of (4a), where the equivalent in German would be (4b).

(4) a. A jidisch mejdl hot sech barimt, as efsher hundert mol **hot** men si schojn gebetn, as si **sol** chassene hobn.
      'A Jewish girl bragged that she had already been asked perhaps a hundred times to marry.'

     b. Ein jüdisches Mädel hat sich gerühmt, dass man sie vielleicht hundert Mal schon gebeten **hat**, dass sie heiraten **soll**.

The generalization is that "the shift from a head-final base to a head-initial base took place in exactly those languages which developed general verb-fronting in embedded clauses" (p. 155).

So Kiparsky's claim is that the change to verb-object order is an endogenous optimization, but one which is "enabled" by certain verb-movement operations. Crucially for our discussion here, Kiparsky recognizes that grammaticalization or, more generally, any optimization account does not by itself explain why a change takes place in one language but not in another closely related one. His account motivates the change to verb-object syntax through environmental factors, and to that extent the account is not historicist, even though he invokes optimization effects. Such a story raises empirical questions about how much influence the internal and external factors each had, but it does not raise the methodological problems of historicism.

6. Trajectories

So far I have been sceptical of work offering principled explanations for purported, long-term historical tendencies, because the historicist principles strike me as implausible. I do not see how an historical law can be anything other than an epiphenomenon, an effect of other aspects of reality.

Niyogi and Berwick (1997) (NB) have developed an interesting computer simulation of language change, which suggests more sympathetic ways of thinking of these long-term tendencies, which do not fall into the traps I have discussed.

As is natural, NB take the problem of grammar acquisition at the individual level as leading logically to the problem of language change at the group or population level. If it is possible that children might not converge on the target grammars, then, over several generations, this could drive language change. If one has an adequate grammatical theory, then there are two means by which the linguistic composition of the population might change over time.

First, the primary data presented to the child might change in some critical way. This might happen because of contact with another speech community, presence of foreigners, or just because the speech community

has taken to using some construction more or less frequently than in a previous generation or with a special kind of pragmatic force.

Second, even if the PLD comes from a single target grammar, the actual data presented to any learner are truncated, some finite subset of what the grammar is capable of generating. After a finite (truncated) sample sequence, children may arrive at a grammar different from that of their parents. This can again lead to a differing linguistic composition in succeeding generations. Once one child converges on some new grammar, then the linguistic composition of the population will change, because the child with the new grammar produces different structures; this may have domino effects.

In short, the diachronic model is this: individual children attempt to attain their caretaker grammar. After a finite number of examples, some are successful, but others may misconverge. The next generation will therefore no longer be linguistically homogeneous. The third generation of children will hear sentences produced by the second – a different distribution – and they, in turn, will attain a different set of grammars. Over successive generations, the linguistic composition evolves as a dynamical system (p. 2).

NB develop a computer simulation which contains precise assumptions about a set of relevant parameters, a learning algorithm, and the primary linguistic data. If these three items are specified appropriately, then the system computes the linguistic composition for the next generation. By repeating the process, NB compute the evolving composition of the population from generation to generation and arrive at a dynamical system.

First, they assume Gibson & Wexler's Trigger Learning Algorithm and they consider the case of a homogeneous population, with no noise or confounding factors like foreign target languages. "Some small proportion of the children misconverge; the goal is to see whether this small proportion can drive language change – and if so, in what direction". (5) shows the language mix after thirty generations. +V2 refers to a grammar with the verb-second property, and the grammars may or may not be Specifier-final or Complement-final, so there are eight language-types.

(5) Initial Language   After 30 generations
        1 (-V2)   2 (0.85), 6 (0.1)
        2 (+V2)   2 (0.98); stable
        3 (-V2)   6 (0.48), 8 (0.38)
        4 (+V2)   4 (0.86); stable
        5 (-V2)   2 (0.97)
        6 (+V2)   6 (0.92); stable
        7 (-V2)   2 (0.54), 4 (0.35)
        8 (+V2)   8 (0.97); stable

This model generates some striking patterns. First, homogeneous populations may split into different groups and they may split at different rates. For example, a population of Language 7 speakers splits over 5-6 generations to one with 54% speaking Language 2 and 35% speaking Language 4 and remains that way with no further shifts through thirty generations. On the other hand, Language 1 eventually gravitates to Language 2, but very little happens over the first six or seven generations, and then the population changes at a much faster rate.

Second, all the verb-second languages are relatively stable and their linguistic composition did not vary significantly over thirty generations. On the other hand, the non-verb-second languages all drift to verb-second. So a population of Language 1 speakers winds up speaking mostly Language 2.

NB have a model which generates diachronic trajectories; there are long-term tendencies for certain language types to change to certain other types. Some of the actual tendencies that they find in their initial model are not realistic. Their model shows verb-second languages to be quite stable, but we know that both English and French lost their verb-second properties, an observation that needs to be explained.

Immediately then, we see that our dynamical system does not evolve in the expected manner. The reason could be due to any of the assumptions behind the model: the parameter space, the learning algorithm, the initial conditions, or the distributional assumptions about sentences presented to learners. Exactly which is in error remains to be seen, but nonetheless our example shows concretely how assumptions about a grammatical theory and learning theory can make evolutionary, diachronic predictions – in this case, incorrect predictions that falsify the assumptions (p. 6).

NB then proceed to change assumptions and to derive different trajectories. For example, they drop one particular constraint from Gibson & Wexler's Trigger Learning Algorithm (the Single Valued Constraint) and the dynamical system yields very different results, shown in (6).

(6) Initial Language   After 30 generations
        1 (-V2)   2 (0.41), 4 (0.19), 6 (0.18), 8 (0.13)
        2 (+V2)   2 (0.42), 4 (0.19), 6 (0.17), 8 (0.12)
        3 (-V2)   2 (0.40), 4 (0.19), 6 (0.18), 8 (0.13)
        4 (+V2)   2 (0.41), 4 (0.19), 6 (0.18), 8 (0.13)
        5 (-V2)   2 (0.40), 4 (0.19), 6 (0.18), 8 (0.13)
        6 (+V2)   2 (0.40), 4 (0.19), 6 (0.18), 8 (0.13)
        7 (-V2)   2 (0.40), 4 (0.19), 6 (0.18), 8 (0.13)
        8 (+V2)   2 (0.40), 4 (0.19), 6 (0.18), 8 (0.13)

Under this scenario, all initially homogeneous populations eventually drift towards the same composition mix after thirty generations, 2, 4, 6, 8. As under the earlier scenario, all populations drift to a population mix of only verb-second languages, and again there is a tendency to gain verb-second systems, contrary to the facts of the history of French and English.

This work opens up the possibility of revising the model in such a way that the diachronic trajectories generated correspond most closely to those that are actually attested, and this introduces a new criterion for the success of a grammatical model. So one seeks a class of grammars and learning theories which yields a dynamical system which matches that of the true evolution of human languages.

That, in turn, suggests that maybe one day work on long-term tendencies may not be as mysterious as I have suggested here, that one can find explanations for long-term diachronic tendencies in terms of the shape of the learning mechanism. To be sure, progress will come only through the precise kind of work that NB exemplify and not through the anecdotal generalizations which have typified some of the work discussed earlier. There will be no historicist principles nor any primitive principles of change. Rather, the explanation for the long-term tendencies, if they emerge, would be a function of the architecture of UG and the learning procedure and of the way in which populations of

speakers behave. In that way the historical tendencies would be epiphenomena, derived in an interesting fashion and not stipulated by brute force.

7. Conclusion

Work on language change has been dominated by nineteenth-century thought. It has been too ambitious, too principled, and has sought to explain too much. Where does that leave us? Language change is fascinating because it represents an interaction between chance oscillations in the trigger experience and the biological necessities of the human language acquisition device. Change may be chaotic and flukey, but it is explainable to a degree. To explain language change, one needs: (a) an account of how trigger experiences shifted and (b) a theory of language acquisition that matches PLD with grammars in a deterministic way. A minor perturbation in a child's trigger experience may entail a new grammar which, in turn, yields dramatically different phenomena. This "sensitive dependence on initial conditions" accounts for why languages change in fits and starts. So we strike a chord with work on "punctuated equilibrium" in biological change and with the work of historians seeking to account for revolutionary political change as the product of minor shifts. But we do so without invoking deterministic, nineteenth-century style laws of history.

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